

# Crime Perception and Voting Behavior: Evidence from Individual Data\*

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## Abstract

This study investigates how exposure to local crime-related news affects individual voting behavior, using geolocated newspaper coverage as a proxy for crime salience. Leveraging a retrospective panel survey of approximately 5,000 voters observed across multiple election rounds, we exploit within-individual variation in exposure to crime news occurring near voters' residences in the pre-election period. Aggregate exposure measures yield weak and unstable effects on voting behavior. Disaggregating by offender nationality reveals systematic heterogeneity: in national elections, immigrant-related crime news reduces support for populist right-wing parties with ambiguous immigration stances and increases support for traditional right-wing "law and order" parties, while Italian-perpetrated crime has no effect. In administrative elections, Italian crime news punishes incumbents, whereas immigrant-related news increases abstention. These findings demonstrate that electoral responses to crime salience—previously obscured in aggregate measures—depend critically on media framing of offender identity.

**Keywords:** crime; elections; immigration; news media; individual voting behavior.

**JEL:** D72; K42; J15; D83; L82.

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## 1. Introduction

Across many European democracies, and especially in Italy, debates over immigration have increasingly been framed through the lens of public safety. A large body of evidence shows that citizens often associate immigration with crime even when such links are weak or distorted in the data, and that media reporting can amplify this association by selectively making certain events salient (Keita et al., 2023; Ajzenman et al., 2023; Couttenier et al., 2024). These dynamics are politically consequential: when crime is framed in ways that emphasize the identity of the perpetrator, it can strengthen narratives of “threat” and shift electoral support toward parties promising tougher enforcement or stricter immigration policies. More generally, research on persuasion highlights that changes in voters’ information sets and beliefs can translate into changes in political behavior (DellaVigna and Gentzkow, 2010; DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Gerber et al., 2009; Barone et al., 2015).

Yet an important question remains unresolved. Do voters primarily react to crime itself, or does crime framed as immigrant-related trigger distinct political responses? The distinction matters because it separates two channels that are often conflated in political discourse. One channel is general crime salience: learning that crime occurred nearby may increase concern about safety and raise support for “law and order” platforms. A second channel is identity framing: when crime is attributed to immigrants, it may also affect attitudes toward immigration and minorities, consistent with mechanisms rooted in stereotypes and availability-driven belief formation (Kahneman and Tversky, 1973; Bordalo et al., 2018). Disentangling these channels is essential for understanding how local shocks to perceived security translate into shifts in party support and the electoral incentives faced by political actors.

This paper investigates whether exposure to salient information about immigrant-related crime in one’s immediate neighborhood shifts individual voting behavior differently from exposure to comparable crimes attributed to Italian nationals. We study this question in Bologna, a large city in Northern Italy, where voting behavior can be measured at the individual level over repeated elections and where local crime information is disseminated through a prominent online outlet. Our key empirical object is *local crime news exposure*. Specifically, we define exposure as the publication of a crime-related news report that can be geocoded within 200 meters of an individual’s home address during the month preceding an election round. Crimes attributed to Italian nationals serve as a benchmark that allows us to separate general crime salience from the immigration-specific framing channel emphasized by populist rhetoric (Keita et al., 2023; Ajzenman et al., 2023).

We interpret our measure as capturing the subset of criminal events that becomes

*salient information* to residents through news reporting. We do not claim that media coverage is a complete or unbiased measure of the universe of crimes; rather, it captures the information set plausibly available to voters in real time and at street-level granularity, which is the relevant object for belief formation and political responses (DellaVigna and Gentzkow, 2010; Mastrorocco and Minale, 2018). Unlike much of the existing literature, our approach embeds fine-grained spatial and temporal variation directly by linking individuals to geocoded local crime reports, rather than relying on cross-sectional variation in media access combined with aggregate time variation in news content.<sup>1</sup>

Our empirical design compares declared voting choices across multiple elections for the same individuals who are differentially exposed to nearby immigrant-related and native-related crime reports. The analysis exploits fine spatio-temporal variation in the timing and location of local crime news, allowing us to track individual switching behavior across elections. Conceptually, the strategy follows a differences-in-differences design that leverages within-individual variation combined with plausibly idiosyncratic shocks in local crime reporting at the street level.

To implement this design, we combine two novel data sources. First, we use a representative survey of 5,000 Bologna residents conducted in 2021 that records retrospective voting behavior across nine elections between 2004 and 2021. Second, we assemble a corpus of approximately 11,000 crime-related articles published between 2011 and 2022 by *BolognaToday*. Using a dictionary-based coding scheme standard in text-as-data applications (Gentzkow et al., 2019), we classify whether reports describe perpetrators as immigrants or Italian nationals and geocode each report at street level. The underlying behavioral premise is straightforward: information about a crime occurring in one's immediate vicinity is more likely to be noticed and perceived as relevant than information about crime occurring farther away, consistent with salience and availability mechanisms (Kahneman and Tversky, 1973; Mastrorocco and Minale, 2018).

Because our empirical strategy relies on local media reporting, a natural concern is whether coverage itself responds to political preferences across space. We address this concern directly. Exploiting a difference-in-discontinuities design in the spirit of Grembi et al. (2016), we compare crime coverage across historically right- and left-leaning neighborhoods around electoral thresholds and find no evidence of differential coverage or framing of crime by area ideology.

Our main findings reveal asymmetric electoral effects. Individuals exposed to nearby immigrant-related crime reports become significantly more likely to shift their support

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<sup>1</sup>This differs from studies exploiting the staggered introduction of Fox News in the United States (DellaVigna and Kaplan, 2007), differential access to independent television in Russia (Enikolopov et al., 2011), or the expansion of digital television channels in Italy (Barone et al., 2015).

toward right-wing and anti-immigration parties, both in national and local elections. By contrast, exposure to comparable crimes attributed to Italian nationals does not generate similar shifts. In national elections, immigrant-related crime exposure reduces switching toward the Center-Left and toward the Lega, while increasing switching toward the broader Center-Right coalition. In local elections, immigrant-related crime exposure reduces switching toward the Center-Left while increasing abstention and support for smaller parties, with additional evidence of mobilization toward the Center-Right.

This paper contributes to several strands of literature. First, it adds to research in the economics of crime documenting that crime has broader consequences beyond victimization itself, affecting economic and political outcomes (Buonanno et al., 2013; Dustmann and Fasani, 2016; Drago et al., 2020). Second, it contributes to the literature on media and political persuasion, which shows that exposure to media content can shift beliefs and electoral outcomes (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Gerber et al., 2009; Barone et al., 2015). Our evidence further complements work documenting partisan distortions in Italian television news (Durante and Knight, 2012) and their electoral consequences (Barone et al., 2015). Related research shows that crime coverage in particular shapes perceptions of safety and attitudes toward immigrants (Mastrorocco and Minale, 2018; Ajzenman et al., 2023; Sacco, 1982; Couttenier et al., 2024).

Third, our results speak to a broader literature on stereotypes and misperceptions, which shows that vivid and salient signals can reinforce distorted beliefs about immigration (Alesina et al., 2023). Our findings are consistent with mechanisms in which immigrant-related crime news disproportionately affects beliefs through availability and stereotype-based distortions (Kahneman and Tversky, 1973; Bordalo et al., 2018). Finally, our analysis relates to work on crime, social dynamics, and political participation (Blanes i Vidal and Mastrobuoni, 2018; Kirchmaier et al., 2020; Vargas et al., 2025).

In sum, we provide novel evidence that exposure to salient information about immigrant-related crime in one's immediate environment has a significant impact on voting behavior. By linking individual voting histories to geocoded local crime reporting, we offer a micro-level perspective on how identity framing and crime salience interact in shaping electoral outcomes. The remainder of the paper proceeds as follows. Section 2 describes the data and the construction of exposure measures. Section 3 outlines the empirical strategy. Section 4 presents the main results and heterogeneity analyses, while Section 5 provides supporting evidence for the identification strategy. Section 6 concludes. Additional information and robustness checks are provided in the Appendix.

## 2. Data

### 2.1. Survey Data

Our analysis relies on the survey data originally designed and collected for the work [Berti Ceroni et al. \(2025\)](#), so most of the technical details can be found therein. We will stress here the fundamental features that are key to this analysis.

The survey was administered in December 2021 by a professional firm and covers 5,000 Italian citizens residing in Bologna, a 400-thousands-inhabitants city in Northern Italy, who were eligible to vote and had lived in the city since at least 2013. Respondents were stratified across the city's 18 neighborhoods, with interviews conducted via telephone (70%) and online (30%). The questionnaire collected detailed demographic and socio-economic characteristics—such as birth year and province, education, employment status, occupation, and family structure and, key to our purposes, retrospective voting choices in all national (2006, 2008, 2013, 2018) and municipal elections (2004, 2009, 2011, 2016, 2021).<sup>2</sup> A distinctive feature of the dataset obtained from the survey is the provision of respondents' street of residence (without house numbers for privacy). This information enables sufficiently precise geo-localization and makes it possible to link individual voters to spatially disaggregated measures of media coverage of crime in their neighborhood.

