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

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Abstract

Can the language used by mass media to cover policy-relevant issues affect readers' policy preferences? 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 dispatches distributed to media outlets by the Associated Press (AP) news wire. Using the text of AP's dispatches and about one million articles from 2200 outlets, I quantify outlets' prior reliance on AP-content and track their language and readers' views on immigration over time. I find that one standard deviation higher AP-intensity leads to a 10 to 14% decline in use of "illegal immigrant" after the ban. This change in language has a tangible impact on readers' views on immigration. Following AP's ban, individuals exposed to outlets with 1 standard deviation higher AP-intensity show 0.7 percentage point lower support for restrictive immigration and border security policies. The effect is driven by less engaged readers, and does not transfer to views on issues other than immigration.

Keywords: *Mass media, media slant, framing, immigration*

JEL Classification: D72, L82, Z13

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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 lead 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 readers in the intended direction, is lacking. From an empirical standpoint, assessing the causal impact of language is challenging for at least two reasons. First, votes-maximizing politicians and profit-maximizing media have an incentive to choose their slant to appeal to their audience's preference for like-minded content (Gentzkow and Shapiro 2006), making it difficult to disentangle cause and effect. Second, slanted language can be accompanied by other politically motivated choices, ranging from selective coverage 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 outlets from all sides of the political spectrum (Fenby 1986). This philosophy has led to extremely strict and rigid guidelines for the use of politically

[1] The implicit policy positions behind these phrases are easy to recognize. "The China virus" is presumably an attempt to shift responsibility for the 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).

sensitive language.

I exploit an abrupt reversal in AP's guidelines on the use of a specific politically charged term – "illegal immigrant". In April 2013, after years of resisting requests to revise its guidelines on the language on immigration, AP switched from officially *recommending* the term "illegal immigrant" to refer to people living in the US without legal authorization, to *banning* its use in AP wire 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 "illegal immigrant" in political speech and in the media.[2] In Congress, Republican representatives mention "illegal immigrant" about 50% of the time they mention "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.

[Figure 1 about here.]

Beyond the clear political charge of the banned term, the setting of AP's ban has several features that make it attractive to study the causal effects of slanted language. First, even though the AP likely considers general trends in public opinion in their decisions on style rules, in contrast to such trends AP's rules take effect in an extremely sharp fashion.[3] Second, the majority of US legacy media, representing viewpoints across the ideological spectrum, are members of the AP. It is therefore unlikely that any *individual* outlet sways decisions on AP's language rules. In other words, from the perspective of any *individual* outlet and the views of its readers, the ban can be viewed as producing exogenous variation in the editorial production function. Third and importantly for my empirical strategy, media outlets differ

[2]The reason for this divide can be traced back to deliberate party strategy. For example, "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]In the case of the ban on "illegal immigrant", the frequency of the term in AP dispatches fell instantaneously from 50% of "immigrant" mentions to 5% of "immigrant" mentions.

greatly in the extent to which they rely on AP's input. 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 content, 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. At the same time, the volume of AP's immigration coverage and other dimensions of AP's slant on immigration, computed following the procedure of Gentzkow and Shapiro (2010), remained largely unchanged. As a substitute for "illegal", the new guidelines suggested the phrase "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 "illegal immigrant". 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.

I then track how this change in AP's language diffuses into the language of media outlets, using text data from more than 2200 print and online outlets. I employ a difference-in-difference strategy comparing the monthly number of "illegal immigrant" articles as a share of "immigrant" articles before and after the ban, in media outlets with different AP-intensity at baseline. Specifically, I measure AP-intensity as the share of "immigrant" articles published by each outlet in the 12 months 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.[4]

My results suggest a large degree of diffusion – one standard deviation in AP-intensity causes a decline in the frequency of "illegal immigrant" articles by 14%. Put differently, outlets with positive AP-intensity decrease their use of the term by on average 28% compared to ones with zero AP-intensity, and for outlets in the top quartile of the AP-intensity distribution this decline reaches 60%. I find that on average, the diffusion effect is driven mostly by articles sourced from AP, as opposed to original ones. Hence, I find the same the (null)

[4]This procedure aims to capture the use of AP copy in cases when AP is credited as a source, and in ones in which AP is not credited (Cage et al. 2020).

effects on volume of immigration coverage and on other aspects of immigration slant as document for AP dispatches, as well as the same gap in reference to legal status. 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 content more or less.

Given the strong charge of the term "illegal immigrant", one could expect differential reactions by left-leaning outlets, for which the ban is ideologically congruent, and by right-leaning ones, for which it is in-congruent. Indeed, I find a significant diffusion for both groups of outlets, but of a magnitude 2 times larger among left-leaning outlets compared to right-leaning ones. This is despite similar diffusion of AP-sourced articles and is instead driven by a spillover effect into the language used in originally produced content that is only present for left-leaning outlets.

I next exploit AP's ban to identify the effect of exposure to "illegal immigrant" articles on readers' views on immigration policy, using pre- and post-ban waves of the Cooperative Congressional Election Study (CCES). To identify the reduced form effect of the ban, I employ a difference-in-difference strategy comparing CCES respondents before and after the ban, in counties with different AP-intensity of locally circulated newspapers. Alternatively, to scale magnitudes in terms of the effect of "illegal immigrant" articles circulated in the respondent's county, I instrument their number (normalized by the number of "immigrant" articles) with the interaction of county-level AP-intensity and the timing of the ban. This strategy accounts for time-invariant effects of other county characteristics correlated with AP-intensity, but relies on the assumption that their effect on readers' views did not change in coincidence with the timing of the ban. I address this threat by controlling for a wide range of baseline county characteristics interacted with time.[5]

The results suggest that one standard deviation higher AP-intensity of locally circulated newspapers is associated with 0.7 percentage points lower support for increasing border

[5]Importantly, my strategy includes time fixed effects, or alternatively, state-specific time fixed effects, which should absorb the effect of any major policy changes on policy views. County characteristics interacted with time should absorb any remaining heterogeneity in the effect of such confounds by socio-economic characteristics.

security after the ban. For comparison, 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 effects are robust to the inclusion of county controls, they occur in coincidence with the ban, and they do not reflect general reliance on newswire content as measured by *Reuters*-intensity.

While the above result applies to the sample of all CCES respondents, it is more pronounced for frequent print newspaper readers, who represent 33% of the sample. On the other hand, the effect is stronger among respondents with lower (self-reported) interest in politics, non-voters and independents. This is consistent with passive news consumers with weak priors on politically sensitive issues being more persuadable by slanted language. The effect is also more pronounced in counties with low shares of immigrants and Hispanics, where the issue of immigration is likely less salient and opinions on immigration policy more reactive to media framing.

I observe a similar shift in support for restricting immigration in 3 out of the 4 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 illegal immigrants, and on fining firms that employ illegal immigrants, and no significant effect on opposition to amnesty. On the other hand, a placebo exercise looking at other politically divisive policies suggests that these effects are specific to immigration. I find no significant change in responses on issues such as abortion, gay marriage or taxes and redistribution. 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 intentions to vote for Republican candidates or on electoral results. However I do find a significant effect on President Obama's approval, which suggests that the change in immigration policy views may have had some political repercussions in this period of heated debate over immigration reform.

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 2020). 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. 2020) or direct endorsement of candidates for office (Chiang and Knight 2011). This paper fits into the second category, but differs by studying a novel and 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 studying of 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 can be sensitive to changes in the framing of the issue. This finding complements recent work documenting a large degree of misinformation regarding the characteristics of immigrants and the effects of immigration, and showing that policy views can react in response to information treatments (Haaland and Roth 2020; Grigorieff et al. 2019; Alesina et al. 2019; Hatte et al. 2019).

[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).

Framing effects, which occur when differences in the presentation of an issue affect individuals' responses, are subject to a large literature in the fields of communication and social psychology (Strömberg 2015; Scheufele and Tewksbury 2007; Chong and Druckman 2007).[7] 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; Merolla and Ramakrishnan 2016; Ommundsen et al. 2014; Knoll et al. 2011). The findings of such studies generally point to significant effects of *issue* frames (e.g. highlighting legal status or associations with crime vs not), while the evidence on *equivalency* frames (i.e. using the term "illegal immigrant" vs "undocumented" or "unauthorized") is mixed. This paper complements this body of work but differs in terms of methodology, providing large scale observational evidence of framing effects "in the wild" rather than in a lab or survey experiment.

The rest of the paper is organized as follows. In section 2 I discuss the details of the ban and analyze how the text of immigration-related articles distributed by AP changed. In section 3 I track the propagation of AP's language into the language of AP-dependent media. In section 4 I analyze the effect of the ban on attitudes related to 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 policy.[8] Up until the change was announced, AP's guidelines stated that "illegal immigrant" was the *preferred* term while the alternative endorsed by the left

[7] 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). For example, Chen (2013) relates the structure of the future tense to inter-temporal trade-offs and outcomes ranging from saving and investment to smoking, risky sex and obesity.

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

– “undocumented immigrant” – was not allowed as AP considers it legally inaccurate (as continues to be the case to this day).

Appendix A.1 presents the exact formulation of AP's guidelines before and after April 2013. As “illegal immigrant” was banned, the new guidelines proposed the following substitutes: “living / entering the county illegally / without legal permission”.[9] Yet, AP executives recognized in their statement that these alternatives are likely harder for writers to use in text compared to the simple label “illegal” (`https://blog.ap.org/announcements/illegal-immigrant-no-more`). The ban took effect immediately in the online guidelines guidelines, which are also embedded in text editors (see Appendix Figure A1).

The ban was perceived as highly influential due to AP's dominant role in the US media landscape.[10] 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 printed newspapers published in the US, also entails permission for the AP to distribute local news reports produced by the member.[11] While AP is the main newswire used in US media, Reuters offers competing services and is used as a substitute by some newspapers[12]. Therefore, in some parts of the analysis I exploit Reuters-intensity as the closest available placebo for AP-intensity.

2.2 Data

To analyze how the language used by AP changed in response to the ban and 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,

[9] According to the guidelines, the ban does not concern “illegal immigrant” used in direct quotes, or the phrase “illegal *immigration*”.

[10] Appendix Figure A2 presents the reaction of the Atlantic with the headline “The AP's Ban on 'Illegal Immigrant' Will Change How We Talk About Immigration” as one example. Full article available at `https://www.theatlantic.com/politics/archive/2013/04/ap-ban-illegal-immigrant/316701/`.

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

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

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". I record the date, headline, word-count and full text of each dispatch. This search results in 28,000 dispatches, 8,000 of which mention the phrase "illegal immigrant".

2.3 Text Analysis Results

Descriptive evidence As a first check of how AP's language on the issue of immigration changed, figure 2 depicts 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. To illustrate this difference in language more concretely, in Appendix A.2 I present an example of two dispatches covering the same issue – a state law on immigrants' drivers licenses – released a month before and a month after the ban. Both dispatches have a neutral tone and highlight arguments from both the left and the right. However, the first dispatch talks about "non-citizens, including illegal immigrants", while the second talks about an "immigrant drivers' license bill".

[Figure 2 about here.]

The timing of this change coincides very precisely with the announcement of the ban. Panel (a) of Figure 3 shows the monthly number of AP dispatches mentioning the phrase "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.[13] Panel (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 "illegal immigrant" (45 percentage points). At the same time, Panel (c) suggests that volume of "immigrant" dispatches did not change around the ban.

[Figure 3 about here.]

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

Substitutes for "illegal immigrant" I use two approaches to gather more exhaustive evidence on what language the banned term was substituted with in practice. First, I analyse the change in the words associations (i.e., the rate of co-occurrence) between "immigrant" and each other unigram contained in the AP-text corpus.[14] I plot the results for the top 50 correlates of "immigrant" in Panel (a) of Figure 4, showing correlations in the full text on the left hand side, and correlations in headlines on the right hand side. The word "illegal" is clearly an outlier from the 45-degree line. Its correlation with "immigrant" drops dramatically from 0.66 (0.54) before the ban – the highest among all pre-ban correlates – to 0.21 (0.07) after the ban. Yet, the figure also suggests that no other unigram compensates for this decline. The closest candidate in article text is "illegally" – indeed, its correlation with"immigrant" increases significantly, but the magnitude corresponds to only about half of the decline of "illegal". In headlines, the substitution is of an even smaller magnitude, likely due to the fact that the synonyms proposed by AP are inconvenient to use in a headline.

An alternative, and arguably 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. Let χ^2_{pl} denote Pearson's χ^2 statistic for each phrase:

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

Panel (b) of Figure 4 presents the 20 words and phrases with highest χ^2_{pl}. "Illegal", "illegal immigrant" and "illegal immigrants" clearly emerge as the phrases 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

[14] 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.

only partially.

[Figure 4 about here.]

To rule out the possibility that these results reflects a shift in topics occupying the news cycle over the sample period, in Appendix C.1 I repeat the exercise separately for each of five topics estimated with a Latent Dirichlet Allocation (LDA) model.[15] The results discussed above are confirmed within each topic (with the exception of the χ^2 ranking within the topic of international affairs).

AP's overall immigration slant Finally, I examine whether the ban on "illegal immigrant" was part of a broader trend towards more liberal slant in AP's immigration coverage. To do so, 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. 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 B.1 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.

Figure 5 presents the top partisan phrases that enter these measures and the evolution of the 2 versions of the slant index over time. The version of the index that does not account for the ban on "illegal immigrant" follows closely the trend in AP's use of this phrase. This is intuitive since use of "illegal immigrant" is highly predictive of a Republican speaker in Congress (as already seen in Figure 1), and therefore receives a high weight in the measure of overall slant. However, once it is excluded and I focus on other dimensions of language, the trend in slant appears stable over time. Furthermore, neither measure of slant exhibits a clear pre-trend. This confirms the anecdotal evidence that the change in AP's language was sharp and unexpected.

[15] The estimated topics can be labeled as follows: law enforcement, immigration-related legislation, immigrants' integration and social issues, international issues such as the refugee crisis in Europe, and elections (Figure C1).

[Figure 5 about here.]

Taking these results together, the analysis of AP text suggests that: (1) As intended, the label "illegal" virtually disappears after the ban; (2) This decline is only partially compensated by the substitutes proposed by AP, while the remainder appears to omit any direct reference to legal status; (3) Other measurable dimensions of AP's language did not change significantly with the ban.

3 Diffusion

In next turn to analyzing the diffusion of AP's ban into the language of more than 2200 media outlets with different baseline reliance on AP copy ("AP-intensity").[16]

3.1 Data

Media content: Mentions of "immigrant" and "illegal immigrant" My main data source for media content is Newslibrary (`newslibrary.com`). I focus on print and online outlets that are covered continuously between July 1st 2009 and July 1st 2017 – there are 2566 such outlets. To cover some of the major US newspapers which are missing in Newslibrary, I supplement with data from ProQuest (`proquest.com`). This adds 125 newspapers.[17]

To construct measures of the language used in immigration coverage I search the database for articles that mention the phrase "illegal immigrant" (in singular or plural), and separately, for articles that mention the word "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, word-count, and the text of the first paragraph.

[16]This sample includes all US print and online outlets for which I am able to gather content data. However, the second stage analysis of readers policy views is restricted to the sample of print newspapers which allows me match geographic newspaper markets to the location of survey respondents. For consistency with this sample, Appendix C.3 replicates all results presented in this section restricting the sample to print newspapers.

[17]Outlets that never mention the word "immigrant" in the sample period drop out of this sample.

Using this information, I compute for each outlet and each month the number of articles that mention "illegal immigrant" normalized by the monthly number of articles that mention "immigrant". I repeat the procedure with wordcount instead of number of articles, for the phrase "illegal immigration" as a percentage of "immigration", and for the potential synonyms "undocumented immigrant" and "unauthorized immigrant" normalized by "immigrant". Lastly, I collect mentions of the alternatives endorsed by AP – "living in / entering the country illegally" or "[...] without legal permission".

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

To capture the cases in which AP is credited explicitly, I search for mentions of "Associated Press" or "AP" in the lead paragraph or byline of the article. A similar procedure was 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" articles identified in this way are indeed wire-copy. However, if media outlets use AP-content without explicit attribution, 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, I additionally run the text of each article through a plagiarism-detection algorithm, comparing it to the set of dispatches released by AP on the previous day. The goal is to detect articles in which large portions of text are verbatim copy from an AP dispatch. In practice, my algorithm looks for overlap of sets of 5-grams that exceeds the threshold of 20% of text. I describe this procedure in detail and discuss summary statistics on crediting and plagiarism in Appendix B.2.

AP-Intensity To proxy a media outlet's 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.

As a robustness check, I also select other pre-ban time windows further removed from the time of the ban (24 to 12 months or 24 to 36 months before the ban). I focus on the pre-ban period to avoid concerns about potential endogenous selection out of AP use in response to the change in AP's language policy. Indeed, Appendix Figure B3 suggests somewhat of a decline in the average extent of AP-use after the ban.

I then measure *AP-intensity* as the the number of articles copied from AP per 1000 articles in the selected pre-ban period – either credited to AP explicitly or flagged by plagiarism detection. Appendix Figure B2 presents the distribution of AP-intensity in my main sample of media outlets. This variable clearly features large variation, with the rate of AP-copying ranging from 0 to more than 750 AP-sourced articles per 1000. Since AP-intensity contains many zeros and has a skewed distribution, in the following analysis I use its inverse hyperbolic sine transformation.[18]

3.2 Empirical Strategy

To estimate the rate of diffusion from AP's language into that of AP-subscribing outlets, I implement a Difference-in-Difference strategy with continuous treatment. Specifically, I exploit the time-variation produced by the announcement of the ban and variation across media outlets in their exposure to the ban, proxied by AP-intensity. I estimate equations of the following form:

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

where $Illimm/Imm_{mt}$ denotes the number of articles in media outlet m and month t that mention the phrase "illegal immigrant" as percent of articles mentioning "immigrant", AP_m is 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 respectively. Standard errors are clustered at the outlet level. To account for the fact that $Illimm/Imm_{mt}$ is imprecisely

[18] About 20% of outlets in the baseline sample have positive AP-intensity (Table B2). This is the case for 40% of outlets in the sample of print newspapers (Table B3).

estimated when the denominator, i.e. the number of "immigrant" articles is low, which is a frequent occurrence at monthly frequency, in my preferred specification this regression is weighted by the number of "immigrant" articles.[19]

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 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 raw frequency of "illegal immigrant" articles in AP-intensive vs non AP-intensive outlets. Figure C5 shows these two series. While non AP-intensive media appear to gradually decrease their use of the term already prior to the ban, the use by AP-intensive media remains flat and quite high up until it exhibits a sharp decline coinciding with the ban. This pattern is in line with anecdotal evidence. For a long time, AP was resistant to demands to change their language policy, while in other media use of the term was gradually declining due to the controversy surrounding it. The figure also suggests that the ban was somewhat of an aggregate shock: even non AP-intensive media experience a decline at the time of the ban, albeit of a smaller magnitude. This is likely due to other outlets interpreting the ban – which was widely publicized – as a signal that the phrase "illegal immigrant" is no longer politically correct. Yet, the difference in magnitudes in the reactions of the two groups of outlets indicates that AP-intensity is a useful proxy for the degree of exposure to this aggregate shock.

[19] The reason I normalize the dependent variable – 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 absolute number of articles 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.

Diffusion estimates Table 1 presents the main regression results corresponding to specification 2. I find a significant negative effect of the ban on use of the term – the magnitude suggests that one standard deviation increase in AP-intensity (=2.1) leads to 3 p.p lower frequency of "illegal immigrant" after the ban, or 14% relative to the mean. In addition to month fixed effects, in columns (3) and (7) I control for month times state fixed effects to absorb the effects of potentially confounding factors that vary over time and by state, such as the availability of state-specific newsworthy events related to illegal immigration. In columns (4) and (8) I control for outlet-specific linear time trends to account for possible differential trends depending on outlet characteristics correlated with AP-intensity. The estimates are stable to these controls, and if anything, *increase* in magnitude. Column (5) shows that despite not being officially banned by AP, use of the term "illegal immigration" also decreased (though by only half the magnitude of "illegal immigrant").

[Table 1 about here.]

Diffusion over time To verify that trends in the use of "illegal immigrant" in high- versus low-AP-intensity outlets did not start to diverge already prior the ban, I split the interaction of AP intensity and *Post Ban* into a set of interactions with half-yearly leads and lags. The results are plotted in Panel (a) of Figure 6. I find that if anything, the relative frequency of"illegal immigrant" increases up until the ban (in other words, trends were diverging rather than converging), at which point it falls abruptly. The decline is persistent, in line with the permanently low supply of "illegal immigrant" AP dispatches.

