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Working Paper

CEPII

The Usual Suspects: Offender Origin, Media Reporting and Natives' Attitudes Towards Immigration

Sekou Keita, Thomas Renault & Jérôme Valette

Highlights

- How the systematic disclosure of criminals' origins in the press affects natives' attitudes towards immigration?
- Systematically mentioning the origins of criminals increases the relative salience of natives' criminality and reduces natives' concerns about immigration.
- It allows to break the implicit link between immigration and crime.

RESEARCH AND EXPERTISE
ON THE WORLD ECONOMY

Abstract

This paper analyses whether the systematic disclosure of criminals' origins in the press affects natives' attitudes towards immigration. It takes advantage of the unilateral change in reporting policy announced by the German newspaper Sächsische Zeitung in July, 2016. Combining individual-level panel data from the German Socio-Economic Panel from 2014 to 2018 with 402,819 crime-related articles in German newspapers and those newspapers' market shares, we find that systematically mentioning the origins of criminals increases the relative salience of natives' criminality and reduces natives' concerns about immigration, breaking the implicit link between immigration and crime.

Keywords

Immigration, Crime, Media Bias.

JEL

F22, K42, L82.

Working Paper

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RESEARCH AND EXPERTISE
ON THE WORLD ECONOMY

The Usual Suspects: Offender Origin, Media Reporting and Natives' Attitudes Towards Immigration.*

Sekou Keita[†], Thomas Renault[‡] & Jérôme Valette[§]

On New Year's Eve, two girls were raped in Weil am Rhein. The suspected perpetrators were arrested. In the Stuttgarter Zeitung newspaper, they are named "three youths and a man". SWR, on the other hand, speaks of "four Syrians between 14 and 21 years of age". [...] Why did the Stuttgarter Zeitung editorial staff refrain from informing the media that the suspects are young men with Syrian citizenship? Frank Buchmeier, Stuttgarter Zeitung, January 9, 2016.

1 Introduction

On New Year's Eve 2015-2016, hundreds of sexual assaults against women occurred in Germany, mainly in Cologne, a city located in the West. In their initial reports, most newspapers followed the guidelines provided by the German press council (Article 12.1 of the German press code) and did not disclose the origins of the suspects. Over the following days, witnesses identified men of "North

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African" and "Arab" appearance, which was later confirmed by a police press release revealing that most offenders were asylum seekers. This voluntary omission by the press led to widespread accusations of a cover-up on social media. It revived the pre-existing debate on whether journalists should disclose criminals' origins when reporting crime events. A few months later, in July 2016, the Saxony-based regional newspaper Sächsische Zeitung officially announced a change in its rules on crime reporting.[1] Specifically, it stated that it would henceforth always disclose the origins of criminals irrespective of whether the person was a foreigner or German. The editor argued that the aim of the German press code of protecting minorities from stigmatization would be better served by *not* following Article 12.1. Despite the importance of the question of the most appropriate crime reporting policy, existing empirical evidence remains insufficient to inform the debate. This paper exploits the unilateral shift in reporting policy by the Sächsische Zeitung as a natural experiment to analyze the causal impact of crime reporting policies on natives' attitudes towards immigration. The regional distribution of the Sächsische Zeitung implies that its market share in the eastern part of the state of Saxony is large enough to have a substantial local impact and small enough to rule out concerns regarding spillovers to other states, which makes it an ideal natural experiment to answer this research question.

Using data collected on 402,819 crime-related articles published between January 2014 and December 2018 in 25 German newspapers, we first provide empirical evidence that this unexpected shift in reporting policy resulted in a positive differential in the disclosure of offenders' origins between the Sächsische Zeitung and other newspapers. We find this differential to be approximately eight percentage points after July 2016 and mainly driven by an increased propensity to disclose the origin of German offenders. We also provide evidence that there were no other changes, apart from the disclosure of the origins of offenders, in the reporting of the Sächsische Zeitung compared with that of other German newspapers. Specifically, we find that the Sächsische Zeitung did not change the share of crime-related or immigration-related articles in its total number of articles published each month, its propensity to report about certain types of crimes, nor the tone employed in crime articles. We also show that the trend in the market share of the Sächsische Zeitung across locations did not change significantly after July 2016 from that in the

[1] The Sächsische Zeitung is a regional German daily newspaper published in Dresden, the capital city of the federal state of Saxony. In 2014, it ranked 12th in terms of its distribution among German newspapers. The newspaper was founded in 1946 and privatized in 1991. It circulates mainly in western Saxony, where its market share is around ten times that of its main competitor, the *Dresdner Neueste Nachrichten*; outside this area, sales are close to zero (see Section 4).

pre-treatment period. Second, we estimate how a reporting policy that systematically discloses the origins of criminals impacts natives' concerns about immigration measured using individual-level survey panel data from the German Socio-Economic Panel (SOEP). Unlike other contributions in the literature, this study does not focus on newspapers' propensity to report crime by natives and immigrants but rather on how to handle sensitive information, such as the origins of offenders. Moreover, this radical local shift in crime reporting policy allows us to overcome challenges associated with estimating the relationship between media coverage and readers' beliefs, such as reverse causality that arises when newspapers adjust their reporting to match their readers' views.

The empirical analysis combines two complementary approaches that exploit market share data at the local level for all major newspapers in Germany and thus rely on similar identifying assumptions and variability. The first identification strategy is a reduced-form analysis with a difference-in-differences estimator that estimates the extent to which natives' attitudes changed after July 2016 in the distribution area of the Sächsische Zeitung compared to other localities in Germany. The treatment intensity corresponds to the 2014 share of Sächsische Zeitung sales in the total number of newspaper sales for each German district. The second identification strategy relies on IV-2SLS estimates. We apply natural language processing methods to a collection of crime-related articles, which allows us to compute the monthly share of articles disclosing the origin of the offender in the most violent crimes. We instrument this variable with the shift in reporting policy implemented by the Sächsische Zeitung in July 2016. This complementary approach can therefore be considered a more direct empirical test, as it precisely captures the overall extent to which each native is exposed to the disclosure of the origins of criminals. The reduced-form analysis has the advantage that it does neither rely on text analysis to identify whether the origin of the offender is revealed in the article text nor on the reduced sample of 25 newspapers exploited in the 2SLS analysis. Thus it is free from any omissions or errors in the detection of perpetrators' origins.[2] In all specifications, estimates include locality fixed effects and regional fixed effects interacted with year-month fixed effects, which control for any time-varying confounders at the regional level such as structural differences in the level of unemployment or the share of foreign-born in the total resident population, as well as time-varying district-year and time-varying individual controls. This greatly reduces identification concerns related to any change that would have been correlated with both the spatial distribution of the Sächsische Zeitung and natives' attitudes towards immi-

[2]We provide additional robustness checks in Section 2 showing that the measurement error in the text analysis employed in the second identification strategy is rather small.

gration. Additional estimates also provide evidence that our main conclusions remain unchanged when we include individual fixed effects, and further event analysis estimates support the absence of a pro-immigration trend in the area of distribution for the Säschische Zeitung in the pre-treatment period.

Overall, the results indicate that systematically disclosing the origins of criminals reduces natives' concerns about immigration. This effect is driven by a disproportionate increase in the disclosure of the origins of native offenders after July 2016. Following DellaVigna and Kaplan (2007) and DellaVigna and Gentzkow (2010), the persuasion rate of this policy is found to be approximately 2.60%.[3] We interpret these findings within the framework of a simple Bayesian model where individuals do not observe crime rates directly but rely on the partial information relayed by the media to form beliefs on foreign and native crime rates. Several studies have shown that media bias towards an over-reporting of foreign crime compared to that of natives could lead to an overestimation in the population of the link between crime and immigration (See Bianchi et al., 2012a; Fasani et al., 2019; Couttenier et al., 2021; Alesina and Tabellini, 2022, among others).[4] In such a model, increasing the salience of native criminality, through a disproportionate increase in the share of articles reporting crimes by natives, induces therefore individuals to revise downwards their beliefs about the crime rates of foreign-born individuals.[5] Since immigration and crime are two first-order issues that are often considered jointly in the reader's mind (Card et al., 2012; Fitzgerald et al., 2012), lower perceived criminality by foreign-born individuals translates into reduced concerns about immigration. Thus, eliminating the subjective judgement made by the journalist regarding whether to conceal perpetrators' origins should reconcile perceptions of the differences in crime rates between natives and foreign-born individuals, implied by newspapers reporting, with the corresponding differences in crime rates observed in official statistics.[6] This interpretation of the results is reinforced by additional estimates that specifically focus on concerns about crime. Following the increasing priming regarding native criminality in the region of distribution for the Sächsische Zeitung, we find that native respondents uncouple the two issues of crime and immi-

[3] As a comparison, Djourelova (2022) estimates a persuasion rate between the 1.9 and 4.4 percent for a comparable policy.

[4] See additional descriptive evidence on media bias in Subsection 3.2.

[5] Couttenier et al. (2021) discuss such a model in detail. They also provide evidence that a simple comparison of the crime rates of foreign-born individuals (pooling all nationalities) with natives in newspapers produces sufficient statistical power to influence natives' attitudes even when crimes and their reporting in the press are sparse.

[6] Our analysis of newspaper content suggests that the share of newspaper articles revealing that a suspect has German origins is much lower than the actual share of Germans suspects in all violent crime that is reported in official crime statistics.

gration (i.e., they blame immigrants less) and therefore become more likely to identify crime as a central issue in itself. Consistent with the Bayesian model described above, a heterogeneity analysis shows that the least informed individuals, namely, young and low-skilled natives, are the most likely to update their concerns following the policy change. Our results can also be interpreted through the lens of models of stereotypes. For instance, Bordalo et al. (2016) provide a theoretical framework based on a social cognition approach that rationalizes how stereotypes can emerge from characteristics in which differences are observed between two population groups. Stereotypes arise from overweighting the characteristic that most distinguishes a specific group (e.g., immigrants or foreign-born individuals) from a reference group (e.g., natives) even if the relevant characteristic relates only to a small fraction of the group, as is typically the case with criminal behaviors. Importantly, stereotypes can change as soon as the perceived distribution of the relevant characteristic in the reference group changes. In the context of stereotypes involving foreigners' criminality, the model predicts that increasing the salience of native criminality would shift the perceived distribution of criminals among natives. As a consequence, criminality should become less distinctive as a characteristic of foreign-born individuals, which could, in turn, improve attitudes towards the latter. Another potential interpretation of our results is that in the absence of information on the offender's origin, native readers are likely to associate foreign-born individuals with each reported crime.[7] Indeed, psychological studies report that majority groups are more likely to associate violent crime with minorities and be more subject to misidentification of criminal suspects as being members of other races (Gordon et al., 1996; Oliver and Fonash, 2002; Hammond-Watson and Hamm Baugh, 2018).[8] Thus, systematically reporting that most crimes are committed by native offenders should reduce the differential in crime rates between natives and foreign-born individuals that natives infer from reported crimes. This echoes recent papers by Alesina et al. (2022), Barrera et al. (2020) and Grigorieff et al. (2020), which provide evidence that information may reduce anti-foreigner attitudes.

This paper contributes to several strands of the literature. *First*, it adds to the literature on the determinants of natives' attitudes towards immigration by highlighting the role of media in shaping

[7] One can regard this interpretation of the results as being closely related to models of statistical discrimination. See Altonji and Pierret (2001); Laouénan and Rathelot (2022), among others, for evidence of reduced discrimination in the presence of increased information.

[8] For instance, Eurobarometer surveys show that crime has been among the top five concerns of European citizens in recent years (Dustmann and Fasani, 2016), which is at odds with the relatively low crime rates measured. Moreover, several studies have shown that immigration is often associated with an increased fear of crime even in the absence of increased victimization (see, Nunziata, 2015, among others).

how people perceive immigration and foreign-born individuals in destination countries (Boomgaarden and Vliegenthart, 2009; De Philippis, 2009; Héricourt and Spielvogel, 2014; de Coulon et al., 2016).[9] Our paper contributes to this literature by showing how a local newspaper's reporting policy on immigration-related issues may affect natives' attitudes towards immigration and foreign-born individuals. We change the focus from the propensity to report on crime to the more subtle question of what information to reveal. In this way, the closest paper to our analysis is Couttenier et al. (2021), who study how media coverage on immigrant criminality in 1,980 Swiss municipalities influenced electoral outcomes in the November 2009 referendum on the "minaret ban". The authors combine comprehensive data on criminality from the Swiss Statistical Office with media coverage on those crimes from 12 newspapers. They compute a pre-vote media bias in the coverage of migrant criminality between newspapers and across municipalities in 2009. Focusing on the most violent crimes, they find that an increase in the differential of crime reporting between foreign and native offenders increased votes in favor of the minaret ban. They estimate that in the absence of media bias, the pro-ban vote would have been 4.1 percentage points lower at the national level. In contrast to Couttenier et al. (2021), our paper does not focus on the propensity of newspapers to report immigrant criminality. Instead, our focus is on how newspapers report on criminality, namely whether they disclose offenders' origins. Another closely related contribution is Djourelova (2022), who also investigates the impact of a change in media reporting related to immigration. The empirical strategy in Djourelova (2022) exploits a natural experiment provided by the exogenous ban of the term "illegal immigrant" from US newswire agency articles (Associated Press) in April 2013. Relying on the propensity of AP-subscribing newspapers to report articles from the agency the year before the ban, she finds that individuals more exposed to newspapers that rely more on AP articles are less likely to support restrictive immigration policies after the ban. The estimated persuasion rate of the policy is between 1.9 and 4.4%. Our paper shares with Djourelova (2022) the empirical strategy of using a radical change in reporting rules and the uneven distribution of newspapers across the country to investigate how media reporting rules influence natives' attitudes towards immigrants. While Djourelova (2022) investigates the effect of media slant, holding the information content of articles constant, the focus of our study is on the effect of information

[9] See Meltzer et al. (2017) for a literature review on the link between media and attitudes towards immigration in communication and media research. Additionally, see Scheve and Slaughter (2001); Mayda (2006); Dustmann and Preston (2007); Facchini and Mayda (2009, 2012); Hainmueller et al. (2015); Barone et al. (2016); Facchini et al. (2017); Brunner and Kuhn (2018); Benesch et al. (2019) for papers on alternative determinants, other than media, of natives' attitudes towards immigration.

provided to the reader in newspaper articles addressing a highly contentious topic, namely, crime. Moreover, while the change in the reporting policy is national by design in Djourelova (2022), such that treatment intensity depends on pretreatment behavior, the treated area in our study is limited and therefore clearly defined due to the local nature of the newspaper that implemented the policy change.[10]

Second, this paper contributes to the fast-growing literature analyzing the overall role of media in shaping economic and political behavior. Many papers notably investigate the impact of media on electoral outcomes using exogenous variations in media access and/or penetration (See, among others, Gentzkow, 2006; DellaVigna and Kaplan, 2007; Gerber et al., 2009; Gentzkow and Shapiro, 2010; Snyder Jr and Strömberg, 2010; Gentzkow et al., 2011; Enikolopov et al., 2011; Drago et al., 2014; Barone et al., 2015; Puglisi and Snyder Jr, 2015; Durante et al., 2019). In this way, Mastrorocco and Minale (2018), in the context of Italy, show that exposure to specific channels with disproportional coverage of crime events strongly affects individuals' perceptions of crime with no significant effect on concerns about immigration. Our paper suggests that the way in which journalists handle sensitive information on immigration-related issues such as crime may be a channel through which the effect of media on electoral outcomes is transmitted. In fact, additional results from our analysis suggest that the reduction in concerns about immigration is also reflected in reduced support for the far-right party AfD, which is characterized by a strong anti-immigration agenda.[11]

The remainder of this paper is organized as follows. Section 2 first describes the data that we collected and used in our empirical analysis, while Section 3 describes our natural experiment and reports the corresponding empirical evidence. Then, Section 4 presents our empirical strategy and main results. Finally, Section 5 offers some concluding remarks.

[10] The present work is also related to the economic literature studying the links between immigration and crime (See, among others, Moehling and Piehl, 2009; Bianchi et al., 2012b; Bell et al., 2013; Chalfin, 2015; Couttenier et al., 2019; Piopiunik and Ruhose, 2017; Amuedo-Dorantes et al., 2020; Ozden et al., 2018; Fasani et al., 2019; Alesina and Tabellini, 2022) focusing on their joint impact on natives through media reporting.

[11] A related literature also studies the impact of immigration on electoral outcomes (Gerdes and Wadensjö, 2010; Otto and Steinhardt, 2014; Halla et al., 2017; Harmon, 2018; Mayda et al., 2022; Edo et al., 2019; Dustmann et al., 2018; Tabellini, 2020; Steinmayr, 2021; Bargain et al., 2022). Most of these papers show that immigration is often associated with an increase in the share of votes for far-right parties. Here, again, our paper suggests that media reporting on immigration may contribute to this relationship by impacting anti-foreigner attitudes.

2 Data

This section describes the data used in the empirical analysis. It details the collection and cleaning of the German crime-related articles in Subsection 2.1 and of the data on natives' attitudes towards immigration from the German SOEP in Subsection 2.2.

2.1 Media reporting on criminality

Table 1 – List of Newspapers

Newspaper	Editorial Board	Region	Subscription	Nb. Prints	Nb. Articles	Share
Bild	Berlin	Berlin	National	2,205,271	4,386	1.09
Berliner Kurier	Berlin	Berlin	Regional	141,722	4,964	1.23
Der Tagesspiegel	Berlin	Berlin	National	110,429	6,682	1.66
Die Welt	Berlin	Berlin	National	202,790	7,627	1.89
Express	Cologne	North Rhine-Westphalia	Regional	132,836	11,558	2.87
Hamburger Abendblatt	Hamburg	Hamburg	Regional	217,566	13,629	3.38
Hamburger Morgenpost	Hamburg	Hamburg	Regional	108,150	2,458	0.61
Hannoversche Allgemeine Zeitung	Hannover	Lower Saxony	Regional	198,365	10,535	2.62
Kieler Nachrichten	Kiel	Schleswig-Holstein	Regional	97,777	6,912	1.72
Kölner Stadt-Anzeiger	Cologne	North Rhine-Westphalia	Regional	273,382	10,633	2.64
Leipziger Volkszeitung	Leipzig	Saxony	Regional	198,882	12,909	3.20
Märkische Allgemeine Zeitung	Potsdam	Brandenburg	Regional	126,682	35,106	8.72
Mitteldeutsche Zeitung	Halle	Saxony-Anhalt	Regional	191,507	16,438	4.08
Nürnberger Nachrichten	Nuremberg	Bavaria	Regional	449,924	7,312	1.82
Ostsee-Zeitung	Rostock	Mecklenburg-Western Pomerania	Regional	159,364	16,994	4.22
Passauer Neue Presse	Passau	Bavaria	Regional	142,824	2,648	0.66
Rheinische Post	Dusseldorf	North Rhine-Westphalia	Regional	323,432	64,383	15.98
Sächsische Zeitung	Dresden	Saxony	Regional	238,977	35,865	8.90
Stuttgarter Zeitung	Stuttgart	Baden-Württemberg	Regional	197,645	29,426	7.31
Süddeutsche Zeitung	Munich	Bavaria	National	477,836	28,313	7.03
Südwest Presse	Ulm	Baden-Württemberg	Regional	291,917	8,631	2.14
Taz - die tageszeitung	Berlin	Berlin	Regional	53,812	5,652	1.40
Thüringer Allgemeine	Erfurt	Thuringia	Regional	272,508	28,786	7.15
Trierischer Volksfreund	Trier	Rhineland-Palatinate	Regional	89,081	18,709	4.64
Wiesbadener Kurier	Wiesbaden	Hesse	Regional	65,915	12,263	3.04
				Total:	402,819	100%

Notes: Editorial board is the city localization of the newspaper's headquarters. Subscription reports whether the newspaper is disseminated at the regional or national level. The number of prints is taken from the 2014 edition of the distribution data of the German daily press, which were collected in the week from November 4 to 10, 2013 by the Information Community for the Assessment of the Circulation of Media (IVW). Nb. Articles and Share are the total number and the share of articles of each newspaper in the baseline sample, respectively. Source: Authors' elaboration on Dow Jones Factiva archives.

This paper takes advantage of the Dow Jones Factiva archives to collect 545,347 articles published between January 1, 2014, and December 31, 2018, by regional and national German newspapers.[12] Factiva classifies all these articles as related to crime and/or legal action. They belong

[12]We restrict the analysis to articles in German since crime news in a foreign language is less likely to shape natives' overall attitudes in the country.

to 25 widely circulated daily regional and national printed German newspapers for which the Dow Jones Factiva archives ensure full coverage over time (see Table 1 for a list of the newspapers included in the analysis). The choice of newspapers is largely determined by their availability in the Dow Jones Factiva archives and our commitment to covering the entire German population. Unfortunately, we could not collect articles in regional newspapers for Saarland or Bremen, the two regions with the lowest populations.

We first exclude irrelevant articles that are not related to real crimes from the sample. To do so, we rely on a simple text analysis based on lexicons (lists of words).[13] This allows us to identify and drop irrelevant articles in the broad crime and/or legal action categories, such as articles describing crimes in books, movies, theater or arts and entertainment (12.24 % of the initial sample). Then, as in Couttenier et al. (2021), we isolate crimes associated with violence that are more likely to change interpersonal attitudes towards foreign-born individuals. Specifically, we exclude articles reporting traffic violations, environmental crime or financial fraud. The classification of crimes that we use follows the International Classification of Crime for Statistical Purpose (ICCS) provided by the United Nations Office on Drugs and Crime, except for criminal acts against property, which we aggregate into a single category irrespective of whether the crimes involve violence. The crime types are terrorism; murder; assault and threats; sexual violence; theft, burglary, robbery, and vandalism; drugs; human trafficking; and illegal immigration (see Table A2 in the appendix for a description of the categories). We attribute each article to one or several of these seven mutually non-exclusive categories using the lexicons. Items that do not belong to any of the seven categories (77,758 articles, representing 14.26% of the observations) are removed from the initial sample.[14] We ultimately obtain a sample of 402,819 articles. It is worth noting that this procedure for collecting a large sample of crime-related articles implies that not all articles necessarily refer to a specific event but may address criminality in general, possibly referring to the specific issue of the perpetrators' origins. In addition, the same event can be reported several times in the same or in different newspapers, reflecting its importance in the news.

[13] All the lexicons used in this paper were developed by the authors and are available upon request.

[14] The Dow Jones Factiva archives have a classification that automatically indexes each article using a collection of thousands of correctly indexed documents assembled by Dow Jones editors. We do not use this classification since we have no reliable information on how articles are classified. However, we can compare the classification obtained with our lexicons with the classification obtained from the Dow Jones Factiva archives. As reported in Appendix Table A1, on average, in 92.13% of the cases, an article identified by Factiva as belonging to a given category is also identified by our classification as belonging to the same category. Conversely, in 82.60% of the cases, an article that Factiva does not identify as belonging to a given category is also not identified by our classification as belonging to the same category.

The analysis uses a bag-of-words model to detect whether the reporter disclosed the offender's origin. We first convert each article into a vector of tokens (words) using the Natural Language Toolkit package (NLTK) in *Python*. Then, we count the number of words from three different lexicons in the article content and title. The first lexicon includes a sample of nationalities whose variations in spelling exclude words related to German citizens.[15] This lexicon includes a total of 1,921 distinct words. For instance, French offenders are identified with the following words: *"france, franzose, franzosen, franzosisch, franzosische, franzosischem, franzosischen, franzosischer, franzosisches"*.[16] The second lexicon allows us to identify German offenders with the six following words: *"deutsch, deutsche, deutschem, deutschen, deutscher or deutsches"*. Finally, the third lexicon includes words that could be associated with foreign-born individuals in Germany with no precisely identified origin or nationality, such as "*gefluchteten* (refugees), *asylbewerber* (asylum seeker), *zuwanderer* (immigrant) or *islamistisch* (islamist)" for instance. This last lexicon includes a total of 98 words.[17]

Section H4 in Appendix H reports three examples of article classification based on words detected by the lexicons. These examples illustrate both the strengths and weaknesses of the lexicon-based approach. The main advantage is the reliable and systematic classification of vast amounts of text, which reduces the subjectivity and risks associated with manual classifications.[18] Simultaneously, the lexicons cannot detect subtle uses of language that would implicitly reveal an origin. In fact, for non-white Germans with an immigration background (naturalized or second-generation migrants), the classification would not detect any origin if the latter is not explicitly revealed but only suggested by a physical description of the offender. Thus, there is a clear trade-off between the lexicons' comprehensiveness and the risk of detecting false positives. To understand the importance of the measurement error in the analysis, Section H5 in Appendix H reports a comparison between the classification of articles and the detection of origins provided by the algorithm and a manual check of more than 900 articles. We find that 60.71% of articles classified as relevant by the lexicon approach, namely, belonging to one of the seven categories of crimes, are true positives and 16.84% of classified articles are true negatives. This overlap corresponds to an

[15] Instead of using spelling variation, we could have used stem words. However, the identification of the offender's origin was less precise when using the Snowball stemmers developed by Porter (2001).

[16] We removed all capital letters and special characters from the words to increase the chances of detecting relevant words.

[17] Further analyses also consider two additional lexicons that more precisely capture the top 10 nationalities of immigrants and of refugees in the German population.

[18] It is worth noting that another advantage of this simple text analysis is its transparency and exact replicability.

overall accuracy rate of 78%. Conditional on classifying an article as relevant, the lexicon correctly detects whether the offender's origin is mentioned in 72% of cases. When the lexicon approach does not detect any origin being mentioned, it is correct in 98% of cases.

Overall, and despite the aforementioned reassurances, we acknowledge that the simple detection algorithm may fail by not detecting some origins or by wrongly attributing origins to non-offenders mentioned in the article, such as victims or witnesses.[19] However, we do not view this as a significant issue since the goal of this paper is not to measure the share of articles reporting offenders' origin with great precision. Instead, it aims to quantify the change in reporting policy implemented by the Sächsische Zeitung. Although the reporting policy measure is likely to be noisy, it is doubtful that the measurement errors would be systematically correlated with the treatment. Ultimately, they would imply less precision in the estimates but no systematic bias in the empirical analysis.

2.2 Individual attitudes towards immigration

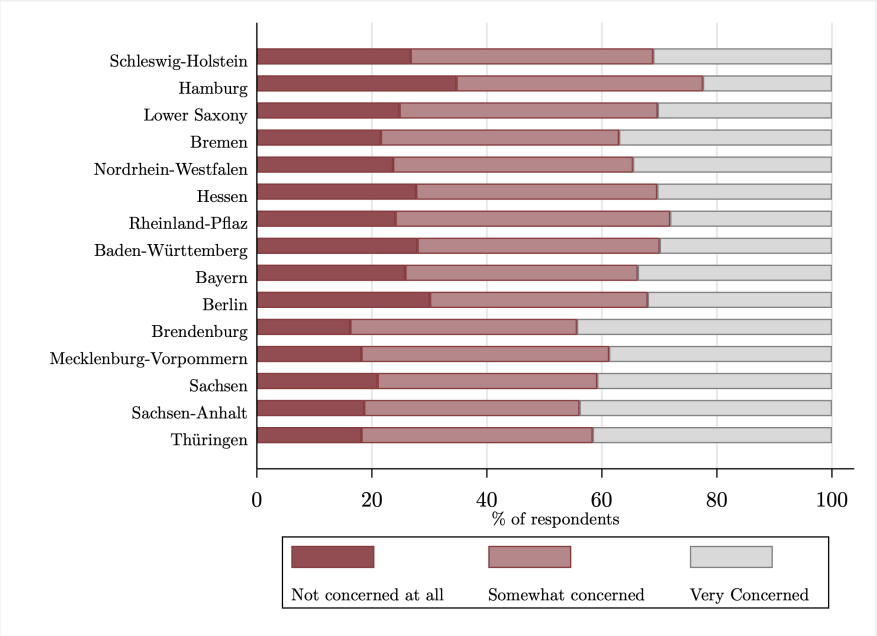
We employ the German SOEP, which includes a question on concerns about immigration. The SOEP is an ongoing representative longitudinal survey that collects information on private households in Germany since 1984. We restrict the analysis to the 2014-2018 period for which we collected data on media reporting. The sample includes only German citizens aged 18 and older. In each of the surveys, individuals are asked to give their opinions on various topics with the following question: *"How concerned are you about the following issues?"*.[20] Individuals may choose between three different answers, namely, "not concerned at all", "somewhat concerned", and "very concerned". We use the month of the interview and the place of residence of the respondent to link individual perceptions on these topics with media reporting on criminality across various national and regional newspapers. To do so, we consider the area of distribution of each newspaper across districts in Germany that is provided by the Information Community for the Assessment of the Circulation of Media (IVW).[21] On average, we find that 41.61% of individuals report that they are very concerned about immigration during the analysis period. The rest of the population falls

[19] The empirical analysis in Section 3 provides reassuring evidence that the detection of the origins of offenders and suspects is robust to a more complex text analysis that isolates sentences in which we explicitly identify keywords associated with victims (*"opfer"*) and perpetrators (*"täter"*).

[20] Since the original question is formulated in German, we report our preferred translation. Alternatively, the question could be translated as *"How worried are you about the following issues?"*, in which case the response items become "Very worried", "Somewhat worried" or "Not worried at all".

[21] See Section 4 for more details on the newspaper distribution data.

Figure 1 – Natives' Attitudes Towards Immigration in German Regions



Notes: This graph depicts the percentage of individuals answering "Not concerned at all", "Somewhat concerned" or "Very concerned" to the question "How concerned are you about immigration?" across German regions over the period 2014-2018. Observations are weighted using individual longitudinal weights provided by the SOEP.
Source: Authors' elaboration on SOEP.

into two categories: those who are somewhat concerned (34.17%) and those who report not being concerned at all (24.22%). Regarding regional differences, the percentage of respondents for each category is reported in Figure 1. Residents from Saxony, the region where the Sächsische Zeitung is distributed, seem to be more concerned about immigration than the rest of the German population, with 40.96% of individuals being very concerned about the issue.

