Media Persuasion through Slanted Language: Evidence from the Coverage of Immigration *

Milena Djourelova†

October 2021

Abstract

Can the language used by mass media to cover policy-relevant issues affect public policy views? I examine this question in the context of the US debate on immigration, exploiting an abrupt ban on the politically charged term 'illegal immigrant' in the dispatches distributed to media outlets by the Associated Press (AP) news wire. I use the timing of the ban combined with variation across media outlets in the extent to which they rely on AP-copy, to study first – the effects of the ban on the media coverage of immigration, and second – its impact on public views on immigration policy. I find that one standard deviation higher AP-intensity is associated with a 15% decline in media outlets' use of the term 'illegal immigrant' after the ban. Individuals exposed to outlets with 1 standard deviation higher AP-intensity show 0.7 percentage points lower support for restrictive immigration and border security policies after the ban. This effect is more pronounced for moderates and in locations with fewer immigrants, and does not transfer to views on issues other than immigration.

Keywords: *Mass media, media slant, framing, immigration*
JEL Classification: D72, L82, Z13

*I thank Elliott Ash, Elias Dinas, Ruben Durante, Ruben Enikolopov, Brian Knight, Maria Petrova, Giacomo Ponzetto, Marta Serra-Garcia, Guido Tabellini, David Strömberg, as as well as the participants at several seminars and conferences, for valuable comments and discussions. I acknowledge financial support from the Spanish Ministry of Economy and Competitiveness through Predoctoral Grant BES2016-076728.

†University of Chicago. E-mail: mdjourelova@uchicago.edu

1 Introduction

Political actors choose carefully the words they put out in the media. In the US, Republicans and Democrats often use strikingly different language to describe the exact same issue, in an attempt to promote views favorable to their platform. Republican politicians and right-leaning media speak about the "China virus", the "death tax", and "illegal immigrants", while Democrats and left-leaning media refer to the same issues as "Covid-19", the "estate tax", and "undocumented immigrants".[1] If language has an impact on how the issue is perceived by the public, such partisan tactics could contribute to polarized perceptions of the same factual reality (Alesina et al. 2020).

Yet, evidence on the persuasiveness of slanted language, i.e. on whether it can indeed sway the public in the intended direction, is lacking. From an empirical standpoint this question is challenging for at least two reasons. First, votes-maximizing politicians and profit-maximizing media have an incentive to slant their language to appeal to the views of their audience (Gentzkow and Shapiro 2006), making it hard to disentangle cause and effect. Second, slanted language can be accompanied by other politically motivated choices, ranging from selective emphasis of certain issues (Puglisi and Snyder 2011), to outright endorsement of policies or candidates (Chiang and Knight 2011), which are likely to independently affect the views of the audience.

To overcome these challenges, I propose a supply-side source of variation in media slant. I take advantage of the fact that many US media outlets source some of their content from the Associated Press (AP) – a newswire agency that gathers and distributes news to subscribing outlets. Since AP distributes a single news feed to all subscribers, their aim is to produce neutral coverage that appeals to all sides of the political spectrum (Fenby 1986). This philosophy has led to extremely strict and rigid guidelines for the use of politically sensitive language.

I exploit an abrupt reversal in AP's guidelines regarding a specific politically charged term – 'illegal immigrant'. In April 2013, after years of resisting requests to revise its guidelines, AP switched from officially *recommending* 'illegal immigrant' to refer to people living in the US without legal authorization, to *banning* its use in AP dispatches.

The ban happened at a time when the issue of immigration, and the language used to talk about it, was extremely politicized. Figure 1 illustrates the partisan divide in use of

[1] The implicit policy positions behind these phrases are easy to recognize. "The China virus" is presumably an attempt to shift responsibility for the Covid crisis to China (`https://edition.cnn.com/2020/03/20/politics/donald-trump-china-virus-coronavirus/index.html`). "Death tax" highlights the alleged unfairness of taxing the deceased, while "estate tax" draws attention to the wealth of the people it applies to (`https://www.businessinsider.com/death-tax-or-estate-tax-2017-10?r=US&IR=T`). "Illegal immigrants" underscores the transgression of crossing the border, while "undocumented immigrants" presents the issue of legal status as a formality (`https://www.al.com/news/2018/07/illegal_vs_undocumented_the_he.html`).

the term 'illegal immigrant' in political speech and in the media.[2] In Congress, Republican representatives use the label 'illegal' about 50% of the time they mention the word 'immigrant', while this frequency is less than 5% among Democrats. Similarly, the term appears twice as frequently in the right-leaning Fox News and Washington Times, compared to the left-leaning MSNBC and Washington Post.

Beyond the clear ideological charge of the banned term, this setting has several features that make it attractive to study the causal effects of slanted language. First, AP members include outlets representing views across the ideological spectrum. It would therefore be hard for demand from the audience of any individual outlet to affect AP's style decisions. If this premise is correct, the ban produces variation in an input to the editorial production function that is orthogonal to outlet-specific demand.

Second, anecodotal evidence supports the view that the ban was not politically motivated. According to the AP, the decision was made for consistency with a policy of "delabeling" people in other contexts. For example, about a month prior to 'illegal immigrant' AP banned the term 'schizophrenic', replacing it with 'diagnosed with schizophrenia'.[3]

Third and importantly for my empirical strategy, media outlets differ greatly in the extent to which they rely on AP copy. This allows me to compare outlets with different degrees of use of this input, i.e. different *AP-intensity*, and the views of their respective readers before vs after the ban.

I start off my analysis by documenting how the ban affected AP's immigration coverage, using the text of all immigration-related AP dispatches released between 2009 and 2017. I find that, as intended, the ban caused the term 'illegal immigrant' to instantaneously disappear from AP's feed. As a substitute for the label 'illegal', the new guidelines suggested the phrases "living in the county illegally" or "without legal permission". However, text analysis reveals that these reformulations compensated for at most half of the decline in use of the banned term. Hence, a significant part of the treatment in this natural experiment consists of substitution from 'illegal immigrant' to 'immigrant', without any reference to legal status. At the same time, I find that the volume of AP's immigration coverage and other dimensions of AP's slant remained largely unchanged.

In the second part of the paper I track how the changes in AP's language diffuse into the language of media outlets, using text data from about than 2300 print and online outlets. I employ a generalized difference-in-difference strategy comparing the monthly number of

[2]The reason for this divide can be traced back to deliberate party strategy. For example, the term 'illegal immigrant' was advocated by Republican strategist Frank Luntz, who is famous for developing talking points for Republican candidates and for coining terms such as 'death tax' (instead of 'estate tax' or 'inheritance tax') and 'climate change' (instead of 'global warming'). Luntz has urged Republicans to always use the term 'illegal immigrant' and to put an emphasis on border security, calling the linguistic distinction between 'illegal immigrant' and 'undocumented immigrant' the "political battle of the decade" (Luntz 2007).

[3]This evidence is also supported by the data – I find no pre-trends in AP's use of the term 'illegal immigrant', or in AP's slant on immigration more generally.

'illegal immigrant' articles as a share of 'immigrant' articles before and after the ban, in media outlets with different AP-intensity. Specifically, I measure AP-intensity as the share of 'immigrant' articles published by each outlet prior to the ban that either credit AP explicitly, or are flagged by a plagiarism detection algorithm comparing their text to that of recent AP dispatches.

My results suggest a large degree of diffusion – one standard deviation higher AP-intensity is associated with a 15% decline in use of the term 'illegal immigrant' after the ban. Put differently, outlets that source between 120 and 850 per 1000 articles from the AP (i.e. ones in the top quartile of positive AP-intensity), reduce their use of the term by 60% compared to outlets that don't use the AP at all. This effect is driven mostly by articles sourced from the AP as opposed to original ones. I also find the same gap in reference to legal status as documented for AP dispatches and the same null effects on volume of immigration coverage and on other aspects of slant. A placebo test exploiting the intensity of another major newswire – *Reuters* – confirms that these results are not driven by general differences between outlets that rely on newswire copy more or less.

Given the strong political charge of the term 'illegal immigrant', one could expect differential reactions by left-leaning outlets, for which the ban is ideologically congruent, compared to right-leaning ones, for which it is in-congruent. Indeed, I find stronger degree of diffusion for left-leaning outlets compared to right-leaning ones. This is despite a very similar effect on AP-sourced articles, and is instead driven by differential reactions in original content. Yet on net, use of the term 'illegal immigrant' declines significantly even in right-leaning outlets.

In the third part of the paper I study the impact of the ban on public views on immigration policy, using pre- and post-ban waves of a large electoral survey – the Cooperative Congressional Election Study (CCES). To estimate the reduced form effect of the ban, I employ a generalized difference-in-difference strategy comparing CCES responses before and after the ban, in counties with different AP-intensity of locally circulated newspapers. Alternatively, to scale magnitudes in terms of the number of locally circulated 'illegal immigrant' articles, I instrument their number (normalized by the number of 'immigrant' articles) by the interaction of county-level AP-intensity and the timing of the ban. This strategy accounts for time-invariant county characteristics correlated with AP-intensity, but relies on the assumption that their effect on respondents' views did not change in coincidence with the ban. I address this threat by showing that the estimates are stable to controlling for a wide range of baseline county characteristics interacted with time. Furthermore, in more demanding specifications I control for fixed effects at the level of state or media market (DMA) interacted with time, which absorb many potential unobserved confounds.[4]

The results suggest that one standard deviation higher AP-intensity of locally circulated

[4]Importantly, time fixed effects, and state/ DMA specific time fixed effects absorb any policy changes that occur at the national, state, or regional level.

media is associated with 0.7 percentage points lower support for increasing border security after the ban. This corresponds to 1.8% of the gap in support for border security between Republican and Democrat respondents. It implies a persuasion rate, i.e. share of readers exposed to the treatment who changed their position (DellaVigna and Kaplan 2007; DellaVigna and Gentzkow 2010), in the range of 1.5 to 3.8%. These estimates are robust to controls, are not driven by pre-trends, and do not reflect general reliance on newswire content as measured by *Reuters*-intensity.

I observe a similar shift in support for restricting immigration in 4 out of the 5 policy questions I am able to track across pre- and post-ban survey waves, as well in an index aggregating all immigration-related CCES questions, including rotating ones. Specifically, I find significant effects on support for increasing border security, on allowing police to question suspected unauthorized immigrants, on fining firms that employ them, on prohibiting their access to public services, and no significant effect on support for legalization.

Moreover, a falsification exercise looking at views on other politically divisive policies suggests that these effects are specific to immigration policy. I find no effect on the responses to any of the other policy items in the CCES, which range questions on abortion and gay marriage to healthcare and economic policy.[5]

While the above results apply to the sample of all CCES respondents, they are more pronounced for the 33% of respondents who self-report reading a print newspaper frequently (i.e. have done so the day before the interview). The effects are also stronger for self-described moderates compared to conservatives or libreals, and for respondents living in places with lower immigrant population. These patterns of heterogeneity are consistent with a persuasion mechanism since news consumers with moderate ideology, or ones with less direct exposure to immigrants, are likely to have weaker priors on the issue, and hence, hold views more susceptible to media framing.

This paper contributes to several streams of literature. First, it ties to a literature on the effects of media on political attitudes and outcomes. The majority of this research exploits quasi-random or experimentally manipulated variation in access to a particular media outlet to estimate its causal effects (DellaVigna and Kaplan 2007; Martin and Yurukoglu 2017; Enikolopov et al. 2011; Durante et al. 2019; Gerber et al. 2009; Levy 2021). By design, the "treatment" in this strategy consists of the bundle of editorial choices that differentiate the outlet of interest from alternative sources of information. Fewer studies are focused on a specific mechanism of media persuasion – e.g. the volume of coverage of a politician or a politically sensitive issue (Puglisi and Snyder 2011; Snyder and Strömberg 2010; Bursztyn et al. 2020b) or direct endorsement of candidates for office (Chiang and Knight 2011). This paper

[5]On net, the relatively small and localized effect on immigration policy views appears (on average) insufficient to shift party support in national elections and I detect no effect on voting or intent to vote for Republican candidates stated in the CCES. I also find no effect on Republican vote share in electoral data.

fits into the second category, but differs by studying an arguably more subtle mechanism – that of slanted language.[6]

Closely related is also research on slant in the language used by media and politicians (Groseclose and Milyo 2005; Gentzkow and Shapiro 2010; Gentzkow et al. 2019), which has focused on the measurement of slant and on its determinants. Importantly, Gentzkow and Shapiro (2010) show that the attitudes of consumers explain about 20% of the variation in slant of US newspapers. Here I study the reverse direction of causality – from exposure to slanted language towards the political views of readers, trying to separate this channel from the tendency of media outlets to serve consumers' preference for like-minded content.

My findings suggest that views on immigration policy can be sensitive to changes in the framing of the issue. This lines up with recent evidence of widespread misinformation regarding the characteristics of immigrants and the effects of immigration (Haaland and Roth 2020; Grigorieff et al. 2019; Alesina et al. 2019; Hatte et al. 2019). While in experimental settings information treatments aiming to correct misperceptions have shown significant but generally small effects, there is some evidence that narrative or "framing" treatments may be more effective (Bursztyn and Yang 2021).[7]

Framing effects, which occur when differences in the presentation of an issue affect individuals' responses, are themselves subject to a large literature in communication and social psychology (Strömberg 2015; Scheufele and Tewksbury 2007; Chong and Druckman 2007).[8] Closest to this paper are survey experiments that randomize respondents' exposure to various immigration-related frames and evaluate their effects on policy questions (Merolla et al. 2013; Haynes et al. 2016; Ommundsen et al. 2014; Knoll et al. 2011). I use a different methodology based on a natural experiment, and provide large scale observational evidence of framing effects "in the wild".

Finally, the large magnitude of diffusion of AP's ban documented in the first part of the paper, and the absence of significant resistance or backlash by right-leaning outlets, suggest a degree of inertia in content production and imply significant power of news wire agencies to influence media slant. This complements evidence from the context of French online media (Cage et al. 2020).

The rest of the paper is organized as follows. In section 2 I discuss the details of the ban

[6]It is not clear a priori how the possible persuasive effect of slanted language might compare to that of more obvious biases. On the one hand, slanted language arguably represents a much milder treatment. On the other hand, more direct biases may be easier for consumers to notice and discount by either switching away from the biased media outlet (Durante and Knight 2012) or by taking its ideological stance into account when making political choices (Chiang and Knight 2011).

[7]For example, a treatment arm in (Alesina et al. 2019) exposing respondents to an anecdote about a "hard-working" immigrant is relatively more effective in correcting misperceptions compared to a treatment providing statistical information.

[8]A tangentially related literature in economics studies the relationship between linguistic features and economic behaviors across societies (see Ginsburgh and Weber (2020) for a recent review).

and analyze how AP's coverage of immigration changed. In section 3 I track the diffusion of the ban into the language used by media outlets across the US. In section 4 I analyze the effect of the ban on views public views on immigration policy.

2 The Ban and It's Effect on AP's Language

2.1 Background

The term 'illegal immigrant' was dropped from AP's guidelines on April 3rd 2013. The decision was surprising since AP had previously resisted pressures from advocacy groups to change their language on the issue.[9] In fact, up until the ban, AP's guidelines stated that 'illegal immigrant' was the preferred "best-use" term. The alternative endorsed by the left – 'undocumented immigrant' – was not sanctioned because AP considers it legally inaccurate, as continues to be the case to this day.

As a reason for the sudden reversal, in their announcement AP named a policy to not label people that was being implemented in other areas (`https://blog.ap.org/announcements/illegal-immigrant-no-more`). For example, a month earlier the term 'schizophrenic' was banned and replaced by 'diagnosed with schizophrenia'. According the announcement, this policy led AP executives back to reconsider 'illegal immigrant'.

Table 1(a) presents the exact wording of AP's guidelines before and after April 2013. As 'illegal immigrant' was banned, the new guidelines suggested the following substitutes: 'living / entering the county illegally / without legal permission'. Yet, AP executives recognized in their statement the possibility that these alternatives may "make it harder for writers" compared to the concise label 'illegal'. The ban took effect immediately in the online guidelines, which are also embedded in text editors (figure A1).

The ban was perceived as highly influential due to AP's prominent role in the US media landscape (figure A2). AP operates as a not-for-profit cooperative of about 1300 US newspapers and broadcasters. Members can have regular or associate status and in both cases have access to AP news and photos, share costs and contribute content. Regular membership, available to print newspapers published in the US, also entails permission for the AP to distribute local news reports produced by the member.[10]

While AP is the main newswire used in US media, *Reuters* offers competing services and is used as a substitute by some newspapers.[11] Since Reuters did not implement any

[9] `https://www.sfexaminer.com/national-news/society-for-professional-journalists-says-using-the-term-illegal-immigrant-is-unconsitutional/`

[10] `https://www.ap.org/about/annual-report/2016/AssociatedPress_2016FinancialStatements.pdf`

[11] `https://www.poynter.org/reporting-editing/2014/eighteen-months-after-dropping-ap-tribune-happy-with-reuters/`

changes in their rules on the term 'illegal immigrant', in some parts of the analysis I exploit Reuters-intensity as a placebo.

2.2 Data

To shed light on the nature of the treatment in this natural experiment, I obtain the text of all immigration-related AP dispatches released in the period July 1st 2009 to July 1st 2017. Specifically, I search the database of *Factiva* (`https://global.factiva.com`) for mentions of the word 'immigrant' (singular or plural), restricting the source to "Associated Press Newswires". This search results in 28,000 dispatches, 8,000 of which mention the phrase 'illegal immigrant'. I record the date, headline and full text of each dispatch.

2.3 Text Analysis Results

Example and descriptive evidence I start off with an example of how the ban affected AP's language on immigration, even when it comes to covering the same issue. Table 1(b) presents two dispatches covering proposed state laws on immigrants' drivers licenses – one in Minnesota, released about 2 weeks before the ban – and one in Oregon, released 2 weeks after. Both dispatches have a neutral tone and mention arguments for and and against the law from both political sides. However, the headline of the first dispatch talks about licenses to 'illegal immigrants', while the second headline talks about an 'immigrant drivers' license bill', which arguably changes the framing of the story. Figure A4 shows that this is not an isolated case by plotting the most frequent 3-grams encountered in headlines of 'immigrant' dispatches before and after the ban – the label 'illegal' clearly features prominently before the ban, and virtually disappears after.

The timing of this change in language coincides very precisely with the announcement of the ban. Figure 2(a) shows the monthly number of AP dispatches mentioning the term 'illegal immigrant' as percent of dispatches mentioning the word 'immigrant'. The share drops from an average of 50% in the period before April 2013, to less than 5% after, suggesting close to perfect compliance.[12] Figure 2(b) plots the frequency of the substitutes suggested by AP's guidelines. While their use increases sharply after the ban, the magnitude of this increase is relatively small (6 percentage points) and insufficient to compensate for the large decline in the term 'illegal immigrant' (45 percentage points). At the same time, figure 2(c) suggests that the volume of 'immigrant' dispatches did not change around the ban. in figure A5 I repeat this analysis focusing on headlines and find virtually no mentions of the term 'illegal immigrant' after the ban, and no mentions of the substitutes proposed by the AP.

[12]Note that this figure includes mentions of 'illegal immigrant' in direct quotes, which are not affected by the ban.

Changes in AP's language I use two approaches to gather more exhaustive evidence on how AP's language changed before vs after the ban. First, I analyse changes in the rate of co-occurrence of the word 'immigrant' with any other unigram from the AP corpus.[13] Figure 3(a) plots the results for the top 50 correlates of the word 'immigrant', with pre-ban correlations on the x-axis and post-ban correlations on the y-axis. Hence, points close to the 45-degree line indicate no significant change in the rate of co-occurrence with the word 'immigrant'.

The word 'illegal' is clearly an outlier from the 45-degree line – its correlation with 'immigrant' drops dramatically from 0.66 before the ban – the highest among all unigrams in the corpus – to 0.21 after the ban. Yet, the figure also suggests that other unigrams do not compensates for this decline. The closest candidate is 'illegally' – indeed, its correlation with 'immigrant' increases significantly, but the magnitude corresponds to only about half of the decline in 'illegal'. In headlines, this substitution is almost completely absent (figure A6(a)).

An alternative, and more flexible approach to examine how language changes due to the ban, is to ask which words and phrases have the highest power in predicting whether a given AP dispatch was published before or after the ban. Let $f_{pl,before}$ and $f_{pl,after}$ denote the total number of times phrase p of length l (one to 3 words) is used before and after the ban, respectively. Let $f_{\sim pl,before}$ and $f_{\sim pl,after}$ denote the total occurrences of length-l phrases that are not phrase p – before and after the ban respectively. Pearson's χ^2 statistic for phrase p of length l is then:

$$\chi^2{}_{pl} = \frac{(f_{pl,before}f_{\sim pl,after}f_{\sim pl,after}f_{\sim pl,before})^2}{(f_{pl,before}+f_{pl,after})(f_{pl,before}+f_{\sim pl,before})(f_{pl,after}+f_{\sim pl,after})(f_{\sim pl,before}+f_{\sim pl,after})} \tag{1}$$

Figure 3(b) presents the 20 words and phrases with highest χ^2. 'Illegal', 'illegal immigrant' and 'illegal immigrants' clearly emerge as most diagnostic of whether an article is published before or after the ban. Notably, 'illegally' has only 1/4 of the predictive power of 'illegal', confirming that synonyms indicating legal status were adopted only partially. For headlines this substitution once again appears to be absent as the word 'illegally' does not feature in the list of predictive phrases (panel (b) of Figure A6). There are also no other phrases that emerge as predictive of post-ban publishing date.

AP's slant on immigration Was the the ban on 'illegal immigrant' part of a broader trend towards more liberal slant in AP's immigration coverage? To examine this question, I compute a measure of immigration-specific slant based on the similarity of AP's language to that used by Republicans vs Democrats in Congress when speaking about the issue.

[13] I stem all words with the exception of 'illegal' and 'illegally' to account for the fact that while 'illegal' was banned, 'illegally' was, if anything, endorsed in the new guidelines.

The procedure follows Gentzkow and Shapiro (2010) in first selecting a set of phrases most predictive of partisanship in Congress, and then measuring their relative occurrence in AP's text over time (see Appendix A.2 for more detail on the method). In order to isolate the influence of the ban from other dimensions of AP's language on immigration, I also compute a version of the index excluding the phrase 'illegal immigrant' and its substitutes.