Diffusion by bin of AP-intensity

In Panel (b) I relax another assumption implicit in equation 2 – that the effect is linear in AP-intensity. I estimate a more flexible specification discretizing the AP-intensity distribution. Specifically, I interact each quartile of the positive part of the AP-intensity distribution with *PostBan*, leaving outlets with zero AP-intensity as the reference category. The results suggest a roughly monotonic relationship in AP-intensity. The strongest effect comes from the top quartile, for which the effect amounts to a decline of 12 percentage points, or about than 60% relative to the mean.

Original vs AP-sourced articles If the documented decline in "illegal immigrant" articles is linked directly to the change in the language of AP dispatches, it should be driven by articles that are sourced from AP. To test this, in Panel (c) of Figure 6 I decompose the diffusion effect into articles copied from AP (with or without credit) vs. original content, and find that it is driven primarily by articles sourced from AP.

[Figure 6 about here.]

Robustness The result that outlets with higher AP-intensity decrease their use of the term"illegal immigrant" after the ban is stable to a number of alternative specifications and definitions of the variables of interest. In Table C1 I estimate specifications replacing the dependent variable with the number of "illegal immigrant" articles, dropping weights, replacing number of articles with word-count and with number of headlines, replacing continuous AP-intensity with a dummy for positive AP-intensity, and replacing *PostBan* with the time-series of "illegal immigrant" articles (normalized by "immigrant" articles) released monthly by AP.

In Table 2 I run the baseline regression with variations of the AP-intensity variable. Instead of accounting for both credited copying and plagiarism from AP, in column (2) I consider only the share of articles credited to AP, and in column (3) – only the share of articles flagged by plagiarism detection. The two measures have a correlation of 0.56 and yield very similar results to the baseline. In column (4), rather than examining the sample of 'immigrant" 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. Finally, as a placebo exercise, in column (5) I consider use of Reuters rather than AP. Since the Reuters newswire did not change their style rules regarding "illegal immigrant", prior reliance on Reuters should not be associated with the degree of reaction to the ban. Indeed, I find no change in use of the term depending on Reuters-intensity.

[Table 2 about here.]

Heterogeneous diffusion by ideological leaning Media outlets decide on the extent to which they want to use AP dispatches and are free to edit their language as they wish. Therefore, a natural question is whether the diffusion effect differs by the ideological position of the outlet, i.e. by how congruent the ban is with their editorial policy. To answer this question I analyze a sub-sample of about 340 newspapers which I can match to the index of ideological leaning constructed by Gentzkow and Shapiro (2010). I split this sample at the 33rd and 66th percentile with respect to this measure of ideological leaning, and label the 3 resulting groups of outlets as "left-leaning", "center" and "right-leaning".

In Panel (a) of Figure 7 I estimate the overall diffusion effect separately for each of the 3 groups. In each case, I find a significant decline in "illegal immigrant" articles.[20] Yet, in line with expectations, the magnitude of diffusion is significantly stronger among left-leaning outlets compared to center and right-leaning ones.

In Panel (b) I repeat the analysis looking only at articles sourced from AP. Interestingly, in this case I find very similar diffusion effects for the 3 groups of outlets, suggesting a limited role of screening and filtering out of AP-dispatches. Instead, in Panel (c) I focus on original articles and find significant differences – here the effect of "illegal immigrant" is negative and significant only for the group of left-leaning outlets, while for the 2 other groups I find, if anything, slightly positive point estimates. In other words, the ban has significant a spillover into original content, but only for outlets whose ideological position is congruent with this change in language.

The above results suggest that even right-leaning outlets comply with AP's ban to a significant extent, despite their (likely) ideological opposition to it. In other words, for the average right-leaning newspaper, inertia in the reliance on AP in content production appears to outweigh ideological considerations.

[20]Figure B4 presents the distribution of the difference in the share of "illegal immigrant" article post- versus pre-Ban by outlet. This difference is negative for 80% of outlets in the main sample, and for 90% of outlets in the restricted sample of print newspapers. This is indicative of mostly positive compliance, though its degree varies widely across outlets.

[Figure 7 about here.]

Other aspects of immigration coverage Having established that the ban on "illegal immigrant" in AP dispatches diffused into the *language* of media outlets, I turn to testing whether other measurable aspects of coverage were affected.

As with AP-dispatches, in Panel (a) of Figure 8 I find that the synonyms proposed by AP ("live(-ing)/enter(-ing) the country illegally / without legal permission") compensate some but not all of the decline in the phrase"illegal immigrant". The figure also suggests that this compensating effect tapered off over time.

Also consistent with AP-dispatches, in Panel (b) I find that the number of articles mentioning the word "immigrant" (normalized by total articles) was not affected by ban. The same null-effect applies to articles mentioning the word "immigration" over total articles.

Finally, I examine the effect of the ban on news outlets' immigration-specific slant. As with 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. [21] Panel (c) of Figure 8 presents the dynamic effect of the ban on slant, regressing the two versions of the index (computed at the level of news outlet $\times$ year) on the outlet's AP-intensity interacted with year fixed effects. The figure resembles the evolution of AP's slant (Figure 5) and suggests that the ban has a significant effect on the index that includes the phrase "illegal immigrant", but no clear effect on the version excluding the this phrase and its synonyms.

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 copied articles drive the majority of the diffusion effect. Crucially, the volume of immigration coverage and its slant, apart from the component driven by the banned phrase, appear to be unaffected. Hence, AP's ban can be interpreted to cause a shock to the use of "illegal immigrant", while leaving other features of coverage largely unaffected.

[Figure 8 about here.]

[21] See appendix B.1 for details on this procedure.

4 Effects on Readers' 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 analyze how it affected public opinion on immigration policy. To this end, I compare pre- and post-ban responses in the CCES electoral survey for respondents living in counties with different AP-intensity of locally circulated newspapers.

4.1 Data

4.1.1 Aggregation to the county level.

Since the CCES survey does not ask *which* newspaper the respondent reads, I rely on county of residence to assess exposure to locally circulated newspapers. Therefore, 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 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.[22] Finally, 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.[23] [24]

I match this data to the sample of Newslibrary/ ProQuest media outlets 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.[25] This ensures that the county-level data on newspapers' content is measured with reasonable precision. The resulting dataset contains about about 2300 counties (out of a total of 3000),

[22]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 voting-age population.

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

[24]Similar results obtain using data on circulation by newspaper and county compiled by Snyder and Strömberg (2010). This data 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.

[25]The results are robust to alternative thresholds, see table **??**.

and 800 daily newspapers (out of a total of 1200).

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:

$$AP_c = \frac{\sum_m (circ_{mc} \times AP_m)}{\sum_m circ_{mc}}, \tag{3}$$

where $circ_{mc}$ is circulation of newspaper m in county c. As in the outlet-level analysis, I take the inverse hyperbolic spline transformation of this variable. Panel (a) of Figure 9 presents the resulting geographic distribution of AP-intensity. Similarly, I aggregate the percentage of "illegal immigrant" relative to "immigrant" articles by county and year, again weighting by circulation:

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

Correlates of AP-intensity In order to understand the correlates of AP-intensity, I collect data on county-level demographic, economic and political characteristics. Data on annual county population is from ICHS. Data on the urban share of population is from the 2010 census. Racial composition, share college educated and share foreign-born are from the 2012 5-year American Community Survey, and the Republican vote share in the 2012 presidential election is from Dave Leip's Atlas. Finally, county-level newspaper circulation per capita is estimated with data from the Alliance of Audited Media combined with the Editor and Publisher yearbooks.

Panel (b) of Figure 9 presents the univariate correlations of each of these variables with AP-intensity. AP-intensity is significantly negatively correlated with population size and density, 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 smaller newspapers in less urban areas are more likely to resort to sourcing content from AP, rather than producing original reporting. A more urban, higher educated audience, as well as one with more immigrants may also have higher demand for original content, particularly on immigration.[26]

[Figure 9 about here.]

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 more than 50,000 respondents per wave (with smaller waves in some years), carried out roughly every 2 years.[27] The survey is administered online and a large portion of participants are YouGov panelists. Conveniently 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 the survey).

Views on Immigration Policy Each CCES respondent is asked to select immigration policies she thinks the US government should undertake, out of a list of options. The set of policies differs in each wave with questions ranging from allowing the police to question suspect unauthorized immigrants to building a wall between the US and Mexico. Appendix B.3 presents the full list of questions and their exact formulation in the survey. Two policies appear consistently in all survey years between 2009 and 2017: *"Increase the number of border patrols on the U.S.-Mexican border"* and *"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"*.

For each policy, I code support for *restricting* immigration (e.g. increasing border control/ *not* granting amnesty) as 1, and opposition as 0. I also compute an index aggregating choices on *all* 9 immigration policies featured in the questionnaire in the respective year, including

[26]For illustration, the 5 newspapers with highest AP-intensity in my sample are the: *The Westerly Sun (RI)* (651 AP-sourced articles per 1000), *The Telegraph Herald (Dubuque, IA)* (653 articles per 1000), *The Logan Banner (WV)* (692 articles per 1000), *The Bismarck Tribune (ND)* (699 articles per 1000) and *The Breeze-Courier (Taylorville, IL)* (763 articles per 1000).

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

rotating ones. I recode each choice in the direction of restricting immigration, and take the average across all standardized choices (following Kling et al. (2007)).

Views on policies other than immigration and voting As placebo outcomes, I also use CCES questions that relate to policy issues other than immigration. Specifically, I create (1) A dummy variable for opposing a woman's right to choose to have an abortion under any circumstances; (2) A dummy variable for preferring to cut public spending rather than increase taxes; (3) A dummy variable for opposing gay marriage; (4) A dummy variable for believing that the state of the economy has gotten worse over the past year.

The survey also asks about the respondent's voting intentions for upcoming presidential, Senate and House elections, as well as about the respondent's approval of the president (i.e. president Obama who is incumbent throughout my baseline sample period). I code dummy variables equal to one if the respondent intends to vote for a Republican candidate for a given office, and a dummy equal to one if the respondent disapproves of Obama's performance in office.

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

4.2 Empirical Strategy

To identify the local average treatment effect of exposure to the phrase "illegal immigrant" on views on immigration policy, I estimate 2SLS equations of the following form:

$$X_{cy} = \alpha_c + \beta_y + \rho \widehat{Illimm/Imm}_{cy} + \epsilon_{cy}, \tag{5}$$

$$Illimm/Imm_{cy} = \alpha_c + \beta_y + \gamma AP_c \times PostBan_y + \epsilon_{cy} \tag{6}$$

where X_{cy} denotes immigration policy preferences of respondents in county c and year y, $Illimm_{cy}$ denotes the percent "illegal immigrant" relative to "immigrant" articles read in that county and year, AP_c is the average AP-intensity of newspapers circulated in county c,

$PostBan_y$ is an indicator equal to one for survey waves carried out after 2013, and α_c and β_y are county and survey-year fixed effects respectively. Standard errors are clustered by county.

The first-stage equation has the same form as the difference-in-difference specification from the previous section, but now estimated at the county and survey-year level (instead of media outlet and 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 $AP_c \times PostBan_y$. The approach is thus akin to a shift-share strategy where $PostBan$ is an aggregate shock and AP_c is local exposure to that shock.

Since the identifying variation is at the county $\times$ survey-year level, this equation can be estimated by aggregating individual survey responses up to that level. Alternatively, it can be estimated at the respondent-level. This has the advantage of allowing to control for respondent characteristics which are likely to correlate with immigration policy attitudes.

The identifying assumption is that the interaction of AP intensity with the timing of the ban affects policy views only through exposure to the term "illegal immigrant". Time-invariant county-characteristics correlated with AP intensity are absorbed by county fixed effects. Therefore, if observed or unobserved characteristics are to confound my results, their effect on attitudes would have to *change* at the same as the ban took effect. To account for this possibility, I examine the sensitivity of the estimates to controlling for a host of county characteristics measured at baseline and interacted with survey-year fixed effects (see Figure 9 for the list of controls and their correlation with AP-intensity).

Additionally, a strict causal interpretation of the LATE estimates requires the assumption that no other aspect of coverage changed in response to the ban, except for the share of "illegal immigrant" articles. This assumption is supported by my analysis of content although I can not rule out other, potentially non-measurable changes in content or tone. Yet, even under valuation of this assumption the reduced form intention-to-treat estimates for the effects of are valid with slightly different interpretation as the effect of the combined changes changes in content induced by the ban.

Finally, the effect identified by equation 5 is 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 (the 1st stage of equation 5). The results presented in Figure 10 suggest that in this sample 1 standard deviation increase in AP-intensity (= 1.5) is associated with 9.5% lower use of the term "illegal immigrant" after the ban.

[Figure 10 about here.]

Intention to treat effects I then turn to the reduced form intention-to-treat effect of the ban on support for restrictive immigration policies. In column (1) of table 3, I examine the effect on an index aggregating all immigration-related CCES questions, conditional on respondent characteristics and baseline county controls interacted with time. In columns (2) to (5) I examine each component of the index that I am able to look at separately, i.e. each question that is asked at least once before the ban and at least once after. With the exception of the question on amnesty, the results suggest a significant negative reduced form effect of the ban of support for restrictive policies. The magnitudes range from 1.2% to 2% reduction in support for a given policy for 1 standard deviation higher AP-intensity. Similar results obtain at the county-level, with the dependent variable collapsed by county times survey-year (Appendix Table C15).

[Table 3 about here.]

Local average treatment effects In table 4 I estimate the 2SLS version of equation 5 for the same set of outcomes, again conditioning on respondent characteristics and county controls interacted with time. Here, the coefficient of interest is the second stage effect of locally circulated "illegal immigrant" articles on support for restrictive immigration policies. The results mirror those of the reduced form – an increase in such articles has a significantly positive effect on support for restrictive policies (with the exception of amnesty). The magnitudes range from 0.9% to 1.4% increase in support for a given policy for 1 percentage point (or 4.8%) higher share of locally circulated "illegal immigrant" articles. These results are also confirmed at the county level (Appendix Table C16).[28]

[Table 4 about here.]

Sensitivity to controls Since the border question is the only one (apart from the one on amnesty) that is asked in each CCES wave in the period of interest, I focus on this question for the remainder of this section. This has the advantage of holding the definition of the dependent variable constant over time, whereas the index aggregates policies of different severity in each wave, making comparisons over time harder to interpret.

Table 5 presents the reduced form and 2SLS effects on support for border security with alternative controls. In the first column, instead of including county and year fixed effects, I present the main effects of $PostBan$ and $AP - intensity$. The results mimic those from table 1. Consistent with the fact that AP-intensive outlets had a higher frequency of "illegal immigrant" articles before the ban, immigration policy views in such counties were more conserve before the ban (main effect on AP-intensity is positive). As with its effect on use of "illegal immigrant", the ban appears to be somewhat of an aggregate shocks to views on

[28] Appendix Table C8 presents the OLS equivalent of the relationship between share of locally circulated "illegal immigrant" articles and immigration policy views. The unconditional correlations (Panel a) are positive and highly significant throughout – as expected, in the cross section slant is strongly related to policy views but the direction of causality of this relation is not clear. Instead, and in contrast to the LATE estimates discussed above, in specifications saturated with fixed effects and controls, I instead find null OLS coefficients (Panel b). This difference is likely due to the nature of compliance to the ban. The group of 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 persuadable readers compared to the general public.

border security (the main effect of *PostBan* is negative), but it is amplified by AP-intensity. The coefficient on the interaction of PostBan with AP-intensity is stable to the inclusion of fixed effects and to county controls interacted with time, which absorb the possibly changing effect of these controls on readers' views. It is also robust to the inclusion of year $\times$ state fixed effects, which absorb the effect of any state-level policy changes – if anything, the 2SLS increase in magnitude.

[Table 5 about here.]

Reduced form over time To examine the dynamics of the reduced form effect, I estimate a regression including a full set of interaction of AP-intensity with indicators for survey waves, leaving the 2012 as the baseline category. In this analysis I can furthermore add the survey years 2007 and 2017, in order to examine longer-term trends. The results, presented in Figure 11 show no evidence of pre-trends – instead, the shift in policy views happens in the period after the ban, and remains roughly constant in following waves.

[Figure 11 about here.]

Reduced form by bin of AP-intensity In figure 12, 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, leaving the first quartile as the baseline category. The results suggest that the effect is monotonic in AP-intensity.

[Figure 12 about here.]

Robustness The above result is robust to alternative specifications, variable and sample construction,

In table 6 I test the robustness of the results to different versions of AP-intensity – using either attribution to AP or plagiarism detection to identify AP-sourced articles, and extending the definition to all articles, instead of ones on immigration. This yields very similar results to the baseline (columns 1 to 4 and 5 to 6). Instead, I find no differential effect of the ban depending on *Reuters*-intensity (column 4). This is reassuring since it suggests that the effect is specific to AP, rather than to the use of news wires in general.

[Table 6 about here.]

As a further robustness check, in Table 7 I vary the threshold for inclusion of a county in the sample based on the share of the county's circulation covered by the Newslibrary & ProQuest content data (set at $\geq$ 90% at baseline). 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 due to measurement error, 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.

[Table 7 about here.]

Finally, in Appendix Table C9 I show that similar results obtain using a dummy variable for AP-intensity above median instead of a continuous measure, clustering standard errors at the higher level of state instead of county, and restricting the sample to counties with one main newspaper, i.e. only one newspaper accounting for ¿=10% of total county circulation. The latter sample restriction holds for 40% of counties in the baseline sample and allows me to match each respondent to only one newspaper, avoiding aggregation of AP-intensity and content.

Heterogeneity by respondent characteristics In the above results I considered the sample of all CCES respondents. Yet, respondents who regularly read a newspaper are likely more exposed to the treatment. Therefore, in Figure 13 I split the sample into respondents who report that they have not read a newspaper in the past 24 hours, those who report that they have, and those who report that they have read a newspaper in print. This analysis has the caveat that the newspaper readership question was not asked in the 2012 wave, so that sample size and power are reduced. Yet, the results suggests a stronger magnitude of the effect among (self-reported) frequent newspaper readers.

[Figure 13 about here.]

I next examine the heterogeneity of the effects by respondents' political engagement and ideology. In Panel (a) of Figure 14 I split the sample into respondents who have voted in the most recent general election vs those who have not, and in Panel (b) I split the sample into respondents with low vs high (self-reported) interest in politics.[29] I find stronger treatment effects for voters who are not politically active and ones who are less interested in politics, in line with individuals with weak priors being more persuadable. This is also confirmed splitting the sample by a 3-point partisanship scale: in Panel (c) I find the strongest effects in the sample of independents.

[Figure 14 about here.]

Heterogeneity by county characteristics In Figure 15 I examine heterogeneity of the effect by the share of foreign born and share of Hispanic population in the county, splitting counties at the respective median value for the US. The results point to somewhat stronger effects in places with less immigrants and ones with less Hispanic population – a pattern that can again be interpreted in line with weaker priors for individuals with less direct content to the issue of immigration.

[Figure 15 about here.]

Views on other policies If these results reflect a general change in political leanings that by chance happens to be correlated with AP-intensity, we would expect that support for other policies endorsed by the Republican party is also affected in the same direction. In table 8 I present the results of a placebo exercise that tests for an effect on support for policies

[29] The exact wording of the question is as follows: *Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs?*

related to taxation, abortion, gay rights, and the respondent's assessment of the state of the economy. I find no significant effect of the on any of these outcomes.

[Table 8 about here.]

Voting Was the change in immigration policy views enough to shift voting choices? The answer appears to be no – in table 9 I show that the ban had no effect on intentions to vote for the Republican candidate in elections for various offices. In table 10 I use electoral data rather than voting intentions reported in CCES, and confirm the null effect for presidential elections and for House midterm elections.

One interpretation of these results is that the effect on voters' views on immigration may not have been large enough to affect voting choices. I do however detect a statistically significant negative effect of the ban on disapproval of President Obama (columns 4 and 8 of table 9). This is in line with the previous results, given Obama's immigration reform agenda.

[Table 9 about here.]

Table 10 about here.

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 treatment effect in the first stage amounts to 9.5% fewer "illegal immigrant" over "immigrant" articles per year. Relative to the mean in the ProQuest/ Newslibrary sample this implies about 9 fewer "illegal immigrant" articles per year. [30] 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

[30] The coverage of these data is not universal, so that this should be taken as a lower bound.

security, and this is the case for 37.6% of Democrats. Hence, the treatment corresponds to between 1.8% to 2.3% of the gap – a relatively mild effect.

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 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, that is, the share of respondents who are dissuaded to support restrictive immigration policy, can be expressed as:

$$f = \frac{db}{de}\frac{1}{1-b_0}, \tag{7}$$

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 $f = (0.009)/(1 * 1) * (1/0.59) \approx 1.5\%$.[31]

This magnitude is in the lower end of the effects estimates in the media literature, consistent with the milder nature of the treatment compared to other studies. For comparison, Chiang and Knight (2011) estimate a persuasion rate of 6,5% for the effect of a (surprising) newspaper electoral endorsement on voting intentions for that candidate.