To enhance recall accuracy regarding retrospective voting, a brief neutral summary of the political environment around each specific electoral round was provided to the participants before asking about their voting behaviors. Moreover, to mitigate potential consistency bias, voting questions were asked in chronological order, starting from the earliest election.

Validation against official electoral outcomes shows close alignment, with minor discrepancies only in the 2011 and 2016 municipal elections, when the Center-Right and Lega presented joint candidates. Internal mobility within Bologna is limited: more than 96% of respondents in 2021 lived in the same area as in 2013, reducing concerns about voting-with-feet within the city boundaries. Additional descriptive evidence and validation exercises are presented in [Appendix A](#).

### 2.2. Crime Measures from Newspapers

Our primary source of crime data is *BolognaToday*, a leading independent online newspaper.<sup>3</sup> According to *Similarweb* data, the outlet averages nearly 6 thousand organic

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<sup>2</sup>The empirical analysis will exploit five electoral rounds: the national elections of 2013 and 2018, and the municipal elections of 2011, 2016, and 2021.

<sup>3</sup>At the time of data collection, the website was freely accessible. Subscription plans for premium content were introduced only in 2025, after the period considered in our analysis.

visitors per month, making it a highly relevant source of information for the local electorate.<sup>4</sup> To construct the dataset, we implemented an automated web scraping protocol targeting the newspaper’s archive section, which is dedicated to local news and daily events. We extracted the complete digital archive of this section from its establishment in 2011 through 2022. The scraper parsed the HTML structure of the website to systematically retrieve the headline, summary, full text, and publication timestamp for approximately 55,000 unique articles.

We classify the content of these articles using a dictionary-based algorithm designed to minimize false positives (Gentzkow et al., 2019; Muço, 2025). Rather than relying on simple keyword matching, our approach identifies crime events based on the co-occurrence of specific crime-category keywords (e.g., *theft*, *assault*, *drugs*, *murder*) with terms indicating a verified police action or report (e.g., “*arrested*”, “*denounced*”, “*investigated*”). Simultaneously, the algorithm scans for linguistic markers distinguishing the nationality of the offenders (Italians vs. non-Italians). This procedure identifies crime-related news in 18.5% of the local news archive.

We successfully geolocated approximately 76% of these crime reports at the street level. It is important to note that serious offenses such as murders or kidnappings represent a marginal fraction of this coverage—roughly 3.7% of crime articles. Because high-profile violent crimes generate highly repeated coverage, the effective number of unique violent events is likely even lower; consequently, the dataset is dominated by less violent categories such as theft or assault.<sup>5</sup> The resulting daily panel dataset consists of approximately 7,800 geolocated crime articles, of which 46% involve immigrant offenders. To address potential measurement error arising from redundant reporting—where a single incident generates multiple articles over several days—we do not use raw article counts as our primary measure of crime intensity. Instead, we construct indicator variables equal to one if a crime is reported in a specific location on a given day. These geocoded indicators form the basis for constructing individual-level measures of exposure to crime news, which we merge with electoral records. Further technical details on the dictionary validation and scraping architecture are provided in [Appendix B](#).

### 3. Empirical Strategy

The empirical analysis examines whether exposure to *crime-related news* in the immediate vicinity of an individual affects voting behavior in national and local elections.

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<sup>4</sup>To put this magnitude in perspective, this readership creates a ratio of roughly one visitor for every two residents in the metropolitan area of Bologna.

<sup>5</sup>According to the Italian National Institute of Statistics (ISTAT), the annual volume of minor property and violent crimes has averaged roughly 3 million in recent years, relative to 350 homicides.

We focus on crime-related news coverage referring to criminal events occurring within a 200-meter radius from the respondent's street of residence during the 30 days preceding each election. This time window coincides with the official electoral campaign period regulated by the Italian "*par condicio*" law (Law 28/2000), which governs media coverage and ensures equal access to public and private media outlets in the run-up to elections.

Throughout this section, we use the term "crime-related news" to denote media coverage of criminal events, rather than underlying crime incidence.

Our contribution lies in distinguishing between crime-related news attributed to Italian citizens and those attributed to immigrants, allowing us to assess whether voters respond differently depending on the perceived nationality of offenders. The data are organized as an individual-level panel, where the cross-sectional dimension is the voter and the time dimension corresponds to successive election rounds.

To estimate the effect of exposure to crime-related news on voting behavior, we rely on fixed-effects specifications of the following form:

$$Vote_{it}^P = \alpha CI_{it} + \beta CNI_{it} + \gamma_i + \delta_t + \epsilon_{it}, \quad (1)$$

$$Vote_{it}^P = \alpha CI_{it} + \beta CNI_{it} + \gamma_i + \delta_{dt} + \epsilon_{it}, \quad (2)$$

where  $Vote_{it}^P$  is a dummy equal to one if individual  $i$  votes for party (or option)  $P$  in election  $t$ , and zero otherwise. Elections are grouped by type (national and administrative), and we estimate separate regressions for each party or voting option, including abstention.

The variables  $CI_{it}$  and  $CNI_{it}$  are indicators equal to one if at least one crime-related news item attributed to Italian citizens or immigrants, respectively, is reported within 200 meters of the individual's residence during the 30 days preceding election  $t$ . We rely on binary exposure measures rather than article counts because a single criminal incident may generate multiple news reports over several days. Using indicator variables mitigates distortions arising from repeated coverage of the same event and focuses on the salience of crime-related news exposure rather than its volume.<sup>6</sup>

Accordingly, we interpret these indicators as measures of exposure to salient crime-related news rather than to underlying crime incidence.

The term  $\gamma_i$  denotes individual fixed effects, which absorb all time-invariant voter characteristics, including baseline political preferences, ideology, and residential location. The fixed effects  $\delta_t$  capture election-specific shocks common to all voters, while

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<sup>6</sup>Appendix C reports robustness checks using alternative radii (300 and 500 meters) and specifications based on article counts.

$\delta_{dt}$  denotes district-by-election fixed effects that flexibly control for time-varying local political and socioeconomic conditions.

Identification of  $\alpha$  and  $\beta$  relies on within-individual variation in exposure to crime-related news across elections, conditional on election-wide and district-specific shocks, and individual fixed effects. This variation arises from the idiosyncratic timing and highly localized spatial nature of crime-related news coverage within districts across election cycles. Intuitively, the coefficients capture how changes in local crime-news exposure relative to an individual's own past elections are associated with changes in the probability of voting for a given party. Because individual fixed effects are included, the estimates do not reflect cross-sectional differences in voting levels, but rather within-individual changes in vote choice relative to previous elections, holding constant baseline political preferences.

Standard errors are clustered at the district level to account for spatial correlation in crime reporting and local media coverage.

Two identification concerns merit discussion. First, crime-news coverage may be strategically manipulated around elections or may correlate with the historical political orientation of neighborhoods. Second, exposure to crime-related news may display short-run persistence around election dates that is not fully absorbed by the fixed effects structure. We address these concerns through a set of diagnostic exercises discussed in [Section 5](#).

Specifically, we implement a difference-in-discontinuities analysis around national elections to test for discontinuous changes in crime-news coverage across districts with different historical political orientations ([Subsection 5.1](#)). In addition, we estimate placebo regressions using crime-related news occurring after election dates. As shown in [Subsection 5.2](#), these exercises reveal no evidence of strategic reporting around elections or of spurious post-election effects. Together, they mitigate concerns related to strategic reporting and short-run persistence, supporting the interpretation of crime-related news exposure as plausibly exogenous with respect to individual vote switching within the election cycle.

## 4. Main Results

Our primary analysis focuses on national elections. Concentrating on national contests mitigates the role of local political issues and institutional idiosyncrasies, and allows us to study voting behavior in a setting where parties compete on national platforms and where law-and-order and immigration are salient dimensions of political debate.

[Table 1](#) reports the main results. [Table 1A](#) presents estimates based on an aggregate measure of crime-related news exposure, while [Table 1B](#) distinguishes between crime-

related news attributed to Italian citizens and to immigrants.