Table A1 presents the top partisan phrases that enter these measures. In line with intuition, phrases such as *'illegal immigrant'*, *'secure border'*, *'drug cartels'* are classified as Republican, while phrases such as *'undocumented immigrant'*, *'domestic violence'* and *'hate crime'* are classified as Democrat. Figure 3(c) shows the evolution of the resulting two versions of the slant index over time. The version including the phrase 'illegal immigrant' follows closely the trend in AP's use of this term. This is intuitive since the phrase is very highly predictive of partisanship in Congress (as seen in figure 1) and therefore receives a disproportionate weight in the index. However, once 'illegal immigrant' and its substitutes are excluded and I focus on other dimensions of language, I find no change in slant over time.[14]

Changes by topic Finally, to rule out the possibility that these results reflect a shift in the immigration-related topics occupying the news cycle over the sample period, I implement a Latent Dirichlet Allocation (LDA) topic model (Blei et al. (2003), described in Appendix A). I use the model to classify dispatches into 5 topics that can be labeled as follows: law enforcement, legislation, immigrants' integration and social issues, international issues such as the refugee crisis in Europe, and elections (figure A7). I find that the distribution of dispatches across these topics is relatively similar before and after the ban (figure A8). Furthermore, all results of the analysis discussed thus far are confirmed within topic (figures A9, A10 and A11).

Taking these results together, the analysis of AP text suggests that: (1) As intended, the label 'illegal' virtually disappears from AP's feed after the ban; (2) This is only partially compensated by the substitutes proposed by AP, and for the most part, the ban leads to omission of direct references to legal status; (3) Other measurable dimensions of AP's language, volume of immigration coverage, or slant, did not change with the ban.

3 Diffusion

I next turn to analyzing the diffusion of AP's ban from AP dispatches into the language of media outlets with different baseline reliance on AP copy.

[14]Furthermore, I find no clear pre-trend in either version of the index. This confirms the anecdotal evidence that the change in AP's language was sharp and unexpected.

3.1 Data

Media content: 'immigrant' and 'illegal immigrant' articles My main data source for media content is NewsLibrary (`newslibrary.com`). I focus on print and online outlets that are covered in the database continuously between July 1st 2009 and July 1st 2017 – there are 2566 such outlets. For major US newspapers which are missing in NewsLibrary, I use data from ProQuest (`proquest.com`). This adds 125 newspapers.

I search the databases for articles that mention the word 'immigrant' (in singular or plural) and ones that mention the term 'illegal immigrant' (in singular or plural). This search results in about one million 'immigrant' articles and 200,000 'illegal immigrant' articles. I record each article's date of publication, name of the publishing outlet, by-line, headline, and the text of the first paragraph.[15] Using this information, I compute for each outlet and each month the number of 'illegal immigrant' articles normalized by the number of 'immigrant' articles.

Identifying articles copied from AP I classify an article as sourced from AP if either one of two conditions is true: (1) "AP" or "Associated Press" is explicitly mentioned in the byline or in the first paragraph (e.g. "according to the AP"), or (2) a large portion of the text of the article is verbatim identical of to the text of a recent AP dispatch.

The first criterion aims to capture cases in which AP is credited explicitly. A similar procedure is employed by Gentzkow and Shapiro (2010) to identify and, in their case, *exclude* news-wire content. Their audit of excluded articles suggests that "virtually all" are indeed wire-copy. However, if media outlets use AP-copy without explicit credit, this procedure alone is likely to produce false negatives. Evidence on copying from the French news wire AFP suggests that this may indeed be a common occurrence (Cage et al. 2020).

Therefore, the second criterion looks at text similarity to recent AP-dispatches, regardless of credit. Specifically, I employ a plagiarism-detection algorithm comparing each article to the set of dispatches released by AP the day before the article is published. The goal is to detect articles in which large portions of text are verbatim identical to an AP dispatch. In practice, the algorithm looks for overlap in sets of 5-grams that exceeds the threshold of 25% of text. I describe this procedure in detail and discuss summary statistics on crediting and plagiarism in Appendix A.3.

AP-Intensity To proxy media outlets' exposure to the change in AP's language, I measure the rate of copying from AP over the 12-months prior to the announcement of the ban. I focus on the pre-ban period to avoid concerns about potential selection in AP use in response to

[15] At the time of data collection NewsLibrary allowed free keyword searches showing the first paragraph of all matching articles, but access to the full article was restricted by a paywall.

the change in the language policy.[16] To rule potential anticipation of the ban in the months leading up to it, I also implement robustness checks considering time windows of 24 to 12 months or 24 to 36 months prior to the announcement of the ban.

I measure *AP-intensity* as the the number of articles sourced from AP per 1000 articles in the selected pre-ban period – either credited to AP explicitly or flagged by plagiarism detection. Figure B3 presents the distribution of AP-intensity. The number of AP-sourced articles ranges from 0 to 820 per 1000. Since this variable contains many zeros and has a skewed distribution, in the following analysis I use its inverse hyperbolic sine transformation.[17]

Additional data on print newspapers For print newspapers, I collect data on the location of headquarters and on baseline circulation from the 2010 *Editor & Publisher* yearbook (digitized from the print edition). This covers the universe of daily print newspapers. I also collect data on circulation over time from the historical reports of the Alliance for Audited Media, available for the period 2011-2017 for 380 newspapers in my sample.[18] I also use the index of ideological slant constructed by Gentzkow and Shapiro (2010), which covers about 370 newspapers in my sample.

3.2 Empirical Strategy

To estimate the rate of diffusion from AP's language to the language used by media outlets I implement a difference-in-difference strategy with continuous treatment. Specifically, I exploit the timing of the announcement of the ban and variation across media outlets in exposure to the ban, proxied by AP-intensity. I estimate regressions of the following form:

$$Illimm/Imm_{mt} = \alpha_m + \beta_t + \rho IHS(APintensity_m) \times PostBan_t + \epsilon_{mt}, \tag{2}$$

where $Illimm/Imm_{mt}$ denotes the number of 'illegal immigrant' articles in media outlet m and month t as percent of 'immigrant' articles, IHS($APintensity_m$) is IHS-transformed AP-intensity measured in the 12 months prior to the ban, $PostBan_t$ is a dummy for post-April 2013, and α_m and β_t are outlet and calendar month FEs. I also consider specifications further controlling for state X month or DMA x month FEs. Standard errors are clustered at the outlet level. To account for the fact that $Illimm/Imm_{mt}$ is imprecisely estimated when the denominator, i.e. the number of 'immigrant' articles is low, which is a frequent occurrence at monthly frequency, I weigh observations by the number of 'immigrant' articles.[19]

[16] Indeed, figure B1 suggests somewhat of a decline in AP-use after the ban.

[17] About 60% of outlets in the baseline sample and 72% of outlets in the sample of print newspapers (Table B1) have positive AP-intensity.

[18] The more comprehensive data from E&P is not readily available for the period after the ban.

[19] The reason I normalize the number of 'illegal immigrant' articles by 'immigrant' articles is twofold. First, the coverage of the NewsLibrary data is not universal and varies across outlets and years, which makes the

The identifying assumption in this strategy is that the frequency of "illegal immigrant" articles in outlets with high AP-intensity vs outlets with low AP-intensity would have followed parallel trends in the absence of the ban. To examine the plausibility of this assumption as well as the timing of the effects, I also estimate a dynamic version of the above equation, splitting the dummy for post-ban into a set of half-yearly leads and lags.

3.3 Results

Preliminary evidence Before proceeding to the estimation of the regression specified in 2, I examine visually the frequency of 'illegal immigrant' articles in media outlets with positive vs zero AP-intensity. Figure B2 shows these two series. While non AP-intensive outlets appear to gradually decrease their use of the term prior to the ban, for AP-intensive outlets it remains flat and relatively high up until it falls sharply in coincidence with the ban. This pattern is in line with anecdotal evidence – while the term was becoming more controversial in the years leading up to the ban, it continued to be recommended as "best use" in AP's guidelines. The figure also suggests that the ban was somewhat of an aggregate shock: non AP-intensive outlets also exhibit a decline at the time of the ban, albeit of a smaller magnitude.[20] Both of these features of the data would lead me to underestimate the impact of the ban in a difference-in-difference setting, so that the estimates I provide should be interpreted as a lower bound.

Diffusion estimates Table 2 presents the main regression results corresponding to specification 2, estimated for the full sample of print and online outlets in panel (a) and for a sample restricted to print daily newspapers in panel (b). I consider use of the term 'illegal immigrant' in headlines in column 1, and in full text in the remaining columns. In both samples, and in both headlines and full text, I find a significant reduction in use of the term 'illegal immigrant' in AP-intensive outlets relatively to less AP-intensive ones after the ban. The magnitude suggests that one standard deviation higher AP-intensity (=2.3 IHS-points) is associated with a reduction in the share of 'illegal immigrant' articles of 3.4 p.p, or about 16% relative to the mean (14% in the sample of print newspapers). The estimates are robust to controlling for month times state fixed effects (column 2), which absorb the effects of any policy changes or newsworthy events that vary by state. For print newspapers I can further control for finer fixed effects at the level of Nielsen's designated market areas (DMAs), which

absolute number of articles somewhat harder to interpret. Second, normalizing by the number of 'immigrant' articles allows me to focus on changes in the *language* used to talk about the issue, conditional on the *volume* of coverage devoted to it. Yet, I obtain consistent results without normalization, i.e. taking the (IHS-transformed) absolute number of 'illegal immigrant' articles as dependent variable (table B2).

[20] This may be due to non AP-intensive outlets interpreting the ban – which was widely publicized – as a signal that the term 'illegal immigrant' is no longer politically correct. It may also reflect measurement error if my procedure does not capture perfectly reliance of the AP.

I assign based on the address of the newspapers' headquarters. This absorbs shocks that occur at this finer level and restricts the comparison within areas with relatively homogeneous TV and online news consumption profiles. The estimates are unaffected by the inclusion of these controls (column b 3).

To examine the timing of this effect in figure 4(a) I split the interaction of AP-intensity and Post-Ban into a set of interactions with half-yearly leads and lags, with the 6 months before the ban as the omitted category. I find if anything an increasing pre-trend (consistent with figure B2), which would lead me to underestimate the degree of diffusion of the ban. The effect on 'illegal immigrant' articles occurs precisely when the ban is announced and is persistent over time, in line with the permanently low supply of 'illegal immigrant' AP dispatches.

In figure 4(b) I estimate a more flexible specification interacting each quartile of the distribution of positive AP-intensity with Post-Ban, leaving outlets with zero AP-intensity as the reference category. The results suggest that the effect of the ban is monotonically increasing in AP-intensity. Relative to outlets with zero AP-intensity, those in the top quartile, i.e. ones that soure between 120 and 850 articles per 1000 articles from the AP, reduce their use of the term by 12 p.p., or by about 60% relative to the mean.

AP-sourced vs original articles If the documented decline in 'illegal immigrant' articles is linked directly to the change in the language in AP's language, one would expect that it is driven by articles sourced from AP rather than original content. To test this, in the last two columns of table 2 I split the dependent variable into 'illegal immigrant' articles that I identify as sourced from AP. i.e. either crediting AP explicitly or flagged by plagiarism detection, vs. the rest. Indeed, I find that on average, the diffusion effects are driven mostly by AP-sourced content and the impact on original articles is instead small only marginally significant in the full sample.[21] Figure 4(c) shows the dynamic effects for the two groups of articles and confirms that pre-trends are if anything increasing.

Robustness and Reuters placebo In table 3 I probe the robustness of the estimates to different variations of the AP-intensity variable. Instead of accounting for both crediting AP and plagiarism, in column (1) I consider only the share of articles credited to AP, and in column (2) – only the share of articles flagged by plagiarism detection. The two measures have a correlation of 0.56 and yield very similar results. In columns (3) and (4) I shift the window in which I measure AP-intensity to 12-24 months and 24-36 months before the ban respectively. I again obtain very similar results, which speaks against selection in AP use in anticipation of the ban. In column (5), rather than examining the sample of 'immigrant'

[21] I find significant declines of similar magnitude looking separately at AP-credited articles and at those flagged by plagiarism detection.

articles, I consider articles on any topic and define AP-intensity as the share of total articles published the 12 months before the ban that credit AP.

As a placebo exercise, in column (6) and (7) I consider use of Reuters rather than AP. Since the Reuters news-wire did not change their style rules on 'illegal immigrant', prior reliance on Reuters should not be associated with changes in use of the term.[22] Indeed, I find no effect when using Reuters-intensity as a placebo treatment.

Diffusion by outlet's ideology Media outlets decide on the extent to which they want to use AP dispatches and are free to edit their content as they wish. Therefore, a natural question is whether the diffusion of the ban differs by the ideological stance of the outlet. To answer this question I analyze a sub-sample of about 370 newspapers which I can match to the index of ideological leaning constructed by Gentzkow and Shapiro (2010). I then estimate the difference-in-difference regression from equation 2 separately for each quartile of the index.

Table 5(a) shows that the ban causes a significant decline in use of the term 'illegal immigrant' in 3 out of the 4 samples, but the relative magnitude of the effect differs. Relative to the sample mean, the degree of diffusion is almost twice larger for the most left-leaning outlets, for which the change in AP's language is presumably ideologically congruent, compared to the most right-leaning ones, for which it is presumably in-congruent. Figure 6 illustrates this result by plotting the diff-in-diff coefficients for the outcome standardized to mean zero and standard deviation one within each sample, and figure B4 confirms the absence of pre-trends in each sample.

With the classification of AP-sourced vs original articles at hand, I can further examine whether this difference stems from different use of AP-copy, or rather from original content. Table 5(b) shows very similar relative effects when it comes to 'illegal immigrant' articles sourced from AP. Instead, table 5(c) suggests opposing effects on original content. I find a negative coefficient for left-leaning outlets, and a positive one for center- and right-leaning outlets – consistent with a possible spillover of AP's language into the original content of ideologically congruent outlets, and backlash from ideologically in-congruent outlets. Yet, the effects are only marginally significant and small in magnitude, so that on net the effects through AP-sourced content dominate.

Overall, the heterogeneity results are consistent with a degree of inertia in content production, such the ban leads to a net reduction in use of the term 'illegal immigrant' even in right-leaning outlets. This is despite their likely ideological opposition to the change in AP's language and possible backlash in original content.

[22]Since use of Reuters copy is much more rare in my sample, I express Reuters-intensity as the share of Reuters-sourced articles per 10,000.

Effects on readership Another margin along which the ban could have faced resistance is the response of readers, who may reduce their readership if the ban is in-congruent with their political views (Durante and Knight 2012). In table B3 I examine the effect of the ban on log annual circulation, looking at the full sample of 380 newspapers in column (1), and splitting the sample by ideology in the remaining columns. I find no evidence of readership effects – neither on average, nor in for right-leaning newspapers whose readers may be most likely to respond to the ban.

Diffusion by outlet size On the other hand, the ban may have limited potential to affect public opinion if the diffusion effects documented above are concentrated in outlets with low readership. I test this using data on circulation in the year 2010 available for all print newspapers. I group newspapers into quartiles of circulation and estimate separate difference-in-difference regressions for each subsample. The results in table B4 suggest significant diffusion of similar magnitude regardless of newspapers' readership.

Other aspects of immigration coverage I next turn to testing whether other measurable aspects of coverage were also affected by the ban. Consistent with the findings documented for the corpus of AP dispatches, table 4(a) shows that the substitutes approved by AP increase significantly but compensate only a small fraction of the decline use of the term 'illegal immigrant'. Also consistent with AP-dispatches, I find that the number of 'immigrant' articles normalized by total articles is not affected by ban. The same null-effect applies to articles mentioning the word 'immigration' over total articles.

In table 4(b) I examine the effect of the ban on news outlets' immigration-specific slant. Following the procedure applied to AP's text, I compute an index of slant based on the similarity of the language of a given news outlet to that of Republican vs Democrat representatives in Congress.[23] Again, similarly to AP dispatches, the slant index computed including the term 'illegal immigrant' follows closely the frequency of this term and shifts significantly in a more left-leaning direction in response to the ban. However, the version of the index that excludes the term 'illegal immigrant' and its substitutes is not affected – the difference-in-difference coefficients suggest an insignificant effect with magnitude close to zero. Figure 5 confirms these findings in dynamic difference-in-difference setting.

To sum up, several measurable features of the language of AP dispatches diffuse into the language used by media outlets, consistent with the result that AP-sourced articles drive the majority of the diffusion of the ban. The null effects on volume of immigration coverage and on other dimensions of slant confirm that the ban can be interpreted as a shock to use of the term 'illegal immigrant', while leaving other features of immigration coverage unaffected.

[23]See appendix A.2 for details on this procedure.

4 Views on Immigration Policy

Having established that the ban produces significant variation in media outlets' use of the term 'illegal immigrant', in this section I exploit this variation to study how it affected public views on immigration policy. To this end, I compare pre- and post-ban responses in the CCES electoral survey in counties with different AP-intensity of locally circulated newspapers.

4.1 Data

4.1.1 Aggregation to the county level

Since the CCES does not ask *which* media outlets the respondent reads, I rely on the county of residence and assess exposure to locally circulated newspapers. The first step in this analysis is to aggregate my measures of newspapers' content to the county level. To do so, I obtain data on the geographic distribution of daily newspapers' circulation from Alliance of Audited Media (AAM). I use their Fall 2012 "Newspaper GEO/Circ" report, which includes circulation by newspaper and zip-code from the most recent audit prior to this date, and aggregate zip-code level data to the county level.[24] Since AAM does not collect geographically disaggregated data for low-circulation newspapers, I impute these observations with data on total circulation from the Editor and Publishes yearbooks, assuming that small newspapers circulate mainly in the county of their headquarters.[25] [26]

I match this data to the data on content from NewsLibrary and ProQuest based on the name, town and state of the newspaper. I then keep counties for which newspapers matched to the NewsLibrary/ ProQuest sample account for at least 90% of total county circulation. This ensures that the county-level data on newspapers' content is measured with reasonable precision. The resulting dataset contains about 2300 counties (out of a total of 3000), and 800 daily newspapers (out of a total of 1200).[27]

I aggregate AP-intensity to the county level by averaging the AP-intensity of newspapers circulated within the county (in number of AP-sourced articles per 1000), weighting each newspaper by its county-specific circulation. Formally:

$$APintensity_c = \frac{\sum_m (circ_{mc} \times APintensity_m)}{\sum_m circ_{mc}}, \tag{3}$$

[24] For the largest nationally circulated newspapers AAM only reports circulation at the DMA level. For these cases I assign circulation to counties in proportion to adult population (i.e. over the age of 18).

[25] The same procedure is used by Seamans and Zhu (2014).

[26] Similar results obtain using data on circulation by newspaper and county compiled by Snyder and Strömberg (2010). This dataset is based on circulation reported to the Audit Beaurau of Circulation (ABC, which later became "Alliance for Audited Media"), combined with the Standard Date and Rate Service for non-ABC newspapers. The drawback of this dataset is that it only covers the period up to 2006.

[27] The results are robust to alternative thresholds, see table C6.

where $circ_{mc}$ is circulation of newspaper m in county c. Figure C1 presents the resulting geographic distribution of AP-intensity. Similarly, I aggregate the circulation-weighted share of 'illegal immigrant' relative to 'immigrant' articles by county and year:

$$Illimm/Imm_{cy} = \frac{\sum_m (circ_{mcy} \times Illimm/Imm_{my})}{\sum_m circ_{mcy}}. \quad (4)$$

Correlates of AP-intensity To assess the correlates of AP-intensity and control for them, I collect data on county-level demographic, economic and political characteristics. Data on annual county population is from ICHS. Data on population density and the urban share of population is from the 2010 census. Turnout and the Republican vote share in the 2012 presidential election are from Dave Leip's Atlas. Racial composition, share college educated, share foreign-born and log income per capita are from the 2012 5-year American Community Survey, and 2012 unemployment rate is from BLS. Air distance from the county centroid to the Mexican border is computed in GIS. Finally, county-level newspaper circulation per capita is estimated with data from the Alliance of Audited Media 2012 "GEO/Circ" report combined with the Editor and Publisher 2010 yearbook.

Figure C2 presents the univariate correlations of each of these variables with AP-intensity. AP-intensity is significantly positively correlated with Republican vote share and significantly negatively correlated with with the share of college educated, with the share of foreign-born and with county-level newspaper circulation. This is consistent with the notion that that smaller newspapers are more likely to resort to sourcing content from the AP, rather than producing original reporting. A higher educated audience, as well as one with more immigrants may also have higher demand for original content, particularly on immigration.

4.1.2 The CCES Survey

To assess how public opinion on immigration policy changed in response to the ban, I use a large nationally representative survey – the *Cooperative Congressoional Election Study* (CCES). CCES is a repeated cross-section with 50 to 60,000 respondents per wave in election years and smaller waves of 10 to 20,000 respondents in non-election years.[28] The survey is administered online and a large portion of participants are YouGov panelists. Importantly for my setting, a large share of survey respondents (33%) report that they regularly read a newspaper in print, i.e. that they have done so in the day before they are surveyed.

Views on immigration policy CCES respondents are asked to select immigration policies they think the US government should implement out of a list of options. The set of

[28]To the best of my knowledge, CCES is the only large-scale survey conducted between 2009 and 2017 that asks questions related to immigration policy.

options differs in each wave and can include increasing border security, granting legal status, allowing the police to question suspected unauthorized immigrants, fining their employers, and prohibiting their access to public services. Table C2 presents the coverage and exact formulation of the policies in the survey.[29] I examine each of the 5 questions separately, constructing a dummy variable for whether the policy was selected or not. I also aggregate all immigration-related questions, including ones asked only sporadically or in a single survey wave, into an index. This includes a total of 12 questions. I recode the response to each one in the direction of restricting immigration, and take the average across the standardized responses (following Kling et al. (2007)).

Views on other policies and voting As placebo outcomes, I also use CCES questions that relate to policy issues other than immigration. I consider all policy questions that appear in the questionnaire at least once before the ban and at least once after. Specifically, I create dummies for: support for always allowing women access to abortion, opposition to gay marriage, support for repealing the Affordable Care Act, opposition to affirmative action, preference for spending cuts rather over tax increases, preference for sales taxes over income taxes, support for foreign military interventions, preference for jobs over the environment, and the opinion that the economy has gotten worse over the past year (see Table C3 for details).

The survey also asks about the respondent's vote (if surveyed after an election) or voting intention (if surveyed before an election) in presidential, Senate and House elections, as well as about the respondent's approval of the incumbent president. I code dummy variables equal to one if the respondent voted or intends to vote for a Republican candidate, and record disapproval of Obama/ approval of Trump on a 4-point scale.

I also obtain county-level Republican vote shares for the 2012 and 2016 presidential elections, i.e. share of votes for Romney and Trump respectively, as well as Republican vote share in House and Senate races from David Leip's Election Atlas.

4.2 Empirical Strategy

To identify the intention-to-treat effect of the ban on views on immigration policy, I implement reduced form difference-in-difference regressions with continuous treatment of the following form:

$$X_{icy} = \alpha_c + \beta_y + \rho IHS(APintensity_c) \times PostBan_y + \epsilon_{icy}, \tag{5}$$

where X_{cy} denotes immigration policy preferences of respondent i in county c and year

[29] I follow the harmonization provided in Dagonel (2021).

y, $APintensity_c$ is the AP-intensity of newspapers circulated in county c, $PostBan_y$ is an indicator equal to one for survey waves carried out after the ban, and α_c and β_y are county and survey-year fixed effects respectively. Standard errors are clustered by county.