Regrettably, the SOEP does not include other questions on natives' attitudes towards immigration. To check the robustness of our results to alternative dependent variables, we provide additional robustness checks in Subsection 4.3 with respondents' support for the different German political parties used as a proxy for political affiliation and voting preferences. Unfortunately, data on voting intentions are recorded in the SOEP only intermittently, usually around election dates with gaps of four years between waves. Therefore, data on voting are not available with a sufficient number of observations within our period of analysis, making the variables of attitudes and

preferred party the best proxies to capture a plausible change in attitudes towards immigration.

3 The Natural Experiment

This section first describes the conventional historical rules governing media reporting on crime in Germany. Then, it details the chronology of events surrounding New Year's Eve 2015-2016 in Cologne, a city located in West Germany. Six months later, in July 2016, these events led the Sächsische Zeitung, which is sold almost exclusively in the eastern part of Saxony,[22] to change its reporting policy regarding the disclosure of criminals' origins. Finally, it provides descriptive and empirical evidence of this change using the data described in the previous section.

3.1 Media reporting on criminality in Germany and New Year's Eve 2015-2016

Rules and conventions that apply to the press in Germany are described in the German Press Code, best known as the "Pressekodex". The Pressekodex comprises guidelines intended to define "the professional ethics of the press" and defend press freedom in Germany. It helps to preserve the "standing and the credibility of the media" in Germany. While these guidelines are not mandatory, the German Press Code is effectively advisory and broadly accepted among German journalists. Edited by the German Press Council since 1973 in its current form, it defines, among other things, rules on crime reporting to prevent discrimination based on sex, disability, ethnic, religious, social, or national affiliation. The 2015 version of the code stated that when reporting crimes, "it is not permissible to refer to the suspect's religious, ethnic or other minority backgrounds unless this information can be justified as being relevant to the readers' understanding of the incident. In particular, it must be borne in mind that such references could stir up prejudices against minorities" (See Section 12 of the Pressekodex, pp-9). Nevertheless, while well defined in the Pressekodex, rules on the disclosure of offenders' origins in Germany have been widely challenged in recent years.

The historical debate on whether newspapers should disclose criminals' origins opposes two different visions that would lead to opposite actions and guidelines. Surprisingly, despite the importance of the discussion, empirical evidence to inform the debate is still lacking. On the one hand, journalists supporting Article 12.1 of the German press code argue that systematically revealing

[22] Figure 3 illustrates the area of distribution for the Sächsische Zeitung. It is clear that the distribution area is geographically opposite the city of Cologne, where the New Year's Eve 2015-2016 events were concentrated.

perpetrators' origins could result in discriminatory behavior against certain minorities. Indeed, it is likely that natives with pre-existing prejudices pay much more attention to articles that disclose foreigner criminals' origins than to articles disclosing the origins of native criminals. This in turn could result in disproportionally higher attention from the public in general towards immigration-related criminality. Thus, guidelines could be necessary to protect minorities from being systematically associated with criminal activities. On the other hand, journalists against Article 12.1 of the German press code mainly argue that offenders' origins should always be disclosed since not mentioning this information leads natives to overestimate the real proportion of crimes committed by foreign-born individuals relative to natives. Indeed, if a biased association between immigration and crime is already widespread in the population, not reporting the origins of criminals would only reinforce initial perceptions and a person with a migration background would systematically be associated with each reported crime. Additionally, they suggest that the rise of the internet and social media makes it difficult to prevent information from reaching the public. Finally, they argue that concealing criminals' origins reduces public confidence in the media's independence and leads readers to feel manipulated by newspapers.[23] Tanit Koch, the former chief editor of BILD, the best-selling German newspaper, declared that Article 12.1 of the German Press Code was "unjustified self-censorship" and that it damaged the credibility of the media as a whole.[24] At present, this debate remains highly sensitive among the German population at large. In a 2017 interview, Nikolaus Jackob, managing director of the Institute for Journalism in Mainz, emphasized that the German Press was an exception in Europe on this specific subject due to, among others, the "widespread discrimination and persecution during the Nazi era".

The debate reached a turning point following New Year's Eve 2015-2016. More than a thousand acts of theft and sexual attack occurred that night, mostly in Cologne.[25] The next day, the police

[23] In a 2015 survey from the Dortmund-based Forsa Institute, 44% of respondents reported that the German press was partially or wholly lying to the people. Early in 2016, the Sächsische Zeitung addressed a survey to its subscribers meant to asses confidence in the newspaper and perceptions of its practices for reporting on crime. The main conclusions revealed that 66% of Sächsische Zeitung readers gave a grade of "very good" or "good" (33% gave a grade of "satisfactory") and that 75% reported no change in trust towards the newspaper since the beginning of the refugee crisis. Only 25% (50%) agreed (did not agree) with the statement that the media had concealed the origins of foreign criminals out of consideration for the criminals.

[24] Source: https://www.presseportal.de/pm/66148/3268159, published on March 4, 2016.

[25] Further investigations and a report from the federal criminal police office acknowledged that many other sexual attacks occurred the same night in Nuremberg and Munich (27 crimes), Bremen (11), Berlin (6), Baden-Württemberg (25) and Hessen (31). However, the largest number of complaints was recorded in Cologne, Düsseldorf, and Bielefeld, cities that all belong to the North Rhine-Westphalia region (1,076). The report also underlines that the victims were mostly female and that among the 1,076 crimes reported, 384 were sexual offenses. Source: https://www.dw.com/de/silvester%C3%BCbergriffe-in-zw%C3%B6lf-bundesl%C3%A4ndern/a-19000199.

press release reported that the suspected criminals were of "North African" and "Arab" appearance, which was confirmed during a press conference on January 4, 2016. To a large extent, traditional media did not mention the offenders' origins in their initial reports. The Saxony-based newspaper Sächsische Zeitung reported sexual assaults in Cologne on January 2 but did not mention any information on the offenders' origins. It was only four days after the events, when the news became widespread and following the police press conference in Cologne on January 4, that the newspaper started to disclose information that was by then already public. These omissions from the press led to numerous accusations of a cover-up on social media, and newspapers were accused of repeatedly withholding information on the origins of perpetrators.[26] Journalists explained that the reason for the choice not to disclose the origins of the perpetrators was Article 12.1 of the German Pressekodex (which again generally prohibited journalists from mentioning the origins of criminals). Nevertheless, the absence of reaction from the media and the fact they deliberately decided to conceal criminals' origins had already contributed to the rebirth of the term "lügenpresse" (lying-press), which was historically used by far-right movements in Germany, as reported in Figure H6 in the Appendix H . Importantly, these events occurred in the context of rising anxiety due to immigration in general and the 2015 refugee crisis in particular where German populism was driven by an increasing distrust in the press, primarily accused of treating information related to migrants in an overly favorable manner.[27] Between 2014 and 2018, Germans ranked immigration issues as the most pressing problem facing the country. At its peak of salience in January 2016, more than 85% of the population reported immigration as the most essential concern.[28]

After January 2016, the discussion became a debate on the foundations of Article 12.1 of the Pressekodex and whether it should be modified or removed.[29] On March 9, despite the public

[26]The outrage was not limited to newspapers but also directed towards German public television, among other media. For instance, ZDF, a major German public-service television broadcaster, did not report the events in Cologne in its daily news bulletin "Heute" (German for "Today") on January 2, 2016. The channel then had to publicly apologize for having deliberately omitted the information. Chief editor Elmar Theveben declared that "it was a mistake that the incidents were not reported at least at 7 p.m. today. We wanted to wait for the crisis meeting on Tuesday to save time for further interviews. However, it was an obvious error of judgment." https://www.thelocal.de/20160106/cologne-police-cant-work-this-way-minister.

[27]Since 2014, and partly due to the refugee crisis, Germany has witnessed its share of foreign-born population in all residents rise from 10.04% to 13.17%, corresponding to an increase of 2,762,487 individuals according to the German Federal Statistical Office. This historical inflow contributed to the rebirth of far-right parties and negative sentiments regarding immigration. For instance, the Alternative for Germany party (AfD) obtained a stunning success in the last German federal elections of 2017, becoming the third-largest political force in the country with 94 seats in the Bundestag.

[28]Information from the German poll *Politbarometer*, accessed on February 16, 2021. Link: https://www.forschungsgruppe.de/Umfragen/Politbarometer.

[29]To highlight the intensity of the debate over the German Press Code in this troubled period, we collected monthly

pressure on the need for newspapers to systematically disclose offenders' origins, the German press council first decided to reaffirm its existing rules on criminal reporting. However, following the Press Council meeting, the Saxony-based newspaper Sächsische Zeitung, through its editor-in-chief Uwe Vetterick, stated that the paper would consider disclosing criminals' nationality irrespective of the origin of the offender. The official announcement was made public several months later in July 2016 in the following terms: *[...] we asked ourselves: does the Directive of the Press Code actually contribute to the protection of minorities in the current situation in Dresden and Saxony? Many Sächsische Zeitung employees, on the contrary, are convinced that it is precisely the failure to name the nationality of offenders and suspects that can create room for rumors that often harm those we would like to protect. [...] That is why, after some thoroughly controversial discussions, we decided not to comply with the guidelines of the German Press Council when reporting on crime committed by foreigners. Instead, we will in the future specify the origin of offenders or suspects at all times. It does not matter if the offender is German, as in most cases, or a foreigner.*[30] We exploit this unexpected policy shift to study how natives in the distribution area of the Sächsische Zeitung reacted to this change. We consider July 2016 to be the date of treatment because it corresponds to the official announcement of the policy change. This choice is discussed extensively in the next Subsection.

3.2 Empirical Evidence

This Subsection provides empirical evidence that the Sächsische Zeitung effectively changed its reporting policy after its official announcement in July 2016. Using crime-related articles collected from several newspapers across the country, we define $Disclose_{n,t}$ as the share of i articles that disclose criminals' origins relative to the total number of crime-related articles in newspaper n at

data on web-search interest on the German Press Code in Germany from 2014 to 2019. This allows us to obtain information on how frequently the term "Pressekodex" was entered into Google's search engine each month during this period. As depicted in Figure H7 in the Appendix H , the largest number of requests including the term "Pressekodex" occurred precisely in January 2016 at the time of the Cologne events. This emphasizes that while the debate already existed in Germany, this event was a primary explanation for the return of the issue to the forefront of public interest.

[30] Source: Fakten gegen Gerüchte, Sächsische Zeitung, July 3, 2016 (Authors' translation). The full version of the article is reported in Appendix H . Note that one year later, on March 22, 2017, the German Press Council finally adjusted the German Press Code, making it slightly looser than it had been previously. Section 12.1 was rewritten such that offenders' origins could be disclosed but only if it is in the public interest. It now states that when reporting on crimes, *"it must be ensured that any reference to a suspect's or perpetrator's membership in ethnic, religious or other minority groups does not result in a discriminatory generalization of individual misconduct. As a rule, membership in a minority group shall not be mentioned unless this is in the legitimate interest of the general public. In particular, it must be borne in mind that such references could stir up prejudices against minorities"* (Section 12, pp-9).

year-month t such as:

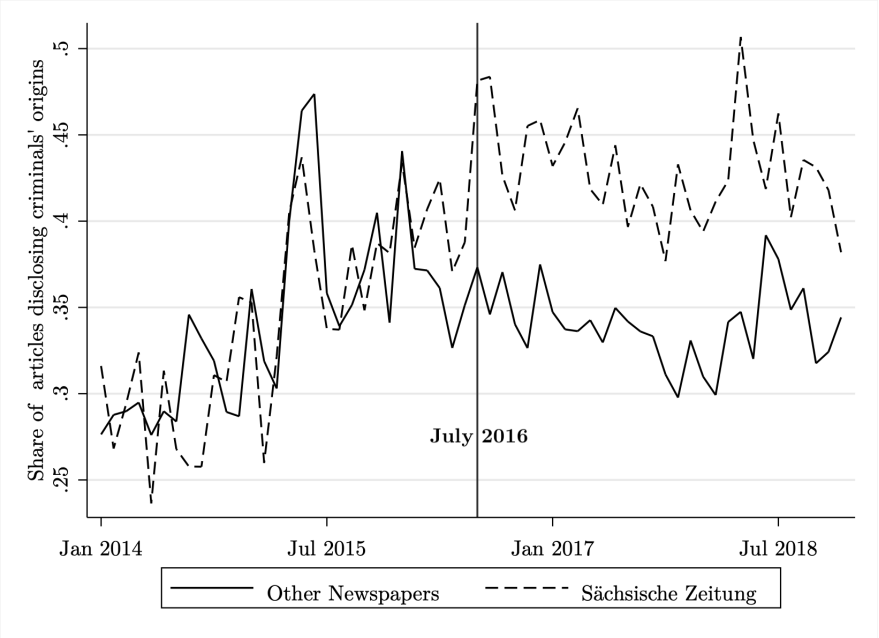
$$Disclose_{n,t} = \sum_i \frac{[Article_{i,n,t}|Origin_{i,n,t} = 1]}{Article_{i,n,t}} \tag{1}$$

where $Article_{i,n,t}$ is crime-related article i published in newspaper n during year-month t and $Origin_{i,n,t}$ is a dummy variable that indicates whether at least one origin is detected in the article by any lexicon.

Descriptive evidence. From January 2014 to December 2018, we find that German newspapers on average disclosed offenders' origins in 36.37% of serious crimes (34.23% before July 2016 and 38.52% after) with substantial variability (standard deviation of 5.78%) as reported in Table B1 in Appendix B . Articles reporting a crime may not disclose the origin of the offender for two reasons: (i) the journalist does not have this information, which is typically the case for unsolved cases, for instance, or (ii) the journalist knows the origin but decides that the information is not relevant in the context, in line with the recommendation of Article 12.1 of the Pressekodex. This uncertainty explains why, even for newspapers with a high disclosure level, the percentage of revealed origin is always far below 100%. The main effect of the shift in reporting policy by the Sächsische Zeitung is to remove the second reason (ii). It is worth noting that conditional on knowing the origin of the offender, decisions about the relevance of the information are essentially highly subjective and reporters might come to different conclusions, which can result in different disclosure rates across origins. For instance, the origin of German suspects might be judged as non-essential information more often than the origin of foreign suspects. In that case, journalists may over-report foreign criminality in the pre-treatment period, implying a larger relative increase in disclosure for German suspects after the reform. Against this background, we pay particular attention to the effect of the change in the reporting policy on individual lexicons in the rest of our analysis.

Table B1 in Appendix B shows that, on average, German newspapers did not change their reporting policies regarding native criminality after July 2016. In contrast, in line with the announced change in reporting policy, we find a radical shift for the Sächsische Zeitung before and after July 2016. We plot these variations in Figure 2 for the Sächsische Zeitung and other German newspapers pooled together. As early as July 2016 and its official announcement, the Sächsische Zeitung unambiguously started to depart from other German newspapers by systematically over-disclosing the origins of criminals. This differential in reporting remained remarkably constant over

Figure 2 – Share of Crime-Related Articles Disclosing Criminals' Origins



Note: This graph depicts the variable $Disclose_{n,t}$, which is the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. All newspapers except the Sächsische Zeitung are pooled together.
Source: Authors' elaboration on Dow Jones Factiva archives.

time. Importantly for the identification strategy, the Sächsische Zeitung's reporting closely matched the reporting of other German newspapers in terms of both levels and trends in the pre-treatment period.

Difference in Differences. We test the validity of the descriptive evidence reported above with a more formal analysis at the newspaper-year-month level. We estimate the following specification:

$$Disclose_{n,t} = \beta SZ_n \times July16_t + \gamma_t + \gamma_n + \varepsilon_{n,t} \tag{2}$$

where $July16_t$ is a dummy variable equal to one after July 2016 and zero before and SZ_n is a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. γ_t and γ_n are year-month and newspaper fixed effects, respectively, which control for time-varying confounders at the country level and time-invariant newspaper characteristics. Thus, β captures the differential in

reporting offenders' origins between the Sächsische Zeitung and other German newspapers after July 2016. As in standard difference-in-differences estimates, the identifying assumption is that in the absence of the July 2016 policy shift, the Sächsische Zeitung would have continued to disclose criminals' origins in the same way as other newspapers in Germany. Standard errors are clustered at the newspaper level and estimates are weighted using the total number of crime-related articles published by each newspaper. Results are reported in Table 2.

Table 2 – Change in Reporting Policy

	(1) All	(2) All	(3) All	(4) Victims	(5) Perpetrators
SZ_n	-0.005				
	(0.032)				
$July16_t$	-0.010				
	(0.016)				
$SZ_n \times July16_t$	0.096***	0.084***	0.078***	0.003	0.019***
	(0.016)	(0.014)	(0.011)	(0.003)	(0.003)
Newspaper FE	No	Yes	Yes	Yes	Yes
Year-month FE	No	Yes	Yes	Yes	Yes
Newspaper $\times$ linear time trend	No	No	Yes	No	No
Nb. Articles weights	Yes	Yes	Yes	Yes	Yes
Nb. Observations	1,475	1,475	1,475	1,475	1,475
Adjusted R^2	0.017	0.827	0.884	0.622	0.623
$AverageDisclose_{n,t}$	0.418	0.418	0.418	0.077	0.115

Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. $July16_t$ is a dummy variable equal to one after July 2016 and zero before, and SZ_n a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. Robust standard errors clustered at the newspaper level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on Dow Jones Factiva archives.

Column (1) reports the results of the main specification estimated without fixed effects. We find a positive and highly significant β, which suggests that the Sächsische Zeitung indeed increased its propensity to disclose offenders' origins in crime-related articles after July 2016.[31] This effect remains in column (2) when controlling for year-month and newspaper fixed effects. Regarding the magnitude of the effect, we estimate that the Sächsische Zeitung policy shift in reporting created a positive differential with other newspapers of approximately 8.4 percentage points. In column

[31] The coefficients are not directly comparable with the results in Table B1, as the latter reports aggregated differences between Sächsische Zeitung and other newspapers pooled together.

(3), we show that this effect is robust to controlling for newspaper-specific linear time trends, allowing for differential trends in newspapers. Interestingly, this new estimate supports the parallel trends assumption described above: in the absence of the change in reporting policy, the Sächsische Zeitung would have continued to disclose criminal origins similarly to other newspapers in Germany.[32]

Perpetrators and Victims. Bag-of-words models have the advantage of being entirely transparent and flexible in their implementation. This methodology allows us to detect in each article whether the origin of the offender is revealed. Nevertheless, this class of algorithms completely discards semantics and the meaning of the sentences, which may increase measurement errors. In this case, one would like to verify that the gap between the Sächsische Zeitung and other newspapers after July 2016 is not driven by an increased propensity to reveal the victims' origins for instance. We replicate the analysis but consider an article to disclose an origin only when it also includes keywords associated with victims or perpetrators. Columns (4) and (5) in Table 2 reveal that the positive gap that we observe in the previous estimates is driven by the new disclosure of the origins of perpetrators. It shows, therefore, that the main effect is robust to more precise identification of the origins associated with offenders.[33]

Event analysis and choice of the treatment date. We divide the treatment variable into a set of interactions using quarterly leads and lags before and after July 2016, the month of the official announcement of the policy change. The estimated coefficients are reported in Figure B2(a) in the appendix. The first quarter before July 2016 (April to June 2016) is the omitted category. We find that before July 2016, the differential in reporting between the Sächsische Zeitung and other newspapers is negative, with no significant differences in any quarter. In line with the natural experiment, we observe that this gap becomes positive in July 2016, i.e., after the Sächsische Zeitung's official announcement. Then, the differential remains positive and significant during the

[32] It is worth noting that these results are robust to controlling for regional fixed effects interacted with year-month effects. The fixed effects absorb any time-varying confounders at the regional level, such as the level of unemployment or share of foreign-born individuals in the resident population. The conclusions remain unchanged under this specification. Estimates without weights also give similar results. All these additional results are available upon request from the authors.

[33] As expected, the positive gap in disclosure between the Sächsische Zeitung and other newspapers decreases in magnitude under this restrictive set of estimates. By definition, it does not take into account all the crime articles for which the Sächsische Zeitung discloses the origins of the offenders without using keywords associated with perpetrators. Moreover, while there are few words to designate victims of crime, many other words not included in this lexicon can be used to refer to offenders. Examples are "accused" or "convicted". We did not extend this specific lexicon beyond a few keywords because the goal is not to capture all possible references to offenders.

remaining quarters.[34]

The timing of the events described previously may raise the question of the choice of the treatment date, as one could be worried that the Sächsische Zeitung initiated the policy change before announcing it officially. Indeed, Figures 2 and B2(a) in the appendix suggest that a slight positive change in disclosure already occurred after April 2016. This means that the Sächsische Zeitung had already started to partially implement its new policy after the quarterly German Press Council meeting but that it only became fully effective in July, after the official announcement. This would imply that the choice to focus on July 2016 as the cut-off date in the empirical analysis is a conservative choice. We implement several checks to verify that July 2016 is the relevant date for the policy change and/or that this choice does not affect the results and main conclusions. First, analysing the share of articles that disclose criminals' origins in the total number of crime-related articles published in the Sächsische Zeitung over the period 2014-2016 as a monthly time series, a simple structural break test with unknown break date rejects the null hypothesis of no structural break and detects a break in July 2016, which corroborates the choice of treatment date.[35] Second, we run additional placebo analyses of the pre-treatment period in the appendix, with the results reported in Tables C1. The coefficients suggest that the Sächsische Zeitung plausibly started to depart from Article 12.1 of the Pressekodex before July 2016 but to a much lesser extent than after that date. Indeed, removing the months after June 2016 but getting closer to the "de jure" treatment date, the coefficient of interest becomes statistically significant at conventional levels.[36] Third, the event analyses in Figure C1 and C2 in the appendix corroborate this interpretation. When either removing the April to June 2016 period or considering April as the treatment date, we find that the Sächsische Zeitung slowly started to implement its new policy in April 2016. However, it is quite clear that the decision was only fully implemented in July, at the time of the official announcement, and then continued until the end of the analysis period. To provide evidence that the conservative choice to use this "de facto" treatment date, instead of the "de jure" treatment date of July 2016, does not affect the results, we replicate all the main estimates of the paper in Appendix B and

[34] The picture is even clearer for priming regarding native offenders, the lexicon on which we mostly focus in the rest of the analysis and for which the coefficients in the pre-treatment period indicate no significant differences relative to the omitted category of April to June 2016.

[35] This result is not reported in the paper but is available upon request from the authors.

[36] Combining results from Tables C1 and Table C2, which focus on July 2014 to March 2016 with alternative placebo treatments for the pre-treatment period, provides additional support for the validity of the parallel trends assumption. In all estimates, the coefficient of interest is not significant, which indicates that the Sächsische Zeitung started to depart from other newspapers only in April 2016 and not before.

show that all our conclusions are unchanged under this alternative specification.[37]

Heterogeneity across lexicons. As previously mentioned in Section 3.2, we anticipate potential heterogeneity in the effects of the change in reporting policy across lexicons due to the subjective nature of the pre-treatment decision to disclose offenders' origins when they are known to the reporter. For this reason, we replicate the previous results for each of the different lexicons. We compute five additional variables that capture, for each newspaper, the monthly share of articles that disclose the origins of natives ($Disclose_{n,t}^{Ger}$), foreign-born individuals ($Disclose_{n,t}^{For}$), and immigrants ($Disclose_{n,t}^{Mig}$), as well as the top 10 foreigner and refugee nationalities ($Disclose_{n,t}^{Top}$ and $Disclose_{n,t}^{ref}$, respectively). Table B1 and Figure B1 in the appendix confirm that regardless of the lexicon considered, the evidence shows that German newspapers did not change their reporting policies in July 2016.[38] As a matter of fact, we find substantial variation for the Sächsische Zeitung before and after July 2016 in the disclosure of German offenders and, to a smaller extent, a significant increase for other foreign nationalities (driven by the top 10 foreign nationalities). This is consistent with the official announcement of the newspaper, which stated that it would report *"the origin of offenders or suspects, at all time [...] no matter if they are German [...] or foreigners"*. These results corroborate all the event analyses reported in Figure B2 for each lexicon separately. The estimated gap in disclosures appears to be driven mostly by the priming regarding native criminality, while we also observe a slight increase (although less precisely estimated) in the disclosure of the main foreign nationalities by the Sächsische Zeitung. In contrast, we find no significant differences in reporting of immigrant markers or refugee nationalities specifically for the Sächsische Zeitung after July 2016.[39] In recognition of the fact that the change in reporting policy has resulted mainly in an increase in the share of articles that reveal the origins of native offenders, we use the German lexicon for the main results in the remainder of the study. However, we show the results for the other lexicons in the appendix.

Comparison with actual crime statistics. How does the measure of newspaper disclosures of the origins of criminals compare to actual crime statistics? Figure B3 in the appendix shows that the

[37] Additional results in Section 4 also provide strong support for July 2016 as the treatment date, as we find no effect of the policy on natives' attitudes towards immigration before this date.

[38] Note that the statistically significant difference in the mean of native offenders' origins reported in other newspapers is not robust to the exclusion of July 2018 from the sample, during which period we observe a one-time increase in disclosures.

[39] The 2SLS results in Section 4 confirm that the slight increase in priming towards foreign criminality is not large enough to create the conditions for a natural experiment that would allow us to separately estimate the impact of the disclosure of the origins of offenders with an immigrant background on natives' attitudes towards immigration.

share of newspaper articles revealing the German origin of a suspect, among all articles disclosing a perpetrator's origin, is much lower than the actual share of all suspects who are German in violent crime cases as reported in official crime statistics (see Figure H3).[40] This is consistent with previous findings showing that beliefs about origin-specific crime rates based on newspaper articles are likely to be biased against foreigners and that the correlation between immigration and crime is likely to be overestimated in the population (in line with Bianchi et al., 2012a; Fasani et al., 2019; Alesina and Tabellini, 2022). Thus, correcting this bias with the appropriated policy could induce readers to revise such misperceptions. Again, Figure B3 shows that following the change in reporting policy, the Sächsische Zeitung disproportionately increased the share of its articles reporting a German nationality (conditional on disclosing the origin of the criminal). Importantly, Figure H5 shows that this evolution is not driven by a change in the proportion of foreign-born individuals among crime suspects in the distribution area of the Sächsische Zeitung, as this share evolved in parallel with the corresponding share in other German regions.

A legitimate concern is that the media bias in reporting could be driven by higher shares of foreign-born individuals among suspects in specific types of violent crime cases that reporters consider particularly newsworthy. Figure H4 shows that the share of foreign-born individuals among suspects in violent crime cases is relatively stable and comparable between all types of crime except for drug crimes. This indicates that the reporting bias is not driven by specific types of crime for which foreign-born individuals are over-represented and that newspapers are more likely to report on.

3.3 Additional results and robustness checks

Type of crimes. Table B2 in the appendix replicates the results for the German lexicon for each type of crime. The coefficient of interest is significant for all types of crimes except immigration crimes. This result makes sense since immigration crimes involve foreign-born individuals by definition, leaving little room for increased disclosure of offender origins. Still, these results must be interpreted with caution, as differences in the precision of the estimated coefficients may be affected by the volume of articles available for each crime type.

Alternative clustering. Standard errors in the previous estimates were clustered at the treatment level, i.e., the newspaper level, in order to adopt the standard, conservative approach of clustering

[40] Note that the decline in the share of Germans among suspects during the period of analysis coincides with the decline in the share of Germans in the population overall.

standard errors using more aggregated clusters (Cameron and Miller, 2015). In Table B3 in the appendix, we provide additional robustness checks that show that the results are robust to alternative levels of clustering. Indeed, the low number of newspapers in the analysis could be a concern, as it is lower than the rule-of-thumb minimum of 50 clusters, which is usually recommended for reliable inference (Cameron and Miller, 2015). From column (1) to column (5), we sequentially present all the possible levels of clustering (from the largest to the smallest) that can be applied to the benchmark specification, namely, region (14), newspaper (25), region-year (69), newspaper-year (124), and region-year-month (818). In addition, column (6) reports the coefficient of interest with bootstrapped standard errors. Columns (1) and (2) report coefficients that are significant at the one percent level. However, the p-values associated with wild cluster bootstrapping with Webb weights, a method that can ensure reliable inference with a small number of clusters (MacKinnon and Webb, 2017), are not significant at conventional levels. Reassuringly, when clustering the standard errors at lower levels, reported in columns (3) to (5), the main coefficient is always positive and significant at the one percent level as well as p-values associated with wild cluster bootstrapping. In all these specifications the number of clusters is above 50.

Additional changes for the Sächsische Zeitung. A legitimate concern regarding the analysis is that the increased disclosure of native offenders may not have been the only change implemented in the new reporting policy of the Sächsische Zeitung. It is important to note that the Sächsische Zeitung is considered a left-leaning newspaper and is not known for holding strong anti-immigrant positions. In fact, the paper carried the subtitle *"Organ of the Dresden Regional Administration of the Socialist Unity Party of Germany"* from 1946 to 1990. This suggests that the risk of the newspaper taking advantage of the policy change to manipulate information about the origins of criminals to incite prejudice against foreign-born individuals, the avoidance of which is the main motivation for the reporting guidelines in the Pressekodex, is rather limited. That said, the newspaper could have changed other aspects of its publication. *First*, we provide evidence in Figures D1 and D3 that the share of articles reporting on violent crime in the total number of articles published each month was identical between the Sächsische Zeitung and other newspapers in Germany before July 2016 and remained similar thereafter. We obtain similar results for the share of immigration-related articles in the total number of articles published each month in Figures D2 and D4. *Second*, Figures D5 and D6 in the appendix reveal that the Sächsische Zeitung did not start to over or underreport any particular type of violent crime. Instead, we observe that the share of immigration-related crimes in the total number of reported crimes remained identical between the Sächsische Zeitung and other

newspapers between 2014 and 2018. *Third*, we show in Figures D7 and D8 that the tone that the Sächsische Zeitung employed in crime articles did not change relative to that in other newspapers after July 2016. We use a simple text analysis with a publicly available German-language resource for sentiment analysis, the Spacy-Sentiws package in *Python*, to detect "emotions" such as joy, surprise, anger, fear, disgust, sadness, or contempt in the articles (Remus et al., 2010). For each emotion, we find no systematic differences between the Sächsische Zeitung and other newspapers before or after the treatment.

