Anti-Muslim Voting and Media Coverage of Immigrant Crimes

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Abstract. We study how news coverage of immigrant criminality impacts voting in one of the most controversial referendums in recent years – the 2009 Swiss minaret ban. We combine a comprehensive crime detection dataset with detailed information on newspaper coverage. We first document a large upward distortion in media reporting of immigrant crime during the prereferendum period. Exploiting quasi-random variations in crime incidence, we find a positive first-order effect of news coverage on support for the ban. Quantification shows that, in absence of the media bias, pro-ban vote would have decreased from 57.6% to 53.5% at the national level.

Keywords: Media, violent crime, immigration, vote.

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1 Introduction

A central element in far-right rhetoric is the notion that communities with growing immigrant populations become unsafe. Since voters are often unable to accurately assess the propensity of immigrants to commit crimes, their beliefs are fed by two possibly non-representative crime samples: observation in their local communities and reports in the news. Media coverage of immigrant criminality, and its potential bias, may thus significantly affect political outcomes and support for far-right parties.

In this article, we investigate the response of voting patterns to reports of crime in the media. To this end, we focus on one of the most controversial referendums in recent years: the Swiss 2009 vote to ban minarets. The unexpected outcome, which drew attention from around the world, was a clear majority (57.5%) in favor of banning minaret construction in Switzerland. Initiated by the far-right Swiss People's Party (*SVP/UDC*), the pro-ban campaign clearly stigmatized Islam, a religion practiced by a recently arrived immigrant community. Importantly, the Swiss People's Party played aggressively on fears of Muslim immigration by linking Islam to terrorism and violence.

Our empirical analysis focuses on so-called *signal crimes* only – such as homicide, murder, manslaughter, and infanticide – which "have a disproportionate impact upon fear of crime" (Innes et al., 2002). We make use of an exhaustive dataset of crime detection, that includes information on perpetrators' nationalities, to compare raw facts and crime news retrieved from 12 major Swiss newspapers, to estimate any potential media bias that might exist in crime reporting, and to disentangle the impact of real versus reported criminality on voting.

We start by estimating a model of crime news provision, i.e. the probability that a given

crime will appear in the news. We find that, everything else being equal, the coverage probability of crimes perpetrated by immigrants is twice as large as the baseline probability. The extent of this media bias varied during the course of the political campaign leading up to the referendum. Thus, in the quarter preceding the referendum, the coverage probability increased to up to 5 times the baseline for foreign perpetrators and then reverted to its pre-campaign level following the referendum, implying that the communication strategy employed by the Swiss People's Party was highly effective in tilting public debate on the minaret ban towards questions of violence and immigration.

In the remainder of the analysis, we provide evidence supporting a causal impact of crime news exposure on the outcome of the Minaret ban referendum. We first carry out an OLS estimation of the municipality-level pro-ban vote share on crime news exposure (CNE). We compute CNE by averaging the *reported* crime propensity differential between immigrants and natives across newspapers using municipality-level market shares as weights. In this naive setup, the main issue pertains to omitted variables, such as latent xenophobia, which may co-determine CNE and voting patterns. We therefore implement an instrumental variable (IV) strategy that exploits a pattern uncovered in the estimation of the model of crime news provision, namely that besides perpetrator nationality and local readership share, a key driver of news coverage is the spatial proximity between the crime location and the newspaper's headquarters.

Our estimation results show that CNE has a positive, statistically significant, and first-order impact on municipality-level voting in favor of the ban. Our quantification indicates that, in absence of media bias, the vote in favor of the ban would have been 4.1 percentage points less at the national level (a decline from 57.6% to 53.5%). The findings are robust to a large battery of

sensitivity checks.[1]

Literature review – The paper contributes primarily to the literature on the interplay between immigration and far-right voting, a topic that has recently received widespread attention. Scholars have used shift-share instrumental variables as well as quasi-random refugee allocation policies to assess the effect of increasing immigration on voting results. This literature considered labor market concerns (Edo et al., 2019; Moriconi et al., 2018), concerns about welfare and compositional amenities (Barone et al., 2016; Halla et al., 2017; Otto and Steinhardt, 2014), and social and cultural factors (Dustmann et al., 2018; Hangartner et al., 2019) as important determinants of the anti-immigration vote. More closely related to our analysis, empirical studies have shown that security concerns, and in particular the fear that immigrants might be more crime-prone than natives, are key drivers of attitudes toward immigration (Card et al., 2012; Fitzgerald et al., 2012; Ward, 2019). In the same vein, far-right campaigns regularly exploit narratives linking crime concerns to immigration, a communication strategy that leads to prejudiced political platforms (Smith, 2010) and which constitutes an example of *issue ownership*, according to which parties benefit from promoting issues that they are perceived as "best at handling" (Petrocik, 1996). Giavazzi et al. (2020) document how perceived threats associated with terrorist attacks have influenced the support for radical right parties in Germany. Conversely, Keita et al. (2021) find that the disproportional me-

[1]In Couttenier et al. (2019), we build a model of crime news and probabilistic voting that structurally grounds the econometric specifications. We also provide additional empirical results on the (in-)capacity of voters to debias news and their over-reaction to crime news perpetrated by foreigners.

dia disclosure of the origins of native offenders in the post-2015 German context reduced natives' concerns about immigration.

Our findings add to this literature by highlighting the role played by the media in fueling personal security concerns linked to immigration. In this respect, it follows in the wake of a well-established series of papers, pioneered by McCombs and Shaw (1972) and surveyed in Strömberg (2015), which acknowledge the impact of media on voting behavior. Drawing on the literature that links media coverage of salient political issues to electoral politics,[2] and exploiting a novel identification strategy, our analysis is the first, to the best of our knowledge, to show a quantitatively large impact of news reporting of immigrant crime news on far-right voting. These results are in line with the view that a step increase in media coverage of immigrant crime acts as a shock on the salience of this issue (Aragonès et al., 2015), known in the literature as *priming*. They are also in line with the empirical results of Mastrorocco and Minale (2018) who show that the crime reporting in the media exacerbates individuals' perception of crime in general (in contrast, we look at the political consequences of crime reporting).