To identify the local average treatment effect of exposure to the term 'illegal immigrant' in local media I estimate 2SLS equations of the form:

$$X_{icy} = \mu_c + \nu_y + \gamma \widehat{Illimm/Imm}_{cy} + \eta_{icy}, \tag{6}$$

$$Illimm/Imm_{cy} = \alpha_c + \rho_y + \gamma IHS(APintensity_c) \times PostBan_y + \epsilon_{cy} \tag{7}$$

The first-stage equation has the same form as in section 3, but is now estimated at the county times survey-year level (instead of at the level of media outlet times month). The excluded instrument for the potentially endogenous frequency of 'illegal immigrant' articles $Illimm/Imm_{cy}$ is the interaction of AP-intensity with an indicator for the period after the ban. The approach is thus akin to a shift-share strategy where $PostBan$ is an aggregate shock and $IHS(APintensity_c)$ is local exposure to that shock.

I estimate the two sets of equations at the respondent-level, controlling for respondent characteristics that are likely to correlate with views on immigration policy – age, age squared, gender, and categories of education, race, immigration status and household income.[30]

The identifying assumption is that policy views in counties with high vs low AP-intensity would have evolved similarly in the absence of the ban. Although these counties are likely different (as suggested by figure C2), any time-invariant county characteristics correlated with AP intensity are absorbed by county fixed effects. Therefore, if unobserved county characteristics are to confound my results, their effect on policy views would have to *change* at the same time as the ban took effect. To account for this possibility, I examine the sensitivity of the estimates to controlling for a host of observed county characteristics (detailed in section 4.1.1) measured at baseline and interacted with year fixed effects. I also consider specifications that restrict the identifying variation within state, or within media market, by including state×year or DMA×year fixed effects, which absorb any unobserved confounders that vary at that level.

Additionally, a strict causal interpretation of the 2SLS estimates requires the assumption that the ban did not affect immigration policy views through channels other than use of the term 'illegal immigrant' in locally circulated newspapers. This may be violated if AP-intensity of other locally consumed media is correlated with that of newspapers, or if there are spillovers from print to other local news coverage (Widmer et al. 2020). Specifications controlling for

[30]The question on immigration status is not asked in the 2015 wave in the survey. Excluding this control and including 2015 does not affect the results since this is small off-election wave – it accounts for only 4.5% of the baseline sample.

DMA×year FEs address this concern to some extent since these media market regions are designed to capture homogeneous TV offerings and online news consumption patterns.

A causal interpretation of the 2SLS results also hinges on the assumption that other aspects of immigration coverage were not affected by the ban. This is supported by the null results on volume and slant of immigration coverage documented in section 3. Yet, I can not rule out other, potentially non-measurable changes in content or tone.

In both cases, the reduced form intention-to-treat estimates remain valid with somewhat different interpretation: they would capture the combined effect of all changes in local immigration coverage induced by the ban.

Finally, equation 6 identifies a local average treatment effect – it applies to readers of newspapers that change their language on immigration solely due to the change in the input supplied by AP. Such newspapers are likely to have a less pronounced stance on immigration policy and potentially more persuadable readers compared to the readers of always- or never-takers.

4.3 Results

First stage I start off by replicating the analysis of the diffusion of the ban for this new sample and unit of observation, i.e. aggregating newspapers' data to the county times year level, and controlling for baseline county characteristics interacted with time (the 1st stage of equation 6). The results presented in figure C3 suggest that in this sample 1 standard deviation higher AP-intensity (= 1.5 IHS-points) is associated with a reduction in use of the term 'illegal immigrant' after the ban of about 7% relative to the mean.

Intention to treat effect I then turn to the reduced form intention-to-treat effect of the ban on support for restrictive immigration policy (equation 5). Table 8 presents the results for support for increasing border security – the most salient item on immigration policy in this period and one of the 2 questions asked consistently in the CCES (along with a question on legalization). I find that respondents in counties with 1 standard deviation higher AP-intensity show about 0.7 p.p. lower support for increasing border security after the ban. This effect is robust to controlling for baseline county controls interacted with time (column 2), and I find if anything a slightly larger magnitude in a specification controlling for state× year FEs (column 3). In column 4 I present the most demanding specification controlling for DMA× year FEs. The magnitude of the estimate is affected only minimally by the inclusion of these finer fixed effects.

Table C4 shows that similar results obtain for the index of support for restrictive immigration policy aggregating all immigration-related questions in the CCES, and that the estimates are robust to controlling for county controls interacted with time, state× year and

DMA× year FEs.

To examine the dynamics of the reduced form effect, I estimate a regression interacting IHS-transformed AP-intensity with indicators for survey wave, with 2012 as the baseline category. In this analysis I can furthermore add the survey years 2007, 2018 and 2019 to my baseline sample in order to examine longer-term trends. The results, presented in figure 7 show if anything an increasing pre-trend, and suggest that the effects persist up until 2019.

In figure 8, I estimate a flexible version of the reduced form equation, splitting the distribution of AP-intensity into quartiles and interacting each one with an indicator for the period after the ban (with the first quartile as the baseline category). I find that the reduced form effect is strongest for by counties in the top quartile of AP-intensity – compared to counties in lowest quartile, respondents in this group reduce their support for border security by 1.8 p.p..

Local average treatment effect In the left hand side panel of table 6 I estimate the LATE effect of exposure to locally circulated 'illegal immigrant' articles (equation 6). The results are consistent with the intention-to-treat estimates and suggest that a 10% increase in the share of 'illegal immigrant' articles is associated with an increase in support for increasing border security of about 1.2 p.p.

Table C5 suggests compatable magnitudes for the LATE expressed in terms of percent of 'illegal immigrant' headlines, or in terms of the IHS-transformed absolute number of 'illegal immigrant' articles.

Robustness and Reuters placebo In table 7 I test the robustness of the results to different versions of AP-intensity – using either credit to AP or plagiarism detection to identify AP-sourced articles, shifting the period of measurement further back in time, and extending measurement to all articles instead of ones limited to immigration. All of these checks yield similar results, both in the reduced form and in the 2SLS estimates.

Instead, I find no significant effect of a placebo treatment defined as the interaction of *Reuters*-intensity and Post-Ban (columns a 6 and 7). This is reassuring since it suggests that the effect is specific to exposure to AP, rather than to newswires in general.

As a further robustness check, in table C6 I vary the threshold for inclusion of a county in the sample. In the baseline sample I require that the NewsLibrary ProQuest data on content covers $\geq 90\%$ of total county circulation. Lowering this threshold increases measurement error as it leads to using data from counties for which the estimates of AP-intensity and share 'illegal immigrant' articles omit newspapers with larger and larger market shares. Consistent with attenuation bias, the estimated effect of the ban decreases in magnitude as I lower the threshold, but is of the same sign and statistically significant throughout, including in the case of keeping all counties. I also probe the accuracy of my procedure of matching CCES

respondents to newspapers by replicating the analysis in a sample restricted to counties with only one major newspaper, thus avoiding aggregation. Table C7 shows that the results are robust to restricting the sample to counties where there is only one newspaper with more than $\geq 10\%$ market share (this is the case for 40% of counties).

Finally, the results are robust to clustering standard errors at a higher level – i.e. by DMA or by state, to allow for correlation in AP-intensity at that level (table C8).

Other immigration-policy questions Similar results obtain for other questions on immigration policy. In table 8 I examine separately the effects on each one of the 5 immigration-related questions asked in the CCES and on an aggregate index of support for restricting immigration that includes a total of 12 questions, including rotating ones.

With the exception of the question on legalization, the results suggest a significant effect of the ban of support for restrictive immigration policies, both in the reduced form and in 2SLS estimates. The magnitudes range from 0.6 to 0.9 p.p. reduction in support for a given policy after the ban in counties with 1 standard deviation higher AP-intensity.[31]

Views on other policies If these results reflect a general change in political leanings that by chance happens to be correlated with AP-intensity, one would expect that support for other policies endorsed by the Republican party also shifts in the same direction. In table 9 I present the results of a placebo exercise where I replicate my baseline specification but replace the dependent with each of 9 policy items featured consistently in the CCES before and after the ban. These range from views on abortion and gay marriage to support for repealing the Affordable Care Act and views on economic policy. I find point estimates close to zero (with the possible exception of abortion) and no significant effect of the ban on any of these outcomes.

Was the change in immigration policy views enough to shift voting choices? The answer appears to be no – in table C11 I show that the ban had no effect on intentions to vote for the Republican candidate in elections for various offices, and a marginally significant effect on presidential approval ratings. in table C10 I use electoral data rather than voting intentions reported in CCES, and confirm the null effect.

[31] Table C9 presents the corresponding OLS results for the relationship between share of locally circulated 'illegal immigrant' articles and immigration policy views. With the exception of the question on access to services, the unconditional correlations are positive and highly significant throughout (panel a) – in the cross section local exposure to the term 'illegal immigrant' is strongly associated with preferences for restrictive immigration policy. Instead, and in contrast to the LATE estimates discussed above, in specifications saturated with fixed effects and county controls interacted with time, I find small and insignificant OLS coefficients (panel b). One explanation for this difference is the nature of compliance to the ban. The group of compliers consists of newspapers that only change their language due to AP's input. In contrast to always-takers (who would have changed their language regardless) and never-takers (who do not change their language despite the ban), such newspapers are likely to have a less pronounced ideological stance on immigration in either direction, and hence, more potentially persuadable readers compared to the general public.

Heterogeneity As a first heterogeneity check, in table 10(a) I examine respondents' self-reported newspaper readership, which proxies the likelihood of exposure to the treatment. I split the sample into respondents who report that they have not read a newspaper in the past 24 hours, those who have, and those who have read a newspaper in print. This analysis has the caveat that the newspaper readership question was not asked in the 2012 and 2017 waves, so that sample size and power are reduced. Yet, the results suggests a stronger magnitude of the effect among (self-reported) frequent newspaper readers.

I next examine the heterogeneity of the effects in terms of respondent ideology and local share of immigrants, which may be informative for the mechanism behind the above result. In particular, a persuasion mechanism would suggest that respondents with weaker ideological attachment, or ones less directly exposed to immigration, would have weaker priors on immigration policy and their views should be more sensitive to media framing. In panel (b) I estimate separate regressions by respondent ideology, and find that the effects are most pronounced for moderates compared to liberals and conservatives.[32] In panel (c) I restrict the sample to respondents in counties in the top vs bottom quartile with respect to the 2012 share of foreign born population. The results point to stronger effects in places with few immigrants.

4.4 Magnitudes

In this section I discuss the magnitude of the estimated effect on immigration policy views. Expressed in terms of one standard deviation higher AP-intensity, the estimated first stage effect in this sample amounts to a 7% reduction in the share of 'illegal immigrant' articles. For comparison, the average circulation-weighted number of 'illegal immigrant' articles before the ban is 6 per month, so we can think of this effect as a reduction of 0.4 articles per month.[33] However coverage of NewsLibrary and ProQuest is not universal, so this should be taken as a lower bound. The corresponding intention to treat effect on support for border security is 0.7 percentage points in the sample of all survey respondents, or 0.9 percentage points in the sample of regular newspaper readers.

One way to benchmark this magnitude is to compare it to the gap between Republican and Democrat respondents in the CCES. Among Republicans, 76.8% support increasing border security, and this is the case for 37.6% of Democrats. Hence, the intention-to-treat effect corresponds to between 1.8% to 2.3% of the gap.

To facilitate comparison to other studies in the media literature, it is also useful to express these magnitudes in terms of persuasion rates. The persuasion rate is defined as the share of people who change their behavior, or in this case – change their survey answer, in response to

[32] Table C12 replicates this exercise for ideology measured on a 5-point scale and confirms this result.
[33] The average circulation-weighted number of 'immigrant' articles is 19 per month.

the treatment, out of the ones who could have potentially done so (DellaVigna and Kaplan 2007; DellaVigna and Gentzkow 2010). The persuasion rate for this treatment, i.e. the share of respondents who are dissuaded to support restrictive immigration policy, can be expressed as:

$$f = \frac{db}{de}\frac{1}{1-b_0}, \tag{8}$$

where b is support for restring immigration, e is exposure to 'illegal immigrant' articles, and b_0 is the share of the population that would oppose restrictive immigration policy in absence of the treatment. With the coefficient estimated for the sample of all respondents, and taking into account that about 1/3 of them report that they read a newspaper and an average of 56% support restrictive immigration policy, this implies a persuasion rate of $f = (0.007)/(0.33 * 1) * (1/0.56) \approx 3.8\%$. With the coefficient estimated from the sample of newspaper readers, the implied persuasion rate is $f = (0.009)/(1*1)*(1/0.59) \approx 1.5\%$. Here, as standard in the calculations of persuasion rates in the media literature, I am assuming that a newspaper reader reads every article. Relaxing this assumption, e.g. assuming that only a fraction of articles are actually read, would lead to a higher persuasion rate. On the other hand, as documented in sections 2 and 3, the ban is more salient and has somewhat stronger diffusion when it comes to headlines. Assuming that readers pay disproportionate attention to headlines would therefore lead to a lower persuasion rate.

This magnitude in the lower range of effects estimated in the media literature, consistent with the milder nature of the treatment. For comparison, Chiang and Knight (2011) estimate a persuasion rate of 2% for the effect of an expected newspaper endorsement and 6,5% for the effect of a surprising endorsement on voting intentions for the respective candidate, and DellaVigna and Kaplan (2007) estimate a persuasion rate of 12% for access to Fox News.

4.5 Mechanisms

Persuasion vs social signaling I interpret the results discussed above as consistent with a persuasion mechanism in which exposure to the term 'illegal immigrant' affects intrinsic views on immigration policy. An alternative interpretation is that the announcement of the ban served as a signal that expressing anti-immigration views is no longer politically correct, making people less likely to state such views in a survey (Braghieri 2021; Bursztyn et al. 2020a). This mechanism is less likely for two reasons.

First, the CCES is administered online and in private, which leaves limited scope for direct experimenter demand effects. Second, a social signaling mechanism would require that the effects are driven by respondents who are explicitly aware of the ban (which received some attention in national media). This seems unlikely since the data suggest that publicity of

the ban is uncorrelated with AP-intensity. To compute this correlation I measure news coverage of the announcement of the ban, searching 'illegal immigrant' articles published in 2013 for mentions of the combination of keywords ("ban" or "Style Guide") and ("AP" or "Associated Press"). I find a total of 37 such articles in the full sample of over 2300 outlets. The correlation between the presence of such articles and AP-intensity is 0.08. Furthermore, a large fraction of them take a critical stance on AP's decision and are hence unlikely to shift norms in the direction of a more liberal stance on immigration.[34]

Issue vs equivalency framing In the natural experiment created by AP's ban the counterfactual to exposure to 'illegal immigrant' is a mix consisting of an equivalence frame – substitution from 'illegal immigrant' to "[immigrant who] entered the country illgeally", and an issue frame – substitution from 'illegal immigrant' to 'immigrant' without any reference to legal status. While the setting does not allow me to disentangle the two treatments, two pieces of evidence suggest that the issue frame component is likely to explain a large portion of the observed effect.

First, as shown in section 2, the text analysis of AP dispatches suggests that the issue-frame component of the treatment, i.e. omission of any reference to legal status, occurred relatively more frequently. This is likely due to lengthy form of the substitutes suggested by the AP and the difficulty of use from the perspective of writers.Second, prior survey experimental studies suggest that views on immigrants and immigration policy are generally reactive to issue frames (e.g. highlighting the aspect of crime or not), while the evidence on equivalency frames (e.g. 'undocumented' vs 'illegal immigrant') is more mixed (Haynes et al. 2016).

5 Conclusion

This paper has documented a large degree of diffusion of language from news wires to media outlets. Changes in news wire language rules, which are determined centrally rather than in consideration of the political leanings of the owners or readers of a particular media outlet, can therefore be a useful source of variation to estimate the effects of slanted language in mass media on public policy views.

Applying this strategy, and exploiting the particularly salient ban on the term 'illegal immigrant' by the AP, I find evidence consistent with the ban leading to lower support for restrictive immigration policies. This effect is more pronounced for news consumers with less

[34]For example, the Miami Herald, the Arizona Daily Sun and the Chattanooga Times Free Press ran articles with the following titles: *"AP should not stop with 'illegal immigrants'"*, *"AP banning clear thinking"*, and *"Decision to ban 'offensive words' means banning thoughts as well"*.

pronounced ideological attachments and lower direct exposure to immigration, consistent with a persuasion mechanism.

This evidence provides proof of concept for the hypothesis that ideologically slanted language can have a persuasive impact. Yet, this evidence is limited to the setting of unauthorized immigration and to exposure to one particular term. More work is needed to understand the external validity of this mechanism of media influence.

References

Alesina, A., A. Miano, and S. Stantcheva (2019): "Immigration and Redistribution," *NBER Working Paper 24733*.

——— (2020): "The Polarization of Reality," *AEA Papers and Proceedings*, 110, 324–328.

Blei, D., A. Ng, and M. Jordan (2003): "Latent Dirichlet Allocation," *Journal of Machine Learning Research*, 3, 993–1022.

Braghieri, L. (2021): "Political Correctness, Social Image, and Information Transmission," .

Bursztyn, L., G. Egorov, and S. Fiorin (2020a): "Misperceptions about Others," *American Economic Review*, 110(11), 3522–48.

Bursztyn, L., A. Rao, C. Roth, and D. Yanagizawa-Drott (2020b): "Misinformation during a Pandemic," *BFI Working Paper 2020-44*.

Bursztyn, L. and D. Y. Yang (2021): "Misperceptions about Others," *Annual Review of Economics*.

Cage, J., N. Herve, and M.-L. Viaud (2020): "The Production of Information in an Online World: Is Copy Right?" *Review of Economic Studies*, forthcoming.

Chiang, C.-F. and B. Knight (2011): "Media Bias and Influence: Evidence from Newspaper Endorsements," *Review of Economic Studies*, 78(3), 795–820.

Chong, D. and J. N. Druckman (2007): "Framing Theory," *Annual Review of Political Science*, 10, 103–26.

Dagonel, A. (2021): "Cumulative CCES Policy Preferences," *Harvard Dataverse*.

DellaVigna, S. and M. Gentzkow (2010): "Persuasion: Empirical Evidence," *Annual Review of Economics*, 2, 643–669.

DellaVigna, S. and E. Kaplan (2007): "The Fox News Effect: Median Bias and Voting," *Quarterly Journal of Economics*, 112(3), 1187–1234.

Durante, R. and B. Knight (2012): "Partisan Control, Media Bias, and Viewer Responses: Evidence from Berlusconi's Italy," *Journal of the European Economic Association*, 10(3), 451–481.

Durante, R., P. Pinotti, and A. Tesei (2019): "The Political Legacy of Entertainment TV," *American Economic Review*.

Enikolopov, R., M. Petrova, and E. Zhuravskaya (2011): "Media and Political Persuasion: Evidence from Russia," *American Economic Review*, 101(7), 3253–85.

Fenby, J. (1986): *The International News Services*, Schocken Books.

Gentzkow, M. and J. M. Shapiro (2006): "Media Bias and Reputation," *Journal of Political Economy*, 114(2).

——— (2010): "What Drives Media Slant? Evidence from U.S. Daily Newsapers," *Econometrica*, 78, 35–71.

Gentzkow, M., J. M. Shapiro, and M. Taddy (2019): "Measuring Group Differences in High-Dimensional Choices: Method and Application to Congressional Speech," *Econometrica*, 87(4), 1307–1340.

Gerber, A. S., D. Karlan, and D. Bergan (2009): "Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions," *American Economic Journal: Applied Economics*, 1(2), 35–52.

Ginsburgh, V. and S. Weber (2020): "The Economics of Language," *Journal of Economic Literature*, 58(2), 348–404.

Grigorieff, A., C. Roth, and D. Ubfal (2019): "Does Information Change Attitudes Towards Immigrants? Representative Evidence from Survey Experiments," .

Groseclose, T. and J. Milyo (2005): "A Measure of Media Bias," *Quarterly Journal of Economics*, 120(4), 1191–1237.

Haaland, I. and C. Roth (2020): "Labor Market Concerns and Support for Immigration," *CESifo Working Paper 8401*.

Hatte, S., M. Thoening, and S. Vlachos (2019): "The Logic of Fear: Populism and the Media Coverage of Immigrants Crimes," *CEPR Discussion Paper 13496*.

Haynes, C., J. L. Merolla, and S. K. Ramakrishnan (2016): *Framing Immigrants: News Coverage, Public Opinion, and Policy*, Russell Sage Foundation.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007): "Experimental Analysis of Neighborhood Effects," *Econometrica*, 75(1), 83–119.

Knoll, B. R., H. Sanborn, and D. Redlawsk (2011): "Framing labels and immigration policy attitudes in the Iowa caucuses: "Trying to Out-Tancredo Tancredo"," *Political Behavior*, 33(3), 433–454.

Levy, R. (2021): "Social Media, News Consumption, and Polarization: Evidence from a Field Experiment," *American Economic Review*, 111(3), 831–70.

Luntz, F. (2007): *Words That Work: It's Not What You Say, It's What People Hear*, Hachette Books.

Martin, G. J. and A. Yurukoglu (2017): "Bias in Cable News: Persuasion and Polarization," *American Economic Review*, 107(9), 2565–99.

Merolla, J., S. K. Ramakrishnan, and C. Haynes (2013): ""Illegal," "Undocumented," or "Unauthorized": Equivalency Frames, Issue Frames, and Public Opinion on Immigration," *Perspectives on Politics*, 11(3), 789–807.

Ommundsen, R., K. V. D. Veer, K. Larses, and D. Eilertsen (2014): "Framing unauthorized immigrants: The effect of labels on evaluations," *Psychological Reports*, 114(2), 461–478.

Puglisi, R. and J. M. Snyder (2011): "Newspaper Coverage of Political Scandals," *The Journal of Politics*, 73(3), 931–950.

Scheufele, D. and D. Tewksbury (2007): "Framing, agenda setting, and priming: the evolution of three media effects models," *Journal of Communication*, 57(1), 9–20.

Seamans, R. and F. Zhu (2014): "Responses to Entry in Multi-Sided Markets: The Impact of Craigslist on Local Newspapers," *Management Science*, 60(2), 265–540.