Overall, these results provide strong evidence that the policy change was implemented as announced. After July 2016, the Saxony-based newspaper Sächsische Zeitung indeed started to over-report offenders' origins compared to other German newspapers with no other substantial change. The identification strategy exploits this radical change to investigate how media reporting on criminality affects natives' attitudes towards immigration. Therefore, the testable assumption is that an increase in the share of articles disclosing the origin of an offender may have impacted natives' attitudes towards foreign-born individuals in the area of distribution of the Saxony-based newspaper by changing their beliefs about the differential in crime propensity between natives and foreign-born individuals. The direction of the effect is ambiguous, as suggested by the tense debate surrounding the Pressekodex in Germany. This ambiguity calls for a more formal empirical analysis in Section 4.

4 Media Reporting and Natives' Attitudes Towards Immigration

This section investigates the impact of a systematic reporting of offenders' origins on natives' attitudes towards immigration. The empirical strategy aims to capture the impact of the Sächsische Zeitung policy change within the newspaper distribution area on individual attitudes towards immigration. We first present the data used, then we describe the empirical strategy. Finally, we present the results along with robustness checks and complementary analysis.

4.1 Data on the distribution of newspapers

Data on the distribution of newspapers are taken from the IVW. Specifically, we use the 2014 edition of the distribution data of the German daily press, which were collected from November 4 to 10, 2013. During this reference week, publishers reported the direct individual sales of daily

newspapers, the number of newspaper copies sold via the distribution partners (press wholesalers, railway station bookstores, other retailers), all copies sold by subscription, and other sales and in-flight copies.[41] The number of sold units was then aggregated at the municipality level. Although e-papers constitute only a small fraction of sales, they were included in the distribution number and attributed to a municipality based on the place of residence of the invoice recipient. The choice of the 2014 edition in the empirical analysis is guided by the desire to use the information on newspapers' distribution areas before July 2016, i.e., that cannot be affected by the treatment.

An essential characteristic of the German newspaper landscape is the purely regional and, in many cases, even local character of the majority of daily newspapers (Kretzschmar et al., 2009). Historically, every city and region has had at least one newspaper (Harnischmacher, 2015) and local news is seen as the most interesting type of news (Hölig and Hasebrink, 2017). According to the report "Data on the media landscape in Germany 2018", daily newspapers reached 39.9 million persons per issue in Germany in 2018, corresponding to more than half the adult population. Around 31.7 million (80 percent) of these readers were readers of regional newspapers (Perspektiven, 2022),[42] emphasizing the fact that local newspapers are essential sources of information. They serve several functions, including (1) providing information on national news and opinions, (2) providing information on local news and everyday life, and (3) providing information on the local state (Bundesland) and region (Vonbun-Feldbauer and Dogruel, 2021). Given their regional focus, local newspapers are often widely distributed within a cluster of a few localities and virtually undistributed in other areas (Wehden and Stoltenberg, 2019). In other words, for most regional newspapers, the distribution area can be clearly identified. This is also the case for the Sächsische Zeitung, as reported in Figure 3. In 2014, the IVW reported that each edition of the Sächsische Zeitung would circulate approximately 238,000 times, placing it in the top 12 German newspapers in terms of circulation. Nevertheless, the newspaper was not diffused at all in most districts, while its market share reached 70% in some districts in Saxony and even exceeded 90 percent in a few municipalities. Hence, this newspaper is large enough to have a meaningful local impact but still too small to influence the overall newspaper landscape. In particular, the distribution of the Sächsische Zeitung is concentrated in the western part of the Saxony region such that the treatment area mainly corresponds to the governmental region (*Regierungsbezirk* in German) of

[41] The original German name is the *Informationsgemeinschaft zur Feststellung der Verbreitung von Werbeträgern*. The IVW is an organization comparable to the Audit Bureau of Circulation in India or in the UK. Its main mission is to certify and audit the circulations of major publications, including newspapers and magazines, within Germany.

[42] See: `http://www.ard-werbung.de/media-perspektiven/basisdaten/` (accessed 14 July 2022).

Dresden.[43]

A legitimate concern regarding the analysis is that the change in reporting policy could have affected the overall circulation of the newspaper or modified its market share. Comparing the distribution of the Sächsische Zeitung relative to other local newspapers in Saxony between the IVW editions of 2014, 2016, and 2018, we find no evidence of a noticeable change in local newspaper consumption.[44] First, Figure F3 in the Appendix shows that the district-level market shares of the Sächsische Zeitung in Saxony remained relatively constant over the period of analysis compared to major local competitors such as *Freie Presse*, *Morgenpost für Sachsen*, and *Leipziger Volkszeitung*. Mapping the distribution of the three IVW editions at the municipality level, Figure F4 also emphasizes no significant changes in distribution at the municipality level. Additionally, we more formally investigate the change in distribution by comparing the mean changes in district-level market shares between the 2014 edition and the 2016 edition of the distribution data (i.e., before the policy change) with the mean changes in district-level market shares between the 2016 edition and the 2018 edition (i.e., after the policy change). Table F1 in the Appendix shows that we cannot reject the null hypothesis that local distribution growth is the same before and after the change in reporting policy.[45] Finally, as shown in Figure F2 in the Appendix, the general distribution of the Sächsische Zeitung exhibited a continuous downward trend over the period of analysis from 2014 to 2018. This is reassuring because it mitigates the concerns that changes in the distribution could confound the effects of the policy change.[46]

Another concern is that crime-related information published by the Sächsische Zeitung could be diffused across the country through social media, implying an attenuation bias in our empirical analysis. To assess the relevance of this concern, we collected all relevant tweets between 2014 and 2018 referring to criminals or suspects from the social media platform Twitter.[47] Next, we identified whether each tweet included at least one nationality from the aforementioned lexicons. We find that before June 2016, Sächsische Zeitung followers published a share of relevant tweets

[43] Only two districts outside of Saxony display positive sales for the Sächsische Zeitung, specifically, Hamburg and Berlin, with market shares close to 0.09% and 1.15%, respectively. This is not surprising, as these two cities are the most populated in Germany, and the IVW data also include copies sold by subscription.

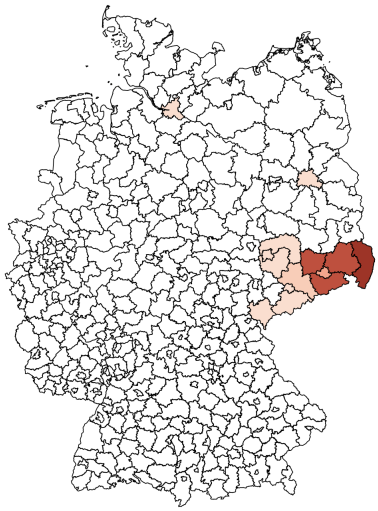
[44] The IVW collected data for the 2014, 2016, and 2018 editions in November 2013, 2015, and 2017, respectively.

[45] We obtain the same result when replicating the analysis at the municipality level in Table F2 in the Appendix.

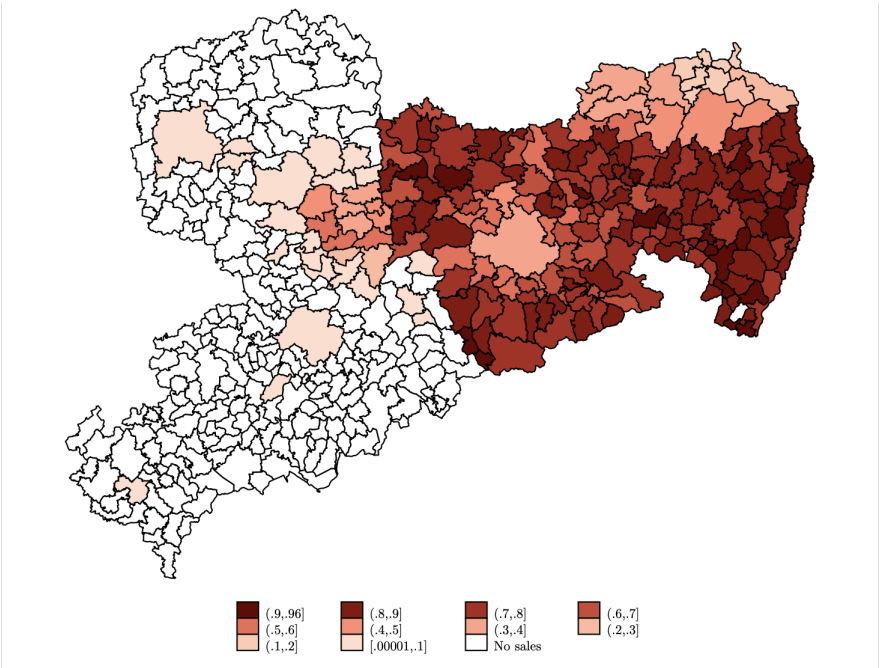
[46] For instance, one could have been concerned that the announcement of the reporting policy change could have induced a sizable expansion or contraction of the readership, in which case the estimated effect would have captured changes in the composition of the exposed persons instead of changes in attitudes.

[47] To identify relevant tweets, we developed a lexicon including all synonyms for the words “criminal”, “suspect”, and “perpetrator”. A tweet was classified as relevant if it contained one of those words.

Figure 3 – Sächsische Zeitung Areas of Distribution, IVW 2014 Edition



(a) All Germany (Districts)



(b) Saxony (Municipalities)

Note: This map depicts the sales of the Sächsische Zeitung as a percentage of total sales at the (a) district and (b) municipality levels.
Source: Authors' elaboration on data from the Information Community for the Assessment of the Circulation of Media (IVW).

containing words related to a German origin similar to that of other Twitter users and that the difference in the share of such tweets became positive in June 2016 and remained so up to 2017 (See Figure F7 in the appendix). On the contrary, we find no differences for foreign nationalities, in line with the previous results based on newspaper articles. These results strongly support the evidence that the Sächsische Zeitung policy change was indeed widely noticed and commented on by Sächsische Zeitung's followers. However, we find a low potential for spillovers in the distribution of this information in other states, as we found that more than 70% and 65% of the Sächsische Zeitung's followers and mentions, respectively, are from Saxony (see Figures F5 and F6 in the appendix).[48]

To assess the effect of the Sächsische Zeitung change in crime reporting policy on natives' attitudes towards immigration in the distribution area of the newspaper, we first compute the variable E_l^{SZ}, which captures the Sächsische Zeitung market share in locality l. This variable is used either as the variable of interest in the reduced-form analysis or as an instrumental variable in the 2SLS estimations as reported in the next Subsection. In the analysis, locality refers to districts (*"Kreise"* or NUTS-3 level areas).[49] Formally, exposure to the Sächsische Zeitung articles in locality l is defined as:

$$E_l^{SZ} = \frac{Sales_{n=SZ,l}}{\sum_n Sales_{n,l}} \tag{3}$$

where $Sales_{n,l}$ is the total number of sales of newspaper n in locality l in November 2013.[50]

4.2 Empirical Strategy

Our empirical strategy exploits variation in exposure to the Sächsische Zeitung across districts to implement two complementary estimation strategies that rely on the same source of identifying

[48]The reported results should to be taken with caution, as Twitter users are not representative of the general population exposed to the change in the crime reporting policy (Blank, 2017; Yildiz et al., 2017; Nguyen et al., 2022). Second, these data were collected from June 22 to August 13, 2022, and thus the lack of historical data going back to the policy reform or earlier implies that information on current location is not necessarily informative regarding location at the time of treatment. Third, most Twitter accounts lack explicit geographical location information, and we approximate the locations of accounts that contain at least partial information on location using the methodology proposed by Nguyen et al. (2022). Fourth, Twitter is not the most popular social media platform in Germany, as Facebook is by far the dominant platform with 49% percent of the population actively using it in 2020 vs. only 13% for Twitter (Newman et al., 2020).

[49]This choice is justified in Subsection 4.6, which also reports that our results remain virtually unchanged when using municipalities as the level of analysis. The municipality level analysis allows us to include additional fixed effects that reinforce the causal interpretation of our results.

[50]Unfortunately, the SOEP data do not include any information on individual media consumption during the analysis period. Thus, we are not able to compute the degree of exposure of an individual to any newspapers except by using his/her geographical location as a proxy.

variation and on similar identifying assumptions. Estimates based on the reduced-form equation can be obtained without using any of the data collected on German newspapers presented in Section 2. Thus, the reduced-form analysis is not influenced by any potential measurement errors—or omissions–in the methodology used to detect the disclosure of perpetrators' origins in crime articles. In addition, the reduced-form analysis takes advantage of information on the market shares of all newspapers in Germany, not only the 25 newspapers from which we collected crime articles. The complementary approach uses our collection of articles to directly distinguish localities where, on average, newspapers tend to disclose criminals' origins regularly from other localities where the German press guidelines are followed more strictly. Unlike the reduced-form, this alternative strategy precisely measures the overall extent to which natives are exposed to the disclosure of criminals' origins and thus complements the first set of results. It also allows us to test alternative mechanisms related to priming regarding native vs. foreign criminality and the type of crime reported. Because newspapers' reporting rules may be endogenous to attitudes towards immigration, this identification strategy requires 2SLS estimates using the Sächsische Zeitung policy shift of 2016 as an instrument. The two approaches are presented in more detail in the next paragraphs.

Reduced-form. The benchmark empirical model features the $Attitudes_{i,l,t}$ of individual i towards immigration in locality l at time t as the dependent variable. We combine the two answer modalities "Not concerned at all" and "Somewhat concerned" about immigration. This allows us to obtain a dummy variable equal to one for "Very concerned" and zero otherwise.[51] We estimate the following specification using a linear probability model:

$$Attitudes_{ilt} = \gamma_l + \gamma_{rt} + \beta E_l^{SZ} \times July16_t + \boldsymbol{\phi'} \boldsymbol{X_{it}} + \boldsymbol{\delta'} \boldsymbol{D_{ly}} + \varepsilon_{ilt} \tag{4}$$

where γ_l are locality (NUTS-3 district) fixed effects and γ_{rt} stands for regional (NUTS-2) fixed effects interacted with year-month fixed effects. The latter controls for any time-varying confounders at the regional level, such as the level of unemployment or the share of foreign-born individuals in the total resident population. $\boldsymbol{X_{it}}$ is a vector of time-varying individual characteristics included to increase the precision of the estimates with age, marital status, education, employment status and the inverse hyperbolic sine of individual earnings.[52] When interacted with $July16_t$ in additional

[51] In Table E8 in the appendix, we report additional results using alternative coding for the dependent variable such as excluding the category "Somewhat concerned", grouping "Somewhat concerned" and "Very concerned" together, and treating the attitudes variable as a continuous variable. These results suggest that the treatment mostly improved the attitudes of natives who were initially very concerned about immigration.

[52] Definitions and sources of the control variables are available in Appendix Table A3.

estimates, it also ensures that the treatment effect is not wrongly attributed to differing trends in observables between individuals in the distribution area of the Sächsische Zeitung and other areas. $\delta' D_{ly}$ is a vector of district controls varying at the year level. It includes the unemployment rate, the share of social transfer recipients, the share of refugees, the net internal and international migration flows, the share of crime in the overall population, and the share of foreign-born individuals in total crime (Destatis, 2020).[53] We also provide additional results obtained after including individual fixed effects to address spatial sorting issues, i.e., the possibility that individuals with strong opinions on immigration moved across districts as a reaction to the policy change. The main conclusions remain unchanged when using this highly demanding specification.[54] β is the coefficient of interest, which captures the impact of the Sächsische Zeitung policy change in the newspaper distribution areas on individual attitudes towards immigration.[55]

As in Couttenier et al. (2021), standard errors are clustered at the commuting zone (CZs) level in all estimates since information most likely circulates within the places where people live and work. Commuting zones are taken from Kosfeld and Werner (2012), who use factor analysis to define labour market regions based on the structure of correlations in commuter flows across German districts. When a commuting zone includes exclusively rural districts, we group it with the closest urban commuting zone, resulting in 76 distinct clusters in our sample. This approach allows us to obtain larger clusters, ensuring that each cluster contains a sufficiently large number of individual observations. It is worth noting that the number of clusters remains sufficiently high (above 50) to ensure meaningful inference Cameron and Miller (2015).[56]

2SLS estimates To capture an individual's exposure to the disclosure of the origins of offenders in crime articles, we first compute a weighted share of articles disclosing offenders' origins in

[53]The results are robust both to controlling for respondents' direct exposure to violence and to information proxied for by the Sächsische Zeitung market shares and crime rate at the commuting zone level, as reported in Table E10 in the appendix. These robustness checks are available upon request from the authors.

[54]It is worth noting that no controls or other fixed effects are absorbed by the individual fixed effects since all our individual controls are time-varying and between 2014 and 2018, five and eight percent of the respondents in the sample changed their district or municipality of residency, respectively. Indeed, including respondent fixed effects implies that the remaining variation identified in such specifications is driven by changes in individual characteristics between survey waves. Such a specification controls for the hypothetical scenario in which many individuals move into or out of the distribution area of the Sächsische Zeitung following the policy change.

[55]Notably, although they are not required for the empirical strategy, the results are fully robust to the use of individual longitudinal weights. Weighted regressions are reported in the appendix, Table E9.

[56]The results are robust to defining treatment at the level of clustering, i.e., commuting zones, as reported in Table E11 in the appendix. When clustering standard errors at the district level, the coefficient of interest is weakly significant but becomes significant at conventional levels when we drop districts for which the number of distinct individuals is below the average. These results are available upon request from the authors.

locality l during year-month t. The weights are the relative distribution of each newspaper in locality l in November 2013:[57]

$$WDisclose_{lt} = \sum_n \frac{Sales_{nl} \times Disclose_{nt}}{\sum_n Sales_{nl}} \tag{5}$$

where $Sales_{nl}$ is the total number of sales of newspaper n in locality l and $Disclose_{n,t}$ the share of articles that disclose criminals' origins in the total number of crime-related articles in newspaper n during year-month t, as described in Section 3. Then, we estimate the following 2SLS specification:

$$Attitudes_{i,l,t} = \gamma_l + \gamma_{rt} + \beta W\widehat{Disclose}_{lt} + \boldsymbol{\phi'X_{it}} + \boldsymbol{\delta'D_{ly}} + \varepsilon_{ilt} \tag{6}$$

with:

$$W\widehat{Disclose}_{lt} = \gamma_l + \gamma_{rt} + \delta E_l^{SZ} \times July16_t + \boldsymbol{\phi'X_{it}} + \boldsymbol{\delta'D_{ly}} + \varepsilon_{lt} \tag{7}$$

being the first-stage equation. Standard errors are again clustered at the commuting zone level. Note that this last equation echoes the equation estimated at the newspaper level in Section 3. δ is the coefficient of interest, which captures the impact of the Sächsische Zeitung policy shift on individual attitudes towards immigration in its area of distribution.

Identification assumptions: The validity of the empirical strategy requires that two essential conditions be met. First, the policy change must induce a substantial shift in the local share of articles disclosing criminals' origins. In other words, natives in the distribution area of the Sächsische Zeitung should have greater exposure to crime articles that disclose criminals' origins after July 2016 than natives in other areas. This assumption can be tested by assessing the Kleibergen–Paap F statistic from the first stage of the IV-2SLS estimation. Second, conditional on the other control variables and fixed effects included in the model, the policy change should not be correlated with changes in attitudes other than through the shift in the local share of articles disclosing criminals' origins. Moreover, the Sächsische Zeitung policy change should not be driven by pro-immigration trends in its area of distribution. While the exogeneity of the policy change cannot be tested empirically, several factors indicate that this assumption is plausible. First, as the results in Section 3 indicate, we do not find evidence that the policy change coincided with other substantial changes in editorial practices that could affect natives' attitudes through other channels. Second, controlling for year-month $\times$ region fixed effects implies that the estimates are not affected by the

[57] We again chose the 2014 edition of the IVW such that the distribution of newspapers across Germany cannot be affected by the treatment in 2016 and thus can be considered exogenous to the treatment.

regional repercussions of events that affected attitudes country-wide, such as the 2015 arrival of refugees or the New Years' Eve 2015-2016 events.[58] In addition, the fact that no major local elections were organized in Saxony in 2016 reduces concerns regarding changes in political discourse during election campaigns. Thus, conditional on control variables and fixed effects, there is a priori no obvious reason why attitudes in the governmental district of Dresden would evolve differently around the treatment date than attitudes in other governmental districts in Saxony in the absence of the shift in reporting policy. Finally, concerns regarding a pre-trend in our analysis are mitigated by further event analyses in Subsection 4.3, which provide evidence of no trends in attitudes in the pre-treatment period.

4.3 Reduced-form analysis

Benchmark results. Table 3 reports the results of estimating Eq. (4) with different combinations of fixed effects and/or control variables. Column (1) shows that the most parsimonious specification, including no fixed effects, estimates a β that is positive and significantly different from zero at conventional levels. However, this effect could be due to the confounding effect of time-varying trends related to concerns about immigration. This explanation seems highly plausible given the context during the period of analysis when Germany experienced a substantial increase in refugee immigration in 2015, which could have influenced individual perceptions of immigration. This period also corresponded to a rise in approval rates for the right-wing political party AfD, founded in 2013. Heterogeneous regional developments characterized both of these phenomena. Adding time fixed effects interacted with regional fixed effects reverses the sign of the coefficient of interest, which becomes negative and remains highly statistically significant (see column 2). This suggests that the region-specific time trends were indeed a confounding factor. In columns (3) and (4), the effect is not substantially affected by further adding district fixed effects and an interaction between individual control variables and a dummy variable identifying the periods before and after the change in reporting policy. Finally, in column (5), which corresponds to our preferred specification, and column (6), we provide two additional challenging estimates, which alternatively add a district linear time trend and individual fixed effects, respectively, to the benchmark specification. In both cases,

[58] It is worth noting that additional estimates at the municipality level, discussed in Subsection 4.6, report that the results are robust to controlling for year-month $\times$ government region (NUTS-2) and year-month $\times$ district (NUTS-3) fixed effects. These demanding specifications strongly reduce the likelihood of omitted variable bias in the analysis and reinforce the causal interpretation of the results.

Table 3 – OLS Regressions, Baseline Estimates
Concerns about Immigration

	(1)	(2)	(3)	(4)	(5)	(6)
E_l^{SZ}	0.126***	0.068***				
	(0.019)	(0.023)				
$July16_t$	-0.005					
	(0.011)					
$July16_t \times E_l^{SZ}$	0.045***	-0.053***	-0.056***	-0.063***	-0.087***	-0.043***
	(0.014)	(0.018)	(0.015)	(0.016)	(0.026)	(0.006)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes
Indiv. Controls $\times July16_t$	No	No	No	Yes	Yes	Yes
District $\times$ linear time trend	No	No	No	No	Yes	No
Individual FE	No	No	No	No	No	Yes
Nb. Observations	110,364	110,364	110,364	110,364	110,364	104,866
Adjusted R^2	0.046	0.080	0.098	0.099	0.100	0.479
Average $Attitudes_{ilt}$	0.342	0.342	0.342	0.342	0.342	0.345

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

the coefficient remains negative, highly significant, and of the same order of magnitude as in the previous estimates.

Interpretation. Overall, it seems that systematically disclosing the origins of offenders reduces natives' concerns about immigration. This echoes the Bayesian framework by Couttenier et al. (2021), which shows that individuals infer migration-specific crime rates from crime reports in the media to form their beliefs about the relationship between crime and migration. Priming regarding native criminality signals a lower differential in crime rates between natives and foreign-born individuals than what was perceived before the change in the reporting policy. This, in turn, induces a decline in concerns about immigration, as long as foreigner criminality is associated with such worries. Regarding the magnitude of the effect, the point estimates across the five last specifi-

cations of Table 3 indicate that a one-standard deviation increase in exposure to the Sächsische Zeitung (0.09) is associated with a 0.004 to 0.008 percentage point decrease in concerns about immigration, depending on the specification. We also compute a persuasion rate defined as the share of individuals who changed their attitudes towards immigration in response to the treatment in comparison with the population at risk of treatment. Following DellaVigna and Kaplan (2007), the persuasion rate f computed for the entire German population may be defined as:

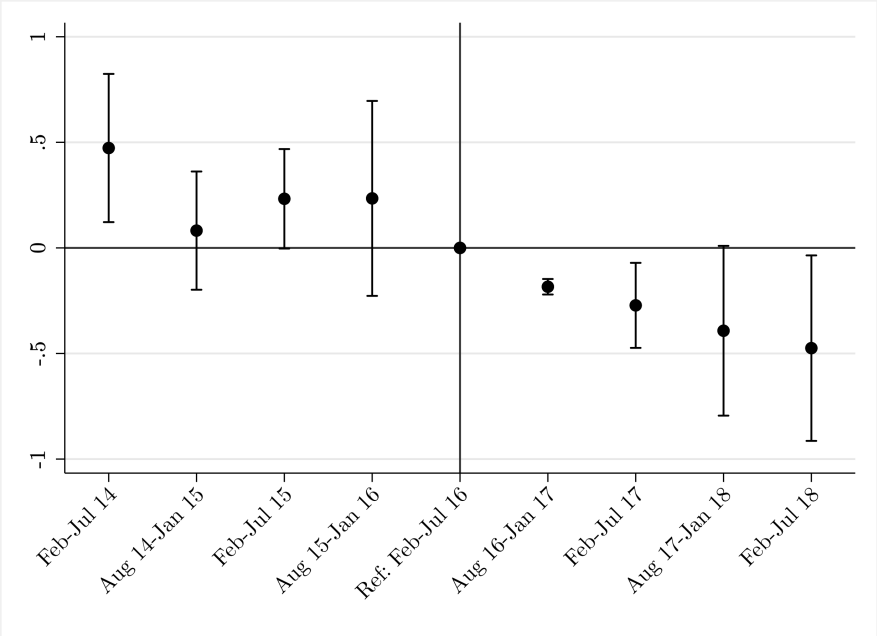
$$f = 100 \times \frac{\hat{\beta}}{N} \times \frac{1}{Attitudes_{Ger}} \tag{8}$$

where $\hat{\beta}$ is the estimated impact of the treatment, namely, the average percentage point decrease in concerns about immigration in response to the new disclosure of criminals' origins (0.006) estimated in Table 3. N is the share of newspaper readers in the German population. According to the Federal Association of Digital Publishers and Newspapers, 68 percent of Germans aged 14 or older read a newspaper at least once a day.[59] $Attitudes_{Ger}$ is the average share of individuals who were very concerned about immigration across the country before July 2016. Implicitly, this calculation assumes no spillovers between treated individuals and the control group. Under these assumptions, the persuasion rate is $f = 100 \times \frac{0.006}{0.68} \times \frac{1}{0.34} = 2.60\%$. This persuasion rate is in the lower range of other persuasion rates estimated in this literature (DellaVigna and Gentzkow, 2010). It is included in the range of persuasion rates estimated by Djourelova (2022) (between 1.9 and 4.4%) for a comparable policy.

East Germany. The effect estimated in Table 3 essentially represents the difference in changes in individual concerns about immigration as a function of the local exposure to the newspaper Sächsische Zeitung. Given the newspaper's very local nature, the vast majority of individuals in the sample have zero exposure. Therefore, a legitimate concern is that individuals in other regions of Germany might not represent a valid control group, essentially due to differences in regional political preferences, as previously mentioned. We address this concern by omitting all individuals residing in West Germany since regions in East Germany constitute a more homogeneous group in terms of political preferences and attitudes. The results shown in Table E2 in the appendix suggest that this does not substantially affect the estimated effect, either in terms of magnitude or in terms

[59] Source: https://www.bdzv.de/nachrichten-und-service/presse/pressemitteilungen/artikel/detail/acht-von-zehn-deutschen-lesen-zeitung/.

Figure 4 – OLS Regressions, Event Analysis



Notes: This graph depicts changes in natives' attitudes towards immigration in the distribution area of the Sächsische Zeitung before and after the policy change. Effects are grouped in bins of six months. The reference period is the period February-July 2016. Coefficients are obtained from a regression in which the dependent variable is a dummy variable for being "very concerned" about immigration. It includes the full vector of individual controls (age, marital status, education, employment status and individual earnings), the full vector of district-year controls (unemployment rate, share of social transfer recipients, share of refugees, net migration flows for native and foreign-born individuals, crime rate in the overall population, and share of crimes involving foreign-born individuals in total crimes), as well as district (NUTS-3) and year-month $\times$ region fixed effects, and a district-level linear time trend. Standard errors are clustered at the commuting zone level. Confidence intervals are reported at the 95% level.
Source: Authors' elaboration on SOEP data.

of statistical significance.[60]

Event analysis. Changes in attitudes towards immigration in the distribution area of the Sächsische Zeitung can be credibly attributed to the policy change only if (i) the sign of the estimated coefficient changes around the treatment date and (ii) the effect persists for at least several months. Thus, we re-estimate the main specification using an event-type analysis by defining bins

[60] We also verified that the results hold when we limit the sample to the region of Saxony. The coefficient remains unchanged but is sometimes imprecisely estimated due to the reduced sample size. These additional results are available upon request from the authors.

of six months.[61] The results are reported in Figure 4. The point estimates in the pre-treatment period do not reveal a downward trend before the policy change. Instead, the estimated difference in attitudes towards immigration is parallel to the horizontal line, especially in the 18 months preceding the policy change. Consistent with an effect from the change in reporting policy, the coefficient of interest is not statistically different from the reference period, i.e., 6 months before the treatment (February to July 2016). It becomes negative only right after the reference period and remains so for several months following the treatment. As previously noted in Section 3, the Sächsische Zeitung plausibly started to slowly increase the share of articles disclosing the origins of criminals in April 2016, before the decision was announced and fully implemented in July. To investigate the potential effect of this "*de facto*" treatment during the four months preceding the "*de jure*" treatment in July 2016, we split the reference period into two sub-periods, namely, February to March 2016 and April to July 2016. The corresponding set of estimates is reported in Figure E1 in the appendix. The point estimate for the months April to July 2016 indicates that the difference in the share of "very concerned" respondents between that period and the February to March 2016 omitted period is not significantly different from zero, indicating the absence of a pre-trend in immigration concerns in our analysis.[62] This confirms that the slight increase in disclosures in April 2016 was not sufficiently strong either to be perceived by readers or to affect their attitudes.