Finally, this analysis and the interpretation of the results has focused on print newspapers, as circulation data allows me to map survey respondents to their respective locally read newspapers. However, views on immigration policy are also affected by consumption of TV

[31]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 section 2, the ban appears to be more salient when it comes to the language used in headlines. Assuming that readers are more likely to pay attention to headlines would therefore lead to a lower persuasion rate.

and Internet outlets, which may also have been affected by the ban. This matters for the interpretation of the results to the extent that the AP-intensity of other media consumed in a given county is positively correlated with that of locally circulated newspapers. In that case, the results would be interpreted as a combined media exposure effect, rather than a per-article effect.

4.5 Mechanisms

4.6 Persuasion vs social signaling

The results discussed above are consistent with a persuasion mechanism in which exposure to the phrase "illegal immigrant" affects readers' views on immigration policy. This interpretation is supported by the heterogeneity of the effect by respondents' political engagement, interest and partisanship – the treatment affects the views of individuals who do not have a strong partisan attachment or direct exposure to the issue of immigration and hence likely have a more malleable stance on immigration policy.

An alternative interpretation to that of persuasion may be that the ban served as a signal that expressing anti-immigration views is no longer socially acceptable (), making people less likely to state this response in a survey even if they do hold anti-immigration views. This would be a plausible interpretation if the announcement the ban received news coverage correlated with AP-intensity. To test if that is the case, I search "illegal immigrant" articles published in 2013 for mentions of the keywords (”ban” or ”Style Guide”) and (”AP” or ”Associated Press”). I find a total of 37 such articles in my sample of 2200 outlets. The correlation between coverage of the ban and AP-intensity is positive but low (0.08). Furthermore, a large fraction of these articles take a critical stance on AP's decision and are hence unlikely to shift norms in the direction suggested by my results.[32]

[32]To name some examples, the Miami Herald, the Arizona Daily Sun and the Chattanooga Times Free Press ran articles with the following respective titles: *”AP should not stop with 'illegal immigrants'”*, *”AP banning clear thinking”*, and *”Decision to ban 'offensive words' means banning thoughts as well”*.

4.7 Issue vs equivalency framing

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 illegeally”) and an issue frame (substitution from “illegal immigrant” to “immigrant”). While the setting of AP’s ban does not lend itself to differentiating between these 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 discussed in section 2, text analysis of AP dispatches suggests that this type of substitution occurred more frequently due to its compactness compared to the equivalency frame proposed by AP, and that was especially the case in headlines.[33]

Second, 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 (Merolla and Ramakrishnan 2016). It therefore seems plausible that the issue-frame component plays an important role in explaining the observed effects.

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, are therefore a useful source of variation to estimate the effects of media slanted language on readers.

Applying this strategy, I find evidence consistent with exposure to the term “illegal immigrant” in local media shifting preferences towards more restrictive immigration policy. This effect is driven by passive readers with less pronounced political views and lower direct exposure to immigration, consistent with a persuasion mechanism.

[33] This result highlights a potentially important aspect of politically slanted language – that frames pushed through short and catchy phrases are more readily adopted by the media compared to nuanced narratives.

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.

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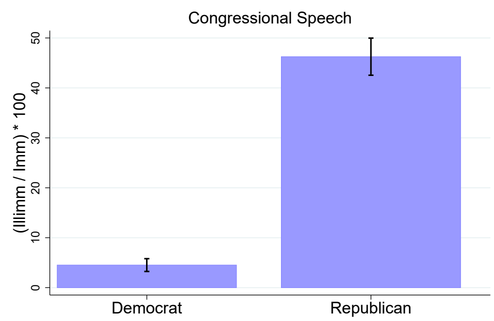
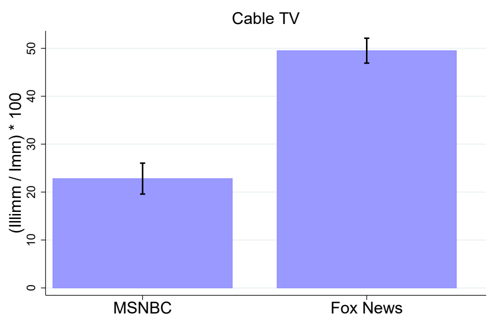
6 Figures

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

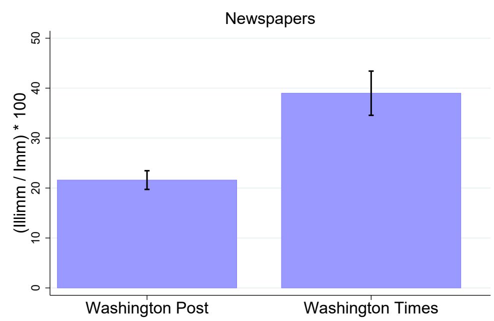
(a): Congressional Speech



(b): TV



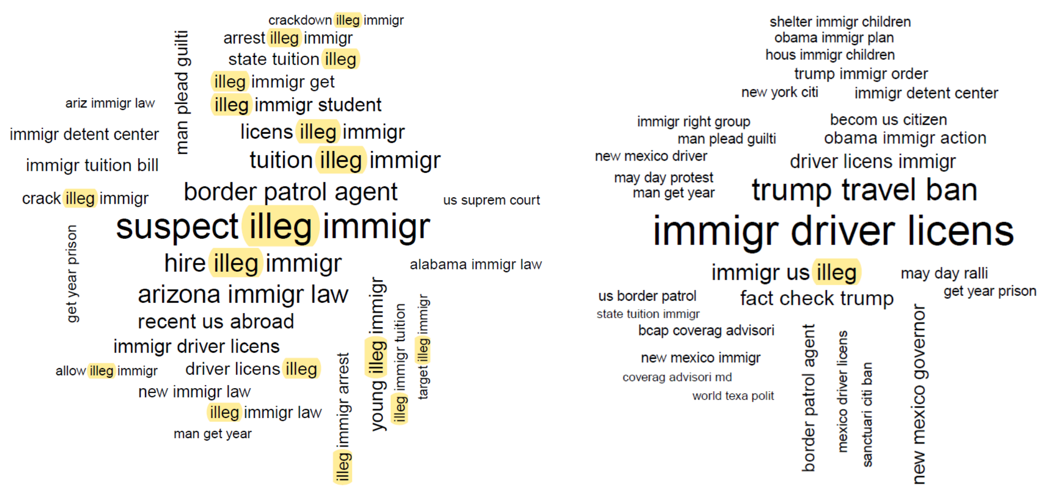
(c): Newspapers



Notes: Frequency of mentions of "illegal immigrant" relative to "immigrant" in congressional speech, in cable TV (comparing MSNBC and Fox News) and in newspapers (comparing the Washington Post and the Washington Times) in the years 2009 to 2017. Data sources: Congressional Record, GDELT TV Archive and ProQuest respectively.

Figure 2: Headlines of "immigrant" dispatches pre- and post-ban

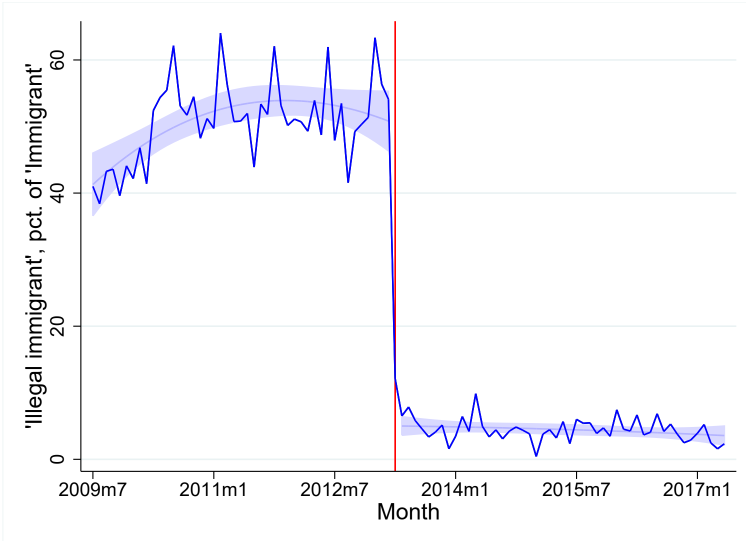
Pre-ban | Post-ban



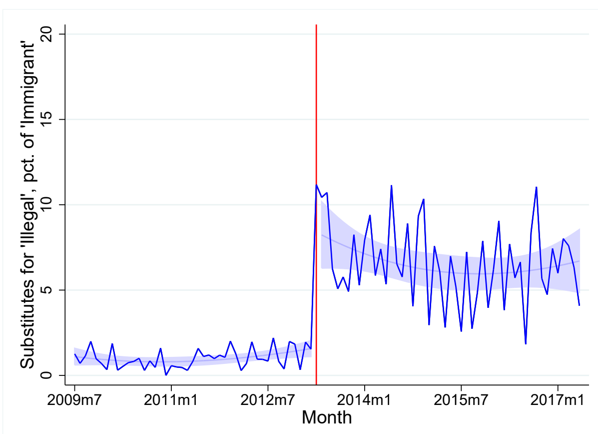
Notes: 50 most frequent tri-grams in the headlines of AP dispatches mentioning the word "immigrant", published before vs. after the ban.

Figure 3: Language of AP dispatches over time

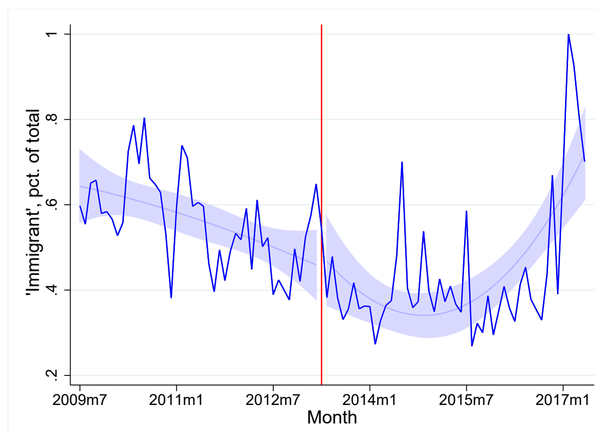
(a) Share "illegal immigrant"



(b) Share of substitutes proposed by post-ban guidelines



(c) Volume of "immigrant"

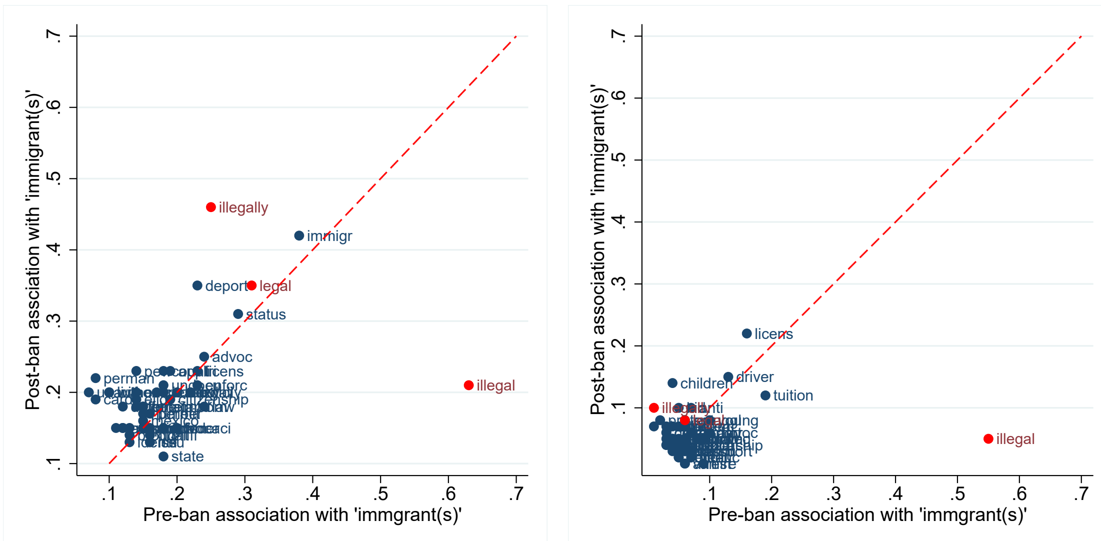


Notes: **Panel (a):** Monthly number of dispatches mentioning the phrase "illegal immigrant", as percent of dispatches mentioning the word "immigrant". **Panel (b):** Monthly number of dispatches mentioning the phrases "enter* / live* in the country illegally/ without legal permission", as percent of dispatches mentioning the word "immigrant". **Panel (c):** Monthly number of dispatches mentioning the word "immigrant", as percent of total dispatches.

Figure 4: Words and phrases used in AP's "immigrant " dispatches before and after the ban

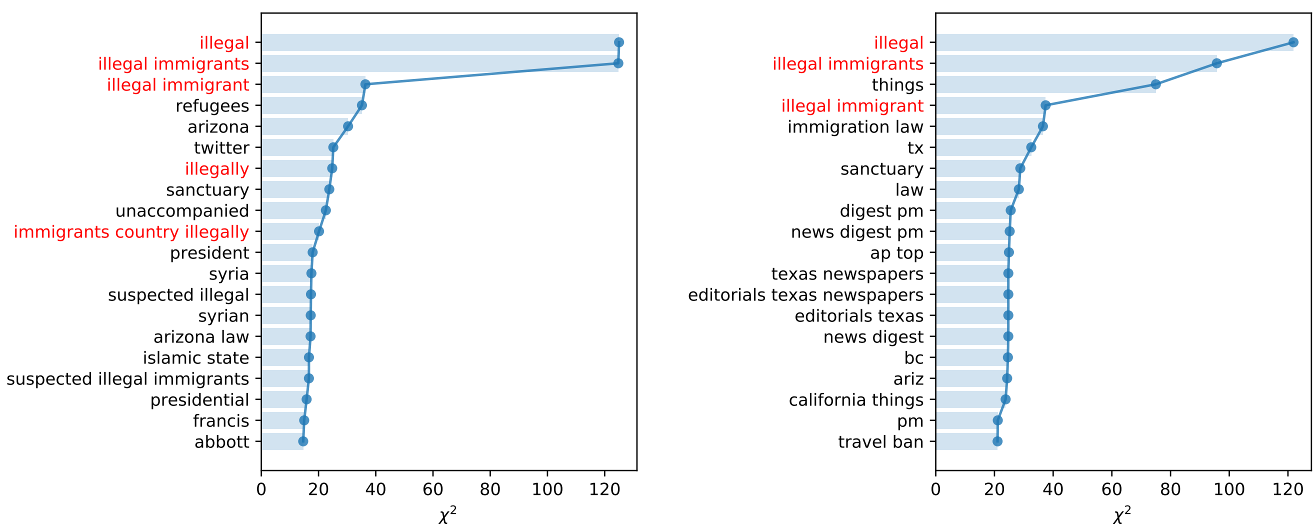
(a): Correlates of the word "immigrant" before and after the ban

Full text (inlc. headline) Headlines only



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

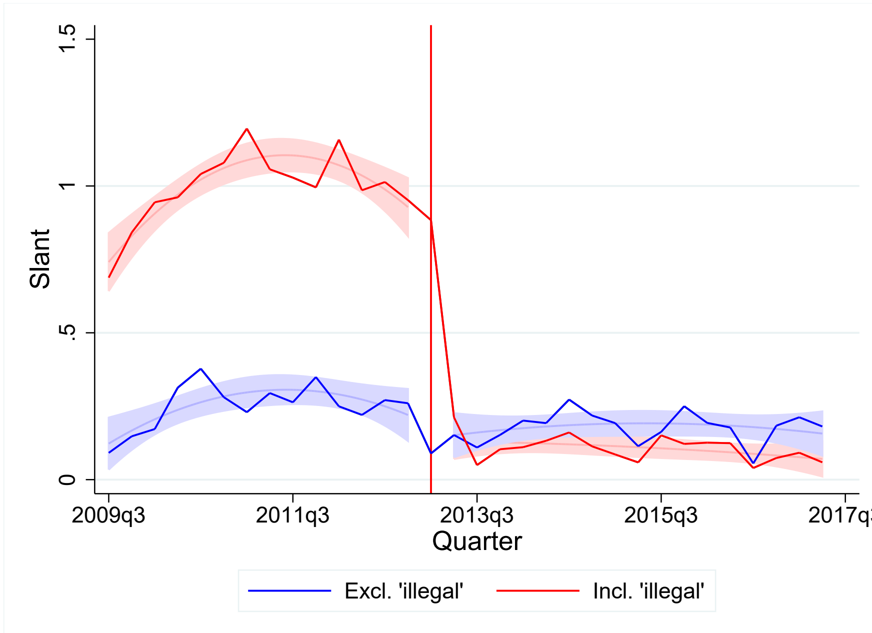
Full text (inlc. headline) Headlines only



Notes: **Panel (a):** Top 50 unigrams with highest association with the word "immigrant" before and after the ban. Association defined as the rate of co-occurrence within the same dispatch. All unigrams are stemmed, with the exception of derivatives of "immigrant" and "illegal". **Panel (b):** Top 20 n-grams ($n \in 1, 2, 3$) in "immigrant" dispatches that are most predictive of a post-ban publishing date based on a χ^2 test-statistic.

Figure 5: AP's slant on immigration

(a): Slant index computed for AP's immigration-related dispatches by quarter



(b): Top partisan phrases in immigration-related Congressional speech

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

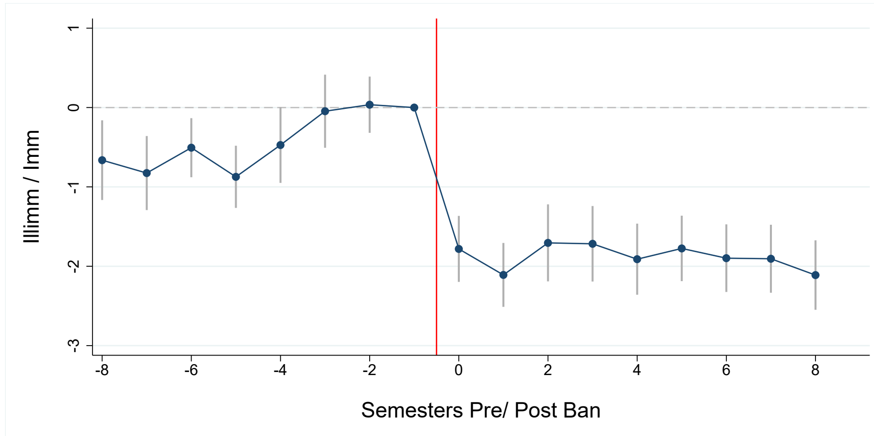
Phrases used more often by Democrats

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

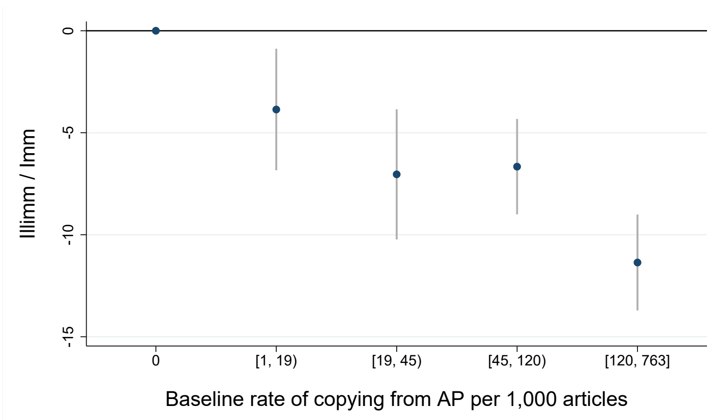
Notes:
Panel (a): Immigration-specific slant of AP dispatches over time. Higher values indicate more right-leaning slant. **Red line:** Baseline measure of slant. **Blue line:** Slant computed excluding phrases containing the term "illegal immigrant" or its substitutes.
Panel (b): Top partisan phrases derived from Congressional speech related to immigration.