Our results contribute to the quantitative literature on media bias (Gentzkow et al., 2015; Puglisi and Snyder, 2015). Firstly, we show that the extent of media bias reacts to the political agenda. Thus, we observe a substantial and transient increase in the coverage of violent crimes committed by immigrants during the Minaret Ban campaign. As shown in Golder (2016), this example of *me-*

[2]See Strömberg (2004); Gentzkow (2006); DellaVigna and Kaplan (2007); Gerber et al. (2009); Oberholzer-Gee and Waldfogel (2009); Gentzkow et al. (2011); Enikolopov et al. (2011); Drago et al. (2014); Durante et al. (2015).

dia populism appears not just in tabloids but also in high-quality newspapers. Secondly, a strand of the literature, starting from Eisensee and Strömberg (2007) and Snyder and Strömberg (2010), has shown how demand for news as well as news pressure drive the extent of media coverage of certain issues. Our findings add to this literature by exploring the filtering of information related to violent crimes, i.e. the fact that a given crime may or may not be reported by a newspaper. We highlight a supply-side shifter in the coverage of criminality, by showing that close proximity between the location where crimes are committed and newspapers' newsrooms significantly increases the probability of coverage.

The rest of the paper is organized as follows. Section 2 provides background information on the Minaret Ban referendum and the data. Section 3 analyzes the empirical determinants of media coverage of violent crime while Section 4 estimates the effect of crime news coverage on vote. Section 5 concludes.

2 Background and Data

2.1 The Minaret Ban Referendum

The minaret ban referendum was initiated primarily by politicians from the far-right Swiss People's Party (*SVP/UDC*). It proposed the addition of a single sentence to the constitution: "The construction of minarets is prohibited". The government, both chambers of the legislature, and all majority parties except the Swiss People's Party opposed the initiative. Interestingly, the Minaret Ban would have little policy relevance since as of 2009 there were only 4 minarets in Switzerland, none of which performed a prayer call. They were, however, depicted as symbolic of the spread of

Islam in the country. The initiators of the referendum exploited the fact that the Muslim population in the country had increased from just 56,600 Muslims in 1980 to close to half a million, most of whom are foreign-born and of non-European origin. The campaign capitalized on fears of Muslim immigration by linking Islam to terrorism and violence. Indeed, the leading campaign poster depicted minarets as missiles flying out of the Swiss flag. According to the nationally-representative VOX survey, 87% of respondents who agree with the statement "Minarets are terrorist shelters" voted in favor of the ban.

The campaign contributed to the perception that the Minaret ban was an efficient way of reducing future migration from Muslim countries toward Switzerland (Mayer, 2011; Wyler, 2017). In reaction, the Swiss Federal council conducted an intense counter-campaign of public relation for restoring a positive image of Switzerland in Muslim countries, both before and after the referendum. This policy of damage control was quite successful and, ultimately, the minaret ban did not translate into a large reduction of immigration from Muslim-majority countries.

The referendum took place on November 29, 2009. Pre-referendum polls had indicated a comfortable majority in opposition to the proposal, though it diminished as the referendum approached. The participation rate (53.9%) turned out to be the highest observed during the previous five years and the initiative unexpectedly won 57.6% of the ballots. While approval of the ban was initially perceived as a response to increased fears of Islam, the voting patterns reveal a strong anti-immigrant component. The correlation between the vote's outcome and that of past immigration referendums is large and significant, ranging from 0.7 to 0.8.

2.2 Data

We collected data on criminality, news coverage and voting patterns at the municipality level. With the dual objective of comparing pre- and post-referendum voting patterns and conducting falsification exercises, we gathered information on news coverage during the year subsequent to the referendum. With respect to the crime data, we used the entire period for which we had data (2009-2013) since our IV strategy is based on short-run deviations of criminality relative to its trend in the intermediate run.

Criminality – The exhaustive dataset on criminality, obtained from the Swiss Statistical Office, contains information on all crimes detected by the police in Switzerland between 2009 and 2013. The data were collected by local police services and include every case in which an individual was charged with infraction(s) according to the Penal Code. Remarkably, this information includes the nationality and residency status of both victims and perpetrators, as well as the place, date, and nature of the crime. We focus on the most violent (and newsworthy) infractions (i.e. homicide, murder, manslaughter, and infanticide, including attempts), which leaves us with a sample of 846 cases perpetrated by 1,200 individuals during the 2009-2013 period. With an average frequency of 14 cases and 20 perpetrators per month, such crimes are thus relatively infrequent in Switzerland – a feature that is likely to contribute to their newsworthiness. Sexual crimes are not included in our baseline analysis because they are far more common.[3] We nonetheless investigate robustness to including sexual crimes (in Online Appendix Section B3.1).

[3]Further details about the selection of crimes appear in Online Appendix Section B1.2. During 2009-2013 there were 4,502 perpetrators of rape and sexual acts with children.

Newspaper coverage of violent crime – Newspapers are still widely read in Switzerland; 85% of the respondents to the VOX survey reported that newspapers were a source of information for the Minaret ban referendum (compared to 77% in the case of TV, and 61% in the case of radio). Therefore, the coverage of violent crime in newspapers is likely to impact beliefs, attitudes, and consequently voting behavior. The sample of crime-related news is constructed based on articles published in 2009 and 2010 in 12 major Swiss newspapers (half of them printed in German- and the other half in French), which represent a 60.4% share of the market.[4] The crime news detection algorithm we employed uses a standard two-step procedure, as in Eisensee and Strömberg (2007). We initially chose a standard set of keywords to identify the articles and restricted the search window to between 2 days prior to the event and 10 days after it, with the goal of minimizing Type-II errors. Following this data scraping procedure, each article was read and cross-checked twice in order to ensure that the algorithm had correctly assigned every article to its relevant crime (with the aim of limiting Type-I errors). This made it possible to match 454 articles, which reported on crimes involving 138 perpetrators (out of a total of 507 perpetrators appearing in the crime data for the 2009-2010 period; for further details, see Online Appendix Section B1.3).