Placebo tests. Related to the concern that the estimated effect could capture a local trend in overall changes in attitudes specific to the distribution area of the newspaper Sächsische Zeitung, we re-estimate the specification in Table 3, column (5), with concerns about different topics as alternative dependent variables. Specifically, we test whether the change in the reporting policy affected concerns that are presumably orthogonal to immigration. As reported in Table 4, we find no effect on such concerns such as personal economic and health situations, the environment, the climate, or the economy. This substantially mitigates concerns about a local trend that could have driven the main results and suggests that the estimated coefficient β captures the genuine effect of the change in the reporting policy on natives' attitudes towards immigration. Nevertheless, we find that the treatment has a significant effect on perceptions of crime. This confirms our initial assumption that immigration is not orthogonal to crime and that they are both first-order issues that

[61] We choose bins of six month to ensure that each bin contains a sufficient number of observations. This is because the SOEP interviews are not evenly distributed across months, with most of them taking place between February and July.

[62] Additional placebo tests with placebo treatments in the pre-treatment period are reported in Table E3 in the appendix and display no significant effect for the coefficient of interest before July 2016, thus leading to the same conclusions.

are often considered jointly in peoples' minds. We test the robustness of this result and discuss its implications in Subsection 4.5 below.

Table 4 – OLS Regressions, Alternative Concerns

	(1) Immigration	(2) Crime	(3) Health	(4) Environment	(5) Pers. Situation	(6) Economy
$July16_t \times E_l^{SZ}$	-0.087***	0.071***	0.049	0.030	-0.018	-0.018
	(0.026)	(0.009)	(0.033)	(0.028)	(0.023)	(0.024)
Nb. Observations	110,364	110,260	110,269	110,225	110,200	110,112
Adjusted R^2	0.100	0.105	0.076	0.032	0.071	0.034
Average $Attitudes_{ilt}$	0.342	0.395	0.176	0.292	0.136	0.135

Note: The dependent variable is a dummy variable for being "very concerned" about a given issue. All estimates include the full vector of individual controls (age, marital status, education, employment status and individual earnings). All estimates include the full vector of district-year controls (unemployment rate, share of social transfer recipients, share of refugees, net migration flows for native and foreign-born individuals, crime rate in the overall population, and share of crimes involving foreign-born individuals in total crimes). All the estimates include district (NUTS-3) and year-month $\times$ region fixed effects, and a district-level linear time trend. Robust standard errors clustered at the commuting zone level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP data.

Other newspapers. In another placebo test, we verify that there are no differences in attitudes before vs. after July 2016 when alternative newspapers are considered as treated. Thus, we replicate our benchmark estimates in Table E4 in the appendix for newspapers with the highest market shares in Saxony, the main region of distribution for the Sächsische Zeitung. Reassuringly, we find no evidence of a significant effect for newspapers other than the Sächsische Zeitung.

Preferred party. To what extent are the changes in attitudes described above reflected in political preferences? Major elections in Saxony were held in 2014 and 2019 (national and European parliamentary elections), while federal elections were held in 2013 and again in September 2017. To the best of our knowledge, there were no other major elections close to the policy change in July 2016, and election results are not available at a sufficiently high frequency (monthly or weekly) to allow for meaningful inference. Therefore, we use political support for different political parties as an alternative dependent variable. Indeed, respondents in the SOEP are asked to provide information on their preferred political party conditional on whether they support any political party at all. For each major political party in the sample (i.e., parties cited as preferred political parties in more than one percent of all answers), we create a dummy variable equal to one if the respondent

supports that political party and zero otherwise.[63] From columns (1) to (8) in Tables 5, we replicate our benchmark specification, sequentially using each of the aforementioned dummy variables as the dependent variable. Grouping political parties into pro-immigration (SPD, Green, Die Linke and FDP) and anti-immigration (CDU/CSU and AfD) parties in column (1) and (2), our coefficient of interest is significant only for anti-immigration parties. This result is driven by political support for the AfD as reported in column (7) and the negative sign indicates that exposure to the Sächsische Zeitung is associated with lower support for this party after July, 2016. This is not surprising, as negative attitudes towards immigration is a unique characteristic of the AfD in Germany, as depicted in Figure H8 in the appendix (Volkens et al., 2021). This figure plots parties' policy positions as reported in the Manifesto Corpus, which are derived from a content analysis of electoral manifestos. Overall, it seems, therefore, that reduced concerns about immigration among natives also translated into less support for the main anti-immigration party in Germany.

Table 5 – OLS Regressions, Preferred Party

	Anti-immig.	Pro-immig.	Anti-immig.		Pro-immig.			
			CDU/CSU	AfD	SPD	Green	Die Linke	FDP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$July16_t \times E_l^{SZ}$	-0.060**	0.008	-0.019	-0.041***	0.019	-0.028	0.019	-0.002
	(0.026)	(0.024)	(0.020)	(0.009)	(0.012)	(0.024)	(0.015)	(0.003)
Nb. Observations	110,364	110,364	110,364	110,364	110,364	110,364	110,364	110,364
Adjusted R^2	0.062	0.068	0.071	0.022	0.052	0.066	0.041	0.018
Party %	0.190	0.236	0.171	0.019	0.122	0.064	0.035	0.016

Note: The dependent variable is a dummy variable for preferred party and zero otherwise. Pro-immig. includes SPD, Green, Die Linke and FDP and Anti-immig. includes CDU/CSU and AfD. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3), Year-Month $\times$ Regional fixed effects, and a district linear time trend. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Heterogeneity analysis. In Table 6, we conduct additional estimations over subsamples as a heterogeneity analysis. Column (1) reports the benchmark specification. In columns (2) to (4), we divide the sample by educational attainment. The estimates show that the treatment has larger effects among low-skilled respondents, although the coefficients are still negative and highly significant for high school graduates. In columns (5) to (8), we divide the sample using the median

[63] We exclude multiple responses, as they represent less than two percent of all responses.

age and employment status. We find a slightly larger coefficient for individuals aged 50 years or less and for unemployed respondents. We replicate this analysis using interactions between individual characteristics and treatment variable. Figure E2 in the appendix reports the marginal impact of the treatment on attitudes towards immigration for the different levels of the modifying variables. The coefficients from the interaction models confirm the previous findings for education and age but provide low support for a differential impact between employed and unemployed respondents. Overall, in line with the Bayesian framework discussed in the introduction, it seems that less-informed individuals, and/or individuals with initially more biased attitudes, may be more likely to update their beliefs following a change in media reporting that provides more information (DellaVigna and Gentzkow, 2010). Instead, older and more skilled native individuals may already have a better understanding of the crime rate differential between immigrants and natives.[64]

Table 6 – OLS Regressions,
Heterogeneity Analysis with Individual Characteristics

	(1) All	(2) No High School	(3) High School	(4) College	(5) Age < 50	(6) Age ≥ 50	(7) Unemployed	(8) Employed
$July16_t \times E_l^{SZ}$	-0.087***	-0.277**	-0.080***	-0.102	-0.085***	-0.080***	-0.098**	-0.080***
	(0.026)	(0.112)	(0.017)	(0.103)	(0.025)	(0.025)	(0.045)	(0.009)
Nb. Observations	110,364	14,157	66,430	29,659	55,358	54,964	39,385	70,927
Adjusted R^2	0.100	0.109	0.080	0.085	0.110	0.105	0.107	0.106
Average $Attitudes_{ilt}$	0.342	0.390	0.391	0.212	0.313	0.372	0.367	0.329

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3), Year-Month $\times$ Regional fixed effects, and a district linear time trend. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

4.4 2SLS

Benchmark results. 2SLS results are reported in Table 7. Columns (1) and (2) show the benchmark specification results without and with individual fixed effects. While we still find a negative sign for the coefficient of interest, it is not significant in column (1). Moreover, the Kleibergen–Paap

[64] It is worth noting that the we do not find any significant results when focusing on non-citizen respondents. This result must be interpreted with caution, as non-citizen respondents in the SOEP are a small group, with too few observations to draw any conclusions. However, in an interaction model estimated over the full sample with citizenship as a modifying variable, we still do not find any significant differences between the two populations. We obtain similar results using birthplace instead of citizenship to define our benchmark sample. This is not surprising given the strong overlap between the two definitions in our sample.

Table 7 – 2SLS Regressions, Baseline Estimates
Concerns about Immigration

	(1) All	(2) All	(3) >50%	(4) >50%	(5) All	(6) All
$WDisclose_{lt}$	-0.778	-0.511***	-1.135***	-0.469**		
	(0.554)	(0.188)	(0.204)	(0.224)		
$WDisclose_{lt}^{Ger}$					-0.777**	-0.509***
					(0.312)	(0.075)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month × Regional	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes	No	Yes
Nb. Observations	109,885	104,396	36,996	35,033	109,885	104,396
KP F-test	4.939	5.764	266.372	386.169	55.135	61.463
First stage	0.072**	0.077**	0.063***	0.074***	0.072***	0.078***
Average $Attitudes_{ilt}$	0.343	0.346	0.353	0.356	0.343	0.346

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

F statistic for these two first-stage estimates suggests that the Sächsische Zeitung policy shift in July 2016 does not provide sufficient variation to explain changes in natives' average exposure to the disclosure of the origins of criminals. However, one must bear in mind that the sample of 25 newspapers does not cover all newspapers sold in Germany. For some localities, we do not have enough coverage of sales to obtain precise estimates of exposure to crime reporting. Therefore, we report additional estimates in columns (3) and (4) in which we eliminate localities for which we do not have coverage of at least 50% of the total number of sales.[65] This allows us to increase the strength of the instrument and to increase the precision of the estimates. We

[65] Figure F1 in the appendix depicts these localities. The coefficient from the reduced-form equation estimated in Table 3 under this alternative sample is significant at the one percent level and falls within the interval [-0.045;-0.135], depending on the specification, as reported in Table E1 in the appendix.

recover a coefficient of interest that is negative and significant at conventional levels.[66] For these regressions, coefficients from the first-stage estimates are significant at the one percent level, and the Kleibergen–Paap F statistic clearly exceeds the Stock–Yogo critical value of 16.38. Thus, as long as we exclude localities with low coverage, the 2SLS results confirm that disclosing offenders' origins indeed improves attitudes towards immigration.[67]

A significant advantage of the 2SLS estimation over the reduced-form estimation is that it allows us to test for different mechanisms, such as whether the improvement that we observe in natives' attitudes towards immigration is driven by disclosing the origins of foreign or native offenders. In this way, Table 7 reports two additional estimates in columns (5) and (6) that only focus on the share of articles disclosing the origins of German offenders. For the benchmark specification, we find that disclosing German offenders' origins significantly reduces concerns about immigration. For this set of estimates, the sample's restriction to districts for which we cover at least 50% of overall sales is not necessary, which confirms that the natural experiment mostly relies on the new disclosure of native offenders by the Sächsische Zeitung.[68] Regarding the magnitude of the coefficient, we find that an increase in exposure to information about offenders' origins (std. dev.=0.05) is associated with a 0.032 percentage point decrease in the probability of reporting strong concerns about immigration. To ensure comparability between this coefficient and the coefficient estimated in the reduced-form analysis, we compute the latter using an increase in disclosure that corresponds to a one-standard deviation higher exposure to the Sächsische Zeitung as estimated from the first-stage equation. We find that a one-standard deviation increase in exposure to the Sächsische Zeitung (0.090) implies a $0.090 \times 0.064 = 0.006$ increase in the weighted share of articles that disclose offenders' origins and thus a persuasion rate f equal to $f = 100 \times \frac{0.006}{0.68} \times \frac{1}{0.34} = 2.60\%$, similar to the persuasion rate found in the reduced-form analysis.

Note that, in line with the changes in the reporting policy over time described in Figure B2 as measured with the alternative lexicons discussed in Section 3, we find that the instrument is weak in the first-stage estimations for the alternative lexicons that focus on foreign origin countries and identifiers, which prevents us from drawing conclusions about the specific impact of priming

[66] The choice of alternative thresholds does not change the main conclusions. As long as the instrument is strong, we always find a negative and significant coefficient for the variable of interest. These results are available upon request.

[67] Coefficients in Table E5 in the appendix confirms the absence of significant results for other major newspapers in Saxony.

[68] Eliminating localities for which we do not have coverage of at least 50% from these regressions does not affect the coefficient of interest.

regarding foreign criminality on natives' attitudes towards immigration.[69] Still, controlling for priming about offenders with a foreign/immigrant background using alternative lexicons, our coefficient of interest for the disclosure of perpetrators' native origins remains significant and of the same order of magnitude. These results are reported in Table E7 in the appendix. It is worth noting that the coefficient for the share of articles disclosing the origins of foreign-born offenders ($WDisclose_{lt}^{For}$) is also positive and significant. This provides suggestive evidence and support for previous findings by Couttenier et al. (2021) that priming regarding foreign criminality could cause attitudes towards immigration to deteriorate. Finally, we replicate the 2SLS analysis but focus on the different types of crimes listed in Table 8. For all crimes except immigration-related crimes, we find that the Sächsische Zeitung policy shift in July 2016 improved natives' attitudes towards immigration in the area of distribution for the paper.

Table 8 – 2SLS Regressions, Types of Crime

	(1) Murder	(2) Assault	(3) Theft	(4) Sexual	(5) Drugs	(6) Immigration	(7) Terrorism
$WDisclose_{lt}^{Ger}$	-0.453***	-0.672**	-1.071***	-0.663***	-0.474**	-4.804	-0.729***
	(0.155)	(0.272)	(0.321)	(0.170)	(0.180)	(8.019)	(0.263)
Nb. Observations	109,885	109,885	109,885	109,885	109,885	109,885	109,885
KP F-test	173.644	51.944	896.743	540.017	77.188	0.507	107.145
First stage	0.124***	0.083***	0.052***	0.085***	0.118***	0.012	0.077***
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.343	0.343

Note: The dependent variable is a dummy variable for "very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3) and Year-Month $\times$ Regional fixed effects. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration on SOEP.

4.5 Concerns about crime

The estimates in Subsection 4.3 indicate a significant effect of the treatment on crime concerns. This confirms the initial assumption that immigration and crime are both first-order issues that are often considered jointly in peoples' minds, as natives tend to blame immigrants for crime. The positive coefficient for crime may be interpreted as a priming effect, indicating that increasing the share of articles in which natives are portrayed as criminals increases the salience of criminality overall

[69] These estimates are available upon request from the authors.

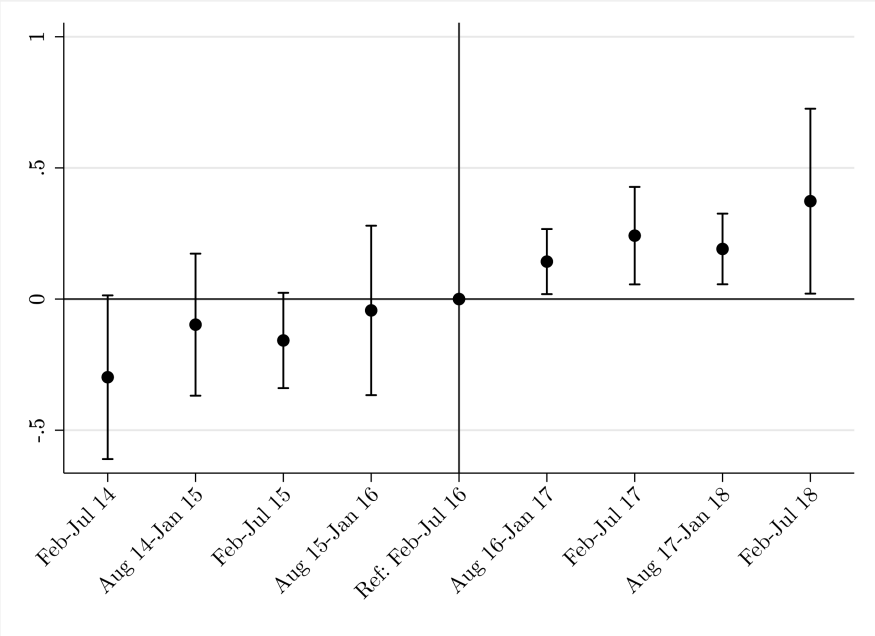
Table 9 – OLS Regressions, Baseline Estimates
Concerns about Crime

	(1)	(2)	(3)	(4)	(5)	(6)
E_l^{SZ}	0.031**	-0.032				
	(0.012)	(0.020)				
$July16_t$	0.032***					
	(0.009)					
$July16_t \times E_l^{SZ}$	0.150***	0.070***	0.070***	0.069***	0.071***	0.051
	(0.012)	(0.016)	(0.019)	(0.020)	(0.009)	(0.031)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes
Indiv. Controls $\times July16_t$	No	No	No	Yes	Yes	Yes
District $\times$ linear time trend	No	No	No	No	Yes	No
Individual FE	No	No	No	No	No	Yes
Nb. Observations	110,364	110,364	110,364	110,364	110,364	104,866
Adjusted R^2	0.046	0.080	0.098	0.099	0.100	0.479
Average $Attitudes_{ilt}$	0.342	0.342	0.342	0.342	0.342	0.345

Note: The dependent variable is a dummy variable for "Very concerned" about crime. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

and translates into additional concerns about crime, as natives outnumber foreign-born individuals. We test the robustness of this result by replicating the benchmark estimates for the reduced-form and the 2SLS estimations in Tables 9 and 10, respectively. Except for the two most demanding specifications that include individual fixed effects, the coefficient of interest is always positive and significant at the five or one percent level. As for immigration concerns, the event analysis in Figure 5 supports the hypothesis of no trends in the pre-treatment period. The positive effect of the treatment on concerns about crime suggests that following the increase in priming regarding native criminality, native respondents uncouple the issues of crime and immigration (blaming immigrants less for crime) and therefore become more likely to identify crime as a central issue in its own right.

Figure 5 – OLS Regressions, Event Analysis
Concerns about Crime



Notes: This graph depicts changes in natives' attitudes towards crime in the distribution area of the Sächsische Zeitung before and after the policy change. Effects are grouped in bins of six months. The reference period is the period February-July 2016. Coefficients are obtained from a regression in which the dependent variable is a dummy variable for being "very concerned" about crime. It includes the full vector of individual controls (age, marital status, education, employment status and individual earnings), the full vector of district-year controls (unemployment rate, share of social transfer recipients, share of refugees, net migration flows for native and foreign-born individuals, crime rate in the overall population, and share of crimes involving foreign-born individuals in total crimes), as well as district (NUTS-3) and year-month $\times$ region fixed effects, and a district-level linear time trend. Standard errors are clustered at the commuting zone level. Confidence intervals are reported at the 95% level.
Source: Authors' elaboration on SOEP data.

4.6 Estimates at the municipality level

The estimates reported so far are based on differences in the penetration of the Sächsische Zeitung across German districts (NUTS-3). However, as depicted in Figure 3, the area of distribution for the Sächsische Zeitung is mainly limited to the state of Saxony. Thus, we investigate the robustness of the previous results, showing that our conclusions remain unchanged when considering municipalities (*"Gemeinde"*, LAU-2) as the level of treatment. It is worth noting that munici-

Table 10 – 2SLS Regressions, Baseline Estimates
Concerns about Crime

	(1) All	(2) All	(3) >50%	(4) >50%	(5) All	(6) All
$WDisclose_{lt}$	0.972	0.662	1.563***	1.125***		
	(0.696)	(0.672)	(0.278)	(0.145)		
$WDisclose_{lt}^{Ger}$					0.970**	0.658
					(0.393)	(0.479)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month × Regional	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes	No	Yes
Nb. Observations	109,781	104,288	36,969	35,010	109,781	104,288
KP F-test	4.932	5.762	267.597	389.777	55.062	61.417
First stage	0.072**	0.077**	0.063***	0.074***	0.072***	0.078***
Average $Attitudes_{ilt}$	0.395	0.397	0.407	0.409	0.395	0.397

Note: The dependent variable is a dummy variable for "Very concerned" about crime. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

palities are not our preferred level of analysis because working at this level increases the likelihood that our empirical analysis violates the SUTVA assumption. Indeed, the municipality in Germany is the lowest level of administration, and it seems unlikely that information (provided by the press or by interpersonal contacts) does not travel across neighbouring municipalities. By construction, this increases the likelihood that attitudes towards immigration for a given respondent vary with the treatment intensity assigned to other individuals in neighbouring municipalities. Using variation at the district level reduces the likelihood of such spillovers. In addition, working at the municipality level does not substantially increase the variability in our dataset, as 78 percent of the variation in exposure, as measured by the standard deviation of the Sächsische Zeitung distribution share, is due to variation between districts and only 22 percent is attributable to variation within districts, i.e., between municipalities in the same district. It also implies that some units of observations contain few individuals.

Table G1 in the appendix reports the results of our benchmark estimates at the municipality level with the same fixed effects and control variable structure as before. We take advantage of this lower level of analysis to test the robustness of our results to alternative fixed effects structures, including year-month × government region (NUTS-2) fixed effects in Table G2 and year-month × district (NUTS-3) fixed effects in Table G3.[70] In most specifications, our coefficient of interest remains highly significant and of the same order of magnitude as before. This reinforces the causal interpretation of the effect of priming regarding native criminality on natives' attitudes towards immigration.

5 Conclusions

In this paper, we estimate how a reporting policy that systematically discloses the origins of criminals impacts natives' attitudes towards immigration as measured with an individual survey from the German Socio-Economic Panel. Specifically, we use the unilateral shift in reporting policy made by the Sächsische Zeitung in July 2016 as a natural experiment to analyze the causal impact of crime reporting policies on natives' concerns about immigration.

To do so, we employ text analysis methods on 402,819 crime-related articles collected between January 2014 and December 2018 in 25 German newspapers. This allows us to provide empirical evidence that the unexpected shift in reporting policy announced by the Sächsische Zeitung indeed created a positive differential in origin disclosure with other newspapers of approximately eight percentage points after July 2016. Notably, we find that the policy translated into a disproportionate priming regarding native criminality. Along with data on local market shares for each newspaper, we compute the relative degree of exposure of individuals to the Sächsische Zeitung and to other newspapers at the monthly level. Using a reduced-form analysis with a difference-in-differences estimator, where the treatment is defined as the 2014 relative share of Sächsische Zeitung sales in the total number of newspaper sales for each German locality, we find that systematically disclosing the origins of criminals reduces natives' concerns about immigration and their electoral support for far-right parties. We estimate the persuasion rate of this policy to be around 2.60%. Conversely, this policy translates into higher concerns about crime since attitudes about immigration and crime are jointly determined. Thus, priming about native criminality helps to break the implicit relationship

[70] It is worth noting that the year-month × district (NUTS-3) fixed effects in Table G3 absorb the effect of the annual district controls included in all previous estimates.

between immigration and criminality in people's minds. Our results are further validated by the IV-2SLS estimates in which the monthly share of articles revealing the origin of the offender in most serious crimes for a given locality across 25 widely circulated newspapers and the shift in reporting policy implemented by the Sächsische Zeitung in July 2016 are used as instruments.

Our results echo the literature on media's impact on individuals' beliefs and attitudes with additional policy implications. Indeed, this paper provides evidence that when linking the coverage of two first-order issues such as criminality and immigration, the way media treat sensitive information and particularly the origins of offenders is crucial. Specifically, our results do not support the assumption underlying Article 12.1 of the German Press Code that systemically revealing a perpetrator's origin would result in discriminatory attitudes towards certain minorities. Instead, we show that the exact opposite policy, systematically mentioning the origins of criminals, particularly in regard to native offenders, significantly reduces natives' concerns about immigration. However, it is crucial to bear in mind that in the policy reform we evaluated, the newspaper Sächsische Zeitung clearly stated that it had no intention of stigmatizing foreign-born individuals, which was confirmed by our text analysis. A policy of systematically disclosing the origins of offenders could still be misused in other contexts, for example, when newspapers follow an anti-migration agenda. Future research, similar to Alesina et al. (2022), for instance, would have to confirm the extent to which the narrative behind immigration-related stories can shape natives' attitudes towards immigration beyond the tense debate surrounding crime.

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Appendix

A Data

Table A1 – Classification of Articles into ICCS Categories

Lexicons →	Murder				Assaults			
Factiva cat. ↓	No		Yes		No		Yes	
No	333,162	90.82 %	33,673	9.18%	192,915	52.72 %	173,007	47.28%
Yes	7,741	21.51%	28,243	78.49%	2,195	5.95 %	34,702	94.05%
Lexicons →	Sexual				Theft			
Factiva cat. ↓	No		Yes		No		Yes	
No	359,779	94.20%	22,143	5.80%	171,330	66.79%	85,199	33.21%
Yes	2,716	13.00%	18,181	87.00%	2,035	1.39%	144,255	98.61%
Lexicons →	Drugs				Immigration			
Factiva cat. ↓	No		Yes		No		Yes	
No	357,580	90.78 %	36,322	9.22 %	353,291	90.18 %	38,469	9.82 %
Yes	87	0.98%	8,830	99.02%	559	5.05 %	10,500	94.95%
Lexicons →	Terrorism							
Factiva cat. ↓	No		Yes					
No	354,716	92.72%	27,867	7.28 %				
Yes	1,459	7.21%	18,777	92.79%				

Note: Factiva categories correspond to the crime categories provided by the Dow Jones Factiva archives. Lexicons correspond to our seven lexicons build for each main ICCS classification. Yes (No) reports the number of articles that are (not) identified as belonging to a given type of crime.
Source: Authors' elaboration on Dow Jones Factiva archives.

Table A2 – Crime Categories

Category	Short description	ICCS section(s)	ICCS description	Nb. articles	Share.
1	Murder	01	Acts leading to death or intending to cause death.	61,916	15.37
2	Assault - Threats	02	Acts leading to harm or intending to cause harm to the person.	207,709	51.56
3	Sexual violence	03	Injurious acts of a sexual nature.	40,324	10.01
4	Theft - Burglary - Robbery - Vandalism	05	Acts against property involving violence or threat against a person-Acts against property only.	229,454	56.96
5	Drugs	06	Acts involving controlled psychoactive substances or other drugs.	45,152	11.21
6	Human trafficking - Smuggling - Illegal Immigration	0805	Acts related to migration.	48,969	12.16
7	Terrorism	0906	Terrorism.	46,644	11.58
			Total:	402,819	100%

Notes: Nb. Articles and Share are the total number and the share of articles in each of the non-mutually exclusive categories in the baseline sample, respectively.
Source: Authors' elaboration on International Classification of crime for Statistical Purpose (ICCS, v1.0). United Nations Office on Drugs and Crime and on Dow Jones Factiva archives.

Table A3 – Sources and Definitions

Variable	Definition	Source
$Attitudes_{i,l,t}$	Dummy variable. Concerns about immigration (Very concerned (1); Somewhat concerned or not concerned at all (0))	SOEP
E_l^{SZ}	Exposure to the Sächsische Zeitung as defined in Eq. (3).	Authors' calculations based on IVW.
$WDisclose_{lt}$	Weighted share of articles disclosing the origins of offenders as defined in Eq. (5).	Authors' calculations based on IVW and Dow Jones Factiva archives.
Age	Categorical variable (18-24 (1); 25-34(2); 35-44(3); 45-54(4); 55-64(5), 65+(6))	SOEP
Education	Categorical variable. (Less than high-school (1), High-school graduates (2); College graduates (3))	SOEP
Married	Dummy variable. (Married (1); Single (0))	SOEP
Employed	Dummy variable. (Employed (1); Not employed (0))	SOEP
Log Indiv. earnings	Individual labor earnings (hyperbolic sine transformation)	SOEP
Unemployment rate	Number unemployed persons each year, divided by the population active in the local labor market in the same year.	Federal Statistical Office.
Share of social transfer recipients	Number of beneficiaries of social transfers in a locality as of December 31 of each year, divided by the total population in the locality in the same year.	Federal Statistical Office.
Share of refugees	Number of refugees residing in the locality as of December 31 of each year, divided by the total population in the locality in the same year.	Federal Statistical Office.
Net internal migration flows % tot pop.	Immigration from other German municipalities – Emigration to other German municipalities, divided by the total population in the locality in the same year.	Federal Statistical Office.
Net international migration flows % tot pop	Immigration from abroad – Emigration abroad, divided by the total population in the locality in the same year.	Federal Statistical Office.
Crimes per 100,000 inhabitants (log)	Number of crimes recorded in the district, adjusted for population size.	Federal Statistical Office.
Share of foreigners in total crime	Number of recorded crimes committed by non-Germans in the district, divided by the total population in the locality in the same year.	Federal Statistical Office.

Source: Authors' elaboration.

Table A4 – Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.
$Attitudes_{i,l,t}$	0.342	0.475	0	1
E_l^{SZ}	0.014	0.090	0	0.719
$WDisclose_{lt}$	0.153	0.129	0.011	0.696
$WDisclose_{lt}^{Ger}$	0.054	0.048	0.002	0.282
Age	3.934	1.587	1	6
Education	2.140	0.615	1	3
Married	0.605	0.489	0	1
Employed	0.643	0.479	0	1
Log Indiv. earnings	7.225	5.022	0	14.691
Unemployment rate	6.873	3.022	1.400	17.100
Share of social transfer recipients	0.004	0.002	0	0.015
Net migration flows (foreign born)	0.015	0.015	-0.026	0.325
Net migration flows (natives)	-0.001	0.004	-0.015	0.023
Share of refugees	0.016	0.009	0.000	0.130
Share of foreigners in total crime	30.659	13.164	3.600	96.900
Crime per 100,000 inhabitants (log)	9.448	0.454	8.388	12.245
Nb. observations:	110,364			

Source: Authors' elaboration on Dow Jones Factiva archives and SOEP.