Figure 6: Diffusion of the ban

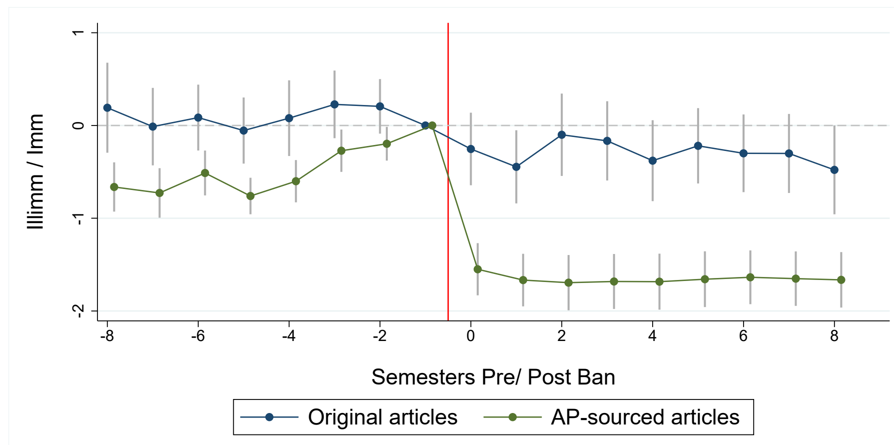
(a): Dynamic diff-in-diff estimates



(b): Effects by quartile of AP-intensity



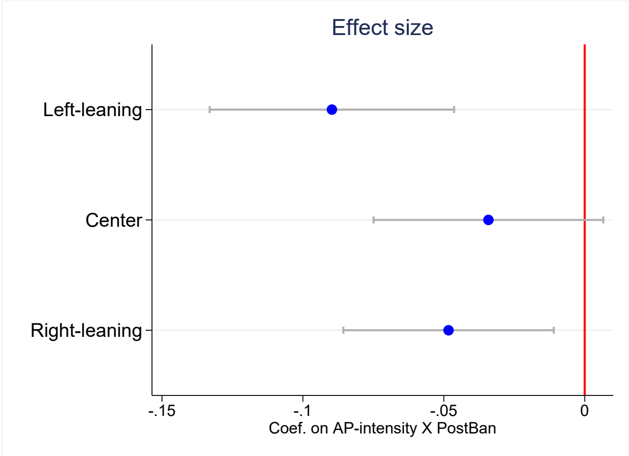
(c): Dynamic diff-in-diff estimates for AP-sourced vs original articles



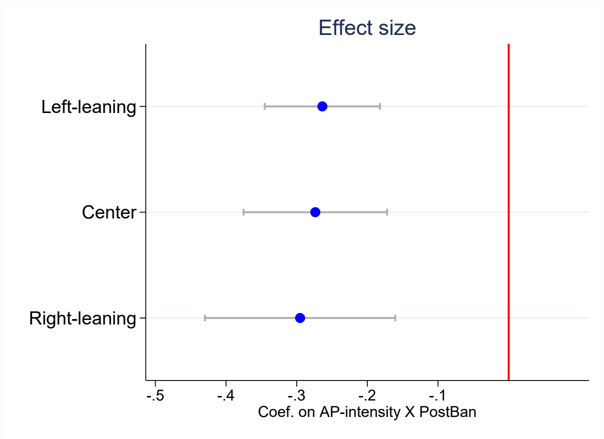
Notes: **Panel (a):** Coefficients and 95% confidence intervals from a regression of the frequency of "illegal immigrant" articles as percent of "immigrant" articles on full set of indicators for semester pre-/post-ban interacted with AP-intensity, controlling for outlet and year-month FEs. The omitted category is the semester before the ban. **Panel (b):** Coefficients and 95% confidence intervals from a regression of frequency of "illegal immigrant" articles as percent of "immigrant" articles on a full set of indicators for quartile of (positive) AP-intensity interacted with Post Ban, controlling for outlet and year-month FEs. The omitted category is AP-intensity = 0. **Panel (c):** Weighted by number of "immigrant" articles. Standard errors clustered by outlet.

Figure 7: Diffusion by Ideology

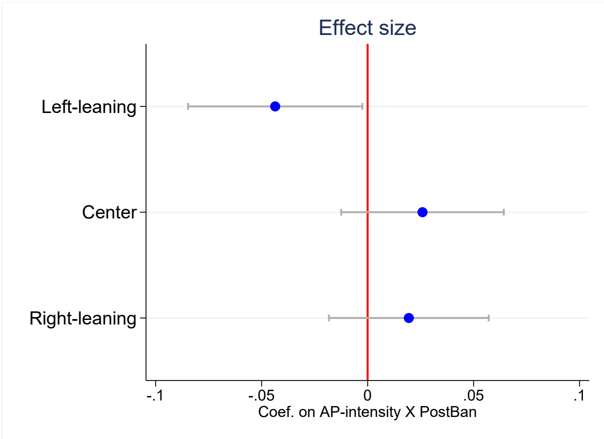
(a) Effect on "illegal immigrant" articles



(b) "Illegal immigrant" articles sourced from AP



(c) Original "illegal immigrant" articles

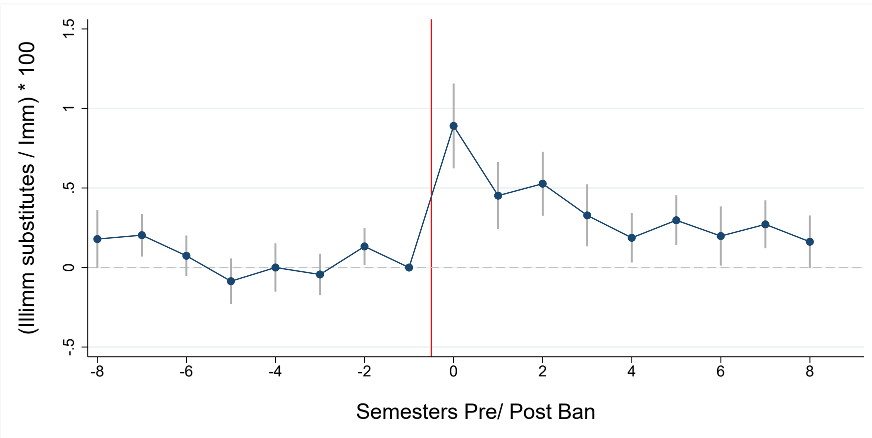


Notes: **Left-leaning**, **Center**, **Right-leaning** denote the sample of outlets in the 1st, 2nd and 3rd tercile of the distribution of the Gentzkow and Shapiro (2010) slant index respectively.

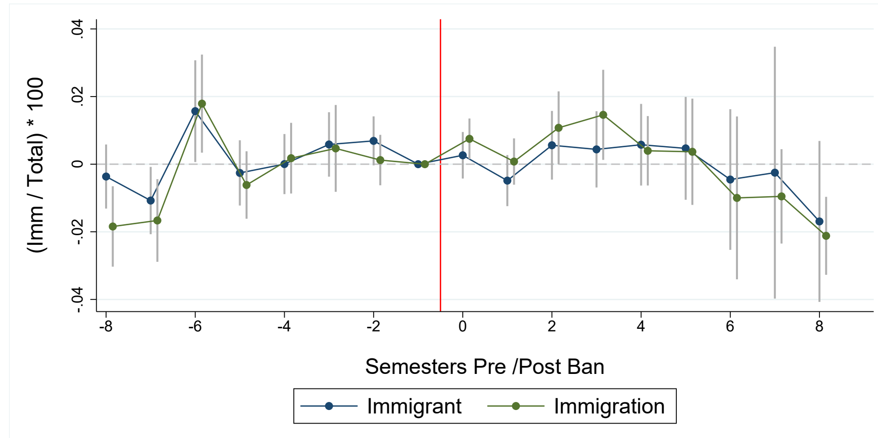
The graphs present coefficients and 95% confidence intervals on the interaction of AP-intensity and PostBan from regressions restricted to one of the 3 samples at a time, with the following dependent variables (standardized to facilitate comparison of the coefficients). **Panel (a):** frequency of "illegal immigrant" articles as percent of "immigrant" articles; **Panel (b):** frequency of "illegal immigrant" articles sourced from AP as percent of "immigrant" articles; **Panel (c):** frequency of "illegal immigrant" articles *not* sourced from AP as percent of "immigrant" articles. Each regression controls for outlet and year-month FEs and is weighted by number of "immigrant" articles. Standard errors clustered by outlet.

Figure 8: Other measures of immigration coverage over time

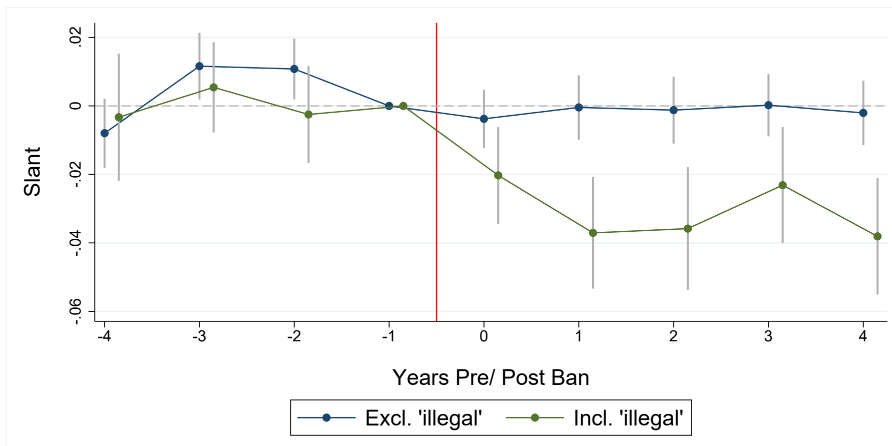
(a) Substitutes proposed by AP



(b) Volume of immigration coverage



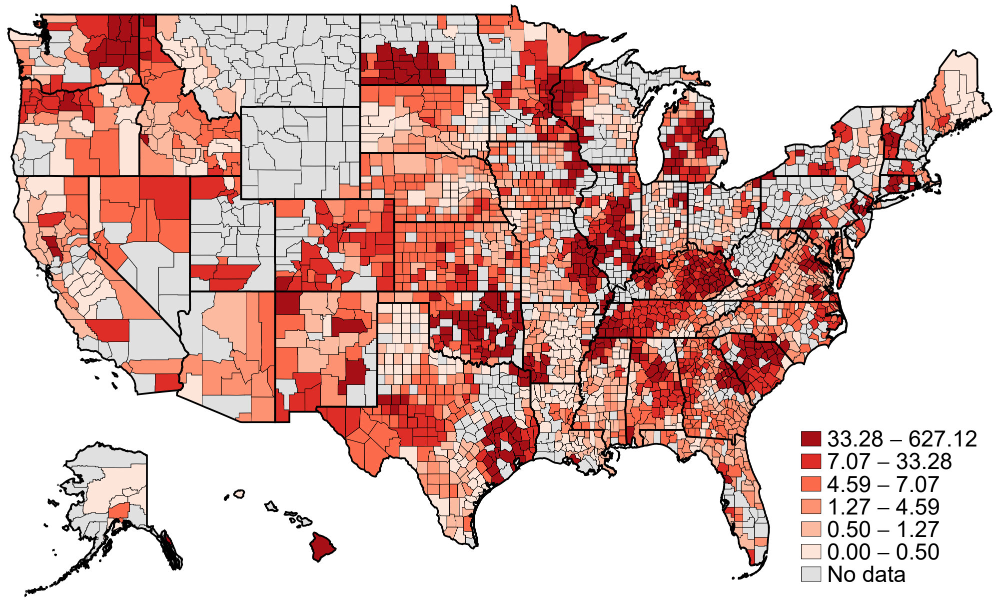
(c) Immigration slant



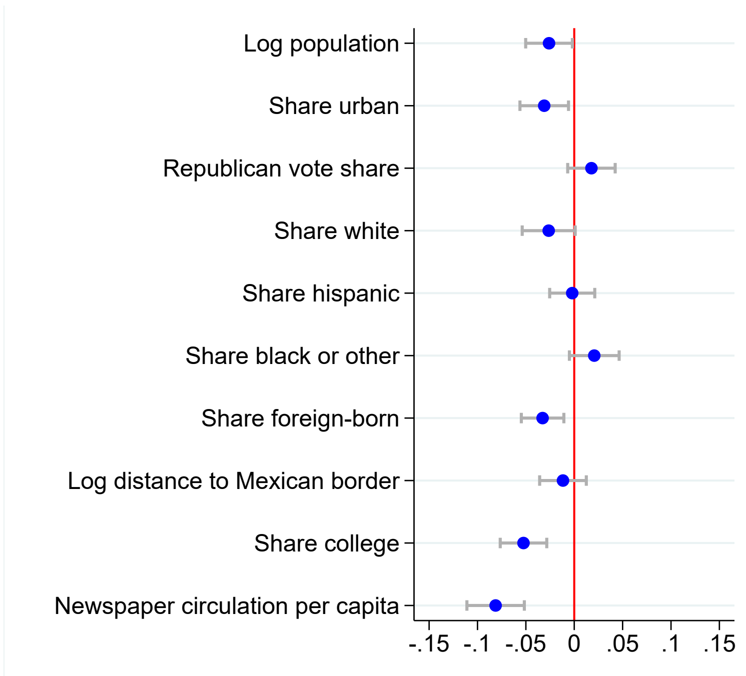
Notes: Coefficients and 95% confidence intervals on AP-intensity interacted with a full set of indicators for semester pre-/ post-ban. In **Panel (a)** the dependent variable is the number of articles mentioning the substitutes proposed by AP ("enter*/ live* in the country illegally/ without legal permission") as percent of "immigrant" articles. In **Panel (b)** the dependent variable is the number of articles mentioning the words "immigrant" or "immigration" as percent of total articles. In **Panel (c)** the dependent variable is the index of immigration slant computed including (green line) or excluding (blue line) the phrase "illegal immigrant" and its substitutes. All regressions control for outlet and year-month FEs. The omitted category is the semester before the ban. In Panel (a) the regression is weighted by number of "immigrant" articles and in Panel (b) by total articles. Standard errors clustered by outlet.

Figure 9: Geographic distribution of AP-intensity and county-level correlates

(a): Geographic distribution of AP-intensity

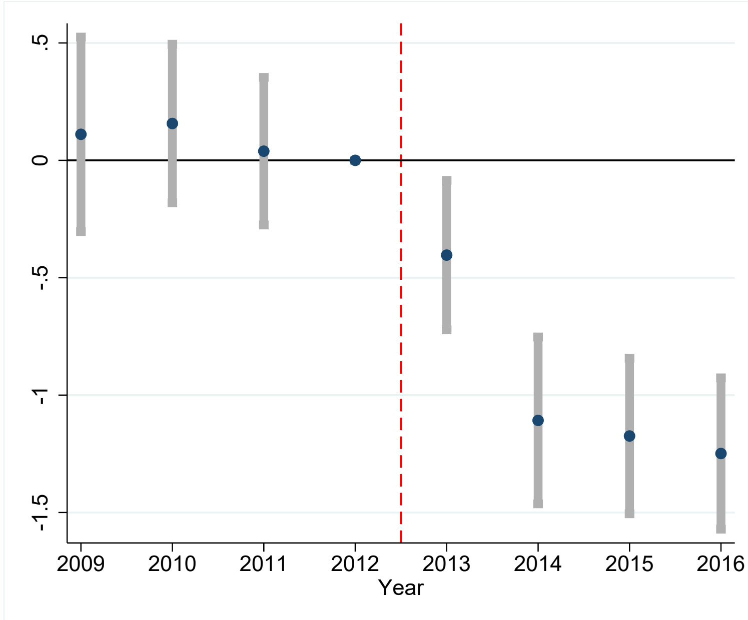


(b): County-level correlates of AP-intensity



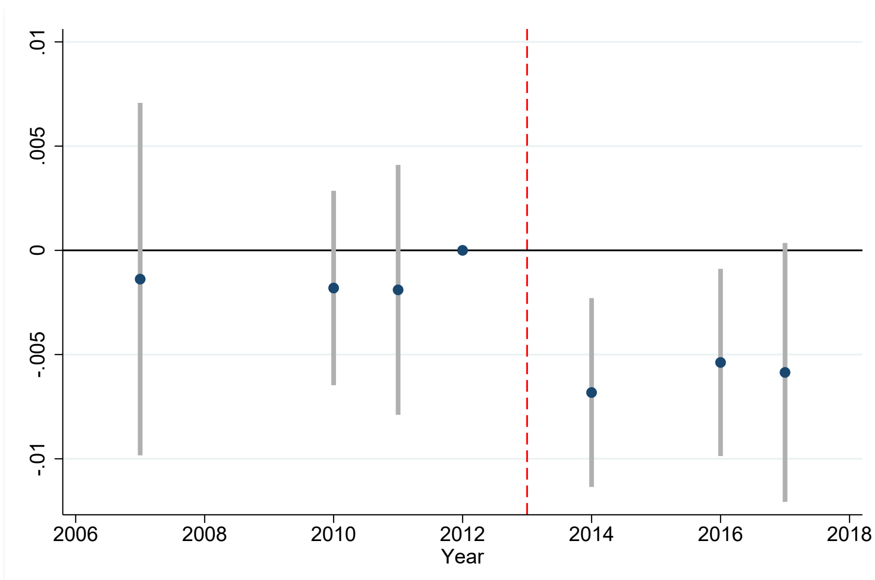
Notes: Panel (b): Coefficients and 95% confidence intervals from univariate regressions of each of the listed county characteristics on AP-intensity. All county characteristics are standardized to facilitate comparison of the magnitudes of the coefficients. Robust standard errors.

Figure 10: Diffusion over time: county × year level



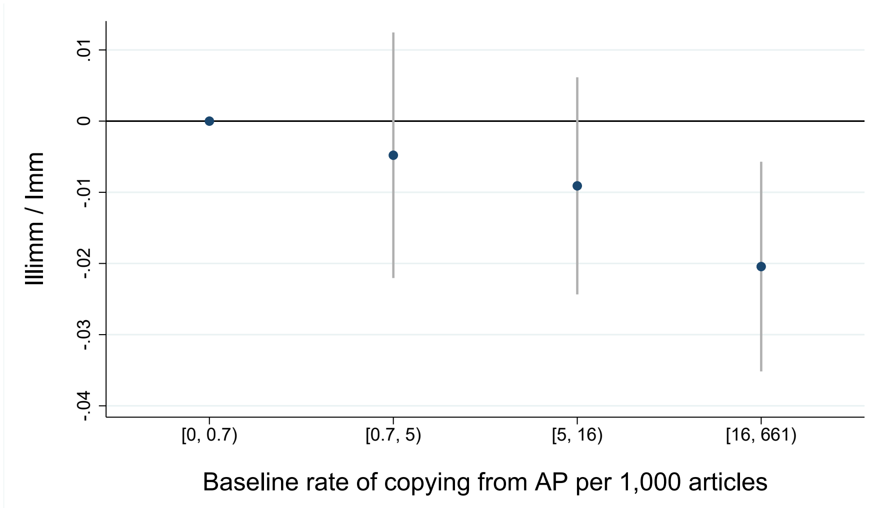
Notes: Point estimates and 95% confidence intervals on the interactions of AP-intensity with year, conditional on year and county FEs. Standard errors clustered by county.

Figure 11: Support for increasing border security: Reduced form effects over time



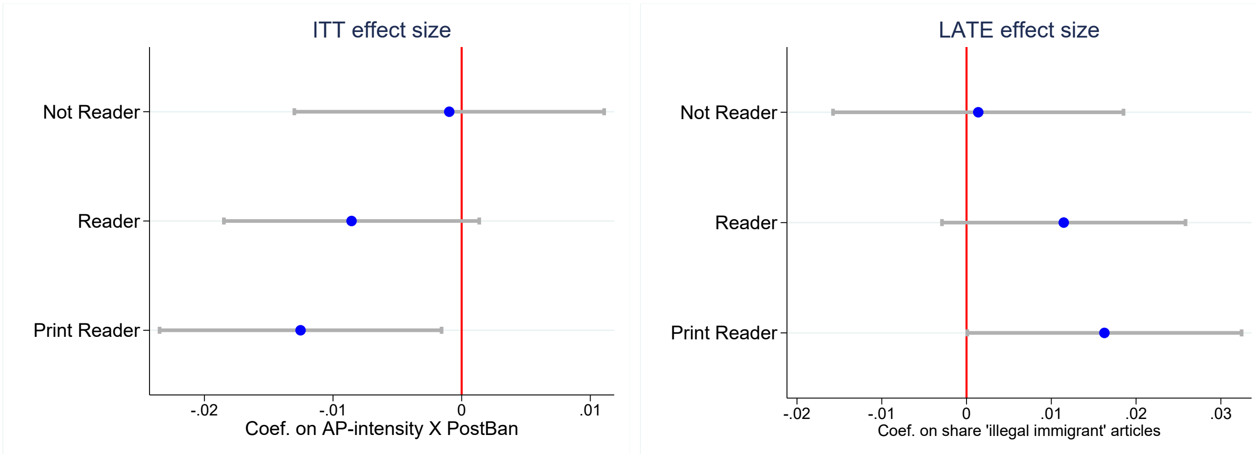
Notes: Point estimates and 95% confidence intervals on the interactions of AP-intensity with survey year, conditional on year and county FEs, respondent controls, and county controls interacted with year FEs. Respondent controls: age, age squared gender, indicators for race, college, and 1st or 2nd generation immigrant. County controls: log population, racial composition, share foreign born, share college degree, log income per capita, share urban, republican vote share (2012 pres. election) – 2012 levels interacted with year FEs. Standard errors clustered by county.