Religion and nationality – Neither police forces nor newspapers in Switzerland disclose information on a perpetrator's religion. During the 2009-2010 period, none of the 454 articles in our sample makes any mention of religious affiliation. Hence, readers must rely on indirect infor-

[4]The largest newspaper for which we do not have data is *Blick* (8.2% of the market, 3rd largest). The Blick archives were not available on Lexis/Nexis at the time of collecting data. We address this issue in Online Appendix B3.1.

mation, such as nationality or immigration status, in order to assess the relative criminality of a religious group. On the other hand, nationality is well-documented both in the crime data (76% of detected cases) and the news articles (48% of reported cases).

Voting outcomes and other data – Data on voting outcomes, including for the Minaret Ban, are collected by the Swiss Statistical Office. The municipality-level voting data provide information on the number of voters registered, total and valid ballots, and votes in favor. Demographics and other municipality-level information are presented in the Online Appendix Section B1.4.

3 Crime News Provision

This section examines the main determinants of crime news provision and in particular, whether newspapers over-report crimes perpetrated by immigrants. Besides the interest in the issue per se, the analysis grounds the first stage of our IV strategy.

3.1 Unconditional Evidence

Crime newsworthiness – In 2009 and 2010, 507 perpetrators of violent crimes were detected by the police forces; of those, 138 were mentioned in the news. Some newspapers covered crime more extensively, such as *20 Minuten DE-CH,* which reported 34 different perpetrators or *20 Minutes F-CH* which reported 19. These two newspapers are usually classified as tabloids. At the other end of the spectrum, *Le Temps*, a nationwide French-language general-audience daily, reported only 3.

Media bias – During the 2009-2010 period, 85 of the 235 foreign perpetrators in our dataset were mentioned in at least one newspaper as compared to 53 of the 272 Swiss perpetrators.[5] At the newspaper level j, these figures translate into an unconditional coverage probability of foreign (native) offenders equal to $\mathbb{P}_{\mathtt{F}j} = 0.031$ ($\mathbb{P}_{\mathtt{CH}j} = 0.017$). We define the reporting bias of newspaper j as $\mathtt{B}_j \equiv (\mathbb{P}_{\mathtt{F}j} - \mathbb{P}_{\mathtt{CH}j})/\mathbb{P}_{\mathtt{CH}j}$; which yields 82% across newspapers. We once again observe substantial heterogeneity across newspapers: while *20 Minutes F-CH* and *24Heures* have a reporting bias of 334% and 154% respectively, *Le Temps* and *Neue Zurcher Zeitung* have almost no reporting bias (16% each). The pre-referendum coverage probability of immigrant perpetrators is larger than the post- referendum coverage ($\mathbb{P}_{\mathtt{F}j} = 0.045$ and $\mathbb{P}_{\mathtt{F}j} = 0.020$), with a large jump 3 months prior to the vote ($\mathbb{P}_{\mathtt{F}j} = 0.079$). Similarly, the unconditional reporting bias reaches 248% just prior to the referendum, and drops to 160% following it.

3.2 Determinants of Crime News Provision

We now assess the main determinants of the probability that a given perpetrator i is mentioned in newspaper j using the raw data (i.e. detected crimes) and their news coverage. We estimate the following Linear Probability Model (LPM) on the full sample of 507 perpetrators $\times$ 12 newspapers over the 2009-2010 period[6]

$$\mathbb{P}(\mathtt{news}_{ij} = 1) = \rho \cdot \mathtt{foreign}_i + \alpha \cdot \mathtt{readershare}_{ij} + \beta \cdot \mathtt{newspaperHQ}_{ij} + \mathbf{X}_i'\boldsymbol{\gamma} + \mathbf{X}_j'\boldsymbol{\lambda} \quad (1)$$

[5]The crime propensity in the detection data is equal to 6.9 crimes per 100,000 inhabitants for immigrants and 2.2 per 100,000 inhabitants for natives.

[6]The Lexis/Nexis data for *St. Galler Tagblatt* is only available for 2010.

where $\texttt{news}_{ij}$ takes the value of 1 when perpetrator i is reported in newspaper j (and her nationality is mentioned), and 0 otherwise. Crimes reported without a nationality are coded as zero.

The main variable of interest is $\texttt{foreign}_i$, a dummy equal to 1 when perpetrator i is an immigrant and 0 otherwise. Its coefficient (ρ) captures whether newspapers are more likely to cover immigrant perpetrators than native ones, conditional on the other determinants of news coverage. We control for the readership share of newspaper j in the municipality where the crime committed by perpetrator i occurred ($\texttt{readershare}_{ij}$) since newspaper j is more likely to provide information on events in areas where a large share of its readership is located (Snyder and Strömberg, 2010). Arguably, even after controlling for the readership effect, some areas may still be more extensively covered than others, for reasons that primarily are related to the cost of journalistic investigation which is likely to increase with distance between the newspaper's newsroom and the location of the crime. We therefore control for this potential effect using the variable $\texttt{newspaperHQ}_{ij}$, a binary variable that takes the value 1 if newspaper j has its offices in the area where the crime of perpetrator i took place.[7] Note that we are also interested in its coefficient β, since our IV strategy in Section 4 relies on the proximity of a crime to the newspaper's offices. We control for a large array of fixed effects ($\mathbf{X}_i$ and $\mathbf{X}_j$): i) day-of-the-week and week-year in order to control for seasonality; ii) the article of the criminal code that was violated to account for severity and newsworthiness;

[7]What we refer to as headquarters throughout this paper are in fact editorial rooms. Some newspapers have editorial rooms in more than one municipality. The editing of newspapers takes place in large cities: Zurich (6 newspapers), Lausanne (5), Bern (3), Geneva (3), St. Gallen (2), Basel, Luzern, and Neuchâtel.

iii) the municipality to control for higher coverage in large cities; and (iv) the outlet capturing any time-invariant newspaper-specific characteristics, such as political orientation and readership composition. We further include perpetrator characteristics such as age, age squared, gender, relation to victim, and recidivism. Standard errors are clustered at the crime level.