**Table 1:** Effects of Crime Perception on Voting at National Elections

Dep.	Panel A: Overall Crimes									
	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Total Crimes	-0.012** (0.004)	-0.011* (0.005)	-0.005 (0.010)	-0.005 (0.012)	0.010 (0.006)	0.010 (0.006)	0.007** (0.003)	0.004** (0.001)	0.007** (0.003)	0.007** (0.002)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts fixed effects × Year fixed effects	✓		✓		✓		✓		✓	
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

Dep.	Panel B: Crimes by Nationality of the Offenders									
	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crimes by Italians	-0.004 (0.007)	-0.004 (0.007)	0.000 (0.014)	-0.002 (0.014)	0.002 (0.013)	0.004 (0.013)	0.004 (0.003)	0.002 (0.003)	0.005 (0.005)	0.007 (0.005)
Crimes by immigrants	-0.023** (0.007)	-0.021** (0.007)	-0.015** (0.004)	-0.010 (0.007)	0.021** (0.006)	0.021** (0.008)	0.007 (0.006)	0.008 (0.006)	0.005 (0.010)	0.001 (0.011)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts fixed effects × Year fixed effects	✓		✓		✓		✓		✓	
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

*Notes:* This table reports OLS estimates of the effect of local crime on voting behavior at national elections. The unit of observation is the individual  $i$  at time  $t$ . The dependent variable is a dummy that takes the value 1 if the individual votes for parties such as M5S, Lega, Center Left, Center Right, or abstains from voting. In Table 1A the independent variable is a dummy that takes value 1 if there is at least one crime around 200m from the residence street, 30 days before the election day. In Table 1B the key explanatory variables are indicators for whether at least one crime is committed by Italians or immigrants within 200 meters of the respondent's street in the 30 days following election day. Robust standard errors are clustered at district level.

**Aggregate crime exposure.** Table 1A shows that when crime-related news exposure is measured in the aggregate—without distinguishing the nationality of the offenders—the estimated effects on voting behavior are small and unstable across parties. While some coefficients are statistically significant, their magnitudes are modest and their signs vary across political options, offering no coherent pattern of electoral response. This lack of structure suggests that aggregate measures of crime-related news obscure important heterogeneity in how voters interpret and react to crime information.

**Crime-related news by offenders' nationality.** Table 1B reveals a markedly different and substantially more structured pattern once crime-related news is disaggregated by the perceived nationality of offenders. Exposure to crime-related news attributed to Italian citizens has no statistically significant effect on voting behavior across parties. In contrast, exposure to crime-related news attributed to immigrants induces systematic changes in individual vote choices.

Specifically, immigrant-related crime news is associated with a decline in the probability of voting for the Five Star Movement (M5S), with estimated effects ranging between 2.1 and 2.3 percentage points. We also observe a smaller and less precisely estimated reduction in support for Lega. At the same time, exposure to immigrant-related crime news increases support for the Center-Right coalition by approximately 2.1 percentage points. These effects indicate a reallocation of votes away from parties with either ambiguous or heterogeneous positions on immigration toward parties that place greater emphasis on law-and-order policies.

#### *4.1. Interpretation and Conceptual Framework*

All estimates discussed above reflect within-individual changes in voting behavior across elections, relative to the same individual's past vote choices, rather than cross-sectional differences in baseline preferences.

The patterns documented above indicate that crime-related news does not affect voting behavior mechanically or uniformly. Instead, electoral responses emerge only when crime-related information activates salient political narratives linking crime, immigration, and security.

In this perspective, crime-related news attributed to Italian citizens does not convey new or politically informative content. Such events are likely interpreted as idiosyncratic or non-systemic, and therefore fail to trigger meaningful revisions in vote choice. By contrast, crime-related news attributed to immigrants resonates with a broader and highly politicized narrative that associates immigration with security concerns. This narrative is actively emphasized in political discourse and media framing, particularly by parties on the right.

Our findings align with theories of issue ownership, according to which voters respond to shocks by reallocating support toward parties perceived as more competent or credible on the relevant issue. In the Italian context, law and order and immigration control are core dimensions of political competition on the right. When crime-related news explicitly references immigrants, voters appear to update their beliefs about which parties are better positioned to address these concerns, leading to systematic vote reallocation.

Importantly, the results also highlight asymmetries within the right-wing bloc. Parties with ambiguous or internally heterogeneous positions on immigration—such as the Five Star Movement and, to a lesser extent, Lega—experience electoral losses following exposure to immigrant-related crime news. In contrast, parties with a more traditional and unambiguous law-and-order platform benefit from these shocks. This pattern suggests that ambiguity on highly salient issues may be electorally costly when voters are

confronted with information that heightens perceived security risks.

More broadly, the contrast between aggregate crime exposure and nationality-specific crime exposure underscores the importance of framing and attribution. When crime-related news lacks clear attribution, its electoral impact is weak and unstable. Once attribution is made salient, however, voter responses become systematic and directional. This finding contributes to a growing literature emphasizing that political reactions to crime are mediated not only by crime itself, but by how crime is narrated and linked to broader social categories.

**Heterogeneity.** The conceptual framework developed above implies that electoral responses to crime-related news should not be uniform across voters. If crime exposure operates by activating narratives about immigration and security, responsiveness should vary systematically across groups that differ in political sophistication, economic vulnerability, and baseline exposure to these narratives.

We therefore examine heterogeneity along key demographic and socioeconomic dimensions, focusing in particular on education and skill levels. These dimensions are plausibly related to how individuals interpret and react to crime-related information, as well as to their prior beliefs about immigration and security. By contrast, heterogeneity by parental status and gender does not yield qualitatively different insights and is therefore reported in [Appendix C](#).

This heterogeneity analysis helps clarify which segments of the electorate are most responsive to crime-related news exposure and provides additional evidence in support of the mechanism proposed above.

#### *4.2. Heterogeneity Effects on National Elections*

The conceptual framework developed above also yields clear predictions about heterogeneous electoral responses to crime-related news. If crime exposure affects voting behavior primarily by activating narratives linking crime, immigration, and security, responsiveness should vary systematically across voter groups that differ in political sophistication, economic vulnerability, and baseline exposure to these narratives. In this subsection, we investigate heterogeneity along education and occupational skill dimensions ([Table 2](#)), which proxy for differences in information processing, political awareness, and sensitivity to law-and-order framing.

**Education.** We begin by examining heterogeneity by educational attainment. Education plausibly captures voters' ability to interpret and contextualize crime-related information within broader political narratives. More educated voters may be better

**Table 2:** Differential Effects of Crime Perception on Voting at National Elections

Dep.	Panel A: By Education Level									
	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crime by Italians × low education	-0.015 (0.010)	-0.014 (0.010)	0.024 (0.023)	0.021 (0.026)	0.002 (0.022)	0.004 (0.024)	0.008 (0.009)	0.006 (0.009)	-0.005 (0.019)	-0.003 (0.018)
Crime by Italians × high education	0.002 (0.015)	0.002 (0.016)	-0.013 (0.018)	-0.016 (0.017)	0.002 (0.017)	0.004 (0.017)	0.001 (0.008)	0.000 (0.007)	0.011 (0.006)	0.012 (0.007)
Crime by immigrants × low education	-0.013 (0.011)	-0.012 (0.012)	-0.039*** (0.009)	-0.034** (0.012)	0.029* (0.013)	0.030* (0.014)	-0.005 (0.010)	-0.003 (0.010)	0.016 (0.013)	0.012 (0.012)
Crime by immigrants × high education	-0.027** (0.008)	-0.026** (0.008)	-0.003 (0.008)	0.002 (0.009)	0.016 (0.009)	0.016 (0.010)	0.013 (0.009)	0.015 (0.010)	-0.001 (0.014)	-0.005 (0.015)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts × Year fixed effects		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

Dep.	Panel B: By Skills Level									
	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crime by Italians × low skills	-0.012** (0.004)	-0.011* (0.004)	0.009 (0.016)	0.006 (0.018)	-0.011 (0.016)	-0.009 (0.017)	0.017 (0.011)	0.015 (0.011)	0.001 (0.008)	0.003 (0.008)
Crime by Italians × high skills	0.013 (0.015)	0.014 (0.016)	-0.019 (0.027)	-0.021 (0.025)	0.030 (0.024)	0.032 (0.024)	-0.026 (0.017)	-0.027 (0.017)	0.014* (0.006)	0.015** (0.005)
Crime by immigrants × low skills	-0.007 (0.012)	-0.006 (0.012)	-0.011 (0.007)	-0.006 (0.010)	0.028** (0.010)	0.028* (0.011)	0.002 (0.015)	0.003 (0.016)	0.011 (0.012)	0.007 (0.014)
Crime by immigrants × high skills	-0.048*** (0.005)	-0.046*** (0.004)	-0.022 (0.015)	-0.017 (0.014)	0.008 (0.006)	0.008 (0.007)	0.016 (0.017)	0.019 (0.016)	-0.006 (0.020)	-0.010 (0.020)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts × Year fixed effects		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.12	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

*Notes:* This table reports OLS estimates of the effect of local crime on voting behavior at national elections. The unit of observation is the individual  $i$  at time  $t$ . The dependent variable is an indicator equal to one if the respondent votes for M5S, Lega, Center Left, Center Right, or abstains from voting. The key explanatory variables are indicators for whether at least one crime is committed by Italians or immigrants within 200 meters of the respondent's street in the 30 days following election day. These variables are interacted with individual characteristics capturing heterogeneity by education (Panel A) and by skill level (Panel B). Robust standard errors, clustered at the district level, are reported in parentheses.

positioned to recognize ideological inconsistencies or ambiguity in parties' stances on immigration and security, and therefore more likely to update their vote choice when salient crime-related news conflicts with these positions.