Figure 12: Support for increasing border security: Reduced form effect by quartile of AP-intensity



Notes: Point estimates and 95% confidence intervals on the interactions of AP-intensity with survey year, conditional on year and county FEs, respondent controls, and county controls interacted with year FEs. Respondent controls: age, age squared gender, indicators for race, college, and 1st or 2nd generation immigrant. County controls: log population, racial composition, share foreign born, share college degree, log income per capita, share urban, republican vote share (2012 pres. election) – 2012 levels interacted with year FEs. Standard errors clustered by county.

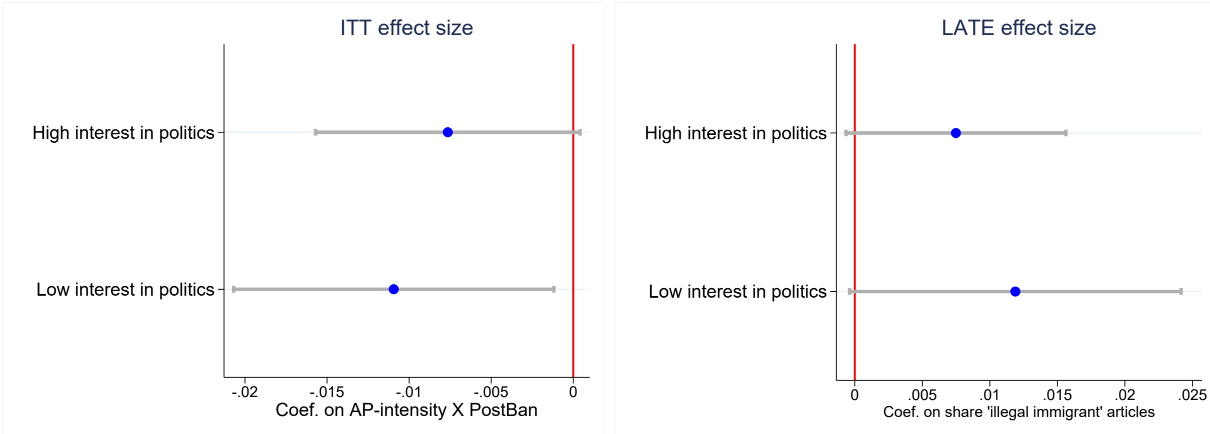
Figure 13: Heterogeneity by newspaper readership



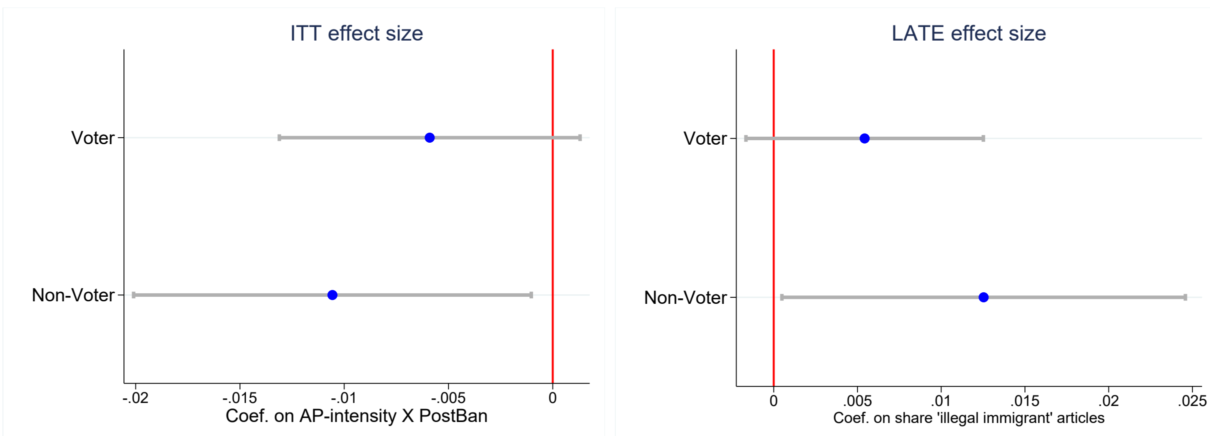
Notes: Each graph presents coefficients and 95% confidence intervals from a regression with support for increasing border security as dependent variable and sample restricted to counties with particular characteristics. In each case the dependent variable is standardized to facilitate comparison of the magnitudes.
Left hand side: Coefficients on the interaction of AP-intensity and PostBan (intention-to-treat). **Right hand side:** Coefficients on share 'illegal immigrant' articles (local average treatment effect).
All regressions control for respondent characteristics, county characteristics interacted with year FEs, county and survey year FEs. Standard errors clustered by county.

Figure 14: Heterogeneity by political interest and participation

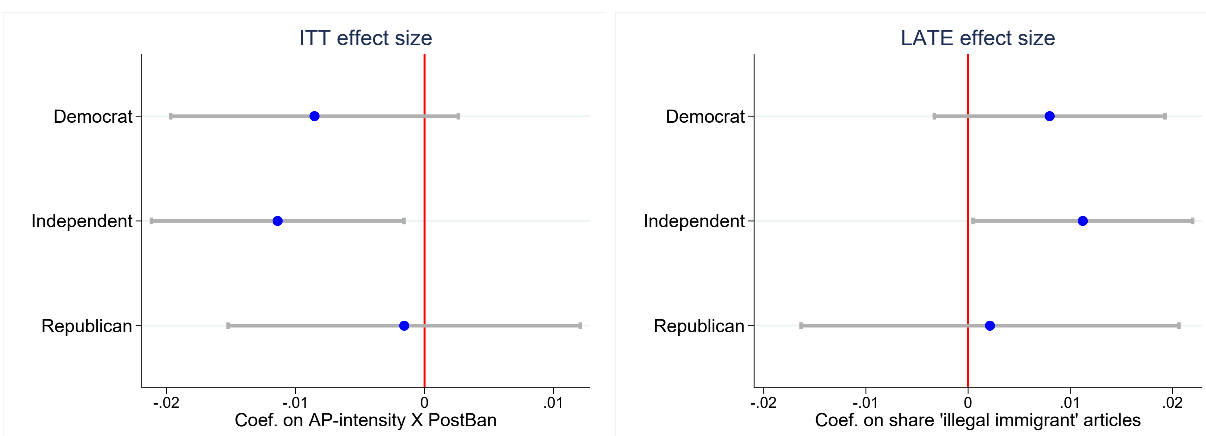
(a) Effects by political interest



(b) Effects by voting participation



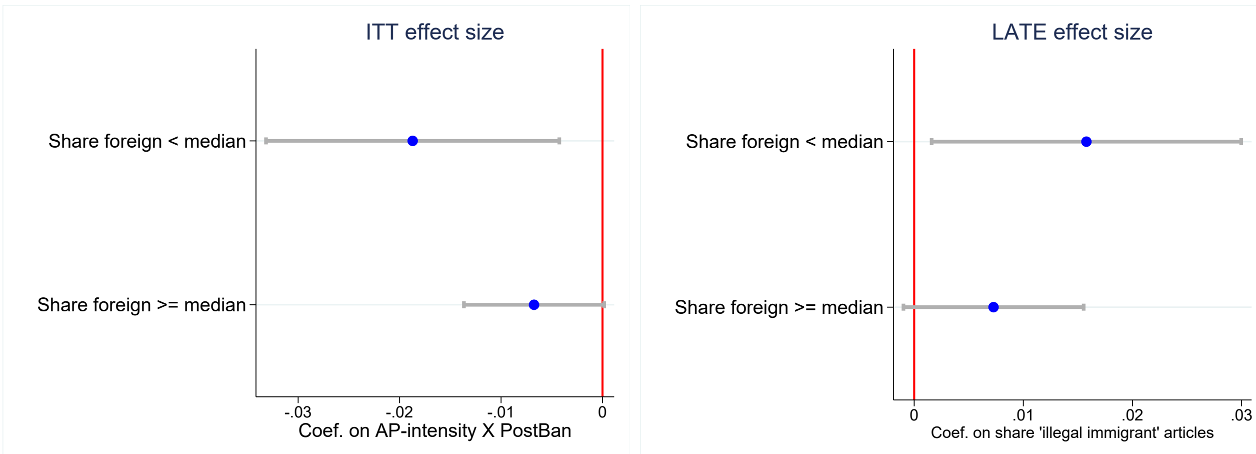
(c) Effects by political affiliation



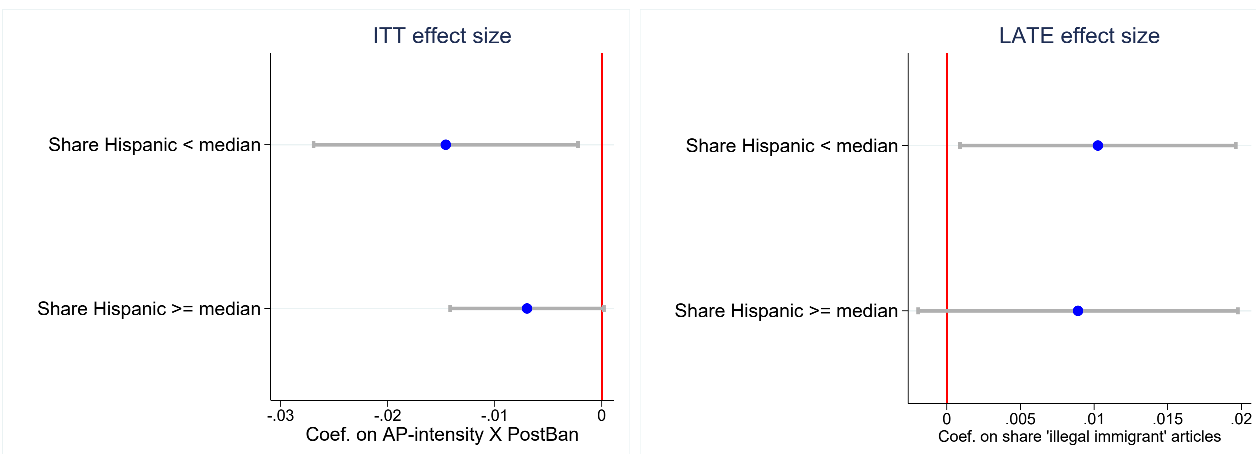
Notes: Each graph presents coefficients and 95% from a regression with support for increasing border security as dependent variable and sample restricted to respondents with particular characteristics. In each case the dependent variable is standardized for the purpose of comparison of the magnitudes.
Left hand side: Coefficients on the interaction of AP-intensity and PostBan (intention-to-treat). **Right hand side:** Coefficients on share 'illegal immigrant' articles (local average treatment effect).
All regressions control for respondent characteristics, county characteristics interacted with year FEs, county and survey year FEs. Standard errors clustered by county.

Figure 15: Heterogeneity by share foreign-born / share Hispanic

Effects by share foreign



Effects by share Hispanic



Notes: Each graph presents coefficients and 95% from a regression with support for increasing border security as dependent variable and sample restricted to counties with particular characteristics. In each case the dependent variable is standardized for the purpose of comparison of the magnitudes.
Left hand side: Coefficients on the interaction of AP-intensity and PostBan (intention-to-treat). **Right hand side:** Coefficients on share 'illegal immigrant' articles (local average treatment effect).
All regressions control for respondent characteristics, county characteristics interacted with year FEs, county and survey year FEs. Standard errors clustered by county.

7 Tables

Table 1: Diffusion of the ban depending on AP-intensity

	(1)	(2)	(3)	(4)	(5)
	'Illigal immigrant', pct. of 'Immigrant'				'Illegal immigration' pct. of 'Immigration'
PostBan × AP intensity	-1.490***	-1.462***	-1.426***	-1.737***	-0.976***
	(0.201)	(0.181)	(0.151)	(0.207)	(0.159)
AP intensity	1.716***				
	(0.215)				
PostBan	-12.497***				
	(0.757)				
Outlet FEs	No	Yes	Yes	Yes	Yes
Year-Month FEs	No	Yes	Yes	Yes	Yes
State × Year-Month FEs	No	No	Yes	Yes	No
Outlet-specific linear trend	No	No	No	Yes	No
Observations	133,349	133,347	133,329	133,329	106,412
Number of outlets	2271	2269	2269	2269	2150
R^2	0.15	0.42	0.49	0.53	0.34
Mean dep. var.	20.79	20.79	20.79	20.79	31.19

Notes: WLS weighted by number of number of "immigrant" articles in columns (1)-(4), and by number of "immigration" articles in column (5). Standard errors clustered by outlet.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Alternative measures of AP-intensity

	(1)	(2)	(3)	(4)
	'Illigal immigrant', pct. of 'Immigrant'			
PostBan × AP-intensity: only AP credited	-1.437***			
	(0.191)			
PostBan × AP-intensity: only AP plagiarised		-1.434***		
		(0.209)		
PostBan × AP-intensity: only AP credited, *all articles*			-1.318***	
			(0.201)	
PostBan × Reuters-intensity: only Reuters credited, *all articles*				0.280
				(0.362)
Outlet FEs	Yes	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes	Yes
Observations	133,347	133,347	123,261	129,344
Number of outlets	2269	2269	2218	2421
R^2	0.42	0.42	0.39	0.40
Mean dep. var.	20.79	20.79	21.39	21.16

Notes: Replication of column (3) of table 1 with the following alternative measures of AP-intensity. Column (1): share of "immigrant" articles credited to AP. Column (2): share of "immigrant" articles flagged by a plagiarism algorithm. Column (3): share of all articles published in the 12 months before the ban that are credited to AP. Column (4): share of all articles published in the 12 months before the ban that are credited to Reuters. Standard errors clustered by outlet.

Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Views on immigration policy: Reduced form

	Reduced Form				
	(1) Index Restict Imm.	(2) Border	(3) No Amnesty	(4) Don't hire	(5) Question
PostBan × AP-intensity	-0.0126***	-0.0045***	-0.0011	-0.0081***	-0.0045**
	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)
Respondent controls	Yes	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Observations	161,490	161,490	161,490	74,055	118,529
Number of counties	2,113	2,113	2,113	1,924	2,066
R^2	0.26	0.14	0.16	0.13	0.22
Mean dep. var.	0.01	0.56	0.52	0.62	0.41

Notes: Reduced form OLS regressions. Respondent controls: age, age squared, gender, race, college, 1st or 2nd generation immigrant, and political ideology. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Views on immigration policy: 2SLS

	2SLS				
	(1) Index Restict Imm.	(2) Border	(3) No Amnesty	(4) Don't hire	(5) Question
'Illegal imm.', pct. of 'Imm.'	0.0136**	0.0050**	0.0012	0.0073***	0.0056**
	(0.006)	(0.002)	(0.002)	(0.003)	(0.003)
Respondent controls	Yes	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
First-Stage F stat.	23.63	23.63	23.63	20.48	12.77
First-Stage coef. on PostBan × AP-intensity	-0.9223***	-0.9223***	-0.9223***	-1.1043***	-0.8028***
	(0.190)	(0.190)	(0.190)	(0.244)	(0.225)
Observations	161,490	161,490	161,490	74,055	118,529
Number of counties	2,113	2,113	2,113	1,924	2,066
R^2	0.22	0.10	0.12	0.09	0.16
Mean dep. var.	0.01	0.56	0.52	0.62	0.41

Notes: 2SLS regressions (upper panel), along with the corresponding 1st-stage coefficients (lower panel). Respondent controls: age, age squared, gender, race, college, 1st or 2nd generation immigrant, and political ideology. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Support for increasing border control

	Reduced Form				2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	"Increase the number of border patrols on the US-Mexican border.": Selected						
PostBan × AP-intensity	-0.0044***	-0.0049***	-0.0046***	-0.0046**			
	(0.002)	(0.002)	(0.002)	(0.002)			
AP intensity	0.0047***						
	(0.002)						
PostBan	-0.0186***						
	(0.006)						
'Illegal imm.', pct. of 'Imm.'					0.0055**	0.0050**	0.0065**
					(0.002)	(0.002)	(0.003)
Respondent controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs × County controls	No	No	Yes	Yes	No	Yes	Yes
County FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Year × State FEs	No	No	No	Yes	No	No	Yes
First-Stage F stat.	.	.	.	.	9.90	23.63	12.19
First stage coef on PostBan × AP-intensity					-0.8859***	-0.9223***	-0.7104***
					(0.282)	(0.190)	(0.204)
Observations	162,057	161,943	161,490	161,490	161,943	161,490	161,490
Number of counties	2,236	2,122	2,113	2,113	2,122	2,113	2,113
R^2	0.12	0.14	0.14	0.14	0.10	0.10	0.10
Mean dep. var.	0.56	0.56	0.56	0.56	0.56	0.56	0.56

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent controls: age, age squared, gender, race, college, 1st or 2nd generation immigrant, and political ideology. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Support for increasing border control: Alternative measures of AP-intensity

	Reduced Form				2SLS		
	(1) Border	(2) Border	(3) Border	(4) Border	(5) Border	(6) Border	(7) Border
PostBan × AP-intensity: AP credited	-0.0047*** (0.001)						
PostBan × AP-intensity: Plagiarism detection		-0.0037** (0.002)					
PostBan × AP-intensity: AP credited, *all articles*			-0.0028** (0.001)				
PostBan × Reuters-intensity: Reuters credited, *all articles*				-0.0006 (0.003)			
'Illegal imm.', pct. of 'Imm.'					0.0057*** (0.002)	0.0047* (0.002)	0.0050** (0.002)
Respondent controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage F stat.	.	.	.	.	20.11	16.69	14.86
PostBan × AP-intensity: AP credited					-0.8255*** (0.184)		
PostBan × AP-intensity: Plagiarism detection						-0.8004*** (0.196)	
PostBan × AP-intensity: AP credited, *all articles*							-0.5729*** (0.149)
Observations	161490	161490	148271	149681	161490	161490	148271
Number of counties	2113	2113	1767	1789	2113	2113	1767
R^2	0.14	0.14	0.14	0.14	0.10	0.10	0.10
Mean dep. var.	0.56	0.56	0.55	0.55	0.56	0.56	0.55

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent and county controls as in Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Support for increasing border control: Alternative thresholds for share of county circulation covered by content data

	Reduced form				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	=1	≥0.75	≥0.50	> 0	=1	≥0.75	≥0.50	> 0
PostBan × AP-intensity	-0.0066***	-0.0042***	-0.0039***	-0.0036**				
	(0.002)	(0.002)	(0.001)	(0.001)				
'Illegal imm.', pct. of 'Imm.'					0.0062***	0.0054**	0.0039**	0.0035**
					(0.002)	(0.002)	(0.002)	(0.001)
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.	.	.	.	.	25.86	16.16	24.73	30.98
First stage coef. on PostBan × AP-intensity					-1.0660***	-0.7843***	-0.9845***	-1.0354***
					(0.210)	(0.195)	(0.198)	(0.186)
Observations	98,993	178,584	202,574	240,638	98,993	178,584	202,574	240,638
Number of counties	1,685	2,209	2,320	2,834	1,685	2,209	2,320	2,834
R^2	0.15	0.14	0.14	0.14	0.10	0.10	0.10	0.10
Mean dep. var.	0.56	0.55	0.55	0.55	0.56	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. Respondent and county controls as in Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Views on other policies

	Reduced Form				2SLS			
	(1) Taxes	(2) Economy	(3) Abortion	(4) Gay marriage	(5) Taxes	(6) Economy	(7) Abortion	(8) Gay marriage
PostBan × AP-intensity	0.0002	0.0012	-0.0015	-0.0024				
	(0.001)	(0.001)	(0.002)	(0.002)				
'Illegal imm.', pct. of 'Imm.'					-0.0002	-0.0013	0.0016	0.0027
					(0.002)	(0.002)	(0.002)	(0.002)
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.	.	.	.	.	23.47	23.38	23.77	23.51
First-Stage coef. on PostBan × AP-intensity					-0.9196***	-0.9185***	-0.9245***	-0.9209***
					(0.190)	(0.190)	(0.190)	(0.190)
Observations	158,737	157,848	160,631	160,265	158,737	157,848	160,631	160,265
Number of counties	2,108	2,109	2,110	2,112	2,108	2,109	2,110	2,112
R^2	0.08	0.23	0.20	0.22	0.05	0.16	0.13	0.17
Mean dep. var.	0.45	0.42	0.47	0.42	0.45	0.42	0.47	0.42

Notes: "Taxes"= 1 if would rather cut public spending than increase taxes. "Economy"= 1 if believe the economy has gotten worse over the past year. "Abortion"= 1 if oppose always allowing women to have an abortion as matter of choice. "Gay marriage"= 1 if oppose gay marriage. Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent controls: age, age squared, gender, race, college, 1st or 2nd generation immigrant, and political ideology. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Voting Intentions

	Reduced Form				2SLS			
	(1) President	(2) Senate	(3) House	(4) Obama Disapprove	(5) President	(6) Senate	(7) House	(8) Obama Disapprove
PostBan × AP-intensity	-0.0026	0.0025	0.0031	-0.0028**				
	(0.002)	(0.002)	(0.002)	(0.001)				
'Illegal imm.', pct. of 'Imm.'					0.0019	-0.0039	-0.0033	0.0030**
					(0.002)	(0.004)	(0.002)	(0.001)
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.	.	.	.	.	32.04	8.66	24.07	23.83
Observations	76,800	97,803	143,835	156,428	76,800	97,803	143,835	156,428
Number of counties	1,931	1,993	2,093	2,109	1,931	1,993	2,093	2,109
R^2	0.47	0.40	0.38	0.48	0.42	0.35	0.32	0.43
Mean dep. var.	0.35	0.39	0.36	0.51	0.35	0.39	0.36	0.51

Notes: Intent to vote for Republican candidate in Presidential, House and Senate elections, and disapproval of President Obama. Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent controls: age, age squared, gender, race, college, 1st or 2nd generation immigrant, and political ideology. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share. Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Electoral results

	Reduced Form				2SLS			
	(1) Rep. Share President	(2) Turnout President	(3) Rep. Share House	(4) Turnout House	(5) Rep. Share President	(6) Turnout President	(7) Rep. Share House	(8) Turnout House
PostBan × AP-intensity	0.0008	0.0003	-0.0019	0.0010				
	(0.001)	(0.000)	(0.002)	(0.001)				
'Illegal imm.', pct. of 'Imm.'					-0.0006	-0.0002	0.0021	-0.0011
					(0.001)	(0.000)	(0.002)	(0.001)
Respondent controls	No	No	No	No	No	No	No	No
Year FEs × County controls	No	No	No	No	No	No	No	No
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage F stat.	.	.	.	.	66.18	66.18	25.26	26.05
Observations	4698	4698	4662	4702	4698	4698	4662	4702
Number of counties	2349	2349	2331	2351	2349	2349	2331	2351
R^2	0.98	0.98	0.86	0.91	-0.04	-0.01	-0.05	-0.06
Mean dep. var.	0.64	0.57	0.65	0.39	0.64	0.57	0.65	0.39

Notes: Republican vote shares and turnout in presidential elections (2012 and 2016) and in House midterm elections (2010 and 2014). Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share.
Standard errors clustered by county.. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Contents (Appendix)

Appendices

A Background

A.1 AP Style Guide entry on “illegal immigrant”

Pre-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*.