Baseline results – Table 1 presents the results. The main coefficient of interest is positive and statistically significant (Column 1), implying that the coverage probability of crimes perpetrated by foreigners is roughly twice as large as the baseline probability. This result is robust to controlling for readership (Column 2). In line with the findings in Snyder and Strömberg (2010), the coverage probability rises with readership. The point-estimate of 0.24 implies that a 10-percentage point expansion in readership share doubles the baseline reporting probability. Interestingly, there is a higher degree of over-reporting of crimes that occur in areas where a newspaper head office is located (Column 3). This effect is precisely estimated even when controlling for readership, which is expected to be large in municipalities with a newspaper headquarter (Column 4). The point estimate is substantial (0.039) comparable to a 16-percentage point increase in readership share. This first-stage of our IV strategy in Section 4 is based on this finding.

Before/after the referendum – We then estimate a flexible version of Equation (1) that allows for a time-varying coefficient for $\texttt{foreign}_i$. The estimation results, presented in Figure 1, confirm the unconditional evidence, in that they show a large increase in the coverage probability of crimes perpetrated by immigrants just prior to the vote. The point estimate increases from 0.029 (during the period March 1 to May 31, 2009), to 0.037 (June 1 to August 31, 2009) and to 0.124 during the three months preceding the referendum (September 1 to November 29, 2009). Interestingly,

this differential likelihood collapses after the vote, implying that the communication strategy of the Swiss People's Party was highly effective. Nonetheless, the coefficients of the differential likelihood following the referendum remain jointly significant ($p - value = 0.081$).

Further results – In Online Appendix Section B2, we complement the analysis with two pieces of evidence. First, religious affiliation affects the likelihood of crime news coverage. Second, while all the newspapers are more inclined to report immigrant crimes, we observe substantial differences in the extent of media bias across newspapers, a feature that drives the identifying variations in Section 4.

4 News and Voting

We now estimate the impact of crime news coverage on the Minaret Ban vote. We start by sketching a conceptual framework and then turn to the empirical analysis.

4.1 Conceptual framework

We see two reasons why an intense pre-vote media coverage of immigrant criminality is likely to impact the minaret ban outcome. First, it makes the link between criminality and immigration more salient and the impact of priming on voting behavior is now well-documented (see our literature review). Second, voters may rationally respond to crime news when they hold the following two beliefs: (B1) the referendum outcome is likely to reduce future criminality by making Switzerland less Islam friendly and by ultimately changing the religious composition of the pool

of immigrants (see Section 2.1); (B2) Muslims are more violent than non-Muslims, a belief based on their reading of the news. For rational voters, interpreting crime news is challenging because, in Switzerland, police forces and newspapers do not communicate religious affiliations, but do report nationalities. Hence, readers' inference procedure is fundamentally based on the comparison of news coverage across nationalities that have different religious compositions (Muslim and non-Muslim). However, with crimes being infrequent and news coverage sparse, nationality-based inference is not precise given the modest size of foreign diaspora in Switzerland. In Couttenier et al. (2019) we show how statistical accuracy increases dramatically when readers pool all the news related to foreign nationalities together and compare the latter to news on natives. Pooling is also cognitively parsimonious from the reader's perspective as it drastically reduces the information set that she has to process.

4.2 Empirical Strategy

Our baseline econometric specification is

$$\overline{\texttt{YES}}_m = \alpha \cdot \texttt{CNE}_m + \bar{\mathbf{X}}_m' \beta + \varepsilon_m \tag{2}$$

where $\overline{\texttt{YES}}_m$ is the share of voters in favor of the minaret ban in municipality m. The main variable of interest, namely crime news exposure, is calculated as follows

$$\texttt{CNE}_m \equiv \sum_j s_m(j) \cdot \left(\frac{\#\texttt{news}_{\texttt{F}j}}{\texttt{pop}_{\texttt{F}}} - \frac{\#\texttt{news}_{\texttt{CH}j}}{\texttt{pop}_{\texttt{CH}}} \right) \tag{3}$$

$\texttt{CNE}_m$ averages over-reporting of crime committed by immigrants across newspapers (while controlling for population sizes at the national-level), using municipality-level market shares $s_m(j)$ as weights. Conceptually, $\texttt{CNE}_m$ captures, for a randomly selected voter in municipality m, the news-based inference of the crime differential between foreigners (F) and natives (CH). In our baseline specifications, the news-related elements of $\texttt{CNE}_m$ are measured over the pre-referendum period (January 1, 2009 to November 29, 2009). Shorter time frames are investigated in the sensitivity analysis. To circumvent potential endogeneity issues, market shares $s_m(j)$ are calculated using pre-sample shares over the 2006-2008 period.

We estimate Equation (2) for a cross-section of 1,980 municipalities in 2009. Standard errors are clustered at the commuting zone level; we report standard errors corrected for cross-sectional spatial correlation (Conley, 1999; Colella et al., 2019). Note that Italian- and Romansh-speaking municipalities are excluded from the sample since the dataset is restricted to German- and French-language newspapers.[8] Moreover, municipalities in districts where a newspaper's headquarter is located are excluded (159 municipalities in total) because: (i) we wish to exclude towns in which a single newspaper has a dominant position (Gentzkow et al., 2014) and (ii) we wish to exploit local crime in areas where newspapers have their head offices as an exogenous source of variation in news coverage.

Our main empirical challenge is a newspaper's tendency to publish information that confirms their readers' ideology and beliefs (Gentzkow and Shapiro, 2010). Demand-driven news provision

[8]The linguistic partition of Switzerland in 2000 was: German 74%, French 21%, Italian 4%, Romansh 0.6%.

implies that market shares of newspapers that over-report criminality by foreigners tend to be larger in municipalities with a positive political bias in favor of the minaret ban.