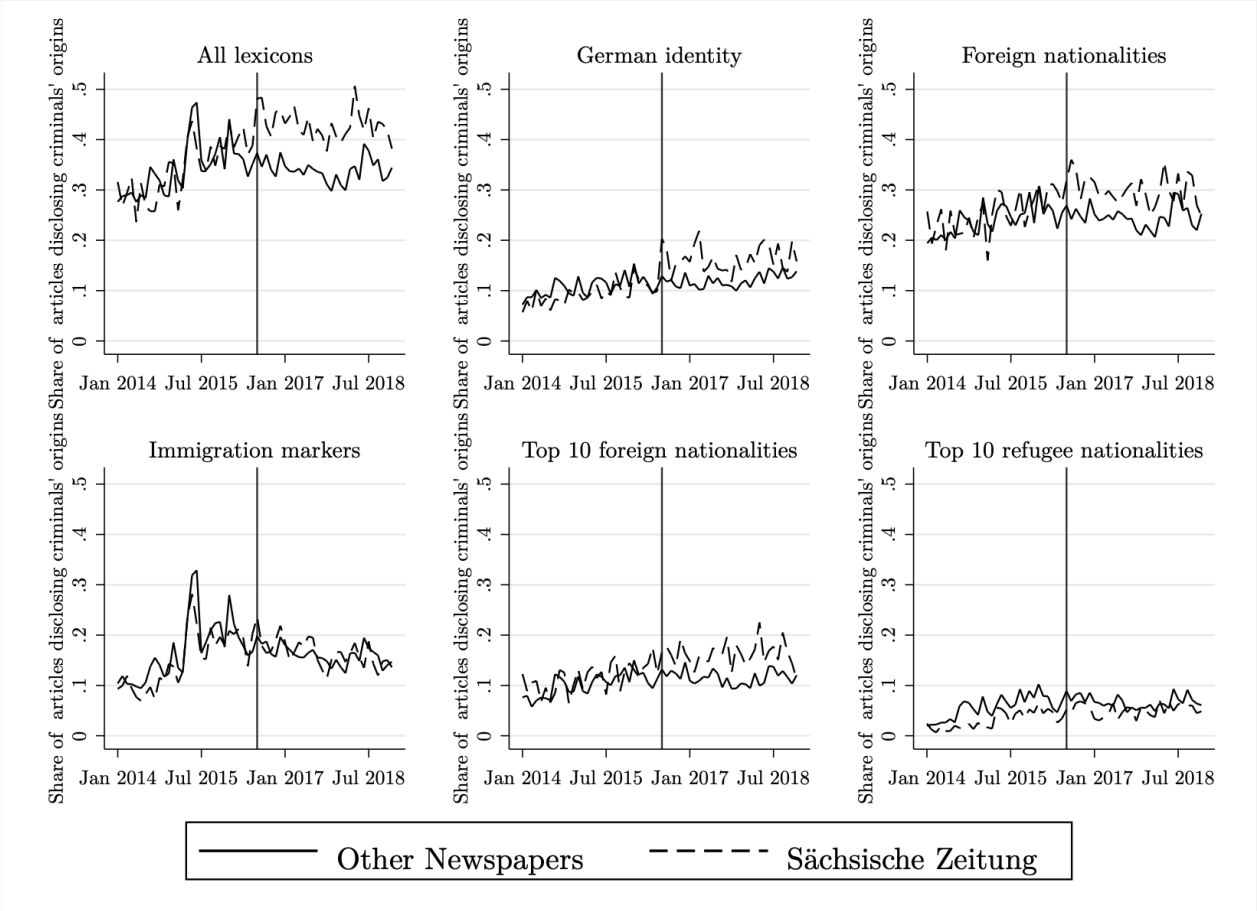
B Additional Robustness Checks on The Natural Experiment

Table B1 – Mean Comparisons in Disclosure Policies before and after July 2016.

	% Mean Before July 2016	% Mean After July 2016	Diff. in mean (%)	Diff. in mean (p-value)
All newspapers:				
$Disclose_{n,t}$	34.23	38.52	4.29	0.000
$Disclose_{n,t}^{Ger}$	10.03	14.18	4.15	0.000
$Disclose_{n,t}^{For}$	24.65	27.31	2.66	0.000
$Disclose_{n,t}^{Mig}$	16.06	16.34	0.28	0.740
$Disclose_{n,t}^{Top}$	10.88	13.88	3.00	0.000
$Disclose_{n,t}^{Ref}$	4.39	5.93	1.55	0.000
Sächsische Zeitung:				
$Disclose_{n,t}$	34.18	43.01	8.82	0.000
$Disclose_{n,t}^{Ger}$	9.33	16.34	7.01	0.000
$Disclose_{n,t}^{For}$	25.24	30.03	4.79	0.000
$Disclose_{n,t}^{Mig}$	15.06	16.55	1.49	0.201
$Disclose_{n,t}^{Top}$	11.55	16.20	4.65	0.000
$Disclose_{n,t}^{Ref}$	3.06	5.13	2.07	0.000
Other newspapers:				
$Disclose_{n,t}$	34.27	34.03	-0.24	0.820
$Disclose_{n,t}^{Ger}$	10.72	12.01	1.29	0.003
$Disclose_{n,t}^{For}$	24.05	24.59	0.53	0.433
$Disclose_{n,t}^{Mig}$	17.06	16.12	- 0.94	0.442
$Disclose_{n,t}^{Top}$	10.21	11.56	1.36	0.007
$Disclose_{n,t}^{Ref}$	5.71	6.74	1.03	0.036

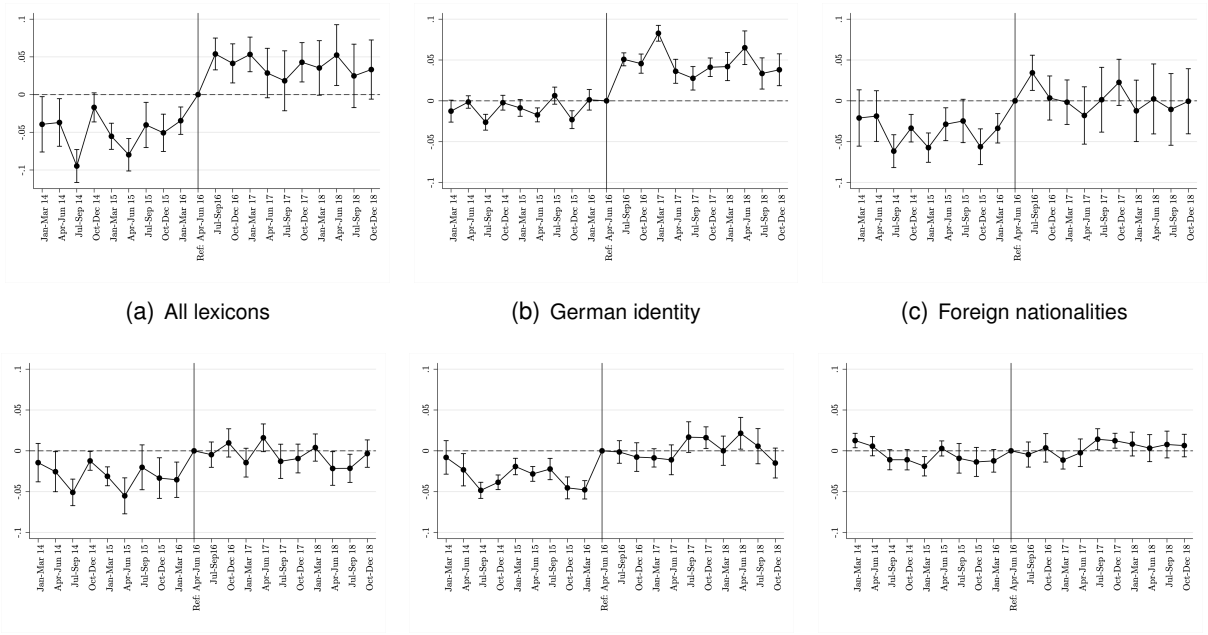
Notes: T-tests on the equality of means. $Disclose_{n,t}$ is the share of articles that disclose criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. It is first computed for all origins and then for the lexicons described in Subsection 2.2, namely, Germans ($Disclose_{n,t}^{Ger}$), foreign nationalities ($Disclose_{n,t}^{For}$), immigration markers ($Disclose_{n,t}^{Mig}$), top 10 foreign nationalities ($Disclose_{n,t}^{Top}$), and top 10 refugee nationalities ($Disclose_{n,t}^{Ref}$).
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure B1 – Share of Crime-related Articles Disclosing Criminals' Origins
Alternative lexicons



Note: This graph depicts the variable $Disclose_{n,t}$, which is the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. All newspapers except the Sächsische Zeitung are pooled together.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure B2 – Changes in Reporting Policy over time
Alternative Lexicons

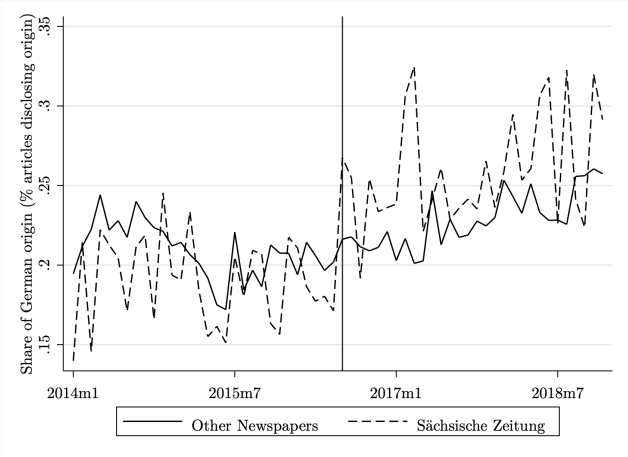


(a) All lexicons (b) German identity (c) Foreign nationalities

(d) Immigration markers (e) Top 10 foreign nationalities (f) Top 10 refugee nationalities

Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t for a particular subset of origins. Each coefficient corresponds to the interaction between the SZ_n variable and a given quarter before and after July 2016. The first quarter before July 2016 (April 2016 to June 2016) is the omitted category. We include 95 percent confidence intervals around the estimated coefficients. Standard errors are clustered at the newspaper level. All estimates include newspaper and year-quarter fixed effects and are weighted by the total number of crime-related articles published by each newspaper. The same graph is obtained without weights and available upon request.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure B3 – Share of German Origin in Crime-related Articles Disclosing Criminals' Origins



Note: This graph depicts the share of articles reporting a German origin, conditional on disclosing criminals' origins. All newspapers except the Sächsische Zeitung are pooled together.
Source: Authors' elaboration on Dow Jones Factiva archives.

Table B2 – Change in Reporting Policy for Native Criminals,
Robustness by Type of Crime

	(1) Assaults	(2) Murder	(3) Theft	(4) Sexual	(5) Drugs	(6) Immig.	(7) Terrorism
$SZ_n \times July16_t$	0.067***	0.065***	0.044***	0.066***	0.119***	-0.004	0.044***
	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)
Newspaper FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nb. Articles weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nb. Observations	1,472	1,465	1,473	1,464	1,466	1,464	1,463
Adjusted R^2	0.674	0.403	0.666	0.282	0.453	0.299	0.364
$AverageDisclose_{n,t}$	0.169	0.183	0.117	0.167	0.174	0.278	0.284

Note: The dependent variable is $Disclose^{Ger}_{n,t}$, the share of i articles disclosing native criminals relative to the total number of crime-related articles in newspaper n at year-month t. $July16_t$ is a dummy variable equal to one after July 2016 and zero before, and SZ_n a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. Robust standard errors clustered at the newspaper level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on Dow Jones Factiva archives.

Table B3 – Change in Reporting Policy, Alternative clustering
German identity

Cluster:	(1) Region	(2) Newspaper	(3) Region-Year	(4) Newspaper-Year	(5) Newspaper-Year-Month	(6) Bootstrap
$SZ_n \times July16_t$	0.055***	0.055***	0.055***	0.055***	0.055***	0.051***
	(0.006)	(0.006)	(0.004)	(0.004)	(0.006)	(0.007)
Wild bootstrap (Webb weights):						
T-Stat	8.611	9.898	13.645	14.3451	9.376	
P-value	0.337	0.281	0.003	0.004	0.000	
Nb. clusters	14	25	69	124	818	-
Replications	999	999	999	999	999	999
Newspaper FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Nb. Articles weights	Yes	Yes	Yes	Yes	Yes	No
Nb. Observations	1,475	1475	1,475	1,475	1,475	1,475
Adjusted R^2	0.784	0.784	0.784	0.784	0.784	0.690
$AverageDisclose_{n,t}$	0.151	0.151	0.151	0.151	0.151	0.151

Note: The dependent variable is $Disclose^{Ger}_{n,t}$, the share of i articles disclosing native criminals relative to the total number of crime-related articles in newspaper n at year-month t. $July16_t$ is a dummy variable equal to one after July 2016 and zero before, and SZ_n a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. Standard errors are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

C Alternative treatment date

This appendix provides additional estimates, which provides evidence that our results are robust to the choice of the treatment date. We first report additional robustness checks that use phantom treatments to show that the Sächsische Zeitung plausibly started to depart from Article 12.1 in April, 2016 but not before. Then, we show that our main conclusions are not affected by using this *de facto* treatment as the treatment date instead of the *de jure* treatment of July, 2016. Finally, we report below additional estimates with i) April, 2016 as the treatment date or ii) removing the April to June, 2016 period from the analysis. We first replicate the benchmark results reported in Section 3 and then the benchmark results reported in Section 4.

Phantom treatments

Table C1 – Phantom treatment before July 2016

	(1)	(2)	(3)	(4)
Time span:	All	January 2014 to June 2016.		
$SZ_n \times July16_t$	0.084***			
	(0.014)			
$SZ_n \times Aug14_t$		-0.001		
		(0.017)		
$SZ_n \times Mar15_t$			0.015	
			(0.010)	
$SZ_n \times Nov15_t$				0.034***
				(0.009)
Newspaper FE	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
Nb. Articles weights	Yes	Yes	Yes	Yes
Nb. Observations	1,475	725	725	725
Adjusted R^2	0.827	0.867	0.867	0.868

Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. $Month16_t$ is a dummy variable equal to one after a given month and zero before, and SZ_n a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. Robust standard errors clustered at the newspaper level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The period of analysis goes from January 2014 to June 2016.
Source: Authors' elaboration on Dow Jones Factiva archives.

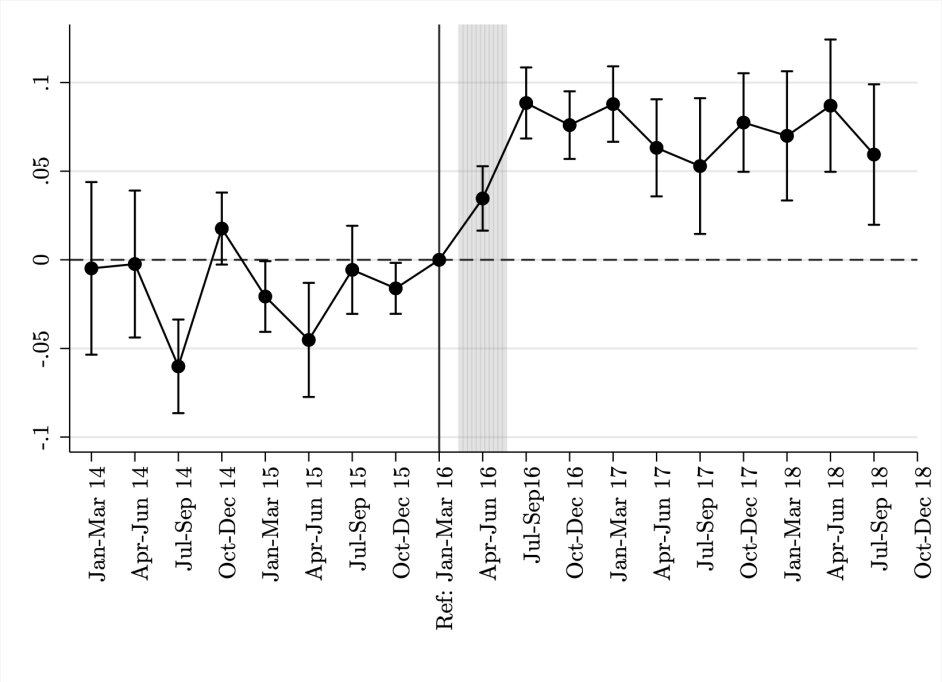
Table C2 – Phantom treatment before April 2016

Time span:	(1) All	(2) January 2014 to March 2016.	(3)	(4)
$SZ_n \times Jun16_t$	0.084***			
	(0.014)			
$SZ_n \times Feb15_t$		-0.011		
		(0.018)		
$SZ_n \times Jan15_t$			-0.001	
			(0.011)	
$SZ_n \times Aug15_t$				0.021
				(0.012)
Newspaper FE	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
Nb. Articles weights	Yes	Yes	Yes	Yes
Nb. Observations	1,475	650	650	650
Adjusted R^2	0.827	0.865	0.865	0.865

Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. $Month16_t$ is a dummy variable equal to one after a given month and zero before, and SZ_n a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. Robust standard errors clustered at the newspaper level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The period of analysis goes from January 2014 to March 2016.
Source: Authors' elaboration on Dow Jones Factiva archives.

April, 2016 is the treatment date

Figure C1 – Changes in Reporting Policy - Event analysis
April 2016 is the treatment date



Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. Each coefficient corresponds to the interaction between the SZ_n variable and a given quarter before and after April 2016. The first quarter before April 2016 (January 2016 to March 2016) is the omitted category. We include 95 percent confidence intervals around the estimated coefficients. Standard errors are clustered at the newspaper level. All estimates include newspaper and year-quarter fixed effects and are weighted by the total number of crime-related articles published by each newspaper.
Source: Authors' elaboration on Dow Jones Factiva archives.

Table C3 – Change in Reporting Policy
April 2016 is the treatment date

	(1) All	(2) All	(3) All	(4) Victims	(5) Perpetrators
SZ_n	-0.011				
	(0.032)				
$July16_t$	-0.010				
	(0.016)				
$SZ_n \times April16_t$	0.098***	0.085***	0.083***	0.002	0.018***
	(0.016)	(0.013)	(0.010)	(0.003)	(0.003)
Newspaper FE	No	Yes	Yes	Yes	Yes
Year-month FE	No	Yes	Yes	Yes	Yes
Newspaper $\times$ linear time trend	No	No	Yes	No	No
Nb. Articles weights	Yes	Yes	Yes	Yes	Yes
Nb. Observations	1,475	1,475	1,475	1,475	1,475
Adjusted R^2	0.017	0.827	0.884	0.622	0.623
$AverageDisclose_{n,t}$	0.418	0.418	0.418	0.077	0.115

Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. $April16_t$ is a dummy variable equal to one after April 2016 and zero before, and SZ_n a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. Robust standard errors clustered at the newspaper level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on Dow Jones Factiva archives.

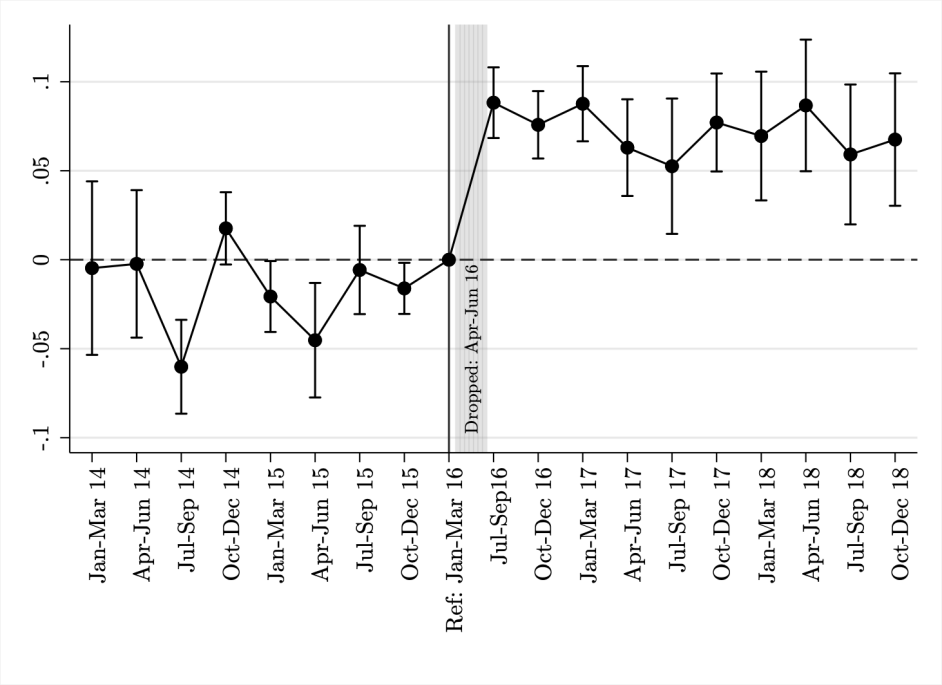
Table C4 – Benchmark OLS/2SLS Regressions
April 2016 is the treatment date

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS >50%	(10) 2SLS >50%	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.120***	0.062***										
	(0.018)	(0.018)										
$April16_t$	0.012											
	(0.010)											
$April16_t \times E_l^{SZ}$	0.040***	-0.038***	-0.045***	-0.051***	-0.037	-0.032***						
	(0.013)	(0.005)	(0.004)	(0.004)	(0.044)	(0.008)						
$WDisclose_{lt}$							-0.615**	-0.366***	-0.473**	-0.279		
							(0.277)	(0.127)	(0.199)	(0.237)		
$WDisclose_{lt}^{Ger}$											-0.636***	-0.378***
											(0.082)	(0.095)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month × Regional	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	110,364	110,364	110,364	110,364	110,364	104,866	109,885	104,396	36,996	35,033	109,885	104,396
Adjusted R^2	0.046	0.080	0.098	0.099	0.100	0.479						
KP F-test							4.456	5.272	242.262	294.388	64.818	69.229
Average $Attitudes_{ilt}$	0.342	0.342	0.342	0.342	0.342	0.345	0.343	0.346	0.353	0.356	0.343	0.346

Note: The dependent variable is a dummy variable for very concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration on SOEP.

Removing the April to June, 2016 period

Figure C2 – Changes in Reporting Policy - Event analysis
April to June 2016 is removed



Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. Each coefficient corresponds to the interaction between the SZ_n variable and a given quarter before and after July 2016. The second quarter before April 2016 (January 2016 to March 2016) is the omitted category. The April to June 2016 period is removed from the analysis. We include 95 percent confidence intervals around the estimated coefficients. Standard errors are clustered at the newspaper level. All estimates include newspaper and year-quarter fixed effects and are weighted by the total number of crime-related articles published by each newspaper.
Source: Authors' elaboration on Dow Jones Factiva archives.

Table C5 – Change in Reporting Policy
April to June 2016 is removed

	(1) All	(2) All	(3) All	(4) Victims	(5) Perpetrators
SZ_n	-0.011				
	(0.032)				
$July16_t$	-0.011				
	(0.017)				
$SZ_n \times July16_t$	0.102***	0.089***	0.098***	0.002	0.019***
	(0.017)	(0.014)	(0.013)	(0.003)	(0.003)
Newspaper FE	No	Yes	Yes	Yes	Yes
Year-month FE	No	Yes	Yes	Yes	Yes
Newspaper $\times$ linear time trend	No	No	Yes	No	No
Nb. Articles weights	Yes	Yes	Yes	Yes	Yes
Nb. Observations	1,400	1,400	1,400	1,400	1,400
Adjusted R^2	0.018	0.827	0.885	0.621	0.627
$AverageDisclose_{n,t}$	0.419	0.419	0.419	0.077	0.114

Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t. $July16_t$ is a dummy variable equal to one after July 2016 and zero before, and SZ_n a dummy variable equal to one for the Sächsische Zeitung and zero otherwise. The April to June 2016 period is removed from the analysis. Robust standard errors clustered at the newspaper level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on Dow Jones Factiva archives.

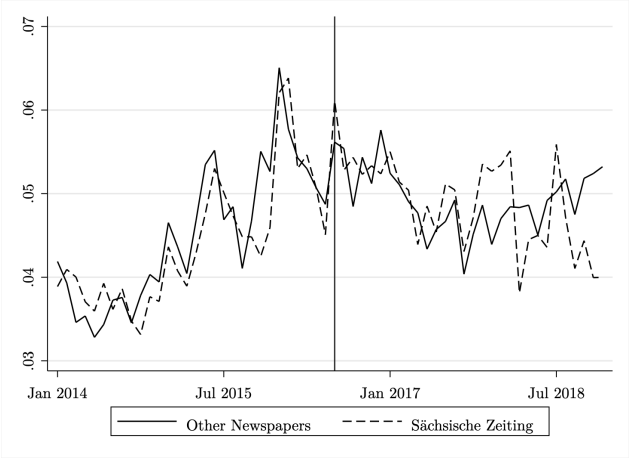
Table C6 – Benchmark OLS/2SLS Regressions
April to June 2016 is removed

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS $>50\%$	(10) 2SLS $>50\%$	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.123***	0.065***										
	(0.018)	(0.019)										
$July16_t$	0.003											
	(0.011)											
$July16_t \times E_l^{SZ}$	0.043***	-0.048***	-0.051***	-0.058***	-0.076***	-0.034***						
	(0.014)	(0.012)	(0.008)	(0.009)	(0.016)	(0.007)						
$WDisclose_{lt}$							-0.684*	-0.377***	-0.847***	-0.338		
							(0.410)	(0.126)	(0.225)	(0.215)		
$WDisclose_{lt}^{Ger}$											-0.695***	-0.383***
											(0.195)	(0.076)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District $\times$ linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	104,804	104,804	104,804	104,804	104,804	99,208	104,334	98,747	35,121	33,112	104,334	98,747
Adjusted R^2	0.046	0.082	0.099	0.100	0.102	0.481						
KP F-test							4.780	5.544	226.004	281.167	60.456	64.760
Average $Attitudes_{ilt}$	0.340	0.340	0.340	0.340	0.340	0.343	0.340	0.343	0.351	0.355	0.340	0.343

Note: The dependent variable is a dummy variable for very concerned about immigration. The April to June 2016 period is removed from the analysis. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

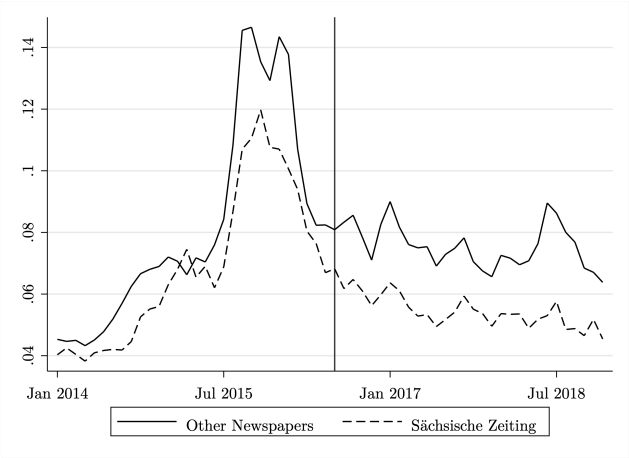
D Additional Changes for the Sächsische Zeitung

Figure D1 – Share of Crime-related Articles in the Total Number of Articles



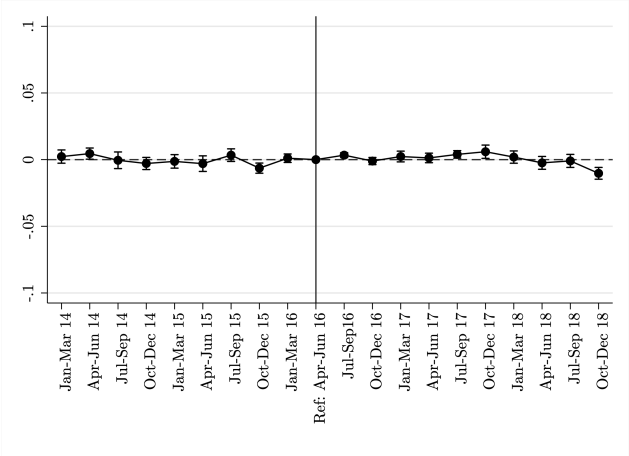
Note: This graph depicts the share of articles related to violent crimes as defined in Table A2 in the total number of articles published each month.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure D2 – Share of Immigration-related Articles in the Total Number of Articles



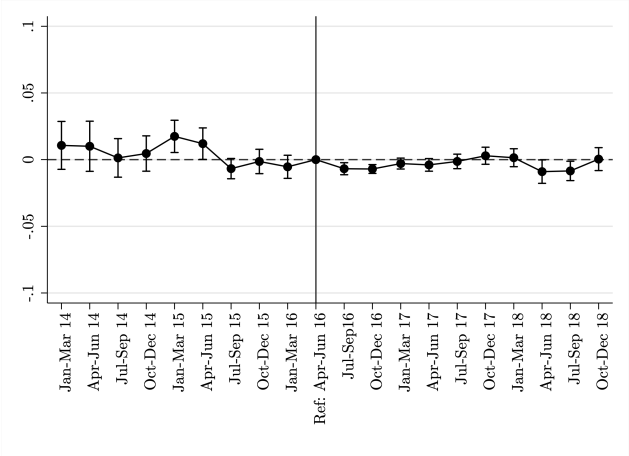
Note: This graph depicts the share of articles related to immigration in the total number of articles published each month.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure D3 – Share of Crime-related Articles in the Total Number of Articles