Consistent with this interpretation, the estimates in [Table 2A](#) indicate that highly educated voters are more likely to abandon the Five Star Movement (M5S) following exposure to immigrant-related crime news. By contrast, lower-educated voters exhibit stronger disengagement from Lega and are more likely to reallocate support toward the Center-Right coalition. This pattern suggests that education shapes not only the intensity of electoral reactions, but also the direction of vote reallocation, with more educated voters penalizing ideological ambiguity and less educated voters responding more directly to law-and-order appeals.

**Occupational skill.** We next turn to heterogeneity by occupational skill level, which captures differences in labor-market vulnerability and exposure to redistributive and security concerns. Lower-skilled voters may perceive crime-related shocks as more directly threatening to economic and social stability, thereby placing greater weight on parties with clear and traditional law-and-order platforms.

The results in [Table 2B](#) closely mirror those by education. Highly skilled voters disproportionately exit M5S following immigrant-related crime exposure, while lower-skilled voters exhibit a systematic shift toward the Center-Right coalition. Together, these findings reinforce the interpretation that crime-related news affects voting behavior through the activation of security-related narratives, with heterogeneous responses reflecting differences in political sophistication and perceived exposure to the underlying risks emphasized by these narratives.

#### *4.3. Administrative Elections*

We now turn to administrative elections, which provide a complementary setting to assess how the electoral impact of crime-related news depends on the level of government at which political accountability operates. Compared to national elections, local contests are characterized by stronger incumbency considerations and clearer attribution of responsibility for local conditions, including public safety.

[Table 3](#) reports estimates analogous to those presented for national elections, distinguishing between crime-related news attributed to Italian citizens and to immigrants.

[Table 3](#) reports estimates analogous to those presented for national elections, distinguishing between crime-related news attributed to Italian citizens and to immigrants.

The results in [Table 3](#) reveal a qualitatively different pattern from that observed in national elections. In administrative contests, exposure to crime-related news attributed

**Table 3:** Differential Effects of Crime Perception on Voting at Administrative Elections

Dep.	M5S		Center Right & Lega		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crimes by Italians	0.003 (0.006)	0.002 (0.006)	0.005 (0.006)	0.007 (0.006)	-0.015* (0.006)	-0.015** (0.006)	0.005 (0.006)	0.003 (0.006)
Crimes by immigrants	-0.003 (0.006)	-0.002 (0.006)	0.004 (0.012)	-0.000 (0.012)	-0.011 (0.011)	-0.007 (0.013)	0.012** (0.005)	0.014** (0.005)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓	
Districts × Year fixed effects		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.27	0.27	0.14	0.14	0.00	0.00
Observations	14436	14436	14436	14436	14436	14436	14436	14436

*Notes:* This table reports OLS estimates of the effect of local crime on voting behavior at administrative elections. The unit of observation is the individual  $i$  at time  $t$ . The dependent variable is a dummy that takes the value 1 if the individual votes for parties such as M5S, Lega, Center Left, Center Right, or abstains from voting. The independent variables are dummies that takes value 1 if there is at least one crime committed by Italians and by immigrants, respectively, around 200m from the residence street, 30 days before the election day. Robust standard errors are clustered at district level.

to Italian citizens leads to a reduction in support for the Center-Left, consistent with electoral punishment of local incumbents. This pattern aligns with standard accountability mechanisms, whereby voters hold local governments responsible for perceived failures in maintaining public order.

By contrast, when crime-related news is attributed to immigrants, we observe an increase in abstention rather than a systematic reallocation of votes across parties. This response suggests that, in local elections, crime narratives linked to immigration do not translate into clear electoral alternatives for voters who are dissatisfied with incumbent performance. Instead of switching parties, some voters disengage from the electoral process altogether.

Taken together, these findings underscore that the political consequences of crime-related news depend not only on attribution, but also on the institutional context in which elections take place. While national elections facilitate vote reallocation across parties competing on immigration and security platforms, local elections emphasize incumbent accountability, leading either to electoral punishment or to abstention when credible alternatives are absent.

## 5. Identification Concerns

In this section, we provide additional evidence supporting the plausibility of our identifying assumptions and the robustness of our main estimates.

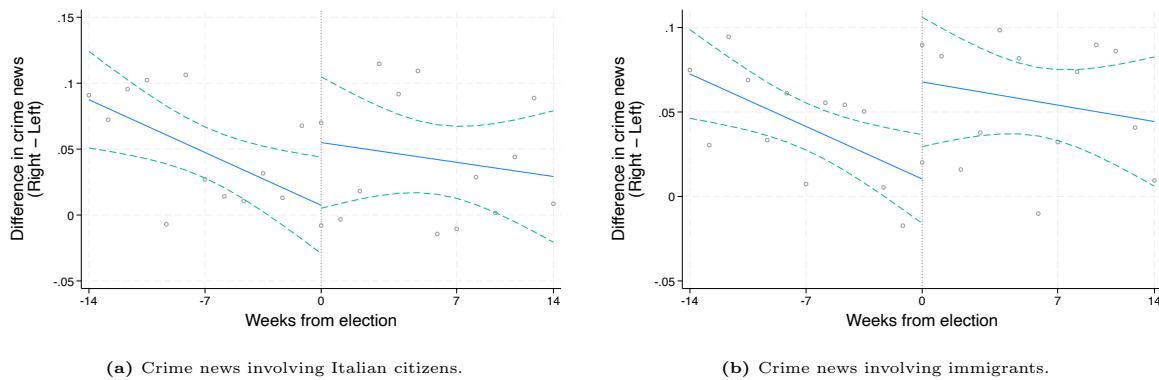
### 5.1. Crime News Coverage and Political Orientation Around Elections

A key identifying assumption of our empirical strategy is that local crime news coverage is not systematically influenced around elections by the political orientation of neighborhoods. In particular, we assume that neither strategic editorial choices by local media outlets nor differential reporting behavior by residents—such as a higher propensity to report crimes or suspicious activities to authorities in certain politically aligned areas—drive changes in crime news coverage during the electoral period. If this assumption were violated, observed variation in crime-related news could reflect partisan reporting dynamics rather than the arrival of salient local information.

To assess the plausibility of this assumption, we examine whether crime news coverage evolves differently around elections in districts historically aligned with the right and with the left. Rather than estimating a causal effect, this exercise serves as a diagnostic check for potential strategic behavior in news reporting around electoral events.

Specifically, we implement a difference-in-discontinuities style analysis around the 2013 and 2018 national elections. For each week relative to the election date, we compute the difference in crime-related news coverage between right-leaning and left-leaning districts, where political orientation is assigned using vote shares from the 2008 general election, which predates our sample period.

[Figure 1](#) plots the evolution of this right–left difference in local crime news coverage in the 14 weeks before and after the elections. [Figure 1a](#) focuses on crime news involving Italian citizens, while [Figure 1b](#) considers crime news involving immigrants. In both panels, solid lines represent linear fits estimated separately on each side of the election threshold, with dashed lines indicating 95% confidence intervals.



**Fig. 1:** Differences in crime news coverage between right- and left-leaning districts around national elections.

*Notes:* This figure plots the difference in district-level weekly average shares of crime-related news mentions between right-leaning and left-leaning districts (Right minus Left). The time window spans 14 weeks before and after the 2013 and 2018 national elections. District political orientation is assigned using vote shares from the 2008 general election. Each dot represents the weekly difference in average crime news coverage. Solid lines are linear fits estimated separately on each side of the election threshold, with dashed lines denoting 95% confidence intervals.

Across both types of crime, we find no evidence of a discontinuous change in the right-left difference at the election threshold. Pre- and post-election trends appear smooth and continuous, and the estimated confidence intervals rule out economically meaningful jumps at the cutoff.

This visual evidence is corroborated by formal difference-in-discontinuities tests, reported in [Appendix C](#), which are consistent with the absence of statistically significant jumps at the election threshold.

Taken together, these results provide no indication that local crime news coverage is strategically adjusted around elections in response to district political alignment. This finding supports our interpretation of media-reported crime as reflecting the diffusion of local information rather than short-run political manipulation.

### *5.2. Testing identifying assumptions: placebo effect of crime news after the elections*

In our primary analysis, we assume that crime perception due to news articles influence voting behavior primarily due to their publication during the political campaign period. To validate this assumption, we estimate the effects of crime perception within the 30-day window preceding the elections, while excluding the first two weeks to mitigate potential persistence effects of news coverage both before and after the electoral event. This approach allows us to isolate the immediate impact of crime-related media exposure on voter decisions, ensuring that our results are not driven by longer-term trends or post-election reporting.

In our primary analysis, we assume that crime-related news affects voting behavior primarily when it occurs during the electoral campaign period. To assess this assumption, we conduct placebo tests using crime-related news exposure occurring after elections. If our main results were driven by persistent trends or spurious correlations, we would expect to observe similar effects following election dates.

Accordingly, we estimate specifications analogous to our baseline models using crime-related news exposure within a 30-day window after elections, excluding the first two weeks to mitigate mechanical overlap with pre-election reporting. This approach allows us to isolate post-election exposure and assess whether crime-related news continues to influence voting behavior outside the campaign period.