Control variables – First, we control for first-order co-determinants of $\texttt{CNE}_m$ and political preferences. We therefore include a measure of the district-level difference between realized immigrant and native crime propensities (referred to as the local crime propensity differential) $\texttt{CPD}_m \equiv \frac{\#\texttt{crime}_{\texttt{F}m}}{\texttt{pop}_{\texttt{F}m}} - \frac{\#\texttt{crime}_{\texttt{CH}m}}{\texttt{pop}_{\texttt{CH}m}}$. Indeed, criminality and readership are both spatially clustered, since individuals tend to read local newspapers and local newspapers tend to report on local crime. We include a measure of past anti-immigrant vote outcomes at the municipality level, constructed as the mean of the vote share in favor of anti-foreigner initiatives during the 2000-2008 period. The inclusion of past voting outcomes makes our econometric model akin to a first-difference specification, since we are correlating deviations from past anti-immigrant vote with the level of crime news exposure in the period preceding the referendum. We control for several municipality characteristics (listed in Table 2), and include a set of commuting zone fixed effects. Summary statistics are provided in Online Appendix Table B1.1.

Instrumental variables – We instrument the news provision component of $\texttt{CNE}_m$ in a 2SLS version of Equation (2). The key insight emerges from our previous finding that proximity to a newspaper's headquarter is a powerful predictor of a crime's news coverage. We therefore compute, for each newspaper j, the difference between realized immigrant and native crime propensities in the municipality where its headquarter is located, which we will refer to as the headquarter crime propensity differential. Formally $\texttt{CPD}_j^{\texttt{HQ}} \equiv \frac{\#\texttt{crime}_{\texttt{F}j}^{\texttt{HQ}}}{\texttt{pop}_{\texttt{F}}^{\texttt{HQ}}} - \frac{\#\texttt{crime}_{\texttt{CH}j}^{\texttt{HQ}}}{\texttt{pop}_{\texttt{CH}}^{\texttt{HQ}}}$. We then aggregate across

newspapers at the municipality-level to obtain a variable comparable to $\texttt{CNE}_m$

$$\texttt{HQC}_m \equiv \sum_j s_m(j) \cdot \texttt{CPD}_j^{\texttt{HQ}} \tag{4}$$

To be a valid instrument, headquarter crime exposure ($\texttt{HQC}_m$) must be orthogonal to the error term ε_m, after conditioning on the set of co-variates in Equation (2). In this regard, note that we control for the local crime propensity differential in order to factor in spatial clustering of criminality. However, conditional exogeneity of the instrument is still at risk since headquarter crime exposure may still correlate with some co-determinants of political preferences. For example, in the case of a metropolitan area where immigrants are discriminated against in the labor market, the negative attitude toward immigrants in the surrounding municipalities may simultaneously increase with immigrant criminality. To overcome this threat, we compute the deviation of the short-run headquarter crime propensity differential (pre-referendum period; 2009) and its long-run counterpart ($\overline{\texttt{CPD}_j^{\texttt{HQ}}}$, post-vote period; 2010-2013). Aggregating across newspapers, we obtain our IV which we denote as the deviation in headquarter crime exposure

$$\Delta\texttt{HQC}_m \equiv \sum_j s_m(j) \cdot \left(\texttt{CPD}_j^{\texttt{HQ}} - \overline{\texttt{CPD}_j^{\texttt{HQ}}}\right) \tag{5}$$

The rationale in exploiting short-run deviations is that they can be viewed as pure sampling errors. While both the short- and long-run crime propensities of immigrants may correlate with headquarter characteristics and political preferences, their deviation should not, namely $\mathbb{E}[(\texttt{CPD}_j^{\texttt{HQ}} - \overline{\texttt{CPD}_j^{\texttt{HQ}}})\varepsilon_j^{\texttt{HQ}}] = 0$. This assumption is tested on a set of observable characteristics for the sub-sample

of cities experiencing violent crimes during the 2009-2013 period. As Figure 2 shows, long-run and short-run foreigner criminality do indeed correlate with city characteristics while short-run deviations in criminality reassuringly do not.[9]

4.3 Results

Main results – Table 2 displays the baseline estimation results for Equation (2). Only the coefficients of the main variables of interest are reported, while crime and news variables are standardized in order to simplify interpretation. Columns (1) and (2) present the OLS estimates while Columns (3) to (5) correspond to our preferred specification, namely the 2SLS estimator with $\Delta\texttt{HQC}_m$ as the exogenous instrument.

We first estimate a parsimonious specification that does not include our two most important control variables, namely past anti-foreigner vote outcomes and local $\texttt{CPD}_m$ (Column 1). The two controls are included in Column (2). $\texttt{CNE}_m$ has a positive and statistically significant effect in both specifications, with the inclusion of controls leading to an improvement in precision. The effect is sizable: a standard deviation increase in $\texttt{CNE}_m$ translates into a 0.22 standard-deviation increase in vote in favor of the ban (Column 2). We interpret the positive coefficient of $\texttt{CPD}_m$ as direct evidence supporting the existence of *crime priming*. Past anti-immigration voting has a point estimate close to 1, illustrating the high level of persistence in anti-immigration attitudes. This confirms that the

[9]By adjusting for long-run crime, our approach is conceptually close to the *recentered instrument* proposed by Borusyak and Hull (2020). Instead of randomizing the date of an offense to calculate the *expected instrument* (expected CPD), we subtract the realized long-run CPD.

minaret ban referendum had a strong anti-immigrant component. Overall, the precision and sign of the estimated coefficients of the control variables are encouraging with respect to the quality of the data.

IV estimation results appear in Column (3) which presents the reduced-form estimates and Columns (4) and (5) which report the first- and second-stage regression results, respectively. The reduced-form estimates indicate that the instrument has a positive and significant effect on the pro-ban vote. In the first-stage estimation, the sign of the estimated coefficient of the instrument and the magnitude of the F-statistic confirm that the short-run deviation in headquarter criminality is a powerful predictor of crime news provision. The second stage coefficient of $\texttt{CNE}_m$, is precisely estimated and close to its OLS counterpart (Column 5, our preferred specification).