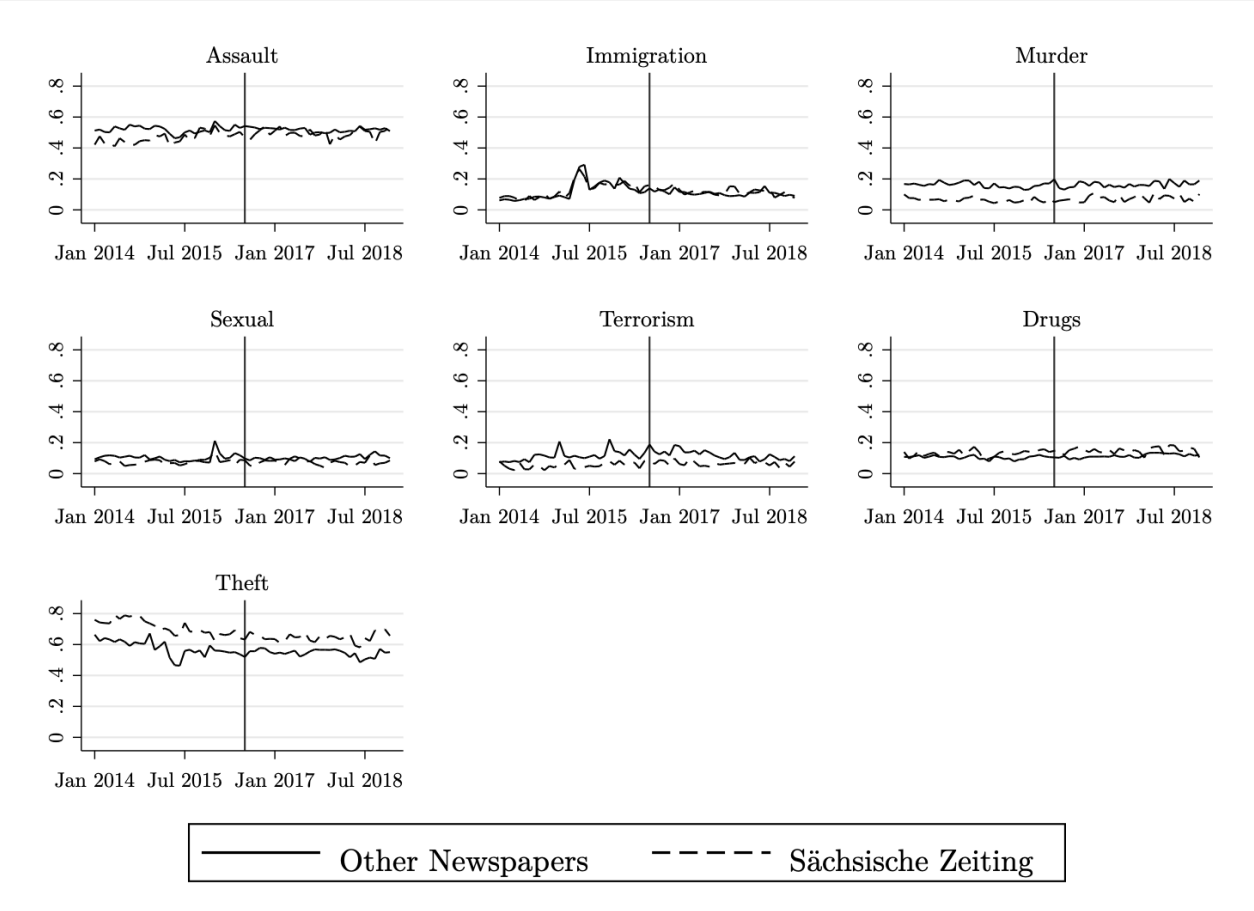
Note: The dependent variable is share of articles related to violent crimes as defined in Table A2 in the total number of articles published each month. Each coefficient corresponds to the interaction between the SZ_n variable and a given quarter before and after July 2016. The first quarter before July 2016 (April 2016 to June 2016) is the omitted category. We include 95 percent confidence intervals around the estimated coefficients. Standard errors are clustered at the newspaper level. All estimates include newspaper and year-quarter fixed effects and are weighted by the total number of crime-related articles published by each newspaper. The same graph is obtained without weights and available upon request.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure D4 – Share of Immigration-related Articles in the Total Number of Articles



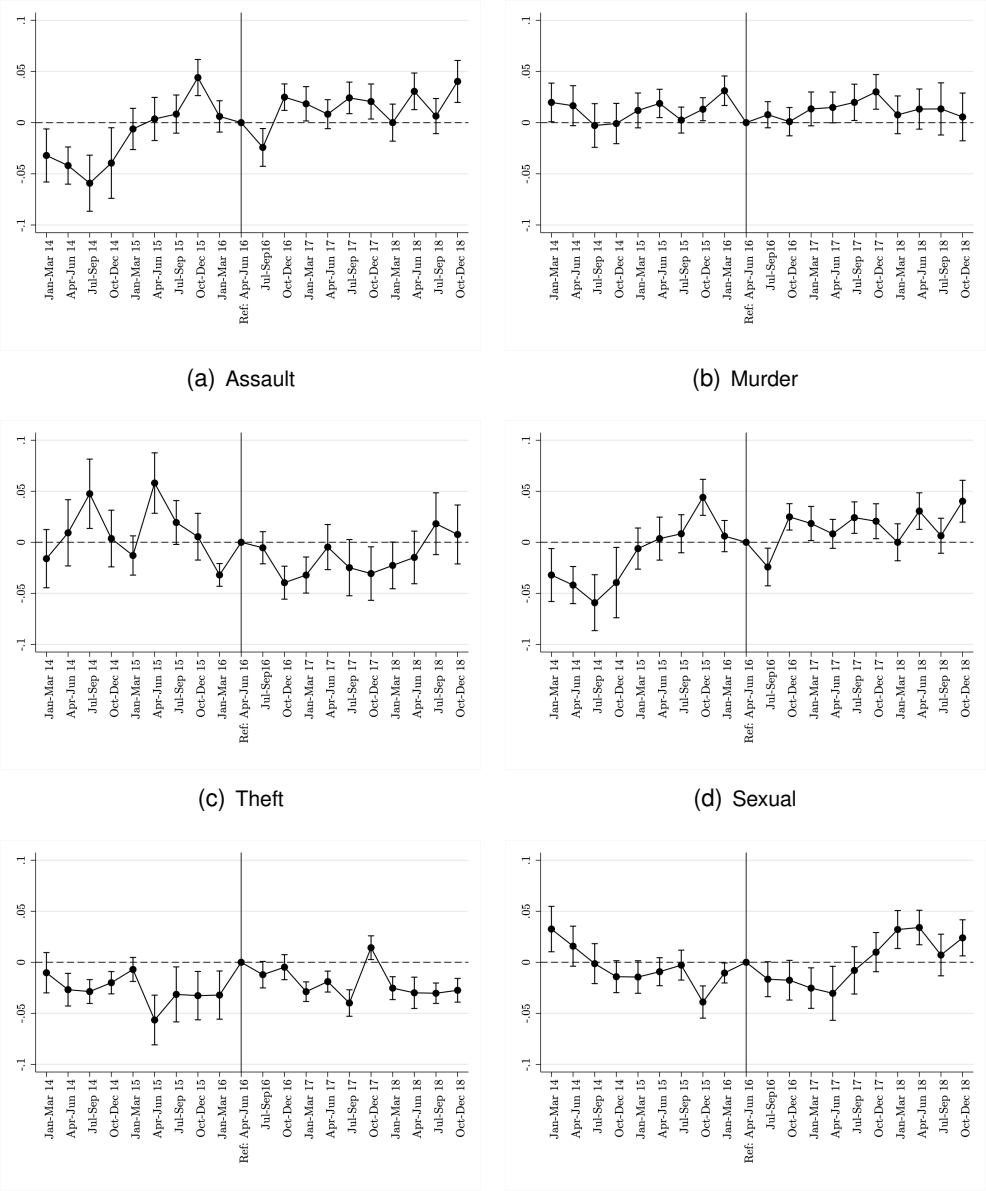
Note: The dependent variable is share of articles related to immigration in the total number of articles published each month. Each coefficient corresponds to the interaction between the SZ_n variable and a given quarter before and after July 2016. The first quarter before July 2016 (April 2016 to June 2016) is the omitted category. We include 95 percent confidence intervals around the estimated coefficients. Standard errors are clustered at the newspaper level. All estimates include newspaper and year-quarter fixed effects and are weighted by the total number of crime-related articles published by each newspaper. The same graph is obtained without weights and available upon request.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure D5 – Change in the Reporting of the Sächsische Zeitung across various types of crime



Note: The figure reports the difference in the share of articles reporting a specific type of crime between the Sächsische Zeitung and other newspapers.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure D6 – Changes in Reporting Policy over time
across various types of crime



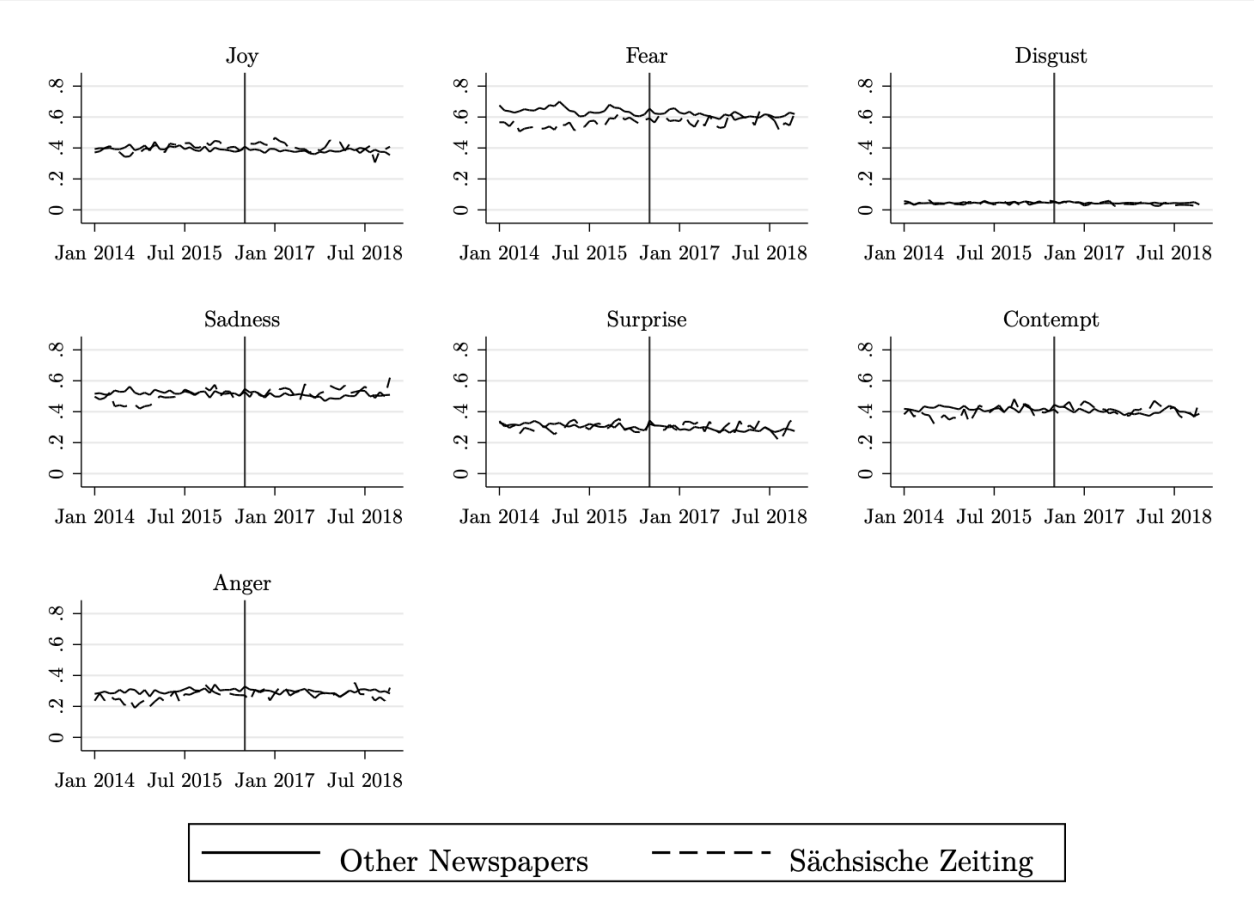
(a) Assault (b) Murder

(c) Theft (d) Sexual

(e) Immigration (f) Terrorism

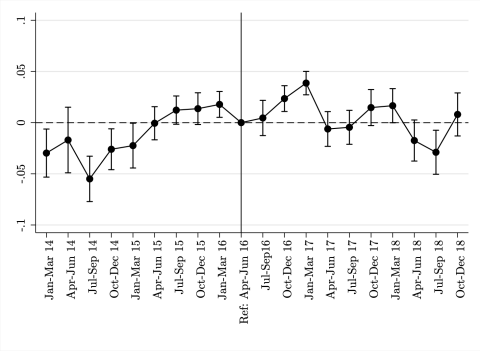
Note: The dependent variable is $Disclose_{n,t}$, the share of i articles disclosing criminals' origins relative to the total number of crime-related articles in newspaper n at year-month t for a particular subset of origins. Each coefficient corresponds to the interaction between the SZ_n variable and a given quarter before and after July 2016. The first quarter before July 2016 (April 2016 to June 2016) is the omitted category. We include 95 percent confidence intervals around the estimated coefficients. Standard errors are clustered at the newspaper level. All estimates include newspaper and year-quarter fixed effects and are weighted by the total number of crime-related articles published by each newspaper. The same graph is obtained without weights and available upon request.
Source: Authors' elaboration on Dow Jones Factiva archives.

Figure D7 – Change in the Tone of the Sächsische Zeitung
across various types of emotions

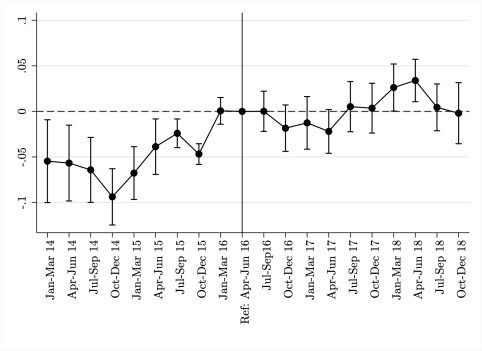


Note: The figure reports the difference in the share of articles reporting a specific emotion between the Sächsische Zeitung and other newspapers.
Source: Authors' elaboration on Dow Jones Factiva archives.

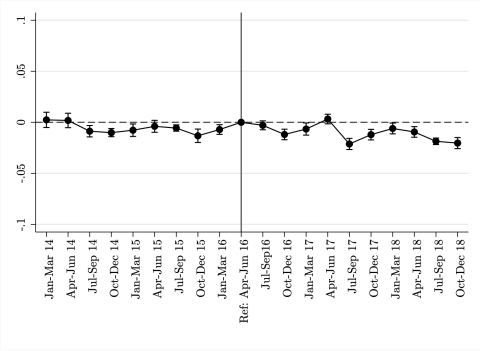
Figure D8 – Change in the Tone of the Sächsische Zeitung
across various types of emotions



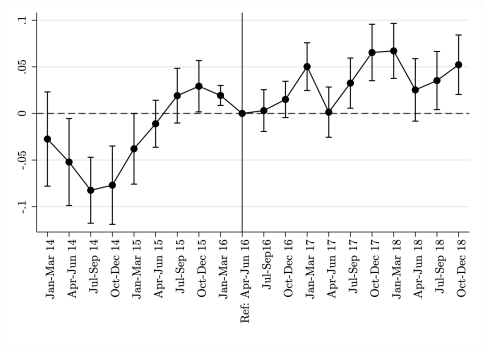
(a) Joy



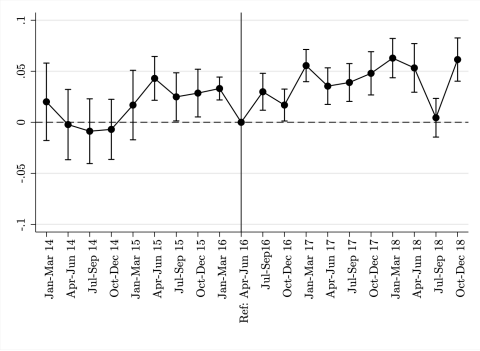
(b) Fear



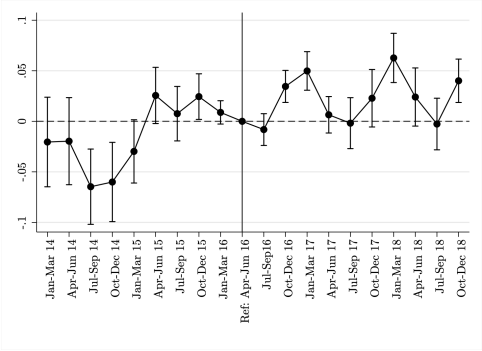
(c) Disgust



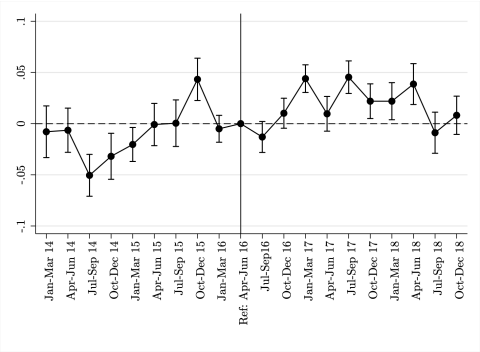
(d) Sadness



(e) Surprise



(f) Contempt



(g) Anger

E Additional Robustness Checks on Media Reporting and Natives' Attitudes Towards immigration

Table E1 – OLS Regressions, Restricted Sample
Eliminating localities with coverage $< 50\%$

	(1)	(2)	(3)	(4)	(5)	(6)
E_l^{SZ}	0.104***	0.081**				
	(0.025)	(0.035)				
$July16_t$	-0.023					
	(0.016)					
$July16_t \times E_l^{SZ}$	0.059**	-0.086***	-0.074***	-0.085***	-0.135***	-0.045***
	(0.023)	(0.014)	(0.013)	(0.013)	(0.010)	(0.016)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. Controls $\times July16_t$	No	No	No	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes
District $\times$ linear time trend	No	No	No	No	Yes	No
Individual FE	No	No	No	No	No	Yes
Nb. Observations	37,507	37,474	37,474	37,474	37,474	35,489
Adjusted R^2	0.060	0.100	0.109	0.110	0.111	0.498
Average $Attitudes_{ilt}$	0.352	0.352	0.352	0.352	0.352	0.355

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table E2 – OLS Regressions, East Germany Only

	(1)	(2)	(3)	(4)	(5)	(6)
E_l^{SZ}	0.018	0.066*				
	(0.034)	(0.036)				
$July16_t$	0.022					
	(0.016)					
$July16_t \times E_l^{SZ}$	-0.027**	-0.054***	-0.059***	-0.070***	-0.095**	-0.049***
	(0.010)	(0.016)	(0.014)	(0.015)	(0.033)	(0.006)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes
Indiv. Controls $\times July16_t$	No	No	No	Yes	Yes	Yes
District $\times$ linear time trend	No	No	No	No	Yes	No
Individual FE	No	No	No	No	No	Yes
Nb. Observations	23,743	23,743	23,743	23,743	23,743	22,698
Adjusted R^2	0.045	0.080	0.103	0.105	0.106	0.486
Average $Attitudes_{ilt}$	0.422	0.422	0.422	0.422	0.422	0.426

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table E3 – OLS Regressions, Phantom Treatment

Time span:	(1) All	(2) Jan. 2014 to June. 2016	(3)	(4)
$July16_t \times E_l^{SZ}$	-0.078***			
	(0.024)			
$Aug14_t \times E_l^{SZ}$		-0.100*		
		(0.056)		
$Mar15_t \times E_l^{SZ}$			-0.007	
			(0.020)	
$Nov15_t \times E_l^{SZ}$				-0.011
				(0.106)
Indiv. Controls	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	Yes	Yes	Yes	Yes
District $\times$ linear time trend	Yes	Yes	Yes	Yes
Nb. Observations	110,364	62,402	62,402	62,402
Adjusted R^2	0.100	0.107	0.107	0.107
Average $Attitudes_{ilt}$	0.342	0.339	0.339	0.339

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3), Year-Month $\times$ Regional fixed effects and a district linear time trend. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table E4 – OLS Regressions, Alternative Journals

	(1) Freie Presse	(2) Bild Bild	(3) Morgenpost für Sachsen	(4) Lausitzer Rundschau	(5) Sächsische Zeitung	(6) Leipziger Volkszeitung
$July16_t \times E_l^{Newsp.}$	-0.002	0.004	-0.008	-0.004	-0.063***	0.043
	(0.017)	(0.098)	(0.089)	(0.014)	(0.016)	(0.027)
Nb. Observations	110,364	110,364	110,364	110,364	110,364	110,364
Adjusted R^2	0.099	0.099	0.099	0.099	0.099	0.099
Average $Attitudes_{ilt}$	0.342	0.342	0.342	0.342	0.342	0.342

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3) and Year-Month $\times$ Regional fixed effects. Robust standard errors clustered at the commuting zones level level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table E5 – 2SLS Regressions, Alternative Journals

	(1) Freie Presse	(2) Bild Bild	(3) Morgenpost für Sachsen	(4) Lausitzer Rundschau	(5) Sächsische Zeitung	(6) Leipziger Volkszeitung
$WDisclose_{lt}^{Ger}$	0.072	-0.643	-0.146	0.177	-0.777**	-105.976
	(0.332)	(4.728)	(1.406)	(2.749)	(0.312)	(6637.750)
Nb. Observations	109,885	109,885	109,885	109,885	109,885	109,885
KP F-test	15.351	0.781	8.868	0.413	55.135	0.000
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.343

Note: The dependent variable is a dummy variable for "Very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3) and Year-Month $\times$ Regional fixed effects. Robust standard errors clustered at the commuting zones level level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table E6 – 2SLS Regressions, Victims vs. Perpetrators

	(1) All	(2) Victims	(3) Perpetrators	(4) All	(5) Victims	(6) Perpetrators
$WDisclose_{lt}$	-0.778	-12.395	-2.652***			
	(0.554)	(25.805)	(0.682)			
$WDisclose_{lt}^{Ger}$				-0.777**	-6.758*	-3.776***
				(0.312)	(3.615)	(1.341)
Nb. Observations	109,885	109,885	109,885	109,885	109,885	109,885
KP F-test	4.939	0.301	963.253	55.135	13.960	133.680
First stage	0.072**	0.005	0.021***	0.072***	0.008***	0.015***
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.343

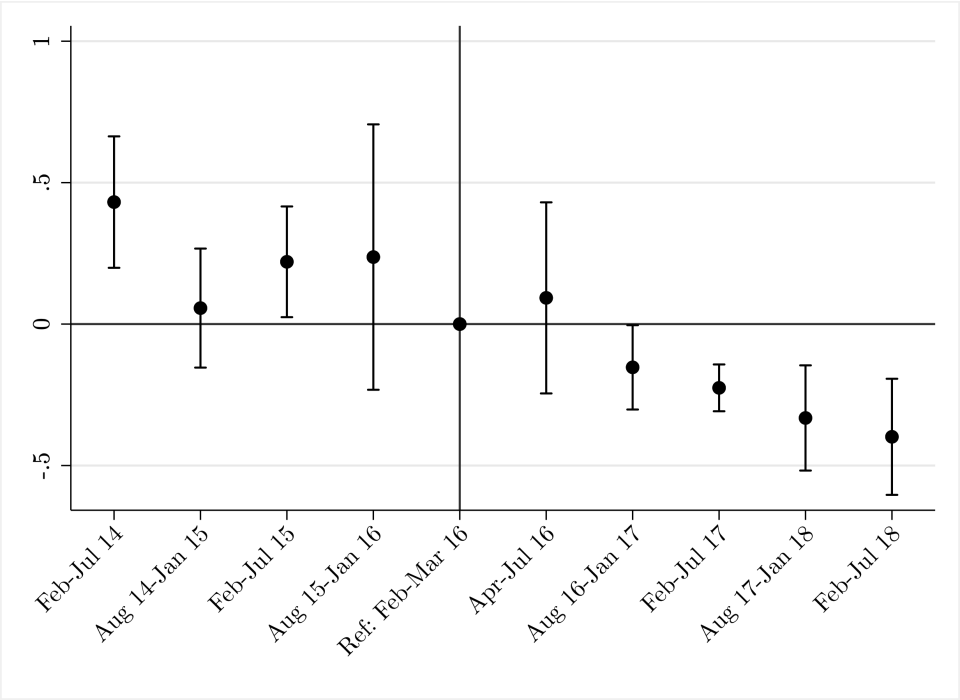
Note: The dependent variable is a dummy variable for "Very concerned" about immigration. Column (2) assumes that articles that do not include the word "öpfer" (victim) do not reveal any particular origin. Columns (3) assumes that articles that do not include the word "täter" (perpetrator), and its variations in spelling, do not reveal any particular origin. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3) and Year-Month $\times$ Regional fixed effects. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table E7 – 2SLS Regressions, Controlling for Alternative Lexicons

	(1)	(2)	(3)	(4)	(5)
$WDisclose_{lt}^{Ger}$	-0.784***	-0.849***	-0.804***	-0.865***	-0.790***
	(0.289)	(0.228)	(0.260)	(0.310)	(0.254)
$WDisclose_{lt}^{Mig}$	0.188				0.044
	(0.134)				(0.106)
$WDisclose_{lt}^{For}$		0.266**			0.303**
		(0.118)			(0.132)
$WDisclose_{lt}^{Ref}$			0.364		0.073
			(0.237)		(0.281)
$WDisclose_{lt}^{Top}$				0.268	-0.231
				(0.218)	(0.220)
Nb. Observations	109,885	109,885	109,885	109,885	109,885
KP F-test	149.482	335.113	15240.207	1111.177	275.432
First stage	0.071***	0.064***	0.069***	0.059***	0.064***
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343

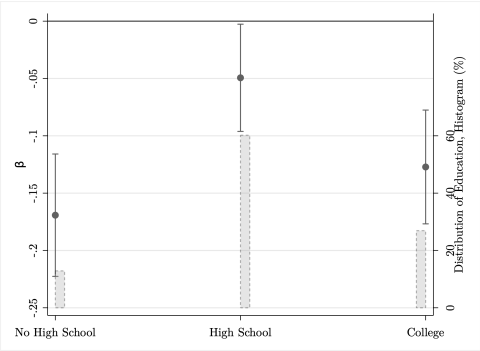
Note: The dependent variable is a dummy variable for "very concerned" about immigration. All estimates include the full vector of individual controls with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3) and Year-Month $\times$ Regional fixed effects. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration on SOEP.

Figure E1 – OLS regressions, Event Analysis

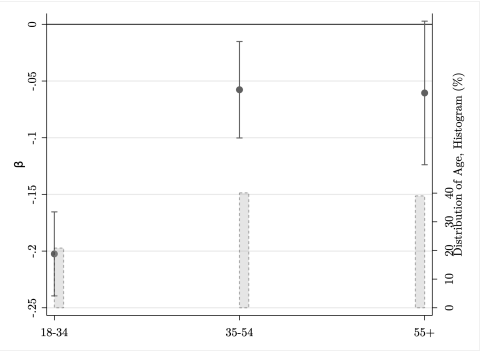


Notes: This graph depicts changes in natives' attitudes towards immigration in the diffusion area of the Sächsische Zeitung before and after the policy change. Effects are grouped in bins of six months. The reference period is the period February-March 2016. Coefficients are obtained from a regression where the dependent variable is a dummy variable for "Very concerned" about immigration. It includes the full vector of individual controls with age, marital status, education, employment status and individual earnings, the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime as well as district (NUTS-3), Year-Month $\times$ Regional fixed effects and a district linear time trend. Standard errors are clustered at the commuting zones level. Confidence intervals are presented at the 95% level.
Source: Authors' elaboration on SOEP.

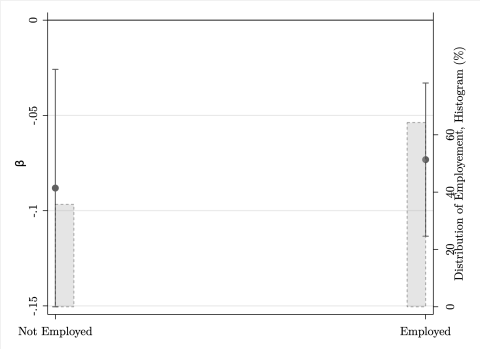
Figure E2 – OLS Regressions,
Heterogeneity Analysis with Individual Characteristics



(a) Educational attainment



(b) Age category



(c) Employment Status

Notes: This graph depicts the marginal effect of the treatment conditional on individual characteristics. Coefficients are obtained from a regression where the dependent variable is a dummy variable for "Very concerned" about immigration and the treatment is interacted with a dummy for each characteristic. Each regression includes the full vector of individual controls with age, marital status, education, employment status and individual earnings and the full vector of district-year controls with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born, share of crime in overall population, and share of foreigners in total crime. All the estimates include district (NUTS-3), Year-Month $\times$ Regional fixed effects, and a district linear time trend. Standard errors are clustered at the commuting zones level. Confidence intervals are presented at the 95% level.
Source: Authors' elaboration on SOEP.