Upon examining the results presented in [Table 4](#), we find no statistically significant effects associated with post-election crime-related news exposure. This suggests that our main estimates are not driven by spurious correlations or persistent trends in crime reporting. The absence of significant post-election effects strengthens the credibility of our identification strategy and supports the interpretation that the observed impact of crime-related news on voting behavior is specific to the electoral campaign period.

**Table 4:** Differential Effects of Crime Perception on Voting after National Elections

Dep.	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crimes by Italians	-0.010 (0.006)	-0.009 (0.006)	-0.012 (0.010)	-0.011 (0.011)	0.008 (0.010)	0.009 (0.010)	-0.010 (0.015)	-0.008 (0.015)	0.015 (0.009)	0.013 (0.010)
Crimes by immigrants	-0.001 (0.008)	0.003 (0.008)	0.000 (0.013)	-0.001 (0.016)	-0.004 (0.015)	-0.001 (0.016)	0.005 (0.018)	0.006 (0.017)	-0.001 (0.009)	-0.002 (0.008)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts fixed effects × Year fixed effects		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.02
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

*Notes:* This table reports OLS estimates of the effect of local crime-related news exposure on voting behavior **after** national elections. The unit of observation is the individual  $i$  at time  $t$ . The dependent variable is a dummy equal to one if the individual votes for parties such as M5S, Lega, Center Left, Center Right, or abstains from voting. The independent variables are indicators equal to one if at least one crime-related news item attributed to Italian citizens or immigrants, respectively, is reported within 200 meters of the respondent's street of residence in the 30 days **after** the election day. Robust standard errors are clustered at the district level.

## 6. Conclusion

This paper studies how crime-related news shapes individual voting behavior, focusing on the role of media salience and offender attribution in Italian elections. Using geolocated crime news matched to a panel of non-relocating voters observed across multiple electoral cycles, we examine whether exposure to crime-related information in the immediate pre-election period affects electoral choices.

Our findings reveal a sharp asymmetry. When crime-related news does not specify offenders' nationality, electoral responses are weak, unstable, and difficult to interpret. By contrast, when crime is explicitly attributed to immigrants, voters exhibit systematic and directional reactions. In national elections, exposure to immigrant-related crime news leads voters to withdraw support from parties with ambiguous or internally heterogeneous positions on immigration—most notably the Five Star Movement—and to reallocate votes toward parties with a clear law-and-order profile. Crimes attributed to Italian citizens, instead, do not generate meaningful electoral responses.

These effects are heterogeneous across voter characteristics. Highly educated and high-skilled voters disproportionately abandon the Five Star Movement, while lower-skilled and less-educated voters are more likely to shift away from Lega. This pattern is consistent with the idea that crime-related news affects voting behavior through the activation of salient political narratives linking immigration and security, rather than through generalized concerns about crime.

Electoral responses differ in administrative elections, where accountability and social context are more localized. In this setting, crimes attributed to Italian citizens tend to generate punishment of incumbent administrations, while immigrant-related crime news primarily increases abstention. This suggests that, at the local level, voters may perceive

switching parties in response to immigration-related crime as socially or ideologically costly, opting instead for electoral disengagement.

Taken together, these results highlight that crime-related news does not influence voting behavior mechanically. Its electoral impact depends critically on attribution, framing, and the political narratives it activates. By showing that voter responses hinge on how crime is reported rather than on crime *per se*, this study contributes to the literature on crime perception, media influence, and electoral behavior, and underscores the importance of narrative framing in shaping democratic accountability.

An important consideration for external validity concerns the political context of Bologna, which has historically been characterized by strong center-left dominance and relatively limited electoral support for anti-immigration platforms. In this sense, the estimated effects should be interpreted as a lower bound of the electoral impact of crime-related news in more politically competitive or right-leaning contexts, where immigration and security concerns may be more salient.

Future research could further refine these insights by incorporating richer information on offender–victim pairings and media framing, allowing for a more granular analysis of how different crime narratives shape political behavior.

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## Appendix

### A. Survey Data, Local Context, and Electoral Validation

This Appendix provides background on the local political context, details on the survey design, and validation exercises comparing survey-based voting reports with official electoral outcomes. Together, these elements support the internal consistency of the data and clarify the scope of external validity of our findings.

#### *A.1. Local Political Context: Bologna*

Bologna is the seventh most populous Italian city, with nearly 400,000 residents and sustained population growth over the past two decades (+15%). Civic engagement is traditionally high, with voter turnout consistently exceeding the national average. Historically, Bologna has been a stronghold of the center-left, although recent elections have witnessed increasing support for populist parties, particularly the Five Star Movement and, to a lesser extent, the Lega (Fetzer, 2019; Guiso et al., 2024).

Crime and public safety are salient issues in local political debate and receive substantial media attention. Local news outlets frequently report crime-related events at a highly granular geographic level, contributing to sustained public exposure to crime-related information.

This political and informational environment makes Bologna a particularly suitable setting to study the electoral consequences of crime-related news exposure. At the same time, its historically left-leaning electorate implies that estimated effects—especially those related to immigration and law-and-order narratives—should be interpreted as a lower bound relative to contexts where right-wing parties have stronger baseline support. In settings with weaker left-wing dominance or higher political polarization, similar crime-related news shocks may plausibly generate larger electoral responses.

#### *A.2. Survey Design and Sample Construction*

The survey was administered in December 2021 to 5,000 Italian citizens who had resided in Bologna continuously since at least 2013. Respondents were stratified across the city's 18 neighborhoods, with 70% interviewed by telephone (C.A.T.I.) and 30% online (C.A.W.I.). The questionnaire collected detailed demographic and socio-economic information, including year and place of birth, education, occupation, and family composition, as well as retrospective voting behavior in national and municipal elections between 2004 and 2021.

A distinctive feature of the survey is the collection of respondents' street of residence

(excluding house numbers), which allows geo-localization at a fine spatial scale. This feature enables us to match individuals to highly localized measures of exposure to crime-related news and to exploit within-city spatial variation in media coverage.

The empirical analysis focuses on a subsample of 4,812 individuals who did not change residence during the observation period. Restricting attention to non-relocating individuals ensures that changes in crime-related news exposure are not mechanically driven by residential mobility, thereby strengthening the interpretation of within-individual variation in exposure across elections.

To mitigate recall bias in retrospective voting reports, voting questions were asked in chronological order and preceded by neutral reminders of the political context of each election. Importantly, the empirical strategy relies on within-individual changes in reported vote choices across elections. As a result, concerns related to level misreporting are mitigated as long as recall errors are not systematically correlated with localized crime-related news exposure.

Survey mode (C.A.T.I. versus C.A.W.I.) is orthogonal to both election timing and local crime exposure. Moreover, all specifications include individual fixed effects, which absorb any time-invariant differences in reporting behavior across respondents and survey modes.

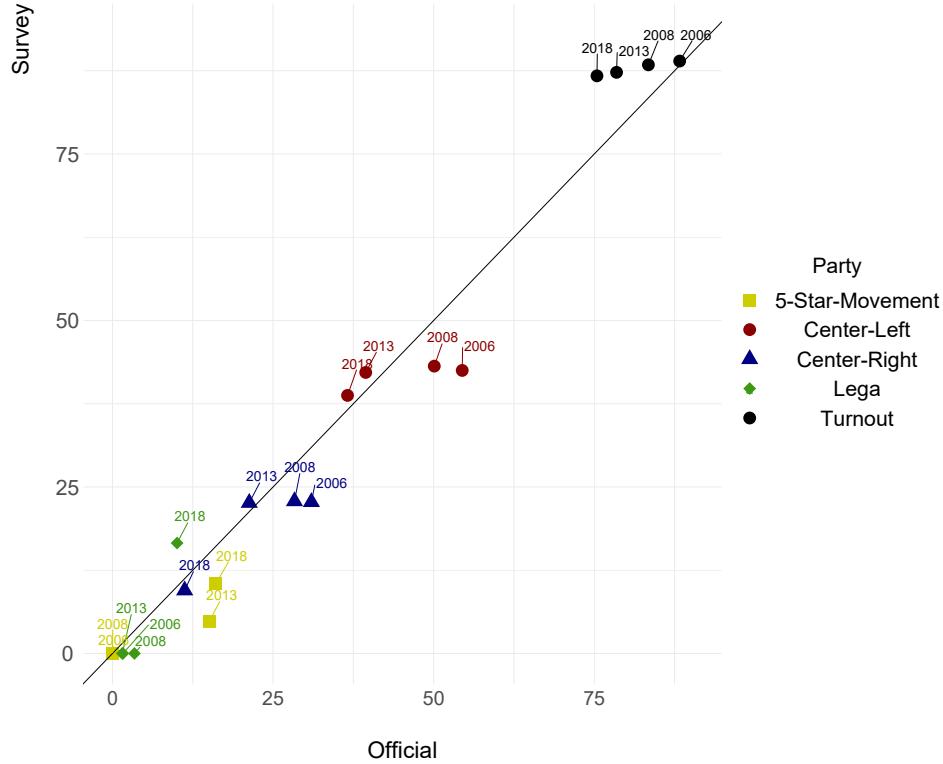
### *A.3. Electoral Validation*

To assess the external validity of survey-based voting reports, we compare self-reported vote shares with official electoral outcomes. [Figure A.1](#) plots survey-based and official vote shares across the five elections used in the empirical analysis and shows a close correspondence for all major parties. Minor deviations emerge in the 2011 and 2016 municipal elections, when the Center-Right and Lega fielded joint candidates, complicating direct comparisons.

