Immigration Attitudes and Labor Market Conditions in the United States

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Abstract

We study how local labor demand shocks shape immigration attitudes in the United States. We combine a novel set of attitude measures, including traditional surveys, newspaper text,Twitter (now X), and Google search queries, to assess whether the relationship is robust across conceptually distinct sources of opinion data. For newspapers and Google searches, we use the relative frequency of the politically charged terms "undocumented immigrants" and "illegal immigrants" as proxies for shifts in immigration attitudes. For Twitter, we use natural language processing to construct sentiment scores for posts containing immigration-related keywords. We measure local labor demand using Bartik-style shocks at state and commuting-zone levels and assess the identifying assumption of shock exogeneity using standard diagnostics and industry-level equivalent regressions. We find that, across most measures and specifications, more positive labor demand shocks increase the prevalence of favorable immigration attitudes. At the same time, the results show that some measures are more sensitive than others to the specification and time horizon, cautioning against reliance on any single proxy. We further examine potential mechanisms by studying heterogeneity in responses across individuals with characteristics linked to labor-market competition, fiscal concerns, and non-economic or cultural factors.

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1 Introduction

The United States is experiencing a substantial and prolonged increase in its foreign-born population of magnitude last seen before the Great Depression (Tavernise (2018)). This demographic change is not going unnoticed, as immigration policy is often one of the key issues for voters. A February 2024 Pew poll[1] found that 57% of Americans believe "dealing with immigration" should be a top priority for the president and Congress to address in the current year, higher than for issues such as "improving job situation" (49%) and "protecting the environment" (45%), but lower than the top priority of strengthening the economy. As an important issue for voters, immigration concerns can impact which policies get adopted, including restrictions on immigration (Facchini and Steinhardt, 2011) and restrictions on employment, through policies such us E-Verify (Barrera et al., 2025), which sometimes prove to be ineffectual or even counter-productive (Ayromloo et al., 2020; Bohn et al., 2015). This makes it all the more important to understand the entirety and complexity of determinants of immigration policy preferences and immigration attitudes more generally. In this study, we examine one potential determinant–we ask whether and why labor demand shocks affect immigration attitudes. In order to do this, we combine various measures of immigration attitudes/policy preferences, each with its own advantages and disadvantages, that together enable more robust conclusions.

What forms one's attitude towards immigrants and immigration policy preferences? Previous research has provided suggestive evidence that those more likely to be adversely impacted by immigrants (of a given skill group/task concentration) in the labor market favor less immigration (Facchini and Steinhardt (2011); Malhotra et al. (2013); Mayda (2006); Mayda et al. (2022);Pardos-Prado and Xena (2019); Scheve and Slaughter (2001) ; Dancygier and Donnelly (2013)); economic considerations regarding taxes/redistribution/public finance implications also appear to be associated with less favorable immigration views (Alesina et al. (2023); Facchini and Mayda (2009); Hanson et al. (2007)). Others have pointed to non-economic factors such as the media slant/slanted language (Facchini et al., 2017a; Keita et al., 2024; Djourelova, 2023), concerns about cultural impact (Hainmueller and Hopkins, 2014), impact on crime (Ajzenman et al., 2023), direct contact and information interventions (Florio, 2022), or have concluded that both economic and non-economic factors matter (Card et al. (2012); Javdani (2020); Müller and Tai (2020); Alesina and Tabellini (2024)).

Although many determinants of immigration attitudes have been examined, little attention has been paid to exogenous (to the local labor market) changes in the labor demand. Notably, they are different from the labor market competition factors discussed in the literature, which are generally focused on the labor supply effect of immigration. What kind of labor demand shocks are considered here and why might they affect immigration attitudes? Since 2000, there have been a number of economy-wide factors that have had

[1] https://www.pewresearch.org/politics/2024/02/29/americans-top-policy-priority-for-2024-strengthening-the-economy/

unequal local effects across the country, including the impact of foreign trade and offshoring, automation, the Great Recession, the COVID19 pandemic, high rates of immigration, as well as periods of prolonged economic growth. The local impact of these changes depends in large part on pre-existing industrial composition, which is captured in a shift-share design of local labor demand shocks (Bartik (1991)). This way of conceiving of employment changes abstracts from the supply-driven factors that would confound the analysis.

There are multiple alternative channels for how local labor demand shocks–whether positive or negative–may affect immigration attitudes. Broadly, they match the three categories of determinants of natives' immigration attitudes as formulated by Dustmann and Preston (2007) and recently used by Alesina and Tabellini (2024) to summarize the current literature: labor market competition, the "welfare magnet" argument, and non-economic factors/cultural concerns.

To begin with, the labor market impact of immigration[2] may be different during negative compared to positive labor demand shocks. Peri (2010) shows that the short-run effect of immigration on native employment and wages and on total factor productivity was more negative during economic downturns and took longer to dissipate than during periods of expansion. The likely reason is that during periods of expansion, immigration reduces unit cost of production of a firm, as immigrant and native workers increasingly specialize in tasks they are best at (Peri and Sparber, 2009, 2011), allowing firms to increase output, which often means hiring more native workers; thereby, the productivity channel partially or fully offsets the substitution channel. Relatedly, Burstein et al. (2020) show that in tradable occupations immigrants do not crowd out native employment precisely because of the output adjustment, which is blunted in nontradable occupations. However, during downturns, firms may have unused capacity and be unable to expand production. Additionally, facing declining and uncertain demand, firms may also be unwilling to make investments in capital to take advantage of immigrant labor in settings where both are necessary for production. Furthermore, even allowing for switching industries or occupations, scanter outside options may make natives' equilibrium outcomes worse.

Next, the cause of the labor demand shock may affect the labor market impact of immigration. While not directly examining attitudes towards immigration Ottaviano and Wright (2013) and Firsin (2023) suggest different mechanisms through which offshoring could potentially isolate native workers from competition with immigrants, thereby reducing the impact of immigration on these workers. If more positive local impact of immigration during increased offshoring exposure is observable, natives may display more positive immigration attitudes.

The second type of determinants of immigration attitudes–welfare magnet–may also operate differently

[2]We will not discuss this literature in detail, but the literature consensus is arguably that the overall impact of immigration on native wages and employment is close to zero, but ranges from negative for some natives to positive for others, depending on the absolute and relative characteristics of immigrants and natives.

in contrasting economic conditions. Local public expenditures often contract during economic downturns, as state governments are not allowed to borrow and have to balance budgets using expenditure cuts. This may exacerbate competition for public resources, such as education, housing, healthcare, etc. On the other hand, enhanced economic climate, spurred by a positive labor demand shock, is likely to exert downward pressure on the reliance upon welfare systems while simultaneously increasing contributions to pensions by both natives and immigrants (see discussion of Storesletten (2000) and Hillman (2002) in O'Rourke and Sinnott (2006)); this may improve social cohesion and reduced resistance to immigration.

In addition to the economic rationale, there may be non-economic reasons for changing immigration attitudes under different labor market conditions. Alesina and Tabellini (2024) find that, on the whole, these explain more of the variation in immigration attitudes. One subclass of explanations is inter-group contact theory, which posits that interactions under different circumstances may produce different effects in terms of inter-group attitudes. In particular, inter-group contact may lead to less prejudice and stereotypes when groups share common goals, there is integroup cooperation, there is equal status between groups and there is institutional or authority support, but inter-group contact may have a negative effect in other circumstances. In particular, Alesina and Tabellini (2024) find that:

> As predicted by "group threat theory" (Blalock 1967, Schlueter and Scheepers 2010), an increase in the size of an out-group might trigger a generalized feeling of anxiety among members of the in-group (in this context, natives), especially in the presence of scarce resources and slack labor markets (Campbell 1965, Esses et al. 2001, Sherif and Sherif 1953).

Since non-economic factors are often found to be more important than the economic ones in determining immigration views, it raises questions about the importance of labor demand shocks in this context, but does not directly dismiss them. Many studies report the importance of various non-economic determinants without testing their effects under different economic conditions. For example, Tabellini (2020) shows that whether immigration produced anti-immigrant views depends on immigrants' cultural distance, rather than the effect on occupation-specific incomes, and anti-immigrant views can form despite a positive economic impact. It does not test, however, whether the negative effect of cultural distance differs under different economic conditions. Hainmueller and Hopkins (2014) literature review finds sociotropic concerns about the cultural impact to be the key to explaining immigration views, but points out that further research is needed to understand circumstances under which sociotropic concerns lead to more or less hostile views. It is, thus, an empirical question whether one or more of the three channels discussed earlier operate to create a connection between labor demand shocks and immigration attitudes.

The study that comes closest to answering the question we pose in the paper is Dancygier and Donnelly

(2013).[3] The authors examine European Social Survey data from several countries from 2002 to 2009, and link immigration policy preferences to growth of the employment sector or national economy of the respondent. They find that workers in sectors with growing overall employment view immigration more positively. They also find that national economic outlook is not associated with immigration views, but better economic outlook makes the association between sectoral immigration flows and immigration views more positive. The first key difference between Dancygier and Donnelly (2013) and the present study is that we use exogenous labor demand shocks as opposed to observed equilibrium employment changes. Employment growth in the industry is the result of changes in both the overall demand for labor and the supply of immigrant and native labor. As an example of just one of multiple possible endogeneity concerns in Dancygier and Donnelly (2013), industries with more positive levels or changes in immigration attitudes (due to changing sectoral demographic composition, for example), will attract more immigrants, leading to higher overall industry employment growth; the direction of causality, however, will be the reverse. Notably, the authors include sectoral non-EU immigration in the main regression specification, but omit EU immigration, which may be associated with immigration attitudes' levels or growth and total employment growth. This and other concerns are addressed when we use plausibly exogenous Bartik shocks. The second key difference is that we use multiple different measures of immigration attitudes, and employ a variety of significantly different specifications, bolstering the robustness of the main findings.

To examine the effect of labor demand shocks on immigration attitudes, we leverage a novel and multi-faceted data collection approach. Traditional survey data, frequently employed in influential studies such as the American National Election Studies (ANES)—utilized by Scheve and Slaughter (2001) and Hanson et al. (2007)—and the Cooperative Congressional Election Study (CCES)[4]—used in Tingley (2013) and Facchini et al. (2017b), serve as foundational elements of our analysis. These sources offer well-structured and widely recognized measures of public opinion but are not without their limitations, including potential biases due to nonclassical measurement error and self-censorship issues (Hayes et al. (2005)).

To complement and enhance the insights provided by survey data, we incorporate Google Trends, messages from Twitter (now X), and newspaper articles into our analysis. Each of these additional data sources introduces a unique dimension to the study of immigration attitudes. Google Trends offers insights into anonymous, high-frequency searches on the Google search engine, reflecting real-time public interests and concerns that may not be captured through traditional surveys. Tweets, characterized by their voluminous and free-form expression, provide a granular view of public sentiment and discourse, including perspectives

[3]There are also studies in the field of sociology (Ruist, 2016; Meuleman et al., 2009; Schneider, 2008) that examine the association between economic conditions and immigration attitudes. They conflict on the sign and size of the association, and do not feature empirical methodologies that would produce confidence in the interpretation of the relationship as causal.

[4]CCES is now called CES, Congressional Election Study.

that individuals may not be willing to express in survey responses. This data, while potentially anonymous, captures a wide range of opinions and can signal shifts in public attitudes in response to current events. Newspaper articles, on the other hand, offer a more formal and detailed perspective on local and national events, providing context and narrative to the statistical patterns observed in other data sources.