Pre-Ban

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.
Specify wherever possible how someone entered the country illegally and from where. Crossed the border? Overstayed a visa? What nationality?
People who were brought into the county as children should not be described as having immigrated illegally. For people guaranteed a temporary right to remain in the U.S. under the Deferred Action for Childhood Arrivals program, use *temporary resident status*, with details on the program lower in the story.

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

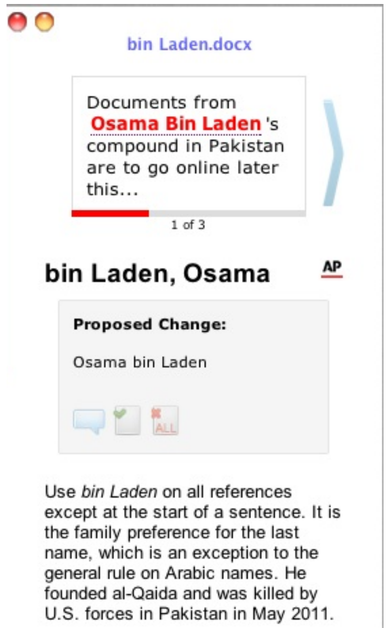


Figure A2: The ban reported in the Atlantic

Atlantic Popular Latest Se

The AP's Ban on 'Illegal Immigrant' Will Change How We Talk About Immigration

That faint sound you hear is Senate reporters from the AP, *The New York Times*, and beyond smacking their delete keys, rethinking their agenda setting aloud, and figuring out how we talk now, amidst a serious legislative discussion

ALEXANDER ABAD-SANTOS | APR 2, 2013 | POLITICS

A.2 Examples of AP dispatches before and after the ban

Pre-Ban

Senate panel OKs letting non-citizens, including illegal immigrants, get driver's licenses

18-Mar-2013

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.

Pre-Ban

Immigrant driver's license bill takes step forward in Oregon Senate committee work

16-Apr-2013

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.

B Data

B.1 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".[34] 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 B1 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 substitutes "country illegally" / "border illegally".

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

[34] I focus on the pre-ban period in light of the result that the ban may have affected the language used in Congressional speech. On the other hand, this has the disadvantage of possibly omitting phrases that emerged after 2013. I obtain similar results taking Congressional speech for the entire sample period.

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 newspaper and year I follow exactly 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]

Table B1: Most partisan phrases in Congressional speech related to 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

Notes: Top 30 Republican and Democrat phrases, ranked by χ^2.

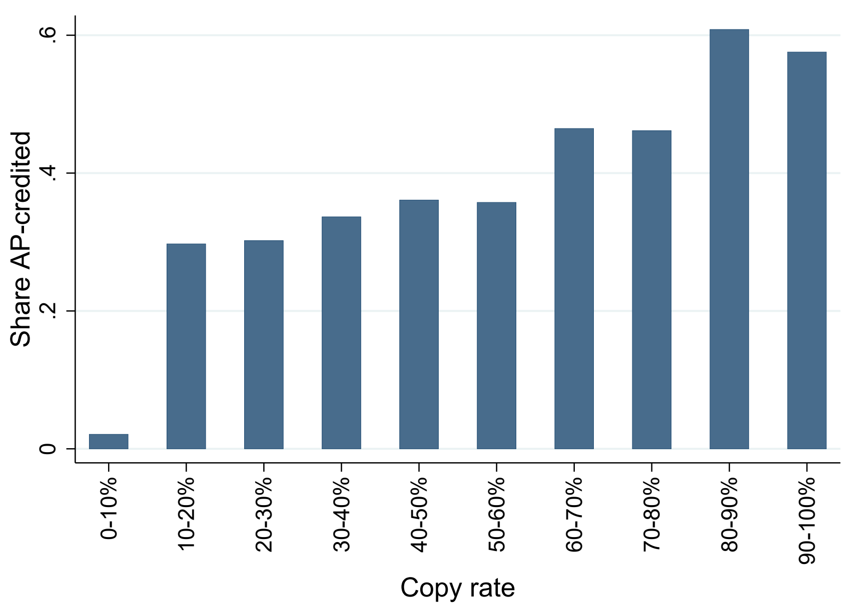
[35]For some newspaper-years none of the selected phrases is contained in the news article's text – in this case the slant index is set to missing.

B.2 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 AP if the maximum text overlap exceeds 20% (equivalent to 70 characters, relative to the mean text length of 350).

Figure B1: AP-copy rate: Attribution and plagiarism



[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.

Figure B1 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 attribution to AP alone would have missed a substantial volume of copied articles. When collapsed at the media outlet level however, the correlation between the two measures is 0.83.

B.3 Immigration questions in the CCES

What do you think the U.S. government should do about immigration? Select all that apply.

- Fine US businesses that hire illegal immigrants.
  (-07, -12, -14, -17)
- 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.
  (-07, -10, -11, -12, -14, -16, -17)
- Increase the number of border patrol on the US-Mexican border.
  (-07, -10, -11, -12, -14, -16, -17)
- Build a wall between the US and Mexico.
  (-07, -17)
- Allow police to question anyone they think may be in the country illegally.
  (-10, -11, -12, -14, -17)
- Prohibit illegal immigrants from using emergency hospital care and public schools.
  (-12)
- Deny automatic citizenship to American-born children of illegal immigrants.
  (-12)
- Identify and deport illegal immigrants.
  (-14, -16, -17)
- Grant legal status to people who were brought to the US illegally as children, but who have graduated from a U.S. high school.
  (-16)

B.4 Descriptive statistics

Figure B2: Histogram of AP-intensity, in # articles per 1000

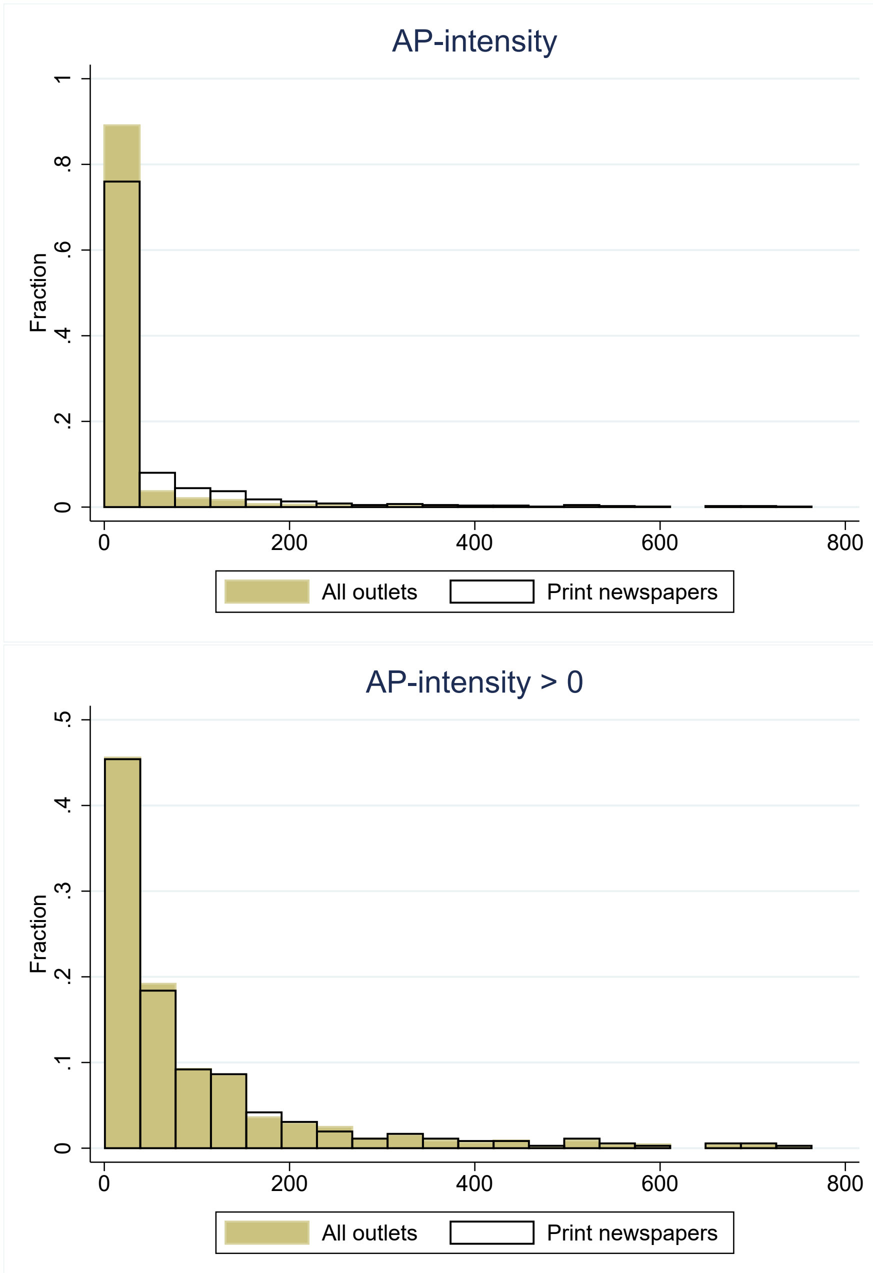
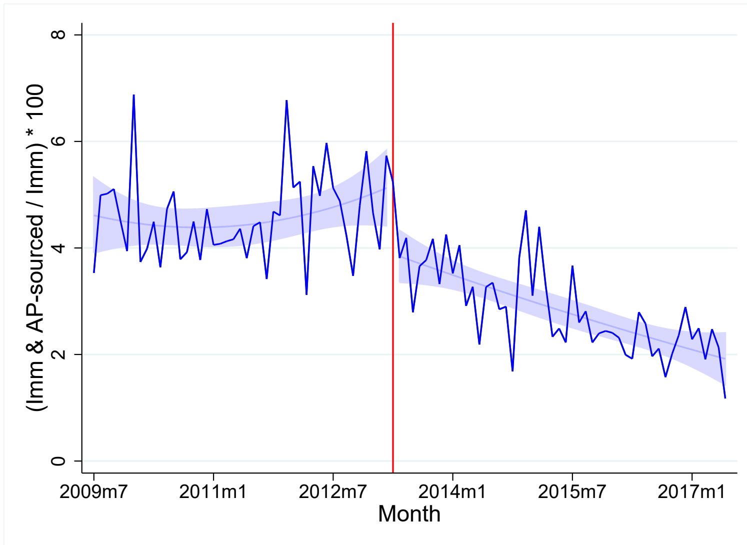
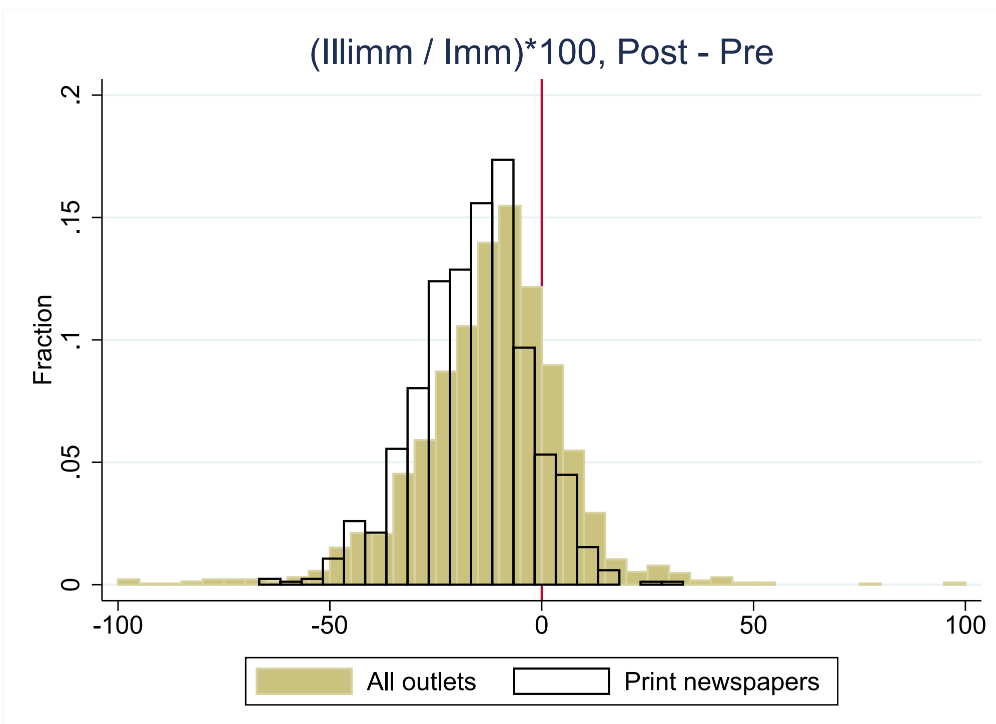


Figure B3: Percent of immigrant articles credited to AP over time



Notes: Monthly number of "immigrant" articles credited to AP, as percent of all "immigrant" articles.

Figure B4: Histogram of the difference between post-ban and pre-ban share of "illegal immigrant" articles by outlet



Notes:

Table B2: Summary statistics: Main sample (all media outlets)

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Immigrant/ Total articles (pct)	0.809	2.179	0	100	232,729
Illigal immigrant / Immigrant articles (pct)	18.584	28.628	0	100	143,696
Illigal immigrant / Immigrant articles (pct) – Pre-Ban	24.761	32.017	0	100	64,561
Illigal immigrant / Immigrant articles (pct) – Post-Ban	13.546	24.4	0	100	79,135
Immigrant & AP-sourced / Immigrant (pct)	2.115	9.918	0	100	143,821
1[AP-intensity > 0]	0.195	0.397	0	1	216,709
AP-intensity (asinh)	0.886	1.881	0	7.331	216,709
AP intensity, plagiarised (asinh)	0.558	1.541	0	7.331	216,709
AP intensity, credited (asinh)	0.668	1.62	0	6.908	216,709
AP-intensity, credited – all articles (asinh)	0.875	1.856	0	7.399	213,984
Reuters-intensity, credited – all articles (asinh)	0.359	0.991	0	7.154	239,328

Table B3: Summary statistics: Restricted sample of print newspapers

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Immigrant/ Total articles (pct)	0.61	0.984	0	100	73,041
Illigal immigrant / Immigrant articles (pct)	22.499	27.418	0	100	65,593
Illigal immigrant / Immigrant articles (pct) – Pre-Ban	30.261	30.423	0	100	29,474
Illigal immigrant / Immigrant articles (pct) – Post-Ban	16.165	22.818	0	100	36,119
Immigrant & AP-sourced / Immigrant (pct)	3.725	12.595	0	100	65,666
1[AP-intensity > 0]	0.431	0.495	0	1	77,204
AP-intensity (asinh)	1.968	2.407	0	7.243	77,204
AP intensity, plagiarised (asinh)	1.192	2.083	0	7.140	77,204
AP intensity, credited (asinh)	1.584	2.193	0	6.908	77,204
AP-int, credited – all articles (asinh)	2	2.37	0	7.399	71,040
Reuters-int, credited – all articles (asinh)	0.455	0.942	0	6.568	73,248

Table B4: Summary statistics: CCES sample (respondent-level)

Variable	Mean	Std. Dev.	Min.	Max.	N
Restrict Imm. Index	0.016	0.729	-1.032	1.112	186,252
Border	0.548	0.498	0	1	186,252
Amnesty	0.524	0.499	0	1	186,252
Question	0.399	0.49	0	1	137,569
Don't hire	0.611	0.488	0	1	92,492
Illigal immigrant / Immigrant articles (pct)	21.303	12.265	0	96.727	180,749
Illigal immigrant / Immigrant articles (pct) – Pre-Ban	29.388	11.245	0	96.439	87,562
Illigal immigrant / Immigrant articles (pct) – Post-Ban	13.707	7.337	0	96.727	93,187
AP-intensity (asinh)	3.062	1.538	0	7.232	187,380
AP-int, credited (asinh)	2.303	1.703	0	7.128	187,380
AP-int, plagiarised (asinh)	2.51	1.498	0	6.814	187,380
AP-int, credited – all articles (asinh)	2.431	2.153	0	7.339	172,007
Reuters-int, credited – all articles (asinh)	0.917	1.193	0	5.916	173,726

Table B5: Summary statistics: CCES county-level sample

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Restrict Imm. Index	0.133	0.463	-1.032	1.112	12,057
Border	0.588	0.313	0	1	12,057
Amnesty	0.59	0.314	0	1	12,057
Question	0.465	0.32	0	1	8,936
Don't hire	0.65	0.311	0	1	6,537
Illigal immigrant / Immigrant articles (pct)	23.388	13.532	0	96.727	10,919
Illigal immigrant / Immigrant articles (pct) – Pre-Ban	31.664	12.352	0	96.439	5,511
Illigal immigrant / Immigrant articles (pct) – Post-Ban	14.956	8.565	0	96.727	5,408
AP-intensity (asinh)	3.185	1.612	0	7.232	12,112
AP-int, credited (asinh)	2.287	1.766	0	7.128	12,112
AP-int, plagiarised (asinh)	2.717	1.565	0	6.814	12,112
AP-int, credited – all articles (asinh)	2.149	2.261	0	7.339	10,347
Reuters-int, credited – all articles (asinh)	0.589	0.902	0	5.916	10,495

C Additional Results

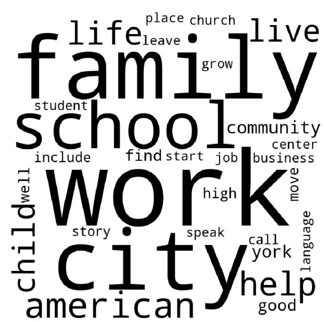
C.1 Text analysis of AP dispatches

Figure C1: Topics of "immigrant" AP dispatches

Topic 1 "Enforcement"

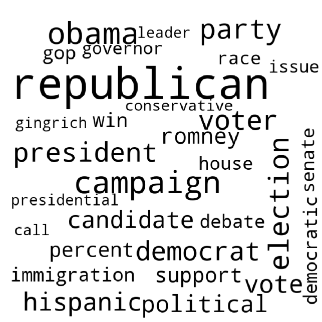
Topic 2 "Legislation"

Topic 3 "Integration"