While this validation is conducted at the aggregate level, it provides reassurance that systematic misreporting is limited. This is particularly relevant given that our empirical analysis focuses on within-individual changes in voting behavior rather than on cross-sectional vote levels.

Summary statistics for the final sample are reported in [Table A.1](#). Respondents were on average born in 1970, lived in households of approximately three members, and attained secondary or post-secondary education. Females account for 54% of the sample, and roughly half of respondents were employed at the time of the survey.

Taken together, the descriptive evidence and validation exercises support the suitability of the survey data for analyzing within-city electoral behavior in Bologna and for studying how localized exposure to crime-related news shapes individual voting



**Fig. A.1:** Comparison between survey-based and official vote shares

*Notes:* The figure compares survey-based and official vote shares in the five elections used in the empirical analysis.

**Table A.1:** Descriptive Statistics of the Survey Sample

Variable	Observations	Mean	Std. Dev.	Min	Max
<b>Individual Characteristics</b>					
Birth Year	4812	1970.145	18.41787	1927	2003
Female	4812	.5367	.4986509	0	1
Household Members	4812	2.954	.922489	1	6
Education (1=PhD,7=Primary School)	4812	5.145	1.109011	1	7
Working	4812	.5054	.4999766	0	1
Student	4812	.1140	.3179048	0	1
Retired	4812	.2640	.4408551	0	1
Homemaker	4812	.0723	.2590022	0	1

*Notes:* Descriptive statistics refer to the final sample of 4,812 non-relocating individuals observed across nine elections.

decisions.

## B. Local News Corpus on Crime and Geographic Exposure

This Appendix documents the construction of the local crime-related news corpus, the procedures used to classify and geolocate crime-related news events, and the main

descriptive patterns of exposure across individuals and neighborhoods.

### *B.1. Data Source and Collection*

Our measures of local crime exposure are based on *BolognaToday*, a leading online local news outlet operating independently of political parties. According to data from *SimilarWeb*, *BolognaToday* attracts approximately 589,000 organic visitors per month. Readers view an average of 7.35 pages per session and spend roughly two minutes per visit. The audience is relatively young and balanced by gender, making the outlet a central source of local information for residents of Bologna.

The archive contains approximately 55,000 articles published between 2010 and 2021. Of these, about 18.5% (roughly 11,000 articles) report on crime-related events. Among crime-related articles, approximately 41% explicitly mention immigrants as perpetrators. Around 75.7% of crime-related articles include sufficiently precise geographic references to allow street-level geolocation, yielding about 7,800 usable observations, of which approximately 46% concern crimes attributed to immigrants.

Importantly, the prominence of crime coverage in *BolognaToday* reflects both the salience of crime in local political debate and editorial choices about which events are deemed newsworthy. As such, our data capture the information environment faced by voters rather than objective crime rates.

Before turning to individual-level exposure measures, we document the main spatial patterns of crime-related news coverage across the city.

### *B.2. Crime Classification and Textual Processing*

Crime-related articles are identified using a dictionary-based classification approach following established methods in computational text analysis ([Gentzkow et al., 2019](#); [Muço, 2025](#)). Articles are classified as crime-related when keywords associated with specific offenses (e.g., theft, assault, fraud, rape, murder, kidnapping, and drug-related crimes) co-occur with institutional terms such as “arrest,” “reported,” or “investigation.”

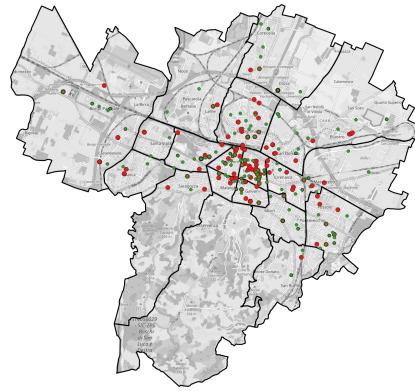
Nationality markers embedded in the text allow us to distinguish between crimes attributed to Italian citizens and those attributed to immigrants. This approach prioritizes transparency and replicability while limiting researcher discretion. Crucially, the classification captures how crime is framed and narrated in the media, rather than the true incidence or severity of criminal activity.

In local crime reporting, references to non-Italian nationality are systematically made explicit, whereas crimes committed by Italian citizens are typically described using place of origin (e.g., city of residence) or initials of the offender’s name and surname,

without mentioning nationality. As a result, the absence of nationality markers in an article is informative and reflects standard editorial conventions rather than missing information. Accordingly, crime-related articles in which no nationality is specified are classified as involving Italian citizens. This classification does not rely on imputation, but mirrors how offender identity is conventionally framed in local media coverage. Our analysis therefore captures differences in media salience and framing between crimes attributed to immigrants and those implicitly attributed to Italians.

### B.3. Descriptive Spatial Patterns in Crime News Coverage

Crime-related news coverage exhibits pronounced spatial heterogeneity across neighborhoods. Peripheral areas such as *Colli* consistently display no reported crimes, while central districts—such as *Bolognina*, *Marconi*, and *Irnerio*—show persistent concentrations of crime reporting (Figure B.1).

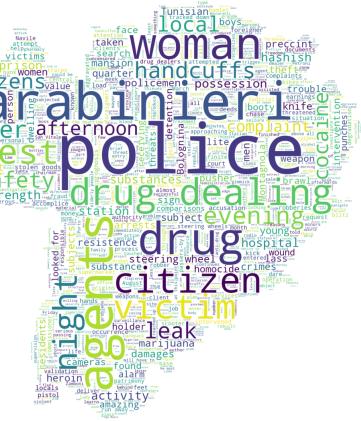


**Fig. B.1:** Spatial distribution of crime news in the five election years (2011, 2013, 2016, 2018, and 2021) in the month before each election.

*Notes:* Red dots (●) indicate crimes attributed to immigrants; green dots (●) indicate crimes attributed to Italians.

The linguistic content of crime reporting, illustrated in Figure B.2, emphasizes institutional actors (e.g., police, carabinieri), enforcement actions (e.g., arrests), and offense types. Frequent references to nationality and demographic attributes suggest that crime reporting is often framed within identity-based narratives, particularly in articles involving immigrants.

Figure B.3a and Figure B.3b display the spatial distribution of crime-related news reports in the months preceding the 2013 and 2018 elections. Both maps reveal clustering in central districts, with immigrant-related incidents appearing more spatially concentrated. While such clustering may partly reflect underlying patterns of criminal activity, it may also capture editorial selectivity in reporting.



**Fig. B.2:** Most frequent terms in the crime-related news corpus.

*Notes:* The figure is a visual representation of text data where words are displayed in varying sizes, with the most frequent words appearing largest and most prominent



**Fig. B.3:** Spatial distribution of crime-related news reports by nationality in the month before the given election.

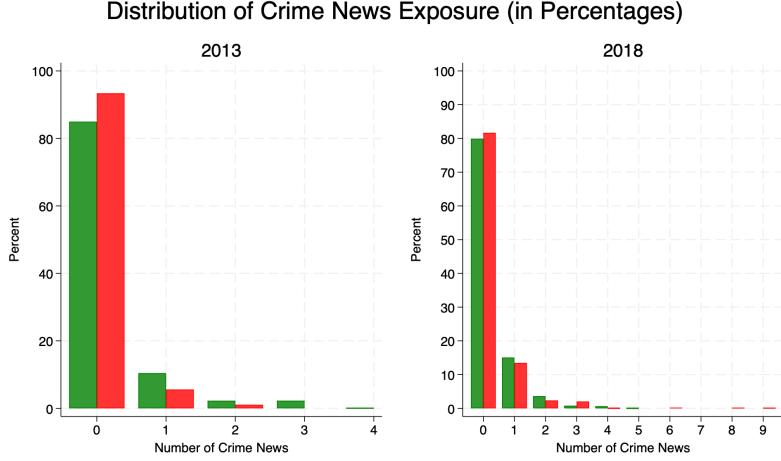
*Notes:* Red dots (●) indicate crimes attributed to immigrants; green dots (●) indicate crimes attributed to Italians

#### B.4. Individual Exposure and Measurement Choices

Individual exposure to crime-related news is highly right-skewed. As shown in Figure B.4, most individuals are linked to zero or one crime-related article within the pre-election window, while a small minority experience substantially higher exposure. This skewness is more pronounced for immigrant-related crime news.