By integrating these diverse data sources, our study aims to achieve a comprehensive understanding of how immigration attitudes evolve over time and respond to labor demand shocks. This approach allows us to capture the multifaceted nature of public opinion, providing depth to our analysis that surpasses what is possible through the use of survey data alone. The inclusion of Google Trends, tweets, and newspaper articles enables us to identify and analyze trends and shifts in immigration attitudes with a level of immediacy and detail that has not been previously attained in the literature.

Following Bartik (1991), we estimate predicted variation in labor demand by using past local industry shares with aggregate national industry growth rates. In the first part of the empirical analysis, our units of observations are organized into state-year and commuting zone-year panels.

We employ a first-difference specification to account for time-invariant location-specific factors and potential trend.[5]. In the second part of the analysis, we further explore the impact of labor demand shocks on changes in individual attitudes using a linear probability model. We examine how individual responses to labor demand shocks differ by individual characteristics suggestive of the different mechanisms for the shock-attitudes connection, while account of other individual-, time- and location-specific factors.

Like most of the literature, when we study the effects of economics conditions, we are interested in roughly contemporaneous outcomes. We investigate the changes in labor demand relative to 1, 2 or 4 years prior to the year of the measurement of attitudes. Alternatively, we investigate the association between contemporaneous economic conditions measured as a deviation from long-term trend. We think of the relationship as relatively temporary. There is some evidence in studies outside of economics (Coenders and Scheepers, 2008; Laaker, 2024; Wilkes and Corrigall-Brown, 2011; McLaren, 2001) and in economics (Cotofan et al., 2024) that macro-level trends, such as immigration flows, unemployment rates, and average income variation experienced in youth are associated with one's immigration views. (Cotofan et al., 2024) show that average incomes in one's census region growing up[6] can affect one's predisposition towards immigrants later in life. We mostly focus on short/medium term effect in this version of the study, but plan to address the question of longer term effects in future work.

Our empirical findings indicate a generally–but not universally–consistent positive link between labor

[5]In the appendix, we present results of a fixed-effects model to explore the robustness of results to underlying model assumptions

[6]The paper focuses on the years during the formation of political attitudes (between ages of 18 and 25), but its Appendix shows similar association is present even for ages 0 to 9.

demand and pro-immigration sentiment. State-level analysis with different measures of immigrant attitudes suggests a one-standard-deviation increase in the Bartik shock leads to a rise in pro-immigration attitudes not far from one standard deviation for all measures except CCES. Using individual level responses from ANES and CCES, we find that both point to individuals responding positively to more positive labor market conditions. Overall, the evidence supports the view that improving labor demand leads to more favorable immigration attitudes, a result that holds across multiple data sources, model specifications, and geographic scales. Delving into heterogeneous responses to examine potential mechanisms, we find all three channels discussed in Alesina and Tabellini (2024) have some support, but it is most consistent for the welfare channel. Examining whether labor market conditions have a linear effect, as opposed to being different for positive versus negative changes, we find the results to be inconclusive, but more in line with greater effect of decreases in labor demand.

In Section 2, we describe the data used for the analysis. Section 3 outlines trends in immigration across different data sources and compares them. In Section 4, we present our empirical framework, detailing the models used to analyze the data. Section 5 discusses our main findings, and Section 6 concludes.

2 Data

This section discusses the various data sources used to measure labor demand shocks, attitudes toward immigrants and characteristics of locations that may affect immigration views. We begin by discussing the Decennial Census and the American Community Survey (ACS) data used to estimate labor demand shocks. Next, we discuss our approach to measuring attitudes towards immigrants that goes beyond traditional survey and Congressional record data. Instead, we draw from a variety of sources that each offer distinct insights into people's opinion. Google searches and Twitter posts provide unfiltered/mildly filtered and candid insights, whereas newspapers offer a more purposeful and deliberate expression of attitudes towards immigrants in the broader public discourse.

2.1 Decennial Census and the American Community Survey

Aggregate measures of noninstitutionalized, civilian population aged 18 and older, employment, and employment by industry are taken from the long-form Decennial Census in 1990 and 2000, and from the ACS thereafter (both obtained from IPUMS USA (Ruggles et al., 2025)).[7] We use provided sample weights to find Public Use Microdata Area (PUMA) totals and aggregate to state- and commuting CZ-level employment

[7]Please note that the ACS was in test mode from 2001 – 2004 and had different sample sizes and designs that may render the data incomparable to the post-2005 data, while the 2005 ACS did not include group quarters. For more information, please refer to: https://www.census.gov/programs-surveys/decennial-census/about/census-acs.html.

totals. We mainly use longitudinally-consistent, 3-digit industry codes based on the Census Bureau's 1990 industry classification scheme from IPUMS. Alternatively, mainly for robustness checks, we use 1997 NAICS industries. We also rely on ACS for location characteristics, such as share college educated, single, male, white, and average age.

2.2 American National Election Studies (ANES)

The American National Election Studies (ANES) is a nationally representative survey conducted during most national election years since 1948. It aims to collect data on electoral behavior, political participation, and public opinion and attitudes. The survey typically involves pre- and post-election interviews during presidential election years, addressing topics such as immigration, while only post-election interviews are conducted during midterm election years. We utilize the Time Series ANES files, which are particularly useful for examining changes in attitudes towards immigrants over time, as they contain questions consistently asked in all survey years (they also contain additional questions that were asked less frequently). There are three immigration-related questions asked consistently since at least 2004. The first question inquiries about whether the number of immigrants to the U.S. should increase, decrease, or remain the same. The second question gauges respondents' perceptions of the likelihood that immigrants will take jobs away from others, with options ranging from extremely likely to not at all likely. The last one is a feelings thermometer towards "illegal immigrants". Notably, the term "illegal" for the feelings thermometer remained the same, even as some other questions changed to using the term "unauthorized."

2.3 CCES

The CCES is a large nationally stratified survey conducted primarily in congressional election years, with more than 50,000 respondents in most even-numbered years and smaller samples, roughly one-third as large, in odd-numbered years. It has been widely used in research on political attitudes and policy preferences, including work on immigration attitudes (Djourelova (2023)). The survey includes both eligible voters and respondents who are not eligible to vote. Geographic identifiers are available at a fine level, down to the ZIP code level, although the survey is designed to be representative only at the national and state levels.[8]

The CCES contains more than ten different questions related to immigration attitudes and policy preferences, but most are not asked consistently over time. Our main analysis therefore focuses on the two questions that appear in most survey years, which we refer to as the "legalization" and "border control"

[8]For that reason, when we aggregate CCES responses to the commuting-zone level, we restrict the data to commuting zones with at least 30 respondents and interpret the resulting estimates with appropriate caution, given the potential for sampling error.

questions. The legalization question asks whether the respondent agrees with the following statement: "What do you think the U.S. government should do about immigration? 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." The border-control question asks whether the respondent agrees with the following: "What do you think the U.S. government should do about immigration? Increase the number of border patrols on the US-Mexican border."

In addition to studying these two questions separately, we also create a broader CCES immigration-attitudes index. At the individual level, we first convert responses to the available immigration questions into Z-scores and then average them. At the geography level, we use a simple average of the available question means rather than averaging Z-scores. The distinction reflects the different objectives of the two settings: at the individual level, we want responses to be measured relative to the cross-person distribution, while at the geography level, we are interested in average changes within a geography over time relative to other geographies. The remaining immigration questions used to construct these broader measures are described in the Appendix.[9]

2.4 Google Trends

Google Trends provides insights on search trends and behavior on the Google search engine. It does so by producing the Search Volume Index (SVI), which measures the relative popularity of a query for a specific time and geography. We use national and state-level Google Trends data for "illegal immigrants" and "undocumented immigrants" to examine changes in attitudes towards immigrants in the U.S.

This reflects the idea that people with more negative attitudes towards immigration are likely to focus on the illegal aspect of immigrants' entry, while those with more positive attitudes use the term "undocumented." In fact, the terms are so distinct, that simply being exposed to the term "illegal immigrant" as opposed to more neutral language induces more negative immigration views (Djourelova, 2023). The SVIs for each term are used as a proxy for the share of positive and negative attitudes towards immigrants. We compile both a national annual time series and a state-year level panel to analyze changes in attitudes towards immigrants. More specifically, since the SVI for each term shows how often it is searched in an area relative to all other searches, we take the ratio of the SVI for "undocumented immigrants" and the SVI for "illegal immigrants." The higher the ratio, the more positive the immigration attitudes in the area. We provide a detailed explanation of how SVI is calculated and of important considerations when interpreting the data in the appendix.

[9] One additional complication is that the response format changes over time. Before 2018, respondents selected whether a policy should be included among the actions the government should take. Beginning in 2018, respondents were instead asked whether they support or oppose each policy separately.

2.5 Twitter

Twitter (now X) is a social media platform that was launched in July 2006 and allows users to interact with one another by posting short messages called tweet, which can contain text, images, videos, links, and hashtags. We used python module "snscrape" to programmatically access and collect tweets between 2006 and 2022 (since there were very few users initially, we mostly use data from 2008). We filtered the tweets to include messages that contained the keywords indicative of immigration-related content.[10] User location was obtained either from tweet geocoordinates or (in most cases) based on user profile location (we matched user location to cities, counties, states, depending on what was available in the profile). Sentiment score was obtained based on dictionary match of positive and negative terms. Importantly, in addition to sentiment score, one also needs to find the sentiment target, since some tweets with negative sentiment expressed that sentiment towards immigrants, while others–to policies/policy makers perceived as hostile towards immigrants. In more recent years, the latter mainly referred to the administration of the former a current President Donald J. Trump. To classify sentiment target, we used the tweets that had almost definitive hashtags, indicative of person's general view on the topic. For example, "#antitrump" was indicative of opposition to President Trump and his policies, while "#buildthewall" was indicative of favoring more restrictions on immigration. The full list of hashtags is described in the appendix. A random forest classifier (based on word frequency) was used to assign sentiment target to tweets without highly-informative hashtags. The sentiment score together with sentiment target determined the tweet's sentiment towards immigrants/immigration. Next, each user's tweets were averaged to find the share of anti-immigrant tweets, with a share of over 50% classified the user as anti-immigrant. Lastly, we found the share of anti-immigrant users within each location and for each year.

2.6 Newspapers

When we use newspaper sentiment to measure immigration attitudes in an area, we make the assumption that local newspapers reflect readers' views, in addition to shaping them. We operationalize this approach by using frequency of search results for articles with pro-immigrant and anti-immigrant terms. Specifically, we search for terms "undocumented immigrant", "illegal immigrant" and "immigrant" in every U.S. state annually, between 2004 and 2024 in the newspapers.com database. We then take the ratio of articles using the term "undocumented immigrant" relative to "illegal immigrant" in every state-year. The higher the ratio, the more pro-immigrant we expect the sentiment to be. Using this measure one has to keep in mind the switching of the articles sources from the Associated Press (AP) from using "illegal immigrant" to other

[10]The searched term was defined as "migrant OR migrants OR immigrant OR immigrants OR immigration OR migration OR illegals OR undocumented OR (illegal alien) OR (illegal aliens) OR (illegal workers)"

terms in April 2013.