Quantification – We quantify the impact of crime news on the minaret ban vote, by assessing the counterfactual voting shares if newspapers had reported on immigrants' criminality in an unbiased way. We first compute the coverage probabilities in a world without bias by setting the coefficient of $\texttt{foreign}_i$ in Equation (1) to zero. We then sum these probabilities across perpetrator origin to obtain the counterfactual crime news reported in newspaper j for foreigners and natives. We subsequently calculate municipality-level crime news exposure according to Equation (3) and predict the counterfactual municipality-level vote outcome using the estimated coefficients of Column (5) in Table 2. The quantification indicates that in the absence of media bias, the pro-ban vote would have been reduced by 4.1 percentage points at the national level, from 57.6% to 53.5%.

Falsifications – In Table 3, we perform several falsification exercises based on the reduced-form specification of Table 2.[10] We first show that, as expected, our instrumental variable, i.e. headquarter crime differentials in 2009, fails to predict the outcome of (i) a vote that is unrelated to immigration and contemporaneous (September 2009) to the minaret ban referendum; and (ii) an immigration-related vote that took place two years earlier, namely the 2007 Federal election vote shares of parties that subsequently endorsed the ban (Columns 1 and 2).[11] We then falsify the timing by evaluating the impact of headquarter crime differentials observed *after* the referendum date on Minaret ban support. For the sake of comparison, Column (3) presents the non-falsified regression (crimes in the three months before referendum) while Column (4) displays the falsified one (crimes in the three months after referendum). As expected, the coefficient of interest is positive and precisely estimated in the former and loses statistical significance in the latter.

Last, we scrutinize the validity of our IV strategy (Columns 5 to 8). If the exclusion restriction holds, then the voting behavior of individuals should not be impacted by crimes that occur in the vicinity of the headquarters of newspapers they *do not* read. To implement this exercise, we rely on a slightly modified construction of the IV where, in equation (5), market share is replaced by one of its most powerful predictors, namely the inverse of spatial distance to headquarter. This solution addresses the problem that municipality-level market shares are by definition zero for newspapers not read. In Column (5), we report the reduced-form estimation for this modified version of the IV using only the newspapers that are read in the municipality (true); Column (6) only considers

[10] In Columns 1-4, (unreported) falsification results are similar when performed with 2SLS estimator – an option that is not feasible for Columns 5-8.

[11] The SVP, SD, EDU, PSL, and Lega parties that jointly received 31.4% of the vote.

instead newspapers that are not read (false). In the same vein, Columns (7) and (8) exploit linguistic differences in Switzerland, by using respectively newspapers written in the language spoken (true) and not spoken (false) in the municipality. In line with the exclusion restriction, all coefficients are statistically significant and positive in the true regressions (Columns 5 and 7) and far from conventional levels of significance in the false ones.

Sensitivity analysis – In the Online Appendix, we perform numerous sensitivity exercises. Our estimates are robust to (i) controlling for criminality in the municipalities where people work; (ii) alternative coding rules for news reports that do not specify nationality; (iii) expanding our estimation sample with the four largest Swiss newspapers, including The Blick, for which crime news are not available; (iv) adding sexual assaults; and (v) correcting for different thresholds of cross-sectional spatial correlation. We also consider alternative ways of constructing the instrumental variable: (a) the instrument in levels, while controlling for long-run headquarter crime; (b) the instrument in difference, where long-run crime is filtered out in a flexible way; (c) the instrument based on municipalities with large readerships; (d) instrumenting for market shares based on proximity to headquarters; (e) exploiting news pressure on the days that a crime is detected in the spirit of George and Waldfogel (2006); and (f) using the re-centered IV methodology of Borusyak and Hull (2020).

5 Conclusion

We examine the impact of news coverage of immigrant crime on voting patterns. Switzerland, a country characterized by direct democracy and cultural heterogeneity, is an ideal laboratory for

assessing the complex interaction between news provision and voting decisions. We scrutinize one of the most controversial referendums in recent years: the 2009 vote to ban the construction of minarets. We first document a large upward distortion in media reporting of immigrant crime over the pre-referendum period. We then estimate the effect of crime news on voting in a cross-section of municipalities and find a positive first-order effect. Quantifications show that had the newspapers reported in an unbiased way during the period preceding the vote, the vote in favor of the ban would have been reduced by 4 percentage points. A question left unaddressed relates to the complex interplay between media agenda-setting and the communication strategies of political parties. Understanding why and how the far-right populist rhetoric is relayed by the mass media is a fascinating and overlooked question that calls for further research.