This distribution motivates our use of binary exposure indicators in the main analysis. By focusing on whether an individual is exposed to at least one crime-related news item within a narrow spatial and temporal window, we capture the salience of crime-related information while limiting the influence of extreme observations driven by repeated reporting of the same incident.

Accordingly, our exposure measures should be interpreted as proxies for localized



**Fig. B.4:** Distribution of individual exposure to crime news in 2013 and 2018.

*Notes:* Exposure is highly right-skewed. Most individuals are linked to zero or one article, while a minority are associated with disproportionately high coverage.

crime salience rather than as measures of crime intensity or frequency. This conservative measurement choice reduces sensitivity to editorial amplification and strengthens the credibility of the estimated electoral effects by ensuring that identification is not driven by repeated reporting of the same incident.

### C. Robustness and Additional Results

This Appendix reports robustness checks and additional heterogeneity analyses that complement the baseline results in the main text. Unless otherwise noted, all regressions use the baseline specification and the same sample restrictions as in the main analysis. Across exercises, the qualitative patterns documented in the main text are generally stable: effects attenuate when exposure is defined over broader spatial or temporal windows, consistent with lower salience of more distant or older news.

#### *C.1. Formal test for a discontinuity in crime-news coverage by district ideology*

Figure 1 plots the weekly difference in crime-news coverage between right-leaning and left-leaning districts (Right minus Left) around the 2013 and 2018 national elections. To complement the visual evidence, Table C.1 reports a formal discontinuity test.

Specifically, we collapse the data to the election-year  $\times$  week level and define the outcome as the weekly Right–Left difference in district-level average crime-news shares. We then estimate a piecewise-linear specification that allows slopes to differ on the two sides of the election week and includes election-year fixed effects. The coefficient on

*Post-election jump (at week 0)* captures the discontinuity at the threshold.

Consistent with the figure, we do not detect statistically meaningful discontinuities at the election threshold, either for crime news involving Italian citizens or for crime news involving immigrants. Overall, these results do not support the hypothesis that crime-news coverage is strategically adjusted around elections in a way that differentially affects right- versus left-leaning districts.

**Table C.1:** Difference-in-discontinuities estimates of crime-news coverage around elections

	Italians	Immigrants
Post-election jump (at week 0)	0.396 (0.264)	0.364* (0.192)
Weeks from election	-0.061*** (0.018)	-0.034** (0.013)
Post × Weeks	0.058* (0.033)	0.042* (0.024)
Election FE	✓	✓
R <sup>2</sup>	0.167	0.468
Observations	52	52

*Notes:* The unit of observation is election-year × week. The dependent variable is the weekly difference in district-level average crime-news shares between right-leaning and left-leaning districts (Right minus Left). The sample includes weeks within ±14 of the 2013 and 2018 national elections. The coefficient on *Post-election jump (at week 0)* captures the discontinuity at the election threshold. Linear trends are allowed to differ before and after the threshold via Post × Weeks. Robust standard errors are reported in parentheses.

### C.2. Heterogeneity by districts' historical political orientation

Table C.2 examines whether the electoral response to crime-related news exposure differs by districts' historical political orientation. The estimates suggest that reactions to immigrant-related crime news are more pronounced in historically right-leaning districts, where exposure is associated with a lower probability of voting for populist/anti-establishment parties (M5S and, to a lesser extent, Lega) and a corresponding reallocation toward the traditional Center Right. In historically left-leaning districts, estimates are generally smaller and less precisely estimated, although we still observe negative responses for M5S and Lega following immigrant-related crime exposure.

For crime-related news attributed to Italian citizens, effects are comparatively weaker and less systematic across district types, consistent with the main-text evidence that nationality attribution is a key driver of structured electoral responses.

**Table C.2:** Effects of crime-related news exposure on voting in national elections, by districts' historical political orientation

Dep.	M5S		Lega		Center		Left		Center Right		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crime by Italians in Right districts	0.006** (0.002)	0.006*** (0.001)	-0.015 (0.024)	-0.018 (0.027)	0.015** (0.005)	0.012** (0.004)	0.012 (0.026)	0.015 (0.028)	0.002 (0.003)	0.003 (0.004)		
Crime by Italians in Left districts	-0.010 (0.010)	-0.009 (0.010)	0.008 (0.013)	0.006 (0.014)	-0.002 (0.006)	-0.003 (0.006)	-0.003 (0.014)	-0.002 (0.015)	0.007 (0.007)	0.009 (0.008)		
Crime by immigrants in Right districts	-0.027*** (0.004)	-0.026*** (0.003)	-0.017** (0.005)	-0.023*** (0.001)	-0.001 (0.003)	-0.001 (0.006)	0.022 (0.013)	0.027* (0.011)	0.011 (0.012)	0.012 (0.015)		
Crime by immigrants in Left districts	-0.020* (0.008)	-0.018 (0.009)	-0.015** (0.005)	-0.002 (0.007)	0.012 (0.008)	0.015 (0.008)	0.020** (0.006)	0.017 (0.009)	0.001 (0.014)	-0.007 (0.015)		
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓		✓	
Districts × Year fixed effects	✓		✓		✓		✓		✓		✓	
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.02	0.02	0.11	0.11	0.01	0.01		
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624		

*Notes:* The unit of observation is individual  $i$  in election  $t$ . The dependent variable is an indicator equal to one if the respondent votes for the party (or option) reported in the column header, and zero otherwise. Key explanatory variables indicate whether at least one crime-related news item attributed to Italian citizens or to immigrants is reported within 200 meters of the respondent's street of residence in the 30 days preceding the election. All specifications include individual fixed effects and election (or district-by-election) fixed effects as in the baseline. Robust standard errors are clustered at the district level.

### C.3. Intensity of exposure (article counts)

The baseline analysis uses binary exposure indicators to capture the salience of at least one crime-related news item while limiting sensitivity to repeated reporting of the same incident. As a robustness check, Table C.3 replaces these indicators with the total number of crime-related news items within the same spatial and temporal window. Coefficients can be interpreted as the marginal change in the probability of voting for a given party associated with one additional crime-related news item.

**Table C.3:** Exposure intensity (article counts)

Dep.	M5S		Lega		Center		Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crimes by Italians	-0.001 (0.004)	0.000 (0.004)	0.002 (0.008)	-0.001 (0.009)	-0.003 (0.008)	-0.001 (0.008)	-0.002 (0.002)	0.006** (0.002)	0.005* (0.004)	-0.000 (0.004)		
Crimes by immigrants	-0.012** (0.004)	-0.011** (0.003)	-0.013** (0.005)	-0.011 (0.007)	0.013 (0.006)	0.014 (0.008)	0.002 (0.002)	0.003 (0.002)	0.006** (0.005)	-0.001 (0.004)		
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓		✓	
Districts × Year fixed effects	✓		✓		✓		✓		✓		✓	
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01		
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624		

*Notes:* Estimates analogous to the baseline specification but replacing the exposure dummies with the number of crime-related news items in the relevant window. Robust standard errors are clustered at the district level.

### C.4. Alternative distance thresholds

Table C.4 and Table C.5 replicate the baseline analysis using alternative radii. When expanding the radius from 200 meters to 300 meters, estimates are similar in sign

and magnitude to the baseline. When using a 500-meter radius, coefficients tend to attenuate and become less precise, consistent with reduced salience of more distant events and with greater measurement noise in broader spatial definitions.

**Table C.4:** Alternative distance threshold: 300m radius

Dep.	M5S		Lega		Center		Right		Center		Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crimes by Italians	-0.013 (0.011)	-0.012 (0.012)	-0.007 (0.011)	-0.010 (0.010)	0.011 (0.009)	0.014 (0.008)	0.010 (0.008)	0.008 (0.008)	0.001 (0.005)	0.003 (0.004)				
Crimes by immigrants	-0.023*** (0.005)	-0.022*** (0.005)	-0.005 (0.007)	0.000 (0.007)	0.012 (0.007)	0.013 (0.008)	0.004 (0.007)	0.005 (0.008)	0.005 (0.006)	0.002 (0.005)				
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓		✓		✓	
Districts fixed effects × Year fixed effects		✓		✓		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01				
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624				

*Notes:* Baseline specification with exposure defined within a 300m radius. Robust standard errors are clustered at the district level.

**Table C.5:** Alternative distance threshold: 500m radius

Dep.	M5S		Lega		Center		Right		Center		Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crimes by Italians	-0.010 (0.008)	-0.009 (0.008)	-0.005 (0.007)	-0.008 (0.006)	0.007 (0.011)	0.010 (0.011)	0.002 (0.006)	0.000 (0.007)	0.004 (0.008)	0.007 (0.008)				
Crimes by immigrants	-0.016* (0.006)	-0.014 (0.008)	-0.009 (0.010)	-0.003 (0.010)	0.004 (0.008)	0.007 (0.011)	-0.000 (0.004)	0.001 (0.005)	-0.004 (0.005)	-0.011*** (0.003)				
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓		✓		✓	
Districts fixed effects × Year fixed effects		✓		✓		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01				
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624				

*Notes:* Baseline specification with exposure defined within a 500m radius. Robust standard errors are clustered at the district level.