3 Descriptive Statistics and Comparison of Measures

3.1 Overall Sentiment Change over Time

Figure 1 shows the average immigration sentiment over time based on the main measures used. Over time, most measures indicate increasingly more positive immigration attitudes. Google searches indicate that the ratio of searches for "undocumented" vs. "illegal immigrants" increased from 1/10 to more than 1/2 from 2004 to 2022. The analogous ratio for these terms in newspaper articles dropped from a bit over 3/10 in 2004 to 2/10 in 2011 but increased to over 7/10 in 2017, and fluctuated between 4/10 and 7/10 thereafter. On CCES question regarding granting path to legal status to "illegal immigrants", the share approving rose from less than 0.4 to almost 0.7 between 2008 and 2020. On all three ANES questions, there appears to be a relatively flat trend between 2004 and 2012, and a moderate growth in positive attitudes thereafter. CCES question regarding more border patrols shows a somewhat different pattern–the number opposing fluctuated between 2008 and 2017, but dropped thereafter–this could be related to increased border crossings/coverage of border crossings. The average of all CCES questions increased initially, and then fluctuated (in a pattern that seems to coincide with election cycles). The only measure that shows a sustained decrease in positive immigration attitudes is the share of pro-immigrant users on Twitter–it appears to decrease from almost 100% in 2008 to 80% in 22. This could be due to early adopters being younger/more liberal people–or to difficulty of classifying early users using hashtags. Of course, it could also be because immigration attitudes become more hostile, but this would be at odds with virtually all the other measures.

3.2 Correlation Between Measures

We next explore state-level bilateral correlations between the different measures over time. We discuss correlations between two of the measures and the rest of the measures and leave the others for the appendix. In Figure 2 we can see that for all measures except news ratio, the correlation with ANES question regarding increasing the level of immigration is the highest in 2020 (at above 0.5)–this is also the only year where all the correlation coefficients are statistically different from 0–and second highest in 2016. In 2012, the correlation values are lower and generally not statistically different from 0, whereas only 3 comparison measures are available for 2008, and 2 out of 3 they are not statistically different from 0. One possible explanation for why SVI and New Ratio where not as strongly correlated with ANES immigration sentiment measure in 2012 may be that the term "illegal immigrants" became a more pejorative term over time, as "undocumented"

became more widely accepted – notably the AP banned the term "illegal immigrants" in April 2013. It is unclear why ANES and CCES measures were not as strongly correlated in 2012 as in 2016 and 2020.

Figure 3 presents a similar picture to the above, but with more data points, since Twitter data is annual. The correlation between the Twitter measure and CCES measures fluctuates around 0.5 throughout the time period studied. The correlation with SVI and ANES starts of at 0 or negative between 2008 and 2011, and then rises to above 0.5 by around 2016. The correlation with news ratio is fluctuates around 0.25 for most of the years, and is statistically different from 0 less than half the time. Since there is a positive and significant correlation between Twitter sentiment and all the measures except ANES in 2012, and ANES is not correlated with any other measure in 2012, it is likely there is an irregularity in just the ANES measure in 2012.

3.3 Measures of Specific Topics and Issues Related to Immigration

We next looks at specific aspects of immigration attitudes. To begin, we look at state-level correlations between different measures part of the same data source. For ANES, Figure 4 shows that the belief that immigrants are somewhat or not at all likely to take jobs away from immigrants is highly correlated with whether people ultimately want more immigration. The Pearson correlation coefficient for the two variables is 0.74, higher than the correlation between the desired number of immigrants and "illegal alien thermometer". It could be because the latter refers to undocumented immigrants specifically, rather than immigrants in general, but it is nevertheless informative that the correlation between views on labor market competition and immigration policy preference is so high in ANES.

People who have lost their jobs and are unemployed may well be more concerned about competition with immigrants than others. Hence, we look at how immigration attitudes differ by the respondent's job status using ANES data. We do this by skill level, proxied for by education level (Table A1). As expected immigration views among the college-educated are more positive for all measures. Interestingly, those unemployed are less likely to think immigrants are only somewhat or not at all likely to take jobs from natives compared to the employed respondents among both college educated (T-stat = -2.4) and non-college educated (T-stat = -3), which is consistent with the hypothesis negative labor market events can cause people to view immigrants as potential competitors; however, their feelings thermometer values towards immigrants and views on whether immigration should be increased are not statistically different from those employed, which suggest people may be able to separate this aspect of immigration from more generalized feelings about immigrants.[11]

[11]The unemployed include temporarily laid-off. The results are for the respondents whose parents were born in the U.S., but the results are similar if we look at all respondents; if we just look at people whose parents where not native born, the

Next, we look at how responses to CCES immigration-related questions differ by employment status within education level group among the natives. The measures in Table A2 and Table A3 are coded such that each ranges from 0 to 1 on a negative-to-positive scale in terms of immigration attitudes. If the question was originally anti-immigration, such as whether the number of immigrants should be reduced by 50%, it was flipped to be whether the person opposed this. As in ANES, those without college education express less pro-immigrant views in virtually all questions. However, counter-intuitively, among the lower skilled, those who were unemployed seemed to be more pro-immigrant on almost all questions. On the other, hand, among the college-educated, those unemployed often expressed less pro-immigrant views. Taking into account the analysis of both ANES and CCES answers, the difference by education level is consistent with the hypothesis of more negative views due to labor market competition, but the employment status results are more mixed.

4 Empirical Framework

The central question in this paper is whether local labor demand shocks affect attitudes toward immigration in the U.S. and, if so, through what mechanism. To answer this question, we use a two-part strategy. The first part uses panel data at the state level and, when the data allow, at the commuting-zone level. The second part uses individual-level repeated cross-sections from the CCES and ANES.

In the panel analysis, our baseline specification is a first-difference model that relates changes in immigration attitudes within a location to changes in predicted local labor demand over time, constructed via a Bartik (1991) shift-share approach. Estimating the model in first differences has several advantages in our setting. First, it removes all time-invariant differences across locations, such as persistent political culture or other fixed local characteristics. Second, it matches the way the Bartik shock is identified in our setting. The local exposure measure is based on initial industry shares, which are fixed, while the source of time variation comes from changes in national industry conditions. Estimating the model in first differences therefore places the focus on whether changes in those national industry shocks, filtered through predetermined local industry structure, are associated with changes in local immigration attitudes. Third, immigration attitudes may be highly persistent over time. In a levels specification, that persistence can make it harder to separate the effect of new labor demand shocks from the simple tendency of attitudes to remain similar from one period to the next. By focusing on changes, the first-difference specification asks a more direct question: when a location experiences a larger change in predicted labor demand, does it also experience a larger change in immigration attitudes?

As a robustness check, we also estimate specifications with location and year fixed effects in levels.

unemployed less-educated are not more likely to think immigrants take jobs away from natives.

Depending on the unit of observation, these are state and year fixed effects or commuting-zone and year fixed effects. These models absorb time-invariant differences across locations and shocks common to all locations in a given year. They provide a useful benchmark, but they are not our preferred specification. One reason is that immigration attitudes may be persistent over time. A levels fixed-effects model that omits lagged attitudes does not model that persistence directly, while adding lagged attitudes would turn the model into a dynamic panel in which the standard fixed-effects estimator is biased when the time dimension is limited (Nickell, 1981). The results, reported in the Appendix, are nonetheless broadly consistent with the baseline findings.

The second part of the analysis uses individual-level repeated cross-sections from the CCES and ANES to examine potential mechanisms and heterogeneity in the survey-based outcomes. Because these surveys do not follow the same individuals over time, this part of the analysis does not track within-person changes. Instead, it relates each respondent's immigration attitudes to changes in the Bartik shock in the respondent's state or commuting zone. Working with individual-level data has two advantages. First, it allows us to test whether the relationship between labor demand shocks and immigration attitudes differs across groups that are especially relevant for the mechanisms we study. Second, it allows us to control for respondent characteristics and to retain information that is lost when attitudes are averaged to the geographic level. The individual-level controls include age, age squared, sex, race and ethnicity, educational attainment, family income percentile, and native-born status. In specifications that also include local controls, we add local demographic characteristics, including average age and the shares of the population that are college educated, male, white, and unmarried.

The next section details the construction of the Bartik shock and our identification assumptions.

4.0.1 Bartik Shocks as Exogenous Labor Demand Shocks

Determining the influence of changes in labor market conditions on attitudes towards immigrants is challenging due to multiple factors, such as the reverse correlations between attitudes towards immigrants and labor market conditions, and the unobservability of labor demand. Hostility towards immigrants could, for example, discourage immigrant labor supply. Additionally, hostility towards immigrants could affect labor demand for immigrants through discrimination. To isolate exogenous variation in labor demand across geographic locations with varying industry compositions, we employ Bartik shocks as per Bartik (1991). Bartik shock B_{lt} for locations $l = 1, \ldots, L$ and year t can be expressed as:

$$B_{lt} = \sum_{n} s_{lnt_0} g_{nt} \tag{1}$$

Here, we observe national employment growth for industries $n = 1, \ldots, N$ and location and industry-specific employment shares anchored in year t_0. In our study, the first geographic locations of interest are the 50 states and the District of Columbia, since this larger level of geography is one at which all the immigrant sentiment measures used are available. Additionally, for a smaller number of measures, we also use commuting zones.

Industries are defined primarily using 3-digit codes from the 1990 Census Bureau industrial classification, with 1990 used as the base year for local industry shares. We use 1990 as the main base year because, in Bartik designs, it is standard to measure local industry composition well before the analysis period so that the shares are predetermined with respect to later shocks and outcomes, especially when share endogeneity is a concern. After restricting the sample to industries that have positive employment in 1990 and are also observed in every year of our analysis through 2024, we are left with 217 industries. For simplicity, we refer to this classification as the 1990 industries.

As a robustness check, we also construct Bartik shocks using the 1997 North American Industry Classification System (NAICS)-based scheme and local industry shares from 2000. We refer to this alternative classification as the 2000 industries. This alternative has two advantages. First, the 1997 NAICS system is may be more comparable to later NAICS vintages, which can reduce error from crosswalking industry codes over time. Second, using 2000 as the base year produces local industry shares that more closely reflect the industrial composition of the period covered by our analysis. After applying the same requirement of consistent availability over time, this alternative classification yields 171 industries with positive employment in 2000 that are observed in all analysis years through 2024. Results using these 2000-based Bartik shocks are reported in the Appendix. [12]

Because the central question in this paper is whether labor demand shocks affect immigration attitudes, it is important to verify that our Bartik measure captures meaningful variation in local labor demand. If changes in the Bartik shock are not associated with realized changes in local employment, then its interpretation as a measure of labor demand is less convincing. We therefore examine whether changes in the Bartik shocks predict changes in realized employment. For this exercise, we use annual data and construct one-year changes. This allows us to use a longer and more finely measured time series than is available in the survey data, which improves statistical precision. We find that, for most of pre-pandemic period, changes in the Bartik shock are strongly associated with changes in employment at both the state and commuting zone levels. However, this relationship weakens sharply in the post-pandemic period, especially at the commuting-zone level.[13] For that reason, although the underlying data extend through 2024, our main causal analysis is restricted to the pre-pandemic period through 2020.

[12] Further details on these classification schemes are provided in the IPUMS help files for `ind1990` and `indnaics`: `https://usa.ipums.org/usa-action/variables/group/work`.

[13] Similar patterns appear in the corresponding fixed-effects specification reported in the Appendix.

More precisely, we estimate a sequence of regressions at the state- and commuting-zone levels, iterating through each year in the analysis period as a possible cutoff year k. For each cutoff, the coefficient on ΔB_{st} is allowed to differ before and after k.