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Exhibit 1

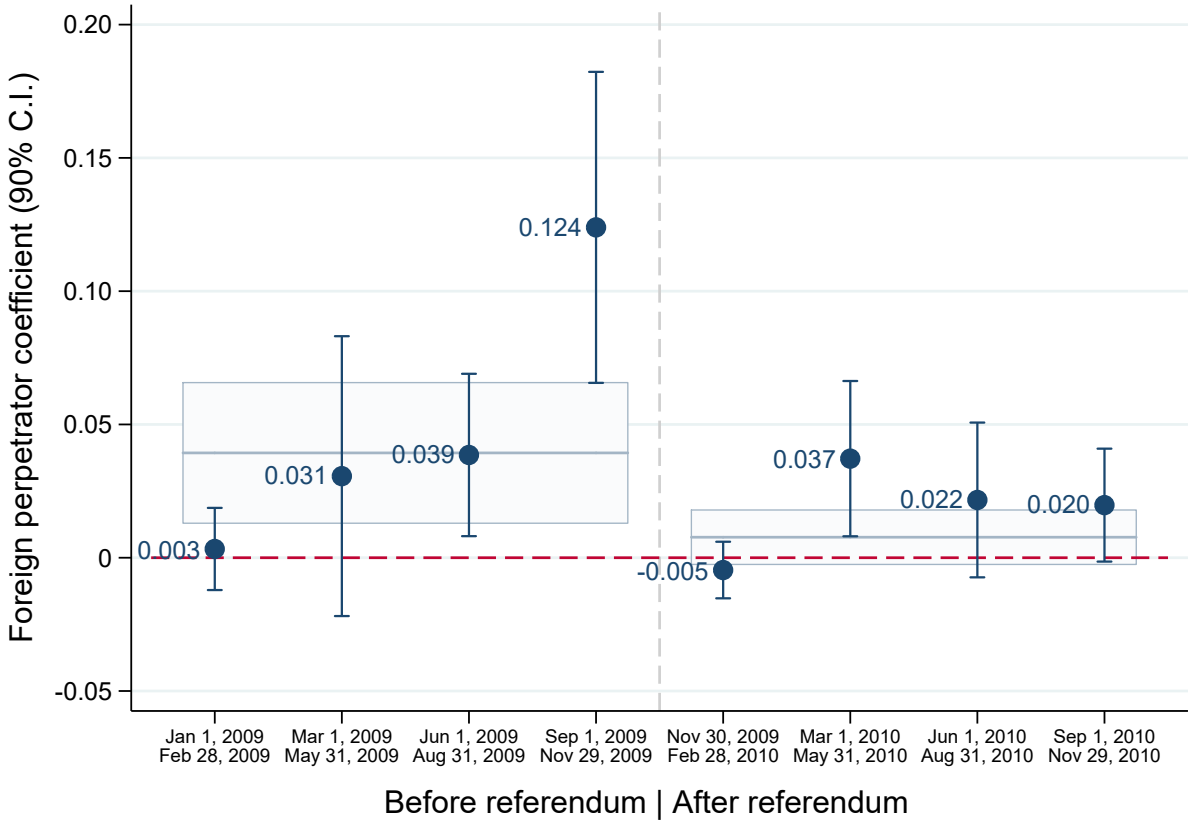
Table 1: Crime news provision

Dependent Variable	News coverage			
	(1)	(2)	(3)	(4)
Foreign perpetrator	0.023[a]	0.023[a]	0.023[a]	0.023[a]
	(0.008)	(0.008)	(0.008)	(0.008)
Readershare		0.240[a]		0.073
		(0.068)		(0.094)
Newspaper HQ area			0.049[a]	0.039[b]
			(0.012)	(0.018)
Observations	5847	5847	5847	5847
R^2	0.237	0.242	0.243	0.244
Sample mean (News coverage)	0.024	0.024	0.024	0.024

Notes: The unit of observation is a perpatetrator $\times$ newspaper dyad. Standard errors clustered at crime event level in parentheses. [c] significant at 10%; [b] significant at 5%; [a] significant at 1%. Linear probability model estimations. Individual characteristics of the perpetrator are included: age, age squared, gender, connection to the victim, and whether the perpetrator is a recidivist. Calendar day, year-week, criminal code article, municipality, and newspaper fixed effects are included.

Exhibit 2

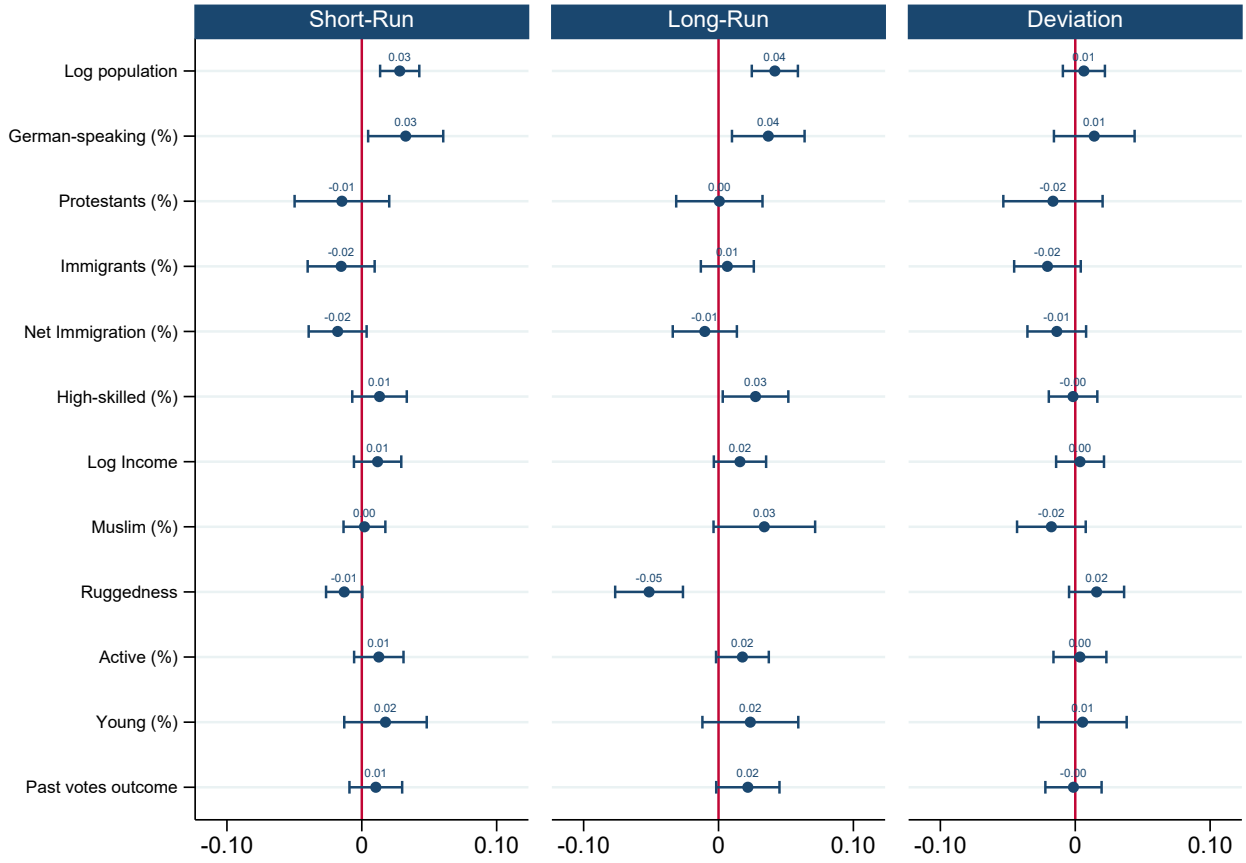
Figure 1: Crime news provision: Before/after the referendum



Notes: The unit of observation is a perpatetrator × newspaper dyad. Standard errors clustered at crime event level in parentheses. [c] significant at 10%; [b] significant at 5%; [a] significant at 1%. Linear probability model estimations. Individual characteristics of the perpetrator are included: age, age squared, gender, connection to the victim, and whether the perpetrator is a recidivist. Calendar day, year-week, crime subcategory, municipality, and newspaper fixed effects are included.