### C.5. Alternative time windows

Table C.6 extends the exposure window from 30 to 90 days. As expected, point estimates tend to be smaller when older news is included, consistent with decaying salience over time. The qualitative pattern, however, remains aligned with the baseline.

### C.6. Alternative exposure measures: minimum distance

Table C.7 measures exposure using the minimum distance from the respondent's street to the closest geolocated crime-related news item in the pre-election window. Results are broadly consistent with the baseline in terms of signs and relative patterns, supporting robustness to alternative exposure metrics.

**Table C.6:** Alternative time window: 200m radius, 90-day exposure

Dep.	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crimes by Italians	-0.006 (0.005)	-0.003 (0.006)	-0.009 (0.010)	-0.006 (0.010)	0.012 (0.012)	0.013 (0.012)	0.002 (0.007)	0.001 (0.007)	0.007 (0.006)	0.005 (0.006)
Crimes by immigrants	0.000 (0.007)	-0.002 (0.007)	-0.013** (0.004)	-0.013* (0.006)	0.020 (0.012)	0.019 (0.013)	-0.001 (0.009)	-0.004 (0.009)	-0.002 (0.008)	-0.002 (0.008)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts fixed effects × Year fixed effects		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.12	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

*Notes:* Baseline specification with exposure measured over the 90 days preceding the election. Robust standard errors are clustered at the district level.

**Table C.7:** Alternative exposure metric: minimum distance (30-day window)

Dep.	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Closest crime by Italians	-0.000 (0.005)	0.001 (0.005)	0.012 (0.009)	0.015 (0.009)	-0.015 (0.010)	-0.016 (0.011)	-0.007 (0.007)	-0.004 (0.007)	0.005 (0.004)	0.002 (0.004)
Closest crime by immigrants	0.007** (0.002)	0.004 (0.005)	0.011** (0.004)	0.016 (0.010)	-0.000 (0.002)	-0.005 (0.007)	0.009* (0.004)	0.025** (0.008)	-0.003 (0.003)	-0.001 (0.003)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts × Year fixed effects		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01
Observations	9620	9620	9620	9620	9620	9620	9620	9620	9620	9620

*Notes:* Baseline specification with exposure measured as minimum distance to the closest crime-related news item in the 30 days preceding the election. Robust standard errors are clustered at the district level.

### C.7. Additional heterogeneity analyses

We further explore heterogeneity by parental status and gender. [Table C.8](#) reports estimates for the 30-day window; [Table C.9](#) replicates the analysis using the 90-day window. Overall, these splits do not overturn the main-text interpretation; if anything, they suggest that responsiveness is somewhat stronger among parents, while gender differences are comparatively modest.

**Table C.8:** Additional heterogeneity: parental status and gender (30-day window)

Dep.	Panel A: By Having Children									
	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crime by Italians × w/ children	-0.009*	-0.008	0.005	0.002	0.002	0.004	-0.001	-0.003	0.005	0.007
	(0.005)	(0.005)	(0.007)	(0.008)	(0.013)	(0.014)	(0.008)	(0.009)	(0.008)	(0.008)
Crime by Italians × w/o children	0.010	0.010	-0.010	-0.012	0.002	0.003	0.018	0.017	0.005	0.006
	(0.020)	(0.020)	(0.039)	(0.039)	(0.035)	(0.035)	(0.026)	(0.025)	(0.023)	(0.024)
Crime by immigrants × w/ children	-0.022**	-0.020*	-0.031**	-0.025	0.019	0.019	-0.007	-0.005	0.000	-0.004
	(0.008)	(0.008)	(0.011)	(0.013)	(0.010)	(0.013)	(0.007)	(0.007)	(0.013)	(0.014)
Crime by immigrants × w/o children	-0.024	-0.023	0.031	0.033	0.024	0.025	0.049*	0.048*	0.017	0.014
	(0.015)	(0.014)	(0.030)	(0.030)	(0.026)	(0.024)	(0.019)	(0.020)	(0.012)	(0.010)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts × Year fixed effects		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

Dep.	Panel B: By Gender									
	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crime by Italians × female	-0.002	-0.001	-0.008	-0.010	0.006	0.008	0.017	0.015	0.001	0.003
	(0.007)	(0.007)	(0.022)	(0.023)	(0.015)	(0.016)	(0.009)	(0.009)	(0.013)	(0.013)
Crime by Italians × male	-0.006	-0.006	0.008	0.006	-0.002	-0.000	-0.009	-0.011	0.009	0.011*
	(0.009)	(0.009)	(0.007)	(0.008)	(0.012)	(0.013)	(0.009)	(0.009)	(0.004)	(0.005)
Crime by immigrants × female	-0.028*	-0.026	-0.015	-0.010	0.020**	0.020*	-0.002	-0.000	0.012	0.008
	(0.013)	(0.013)	(0.011)	(0.011)	(0.007)	(0.008)	(0.016)	(0.016)	(0.011)	(0.011)
Crime by immigrants × male	-0.016*	-0.015*	-0.016	-0.011	0.021	0.021	0.017	0.019	-0.004	-0.008
	(0.007)	(0.007)	(0.016)	(0.018)	(0.012)	(0.013)	(0.009)	(0.010)	(0.012)	(0.012)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts × Year fixed effects		✓		✓		✓		✓		✓
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.11	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

*Notes:* Baseline specification estimated separately by subgroup. Exposure is defined within 200m in the 30 days preceding the election. Robust standard errors are clustered at the district level.

**Table C.9:** Additional heterogeneity: parental status and gender (90-day window)

Panel A: By Having Children

Dep.	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crime by Italians × w/ children	-0.000 (0.006)	0.002 (0.007)	-0.011 (0.013)	-0.009 (0.013)	0.011 (0.017)	0.012 (0.016)	-0.001 (0.009)	-0.002 (0.008)	0.001 (0.007)	0.000 (0.007)
Crime by Italians × w/o children	-0.021** (0.005)	-0.019** (0.006)	-0.001 (0.009)	0.002 (0.008)	0.013 (0.017)	0.013 (0.018)	0.023 (0.009)	0.012 (0.010)	0.011 (0.016)	0.022 (0.015)
Crime by immigrants × w/ children	-0.000 (0.009)	-0.002 (0.009)	-0.024** (0.008)	-0.023** (0.008)	0.019 (0.016)	0.018 (0.017)	-0.008 (0.005)	-0.011** (0.004)	-0.006 (0.011)	-0.006 (0.012)
Crime by immigrants × w/o children	0.001 (0.015)	-0.003 (0.014)	0.021 (0.030)	0.017 (0.031)	0.024 (0.022)	0.023 (0.022)	0.021 (0.027)	0.016 (0.028)	0.008 (0.011)	0.010 (0.011)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts × Year fixed effects	✓		✓		✓		✓		✓	
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.12	0.02	0.02	0.01	0.02
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

Panel B: By Gender

Dep.	M5S		Lega		Center Right		Center Left		Abstention	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Crime by Italians × female	-0.006 (0.006)	-0.004 (0.006)	-0.006 (0.014)	-0.003 (0.015)	0.004 (0.019)	0.004 (0.019)	0.013 (0.011)	0.012 (0.011)	0.002 (0.009)	0.001 (0.009)
Crime by Italians × female	-0.005 (0.005)	-0.003 (0.006)	-0.013 (0.008)	-0.010 (0.008)	0.020** (0.007)	0.021** (0.007)	-0.010 (0.012)	-0.011 (0.012)	0.011 (0.008)	0.010 (0.008)
Crime by immigrants × female	0.006 (0.012)	0.004 (0.012)	-0.016 (0.010)	-0.016 (0.010)	0.028* (0.012)	0.027* (0.013)	0.002 (0.005)	-0.002 (0.005)	-0.006 (0.009)	-0.005 (0.010)
Crime by immigrants × female	-0.007 (0.006)	-0.009 (0.006)	-0.009 (0.017)	-0.010 (0.018)	0.011 (0.016)	0.010 (0.017)	-0.004 (0.016)	-0.008 (0.016)	0.001 (0.007)	0.002 (0.008)
Individual fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓		✓		✓		✓		✓	
Districts × Year fixed effects	✓		✓		✓		✓		✓	
R <sup>2</sup>	0.05	0.05	0.16	0.16	0.11	0.12	0.02	0.02	0.01	0.01
Observations	9624	9624	9624	9624	9624	9624	9624	9624	9624	9624

*Notes:* Baseline specification estimated separately by subgroup. Exposure is defined within 200m in the 90 days preceding the election. Robust standard errors are clustered at the district level.

Overall, the robustness exercises in this Appendix indicate that the main qualitative patterns are not an artifact of a specific exposure definition or window, and they behave in the expected direction when salience is mechanically reduced (broader radii or longer windows).