At the state level, we estimate the following:

$$\Delta \ln(\text{emp}_{st}) = \tau_t + \omega_0 \left(\Delta B_{st} \times \mathbf{1}[t \leq k]\right) + \omega_1 \left(\Delta B_{st} \times \mathbf{1}[t > k]\right) + \Delta X'_{st}\lambda + \varepsilon_{st}, \tag{2}$$

where $\Delta \ln(\text{emp}_{lt})$ is the change in log employment in location l between $t-1$ and t, ΔB_{lt} is the change in the Bartik shock, k denotes the cutoff year. The coefficients ω_0 and ω_1 capture the relationship between changes in the Bartik shock and employment before and after the cutoff year, respectively; ΔX_{lt} includes changes in the same demographic controls that will be used in the main analysis with immigration attitudes as the outcome (age composition, education, sex, race and ethnicity, and marital status shares). Year fixed effects τ_t absorb common shocks across locations. Standard errors are clustered at the state level.

At the commuting-zone level, we estimate the following:

$$\Delta \ln(\text{emp}_{ct}) = \tau_t + \alpha_s + \omega_0 \left(\Delta B_{ct} \times \mathbf{1}[t \leq k]\right) + \omega_1 \left(\Delta B_{ct} \times \mathbf{1}[t > k]\right) + \Delta X'_{ct}\lambda + \varepsilon_{ct}, \tag{3}$$

where c indexes commuting zones, s denotes the majority state associated with commuting zone c, and other variables are as previously defined. Standard errors are clustered at the commuting-zone level.

Figures 6 and 5 plot the estimates of ω_0 and ω_1 for each cutoff year. At the state level, the estimated slope through the cutoff year is always positive and statistically significant, with especially large estimates for the earliest cutoffs and estimates that settle into a fairly stable range thereafter. The estimated slope after the cutoff year is also positive and precisely estimated for cutoff years through 2021, generally lying in a moderate range. It falls markedly only in the latest post-pandemic cutoff samples: for cutoff years 2022 and 2023, the post-cutoff point estimates are much smaller, and their confidence intervals include zero. At the commuting-zone level, the through-cutoff slope is again positive and fairly stable across cutoffs. The post-cutoff slope is likewise positive and precisely estimated through cutoff year 2020, but it drops noticeably for cutoff year 2021 and becomes imprecisely estimated. For cutoff years 2022 and 2023, the post-cutoff point estimates turn negative, although they are not statistically distinguishable from zero at conventional levels. Overall, the deterioration in predictive power appears earlier and more sharply at the commuting-zone level than at the state level.

This pattern is unlikely to be driven solely by the shorter time span available in the later cutoff samples. For example, when the cutoff year is 2006 or 2007, the corresponding through-cutoff estimates are based on only one or two annual changes, yet they remain positive, precisely estimated, and statistically significant.

We therefore interpret the post-pandemic pattern as evidence of a breakdown in the empirical link between predicted labor demand and realized employment, rather than as evidence that the underlying economic relationship has reversed. One plausible explanation is that labor supply constraints, sectoral reallocation, geographic mobility, and other structural changes weakened the extent to which local employment adjusted to demand shocks. In that environment, the Bartik measure may still capture shifts in labor demand, but those shifts no longer translate into realized employment changes in a stable way. Because our identification strategy relies on interpreting the Bartik shock as a measure of local labor demand, this decline in predictive power motivates our focus on the pre-pandemic period in the main analysis.

Within this pre-pandemic window, our baseline immigration-attitudes analysis uses endpoint years 2004, 2008, 2012, 2016, and 2020. We focus on four-year intervals because the ANES is available only every four years, coinciding with presidential election years, and using common endpoints allows us to compare results across multiple outcome measures. Where outcome availability permits, we also examine alternative time windows and increments in the Appendix, and the main conclusions are similar.

Having established that, in the pre-pandemic period, changes in the Bartik shock closely track realized changes in employment, we next use this measure to study whether labor demand shocks affect immigration attitudes. The logic is straightforward: if the Bartik shock captures economically meaningful changes in local labor demand, then it provides a plausible proxy for the labor demand shocks whose effects on immigration attitudes are the focus of this paper. We therefore restrict the main analysis to the pre-pandemic period and estimate how changes in local immigration attitudes relate to changes in the Bartik shock.

At the state-year level, we start with the following specification:

$$\Delta y_{st} = \tilde{\tau}_t + \tilde{\omega}\Delta B_{st} + \Delta X'_{st}\tilde{\lambda} + \Delta\tilde{\varepsilon_{st}}, \tag{4}$$

where $\Delta y_{st} = y_{s,t} - y_{s,t-1}$ is the change in the outcome for state s between $t-1$ and t, ΔB_{st} is the change in the Bartik shock, interpreted as the predicted, labor demand-driven, proportional change in state employment, ΔX_{st} is a vector of changes in time-varying state-level demographic characteristics. $\tilde{\tau}_t$ denotes year fixed effects, which absorb shocks common to all states in year t. Estimating the model in first differences also removes any time-invariant differences across states, such as persistent political culture or other fixed characteristics that may be correlated with immigration attitudes. The coefficient $\tilde{\omega}$ si therefore identified from within-state changes over time. This is useful in our setting because immigration attitudes may be serially correlated and may also follow slow-moving trends. In a levels specification, such persistence can make it harder to distinguish the effect of new labor demand shocks from the tendency of attitudes to remain similar from one period to the next or to drift gradually over time. By relating changes in attitudes from

$t-1$ to t to changes in the Bartik shock over the same interval, the first-difference specification reduces the influence of these low-frequency movements and focuses more directly on whether new labor demand shocks are followed by changes in immigration attitudes.

For our Bartik shift-share measure to yield plausibly causal estimates, it must be uncorrelated with any unobserved factors that independently affect immigration attitudes. More generally, for any location unit l in our analysis, whether a state or a commuting zone, the Bartik shock B_{lt} should be orthogonal to the corresponding error term $\tilde{\varepsilon_{lt}}$ in the attitude equation. Recent research shows this orthogonality can be justified through one of two equivalent conditions. The first is shock exogeneity (Borusyak et al., 2022), under which the underlying national industry growth rates (g_{nt}) are exogenous (as-good-as random) conditional on fixed-effects and controls. The second is share exogeneity (Goldsmith-Pinkham et al., 2020), under which the base-year industry employment shares (s_{lnt_0}) are exogenous. If either condition holds, then the composite Bartik shock as defined in Equation (1) is exogenous.

In our setting, share exogeneity is the less plausible of the two conditions. Share exogeneity would require the base-year industry employment shares to be orthogonal to later unobserved determinants of immigration attitudes. That is a strong requirement here. States and commuting zones with different initial industry mixes may differ systematically in later exposure to immigrant inflows, labor supply pressures, or political responses to immigration. For example, places specialized in agriculture, construction, hospitality, or high-skill services may face different immigration-related economic and political dynamics precisely because of that initial industrial structure. If so, the base-year shares may themselves be correlated with later shocks to immigration attitudes, making share exogeneity difficult to defend in our setting.

We therefore view shock exogeneity as the more plausible route to identification. Under this condition, the national industry-specific growth rates are orthogonal to local unobserved determinants of immigration attitudes, conditional on the fixed effects and controls included in our regressions. In our context, that assumption is more credible when the Bartik measure is built from many distinct national industry shocks with dispersed weights, rather than being driven by a small number of sectors. Intuitively, if the shift-share measure averages many relatively small industry shocks, then the resulting variation is closer to the quasi-experimental environment emphasized by Borusyak et al. (2022).

To assess whether this is true in our data, Table A4 reports summary statistics for the national industry growth rates used in the Bartik construction. The purpose of the table is not to prove shock exogeneity, but to show whether the identifying variation is broad and dispersed rather than concentrated in a few industries. In the 1990-base specification, the weighted mean national industry growth rate is 0.28, with a standard deviation of 0.58 and an interquartile range of 0.51. In the 2000-base specification, the weighted mean is 0.13, with a standard deviation of 0.39 and an interquartile range of 0.25. Thus, in both specifications,

growth rates display substantial dispersion across industries and time periods.

The table also shows that the shock weights are not highly concentrated. In the 1990-base specification, the effective number of industry-by-year shocks, measured by the inverse Herfindahl-Hirschman Index, is 272.69; in the 2000-base specification, it is 161.92. The largest individual industry share is only 0.01 in the 1990-base specification and 0.02 in the 2000-base specification. Even after grouping industries more coarsely, the effective number of industry-group-by-year shocks remains 34.49 and 19.61, respectively, while the largest industry-group shares are 0.08 and 0.13. Across the two specifications, the Bartik measure is constructed from 1,085 and 855 industry-by-year shocks spanning 217 and 171 industries, respectively. Taken together, these diagnostics indicate that the Bartik measure in our setting is built from many shocks with relatively dispersed weights rather than from a small number of dominant industries. That pattern makes shock exogeneity more credible here than share exogeneity.

Having established the plausibility of within-period exogeneity of national industry growth rates, we will leverage an equivalent industry-level regression for equation 4 to obtain valid exposure-robust standard errors and compare them to those obtained in the location-level regression.

4.1 Commuting-Zone (CZ)-Level Estimation

For the subset of outcomes available at the commuting zone level (Twitter sentiment and CCES survey responses), we exploit finer geographic variation. In these models, we can include state-by-year fixed-effects to control for any shocks common to all CZs within a state in a given year. Specifically, we estimate the following specification:

$$\Delta y_{zt} = \alpha_l + \tau_t + \omega \Delta B_{zt} + \Delta X'_{zt}\lambda + \Delta \varepsilon_{zt}, \tag{5}$$

Where $\Delta y_{z,t} = y_{zt} - y_{z,t-1}$, ΔB_{zt} is the change in Bartik shock at the CZ level (constructed analogously to state), $\Delta X'_{zt}$ is a vector of change in time-varying commuting zone characteristics, α_l is average state-level trends and τ_t represents residual national shocks in the outcome. Essentially, by differencing we remove any time-invariant CZ-specific factors, and we include state dummies to control for trend differences between states, along with year effects. This CZ-level analysis provides a robustness check that uses more localized variation: it answers whether within the same state, areas with larger labor demand shocks experience larger attitude changes. One downside of this approach is that CZ-level aggregation may be less precise, as we have fewer tweets and CCES respondents than at the state level, making this aggregated figure potentially less reflective of true values.

4.2 Individual-Level Outcomes

Because one goal of this paper is to compare different measures of immigration attitudes, the first part of the analysis is conducted as state level, which is the finest level of geography at which all of our main attitude measures are available. However, beyond establishing *whether* local labor demand shocks affect immigration attitudes, we also want to examine potential mechanisms. For that purpose, it is useful to exploit the rich set of individual characteristics available in the ANES and CCES together with respondents' answers to immigration-policy questions. In particular, we study whether the response to labor demand shocks differs with characteristics that are more or less salient to the three mechanisms discussed earlier: labor market competition, welfare magnet concerns,and cultural or ideological factors.

The ANES has a smaller sample and states as the finest geography in the publicly available data. By contrast, the CCES has much larger samples and provides county-level data, which we map to commuting zones. This makes it possible to estimate individual-level specifications using either state-level or commuting-zone-level labor demand shocks.