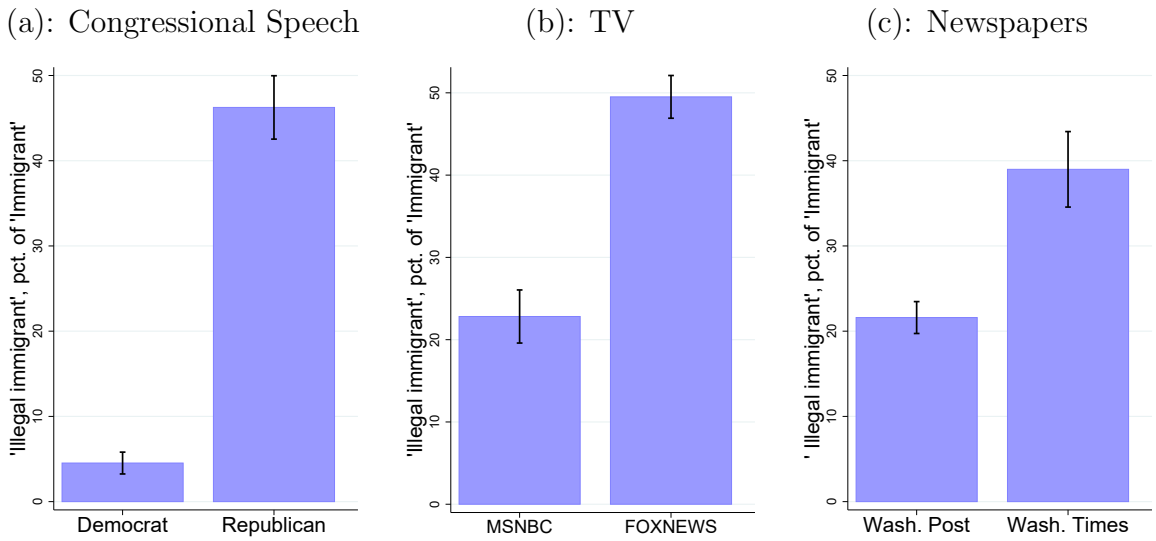
Snyder, J. M. and D. Strömberg (2010): "Press Coverage and Political Accountability," *Journal of Political Economy*, 118(2), 355–408.

Strömberg, D. (2015): "Media and Politics," *Annual Review of Economics*, 7, 173–205.

Widmer, P., S. Galletta, and E. Ash (2020): "Media Slant is Contagious," *Working Paper*.

6 Figures

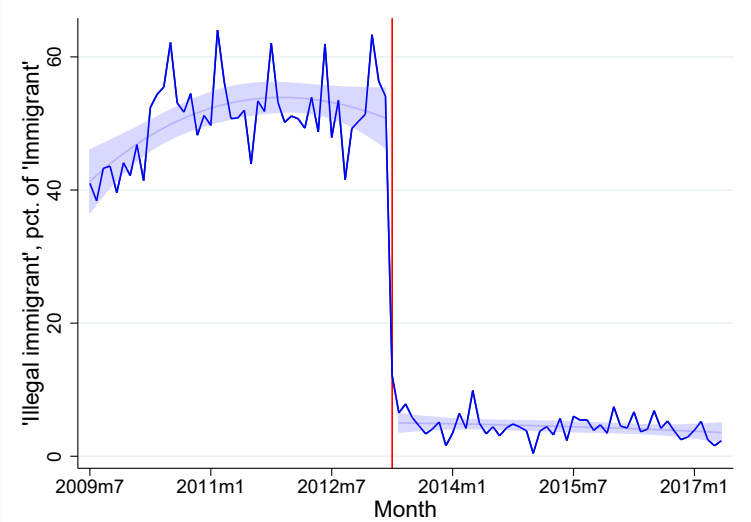
Figure 1: Ideological charge of the term "illegal immigrant"



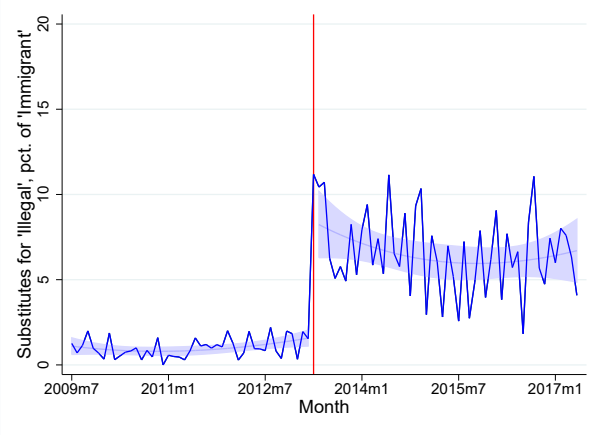
Notes: Use of the term 'illegal immigrant' relative to the word 'immigrant' in congressional speech (comparing Democrats and Republicans), in cable TV (comparing MSNBC and Fox News) and in newspapers (comparing the Washington Post and the Washington Times). The data covers the years 2009 to 2017. Data sources: Congressional Record, GDELT TV Archive and ProQuest respectively.

Figure 2: AP's language on immigration over time

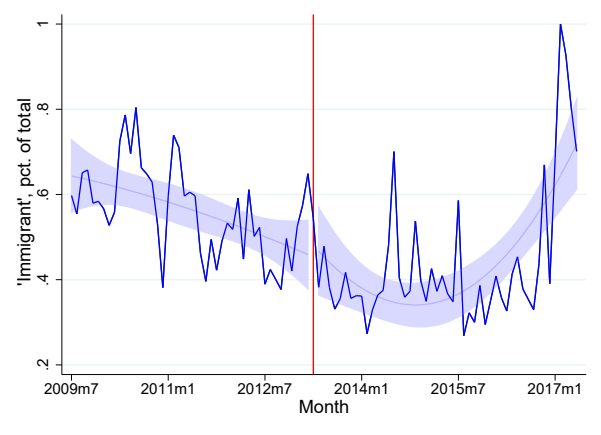
(a) 'Illegal immigrant'



(b) AP-approved substitutes for 'illegal immigrant'



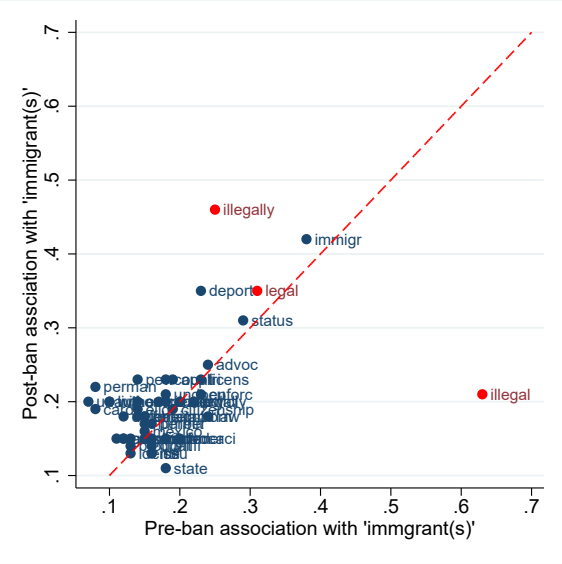
(c) 'Immigrant'



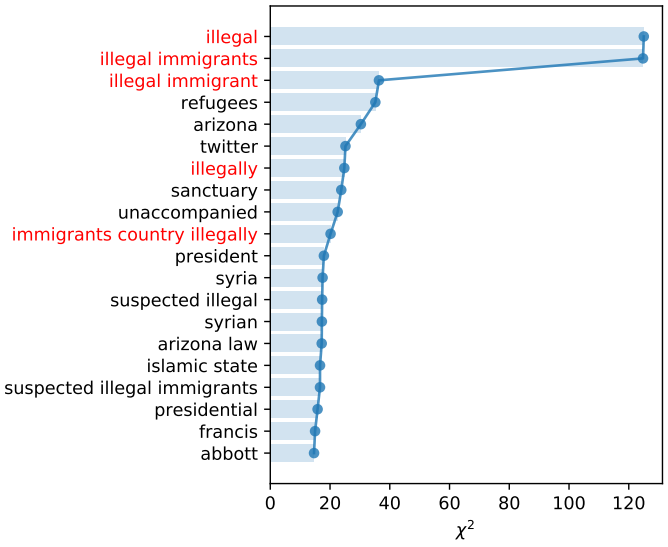
Notes: Panel (a): Number of 'illegal immigrant' dispatches, as percent of 'immigrant' dispatches. Panel (b): Number of dispatches mentioning the AP-approved substitutes "enter* / live* in the country illegally/ without legal permission", as percent of 'immigrant' dispatches. Panel (c): Number of 'immigrant' dispatches, as percent of total dispatches.

Figure 3: AP's language on immigration before and after the ban

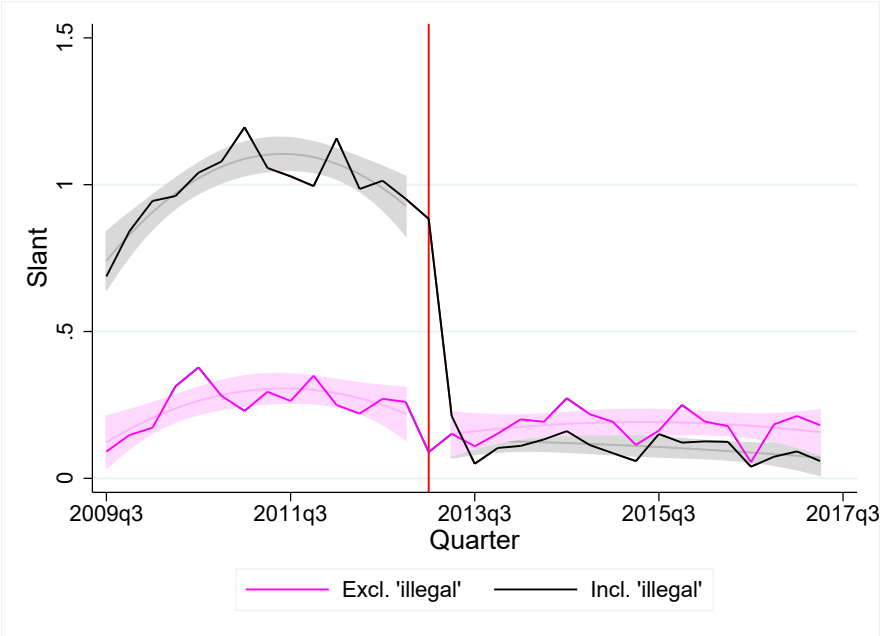
(a): Correlates of the word "immigrant" before and after the ban



(b): Phrases predictive of pre- vs post-ban publishing date



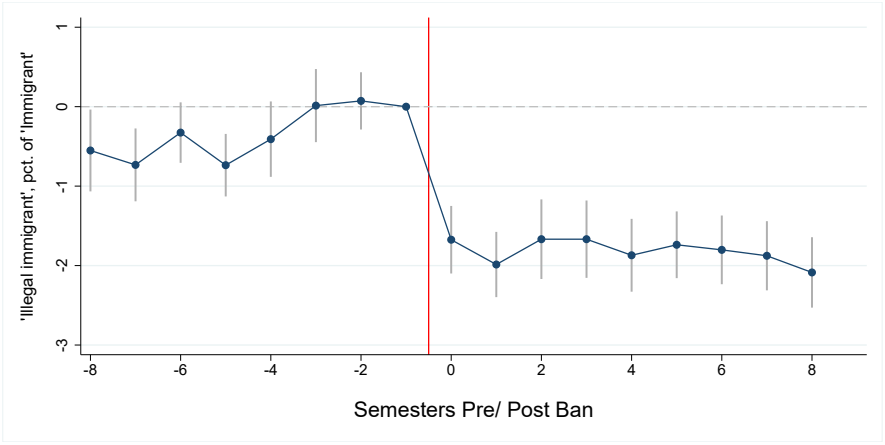
(c): AP's slant on immigration



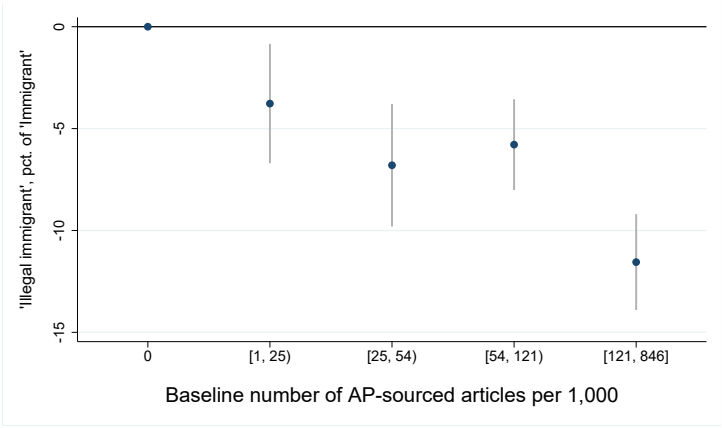
Notes: Panel (a): Top 50 unigrams with highest rate of co-occurrence with the word 'immigrant' before and after the ban. Panel (b): Top 20 n-grams ($n \in 1, 2, 3$) with highest predictive power of a pre- vs post-ban publishing date, based on a χ^2 statistic. Panel (c): Slant index, measuring similarity to Republican vs Democrat speech in Congress, computed including or excluding the term 'illegal immigrant' and its substitutes.

Figure 4: Diffusion of the ban

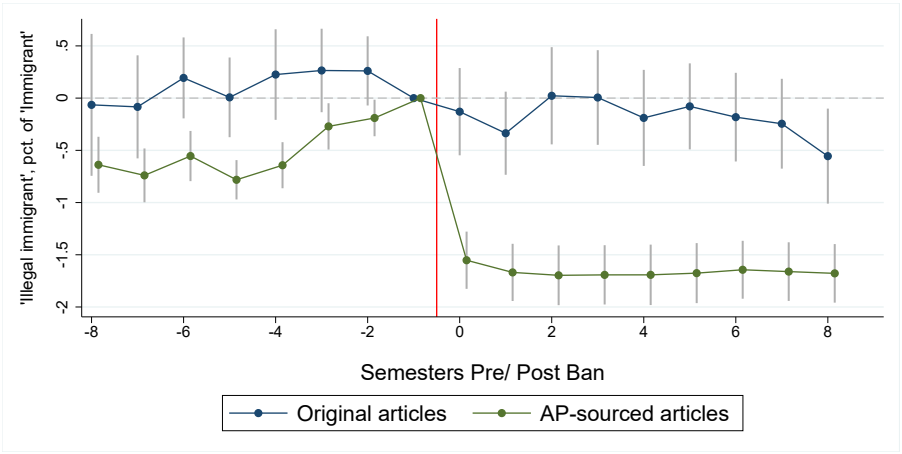
(a): Dynamic diff-in-diff



(b): Effects by bin of AP-intensity



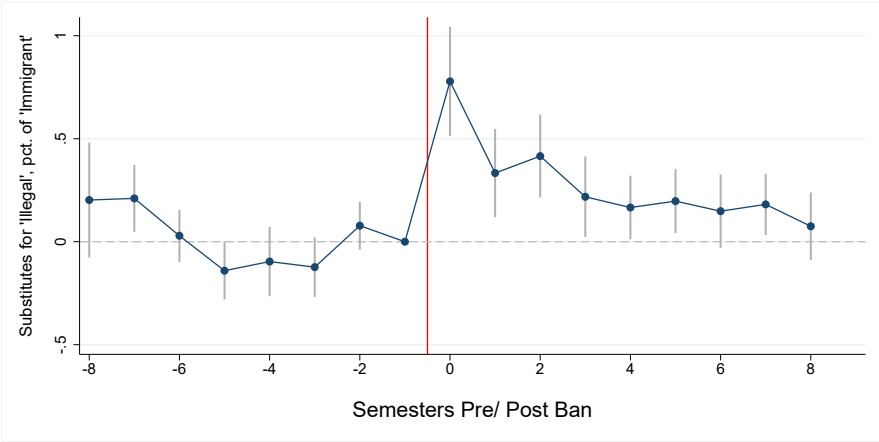
(c): Dynamic diff-in-diff: AP-sourced vs original articles



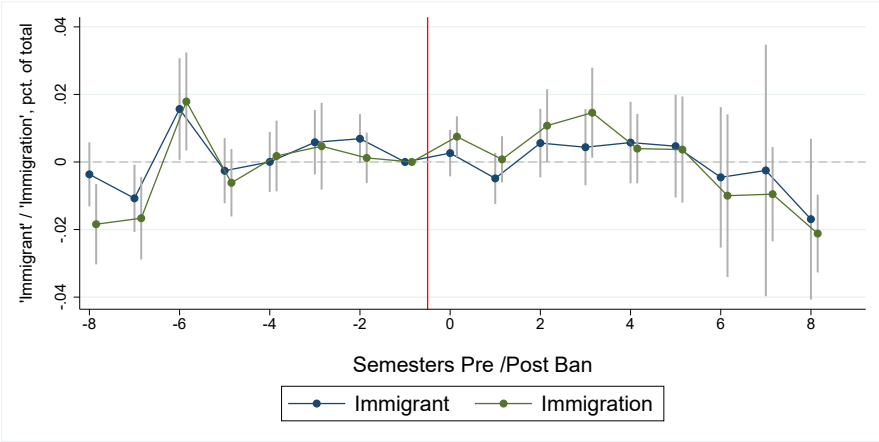
Notes: Effect of AP's ban on media outlets' use of the term 'illegal immigrant'. Panel (a): Coefficients and 95% confidence intervals from a regression of number of 'illegal immigrant' as percent of 'immigrant' articles on the interaction of IHS(AP-intensity) with a set of indicators for semester pre-/ post- ban. The omitted category is the semester before the ban. Panel (b): Coefficients and 95% confidence intervals from a regression of number of 'illegal immigrant' articles as percent of 'immigrant' articles on a set of indicators for quartile of (positive) AP-intensity interacted with Post Ban. The omitted category is AP-intensity = 0. Panel (c) replicates panel (a), splitting the dependent variable into 'illegal immigrant' articles sourced from AP (green) and the rest (blue), both expressed as percent of 'immigrant' articles. All regressions control for outlet and month FEs and are weighted by number of 'immigrant' articles. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, ***. $p < 0.01$.

Figure 5: Diffusion of the ban: Other aspects of immigration coverage

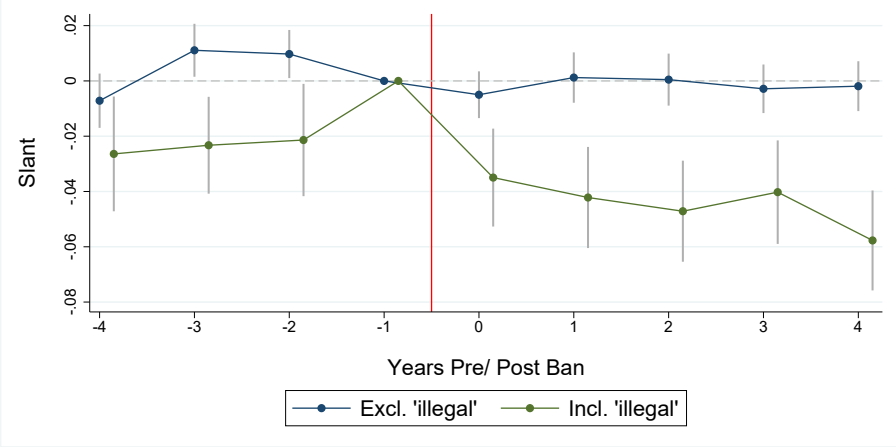
(a) AP-approved substitutes for 'illegal immigrant'



(b) Volume of immigration coverage



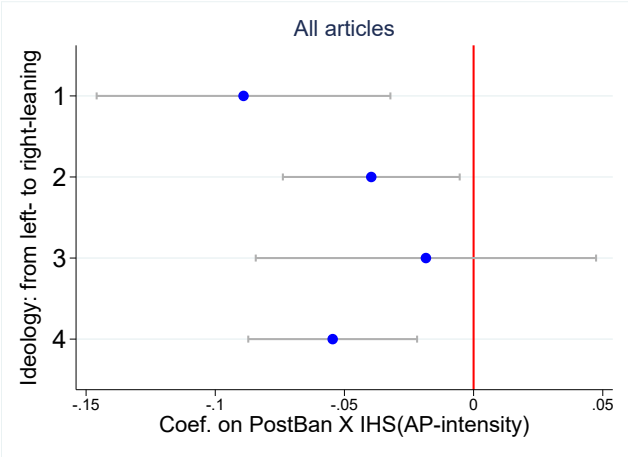
(c) Immigration slant



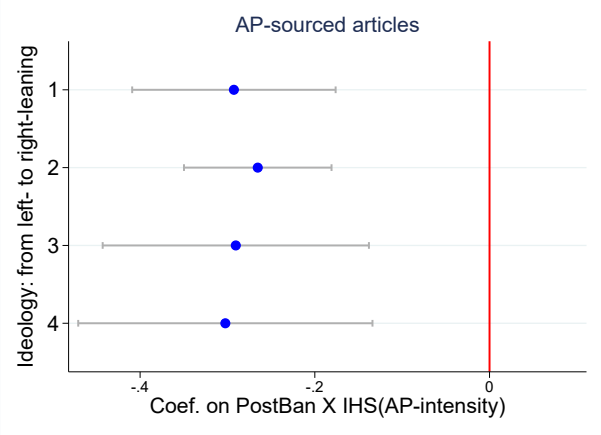
Notes: Effect of AP's ban on other aspects of immigration coverage. Panel (a): Coefficients and 95% confidence intervals from a regression of the share of 'immigrant' articles mentioning AP-approved substitutes, on the interaction of IHS(AP-intensity) with a set of indicators for semester pre-/ post- ban. The omitted category is the semester before the ban. Weighted by number of 'immigrant' articles. Panel (b): Coefficients and 95% confidence intervals from a regression of the number of 'immigrant' or 'immigration' articles as percent of total articles on the interaction of IHS(AP-intensity) with a set of indicators for semester pre-/ post- ban. The omitted category is the semester before the ban. Weighted by number of total articles. Panel (c): Coefficients and 95% confidence intervals from a regression of an index of slant – computed either including in excluding the term 'illegal immigrant' and its substitutes – on the interaction of IHS(AP-intensity) with year pre-/post-ban. Standard errors are clustered by outlet in all specifications. Significance levels: * $p < 0.1$, ** $p < 0.05$, ***. $p < 0.01$.