Table E8 – OLS/2SLS Regressions, Baseline Estimates with Alternative Dependent Variables

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) OLS	(6) OLS	(7) 2SLS	(8) 2SLS	(9) OLS	(10) OLS	(11) 2SLS	(12) 2SLS	(13) OLS	(14) OLS	(15) 2SLS	(16) 2SLS
Dependent variable:																
Very Concerned	1	1	1	1	2	2	2	2	1	1	1	1	1	1	1	1
Somewhat concerned	0	0	0	0	1	1	1	1	x	x	x	x	1	1	1	1
No concern at all	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
$July16_t \times E_l^{SZ}$	-0.056***	-0.040***			-0.144***	-0.104***			-0.079***	-0.033*			-0.032**	-0.024		
	(0.015)	(0.006)			(0.045)	(0.022)			(0.025)	(0.018)			(0.015)	(0.021)		
$WDisclose_{lt}^{Ger}$			-0.777**	-0.509***			-1.991**	-1.325***			-1.092**	-0.426			-0.436*	-0.308
			(0.312)	(0.075)			(0.875)	(0.411)			(0.465)	(0.274)			(0.257)	(0.307)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	110,364	104,866	109,885	104,396	110,364	104,866	109,885	104,396	64,279	55,544	63,990	55,279	110,364	104,866	109,885	104,396
Adjusted R^2	0.098	0.478			0.123	0.556			0.199	0.769			0.089	0.467		
KP F-test			55.135	61.463			55.135	61.463			69.026	89.840			55.135	61.463
Average $Attitudes_{ilt}$	0.342	0.345	0.343	0.346	0.342	0.345	0.343	0.346	0.588	0.597	0.588	0.598	0.342	0.345	0.343	0.346

Note: The dependent variable from column (1) to (4) is a dummy variable for very concerned about immigration. The dependent variable from column (5) to (8) is a continuous variable of attitudes toward immigration. The dependent variable from column (9) to (12) is a dummy variable for very concerned about immigration and zero otherwise (somewhat concerned is excluded). The dependent variable from column (13) to (16) is a dummy variable for very and somewhat concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table E9 – OLS/2SLS Regressions, Baseline Estimates with Individual Longitudinal Weights

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS $> 50\%$	(10) 2SLS $> 50\%$	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.128***	0.069***										
	(0.018)	(0.025)										
$July16_t$	-0.003											
	(0.011)											
$July16_t \times E_l^{SZ}$	0.016	-0.076***	-0.079***	-0.086***	-0.112***	-0.039***						
	(0.013)	(0.018)	(0.014)	(0.015)	(0.035)	(0.006)						
$WDisclose_{lt}$							-1.079	-0.455**	-1.388***	-0.402		
							(0.648)	(0.196)	(0.231)	(0.244)		
$WDisclose_{lt}^{Ger}$											-1.075***	-0.450***
											(0.324)	(0.086)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District $\times$ linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	104,791	104,788	104,788	104,788	104,788	97,784	104,347	97,373	34,999	32,529	104,347	97,373
Adjusted R^2	0.045	0.082	0.099	0.100	0.102	0.484						
KP F-test							5.405	5.778	234.732	333.701	61.224	63.173
Average $Attitudes_{ilt}$	0.341	0.341	0.341	0.341	0.341	0.347	0.342	0.347	0.353	0.359	0.342	0.347

Note: The dependent variable is a dummy variable for very concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration on SOEP.

Table E10 – OLS/2SLS Regressions, Baseline Estimates
Controlling for the share of SZ and the share of crime at the CZs-year level

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS ¿ 50%	(10) 2SLS ¿50%	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.004	0.047										
	(0.172)	(0.144)										
$July16_t$	-0.002											
	(0.011)											
$July16_t \times E_l^{SZ}$	0.032**	-0.056***	-0.093***	-0.102***	-0.090*	-0.072***						
	(0.012)	(0.015)	(0.008)	(0.009)	(0.047)	(0.020)						
$WDisclose_{lt}$							-1.133***	-0.758*	-1.061***	-0.797**		
							(0.313)	(0.450)	(0.309)	(0.314)		
$WDisclose_{lt}^{Ger}$											-1.260***	-0.825**
											(0.214)	(0.351)
E_{yCZ}^{SZ}	0.142	0.027	0.722*	0.770**	0.414	0.577*	0.491	0.359	-0.201	0.423	0.676	0.470
	(0.175)	(0.134)	(0.389)	(0.360)	(1.921)	(0.324)	(1.437)	(1.104)	(0.274)	(0.266)	(0.785)	(0.608)
$Crime_{yCZ}$	0.091***	0.010	0.041*	0.040*	0.031	0.041	0.041*	0.039	0.094***	0.104***	0.046**	0.044
	(0.024)	(0.017)	(0.021)	(0.021)	(0.019)	(0.026)	(0.024)	(0.029)	(0.020)	(0.027)	(0.021)	(0.026)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month × Regional	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	109,885	109,885	109,885	109,885	109,885	104,396	109,885	104,396	36,996	35,033	109,885	104,396
Adjusted R^2	0.047	0.080	0.098	0.099	0.100	0.479						
KP F-test							21.935	11.097	215.552	170.185	103.117	69.529
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.346	0.343	0.346	0.353	0.356	0.343	0.346

Note: The dependent variable is a dummy variable for very concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p¡0.01, ** p¡0.05, * p¡0.1.
Source: Authors' elaboration on SOEP.

Table E11 – OLS/2SLS Regressions, Baseline Estimates at the Commuting Zones level

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS ¿ 50%	(10) 2SLS ¿50%	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.130***	0.071***										
	(0.016)	(0.016)										
$July16_t$	-0.005											
	(0.011)											
$July16_t \times E_l^{SZ}$	0.056***	-0.047**	-0.049**	-0.055***	-0.083***	-0.041***						
	(0.014)	(0.019)	(0.020)	(0.021)	(0.031)	(0.010)						
$WDisclose_{lt}$							-0.673*	-0.586***	-0.706**	-0.392		
							(0.399)	(0.158)	(0.285)	(0.241)		
$WDisclose_{lt}^{Ger}$											-0.723**	-0.618***
											(0.280)	(0.077)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CZ FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CZ $\times$ linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	109,885	109,885	109,885	109,885	109,885	104,396	109,885	104,396	36,996	35,033	109,885	104,396
Adjusted R^2	0.045	0.080	0.083	0.084	0.084	0.478						
KP F-test							8.634	11.702	64.451	48.859	149.086	167.739
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.346	0.343	0.346	0.353	0.356	0.343	0.346

Note: The dependent variable is a dummy variable for very concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p¡0.01, ** p¡0.05, * p¡0.1. Source: Authors' elaboration on SOEP.

F Sächsische Zeitung Diffusion Across Municipalities and Districts (2013-2017)

Table F1 – Variation over Time in the Market Share of the Sächsische Zeitung, by District

	Observations	Mean	S.e.	S.d.	95 percent c.i.	
Share 2017 - share 2015 (1)	10	0.011	0.004	0.013	0.002	0.021
Share 2015 - share 2013 (2)	10	0.013	0.004	0.012	0.004	0.022
Difference (2) -(1)	10	-0.001	0.002	0.006	-0.006	0.003
t with 9 degrees of freedom				-0.723		
T-test (2) -(1) != 0				$Pr(\|T\| > \|t\|) =$ 0.488		

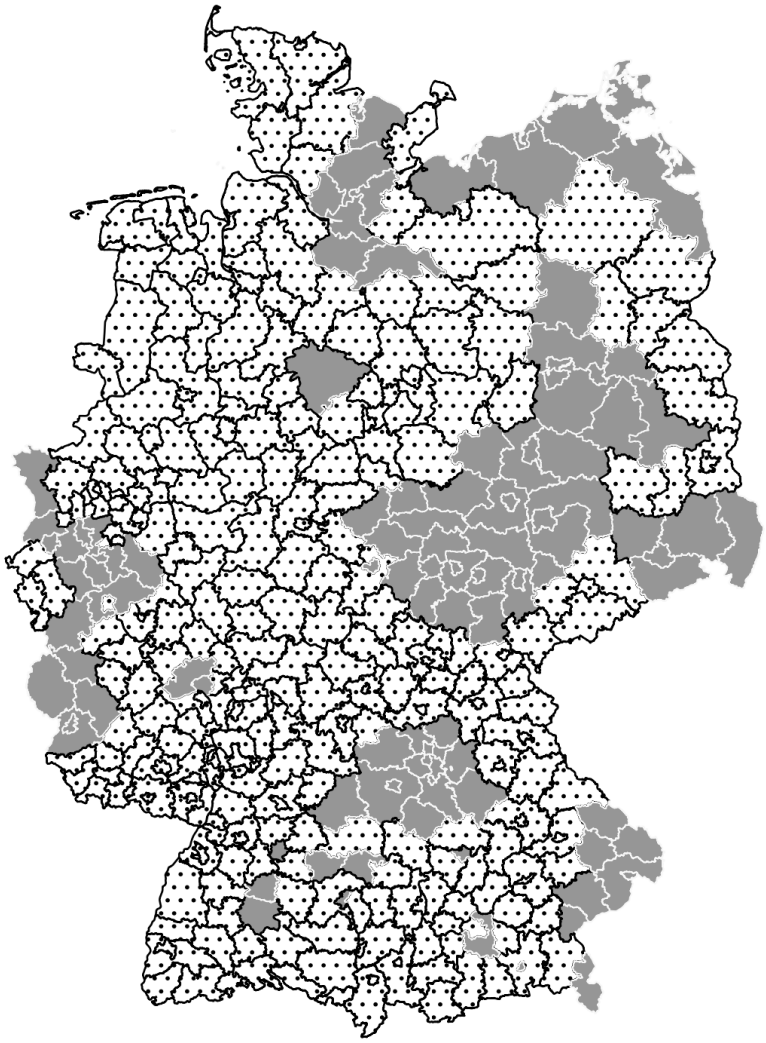
Note: The average variation in the diffusion share of the newspaper Sächsische Zeitung is calculated for districts in the state of Saxony where at least one copy of the newspaper was sold in each edition of the IVW diffusion analysis. The IVW collected data for the 2014, 2016, and 2018 editions in November of the years 2013, 2015, and 2017, respectively.
Source: Authors' elaboration on IVW data.

Table F2 – Variation over Time in the Market Share of the Sächsische Zeitung, by Municipality

	Observations	Mean	S.e.	S.d.	95 percent c.i.	
Share 2017 - share 2015 (1)	200	0.015	0.002	0.031	0.011	0.019
Share 2015 - share 2013 (2)	200	0.015	0.002	0.025	0.012	0.019
Difference (2) -(1)	200	0.000	0.003	0.041	-0.006	0.005
t with 199 degrees of freedom				-0.102		
T-test (2) -(1) != 0				$Pr(\|T\| > \|t\|) =$ 0.919		

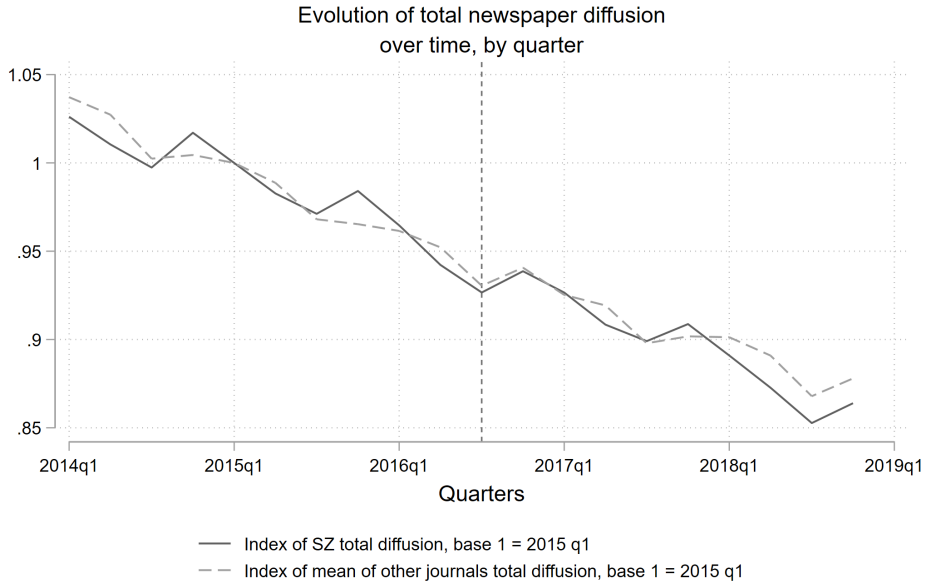
Note: The average variation in the diffusion share of the newspaper Sächsische Zeitung is calculated for municipalities in the state of Saxony where at least one copy of the newspaper was sold in each edition of the IVW diffusion analysis. The IVW collected data for the 2014, 2016, and 2018 editions in November of the years 2013, 2015, and 2017, respectively.
Source: Authors' elaboration on IVW data.

Figure F1 – IV sample
Localities for which we cover at least 50% of the total number of sales



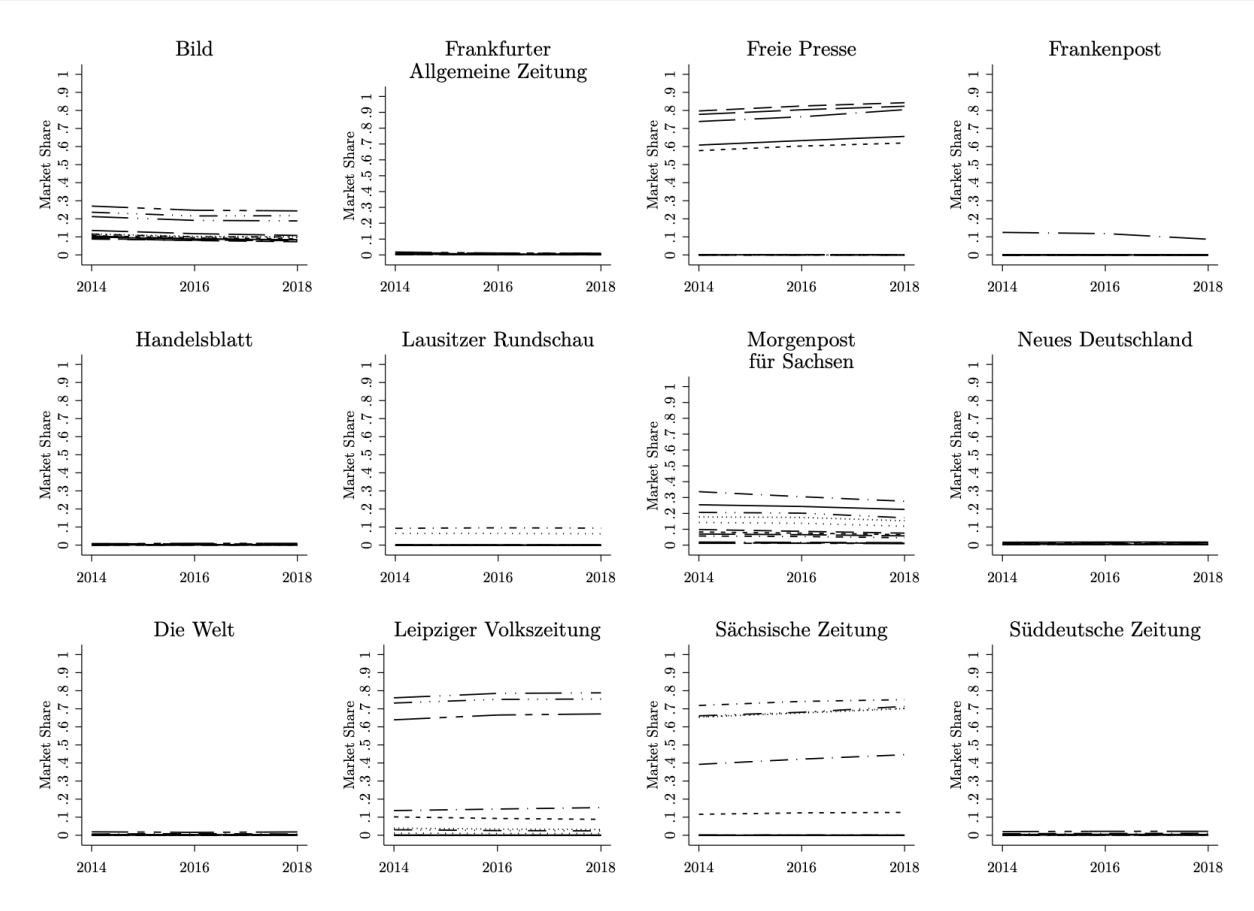
Note: This map depicts in grey all localities for which our data cover at least 50% of the total number of sale. Source: Author's elaboration on information Community for the Assessment of the Circulation of Media (IVW).

Figure F2 – Evolution of Total Newspaper Diffusion over Time, by Quarter



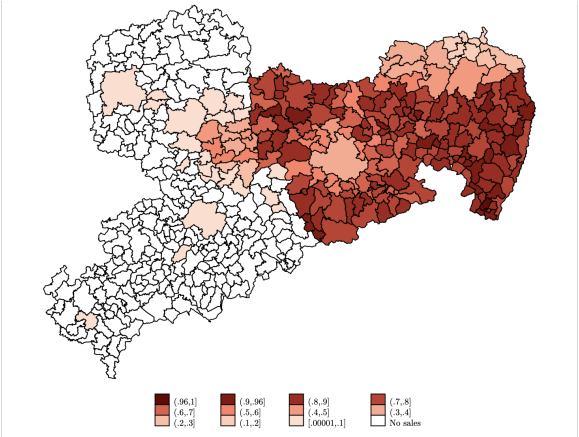
Note: The vertical line corresponds to the third quarter of 2016.
Source: Authors' elaboration on Information Community for the Assessment of the Circulation of Media (IVW).

Figure F3 – Evolution of Sächsische Zeitung Diffusion over Time and Across Districts in Saxony

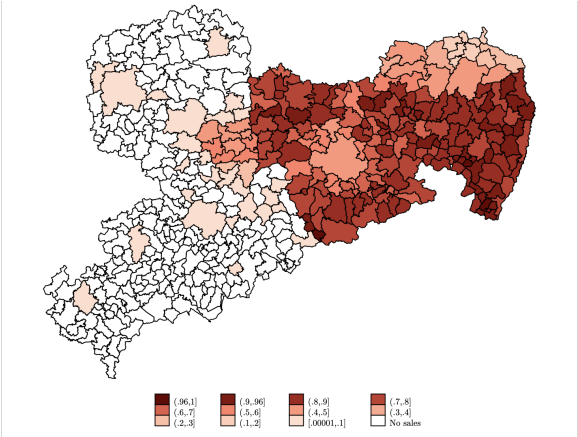


Note: This graph shows the relative diffusion of the Sächsische Zeitung and other newspapers over time for each district in Saxony. Each line represents the total number of sales for each newspaper in the total number of sales for all newspapers in each district. It includes newspapers for which the number of sales during the week of reference exceeds 1,000 copies for at least one year between the 2014 and the 2018 editions.
Source: Authors' elaboration on Information Community for the Assessment of the Circulation of Media (IVW).

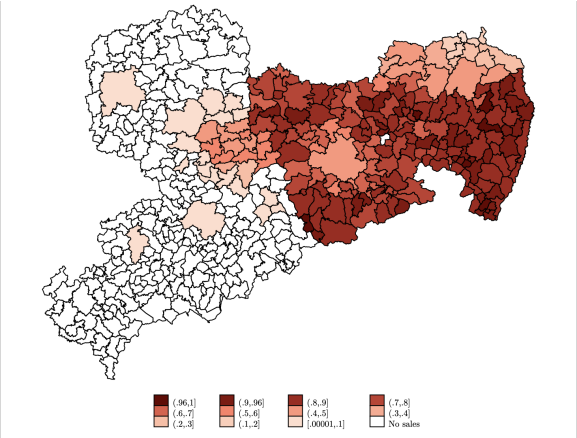
Figure F4 – Sächsische Zeitung Areas of Diffusion, Municipalities



(a) 2014 edition



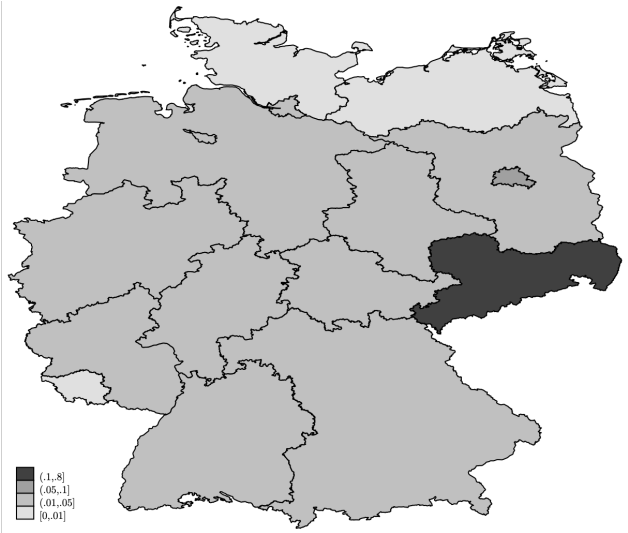
(b) 2016 edition



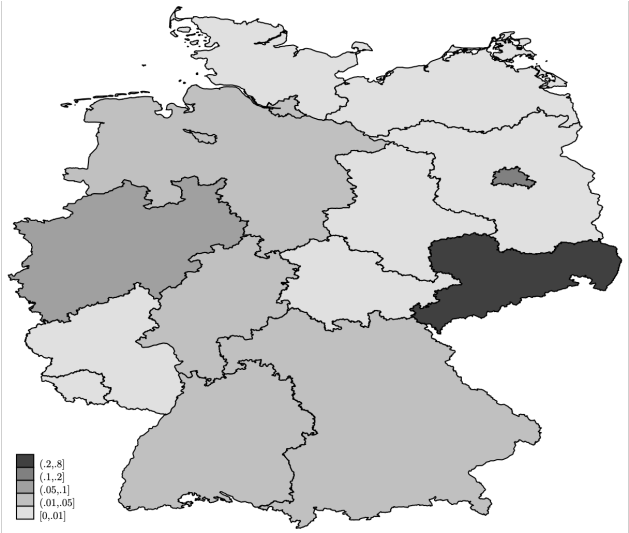
(c) 2018 edition

Note: These maps depict the sales of the Sächsische Zeitung as a percentage of total sales at the municipality level.
Source: Authors' elaboration on Information Community for the Assessment of the Circulation of Media (IVW).

Figure F5 – Sächsische Zeitung followers and mentions on Twitter (NUTS-1)



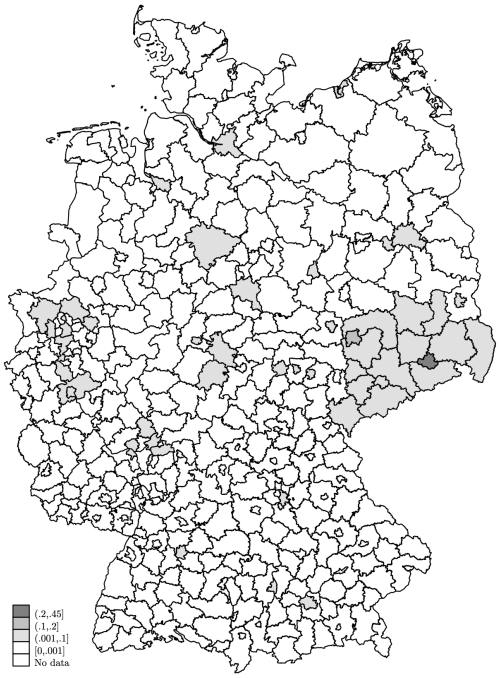
(a) Followers



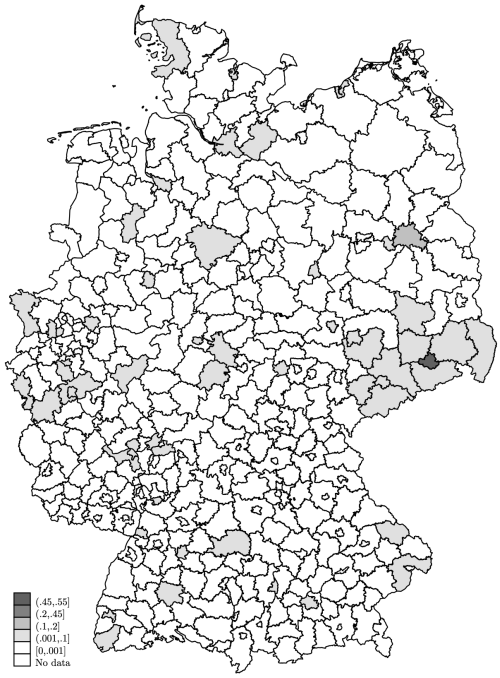
(b) Mentions

Note: These maps depict the share of followers and mentions from the Sächsische Zeitung Twitter feed at the NUTS-1 level.
Source: Authors' elaboration on Twitter data.

Figure F6 – Sächsische Zeitung followers and mentions on Twitter (NUTS-3)



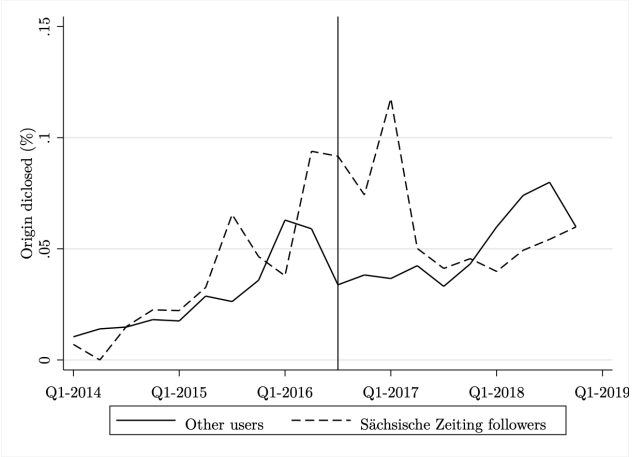
(a) Followers



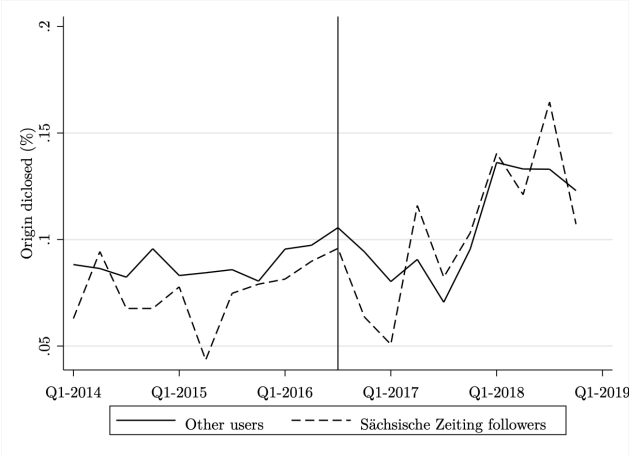
(b) Mentions

Note: These maps depict the share of followers and mentions from the Sächsische Zeitung Twitter feed at the NUTS-3 level.
Source: Authors' elaboration on Twitter data.

Figure F7 – Sächsische Zeitung followers and non-followers
Percentage of crime-related tweets and retweets disclosing at least one nationality



(a) German identity



(b) Foreign nationalities

Note: These maps depict the share of crime-related tweets and retweets disclosing a nationality. The vertical bar identifies Q3 2016.
Source: Authors' elaboration on Twitter data.

G Estimates at the municipality level

Table G1 – OLS/2SLS Regressions, Baseline Estimates
Municipality level

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS $> 50\%$	(10) 2SLS $> 50\%$	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.126***	0.075***										
	(0.021)	(0.027)										
$July16_t$	-0.005											
	(0.011)											
$July16_t \times E_l^{SZ}$	0.047***	-0.050***	-0.054***	-0.060***	-0.107***	-0.035***						
	(0.013)	(0.017)	(0.004)	(0.004)	(0.024)	(0.005)						
$WDisclose_{lt}$							-0.676**	-0.380***	-0.524***	-0.279**		
							(0.269)	(0.139)	(0.153)	(0.109)		
$WDisclose_{lt}^{Ger}$											-0.704***	-0.395***
											(0.096)	(0.063)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ Regional	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality $\times$ linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	109,864	109,864	109,649	109,649	109,649	104,180	109,649	104,180	37,094	35,101	109,649	104,180
Adjusted R^2	0.046	0.081	0.158	0.159	0.160	0.466						
KP F-test							5.919	6.612	103.147	149.208	54.507	59.187
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.346	0.343	0.346	0.353	0.356	0.343	0.346

Note: The dependent variable is a dummy variable for very concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

Table G2 – OLS/2SLS Regressions, Baseline Estimates
Municipality level with NUTS-2 $\times$ Year-Months FE

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS $> 50\%$	(10) 2SLS $> 50\%$	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.126***	0.101										
	(0.021)	(0.081)										
$July16_t$	-0.005											
	(0.011)											
$July16_t \times E_l^{SZ}$	0.047***	-0.069***	-0.064***	-0.067***	-0.114*	-0.031***						
	(0.013)	(0.023)	(0.007)	(0.007)	(0.067)	(0.011)						
$WDisclose_{lt}$							-0.551***	-0.254	-0.885***	-0.605***		
							(0.095)	(0.154)	(0.218)	(0.186)		
$WDisclose_{lt}^{Ger}$											-0.735***	-0.329**
											(0.079)	(0.164)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ NUTS-2	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality $\times$ linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	109,864	109,819	109,604	109,604	109,604	104,134	109,604	104,134	37,066	35,065	109,604	104,134
Adjusted R^2	0.046	0.087	0.161	0.162	0.163	0.467						
KP F-test							25.996	22.751	981.835	926.125	100.536	108.604
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.346	0.343	0.346	0.353	0.356	0.343	0.346

Note: The dependent variable is a dummy variable for very concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

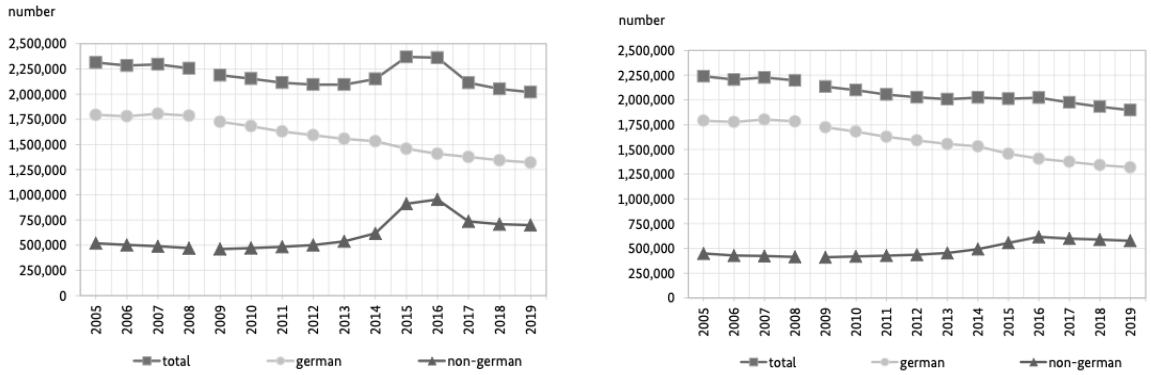
Table G3 – OLS/2SLS Regressions, Baseline Estimates
Municipality level with NUTS-3 $\times$ Year-Months FE

	(1) OLS All	(2) OLS All	(3) OLS All	(4) OLS All	(5) OLS All	(6) OLS All	(7) 2SLS All	(8) 2SLS All	(9) 2SLS $> 50\%$	(10) 2SLS $> 50\%$	(11) 2SLS All	(12) 2SLS All
E_l^{SZ}	0.126***	0.006										
	(0.021)	(0.029)										
$July16_t$	-0.005											
	(0.011)											
$July16_t \times E_l^{SZ}$	0.047***	0.002	-0.066	-0.067	-0.176	-0.067**						
	(0.013)	(0.037)	(0.055)	(0.060)	(0.142)	(0.029)						
$WDisclose_{lt}$							-0.570	-0.544***	-0.285***	-0.464***		
							(0.378)	(0.096)	(0.022)	(0.040)		
$WDisclose_{lt}^{Ger}$											-0.749	-0.689***
											(0.568)	(0.199)
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year Controls	Yes	No	No	No	No	No	No	No	No	No	No	No
Municipality FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month $\times$ NUTS-3	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality $\times$ linear time trend	No	No	No	No	Yes	No	No	No	No	No	No	No
Individual FE	No	No	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Nb. Observations	109,864	107,759	107,519	107,519	107,519	101,666	107,519	101,666	36,572	34,482	107,519	101,666
Adjusted R^2	0.046	0.130	0.176	0.176	0.173	0.474						
KP F-test							31.476	22.818	6.20e+06	5.24e+05	139.811	107.813
Average $Attitudes_{ilt}$	0.343	0.343	0.343	0.343	0.343	0.345	0.343	0.345	0.352	0.355	0.343	0.345

Note: The dependent variable is a dummy variable for very concerned about immigration. All estimates include the full vector of individual control with age, marital status, education, employment status and individual earnings. All estimates include the full vector of district-year control with unemployment rate, share of social transfer recipients, share of refugees, net migration flows for natives and foreign-born respectively, share of crime in overall population, share of foreigners in total crime. Robust standard errors clustered at the commuting zones level are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' elaboration on SOEP.