As a baseline, we estimate the following individual-level specification with state-level shocks:

$$y_{ist} = \alpha_s + \tau_t + \omega \Delta B_{st} + X'_{ist}\lambda + \epsilon_{ist}, \tag{6}$$

where y_{ist} is the immigration-attitudes outcome for individual i in state s at time t, ΔB_{st} is the change in the state-level Bartik shock, X_{ist} is a vector of individual characteristics, and α_s and τ_t are state and year effects, respectively.

For specifications using commuting-zone shocks, we estimate:

$$y_{izst} = \alpha_z + \alpha_s \times \tau_t + \omega \Delta B_{zt} + X'_{izst}\lambda + \epsilon_{izst}, \tag{7}$$

where z indexes commuting zones, ΔB_{zt} is the change in the commuting-zone Bartik shock, α_z are commuting-zone fixed effects, and $\alpha_s \times \tau_t$ are state-year effects. The inclusion of state-year effects means that identification comes from differences across commuting zones within the same state and year. Put differently, the coefficient on ΔB_{zt} is identified from whether a commuting zone that experiences a larger change in labor demand than other commuting zones in the same state-year also exhibits more pro-immigration individual responses..

4.2.1 Response Heterogeneity

We use the rich individual characteristics available in the ANES and CCES to study heterogeneity in the response to labor demand shocks. The goal is not only to estimate the average effect of local labor demand on immigration attitudes, but also to examine whether that effect is stronger for groups whose views may be more relevant to the mechanisms discussed earlier. We organize this analysis around four broad dimensions: nativity, labor-market vulnerability, views on public spending, and non-economic or cultural factors.

Overall vs. Native vs. Immigrant Attitudes

A first question is whose immigration attitudes are being captured in the survey data. Natives and immigrants may respond differently to changes in local economic conditions, and it is useful to separate these responses where the data allow. In the ANES and CCES, we can do this by classifying respondents by nativity or immigration background.[14] For Twitter, newspapers, and Google Trends, no such separation is possible. For that reason, when comparing results across different attitude measures, we use all respondents in the survey-based measures. We then separately present results for natives and for non-natives in the individual-level analysis.

Education, Employment Status, Union Membership, and Immigrant-Intensive Industries

We next examine heterogeneity along dimensions relevant to labor-market competition. Because immigrants are, on average, less skilled than natives, more highly educated respondents may be less directly exposed to labor-market competition from immigrants and may instead be complements to immigrant labor. Union members may also be less vulnerable to such competition if unions provide bargaining power or job protection. By contrast, unemployed respondents may be especially concerned about labor-market competition. Finally, respondents working in industries with high immigrant shares may be more exposed to immigrant labor and therefore more likely to respond to local labor demand shocks through a labor-market-competition channel. We examine these possibilities by interacting the Bartik shock with education, employment status, union membership, and measures of immigrant intensity in the respondent's industry.

Welfare and Public-Spending Views

Another possible mechanism is concern about the fiscal consequences of immigration. The CCES asks respondents about government spending across a range of categories, including welfare, education, healthcare, infrastructure, and police. If immigration attitudes partly reflect concerns about pressure on public resources, then responses to labor demand shocks may differ systematically with respondents' views on public spending.

[14] Specifically, ANES asks, "Were both your parents born in this country?" A "yes" answer is a conservative way to identify natives, since having two native-born parents is sufficient but not necessary for being native born. ANES also samples eligible voters only. By contrast, CCES includes non-citizens. The CCES nativity categories include immigrant citizens, immigrant non-citizens, respondents born in the United States to at least one immigrant parent, respondents whose parents are native but with at least one immigrant grandparent, and respondents whose grandparents are all native born.

We examine this by interacting the Bartik shock with respondents' spending preferences, measured on scales ranging from greatly decrease to greatly increase spending.

Non-Economic and Cultural Factors

Finally, we examine heterogeneity along dimensions more closely related to non-economic mechanisms. One possibility is that concerns about immigration partly reflect perceived cultural distance or broader ideological predispositions rather than, or in addition to, direct economic competition. We therefore test whether the response to labor demand shocks differs by ideological orientation, as more conservative respondents may be more concerned about cultural change associated with immigration. We also examine heterogeneity by race, ethnicity, and immigration background, which may proxy for differences in cultural proximity to immigrants or in how respondents perceive the social consequences of immigration.

4.3 Symmetry or Asymmetry in the Effect of Labor Demand Shocks

As an additional diagnostic, we examine whether immigration attitudes respond symmetrically to positive and negative labor-demand shocks. The baseline specifications impose a common linear slope, so that an increase in predicted labor demand of a given size has the same effect in absolute value as a decrease of the same size, but with the opposite sign. That restriction may be too strong. It is possible, for example, that worsening labor-market conditions make respondents more hostile to immigration, while improvements in labor-market conditions have a weaker effect in the opposite direction. To assess this possibility, we allow the slope on the Bartik shock to differ depending on whether the predicted labor-demand change is positive.

Specifically, we estimate:

$$\Delta y_{lt} = \alpha_l + \tau_t + \omega_1 \Delta B_{lt} + \omega_2 (\Delta B_{lt} * 1[\Delta B_{lt} > 0]) + \Delta X'_{lt}\lambda + \Delta \epsilon_{lt}, \tag{8}$$

where ω_1 gives the slope for non-positive Bartik changes and $\omega_1 + \omega_2$ gives the slope for positive Bartik changes. Under symmetry, $\omega_2 = 0$. A negative ω_2 implies that positive labor-demand shocks have a smaller effect than negative shocks of the same size, while a positive ω_2 implies the opposite. The individual-level specification is analogous.

In practice, this exercise is not very informative at the state level. State-level predicted labor-demand shocks are closely tied to national industry conditions, so differences in states' initial industry composition mainly affect the magnitude of the Bartik shock, not its sign. As a result, a state's predicted labor-demand change becomes negative only in years when national industry shocks are sufficiently negative across many industries. When that happens, the negative national component is strong enough that predicted labor-demand shocks are negative for essentially all states, not just for a particular subset of them. In our data,

this occurs in 2009, 2010, and 2020, while state Bartik shocks are positive in the other years. A state-level asymmetry test would therefore largely compare those common downturn years to the rest of the sample, rather than exploiting meaningful cross-state variation in whether the shock is positive or negative. There is substantially more variation in the sign of the Bartik shock at the commuting-zone level, so we focus the asymmetry analysis on commuting-zone shocks. At the aggregate level, we estimate this specification for the Twitter and CCES outcomes available at the commuting-zone level. At the individual level, we apply the same idea to the CCES outcomes while maintaining the baseline set of commuting-zone, state-year, and individual controls.

5 Results and Discussion

This section proceeds in five steps. We begin with our baseline state-level results using four-year changes, which allow the broadest comparison across outcome measures because ANES is only available every four years. We then show the corresponding industry-level equivalent regressions, following the shock-level approach in Borusyak et al. (2022), to assess how inference changes when standard errors are computed at the level of the shocks. Next, we examine alternative time intervals at the state level. We then turn to commuting-zone results for the outcomes that can be measured below the state level. Finally, we use the individual-level survey data to study whether the patterns seen in the aggregate data also appear when respondent-level information is retained. Throughout the geography-level analysis, both the Bartik shock and the outcomes are standardized, so the coefficients can be read as the change in the outcome, in standard deviations, associated with a one-standard-deviation increase in the Bartik shock. In the individual-level analysis, the ANES outcomes are standardized, while the CCES outcomes remain binary.

5.1 State-Level Outcomes

We begin with the baseline state-level specifications using four-year changes between 2004 and 2020. We focus on four-year intervals here because they align with the ANES sample years and therefore allow the cleanest comparison across the different measures of immigration attitudes.

Table B1 shows a clear positive relationship between labor demand shocks and several conceptually distinct measures of immigration attitudes. The coefficient on the ANES measure of preferred immigrant numbers is 1.196 with a standard error of 0.691, the coefficient on the ANES jobs question is 1.403 with a standard error of 0.424, and the coefficient on the ANES feeling thermometer is 0.632 with a standard error of 0.488. The Google-search ratio coefficient is 0.944 with a standard error of 0.389, the newspaper-sentiment coefficient is 1.098 with a standard error of 0.451, and the Twitter coefficient is 0.447 with a standard error

of 0.195. By contrast, the three CCES-based aggregate outcomes are small and imprecisely estimated: 0.154 for the border-patrol question, -0.092 for the legalization question, and 0.324 for the CCES index, with standard errors of 0.359, 0.212, and 0.306, respectively.

The main message from Table B1 is therefore not that every measure moves identically, but that several very different measures point in the same direction. A one-standard-deviation increase in the Bartik shock is associated with roughly a 0.94 to 1.40 standard deviation increase in the ANES Number, ANES Jobs, Google-search, and newspaper-sentiment outcomes. The Twitter coefficient is smaller, about 0.45 standard deviations, but still positive and statistically significant. The CCES aggregates do not show the same pattern in this baseline state-level specification.

5.2 Industry-Level Equivalent Regression

Table B2 re-estimates the state-level regressions in their industry-level equivalent form on the common sample for which the equivalence can be implemented. For the geography-level and industry-level specifications to be exactly equivalent, all states and all industries must be present in every period used in the regression. That requirement rules out the Google Trends and newspaper-sentiment outcomes, because Google Trends is not available for all states in all years and the newspaper data do not contain enough immigration-related local coverage for some smaller states. We therefore carry out this exercise only for ANES, Twitter, and CCES, and only for the years 2012, 2016, and 2020, when all states are observed for those measures.

The point estimates in Panel A and Panel B are identical by construction. In Panel A, the state-level coefficients are 1.747 for ANES Number, 0.277 for ANES Therm, 1.629 for ANES Jobs, 0.826 for Twitter, -0.0798 for CCES Legalize, 0.134 for CCES Border, and 0.281 for CCES All. Panel B reproduces these same coefficients exactly. The standard errors, however, are often smaller in the industry-level regressions. For ANES Number, the standard error falls from 0.791 to 0.498. For ANES Jobs, it falls from 0.504 to 0.309. For CCES Legalize, it falls from 0.184 to 0.121. For CCES All, it falls from 0.266 to 0.234. The standard error for CCES Border is unchanged at 0.311 in both panels, and the Twitter standard error is almost identical, 0.225 in Panel A and 0.232 in Panel B. It is, therefore, reasonable to interpret inference using the location-level standard errors as more conservative. For this reason, and for not having to apply restrictive balance sample requirements, we estimate location-level standard errors in subsequent analyses.

5.3 Smaller Time Intervals

The four-year interval used up to this point is dictated by ANES availability, which mainly follows presidential-election years. We now focus on the non-ANES measures in order to examine shorter horizons. This lets us

ask whether immigration attitudes respond to short-run movements in local labor demand and also allows us to use more of the available data. Table B3 reports two-year first0difference results, with Panel A limited to even years and Panel B using all available years. Because both panels use changes, they are directly comparable to the four-year first-difference results in Table B1.

The two-year first-difference results are much noisier than the four-year baseline results. In Panel A, which uses even years, only the search-volume ratio remains statistically significant: the coefficient is 1.628 with a standard error of 0.668. The remaining estimates are -0.228 for news, -0.800 for CCES Border, 0.847 for CCES Legalize, 0.409 for CCES All, and -0.277 for Twitter, all with large standard errors. In Panel B, which uses all years, none of the coefficients is precisely estimated. The estimates are 1.155 for search, -0.673 for news, -0.474 for CCES Border, 0.630 for CCES Legalize, 0.309 for CCES All, and -0.164 for Twitter.