Figure 6: Diffusion by Ideology

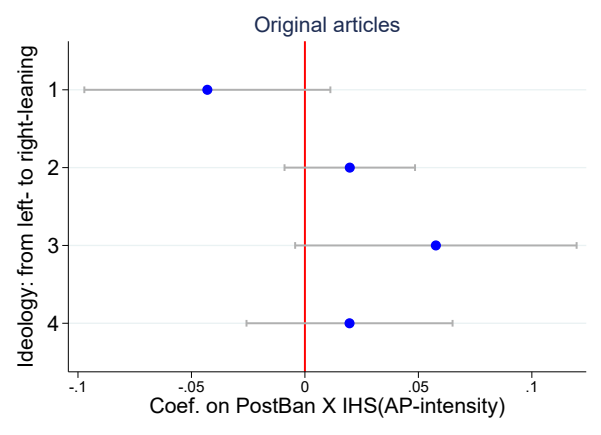
(a) "Illegal immigrant", pct. of "immigrant"



(b) "Illegal immigrant" & sourced from AP, pct. of "immigrant"

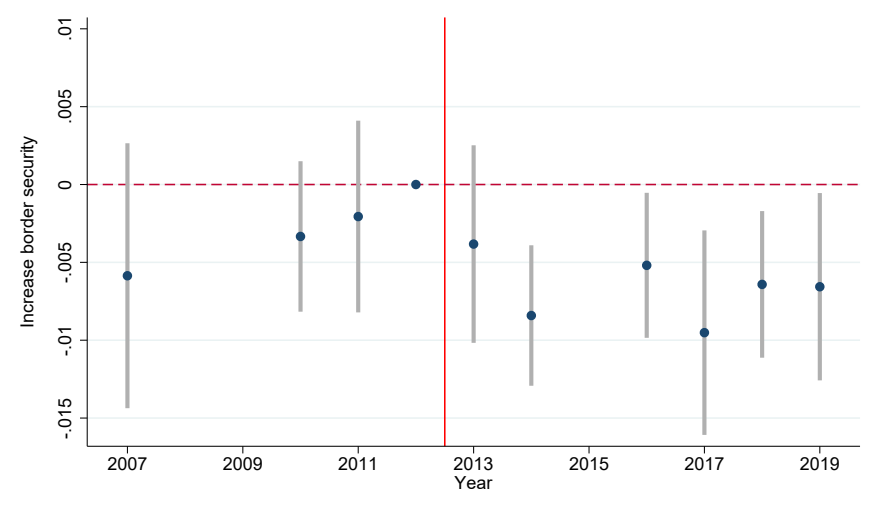


(c) "Illegal immigrant" & not sourced from AP, pct. of "immigrant"



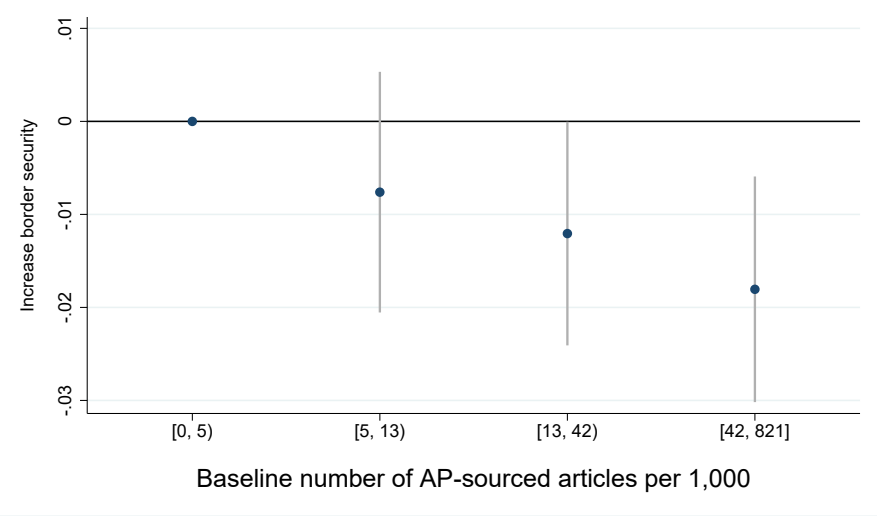
Notes: Effect of the AP's ban on media outlets' use of the term 'illegal immigrant', estimated separately by quartile of the Gentzkow-Shapiro index of ideology. Panel (a): Coefficients and 95% confidence intervals from regressions restricted to one quartile at a time of the frequency of "illegal immigrant" articles as percent of "immigrant" articles on the interaction of IHS(AP-intensity) and PostBan, controlling for outlet and year-month FEs and weighted by number of "immigrant" articles. The dependent variable is standardized to mean zero and standard deviation one within each sample. Panels (b) and (c) replicate Panel (a), splitting the dependent variable into 'illegal immigrant' articles sourced from AP (b) and the rest (b), both expressed in percent of 'immigrant' articles.

Figure 7: Support for increasing border security: Reduced form over time



Notes: Coefficients and 95% confidence intervals from a regression of support for increasing border security on the interaction of IHS(AP-intensity) with survey year. The omitted category is 2012 - the year before the ban. The regression controls for respondent characteristics, baseline county characteristics interacted with time, and county and year FEs. Standard errors are clustered by county.

Figure 8: Support for increasing border security: Reduced form by quartile of AP-intensity



Notes: Coefficients and 95% confidence intervals from a regression of support for increasing border security on the interaction of Post Ban with quartile of AP-intensity. The omitted category is the first quartile. The regression controls for respondent characteristics, baseline county characteristics interacted with time, and county and year FEs. Standard errors are clustered by county.

7 Tables

Table 1: AP's ban on 'illegal immigrant'

Panel (a): AP Stylebook guidelines before and after the ban

Pre-Ban	**Post-Ban**
illegal immigrant Used to describe those who have entered the country illegally, it is the preferred term, rather than illegal alien or undocumented worker. Do not use the shortened term illegals.	**illegal immigration** Entering in a country in violation of civil or criminal law. Except in direct quotes essential to the story, use illegal only to refer to an action, not a person: illegal immigration, but not illegal immigrant. Acceptable variations include living in or entering a county illegally or without legal permission. Expect in direct quotations, do not use the terms illegal alien, an illegal, illegals or undocumented. Do not describe people as violating immigration laws without attribution. [...]

Panel (b): Example of AP dispatches before and after the ban

18-Mar-2013: **2 weeks Pre-Ban**	16-Apr-2013: **2 weeks Post-Ban**
Senate panel OKs letting non-citizens, including illegal immigrants, get driver's licenses	**Immigrant driver's license bill takes step forward in Oregon Senate committee work**
ST. PAUL, Minn. (AP) — Bills that would let illegal immigrants get a Minnesota driver's license are moving forward at the Capitol. The Senate Transportation and Public Safety Committee on Monday passed a bill to ease restrictions on driver's licenses for non-U.S. citizens. A House committee endorsed a similar bill last week. Sen. Bobby Joe Champion, a Minneapolis Democrat, says his bill would make Minnesota roads safer by funneling more drivers through the state's driving test and making it easier for them to buy automobile insurance. Republicans say the change could lead to unintended consequences, like illegal immigrants using state IDs to vote. The bill passed 10-7, with all Democrats in favor and all Republicans voting against it.	SALEM, Ore. (AP) – An Oregon Senate committee has advanced a bill granting four-year driver's licenses to people who can't prove they're legally in the United States. The Senate Business and Transportation Committee approved the measure Monday on a 4-2 vote. The bill would allow immigrants who have lived in Oregon for at least a year and meet other requirements to apply for driver's cards without proving legal presence. The card would be valid for only four years– half as long as a standard Oregon license– and would state "driving privilege only." Supporters say it will make Oregon roads safer because there would be fewer untrained and uninsured drivers, but opponents say it could create a culture of crime in the state. The bill goes to a legislative budget committee.

Notes: Panel (a): Extract from AP's Stylebook entry on illegal immigration before and after the ban. Panel(b): Example of AP dispatches before and after the ban.

Table 2: Diffusion of the ban

Panel (a): Full sample - print & online

	(1) Headlines 'Illegal imm.', pct. of 'Imm.'	(2)	(3) 'Illigal immigrant' pct. of 'Immigrant'	(4) 'Illegal imm.'& not AP-sourced, pct of 'Imm.'	(5) 'Illegal imm.'& AP-sourced, pct of 'Imm.'
PostBan × IHS(AP-intensity)	-1.034***	-1.492***	-1.415***	-0.287*	-1.196***
	(0.291)	(0.178)	(0.156)	(0.174)	(0.118)
State × Year-Month FEs	No	No	Yes	No	No
Outlet FEs	Yes	Yes	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes	Yes	Yes
Observations	18,976	133,633	133,615	133,035	133,128
Number of outlets	1414	2273	2273	2270	2270
R^2	0.24	0.42	0.50	0.38	0.37
Mean dep. var.	14.46	20.73	20.73	19.61	1.16

Panel (b): Daily print newspapers

	(1) Headlines 'Illegal imm.', pct. of 'Imm.'	(2)	(3)	(4) 'Illigal immigrant' pct. of 'Immigrant'	(5) 'Illegal imm.'& not AP-sourced, pct of 'Imm.'	(6) 'Illegal imm.'& AP-sourced, pct of 'Imm.'
PostBan × IHS(AP-intensity)	-1.256***	-1.258***	-1.294***	-1.297***	-0.018	-1.365***
	(0.344)	(0.219)	(0.188)	(0.222)	(0.218)	(0.149)
DMA × Year-Month FEs	No	No	No	Yes	No	No
State × Year-Month FEs	No	No	Yes	No	No	No
Outlet FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,249	65,752	65,682	58,726	65,560	65,613
Number of outlets	678	837	837	768	837	837
R^2	0.22	0.43	0.53	0.62	0.38	0.39
Mean dep. var.	15.15	21.67	21.62	21.37	20.18	1.51

Notes: Effect of AP's ban on media outlets' use of the term 'illegal immigrant'. The level of observation is media outlet × month. Dependent variables: 'illegal immigrant' headlines in columns (a) 1 and (b) 1, 'illegal immigrant' articles in columns (a) 2-3 and (b) 2-4, 'illegal immigrant' articles not sourced from AP in columns (a) 4 and (b) 5, and 'illegal immigrant' articles sourced from AP in columns a5 and b6. All in percent of 'immigrant' headlines/ articles. Weighted by number of 'immigrant' headlines in columns (a) 1 and (b) 1, and by number of 'immigrant' articles in columns (a) 2-5 and (b) 5-6. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, ***. $p < 0.01$.

Table 3: Diffusion of the ban: Robustness and Reuters placebo

Panel (a): Full sample - print & online

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	'Illigal immigrant', pct. of 'Immigrant'						
PostBan × IHS(AP-int.)	-1.475***	-1.450***	-1.548***	-1.535***	-2.013***		-2.040***
	(0.190)	(0.192)	(0.186)	(0.207)	(0.263)		(0.262)
PostBan × IHS(Reuters-int.)						0.280	0.469
						(0.362)	(0.306)
Method	credit	plagiarism	both	both	credit	credit	credit
Period	1-12mo	1-12mo	12-24mo	24-36mo	1-12mo	1-12mo	1-12mo
Sample	immigrant	immigrant	immigrant	immigrant	all	all	all
Observations	133,633	133,633	133,580	134,135	129,344	129,344	129,344
Number of outlets	2273	2273	2279	2314	2421	2421	2421
R^2	0.42	0.42	0.42	0.42	0.40	0.40	0.40
Mean dep. var.	20.73	20.73	20.72	20.74	21.16	21.16	21.16

Panel (b): Daily print newspapers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	'Illigal immigrant', pct. of 'Immigrant'						
PostBan × IHS(AP-int.)	-1.124***	-1.290***	-1.298***	-1.153***	-1.502***		-1.509***
	(0.221)	(0.220)	(0.218)	(0.240)	(0.309)		(0.309)
PostBan × IHS(Reuters-int.)						0.460	0.502
						(0.558)	(0.484)
Method	credit	plagiarism	both	both	credit	credit	credit
Period	1-12mo	1-12mo	12-24mo	24-36mo	1-12mo	1-12mo	1-12mo
Sample	immigrant	immigrant	immigrant	immigrant	all	all	all
Observations	65,752	65,752	65,669	65,796	59,250	59,250	59,250
Number of outlets	837	837	836	840	767	767	767
R^2	0.43	0.43	0.44	0.44	0.41	0.41	0.41
Mean dep. var.	21.67	21.67	21.66	21.65	22.98	22.98	22.98

Notes: Effect of AP's ban on media outlets' use of the term 'illegal immigrant': Robustness to alternative measures of AP-intensity and Reuters-intensity placebo. "Method", "Period" and "Sample" describe the construction of AP-intensity (Reuters-intensity) in the respective column. The level of observation is media outlet × month. The dependent variable is number of 'illegal immigrant' articles as percent of 'immigrant' articles. Weighted by number of 'immigrant' articles. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, ***.

Table 4: Diffusion of the ban: other aspects of immigration coverage

Panel (a): Substitutes and volume of coverage

	Full sample	Newspapers	Full sample	Newspapers
	(1) AP-approved substitutes, pct. of 'Immigrant'	(2) 'Immigrant', pct. of total	(3) AP-approved substitutes, pct. of 'Immigrant'	(4) 'Immigrant', pct. of total
PostBan × IHS(AP-intensity)	0.296***	-0.005	0.231***	-0.007
	(0.069)	(0.005)	(0.081)	(0.005)
Observations	124,900	204,394	58,903	71,910
Number of outlets	2167	2167	754	752
R^2	0.26	0.55	0.14	0.54
Mean dep. var.	6.29	0.62	6.72	0.64

Panel (b): Slant

	Full sample	Newspapers	Full sample	Newspapers
	(1) Slant incl. 'illegal'	(2) Slant excl. 'illegal'	(3) Slant incl. 'illegal'	(4) Slant excl. 'illegal'
PostBan × IHS(AP-intensity)	-0.027***	0.001	-0.020***	-0.002
	(0.005)	(0.003)	(0.007)	(0.004)
Observations	15,244	15,244	6,930	6,930
Number of outlets	2137	2137	836	836
R^2	0.33	0.26	0.34	0.27
Mean dep. var.	0.38	0.13	0.42	0.15

Notes: Panel (a): Effects of AP's ban on use of the AP-approved substitutes for 'illegal immigrant' and on volume of immigration coverage. Panel (b): Effects of AP's ban on AP's slant on immigration. The level of observation is media outlet × month in panel (a) and media outlet × year in panel (b). Weighted by number of 'immigrant' articles columns (a) 1 and 3, and by total articles in columns (a) 2 and 4. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, ***.

Table 5: Diffusion of the ban: By outlet ideology

Panel (a): All 'illegal immigrant' articles

Sample restricted by quartile of ideology:	left Q1	Q2	Q4	right Q4
	(1)	(2)	(3)	(4)
	'Illigal immigrant' pct. of 'Immigrant'			
PostBan × IHS(AP intensity)	-1.7918***	-0.9057**	-0.4350	-1.4502***
	(0.5752)	(0.3938)	(0.7812)	(0.4370)
Observations	7,766	8,038	7,927	7,319
Number of outlets	85	90	90	88
R^2	0.56	0.48	0.53	0.45
Mean dep. var.	17.27	22.57	23.45	23.81

Panel (b): AP-sourced articles

Sample restricted by quartile of ideology:	left Q1	Q2	Q3	right Q4
	(1)	(2)	(3)	(4)
	'Illegal immigrant' & AP-sourced, pct. of 'Immigrant'			
PostBan × IHS(AP intensity)	-1.1172***	-1.6147***	-1.8715***	-2.0993***
	(0.2236)	(0.2587)	(0.4942)	(0.5882)
Observations	7,730	7,996	7,873	7,264
Number of outlets	85	90	90	88
R^2	0.50	0.52	0.47	0.49
Mean dep. var.	0.92	2.43	1.80	1.94

Panel (c): Original articles

Sample restricted by quartile of ideology:	left Q1	Q2	Q3	right Q4
	(1)	(2)	(3)	(4)
	'Illegal immigrant' & not AP-sourced, pct. of 'Immigrant'			
PostBan × IHS(AP intensity)	-0.8376	0.4364	1.3198*	0.5109
	(0.5311)	(0.3194)	(0.7138)	(0.5936)
Observations	7,725	7,991	7,872	7,258
Number of outlets	85	90	90	88
R^2	0.48	0.40	0.49	0.40
Mean dep. var.	16.44	20.12	21.52	22.10

Notes: Effects of AP's ban on use of the term 'illegal immigrant' by outlet ideology. Q1 to Q4 denote quartiles of the index of ideology constructed by Gentzkow and Shapiro (2010), with higher values indicating more right-leaning ideology. The level of observation is media outlet × month. The dependent variable is number of 'illegal immigrant' articles in panel (a), the number of 'illegal immigrant' articles sourced from AP in panel (b) and the number of original 'illegal immigrant' articles in panel (c), all expressed in percent of 'immigrant' articles. All specifications control for outlet and month FEs and are weighted by number of 'immigrant' articles. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, ***.

Table 6: Support for increasing border security

	Reduced Form				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Increase border security							
PostBan × IHS(AP-intensity)	-0.0054***	-0.0048***	-0.0062***	-0.0059**				
	(0.002)	(0.002)	(0.002)	(0.002)				
'Illegal imm.', pct. of 'Imm.'					0.0066**	0.0051**	0.0077***	0.0057**
					(0.003)	(0.002)	(0.003)	(0.002)
Year × DMA FEs	No	No	No	Yes	No	No	No	Yes
Year × State FEs	No	No	Yes	No	No	No	Yes	No
Year FEs × County controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Respondent controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage coef.								
PostBan × IHS(AP-intensity)					-0.8127***	-0.9466***	-0.8043***	-1.0450***
					(0.282)	(0.210)	(0.186)	(0.237)
First-Stage F stat.					8.32	20.29	18.74	19.46
Observations	192,551	192,024	192,024	192,020	192,551	192,024	192,024	192,020
Number of counties	2,151	2,142	2,142	2,142	2,151	2,142	2,142	2,142
R^2	0.09	0.09	0.09	0.09				
Mean dep. var.	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55

Notes: Effect of AP's ban on support for increasing border security. Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Support for increasing border security: Robustness and Reuters placebo

Panel (a): Reduced form

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Increase border security						
PostBan × IHS(AP-int.)	-0.0044***	-0.0037**	-0.0040***	-0.0042***	-0.0043***		-0.0043***
	(0.0014)	(0.0016)	(0.0015)	(0.0015)	(0.0016)		(0.0016)
PostBan × IHS(Reuters-int.)						-0.0018	-0.0021
						(0.0027)	(0.0027)
Method	credit	plagiarism	both	both	credit	credit	credit
Period	1-12mo	1-12mo	12-24mo	24-36mo	1-12mo	1-12mo	1-12mo
Sample	immigrant	immigrant	immigrant	immigrant	all	all	all
Observations	192,024	192,024	192,024	192,024	179,858	179,858	179,858
Number of outlets	2142	2142	2142	2142	1829	1829	1829
R^2	0.09	0.09	0.09	0.09	0.09	0.09	0.09
Mean dep. var.	0.55	0.55	0.55	0.55	0.55	0.55	0.55

Panel (b): 2SLS

	(1)	(2)	(3)	(4)	(5)
	Increase border security				
'Illegal imm.', pct. of 'Imm.'	0.0055***	0.0041**	0.0044**	0.0056**	0.0042**
	(0.0021)	(0.0021)	(0.0019)	(0.0024)	(0.0018)
Method	credit	plagiarism	both	both	credit
Period	1-12mo	1-12mo	12-24mo	24-36mo	1-12mo
Sample	immigrant	immigrant	immigrant	immigrant	all
First-Stage F stat.	17.49	19.01	23.14	12.59	23.17
Observations	192,024	192,024	192,024	192,024	179,858
Number of counties	2,142	2,142	2,142	2,142	1,829
Mean dep. var.	0.55	0.55	0.55	0.55	0.55

Notes: Effect of AP's ban on support for border security: Robustness to alternative measures of AP-intensity and Reuters-intensity placebo. "Method", "Period" and "Sample" describe the construction of AP-intensity (Reuters-intensity) in the respective column. Reduced-form OLS regressions in panel (a), 2SLS regressions in panel (b). All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Views on immigration policy: all questions

Panel (a): Reduced form

	(1) Increase border security	(2) Sanction employers	(3) Grant legal status ×-1	(4) Allow police questioning	(5) Prohibit services	(6) Index Restrict imm.
PostBan × IHS(AP-intensity)	-0.0049***	-0.0063***	-0.0011	-0.0048***	-0.0040*	-0.0063***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	192,024	108,632	192,024	148,976	85,832	192,024
Number of counties	2,142	2,007	2,142	2,100	1,972	2,142
R^2	0.09	0.13	0.09	0.13	0.15	0.14
Mean dep. var.	0.55	0.61	-0.48	0.40	0.42	-0.00

Panel (b): 2SLS

	(1) Increase border security	(2) Sanction employers	(3) Grant legal status ×-1	(4) Allow police questioning	(5) Prohibit services	(6) Index Restrict imm.
'Illegal imm.', pct. of 'Imm.'	0.0052**	0.0056**	0.0011	0.0053**	0.0038*	0.0067**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
First-Stage F stat.	20.29	18.65	20.29	16.13	13.90	20.29
Observations	192,024	108,632	192,024	148,976	85,832	192,024
Number of counties	2,142	2,007	2,142	2,100	1,972	2,142
Mean dep. var.	0.55	0.61	-0.48	0.40	0.42	-0.00

Notes: Effect of AP's ban on views on immigration policy views: all immigration-related CCES questions. Reduced-form OLS regressions in panel (a), 2SLS regressions in panel (b). All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Placebo: Views on policies other than immigration

Panel (a): Reduced form

	(1) Allow abortion ×-1	(2) Oppose gay marriage	(3) Repeal ACA	(4) Oppose affirmative action	(5) Support military intervention	(6) Spending cuts over tax increase	(7) Sales tax over income tax	(8) Jobs over environment	(9) Economy worse
PostBan × IHS(AP-intensity)	0.0024	0.0013	-0.0010	-0.0003	-0.0013	-0.0013	0.0017	-0.0005	0.0006
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)
Observations	200,492	187,752	138,972	145,673	201,545	143,484	135,891	85,085	196,968
Number of counties	2,148	2,138	2,064	2,096	2,150	2,069	2,061	1,999	2,147
R^2	0.48	0.09	0.10	0.22	0.33	0.05	0.05	0.07	0.12
Mean dep. var.	-0.28	0.58	0.50	0.62	0.68	0.71	0.51	0.39	0.42

Panel (b): 2SLS

	(1) Allow abortion ×-1	(2) Oppose gay marriage	(3) Repeal ACA	(4) Oppose affirmative action	(5) Support military intervention	(6) Spending cuts over tax increase	(7) Sales tax over income tax	(8) Jobs over environment	(9) Economy worse
'Illegal imm.', pct. of 'Imm.'	-0.0024	-0.0013	0.0008	0.0004	0.0013	0.0014	-0.0018	0.0008	-0.0007
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)	(0.002)
First-Stage F stat.	19.98	18.19	18.95	11.23	19.79	18.66	18.22	7.99	19.49
Observations	200,492	187,752	138,972	145,673	201,545	143,484	135,891	85,085	196,968
Number of counties	2,148	2,138	2,064	2,096	2,150	2,069	2,061	1,999	2,147
Mean dep. var.	-0.28	0.58	0.50	0.62	0.68	0.71	0.51	0.39	0.42

Notes: Effect of AP's ban on views on policies other than immigration. See Table C2 for a description of the questions and their coverage in the CCES. Reduced form OLS regressions in panel (a), 2SLS regressions in panel (b). All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Support for increasing border security: Heterogeneity