H Additional Documentation

Figure H1 – Criminality in Germany, 2019 Development of Suspects.



(a) Total offenses (b) Excluding offenses against foreigners' law

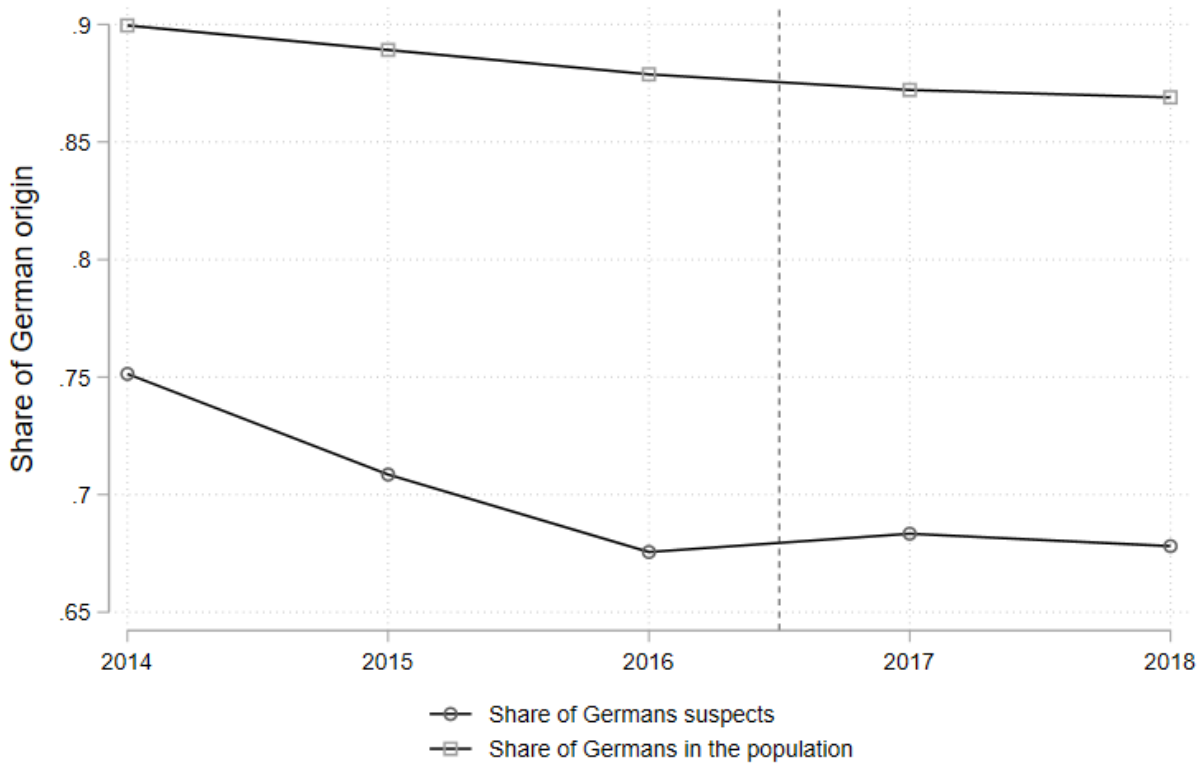
Note: In 2009, police crime statistics started to count the "real" number of suspects at the federal level. The number of suspects is therefore not comparable with values for earlier years.
Source: Police crime statistics. Federal Republic of Germany Report 2019. V 1.0.

Figure H2 – Criminality in Germany, Share of Foreigners.

year	recorded cases			total offences, excluding offences against the Residence Act, the Asylum Act, and the Freedom of Movement Act/E.U. (since 2005 key 890000)		
	suspects total	non German suspects		suspects total	non German suspects	
		number	in %		number	in %
2005	2,313,136	519,573	22.5	2,238,550	448,544	20.0
2006	2,283,127	503,037	22.0	2,204,819	427,911	19.4
2007	2,294,883	490,278	21.4	2,225,139	423,288	19.0
2008	2,255,693	471,067	20.9	2,196,728	414,347	18.9
*) 2009	2,187,217	462,378	21.1	2,133,703	410,518	19.2
2010	2,152,803	471,812	21.9	2,098,601	419,232	20.0
2011	2,112,843	484,529	22.9	2,054,232	427,259	20.8
2012	2,094,118	502,390	24.0	2,025,952	435,559	21.5
2013	2,094,160	538,449	25.7	2,007,328	453,015	22.6
2014	2,149,504	617,392	28.7	2,023,623	492,610	24.3
2015	2,369,036	911,864	38.5	2,011,898	555,820	27.6
2016	2,360,806	953,744	40.4	2,022,414	616,230	30.5
2017	2,112,715	736,265	34.8	1,974,805	599,357	30.4
2018	2,051,266	708,380	34.5	1,931,079	589,200	30.5
2019	2,019,211	699,261	34.6	1,896,221	577,241	30.4

Note: In 2009, police crime statistics started to count the ”real” number of suspects at the federal level. The number of suspects is therefore not comparable with values for earlier years.
Source: Police crime statistics. Federal Republic of Germany Report 2019. V 1.0.

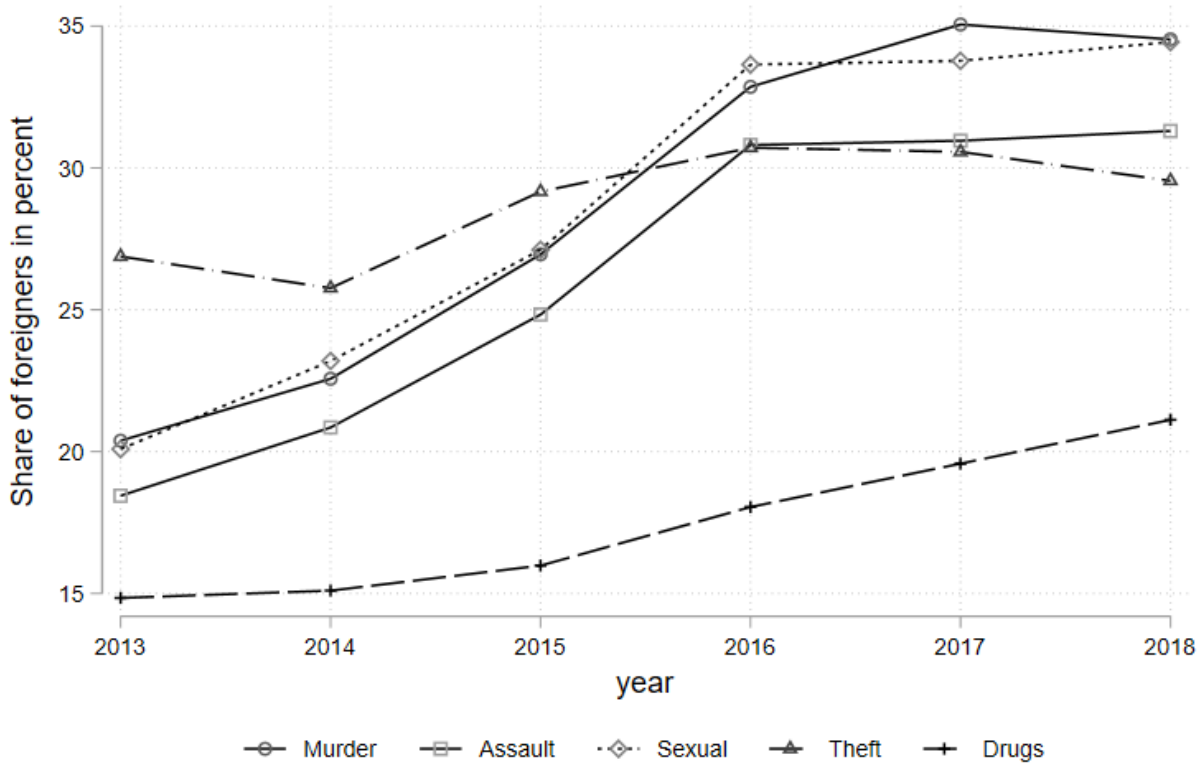
Figure H3 – Share of Germans among Suspects in Violent Crimes Cases



Note: This graph depicts the share of Germans among suspects in violent crime cases and the share of Germans in the general population.

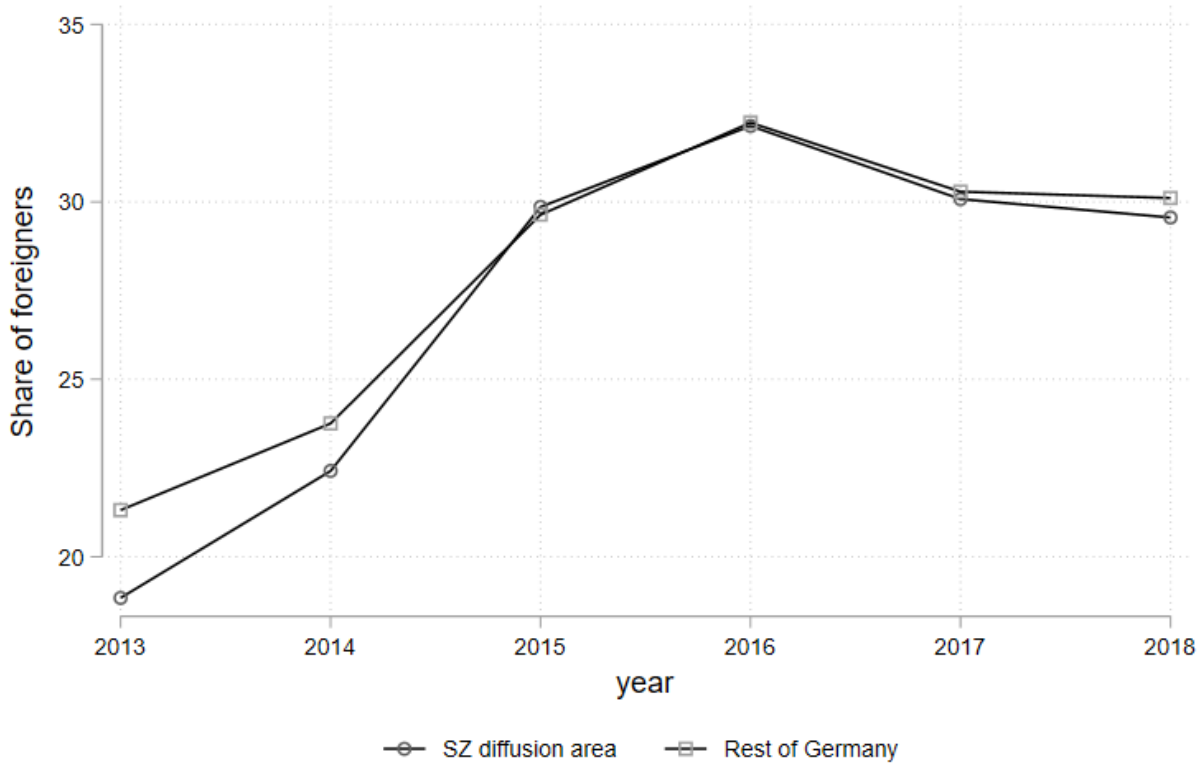
Source: Authors' elaboration on Federal police crime statistics.

Figure H4 – Share of Foreigners among Suspects by type of crime



Note: This graph depicts the share of Foreigners among suspects in violent crime cases by type of crime.
Source: Authors' elaboration on Federal police crime statistics.

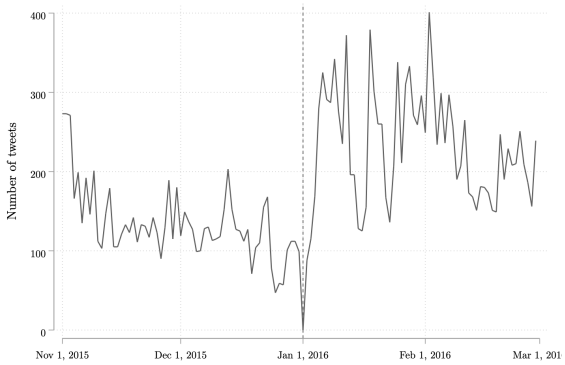
Figure H5 – Share of Foreigners among Suspects in Crimes, SZ diffusion area



Note: This graph depicts the share of Foreigners among suspects in violent crime cases in the SZ diffusion area and the rest of Germany.

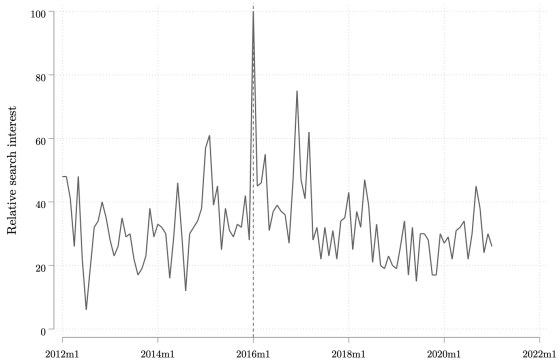
Source: Authors' elaboration on Federal police crime statistics.

Figure H6 – “Lügenpresse” in Germany from Twitter



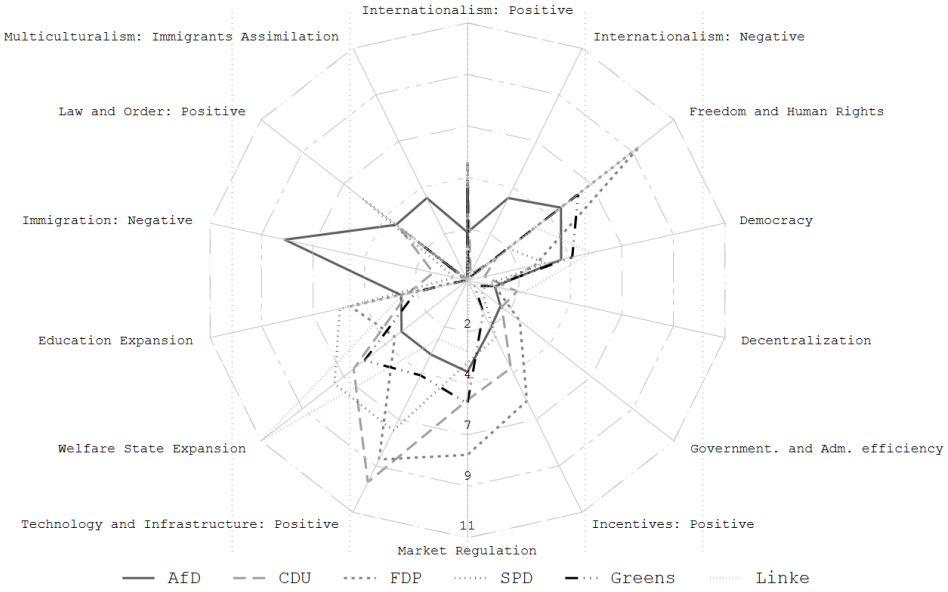
Notes: This graph depicts the number of tweets on Twitter including the term “Lügenpresse” between November 1, 2015, and March 1, 2016.
Source: Authors’ elaboration on Twitter.

Figure H7 – “Pressekodex” in Germany from January 2014 to May 2019.
Google Trends Search Interest



Notes: Google Trends does not allow us to obtain the exact number of search requests with the word “Pressekodex” made by German residents. The only information that is available is the monthly search intensity relative to the total number of searches received by Google over the period. Thus, the score that is depicted here is a deviation from the highest relative search volume, which is set at 100. A score of 0 means virtually zero searches of the term “Pressekodex” for a given month.
Source: Authors’ elaboration on Google Trends.

Figure H8 – parties' policy positions, Manifesto project



Notes: This figure depicts parties' policy positions derived from a content analysis of parties' electoral manifestos.
Source: Authors' elaboration on the Manifesto Corpus.

H1 Sächsische Zeitung policy change.

Title: Facts against rumors.
Subtitle: Why the Sächsische Zeitung will always mention the nationality of offenders in the future. Regardless of whether they are Germans or foreigners.
By Oliver Reinhard.

What is truth? The one in the papers? Well: Even journalists are neither wise nor omniscient. But as a rule, they do their best to get as close as possible to the truth with their work, even uncomfortable, ugly or bad truths, about which one neither likes to write nor read. This is one of the highest virtues of journalism, of professional ethics.

But: Many people doubt that journalists really do strive for the highest possible truth content. They instead believe that journalists manipulate, halve, and suppress truths. Especially since the refugee crisis, there has been much talk of declining trust in the media. However, studies show that the degree and proportion of mistrust and confidence in the press have not changed for decades.

We wanted to know more about this. That's why we asked Professor Lutz Hagen from the Institute for Communication Science at the TU Dresden to conduct a representative survey among our subscribers in the spring of this year. To find out what they think about the coverage of the Sächsische Zeitung.

We were delighted with the result. 56 percent gave our work the grade Good, 34 percent a Satisfactory, six percent even a Very Good. 72 percent of the subscribers also said that nothing had changed in their trust in the Sächsische Zeitung since the beginning of the refugee crisis.

Of course, these findings are no reason to be satisfied with everything and just carry on like this. After all, striving for quality and trust among readers is also part of the editorial team's constant and everyday tasks. Since we know that the issue of foreigner crime is a particularly sensitive question of reader confidence, we have dealt with it separately. It is no secret that many Germans believe that the media, in their reporting, conceal foreign criminals' origins out of consideration for them. Although a majority of 53 percent of our subscribers surveyed do not share this opinion, another 15 percent say "I don't know". But at least 25 percent think so.

Our goal: protecting minorities

The truth is that almost all media, including the Sächsische Zeitung, adhere to guideline 12.1 of the press codex, issued by the German Press Council, when it comes to foreigner crimes. The

latter recommends: “In reporting on crimes, the suspects’ or perpetrators’ affiliation to religious, ethnic or other minorities is only mentioned if there is a justifiable factual connection for understanding the reported event.” This applies, for example, to a crime committed for religious motives. But not for theft out of greed or poverty.

The directive goes on to say: “It should be noted in particular that mention could incite prejudice against minorities.” And such stigmatization – or worse – of minorities has indeed been on the rise for some time. Also and especially in Saxony

It is imperative to us to protect the vast majority of non-criminal refugees in Dresden and the other communities in our area of distribution and to protect them from discrimination. Nevertheless, we have asked ourselves: does the Press Codex Directive really contribute to the protection of minorities in the current situation in Dresden and Saxony?

On the contrary, many SZ employees are convinced: Especially not mentioning the nationality of criminals and suspects can create room for rumors that often harm precisely those we would like to protect. Like most of our colleagues, four out of five SZ subscribers do not consider the naming of the nationality of perpetrators to be discriminatory and also plead for naming the nationality.

That is why, after quite controversial discussions, we have decided to no longer adhere to the guidelines of the German Press Council when reporting on foreigner crime as of today. Instead, we will in the future always state the origin of offenders or suspects. Regardless of whether they are Germans, which is the rule, or foreigners.

However, we will be able to report the origin only for crimes of which the police authorities also inform us, which is not usually the case with minor cases such as petty thefts or tax evasion. And if the police do not report the origin of the perpetrators and suspects of more serious offenses, we cannot do so either. If they do, we will not conceal this information.

An important motive for our decision was also the findings of the subscriber survey in the spring. Although the SZ has rarely mentioned the origin of the perpetrators so far – usually only if it was directly related to the crime – many readers estimate the number of criminal refugees in Saxony to be considerably higher than it is. This overestimation of crime committed by foreigners is a serious problem nationwide because it can promote racist prejudices.

Foreigners are not more criminal than Germans.

We are aware of this: Like so many media users – and many a journalist – some SZ readers also take information from the press very selectively.

https://www.saechsische.de/fakten-gegen-geruechte-3434300.html. Translated from Ger-

man to English by the authors.

H2 Article 12.1 of the German Press Code

The German Press code is a guideline for journalist work that is published by the German Press Council. It aims at preserving the "standing and the credibility of the media" in Germany. Section 12 describes rules on discrimination with a specific focus on criminality in subsection 12.1. We report below the English version of the guideline that is free-access on the German Press Council website.[71] We highlight, in Section 12, important changes between the versions of March 2015 and March 2017.

H3 Section 12

There must be no discrimination against a person because of his/her sex, disability or membership in an ethnic, religious, social or national group.

Guideline 12.1, Reports on crimes :

March, 2015:

When reporting on crimes, *it is not permissible to refer to the suspect's religious, ethnic or other minority membership unless this information can be justified as being relevant to the readers' understanding of the incident.* In particular, it must be borne in mind that such references could stir up prejudices against minorities.

March, 2017:

When reporting on crimes, *it must be ensured that any reference to a suspect's or perpetrator's membership in ethnic, religious or other minority groups does not result in a discriminatory generalization of individual misconduct. As a rule, membership in a minority group shall not be mentioned, unless this is in the legitimate interest of the general public.* In particular, it must be borne in mind that such references could stir up prejudices against minorities.

[71] https://www.presserat.de/pressekodex/pressekodex/

H4 Examples of articles published in the Sächsische Zeitung

This section presents three examples of articles taken from the newspaper Sächsische Zeitung. It illustrates which words are detected from the crime lexicons, allowing us to identify relevant articles. Words detected in the origin lexicons are used to identify articles revealing the origin of offenders. Articles were translated from German to English by the authors, and original German versions are available upon request.

Example 1

Title *The senseless death of a construction yard employee still troubles minds four days after the crime. Now a witness has come forward.*

Publication date: *September 26, 2016.*

Relevant extract: *"The shock still sits deep", says mayor Tobias Goth (CDU). With this, he surely speaks for many Leisnig residents who continue to deal with the events of last Friday evening. A 53-year-old man who was employed in the building yard of the city of Leisnig was* ***attacked*** *by a 25-year-old* ***German*** *in the middle of the market square and so seriously injured that he succumbed to his injuries. There are various reports about the exact incident. The DA has meanwhile been able to speak exclusively with one of the first responders*, who also witnessed the crime.[...]*

Crime lexicon words detected: attacked → assault lexicon.

Origin lexicon words detected: German.

Example 2

Title: *Radeberg; attack at the Pulsnitzer Straße.*

Publication date: *March 14, 2017.*

Relevant extract: *A man was injured at night on Sunday in Radeberg. The 30-year-old visited a pub on Pulsnitzer Straße in the evening. When he wanted to leave the pub together with an acquaintance at about 3 o'clock, a 21-year-old* ***German*** *jumped into his legs from behind. The man pulled his* ***victim*** *to the ground. Witnesses eventually took the attacker outside. The 30-year-old sustained minor injuries as a result of the* ***attack****. The exact background of the crime is so far*

unclear. The police were informed of the incident on Monday. The officers are now investigating for ***physical injury****. (SZ)*

Crime lexicon words detected: Attack; physical injury → assault lexicon.

Origin lexicon words detected: German.

Example 3

Title: *One of three thieves caught in the act.*

Publication date: *March 29, 2017.*

Relevant extract: *Zittau. A patrol of the police station Zittau-Oberland set on Monday afternoon in Zittau a* ***thief*** *after a short escape. "A witness had observed how two unknown men deposited various goods worth more than 600 euro at an emergency exit door in a store on Hochwaldstraße and opened it a short time later," the Görlitz police department announced on Tuesday. "The perpetrators handed over part of the loot to an accomplice waiting outside." The witness intervened and informed the police. The accomplice was initially able to flee, but a little later was placed near the store by an alerted patrol of the Zittau police station and temporarily arrested. "The officers seized* ***stolen goods*** *worth about 50 euro carried by the* ***Czech****," the police statement said. "The two unknown* ***suspects*** *who had acted in the store were able to flee without loot." The criminal investigation department has taken up the investigation. (SZ)*

Crime lexicon words detected: thief; stolen goods → theft lexicon.

Origin lexicon words detected: Czech.

H5 Accuracy of lexicon-based classifications of newspaper articles

In building the lexicons, we read a sample of 980 newspaper articles published in March 2017. After reading the articles, we first decided whether they were indeed relevant to our analysis, i.e., if they reported one or more of the violent crime categories retained for our study. We then noted all words that allowed us to identify the type of crime mentioned in the relevant articles. Next, we checked whether the articles mentioned the origin of the criminal and, if so, which words allowed us to identify the origin. The two-step process entails that we checked for the criminal's origin only in

articles classified as relevant. Hence, this information is missing for articles classified as irrelevant (book or movie reviews, biographies of police officers, general discussions on crime, etc.). The lists of words obtained from this exercise were the core of the lexicons, which were then completed by adding synonyms and declinations.

After using the lexicons to assign the articles to crime categories, we can compare the automatic classification to the manual classification of the 980 articles. The results presented in Table H1 indicate that 77.55 percent of articles are correctly classified as relevant or nonrelevant. All misclassifications are due to false positives. This is not surprising because the words used to identify relevant articles all included lexicons, significantly reducing the probability of missing a relevant article. Among the articles identified as relevant based on the lexicons, 73 percent were true positives. This leaves a substantial but acceptable measurement error. The critical aspect for our identification strategy is that the measurement error should not be systematically correlated with the treatment, i.e., it should stay constant before and after the change in reporting policy implemented by the Sächsische Zeitung.

Table H1 – Classification of Article Relevance
Comparing Manual Checks to Lexicon Classification

Article relevance by method		Number articles	Share in %	Cumulated share
Both methods classify as irrelevant	TN	165	16.84	16.84
Both methods classify as relevant	TP	595	60.71	77.55
Manual check yes, lexicon no	FN	0	0	77.55
Manual check no, lexicon yes	FP	220	22.45	100
Total		980	100	
Positive predictive value	PPV		73%	
Negative predictive value	NPV		100%	
Accuracy	ACC		78%	

Note: TN stands for true negative, TP is true positive, FN means false negative, and FP means false positive. $PPV = TP/(TP+FP)$; $NPV = TN/(TN+FN)$; and $ACC = (TP+TN)/(TP+TN+FP+FN)$.
Source: Author's elaboration on Dow Jones Factiva archives.

Next, we assessed the accuracy of origin detection in relevant articles. Here, we focus on the

subset of 595 articles classified as relevant by both lexicon-based classification and manual classification because, as mentioned previously, the presence of origins was not checked for irrelevant articles in the manual classification. The results in Table H2 show that the overall accuracy rate stands at 89 percent. Again, the misclassifications are mostly driven by false positives since the lexicon-based approach detects some words associated with origins that are not associated with the criminal. For example, the detected words may describe the victim, a police officer in charge, a witness, or objects involved, such as guns bought abroad or car registration numbers. Overall, 72 percent of detected positives are true positives, which we consider an acceptable level of precision given the simplicity of the algorithm we implement. The few false negatives are due to very subtle word combinations that suggest a foreign origin to the reader but that we consider too vague to be included in the lexicons.

Table H2 – Origin Detection in Articles
Comparing Manual Checks to Lexicon Classification

Origin detection by method		Number articles	Share in %	Cumulated share
No method detects origin	TN	397	66.72	66.72
Both methods detect origin	TP	131	22.02	88.74
Manual check yes, lexicon no	FN	14	2.35	91.09
Manual check no, lexicon yes	FP	53	8.91	100
Total		595	100	
Positive predictive value	PPV		72%	
Negative predictive value	NPV		98%	
Accuracy	ACC		89%	

Note: TN stands for true negative, TP is true positive, FN means false negative, and FP means false positive. $PPV = TP/(TP + FP)$; $NPV = TN/(TN + FN)$; and $ACC = (TP + TN)/(TP + TN + FP + FN)$.
Source: Author's elaboration on Dow Jones Factiva archives.