Exhibit 3

Figure 2: News and voting: Testing the identifying assumption



Notes: Correlation between observable municipality characteristics and crime propensity differential in municipalities with at least 1 violent crime in the 2009-2013 period (OLS estimations). The unit of observation is a municipality. Standard errors clustered at agglomeration level (90% confidence intervals reported). Left graph displays the correlation between observable municipality characteristics and short-run crime propensity ($\texttt{HQC}_m$); center graph displays the correlation with long-run crime propensity ($\overline{\texttt{HQC}}_m$); the right graph displays the correlation with the deviation between short- and long-run crime propensity differential ($\Delta\texttt{HQC}_m$). All variables are constructed using data for the year of the aggression with the exception of language, religion, and skills level that are constructed using data from 2000, and past voting outcomes that refer to the 2000-2008 period.

Exhibit 4

Table 2: News and voting: Main results

Specification	OLS		Reduced Form	2SLS 1st Stage	2SLS 2nd Stage
Dependent Variable	%Yes	%Yes	%Yes	CNE	%Yes
	(1)	(2)	(3)	(4)	(5)
Crime News Exposure (CNE)	2.458	1.717			2.474
	(0.840)^a	(0.366)^a			(0.874)^a
	[0.983]^b	[0.496]^a			[0.734]^a
Past Vote Outcomes		1.001	1.006	0.003	0.999
		(0.040)^a	(0.040)^a	(0.002)^c	(0.041)^a
		[0.034]^a	[0.035]^a	[0.002]^c	[0.034]^a
Local Crime Propensity Differential (CPD)		0.269	0.301	0.017	0.259
		(0.188)	(0.207)	(0.019)	(0.172)
		[0.127]^b	[0.142]^b	[0.019]	[0.121]^b
HQ Crime Prop. Diff. Deviation (ΔHQC)			1.077	0.436	
			(0.427)^b	(0.133)^a	
			[0.454]^b	[0.123]^a	
Observations	1980	1980	1980	1980	1980
Adjusted R^2	0.687	0.851	0.850	0.950	0.851
First-stage F-statistic (agglo cluster)				10.79	
First-stage F-statistic (spatial cluster)				12.47	

Notes: The unit of observation is a municipality. Standard errors clustered at agglomeration level in parentheses. Standard errors corrected for cross-sectional spatial correlation within 15km in brackets. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. Columns (1) and (2) show the OLS estimates. Columns (3) to (5) present the estimates of our preferred specification, the 2SLS estimation based on the ΔHQC_m instrument computed as the short-run Crime Propensity Differential in newspaper headquarter areas (HQC_m) in deviation from its long-run counterpart (HQC_m^{LR}). Municipality characteristics are included in all specifications: population size, share of German-speaking population, share of immigrants, net immigration, sectoral employment, average income, squared average income, elevation, ruggedness, share of active population, share of young population (15-35 population), share of Protestants, share of Muslims, property crimes, and total market shares of the eleven newspapers included in this analysis. Agglomeration fixed effects are included in all specifications.

Exhibit 5

Table 3: News and voting: Falsification exercises

Falsification	Outcome		Time frame		Readership		Language	
	Preceding referendum	2007 election	3 months before	3 months after	Read yes	Read no	Spoken yes	Spoken no
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔHQC (HQ CPD Deviation)	0.271	–0.453	1.974[a]	–0.631				
	(0.896)	(1.399)	(0.741)	(0.738)				
Distance based ΔHQC					0.401[a]	–0.213	0.860[c]	–1.064
					(0.143)	(0.137)	(0.458)	(1.272)
Past Votes Outcomes	–0.492[a]	0.742[a]	1.004[a]	1.005[a]	1.004[a]	1.002[a]	1.007[a]	1.005[a]
	(0.070)	(0.074)	(0.040)	(0.040)	(0.039)	(0.040)	(0.039)	(0.040)
CPD (Local Crime Prop. Diff.)	–0.059	–0.348	0.294	0.294	0.317	0.319	0.289	0.314
	(0.141)	(0.447)	(0.203)	(0.211)	(0.213)	(0.211)	(0.207)	(0.221)
Observations	1980	1969	1980	1980	1980	1980	1980	1980
Adjusted R^2	0.639	0.805	0.851	0.850	0.850	0.850	0.850	0.850

Notes: The unit of observation is a municipality. Standard errors clustered at agglomeration level in parentheses. [c] significant at 10%; [b] significant at 5%; [a] significant at 1%. Column (1) shows the reduced-form estimates of an estimation where the outcome is the vote share in favor of the *Abolition of the general popular initiative* that took place in the referendum preceding the Minaret Ban vote (Sep 27, 2009). Column (2) shows the reduced-form estimation when using the 2007 federal election vote share of parties that recommended voting in favor of the Minaret Ban as the outcome. Columns (3) and (4) present instrument falsifications by estimating the baseline only focusing on the 3 months preceding the referendum (true) and the three months after (false). Columns (5) to (8) present the estimates of the readership and language reduced-form falsifications. In Column (5) we replicate the reduced-form estimation of Table 2 focusing on the newspapers that are read in the municipality; in Column (6) we keep only newspapers not read in a municipality. In Column (7) we only keep outlets edited in the language spoken in the municipality; in Column (8) we focus on newspapers that are written in a language not spoken in the municipality. Since the instrument takes the value 0 once a newspaper is not read, we also instrument for market shares by weighting each outlet by the relative distance between the (voting) municipality and the nearest headquarter municipality of that newspaper. All regressions include the full vector of controls of Table 2.

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