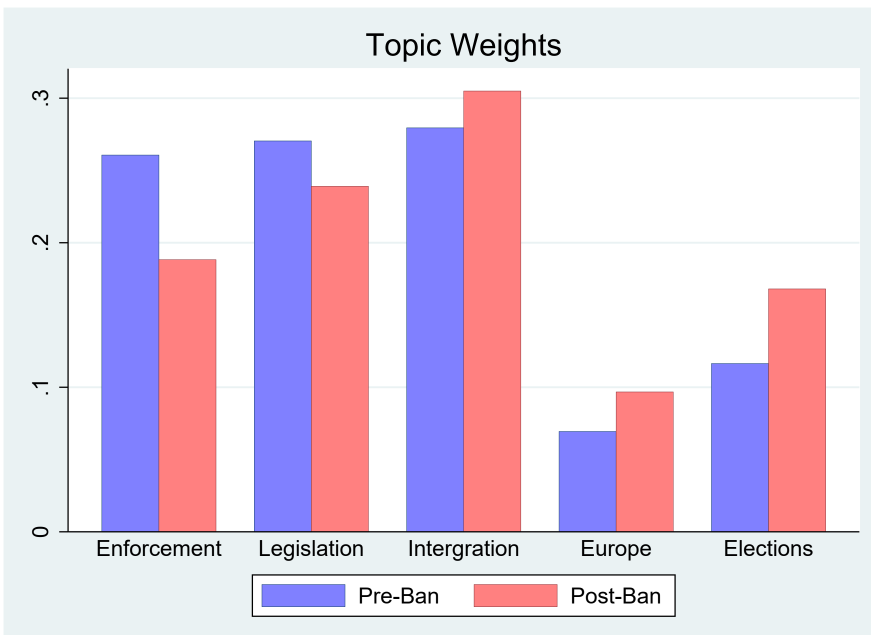
Topic 4 "Europe"

Topic 5 "Elections"



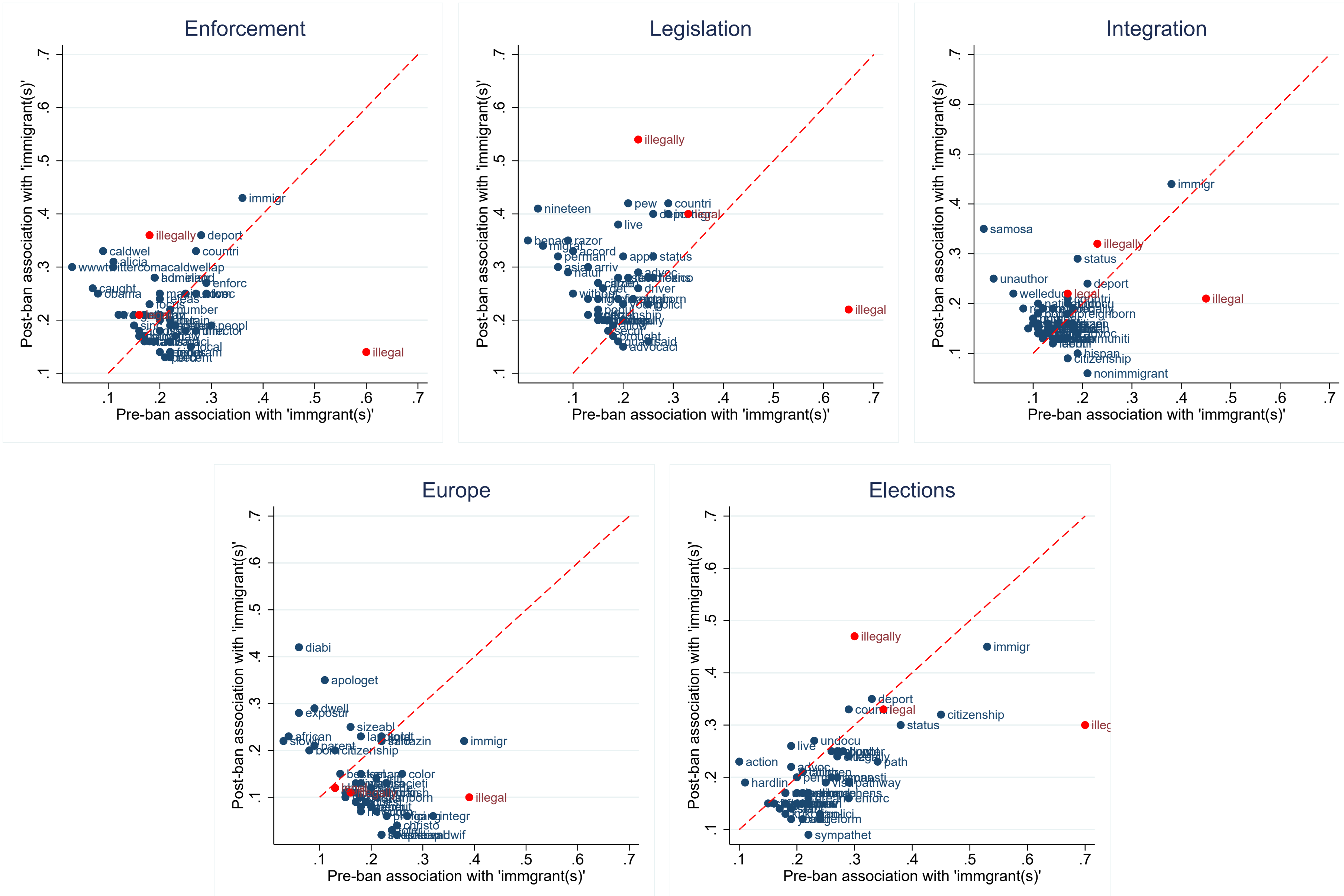
Notes: Word-clouds describing the 5 topics obtained with a Latent Dirichlet allocation (LDA) model applied to the corpus of AP dispatches mentioning the word "immigrant". The corpus excludes the phrase "illegal immigrant" and its synonyms. The number of topics is chosen for interpretability.

Figure C2: Topics distribution pre- and post-ban



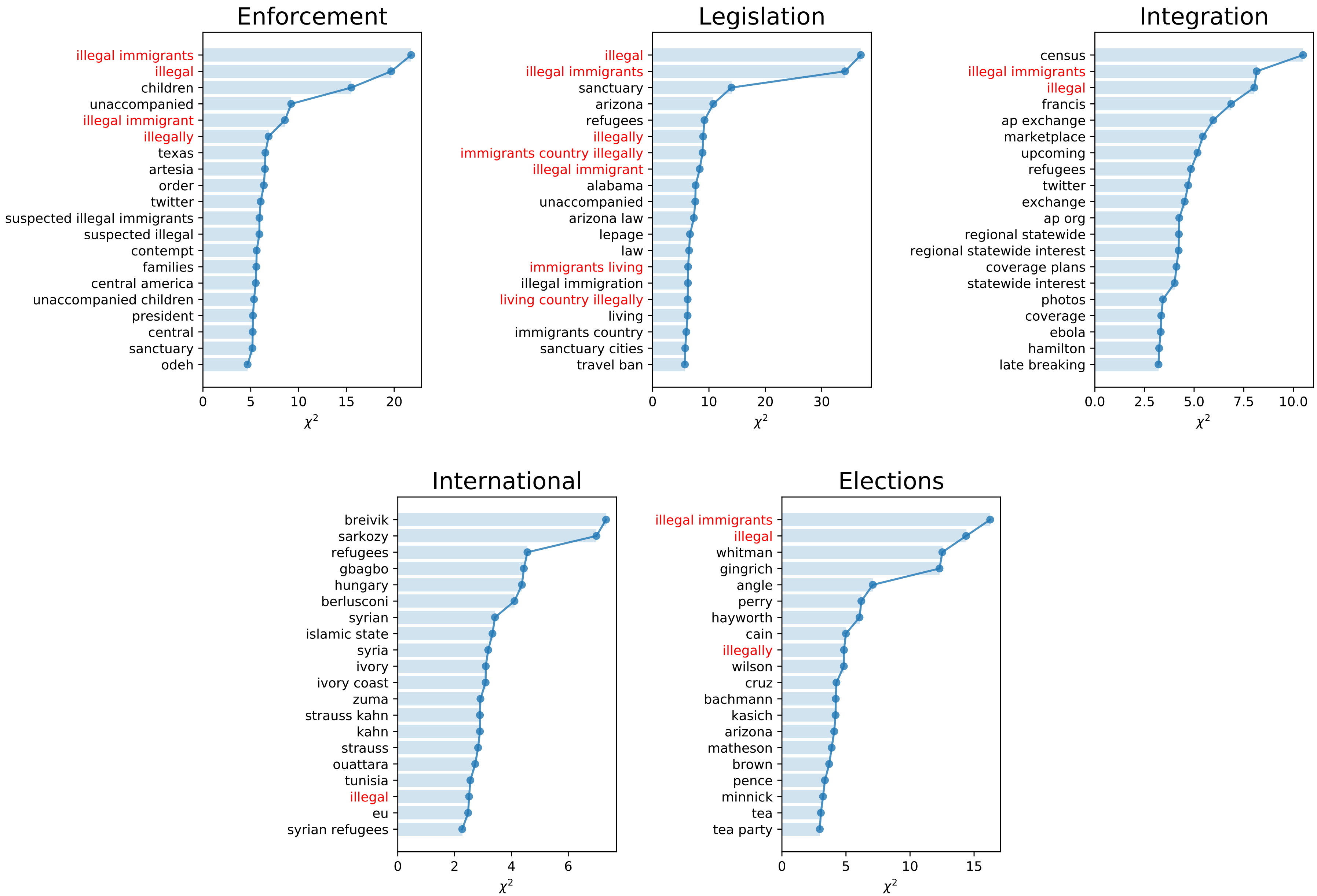
Notes: Distribution of topic weights before vs after the ban.

Figure C3: Correlates of the word “immigrant” before and after the ban; Estimated separately 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.

Figure C4



Notes:

C.2 Diffusion

Figure C5: Share of "illegal immigrant" articles in AP-intensive vs non AP-intensive outlets

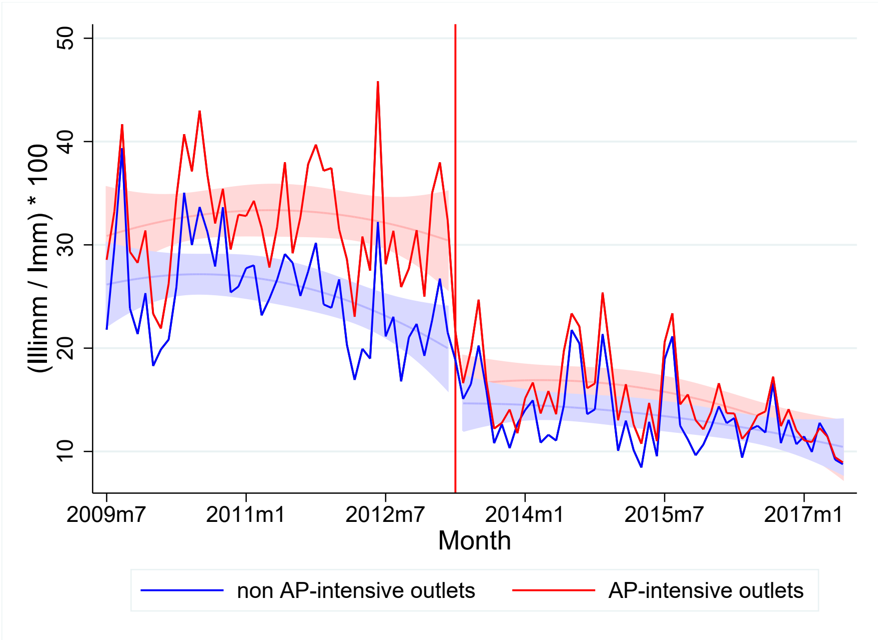


Table C1: Alternative specifications

	Not normalized	Unweighted	Word-count	Headlines	AP dummy	Elasticity
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(1 + 'Illegal Immigrant')	'Illigal immigrant', pct. of 'Immigrant'				
PostBan × AP intensity	-0.058***	-1.607***	-1.755***	-1.071***		
	(0.005)	(0.124)	(0.209)	(0.285)		
PostBan × I[AP-int > 0]					-5.541***	
					(1.059)	
$(Illimm/Imm)_{AP}$ × AP intensity						0.121***
						(0.022)
Outlet FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	216,709	133,347	124,232	18,976	133,347	131,920
Number of outlets	2271	2269	2160	1414	2269	2269
R^2	0.56	0.21	0.36	0.24	0.42	0.42
Mean dep. var.	0.34	19.52	19.17	14.46	19.52	20.93

Notes: Replication of column (3) of table 1 with the following modifications: (1) Replacing the dependent variable with the log of 1 + number of "illegal immigrant" articles and dropping weights; (2) Regression without weights; (3) Replacing number of articles with word-count; (4) Replacing articles with number of headlines; (5) Replacing continuous AP-intensity with a dummy for positive AP-intensity; (6) Replacing *PostBan* with the time-series of "illegal immigrant" articles (normalized by "immigrant" articles) released monthly by AP. Standard errors clustered by outlet.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: AP-sourced vs. original articles

	(1) AP-credited	(2) AP-plagiarised	(3) not AP-sourced
PostBan × AP intensity	-1.029***	-0.175***	-0.364**
	(0.124)	(0.021)	(0.185)
Outlet FEs	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes
Observations	133,469	133,469	133,404
Number of outlets	2269	2269	2269
R^2	0.42	0.10	0.44
Mean dep. var.	0.77	0.22	16.31

Notes: WLS weighted by number of number of "immigrant" articles. Standard errors clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Heterogeneity by ideology

	Slant < p33 (Dem)	p33 ≤ Slant < p66 (Indep)	Slant ≥ p66 (Rep)
	(1)	(2)	(3)
	'Illegal immigrant', pct. of 'Immigrant'		
PostBan × AP intensity	-1.817***	-0.800*	-1.258**
	(0.443)	(0.482)	(0.491)
Outlet FEs	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes
Observations	9,705	9,886	9,608
Number of outlets	106	110	113
R^2	0.55	0.48	0.50
Mean dep. var.	17.55	22.41	25.22

Notes: WLS weighted by number of "immigrant" articles. Standard errors clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C4: Heterogeneity by ideology: AP-sourced vs original articles

	Slant < p33 (Dem)	p33 ≤ Slant < p66 (Indep)	Slant ≥ p66 (Rep)
	(1)	(2)	(3)
	'Illegal immigrant' & AP-sourced, pct. of 'Immigrant'		
PostBan × AP intensity	-1.140***	-1.773***	-1.939***
	(0.178)	(0.332)	(0.447)
Outlet FEs	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes
Observations	9,718	9,894	9,619
Number of outlets	106	110	113
R^2	0.47	0.52	0.48
Mean dep. var.	0.98	2.35	1.67

	Slant < p33 (Dem)	p33 ≤ Slant < p66 (Indep)	Slant ≥ p66 (Rep)
	(1)	(2)	(3)
	'Illegal immigrant' & not AP-sourced, pct. of 'Immigrant'		
PostBan × AP intensity	-0.825**	0.579	0.484
	(0.393)	(0.432)	(0.473)
Outlet FEs	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes
Observations	9,711	9,890	9,612
Number of outlets	106	110	113
R^2	0.51	0.41	0.49
Mean dep. var.	15.35	18.95	22.62

Notes: WLS weighted by number of "immigrant" articles. Standard errors clustered by outlet. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

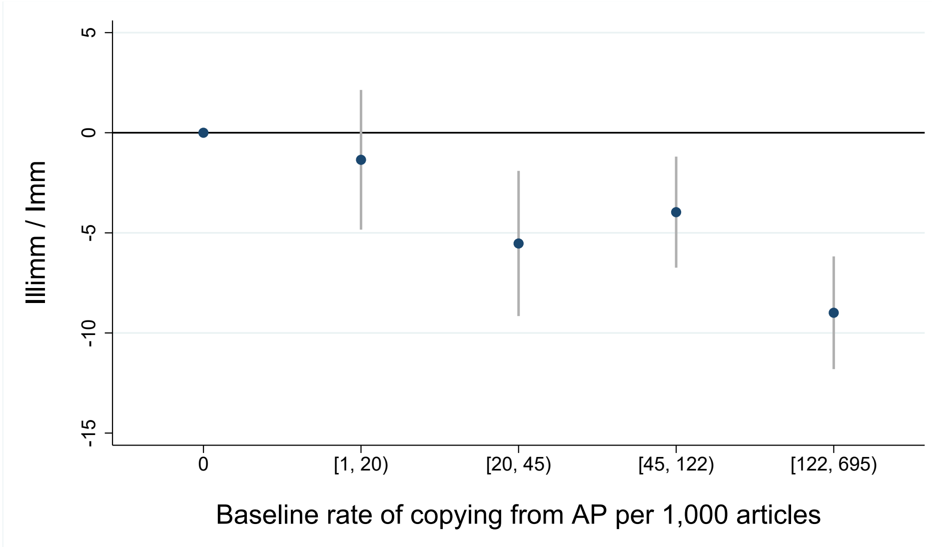
Table C5: Synonyms of "illegal immigrant" and volume of immigration coverage

	(1) AP-approved synonyms pct. of 'Immigrant'	(2) 'Undocumented immigrant' pct. of 'Immigrant'	(3) 'Immigrant' pct. of total articles	(4) 'Immigration' pct. of total articles
PostBan × AP intensity	0.317***	0.001	-0.002	0.002
	(0.061)	(0.126)	(0.006)	(0.005)
Outlet FEs	Yes	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes	Yes
Observations	133,188	133,330	204,175	204,180
Number of outlets	2269	2269	2162	2162
R^2	0.20	0.34	0.55	0.48
Mean dep. var.	5.06	8.74	0.62	0.51

Notes: WLS weighted by number of "immigrant" articles in column (1), and by total articles in columns (2) and (3). Standard errors clustered by outlet. AP-approved synonyms are "living in the country illegally/ without legal permission", "enter(-ing/-ed) the country illegally/ without legal permission". Standard errors clustered by outlet.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

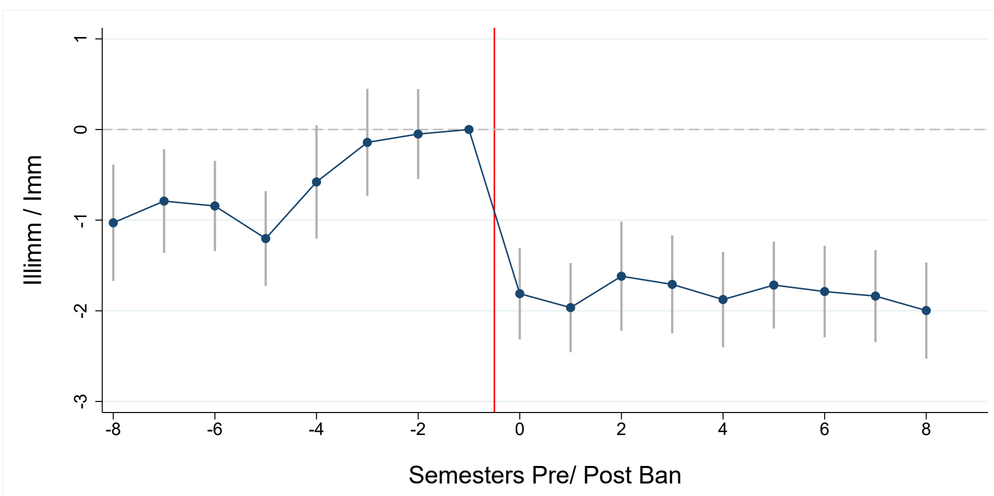
C.3 Diffusion: Subsample of print newspapers

Figure C6: Diffusion by degree of AP intensity



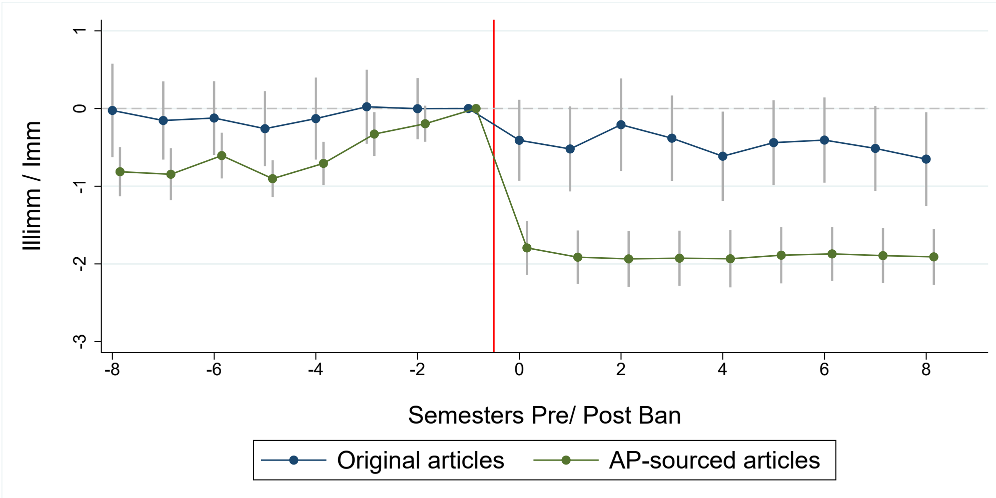
Notes: Coefficients and 95% confidence intervals from a regression of frequency of "illegal immigrant" articles as percent of "immigrant" articles on a full set of indicators for quartile of (positive) AP-intensity interacted with Post Ban, controlling for outlet and year-month FEs. The omitted category is AP-intensity = 0. Weighted by number of "immigrant" articles. Standard errors clustered by outlet.