Panel (a): By respondent newspaper readership

	Reduced Form			2SLS		
	(1) Did not read newsp.	(2) Read newsp.	(3) Read print	(4) Did not read newsp.	(5) Read newsp.	(6) Read print
PostBan × IHS(AP-intensity)	-0.0020	-0.0040*	-0.0061**			
	(0.003)	(0.002)	(0.003)			
'Illegal imm.', pct. of 'Imm.'				0.0025	0.0052	0.0079*
				(0.004)	(0.003)	(0.004)
First stage F-stat.				14.46	11.88	11.54
Observations	65,610	76,607	46,614	65,610	76,607	46,614
Number of counties	1,889	1,866	1,674	1,889	1,866	1,674
R^2	0.11	0.09	0.09			
Mean dep. var.	0.54	0.56	0.59	0.54	0.56	0.59

Panel (b): By respondent ideology

	Reduced Form			2SLS		
	(1) Liberal	(2) Moderate	(3) Conservative	(4) Liberal	(5) Moderate	(6) Conservative
PostBan × IHS(AP-intensity)	-0.0036	-0.0068**	-0.0013			
	(0.003)	(0.003)	(0.002)			
'Illegal imm.', pct. of 'Imm.'				0.0034	0.0070**	0.0015
				(0.003)	(0.003)	(0.003)
First-Stage F stat.				17.46	19.02	18.92
Observations	50,158	58,978	67,666	50,158	58,978	67,666
Number of counties	1,505	1,735	1,932	1,505	1,735	1,932
R^2	0.08	0.08	0.11			
Mean dep. var.	0.31	0.52	0.77	0.31	0.52	0.77

Panel (c): By county share of foreign born population

	Reduced Form		2SLS	
	(1) Foreign-born > p75	(2) Foreign-born ≤ p25	(3) Foreign-born > p75	(4) Foreign-born ≤ p25
PostBan × IHS(AP-intensity)	-0.0005	-0.0087***		
	(0.004)	(0.003)		
'Illegal imm.', pct. of 'Imm.'			0.0004	0.0056***
			(0.003)	(0.002)
First-Stage F stat.			7.62	35.75
Observations	46,020	47,584	46,020	47,584
Number of counties	105	1,447	105	1,447
R^2	0.08	0.10		
Mean dep. var.	0.49	0.59	0.49	0.59

Notes: Effect of AP's ban on support for increasing border security: heterogeneity by newspaper readership (i.e. whether the respondent read a newspaper the day the interview), by respondent ideology, and by share of foreign born population in the respondent's county of residence. All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Contents (Appendix)

Appendices: For Online Publication **2**

A Background and text analysis of AP dispatches 2

A.1 AP's ban 2

A.2 Computation of immigration slant 3

A.3 Plagiarism detection algorithm 4

A.4 Additional results 6

B Diffusion of the ban 14

B.1 Descriptive statistics 14

B.2 Additional results 16

C Views on immigration policy 19

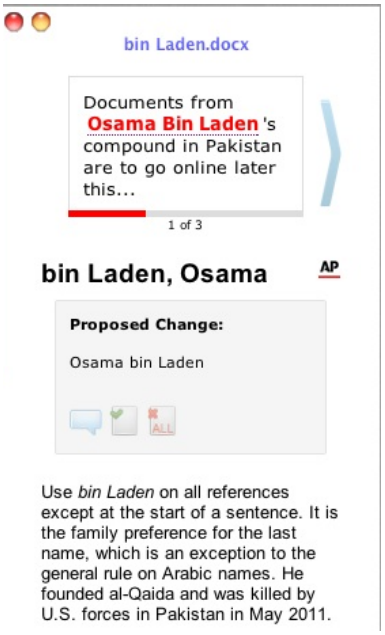
C.1 Data and descriptive statistics 19

C.2 Additional results 24

A Background and text analysis of AP dispatches

A.1 AP's ban

Figure A1: AP's Style Guide embedded in a text editor



Notes: `https://store.apstylebook.com/apstylechecking.html`

Figure A2: The ban reported in the Atlantic



Notes: Full article available at `https://www.theatlantic.com/politics/archive/2013/04/ap-ban-illegal-immigrant/316701/`

A.2 Computation of immigration slant

In this section I describe the procedure for computing an index for the immigration-specific slant of AP dispatches released in each quarter, and that of the articles published by each news outlet in a given year. This follows closely the method developed by Gentzkow and Shapiro (2010).

AP's slant over time I start off with the set of all Congressional speeches for the period 2009-2012 (i.e., before AP's ban) that mention the word 'immigrant'. First, after pre-processing the text (removing stop-words and lemmatising), I rank the 500 bi-grams that are most predictive of the speakers' party based on the Pearson's χ^2 statistic. I keep the ones encountered in the similarly pre-processed corpus of AP dispatches at least 10 times – this results into 363 phrases. Table A1 lists the phrases with highest χ^2 that are encountered more often in Republican vs Democrat speech respectively, i.e. the phrases with highest partisanship. In one version of the slant measure, in this step I further exclude the phrase "illegal immmigrant" and its (lemmatized) substitutes "country illegally" / "border illegally".

Table A1: Partisan phrases in Congressional speech on immigration

Phrases used more often by Republicans		
illegal immigrant	illegal immigration	enforce immigration
illegal alien	amnesty illegal	human smuggling
secure border	citizen legal	lottery program
federal government	taxpayer dollar	immigrant program
immigration law	visa lottery	american taxpayer
american people	insurance policy	social security
enforce law	yuma sector	country illegally
drug cartel	raise taxis	legal worker
free enterprise	immigration nationality	security number
illegal worker	national language	national medium
Phrases used more often by Democrats		
domestic violence	rhode island	charter school
violence woman	jewish american	federal employee
asian pacific	hate crime	immigrant student
pacific american	undocumented immigrant	visa program
victim domestic	american worker	heritage month
immigrant woman	sexual violence	house republican
young people	american community	comprehensive immigration
sexual assault	health care	violence sexual
rule pass	senate bill	protect victim
american woman	native american	homeland security

For each phrase p and for each congressperson c I compute the relative frequency of the phrase in the congressperson's speech as $\tilde{f_{pc}} = f_{pc} / \sum_p f_{pc}$. I then regress the relative frequency of the phrase by congressperson on a continuous measure of the congressperson's ideology. Specifically, I measure ideology as the first dimension of the DW-nominate score

provided by *Voteview* – a widely used index of ideology derived from roll-call voting. I obtain phrase-specific intercept and slope coefficients a_p and b_p.

Finally, I compute the relative frequency of each phrase in AP dispatches released in a given quarter – $\tilde{f_{pq}}$ – and regress $(\tilde{f_{pq}} - a_p)$ on b_p. The resulting slope coefficient is the quarter-specific measure of slant.

As a within-sample validation of this measure, I also compute the analogous index by congressperson and correlate it with true ideology. The correlation is 0.59 for the baseline version of slant, and 0.62 for the version excluding 'illegal immigrant' and its substitutes. Taking the square of these coefficients, this implies that respectively 34% and 38% of the variation in these measures is due to variation in ideology, with the rest due to noise.

Slant of news outlets over time To compute immigration-specific slant by media outlet and year I follow the procedure outlined above with three modifications. First, I replace the AP dispatch corpus with a corpus containing the text (headline + first paragraph) of each news article mentioning the word 'immigrant'. Second, given the high volume of data I set the threshold for a phrase's occurrence in the corpus to 50, which results into 338 phrases. Third, in the final set of regressions I operate at the level of news outlet × year rather than by quarter.[35]

A.3 Plagiarism detection algorithm

In this section I describe the algorithm I use to identify 'immigrant' articles that are copied from AP but do not necessarily credit AP.

The first step of the algorithm is to assign to each article a set of AP dispatches that could potentially have been used in the writing of the article. I focus on AP dispatches released in the day before publication and mentioning the word 'immigrant'.[36] This is a simplified version of the procedure used in Cage et al. (2020), which first clusters articles by the event they cover, and then forms the set of potentially plagiarized articles as those that cover the same event and are published prior to the article of interest.

The second step in the algorithm is to compute a measure of verbatim copying. I pre-process all texts by removing punctuation and stop-words, stemming, and tokenizing into 5-grams. I then measure the share of the article's text that is identical to each paired dispatch and take the maximum over all paired dispatches. I label an article as copied from

[35]For some newspaper-years none of the selected phrases is contained in the news article's text – in this case the slant index is set to missing.

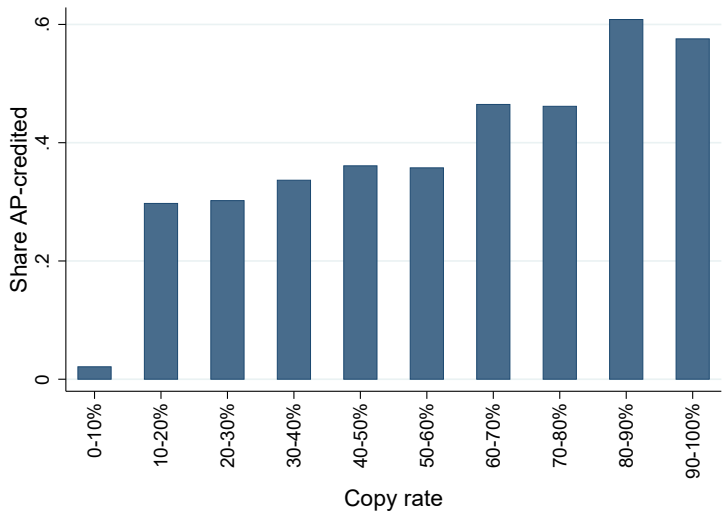
[36]I do not use contemporaneous (same-day) AP-dispatches because the origin of the content is more ambiguous in this case – text similarity could be due the media outlet copying AP, or to AP redistributing content produced by a member outlet. I also find that detected text similarity falls off sharply looking back further than 1 day.

AP if the maximum text overlap exceeds 25%, or about 90 characters for a paragraphs of average length (350 characters).

Figure A3 presents the relationship between copying and crediting AP, plotting the average share of credited articles by bin of the copy-rate distribution (i.e. by share of text overlapping with an AP dispatch). It is notable that even among articles whose lead paragraph is virtually identical to an AP dispatch (with 90-100% identical text), the rate of crediting AP never exceeds 60%. In other words, relying on credit to AP alone would have missed a substantial volume of copied articles.

When collapsed at the media outlet level, the correlation between the two independent measures of AP-intensity (i.e. constructed based on AP-credit vs constructed based on plagiarism) is 0.56.

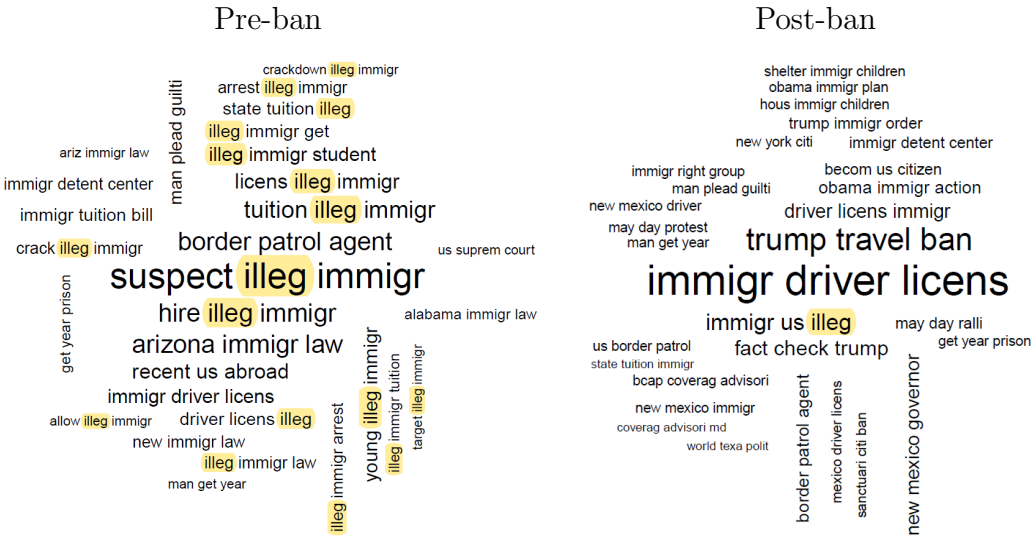
Figure A3: AP-credited articles by copy rate



Notes: Share of articles in which AP is credited (i.e. mentioning "AP" or "Associated Press" in the byline or first paragraph) by bin of the AP-copy rate distribution. The AP-copy rate is defined as the share of the article's text that is identical to the text of an AP dispatch released on the previous day.

A.4 Additional results

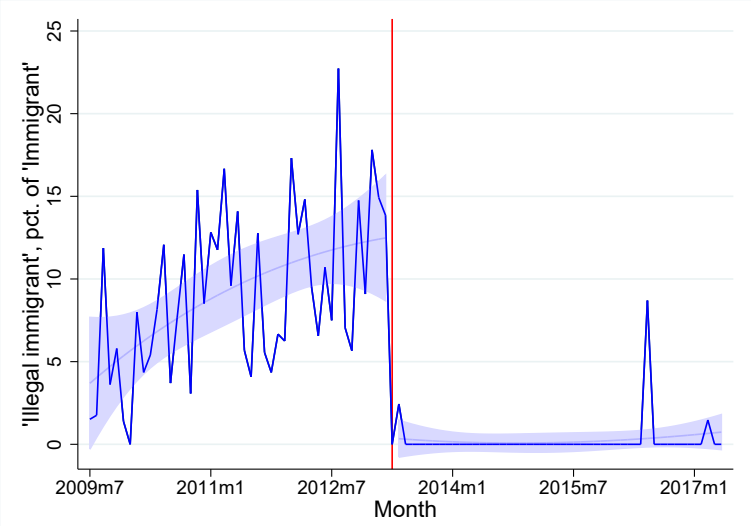
Figure A4: Headlines of "immigrant" dispatches pre- and post-ban



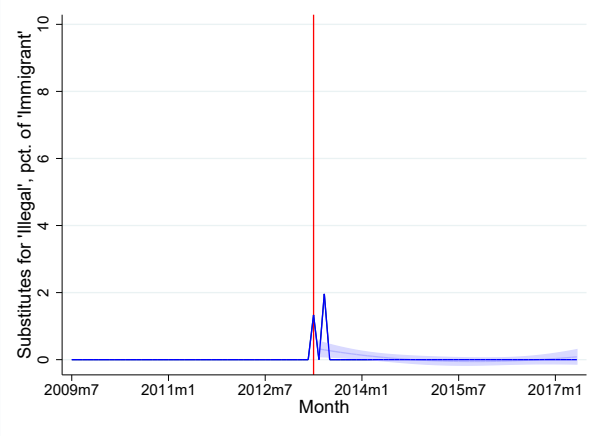
Notes: 50 most frequent tri-grams in the headlines of "immigrant" dispatches published by the AP before vs. after the ban.

Figure A5: AP's language on immigration over time: **Headlines**

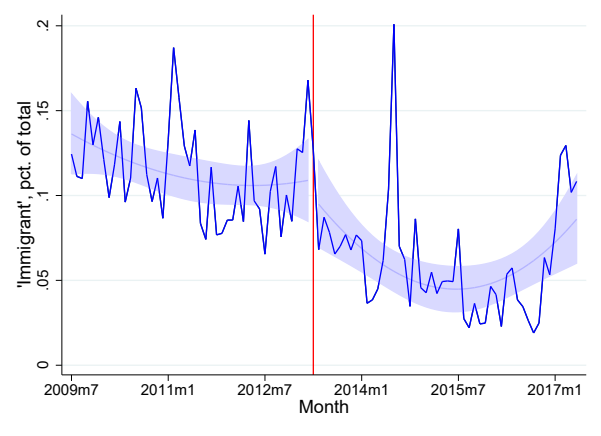
(a) Share "illegal immigrant"



(b) Share AP-approved substitutes



(c) Volume of "immigrant"

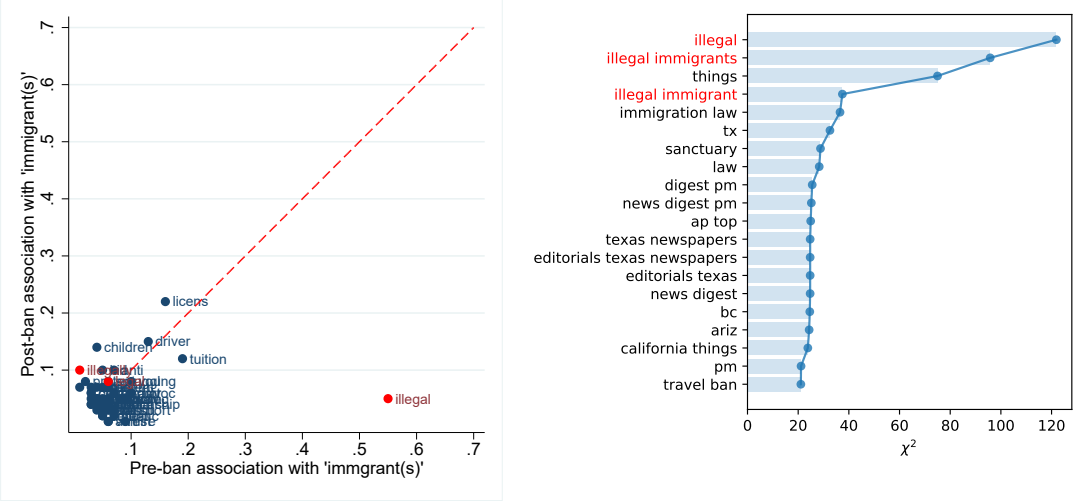


Notes: Panel (a): Number of 'illegal immigrant' headlines, as percent of 'immigrant' headlines. Panel (b): Number of headlines mentioning the AP-approved substitutes "enter* / live* in the country illegally/ without legal permission", as percent of 'immigrant' headlines. Panel (c): Number of 'immigrant' headlines, as percent of total.

Figure A6: AP's language on immigration before and after the ban: **Headlines**

(a): Correlates of the word "immigrant" before and after the ban

(b): Phrases predictive of pre- vs post-ban publishing date



Notes: Panel (a): Top 50 unigrams with highest rate of co-occurrence with the word 'immigrant' in the same headline before and after the ban. Panel (b): Top 20 n-grams ($n \in 1, 2, 3$) in AP headlines with highest predictive power of a pre- vs post-ban publishing date, based on a χ^2 statistic.

Figure A7: Topics of "immigrant" AP dispatches

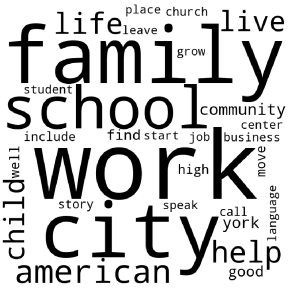
Topic 1 "Enforcement"



Topic 2 "Legislation"



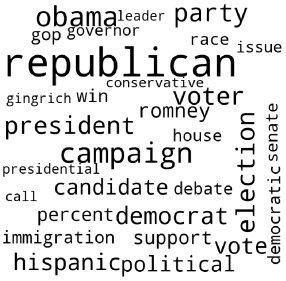
Topic 3 "Integration"



Topic 4 "Europe"

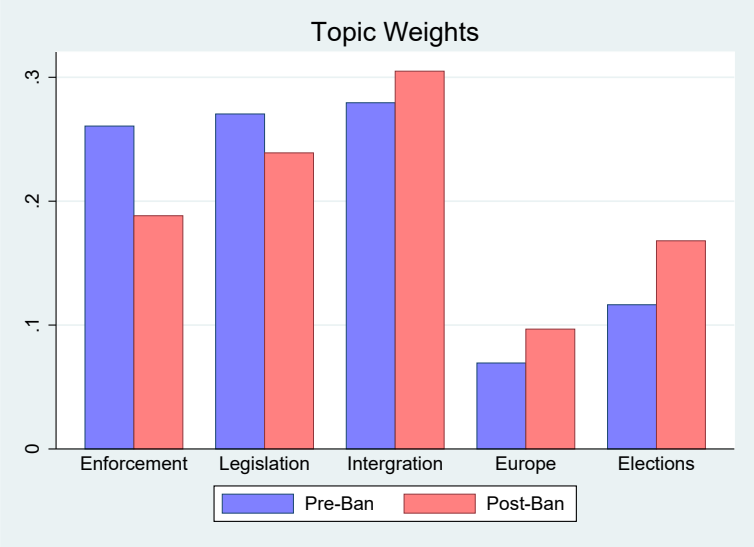


Topic 5 "Elections"



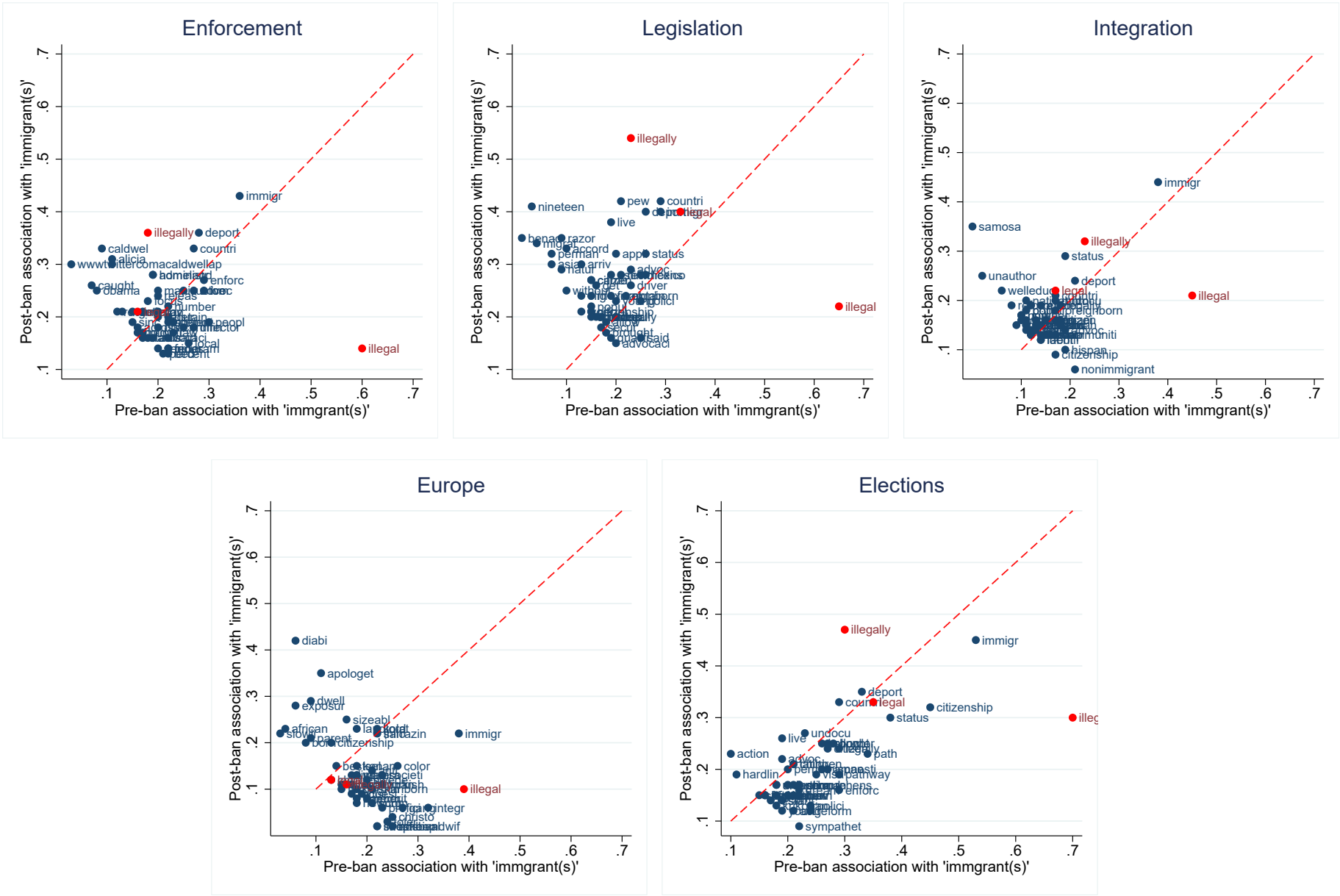
Notes: Word-clouds summarizing the 5 topics estimated from a Latent Dirichlet allocation (LDA) model applied to the corpus of AP dispatches mentioning the word "immigrant". The corpus excludes the term "illegal immigrant" and its substitutes. The number of topics is chosen for interpretability.