Compared with the four-year results in Table B1, the shorter-horizon first-difference estimates are clearly less stable and less precise. At the four-year horizon, ANES, Google search, newspaper sentiment, and Twitter all point to a positive relationship between labor-demand shocks and pro-immigration attitudes, with coefficients ranging from 0.447 to 1.403. By contrast, at the two-year horizon only the Google-search measure remains statistically significant, and only in the even-years sample. The state-level evidence therefore suggests that the relationship is much easier to detect over four-year changes than over two-year changes. This may reflect slower adjustment in immigration attitudes, greater noise in shorter-difference specifications, or both.

Taken together, the state-level results show three things. First, at four-year horizons, ANES, Google-search, newspaper sentiment, and Twitter all point to the same substantive conclusion: more positive labor demand shocks lead to more pro-immigration attitudes. Second, in the common sample used for the industry-level equivalence exercise, the state-level standard errors are often somewhat conservative, especially for the ANES outcomes. Third, the shorter-horizon first-difference results are much nosier, and the positive pattern that is clear at four-year intervals largely disappears at two-year intervals.

5.4 Commuting Zone Level Outcomes

We next turn to commuting zones. A commuting zone is closer to a local labor market than a state, so it is a natural unit at which to study labor-demand shocks. In practice, however, only Twitter and CCES can be measured consistently at that level.

Table B4 reports the CZ-level results. The Twitter coefficients are positive in every specification, but they are not very stable in magnitude and are only weakly estimated. In the even/leap-year sample in Panel A, the coefficients are 0.110, 0.0417, and 0.306, with standard errors of 0.0648, 0.139, and 0.343. In the

all-years sample in Panel B, they are 0.110, 0.142, and 0.229, with standard errors of 0.0648, 0.128, and 0.205. So the Twitter results remain positive throughout, but only the 0.110 coefficient is even marginally statistically significant.

The CCES results at the CZ level are weaker and less consistent. For the border-patrol outcome, the coefficients are negative in every specification: -0.224, -0.316, and -1.206 in Panel A, and -0.224, -0.277, and -1.366 in Panel B. The one-year estimate is marginally significant in both panels, while the longer-horizon coefficients are much less precise. For the legalization outcome, the coefficients are positive but imprecise throughout: 0.0646, 0.238, and 0.401 in Panel A, and 0.0646, 0.0656, and 0.779 in Panel B. For the overall CCES index, the coefficients are close to zero in every case: -0.129, -0.149, and -0.222 in Panel A, and -0.129, -0.143, and 0.0415 in Panel B.

The CZ-level aggregate evidence is therefore weaker than the state-level evidence. Twitter continues to move in the expected direction, but the estimates are modest and not very precise. The CCES aggregates do not show a robust positive relationship at the CZ level, and for the border-patrol outcome the estimated sign is consistently negative. So the commuting-zone aggregate results do not strengthen the baseline state-level conclusions; if anything, they suggest that once these measures are aggregated to smaller geographies, sampling noise and limited coverage become more important.

5.5 Individual-Level Outcomes

Up to this point, the analysis has focused on comparing different measures of immigration attitudes while holding the rest of the empirical design as constant as possible, including the geographic level, time interval, and assumptions about time trends. That comparison is useful for assessing whether the main pattern is robust across conceptually distinct outcome measures. At the same time, geography-level averages have two important limitations. First, they do not use the rich set of respondent characteristics available in the survey data. Second, averaging attitudes to the geography-year level introduces additional measurement error, which can reduce precision.

We therefore next turn to individual-level regressions using the ANES and CCES. These specifications allow us to retain respondent-level variation, control directly for individual characteristics, and examine whether the aggregate patterns continue to hold once the outcomes are no longer collapsed to state or commuting-zone means. Because the response scales differ across the ANES questions, we re-express the ANES outcomes in standard deviations so that the coefficients are comparable across questions. By contrast, the main CCES outcomes are binary indicators, so we leave them in their original form. The CCES coefficients should therefore be interpreted as changes in probabilities.

We begin with regressions estimated on all respondents, which keeps this part of the analysis comparable to the earlier geography-level results. We then turn to nativity-specific estimates. This distinction is substantively important because most people in the United States are native born, and an even larger share of voters are native born. If labor-demand shocks affect immigration attitudes mainly through the responses of natives, that would be especially relevant for understanding how local economic conditions translate into political outcomes.

Table B5 begins with the ANES outcomes. In Panel A, the results are strong and very consistent across all three questions. A one-standard-deviation increase in the Bartik shock is associated with a 0.181 standard deviation increase in the ANES number-of-immigrants outcome, a 0.184 standard deviation increase in the ANES jobs outcome, and a 0.174 standard deviation increase in the feeling thermometer. The standard errors are 0.0367, 0.0403, and 0.0357, so all three coefficients are precisely estimated. Relative to the state-level aggregate ANES results, the individual-level ANES estimates are smaller in magnitude, but they are more uniform and much more precise.

Panel B presents the comparable CCES results using state-level shocks. Here the coefficients are much smaller and mostly indistinguishable from zero. For the "oppose more border patrols" outcome, the coefficients are -0.00346, -0.00156, and 0.00712 in the one-year, two-year, and four-year specifications, respectively. For "grant legal status," the coefficients are -0.00256, 0.000423, and 0.00391. For the overall CCES index, the coefficients are 0.00170, -0.00295, and 0.0204. Only the last of these is even marginally significant at the 10 percent level. So, when shocks are measured at the state level, the individual-level CCES regressions look much like the state-level CCES aggregate regressions: most effects are small and imprecisely estimated.

Panel C then uses the more localized commuting-zone-level shocks. Here the results are more supportive, though still not as uniformly strong as in the ANES. For the border-patrol outcome, the coefficients are 0.00824, 0.00646, and 0.00791 in the one-year, two-year, and four-year specifications. The one-year and four-year estimates are marginally significant, while the two-year estimate is not. For legalization, the coefficients are 0.00283, 0.00369, and 0.00464, all small and imprecisely estimated. For the overall CCES index, the coefficients are 0.00865, 0.00794, and 0.0138, with the four-year estimate statistically significant at the 5 percent level. Because these outcomes are binary, the coefficient of 0.0138 implies that a one-standard-deviation increase in the Bartik shock is associated with about a 1.4 percentage point increase in the pro-immigration CCES index. Thus, the individual-level CCES evidence is not uniformly strong, but it is clearly more supportive when shocks are measured at the commuting-zone rather than state level. The strongest CCES patterns are for the border-patrol outcome and the overall CCES index, while the legalization question remains weak throughout.

Taken together, the individual-level results show three things. First, all three ANES questions produce

more pro-immigration responses as local labor demand shocks become more positive. Second, the CCES results are much weaker when shocks are measured at the state level. Third, once shocks are measured at the commuting-zone level, there is evidence of a positive relationship for the border-patrol outcome and the overall CCES index, but not for the legalization question. The overall pattern is therefore similar to what we saw in the geography-level analysis: ANES gives the clearest and most stable evidence, while CCES is more sensitive to geography and outcome definition.

We next split the individual-level sample by nativity. This is substantively important because natives make up the overwhelming majority of respondents and voters, so the full-sample estimates are likely to reflect native responses most strongly.

In Table B6 which restricts the sample to natives, the ANES results remain very similar to those for the full sample and are, if anything, somewhat larger. The coefficients are 0.191 for ANES Number, 0.224 for ANES Jobs, and 0.210 for the feeling thermometer, with standard errors of 0.0545, 0.0323, and 0.0476. All three are highly statistically significant. In Panel B, the state-level CCES results remain weak: the border-patrol coefficients are -0.00535, -0.00274, and 0.00849; the legalization coefficients are -0.00426, -0.00128, and 0.00277; and the overall-index coefficients are 0.000274, -0.00463, and 0.0202. None is statistically distinguishable from zero. Panel C again shows that the results become more supportive when shocks are measured at the commuting-zone level. For the border-patrol outcome, the coefficients are 0.00828, 0.00449, and 0.00811, with the one-year estimate marginally significant and the four-year estimate significant at the 5 percent level. For legalization, the coefficients are 0.00333, 0.00261, and 0.00378, all small and imprecise. For the overall CCES index, the coefficients are 0.00843, 0.00633, and 0.0137, with the four-year coefficient significant at the 5 percent level. So for natives, the pattern is essentially the same as in the full sample: strong and precise ANES effects, little evidence in state-level CCES, and more supportive evidence for the border-patrol item and overall CCES index when shocks are measured at the commuting-zone level.

Table B7 restricts the sample to immigrants. Here the ANES estimates become much noisier. The coefficient on ANES Number is 0.193 with a standard error of 0.110, so it is only marginally significant. The ANES Jobs coefficient is essentially zero, 0.000172, with a standard error of 0.0701, and the thermometer coefficient is 0.0934 with a standard error of 0.104. In Panel B, the state-level CCES estimates are again mostly imprecise, though the one-year legalization coefficient is 0.0275 with a standard error of 0.0141 and is marginally significant. The most notable immigrant results appear in Panel C, which uses commuting-zone shocks. For the border-patrol outcome, the coefficients are 0.00647, 0.0387, and 0.0179, with only the two-year estimate marginally significant. For legalization, the coefficients are 0.0150, 0.0455, and 0.0465, and the two-year and four-year coefficients are statistically significant at the 1 percent level. For the overall CCES index, the coefficients are 0.0349, 0.0610, and 0.0577, with the two-year and four-year coefficients

significant at the 5 percent level. These are economically meaningful effects: for example, the coefficient of 0.0610 implies about a 6.1 percentage point increase in the pro-immigration CCES index for a one-standard-deviation increase in the Bartik shock. The immigrant sample is much smaller than the native sample, so the greater imprecision in ANES is not surprising. But the CZ-level CCES results suggest that immigrant attitudes may respond quite strongly to local labor-demand shocks, especially for legalization and the broader CCES index.

Overall, the nativity split suggests that the full-sample results are driven mainly by natives, which is unsurprising given their much larger share of the sample. At the same time, the immigrant-only sample does not simply show no response. Rather, the immigrant results are noisier in ANES but quite strong for several CZ-level CCES outcomes, especially legalization and the overall CCES index.

Alternative CCES Outcomes We have so far focused on the two main CCES questions that appear most consistently over time, along with the overall CCES index. However, the border-patrol and legalization results do not always move together. One reason may be that the legalization question includes important conditions: holding a job, paying taxes for three years, and not being convicted of a felony, which changes the reference point from immigration more broadly to a narrower group of "deserving" immigrants. To assess whether the earlier CCES findings are specific to those two questions, Table B8 examines a broader set of CCES immigration outcomes, including the average of the border and legalization questions, the overall index, and an index of all questions other than the border question.

Panel A of Table B8 looks at one-year Bartik changes for the core CCES questions. The border-patrol coefficient is 0.00882 with a standard error of 0.00472, so it is marginally significant. The legalization coefficient is 0.00333 with a standard error of 0.00465 and is not significant. The average of the border and legalization questions is 0.0119 with a standard error of 0.00761, the overall index is 0.00843 with a standard error of 0.00666, and the index excluding the border question is 0.00741 with a standard error of 0.00691; none of these is precisely estimated. The remaining one-year outcomes in Panel A are also imprecise. Thus, at the one-year horizon, the clearest positive result remains the border-patrol question itself.