Figure C7: Diffusion over time



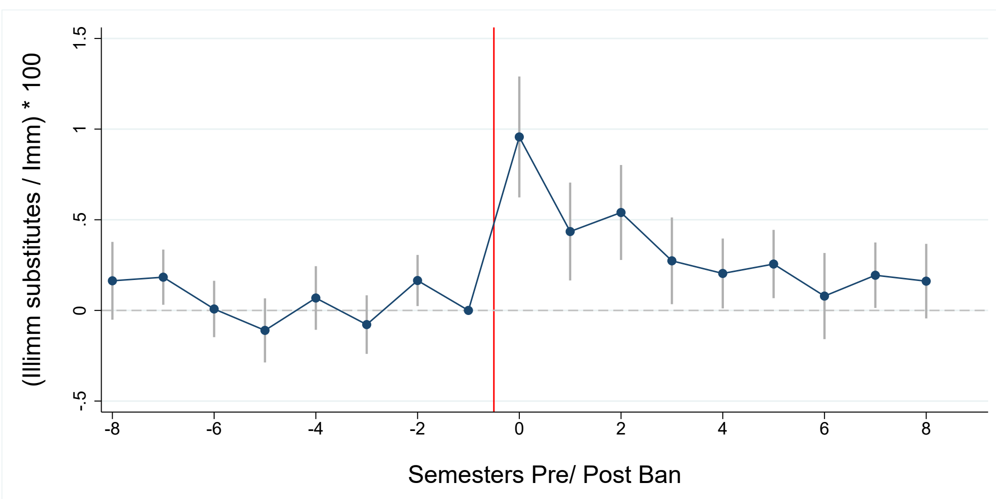
Notes: Coefficients and 95% confidence intervals from a regression of frequency of "illegal immigrant" articles as percent of "immigrant" articles on full set of indicators for semester pre-/post-ban interacted with AP-intensity, controlling for outlet and year-month FEs. The omitted category is the semester before the ban. Weighted by number of "immigrant" articles. Standard errors clustered by outlet.

Figure C8: Diffusion over time: AP-sourced vs original articles



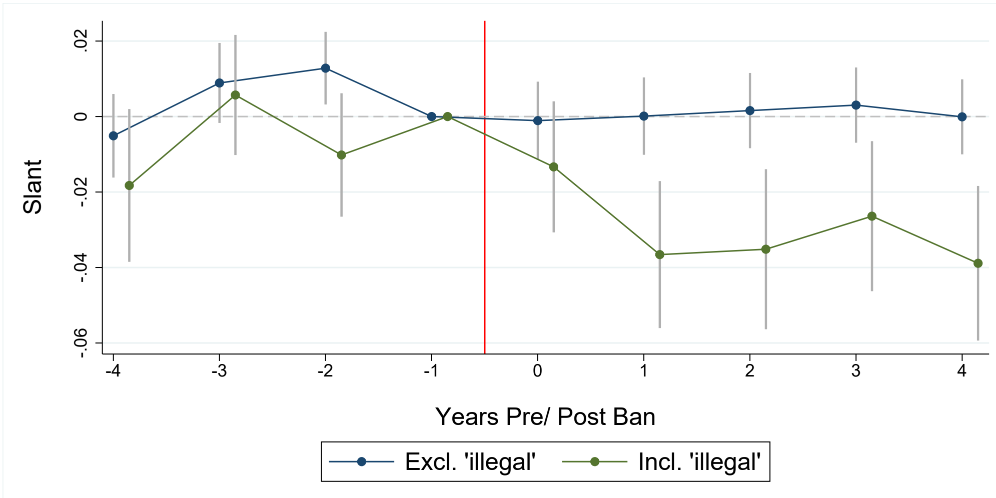
Notes: Green: Articles sourced from AP (attributed or plagiarized). Blue: All other articles. Coefficients and 95% confidence intervals from a regression of frequency of "illegal immigrant" articles as percent of "immigrant" articles on full set of indicators for semester pre-/post-ban interacted with AP-intensity, controlling for outlet and year-month FEs. The omitted category is the semester before the ban. Weighted by number of "immigrant" articles. Standard errors clustered by outlet.

Figure C9: Substitutes proposed by AP



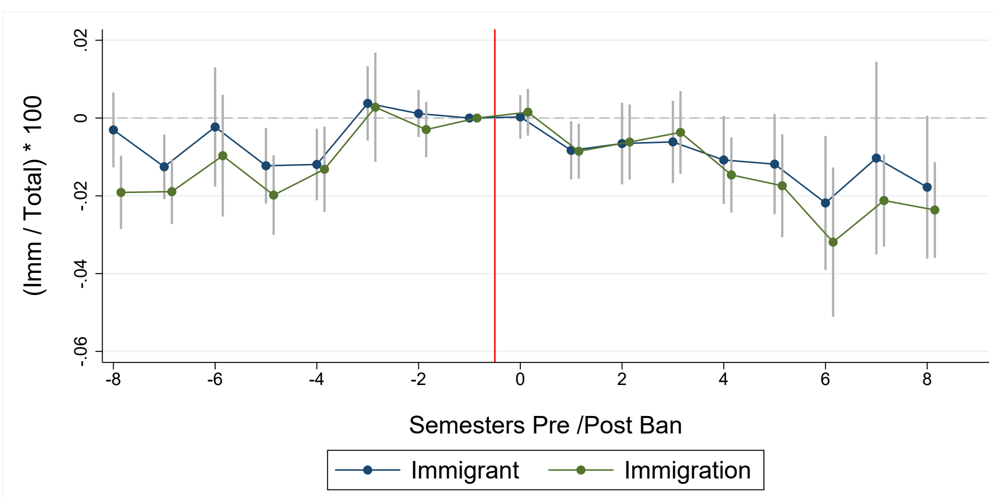
Notes: Coefficients and 95% confidence intervals from a regression of number of articles mentioning the substitutes proposed by AP as percent of "immigrant" articles, on full set of indicators for semester pre-/post-ban interacted with AP-intensity, controlling for outlet and year-month FEs. The omitted category is the semester before the ban. Weighted by number of "immigrant" articles. Standard errors clustered by outlet.

Figure C10: News outlets' slant over time



Notes: Green: immigration-specific slant computed including the phrase "illegal immigrant" and its substitutes. Blue: excluding the phrase "illegal immigrant" and its substitutes. Coefficients and 95% confidence intervals from a regression of frequency of "illegal immigrant" articles as percent of "immigrant" articles on full set of indicators for year pre-/post-ban interacted with AP-intensity, controlling for outlet and year FEs. The omitted category is 2012. Weighted by number of "immigrant" articles. Standard errors clustered by outlet.

Figure C11: Volume of immigration coverage over time



Notes: Blue: "immigrant" articles as percent of total articles. Green: "immigration" articles as percent of total articles. Coefficients and 95% confidence intervals from a regression of volume of immigration-related articles on full set of indicators for year pre-/post-ban interacted with AP-intensity, controlling for outlet and year FEs. The omitted category is the semester before the ban. Weighted by number of total articles. Standard errors clustered by outlet.

Table C6: Diffusion estimates

	(1)	(2)	(3)	(4)	(5) 'Illegal immigration' pct. of 'Immigration'
	'Illigal immigrant', pct. of 'Immigrant'				
PostBan × AP intensity	-1.235***	-1.228***	-1.299***	-1.791***	-0.785***
	(0.230)	(0.216)	(0.177)	(0.251)	(0.150)
AP intensity	0.982***				
	(0.249)				
PostBan	-14.378***				
	(0.990)				
Outlet FEs	No	Yes	Yes	Yes	Yes
Year-Month FEs	No	Yes	Yes	Yes	Yes
State × Year-Month FEs	No	No	Yes	Yes	No
Outlet-specific linear trend	No	No	No	Yes	No
Observations	63,820	63,820	63,568	63,568	52,297
Number of outlets	815	815	813	813	733
R^2	0.20	0.43	0.53	0.56	0.35
Mean dep. var.	21.68	21.68	21.64	21.64	32.34

Notes: WLS weighted by number of number of "immigrant" articles in columns (1)-(4), and by number of "immigration" articles in column (5). Standard errors clustered by outlet.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C7: Alternative measures of AP-intensity

	(1)	(2)	(3)	(4)
	'Illigal immigrant', pct. of 'Immigrant'			
PostBan × AP-intensity: AP credited	-1.073***			
	(0.225)			
PostBan × AP-intensity: AP plagiarized		-1.269***		
		(0.241)		
PostBan × AP-intensity: AP credited, *all articles*			-0.963***	
			(0.231)	
PostBan × Reuters-intensity: Reuters credited, *all articles*				0.660
				(0.520)
Outlet FEs	Yes	Yes	Yes	Yes
Year-Month FEs	Yes	Yes	Yes	Yes
Observations	63,820	63,820	57,220	57,604
Number of outlets	815	815	740	748
R^2	0.43	0.43	0.41	0.41
Mean dep. var.	21.68	21.68	23.12	23.01

Notes: Replication of column (3) of table 1 with the following alternative measures of AP-intensity. Column (1): AP-intensity defined as share of "immigrant" articles published in the 12 months before the ban that are either credited to AP or flagged by a plagiarism algorithm (baseline). Column (2): share of "immigrant" articles credited to AP. Column (3): share of "immigrant" articles flagged by a plagiarism algorithm. Column (4): share of all articles published in the 12 months before the ban that are credited to AP. Standard errors clustered by outlet.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.4 Views on immigration policy

Table C8: OLS correlations between slant and policy views

	Reduced Form				
	(1) Index Restict Imm.	(2) Border	(3) Amnesty	(4) Don't hire	(5) Question
'Illegal imm.', pct. of 'Imm.'	0.0050***	0.0028***	0.0037***	0.0018***	0.0034***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	169,545	169,545	169,545	75,413	126,422
Number of counties	2,251	2,251	2,251	2,128	2,217
R^2	0.00	0.00	0.01	0.00	0.01
Mean dep. var.	0.01	0.56	0.53	0.62	0.41

	Reduced Form				
	(1) Index Restict Imm.	(2) Border	(3) Amnesty	(4) Don't hire	(5) Question
'Illegal imm.', pct. of 'Imm.'	0.00053	-0.00004	0.00005	0.00019	-0.00007
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Respondent controls	Yes	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Observations	162,456	162,456	162,456	74,705	119,552
Number of counties	2,113	2,113	2,113	1,924	2,066
R^2	0.27	0.14	0.16	0.13	0.22
Mean dep. var.	0.01	0.56	0.52	0.62	0.41

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

Table C9: Alternative Specifications

	Reduced form			2SLS		
	(1) APint $\geq$ median	(2) Clustered by state	(3) 1-newspaper counties	(4) APint $\geq$ median	(5) Clustered by state	(6) 1-newspaper counties
PostBan $\times$ I[AP-int $>$ median]	-0.0133**					
	(0.005)					
PostBan $\times$ AP-intensity		-0.0046**	-0.0053***			
		(0.002)	(0.002)			
'Illegal imm.', pct. of 'Imm.'				0.0049**	0.0048*	0.0050**
				(0.002)	(0.003)	(0.002)
Respondent controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs $\times$ 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.	.	.	.	11.78	9.27	7.77
Observations	168,409	168,409	87,880	168,409	168,409	87,880
Number of counties	2,125	2,125	841	2,125	2,125	841
R^2	0.08	0.08	0.08	0.04	0.04	-0.00
Mean dep. var.	0.56	0.56	0.56	0.56	0.56	0.56

Notes: OLS reduced form regressions in the left hand side panel, 2SLS regressions in the right hand side panel. Respondent controls and county controls as in Table 5.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C10: Heterogeneity by newspaper readership

	Reduced Form			2SLS		
	(1) Not Reader	(2) Reader	(3) Print Reader	(4) Not Reader	(5) Reader	(6) Print Reader
PostBan × AP-intensity	-0.0005	-0.0042*	-0.0062**			
	(0.003)	(0.003)	(0.003)			
'Illegal imm.', pct. of 'Imm.'				0.0007	0.0057	0.0080**
				(0.004)	(0.004)	(0.004)
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
First-Stage F stat.	.	.	.	9.52	10.12	10.04
Observations	58174	66777	40192	58174	66777	40192
Number of counties	1844	1805	1596	1844	1805	1596
R^2	0.15	0.16	0.17	0.10	0.11	0.10
Mean dep. var.	0.54	0.56	0.59	0.54	0.56	0.59

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Reader = 1 if read newspaper in the past 24 hours. Print reader = 1 if read print newspaper in the past 24 hours.
Respondent controls and county controls as in Table 5.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C11: Heterogeneity: Interest in Politics and Voting

	Reduced Form		2SLS	
	(1) Voter	(2) Non-voter	(3) Voter	(4) Non-voter
PostBan × AP-intensity	-0.0029	-0.0053**		
	(0.002)	(0.002)		
'Illegal imm.', pct. of 'Imm.'			0.0027	0.0063**
			(0.002)	(0.003)
First-Stage F stat.	.	.	23.46	14.31
Observations	85618	63056	85618	63056
Number of counties	1950	1830	1950	1830
R^2	0.19	0.12	0.14	0.07
Mean dep. var.	0.58	0.52	0.58	0.52

	Reduced Form		2SLS	
	(1) High interest	(2) Low interest	(3) High interest	(4) Low interest
PostBan × AP-intensity	-0.0037*	-0.0055**		
	(0.002)	(0.002)		
'Illegal imm.', pct. of 'Imm.'			0.0036*	0.0059*
			(0.002)	(0.003)
First-Stage F stat.	.	.	21.61	16.98
Observations	86443	71617	86443	71617
Number of counties	1939	1875	1939	1875
R^2	0.20	0.10	0.15	0.05
Mean dep. var.	0.61	0.50	0.61	0.50

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent controls and county levels as in Table 5.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.5 Views on immigration policy: County-level

Table C12: Heterogeneity by interest in politics

	Reduced Form		2SLS	
	(1) High interest	(2) Low interest	(3) High interest	(4) Low interest
PostBan × AP-intensity	-0.0037*	-0.0055**		
	(0.002)	(0.002)		
'Illegal imm.', pct. of 'Imm.'			0.0036*	0.0059*
			(0.002)	(0.003)
First-Stage F stat.	.	.	21.61	16.98
Observations	86443	71617	86443	71617
Number of counties	1939	1875	1939	1875
R^2	0.20	0.10	0.15	0.05
Mean dep. var.	0.61	0.50	0.61	0.50

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent and county controls as in Table 5. Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C13: Heterogeneity by ideology

	Reduced Form			2SLS		
	(1) Dem.	(2) Indep.	(3) Rep.	(4) Dem.	(5) Indep.	(6) Rep.
PostBan × AP-intensity	-0.0042	-0.0056**	-0.0007			
	(0.003)	(0.002)	(0.003)			
'Illegal imm.', pct. of 'Imm.'				0.0039	0.0056**	0.0009
				(0.003)	(0.003)	(0.004)
First-Stage F stat.	.	.	.	17.93	22.39	11.92
Observations	60047	59697	42101	60047	59697	42101
Number of counties	1679	1815	1744	1679	1815	1744
R^2	0.07	0.11	0.08	0.02	0.05	0.03
Mean dep. var.	0.39	0.57	0.77	0.39	0.57	0.77

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent and county controls as in Table 5.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C14: Heterogeneity by share foreign born and share Hispanic

	Reduced Form			2SLS
	(1) Share Imm. < median	(2) Share Imm. ≥ median	(3) Share Imm. < median	(4) Share Imm. ≥ median
PostBan × AP-intensity	-0.0091**	-0.0034*		
	(0.004)	(0.002)		
'Illegal imm.', pct. of 'Imm.'			0.0077**	0.0036*
			(0.004)	(0.002)
Respondent controls	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes
County and Year FEs	Yes	Yes	Yes	Yes
First-Stage F stat.	.	.	19.76	13.55
Observations	19498	142958	19498	142958
Number of counties	1052	1061	1052	1061
R^2	0.15	0.14	0.07	0.11
Mean dep. var.	0.61	0.55	0.61	0.55

	Reduced Form			2SLS
	(1) Share Hisp. < median	(2) Share Hisp. ≥ median	(3) Share Hisp. < median	(4) Share Hisp. ≥ median
PostBan × AP-intensity	-0.0072**	-0.0035*		
	(0.003)	(0.002)		
'Illegal imm.', pct. of 'Imm.'			0.0050**	0.0044
			(0.002)	(0.003)
Respondent controls	Yes	Yes	Yes	Yes
Year FEs × County controls	Yes	Yes	Yes	Yes
County and Year FEs	Yes	Yes	Yes	Yes
First-Stage F stat.	.	.	29.79	8.20
Observations	28906	133550	28906	133550
Number of counties	1055	1058	1055	1058
R^2	0.15	0.14	0.09	0.11
Mean dep. var.	0.59	0.55	0.59	0.55

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. Respondent and county controls as in Table 5.
Standard errors clustered by county. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C15: Views on immigration policy: Reduced form; county-level

	Reduced Form				
	(1) Index Restrict Imm.	(2) Border	(3) No Amnesty	(4) Don't hire	(5) Question
PostBan $\times$ AP-intensity	-0.0206*	-0.0086**	-0.0035	-0.0117**	-0.0082*
	(0.011)	(0.003)	(0.004)	(0.005)	(0.004)
Year FEs $\times$ County controls	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
First-Stage F stat.	9,239	9,239	9,239	3,536	7,232
Observations	2,104	2,104	2,104	1,768	2,040
Number of counties	0.39	0.35	0.38	0.58	0.41
R^2	0.26	0.61	0.59	0.67	0.49

Notes: Reduced form OLS regressions in the left hand-side panel. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C16: Views on immigration policy: 2SLS; county-level

	2SLS				
	(1) Index Restrict Imm.	(2) Border	(3) No Amnesty	(4) Don't hire	(5) Question
'Illegal imm.', pct. of 'Imm.'	0.0244*	0.0102**	0.0042	0.0112**	0.0108*
	(0.013)	(0.005)	(0.004)	(0.005)	(0.006)
Year FEs $\times$ County controls	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
First-Stage F stat.	36.83	36.83	36.83	29.08	20.27
First-Stage coef. on PostBan $\times$ AP-intensity	-0.8408***	-0.8408***	-0.8408***	-1.0508***	-0.7588***
	(0.138)	(0.138)	(0.138)	(0.195)	(0.168)
Observations	9,239	9,239	9,239	3,536	7,232
Number of counties	2,104	2,104	2,104	1,768	2,040
R^2	-0.05	-0.09	-0.01	-0.13	-0.11
Mean dep. var.	0.26	0.61	0.59	0.67	0.49

Notes: 2SLS regressions (upper panel), along with the corresponding 1st-stage coefficients (lower panel). County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C17: Support for increasing border security: county-level

	Reduced Form			2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	"Increase the number of border patrols on the US-Mexican border.": Selected						
PostBan × AP-intensity	-0.0083**	-0.0079**	-0.0086**	-0.0098**			
	(0.003)	(0.003)	(0.003)	(0.004)			
AP intensity	0.0093***						
	(0.003)						
PostBan	-0.0186						
	(0.012)						
'Illegal imm.', pct. of 'Imm.'					0.0082**	0.0102**	0.0121**
					(0.004)	(0.005)	(0.005)
Year FEs × County controls	No	No	Yes	Yes	No	Yes	Yes
County FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Year × State FEs	No	No	No	Yes	No	No	Yes
First-Stage F stat.	.	.	.	.	44.40	36.83	31.70
First-Stage coef. on PostBan × AP-intensity					-0.9661***	-0.8408***	-0.8140***
					(0.145)	(0.138)	(0.144)
Observations	9,407	9,274	9,239	9,224	9,274	9,239	9,224
Number of counties	2,245	2,112	2,104	2,101	2,112	2,104	2,101
R^2	0.01	0.34	0.35	0.36	-0.07	-0.09	-0.11
Mean dep. var.	0.61	0.61	0.61	0.61	0.61	0.61	0.61

Notes: Reduced form OLS regressions in the left hand-side panel, 2SLS regressions in the right hand-side panel. County controls: log population, share urban, racial composition, share foreign born, share college degree, log income per capita, newspaper circulation per capita and Republican vote share. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.