Figure A8: Topics distribution pre- and post-ban



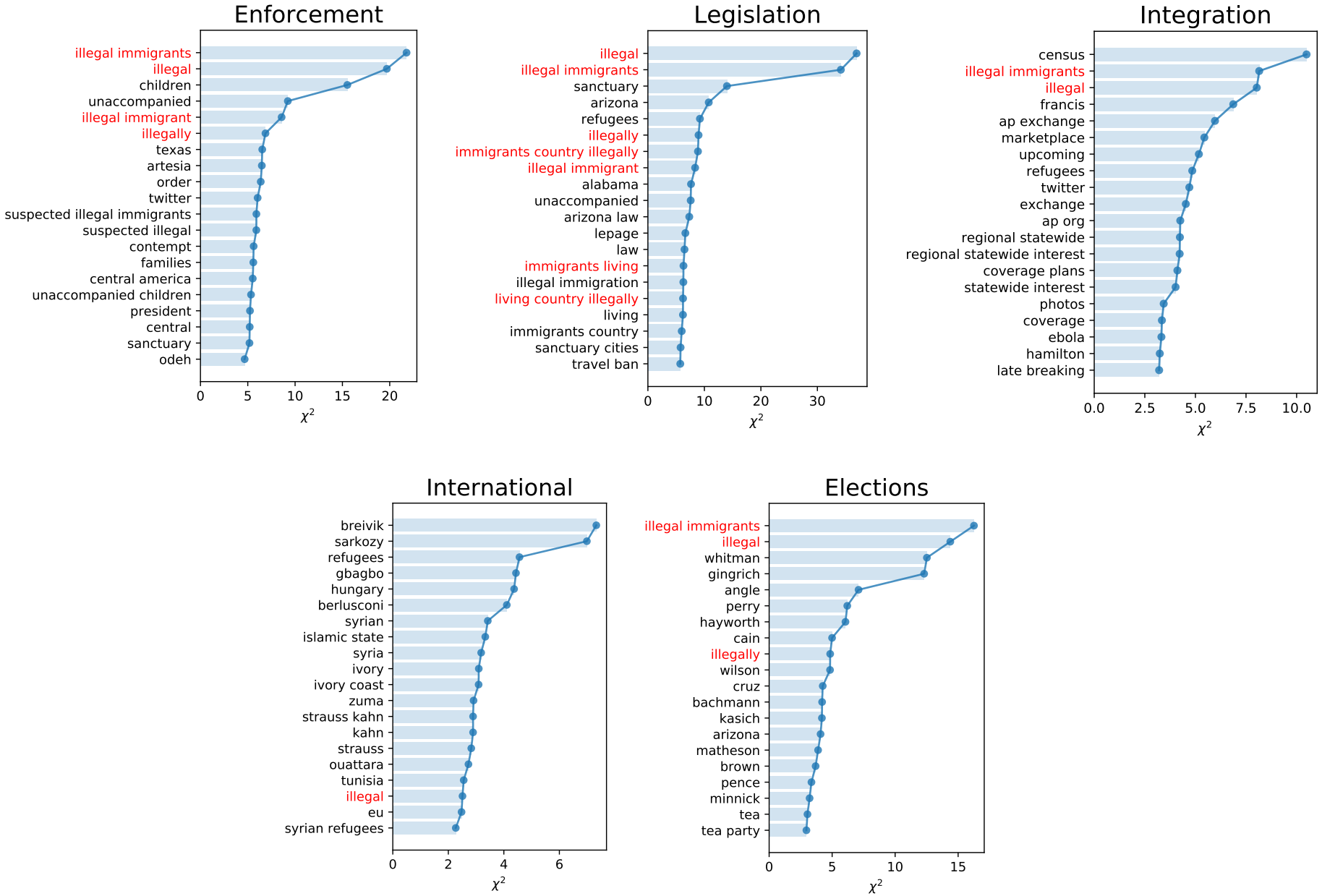
Notes: Distribution of topic weights before vs after the ban. The 5 topics are estimated from a Latent Dirichlet allocation (LDA) model applied to the corpus of AP dispatches mentioning the word "immigrant". The corpus excludes the term "illegal immigrant" and its synonyms. The number of topics is chosen for interpretability.

Figure A9: Correlates of the word "immigrant" before and after the ban: By topic



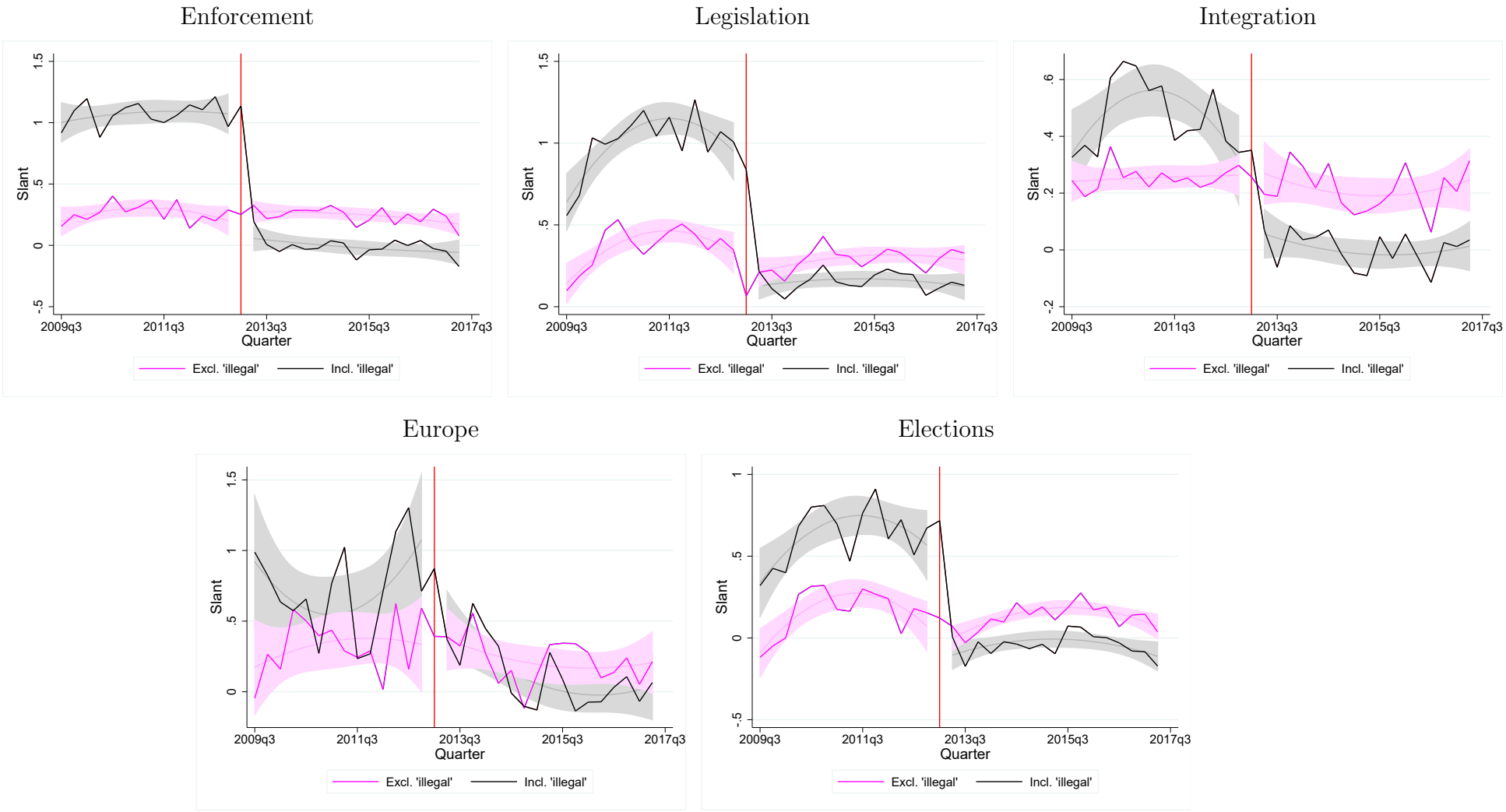
Notes: Top 50 unigrams with highest correlation with the word "immigrant", before and after the ban. Correlations defined based on rate of occurrence within the same article. Derivatives of 'immigr' and 'illeg' are not stemmed for illustration purposes.
Estimated separately for each of 5 topics derived from an LDA topic model.

Figure A10: Phrases predictive of post-ban publishing date: By topic



Notes: Top 20 n-grams (n ∈ 1,2,3) in "immigrant" dispatches with highest predictive power of a post-ban publishing date, based on a χ^2 test-statistic. Estimated separately for each of 5 topics derived from an LDA topic model.

Figure A11: Slant Index: By topic

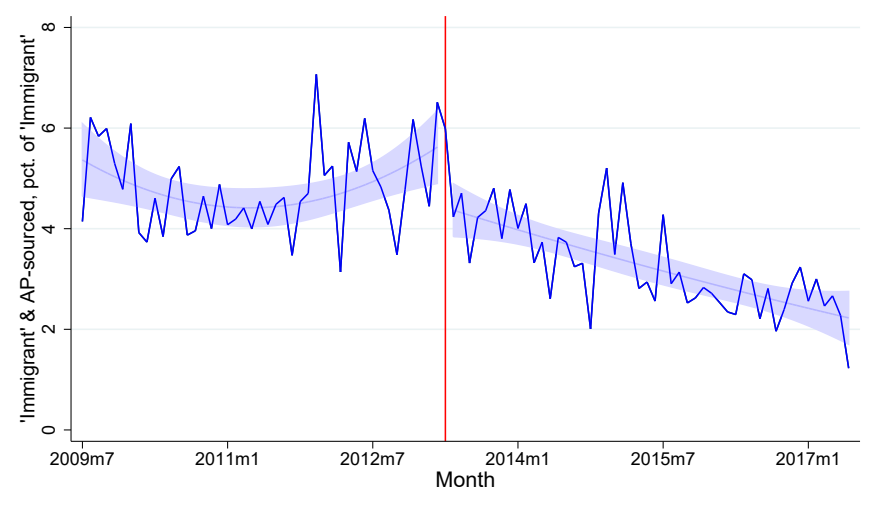


Notes: Immigration-specific slant of AP-dispatches by quarter, estimated separately for each of 5 topics derived from an LDA topic model, including (grey) or excluding (magenta) the term "illegal immigrant" and its substitutes. Higher values indicate more right-leaning slant.

B Diffusion of the ban

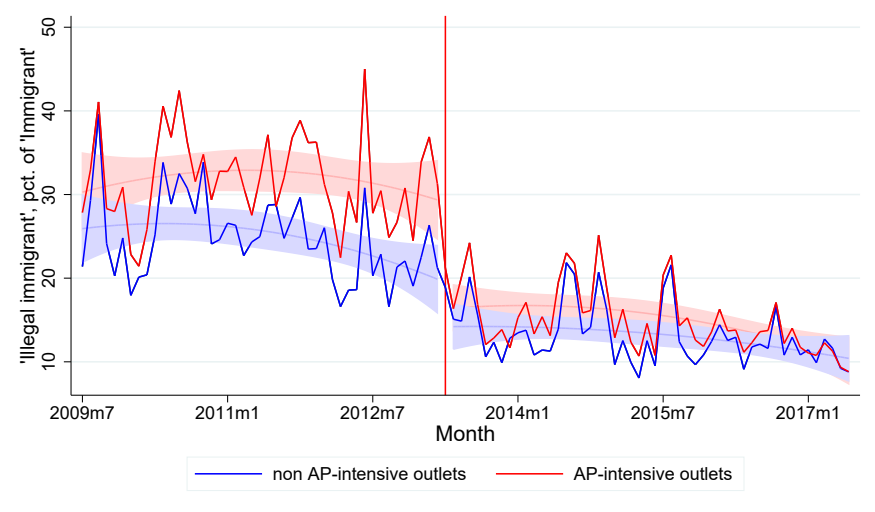
B.1 Descriptive statistics

Figure B1: 'Immigrant' articles sourced from the AP over time



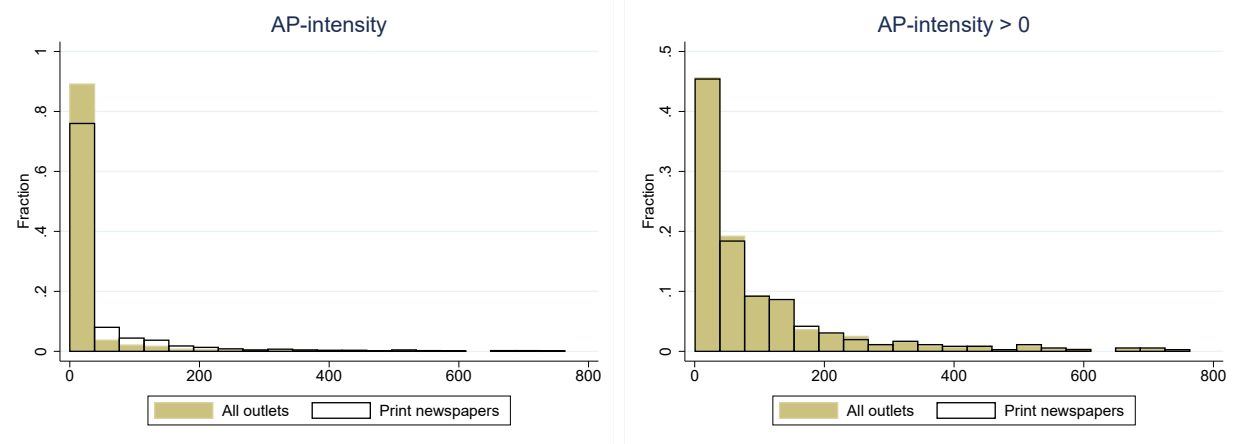
Notes: Average monthly number of AP-sourced 'immigrant' articles, as percent of all 'immigrant' articles.

Figure B2: Use of the term 'illegal immigrant' in AP-intensive vs non AP-intensive media outlets over time



Notes: Monthly number of 'illegal immigrant' articles, as percent of 'immigrant' articles, in outlets with zero AP-intensity vs ones with positive AP-intensity.

Figure B3: Distribution of AP-intensity



Notes: Distribution of AP-intensity, measured in number of AP-sourced 'immigrant' articles (either credited or flagged by plagiarism detection) by 1000.

Table B1: Summary statistics

Full sample: print & online media

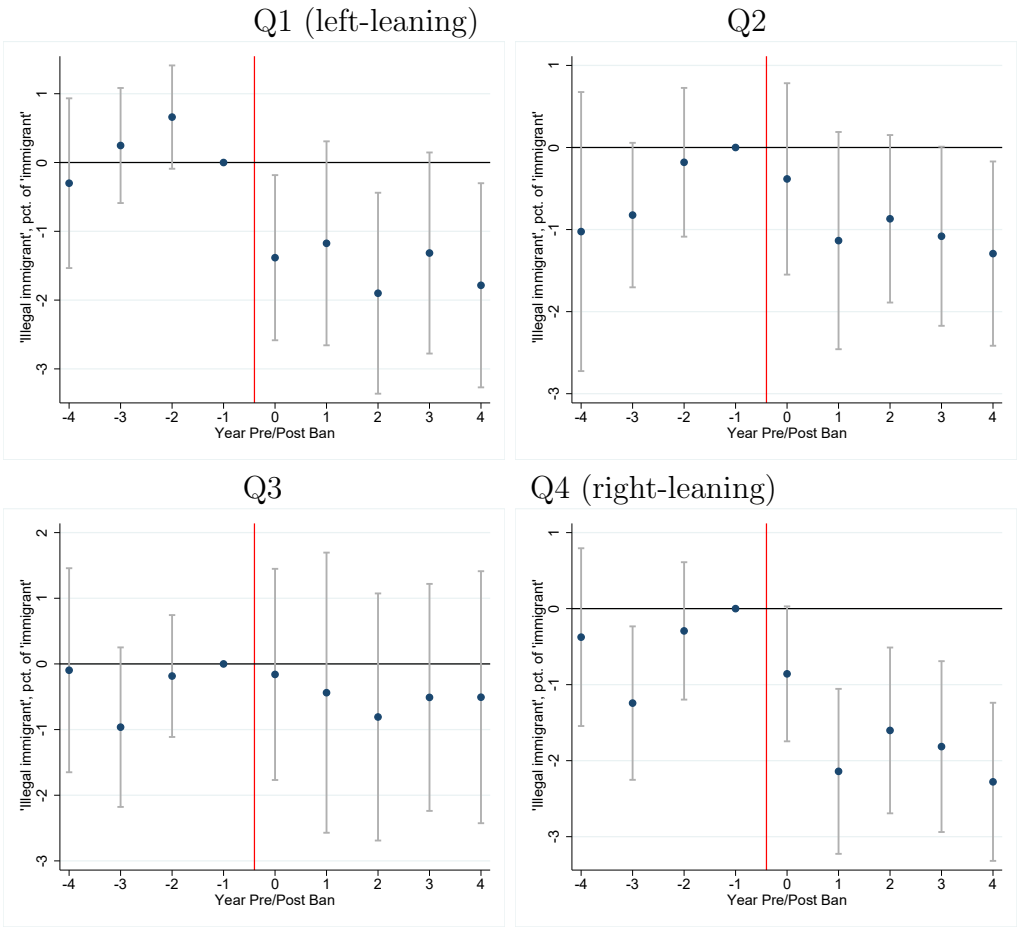
Variable	Mean	Std. Dev.	Min.	Max.	N
'Immigrant', pct. of total	1.934	3.041	0.005	100	128846
'Illegal immigrant', pct. of 'Immigrant'	20.08	21.182	0	100	143696
'Illegal immigrant', pct. of 'Immigrant' – Pre-Ban	28.885	23.546	0	100	64561
'Illegal immigrant', pct. of 'Immigrant' – Post-Ban	13.758	16.647	0	100	79135
'Immigrant' & AP-sourced, pct. of 'Immigrant'	3.809	10.684	0	100	137411
IHS(AP-intensity)	2.354	2.287	0	7.434	133757
1[AP-intensity $>$ 0]	0.602	0.489	0	1	133757
IHS(AP-intensity), credit	1.434	2.081	0	7.331	133757
IHS(AP-intensity), plagiarism	1.968	2.183	0	7.115	133757
IHS(AP-intensity), credit – all articles	1.706	1.715	0	6.517	129433
IHS(Reuters-intensity), credit – all articles (asinh)	0.801	1.397	0	7.154	129433

Daily print newspapers

Variable	Mean	Std. Dev.	Min.	Max.	N
'Immigrant', pct. of total	1.272	1.276	0.005	100	59390
'Illegal immigrant', pct. of 'Immigrant'	21.397	19.925	0	100	67538
'Illegal immigrant', pct. of 'Immigrant' – Pre-Ban	31.625	21.423	0	100	30356
'Illegal immigrant', pct. of 'Immigrant' – Post-Ban	14.115	15.014	0	100	37182
'Immigrant' & AP-sourced, pct. of 'Immigrant'	4.927	11.971	0	100	66239
IHS(AP-intensity)	2.893	2.256	0	7.434	65824
1[AP-intensity $>$ 0]	0.72	0.449	0	1	65824
IHS(AP-intensity), credit	1.777	2.217	0	7.331	65824
IHS(AP-intensity), plagiarism	2.477	2.212	0	7.115	65824
IHS(AP-intensity), credit – all articles	2.105	1.831	0	6.517	59295
IHS(Reuters-intensity), credit – all articles (asinh)	0.683	1.216	0	6.568	59295

B.2 Additional results

Figure B4: Diffusion by ideology: Dynamic diff-in-diff



Notes: Effect of the AP's ban on media outlets' use of the term 'illegal immigrant', estimated separately by quartile of the Gentzkow-Shapiro index of ideology. The graph present the coefficients and 95% confidence intervals from regressions restricted to one sample at a time of the number of "illegal immigrant" articles as percent of "immigrant" articles on the interaction of IHS(AP-intensity) and year relative to the ban, controlling for outlet and year-month FEs and weighted by number of "immigrant" articles. Standard errors are clustered by outlet.