Panel B extends the one-year analysis to additional CCES outcomes that are available less consistently. Here the pattern is mixed. The coefficient on the reverse-coded item about prohibiting services to undocumented immigrants is 0.00909 with a standard error of 0.0056 and is marginally significant. The coefficient on the reverse-coded item about denying automatic citizenship is 0.0893 with a standard error of 0.0411 and is significant at the 5 percent level. On the other hand, the coefficient on the reverse-coded "build a wall" item is -0.0173 with a standard error of 0.00733 and is significant at the 1 percent level, which goes in the less pro-immigration direction after the coding reversal. The remaining one-year coefficients in Panel B are not precisely estimated. So the broader one-year CCES evidence is mixed rather than uniformly positive.

Panels C and D repeat the same exercise using four-year Bartik changes. The results are stronger and more coherent than in the one-year case. In Panel C, the border-patrol coefficient is 0.00810 with a standard error of 0.00397 and is significant at the 5 percent level. The average of the border and legalization questions is 0.0122 with a standard error of 0.00592 and is also significant. The overall index is 0.0137 with a standard error of 0.00582, and the index excluding the border question is 0.0142 with a standard error of 0.00584; both are significant at the 5 percent level. By contrast, the legalization coefficient remains small and imprecise at 0.00378 with a standard error of 0.00349. In Panel D, the pattern is again mixed at the level of individual items, but a few coefficients stand out: the reverse-coded "build a wall" item has a coefficient of -0.0238 with a standard error of 0.00933, the "more guest workers" item has a coefficient of 0.0364 with a standard error of 0.0208 and is marginally significant, and the "do not build the wall" item has a coefficient of 0.0598 with a standard error of 0.0298 and is significant at the 5 percent level. The other outcomes remain imprecisely estimated.

The broad conclusion from Table B8 is therefore cautious but informative. The positive CCES results are not driven solely by the border-patrol question, because the broader CCES indexes also become positive and statistically significant in the four-year specifications. At the same time, the evidence is not uniform across all individual CCES items, and some specific outcomes move weakly or even in the opposite direction. The most stable positive CCES patterns remain the border-patrol question and the broader indexes, especially at the four-year horizon. The legalization question continues to stand out as the least responsive of the main CCES outcomes.

5.5.1 Heterogeneity and Mechanisms

The results so far point to a broadly positive relationship between labor-demand shocks and pro-immigration attitudes. We now ask whether that relationship is stronger for groups whose views should be more sensitive to particular mechanisms. This exercise is not meant to identify mechanisms causally. Rather, it asks whether the estimated response to labor-demand shocks varies across groups in ways that are consistent with the labor-market-competition, welfare-concern, and cultural-concern channels discussed earlier. To keep the presentation manageable, Tables B9–B11 use the average of all CCES immigration-policy questions as the outcome and focus on the commuting-zone individual-level specification. In each case, the coefficient on D.Bartik gives the slope for the omitted group, and the interaction terms show how the slope differs for the other groups.

Mechanisms: Labor Market Competition

Labor market competition theory suggest that competition with immigrants should concern those natives who are more likely to be affected. On average, immigrants have a lower level of education than natives,

which is especially true of those affected by policies aimed at curbing illegal immigration, and present more direct competition for lower skilled natives. Table B9 examines whether the response to labor-demand shocks is stronger for groups that may face more direct labor-market competition from immigrants. The evidence is partly consistent with that channel. For brevity, we present the results with just the index of all CCES questions, but the results are similar focusing on just the border question.

Columns (1) and (2) show heterogeneity by education. The omitted category is respondents with no high school education. For that group, a one-standard-deviation increase in the Bartik shock raises the CCES immigration index by 0.0162 in the one-year specification and 0.0090 in the four-year specification; the one-year effect is marginally significant, while the four-year effect is not. Relative to this omitted group, respondents with high school education do not differ meaningfully: the interaction coefficients are -0.0007 and 0.0058. Those with some college also look similar in the one-year specification, but are somewhat more responsive in the four-year specification, with an interaction of 0.0118. The main contrast is at the top of the education distribution. Respondents with a four-year degree have a one-year slope that is 0.0122 lower than the no-high-school group, and postgraduates have slopes that are 0.0217 lower in the one-year specification and 0.0145 lower in the four-year specification. So the clearest pattern is that the most educated respondents respond less positively to labor-demand shocks than the least educated, especially in the one-year results.

Columns (3) and (4) consider employment status. The omitted group is full-time workers. For them, the Bartik coefficient is 0.0153 in the one-year specification and 0.0204 in the four-year specification, with the latter statistically significant. Relative to full-time workers, part-time workers are not meaningfully different: the interaction terms are 0.0051 and 0.0001. Respondents temporarily laid off are substantially less responsive, with interaction terms of -0.0242 and -0.0299. The unemployed are not different from full-time workers in the one-year specification, but in the four-year specification their slope is 0.0080 larger. So the employment-status results do not show a simple monotonic pattern, but they do suggest that those temporarily laid off react less positively to better labor-demand conditions than the full-time employed.

Columns (5) and (6) turn to union membership. The omitted group is respondents who are currently union members. Their estimated slope is slightly negative: -0.0046 in the one-year specification and -0.0135 in the four-year specification, with the four-year coefficient marginally significant. Relative to current union members, former union members are more responsive, with interaction terms of 0.0099 and 0.0231, and respondents who were never union members are even more responsive, with interaction terms of 0.0157 and 0.0299. This implies that the positive response to labor-demand shocks is concentrated among those without current union protection, which is consistent with a labor-market-competition story.

Columns (7) and (8) examine whether the effect is stronger for respondents working in industries with larger immigrant shares. Here the interaction is positive and statistically significant at both horizons:

0.00145 in the one-year specification and 0.00418 in the four-year specification. This means that respondents employed in more immigrant-intensive industries show a stronger positive relationship between labor-demand shocks and immigration attitudes. That pattern is again consistent with labor-market competition playing some role.

Taken together, Table B9 offers suggestive, though not decisive, evidence for the labor-market-competition channel. The strongest support comes from the union-membership and industry-immigrant-share results, and to a lesser extent from the weaker response among postgraduates. The employment-status results are more mixed, so the table should be read as broadly consistent with the competition mechanism rather than as a clean test of it.

Mechanisms: Welfare and Public Spending Concerns

Table B10 asks whether the response to labor-demand shocks differs by respondents' broader views about government spending. If immigration attitudes partly reflect concerns about competition for publicly provided resources, then one might expect stronger responses among respondents who are already more skeptical of public spending.

That is broadly what the table shows for welfare, healthcare, education, and the spending-versus-tax tradeoff. In columns (1) and (2), the omitted group is respondents who want to greatly increase welfare spending. For them, the Bartik coefficient is negative: -0.0585 in the one-year specification and -0.0768 in the four-year specification. Relative to that group, respondents who want to maintain welfare spending have slopes that are 0.0716 and 0.106 higher; those who want to slightly decrease welfare spending have slopes that are 0.112 and 0.150 higher; and those who want to greatly decrease welfare spending have slopes that are 0.108 and 0.135 higher. So the more anti-spending the respondent is on welfare, the more positive the response to better labor-demand conditions.

The same pattern appears for healthcare and education. For healthcare, the omitted "greatly increase spending" group has coefficients of -0.0515 and -0.0641, while the interaction terms rise steadily as respondents become less supportive of healthcare spending: 0.0453 and 0.0711 for "slightly increase," 0.123 and 0.166 for "maintain," 0.118 and 0.158 for "slightly decrease," and 0.0952 and 0.119 for "greatly decrease." For education, the omitted group again has negative coefficients, -0.0346 and -0.0282, and the interactions become more positive as support for education spending falls: 0.0203 and 0.0328, 0.0805 and 0.102, 0.106 and 0.133, and 0.0991 and 0.120. These are large and precisely estimated differences.

Infrastructure is the exception. In columns (7) and (8), the omitted group has coefficients close to zero, -0.0044 and 0.0173, and most of the interaction terms are small and statistically insignificant. So the general public-spending pattern is much weaker for infrastructure than for welfare, healthcare, and education.

Police is different still. In columns (9) and (10), the omitted group, those who want to greatly increase

police spending, has positive and significant Bartik coefficients of 0.0733 and 0.103. The interaction terms then become more negative as support for police spending falls: -0.0346 and -0.0405 for "slightly increase," -0.103 and -0.125 for "maintain," -0.212 and -0.286 for "slightly decrease," and -0.248 and -0.345 for "greatly decrease." So for police spending, the pattern goes in the opposite direction: respondents who want more police spending respond more positively to labor-demand shocks.

Finally, columns (11) and (12) summarize views on the broader spending-versus-tax tradeoff. The omitted group is respondents who prefer tax increases over spending cuts, and for them the Bartik coefficient is negative: -0.0383 and -0.0156. Respondents who prefer spending cuts have interaction terms of 0.0643 and 0.0402, implying a substantially more positive response to labor-demand shocks.

Overall, Table B10 is consistent with a welfare-concern mechanism, but with an important qualification. The clearest pattern appears for attitudes toward welfare, healthcare, education, and the general spending-versus-tax tradeoff. Infrastructure does not show the same pattern, and police looks different, perhaps because respondents may view police less as a welfare expenditure and more as a law-and-order issue. As with the labor-market-competition results, these differences should be interpreted as suggestive rather than causal, since spending views are themselves correlated with ideology and other underlying characteristics.

Mechanisms: Non-Economic and Cultural Concerns

Table B11 turns to factors more closely related to ideology, politics, race, and immigration background. If cultural or identity-based concerns matter, we would expect the response to labor-demand shocks to differ systematically across these dimensions.

Columns (1) and (2) show heterogeneity by self-reported ideology. The omitted group is very liberal respondents. For them, the Bartik coefficient is negative: -0.0098 in the one-year specification and -0.0157 in the four-year specification. Relative to very liberals, liberals are even less responsive in the one-year specification, with an interaction of -0.0128, though slightly more responsive in the four-year specification, with an interaction of 0.0070. Moderates are much more positive than very liberals, with interactions of 0.0097 and 0.0162. Conservatives and very conservatives are substantially more responsive still. Conservatives have interaction terms of 0.0421 and 0.0406, and very conservatives have 0.0397 and 0.0341. These are large differences, implying that the response to labor-demand shocks is far more positive among conservatives than among very liberals.

Columns (3) and (4) provide a related result using a broader measure of conservative political views. The omitted group has coefficients of essentially zero, 0.0005 and 0.0094, while respondents with conservative views have interaction terms of 0.101 and 0.0594. This is one of the largest and clearest heterogeneity patterns in the table. It suggests that the positive association between labor-demand shocks and pro-immigration attitudes is concentrated much more heavily among respondents with conservative predispositions.

Columns (5) and (6) examine race. The omitted group is white respondents. Their coefficients are 0.0081 and 0.0219, with the four-year coefficient statistically significant. Relative to whites, Black respondents are slightly less responsive in the one-year specification, with an interaction of -0.0069, and substantially less responsive in the four-year specification, with an interaction of -0.0433. Hispanic respondents are more responsive than whites in the one-year specification, with an interaction of 0.0091, but less responsive in the four-year specification, with an interaction of -0.0273. Respondents in the "other" race category (which includes Asian, Native American, Middles Eastern, Mixed Races and other respondents who do not identify as White, Black or Hispanic) are more responsive than whites at both horizons, with interactions of 0.0110 and 0.0137. So the race results are mixed and do not line up cleanly with a simple cultural-concerns story.