Table B2: Diffusion estimates: IHS-transformed number of 'illegal immigrant' articles

	Full sample: print & online	Daily print newspapers
	(1) IHS('Illegal immigrant')	(2) IHS('Illegal immigrant')
PostBan × IHS(AP intensity)	-0.063***	-0.068***
	(0.006)	(0.007)
Outlet FEs	Yes	Yes
Year-Month FEs	Yes	Yes
Observations	217,189	79,315
Number of outlets	2276	837
R^2	0.56	0.51
Mean dep. var.	0.44	0.83

Notes: Effect of the ban on the IHS-transformed number of 'illegal immigrant' articles. The level of observation is media outlet times month. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Effects on readership

Sample restricted by quartile of ideology:	Full sample	Q1 left	Q2	Q3	Q4 right
	(1)	(2)	(3)	(4)	(5)
		Log circulation			
PostBan × AP-intensity	-0.002	0.006	0.011	-0.016	-0.005
	(0.004)	(0.008)	(0.008)	(0.014)	(0.006)
Outlet FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Observations	1,597	243	253	256	253
Number of outlets	380	47	51	56	65
R^2	0.99	0.99	0.99	0.99	0.99
Mean dep. var.	10.69	11.98	11.34	10.98	10.29

Notes: Effect of AP's ban on newspaper circulation. The level of observation is media-outlet × year. Standard errors are clustered by outlet. The subsamples by ideology are defined based on the index of ideology constructed by Gentzkow and Shapiro (2010), with higher values indicating more right-leaning ideology. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Diffusion estimates: By newspaper circulation

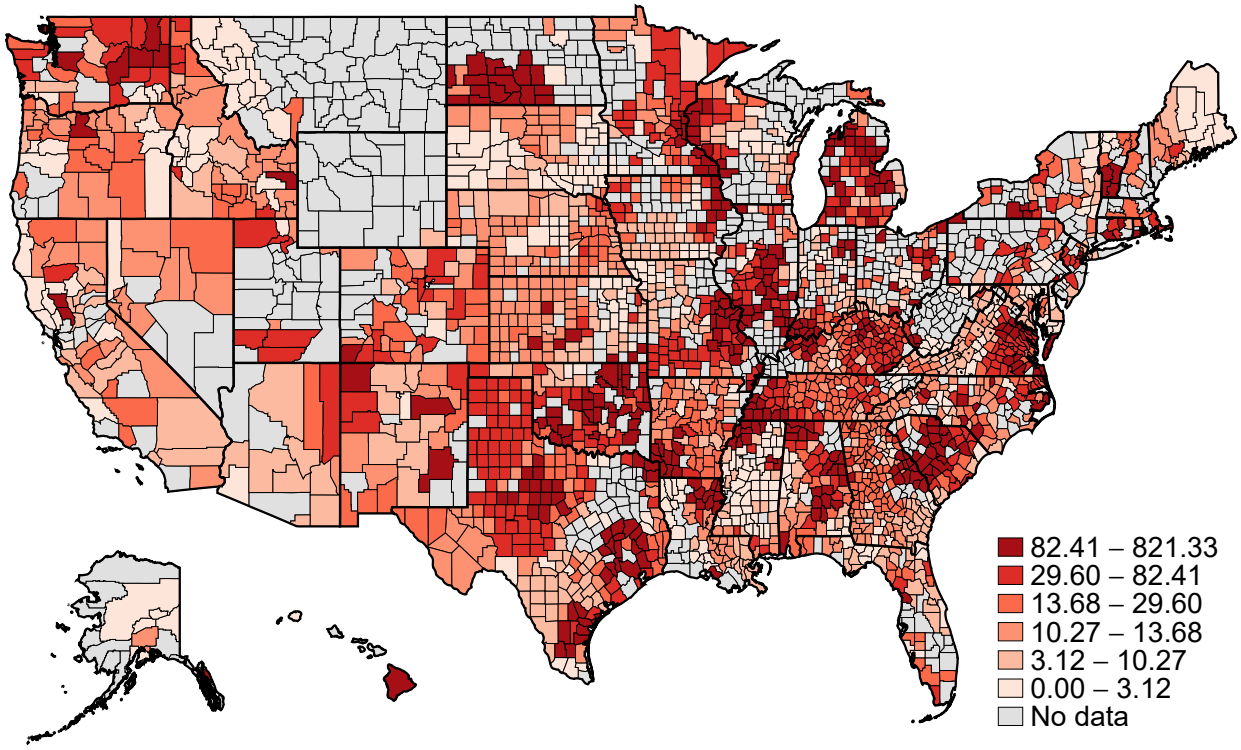
Sample restricted by quartile of circulation:	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
	'Illigal immigrant', pct. of 'Immigrant'			
PostBan × IHS(AP intensity)	-1.6591***	-1.2952***	-1.5107***	-0.9954***
	(0.4590)	(0.3907)	(0.3435)	(0.3763)
Outlet FEs	Yes	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes	Yes
Observations	12,762	15,175	18,093	18,733
Number of outlets	201	211	212	201
R^2	0.25	0.31	0.40	0.56
Mean dep. var.	24.30	23.80	23.49	20.35

Effect on AP's ban on use of the term 'illegal immigrant': heterogeneity by newspaper circulation. Q1 to Q4 denote quartiles of 2010 circulation, with higher values indicating higher circulation. The level of observation is media outlet × month. The dependent variable is the number of 'illegal immigrant' articles as percent of 'immigrant' articles. Weighted by number of 'immigrant' articles. Standard errors are clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, ***

C Views on immigration policy

C.1 Data and descriptive statistics

Figure C1: Geographic distribution of AP-intensity

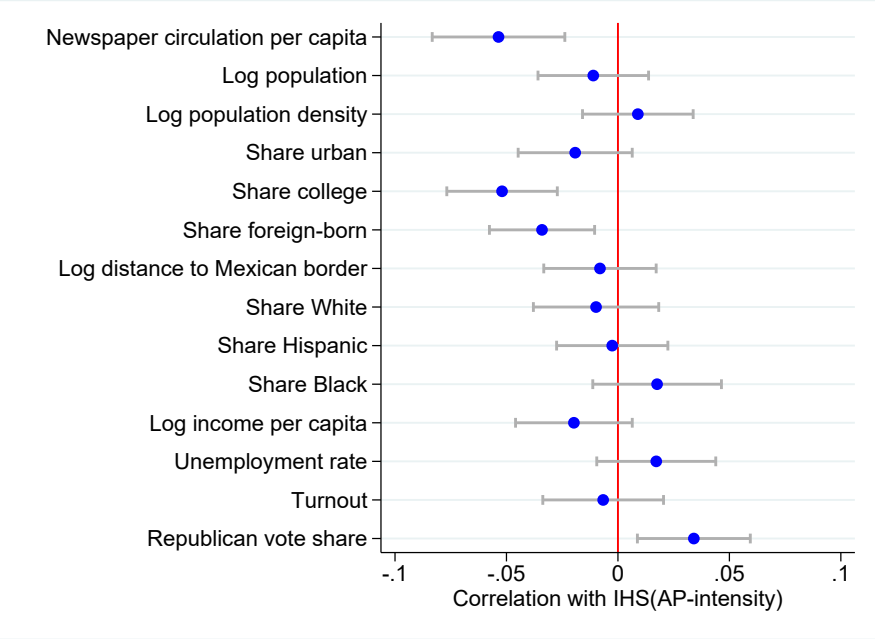


Notes: Distribution of AP-intensity by county, measured as the circulation-weighted number of AP-sourced articles per 1000.

Table C1: Summary statistics: CCES

Variable	Mean	Std. Dev.	Min.	Max.	N
Increase border security	0.546	0.498	0	1	203193
Sanction employers	0.612	0.487	0	1	119331
Grant legal status $\times -1$	-0.48	0.5	-1	0	203193
Allow police questioning	0.395	0.489	0	1	159932
Prohibit services	0.419	0.493	0	1	86715
Media Use - Read a newspaper	0.539	0.498	0	1	143549
Media Use - Newspaper Print	0.616	0.486	0	1	76639
Ideology:					
Very Liberal	0.088	0.283	0	1	203046
Liberal	0.176	0.381	0	1	203046
Moderate	0.308	0.462	0	1	203046
Conservative	0.234	0.424	0	1	203046
Very Conservative	0.118	0.323	0	1	203046
Age	50.272	16.351	18	99	203193
Gender (Male)	0.458	0.498	0	1	203193
Race:					
White	0.73	0.444	0	1	203193
Black	0.134	0.34	0	1	203193
Hispanic	0.071	0.257	0	1	203193
Other	0.066	0.248	0	1	203193
Education:					
No HS	0.029	0.168	0	1	203193
High School Graduate	0.264	0.441	0	1	203193
Some College	0.253	0.435	0	1	203193
2-Year	0.098	0.297	0	1	203193
4-Year	0.236	0.425	0	1	203193
Post-Grad	0.119	0.324	0	1	203193
Immigration status:					
Immigrant Citizen	0.048	0.213	0	1	192908
Immigrant non-citizen	0.015	0.121	0	1	192908
First generation	0.083	0.275	0	1	192908
Second generation	0.209	0.407	0	1	192908
Third generation	0.645	0.478	0	1	192908
Household income:					
Less than 20k	0.123	0.329	0	1	202970
20-40k	0.211	0.408	0	1	202970
40-60k	0.184	0.387	0	1	202970
60-80k	0.136	0.343	0	1	202970
80-120k	0.137	0.344	0	1	202970
More than 120k	0.096	0.295	0	1	202970
Illigal immigrant / Immigrant articles (pct)	20.827	12.175	0	96.627	203184
Illigal immigrant / Immigrant articles (pct) – Pre-Ban	29.55	11.308	0	96.435	88703
Illigal immigrant / Immigrant articles (pct) – Post-Ban	14.068	7.705	0	96.627	114481
IHS(AP-intensity)	3.291	1.53	0	7.404	203155
IHS(AP-intensity), credit	2.342	1.741	0	7.127	203155
IHS(AP-intensity), plagiarism	2.849	1.529	0	7.086	203155
IHS(AP-intensity), credit – all articles	2.178	1.579	0	6.356	190296
IHS(Reuters-intensity), credit – all articles	0.902	1.15	0	5.916	190296

Figure C2: County-level correlates of AP-intensity



Notes: Coefficients and 95% confidence intervals from univariate regressions of each of the listed county characteristics on IHS(AP-intensity). County characteristics are measured at baseline and standardized to mean zero and standard deviation one. Standard errors are robust.

Table C2: Immigration policy questions in the CCES

What do you think the US government / Congress and the President should do about immigration? Select all that apply:

	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Grant legal status to all illegal immigrants who have held jobs and paid taxes for at least 3 years, and not been convicted of any felony crimes.		✓			✓	✓[a]	✓	✓	✓	✓	✓	✓		✓
Increase the number of border patrol on the US-Mexican border.		✓			✓	✓[b]	✓	✓	✓	✓	✓	✓	✓[c]	✓
Allow police to question anyone they think may be in the country illegally.					✓	✓[d]	✓	✓	✓	✓		✓		
Fine US businesses that hire illegal immigrants.		✓			✓		✓	✓	✓	✓	✓	✓		
Prohibit illegal immigrants from using emergency hospital care and public schools.							✓	✓	✓[e]					

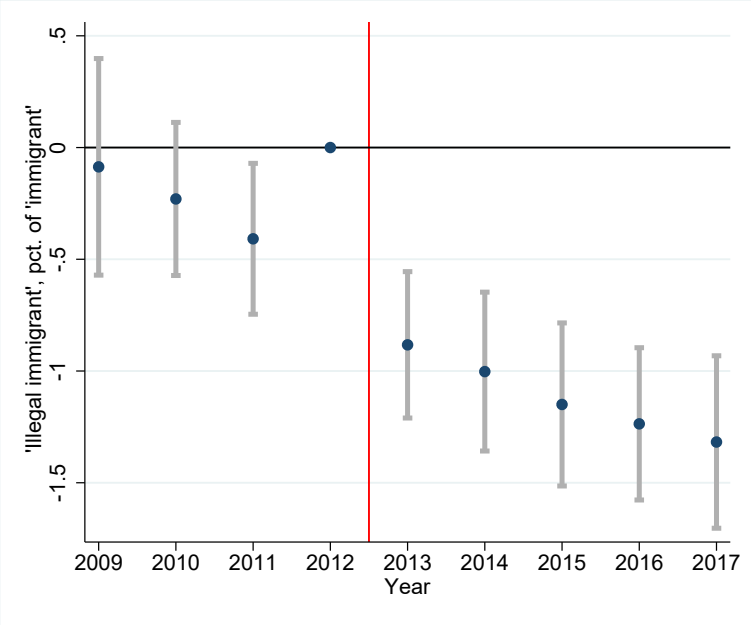
[a] Amnesty long term residents.
[b] Increase border presence.
[c] Increase spending on border security by 25 billion, including building a wall between the U.S. and Mexico.
[d] Police question those reasonably suspected of being illegal.
[e] Identify and deport illegal immigrants.

Table C3: Other policy questions in the CCES

	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Support scale for access to abortion (1 = Never permit, 4 = Always allow)	✓	✓	✓	✓	✓	✓	✓	✓						
Always allow abortion									✓	✓	✓	✓	✓	✓
Opposition to gay marriage scale (1=Strongly support, 4=Strongly oppose)	✓	✓												
Opposition to gay marriage			✓	✓	✓	✓	✓	✓	✓	✓	✓			
Repeal the Affordable Care Act							✓	✓	✓	✓	✓	✓	✓	✓
Opposition to affirmative action			✓	✓	✓	✓	✓	✓	✓					
Preference scale from 0 (increasing taxes) to 100 (spending cuts on education, health care, welfare and construction)	✓	✓	✓				✓	✓	✓	✓	✓	✓	✓	
Preference scale from 0 (increase income tax) to 100 (increase sales tax)	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓		
Preference scale between environmental protection and job availability (1=Environmental protection, 5=Job protection)	✓	✓	✓		✓		✓	✓						
Approve of military to assist in spread democracy / intervene in genocide or civil war / help the United Nations uphold international law / ensuring oil supply / protect US allies under attack / destroy terrorist camps	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓			

C.2 Additional results

Figure C3: Diffusion of the ban by county × year



Notes: Point estimates and 95% confidence intervals on the interactions of IHS(AP-intensity) with year, conditional on year and county FEs and baseline county controls interacted with time. Standard errors are clustered by county.

Table C4: Support for restrictive immigration policy: Standardized index

	Reduced Form				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Index restrict immigration							
PostBan × IHS(AP-intensity)	-0.0067***	-0.0063***	-0.0064***	-0.0067**				
	(0.002)	(0.002)	(0.002)	(0.003)				
'Illegal imm.', pct. of 'Imm.'					0.0083**	0.0067**	0.0080**	0.0065**
					(0.004)	(0.003)	(0.003)	(0.003)
Year × DMA FEs	No	No	No	Yes	No	No	No	Yes
Year × State FEs	No	No	Yes	No	No	No	Yes	No
Year FEs × County controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Respondent controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage coef.								
PostBan × IHS(AP-intensity)					-0.8127***	-0.9466***	-0.8043***	-1.0450***
					(0.282)	(0.210)	(0.186)	(0.237)
First-Stage F stat.					8.32	20.29	18.74	19.46
Observations	192,551	192,024	192,024	192,020	192,551	192,024	192,024	192,020
Number of counties	2,151	2,142	2,142	2,142	2,151	2,142	2,142	2,142
R^2	0.14	0.14	0.14	0.15				
Mean dep. var.	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00

Notes: Effect of AP's ban on support for restrictive immigration policy. The dependent variable is a summary index of all immigration-policy questions in CCES, standardized to mean 0 and standard deviation 1. Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C5: Support for increasing border security: Effects in terms of headlines and IHS-transformed number of 'illegal immigrant' articles

	2SLS	
	(1)	(2)
	Increase border security	
Headlines: 'Illegal immigrant', pct. of 'Immigrant'	0.0033**	
	(0.001)	
IHS('Illegal immigrant' articles)		0.0479***
		(0.018)
Respondent controls	Yes	Yes
Year FEs × County controls	Yes	Yes
County FEs	Yes	Yes
Year FEs	Yes	Yes
First-Stage F stat.	14.97	55.95
Observations	171,861	192,242
Number of counties	1,753	2,142
Mean dep. var.	0.54	0.55

Notes: 2SLS regressions of support for increasing border security on percent of circulated 'illegal immigrant' headlines over 'immigrant' headlines in column (1) and on the IHS-transformed number of 'illegal immigrant' articles in column (2). All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C6: Support for increasing border security: Alternative thresholds for inclusion in the sample

Panel (a): Reduced form

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Increase border security						
PostBan × IHS(AP-intensity)	-0.0072***	-0.0049***	-0.0050***	-0.0048***	-0.0047***	-0.0046***	-0.0045***
	(0.0019)	(0.0016)	(0.0016)	(0.0016)	(0.0015)	(0.0014)	(0.0014)
Threshold for inclusion in sample	==1	≥ 0.95	≥ 0.85	≥ 0.75	≥ 0.50	≥ 0.25	> 0
Observations	117,073	175,746	202,899	215,401	238,804	257,564	283,407
Number of counties	1723	2077	2186	2246	2356	2522	2870
R^2	0.10	0.09	0.09	0.09	0.09	0.09	0.09
Mean dep. var.	0.55	0.55	0.55	0.54	0.54	0.54	0.55

Panel (b): 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Increase border security						
'Illegal immigrant', pct. 'Immigrant'	0.0078***	0.0049**	0.0055**	0.0057**	0.0047***	0.0044***	0.0040***
	(0.0028)	(0.0019)	(0.0022)	(0.0024)	(0.0017)	(0.0016)	(0.0014)
Threshold for inclusion in sample	==1	≥ 0.95	≥ 0.85	≥ 0.75	≥ 0.50	≥ 0.25	> 0
F-stat.	14.26	21.49	18.72	16.00	25.71	29.29	34.01
Observations	117,073	175,746	202,899	215,401	238,804	257,564	283,407
Number of counties	1723	2077	2186	2246	2356	2522	2870
Mean dep. var.	0.55	0.55	0.55	0.54	0.54	0.54	0.55

Notes: Effect of AP's ban on support for increasing border security: Robustness to alternative thresholds for inclusion in the baseline sample – share of total county circulation covered by the NewsLibrary/ProQuest content data. Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C7: Support for increasing border security: Counties with one major newspaper

	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
	Increase border security			
PostBan × IHS(AP-intensity)	-0.0057***	-0.0037		
	(0.002)	(0.003)		
'Illegal imm.', pct. of 'Imm.'			0.0050**	0.0078
			(0.002)	(0.007)
Respondent controls	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Sample	1 major newsp.	>1 major newsp.	1 major newsp.	>1 major newsp.
First-Stage F stat.			16.49	3.03
Observations	102,326	89,916	102,326	89,916
Number of counties	855	1,287	855	1,287
Mean dep. var.	0.55	0.55	0.55	0.55

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Sample of counties with 1 major newspaper in columns (1) and (3), more than one major newspaper in columns (2) and (4). Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C8: Support for increasing border security: Alternative clustering

	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
	Increase border security			
PostBan × IHS(AP-intensity)	-0.0049***	-0.0049**		
	(0.002)	(0.002)		
'Illegal imm.', pct. of 'Imm.'			0.0051**	0.0051*
			(0.002)	(0.003)
Respondent controls	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Clustering	DMA	State	DMA	State
First-Stage F stat.			11.63	11.66
Observations	192,242	192,242	192,242	192,242
Number of counties	2,142	2,142	2,142	2,142
Mean dep. var.	0.55	0.55	0.55	0.55

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Standard errors clustered by DMA in columns (1) and (3), and by state in columns (2) and (4). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C9: "Illegal immigrant" articles and views on immigration policy: OLS

Panel (a): Cross-sectional correlations

	(1) Increase border security	(2) Sanction employers	(3) Grant legal status ×-1	(4) Allow police questioning	(5) Prohibit services	(6) Index Restrict imm.
'Illegal imm.', pct. of 'Imm.'	0.0031***	0.0024***	0.0037***	0.0039***	-0.0032***	0.0022***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	203,184	119,327	203,184	159,930	86,715	203,184
Number of counties	2,260	2,186	2,260	2,237	2,149	2,260
R^2	0.01	0.00	0.01	0.01	0.01	0.00
Mean dep. var.	0.55	0.61	-0.48	0.39	0.42	-0.00

Panel (b): Conditional on FEs and controls

	(1) Increase border security	(2) Sanction employers	(3) Grant legal status ×-1	(4) Allow police questioning	(5) Prohibit services	(6) Index Restrict imm.
'Illegal imm.', pct. of 'Imm.'	0.0000	0.0001	0.0000	0.0002	0.0004	0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Respondent controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	192,024	108,632	192,024	148,976	85,832	192,024
Number of counties	2,142	2,007	2,142	2,100	1,972	2,142
R^2	0.09	0.13	0.09	0.13	0.15	0.14
Mean dep. var.	0.55	0.61	-0.48	0.40	0.42	-0.00

Notes: OLS regressions of immigration policy views on the number of locally circulated "illegal immigrant" articles as percent of "immigrant" articles. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C10: Political effects: Voting intentions in the CCES

	Reduced Form				2SLS			
	(1) President Rep. vote	(2) Senate Rep. vote	(3) House Rep. vote	(4) Disapproval Obama Approval Trump	(5) President Rep. vote	(6) Senate Rep. vote	(7) House Rep. vote	(8) Disapproval Obama Approval Trump
PostBan × IHS(AP-intensity)	-0.0011	0.0008	0.0016	-0.0053				
	(0.001)	(0.003)	(0.002)	(0.003)				
'Illegal imm.', pct. of 'Imm.'					0.0011	-0.0012	-0.0017	0.0054
					(0.001)	(0.005)	(0.003)	(0.004)
Respondent controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage F stat.					22.59	5.94	17.48	22.32
Observations	164,046	86,609	123,003	194,775	164,046	86,609	123,003	194,775
Number of counties	2,121	1,947	2,056	2,144	2,121	1,947	2,056	2,144
Mean dep. var.	0.46	0.46	0.49	2.66	0.46	0.46	0.49	2.66

Notes: Effect of AP's ban on having voted/ intention to vote for the Republican candidate in Presidential, Senate and House elections, and on the disapproval of president Obama/ approval of president Trump. Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C11: Political effects: Electoral data

	Reduced Form			2SLS		
	(1) Rep. Share President	(2) Rep. Share Senate	(3) Rep. Share House	(4) Rep. Share President	(5) Rep. Share Senate	(6) Rep. Share House
PostBan × AP-intensity	-0.0009	0.0027	0.0006			
	(0.001)	(0.003)	(0.002)			
'Illegal imm.', pct. of 'Imm.'				0.0008	-0.0048	-0.0007
				(0.001)	(0.005)	(0.002)
Year FEs × County controls	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage F stat.	.	.	.	20.67	4.96	20.72
Observations	4,694	6,272	9,369	4,694	6,272	9,369
Number of counties	2,347	2,348	2,348	2,347	2,348	2,348
Mean dep. var.	0.64	0.63	0.66	0.64	0.63	0.66

Notes: Effect of AP's ban on the Republican vote share in Presidential, Senate and House elections. Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. All specifications control for baseline county controls interacted with time, county and year FEs. Weighted by 2012 voting-age population. Standard errors are clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C12: Support for increasing border security: Heterogeneity by ideology, 5-point scale

	Reduced Form					2SLS				
	(1) Very Lib.	(2) Lib.	(3) Moderate	(4) Cons.	(5) Very Cons.	(6) Very Lib.	(7) Lib.	(8) Moderate	(9) Cons.	(10) Very Cons.
PostBan × IHS(AP-intensity)	0.0012	-0.0070*	-0.0068**	0.0008	-0.0046					
	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)					
'Illegal imm.', pct. of 'Imm.'						-0.0011	0.0069	0.0070**	-0.0010	0.0052
						(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
First-Stage F stat.						17.23	15.37	19.02	17.03	19.66
Observations	16,254	33,497	58,978	44,952	22,323	16,254	33,497	58,978	44,952	22,323
Number of counties	959	1,335	1,735	1,759	1,470	959	1,335	1,735	1,759	1,470
R^2	0.13	0.08	0.08	0.12	0.16					
Mean dep. var.	0.22	0.35	0.52	0.75	0.81	0.22	0.35	0.52	0.75	0.81

Notes: Effect of AP's ban on support for increasing border security: heterogeneity by respondent ideology on a 5-point scale. All specifications control for respondent characteristics, baseline county characteristics interacted with time, county and year FEs. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.