Columns (7) and (8) look at immigration background. The omitted group is natives without recent immigrant ancestry. Their coefficients are 0.0068 and 0.0168. Relative to this group, non-natives are more responsive in the one-year specification, with an interaction of 0.0130, but much less responsive in the four-year specification, with an interaction of -0.0323. Natives with recent immigrant ancestry do not differ meaningfully from natives without recent immigrant ancestry. This again points to a mixed pattern rather than a simple one.

Overall, Table B11 provides the clearest support for a cultural or ideological mechanism through ideology and political leaning. Conservatives and respondents with conservative views respond much more strongly to labor-demand shocks than very liberals or non-conservatives. By contrast, the race and immigration-background patterns are more complex and do not fit neatly into a single cultural-concerns interpretation.

Taken together, the heterogeneity results suggest that no single mechanism explains all of the patterns in the data. The strongest evidence for labor-market competition comes from union membership and immigrant industry exposure. The strongest evidence for welfare concerns comes from respondents' views on welfare, healthcare, education, and spending cuts versus tax increases. The clearest evidence for non-economic concerns comes from ideology and broader conservative political orientation. The results are therefore most consistent with multiple mechanisms operating at the same time.

5.6 Symmetry/Asymmetry in Bartik Shock Effect

A final question is whether immigration attitudes react more strongly to deteriorating labor-market conditions than to improving ones. Table B12 reports the aggregate commuting-zone results, and Table B13 reports the corresponding individual-level CCES estimates. In both tables, the coefficient on ΔB gives the slope for non-positive Bartik changes, while the interaction term shows how that slope changes when the Bartik shock is positive.

The aggregate CZ-level evidence in Table B12 provides little support for strong asymmetry. For Twitter, the baseline slopes for non-positive shocks are 0.100, 0.143, and 0.228 in the one-year, two-year, and four-year specifications, respectively, while the interaction terms are -0.0946, -0.129, and 0.00171. None of those interaction terms is statistically distinguishable from zero, so the Twitter results are consistent with a roughly symmetric response. The implied slopes for positive shocks are therefore close to zero in the one- and two-year specifications and about 0.230 in the four-year specification.

For the CCES border outcome in Table B12, the non-positive-shock slope is negative in all three specifications: -0.248, -0.446, and -1.331. The interaction term is negative and statistically significant only in the one-year specification, where it equals -0.260. This implies a one-year positive-shock slope of about -0.508, compared with -0.248 for non-positive shocks. In the two-year and four-year specifications, however, the interaction terms are 0.284 and 0.527 and are not precisely estimated. So even for the border outcome, the aggregate evidence for asymmetry is limited and not stable across horizons.

For the CCES legalization outcome, the non-positive-shock slopes are 0.0609, 0.322, and 0.374, and the interaction terms are -0.0409, -0.432, and 1.609. None of these interaction terms is statistically significant. For the CCES overall index, the non-positive-shock slopes are -0.139, -0.146, and 0.0317, while the interaction terms are -0.126, -0.0192, and -0.00216, again all imprecisely estimated. Taken together, Table B12 suggests that at the aggregate CZ level there is very little systematic evidence that positive and negative labor-demand shocks have meaningfully different effects. The main exception is the one-year border result, and even there the broader pattern is not stable across horizons.

The individual-level evidence in Table B13 is more informative, but also more mixed. For the CCES border outcome, the slope for non-positive shocks is 0.0206 in the one-year specification, 0.0121 in the two-year specification, and -0.0132 in the four-year specification. The corresponding interaction terms are -0.0122, -0.00885, and 0.0269. The one-year interaction is statistically significant at the 5 percent level, and the four-year interaction is highly significant. These estimates imply that for one-year shocks, the positive-shock slope is about 0.0084, smaller than the non-positive-shock slope of 0.0206. For two-year shocks, the implied positive-shock slope is about 0.0033, again smaller than the non-positive-shock slope, though the interaction is not precisely estimated. For four-year shocks, however, the pattern reverses: the implied positive-shock slope is about 0.0137, while the non-positive-shock slope is -0.0132. So for the border outcome, shorter-horizon results suggest that positive shocks have weaker effects than non-positive shocks, whereas the four-year results point in the opposite direction.

For the legalization outcome, the individual-level evidence is much weaker. The non-positive-shock slopes are 0.00755, 0.00687, and 0.00711, and the interaction terms are -0.00417, -0.00498, and -0.00420. None of these interaction terms is statistically distinguishable from zero. The implied positive-shock slopes are

therefore only slightly smaller than the non-positive-shock slopes, and in all cases the differences are too imprecisely estimated to support a strong conclusion. This is consistent with the broader pattern in the paper, where the legalization question is generally the least responsive of the main CCES outcomes.

For the overall CCES index, the slope for non-positive shocks is 0.0162 in the one-year specification, 0.0166 in the two-year specification, and -0.00303 in the four-year specification. The interaction terms are -0.00762,-0.0115, and 0.0202. The one-year and two-year interaction terms are negative but imprecise, while the four-year interaction is positive and marginally significant. The implied positive-shock slopes are about 0.0086, 0.0051, and 0.0172. So, as with the border outcome, the short-horizon estimates suggest smaller effects of positive than of non-positive shocks, while the four-year estimates suggest the reverse.

The most sensible reading of Tables B12 and B13 is therefore cautious. The aggregate CZ-level provides little evidence that the effects of positive and negative labor demand shocks differ systematically. At the individual level, the shorter-horizon CCES results are somewhat more consistent with the idea that worsening labor-market conditions matter more than improving ones, since the one-year and, to a lesser extent, two-year interaction terms tend to be negative. The four-year interactions, however, often go in the opposite direction. A likely reason is that negative Bartik changes are concentrated around major downturns, especially the Great Recession and the first year of the pandemic. Four-year changes can therefore remain negative well into the recovery, even when one-year and two-year changes have already turned positive. For that reason, the one-year and two-year estimates are probably more informative about whether acute labor-market deterioration has stronger attitudinal effects than comparable improvements. On that basis, the individual-level evidence is somewhat more consistent with stronger responses to negative than to positive shocks, but the results are not stable enough across horizons to support a sharp conclusion.

6 Conclusion

There is growing evidence that economic shocks can shape a wide range of preferences and beliefs (Giuliano and Spilimbergo, 2025). This paper contributes to that literature by examining how local labor demand shocks affect immigration attitudes in the United States. Using Bartik-style variation across states and commuting zones, and restricting the main causal analysis to the pre-pandemic period (2004 – 2020), when the Bartik measure clearly tracks realized employment, we find a broad and fairly consistent pattern: more positive labor demand shocks lead to more favorable attitudes toward immigrants.

A central contribution of the paper is that this conclusion does not depend on a single measure of immigration attitudes or a single empirical specification. In the state-level analysis, we study survey responses from the ANES and CCES, as well as local newspaper sentiment, Google search behavior, and Twitter/X

content. Our baseline specification is a state-level first-difference model using four-year changes, which allows the broadest comparison across measures because the ANES is only available every four years. In that baseline design, the ANES outcomes, Google-search measure, newspaper-sentiment measure, and Twitter measure all point in the same direction: places experiencing more positive labor demand shocks become more pro-immigration. That agreement across multiple sources is one of the most important takeaways of the paper. It makes it much less likely that the main result is driven by the wording of one survey question, the noise of one online platform, or one particular way of measuring attitudes.

The rest of the paper then shows where that result is strongest, where it is weaker, and what else we can learn from the data. We first show that, in the common sample where the industry-level equivalent regressions can be estimated, the geography-level standard errors are often somewhat conservative rather than too small. We then examine shorter horizons and find that the state-level two-year first-difference results are much noisier than the baseline four-year results, suggesting that the attitudinal response is easier to detect over longer horizons. Next, we move to commuting zones, which are closer to actual labor markets, but find that the aggregate commuting-zone results are less stable than the baseline state-level results. Finally, we turn to individual-level survey data, which allow us to retain respondent-level information, use individual controls, and study heterogeneity directly.

The individual-level survey results sharpen the picture further. In the ANES, the evidence is especially clear: more positive labor demand shocks are associated with more pro-immigration responses across all three major questions. In the CCES, the pattern is weaker when shocks are measured at the state level, but becomes more supportive when shocks are measured at the commuting-zone level. There, the most consistent positive responses appear for the border-patrol question and for broader CCES indices, while the legalization question is repeatedly the weakest of the main CCES outcomes. It suggests that local economic conditions do not move every dimension of immigration opinion in the same way. More general and enforcement-related attitudes appear more responsive than attitudes toward conditional legalization.

The heterogeneity results show that the effect of labor demand shocks is not the same for everyone. One important dimension is nativity. The full-sample estimates are driven mainly by natives, which is unsurprising given that natives make up the overwhelming majority of respondents and voters. For natives, the ANES evidence remains strong, and the CCES results broadly resemble those in the full sample. For immigrants, the ANES estimates are much noisier because the sample is smaller, but several of the commuting-zone CCES outcomes still show sizable positive responses to labor demand shocks. So the paper does not support a simple story in which only natives respond to local economic conditions. Rather, the groups differ in where the evidence is strongest and how precisely it can be estimated.

More broadly, the heterogeneity analysis suggests that no single mechanism explains all of the results.

The response to labor demand shocks is stronger among some groups for whom labor market competition should be more salient, including non-union respondents and workers in more immigrant-intensive industries. It is also stronger among respondents who are more skeptical of public spending and among respondents with more conservative ideological views. Taken together, these findings point to a layered interpretation of immigration attitudes. Economic conditions matter, but they matter through groups that bring different vulnerabilities, policy concerns, and prior beliefs to the issue. Labor market competition, welfare concerns, and broader ideological or cultural orientations all seem to play some role.

The asymmetry analysis adds another important layer to the story. At the aggregate commuting-zone level, there is little stable evidence that positive and negative labor demand shocks have systematically different effects. At the individual level, there is some indication that short-run negative shocks may matter more than short-run positive shocks, but that pattern is not stable across horizons. The safest conclusion is therefore modest: there may be some asymmetry, especially around sharp downturns, but the evidence is not strong enough to make that the central result of the paper.

The broader substantive conclusion is that immigration sentiment is pro-cyclical. When local labor markets strengthen, attitudes toward immigrants generally become more favorable. When local labor markets weaken, attitudes often move in the opposite direction. This matters because debates over immigration are often framed as if economic and non-economic explanations compete with one another. Our results suggest that this is too simple. Economic conditions do not replace cultural or political factors. They interact with them. People appear to interpret immigration through the lens of local economic security, but that interpretation is filtered through education, union status, spending preferences, ideology, and other preexisting attitudes.

For policymakers, the main lesson is not that stronger labor markets alone will quickly solve immigration politics. Our strongest and most consistent evidence comes from the baseline four-year state-level design, which suggests that market-driven changes in attitudes may take time to appear. If policymakers want to improve sentiment toward immigration, supporting strong labor markets is likely part of the answer, but it is probably not enough on its own and may not work quickly by itself. A more practical implication of the heterogeneity results is that policy should also focus directly on the vulnerabilities that make immigration attitudes more sensitive to economic conditions. That includes reducing economic insecurity among workers who are more exposed to labor-market competition, easing pressure on local public services where fiscal concerns are salient, and addressing the political and informational environments in which economic shocks are interpreted. In other words, better labor markets likely help, but faster progress in public sentiment may require directly addressing the specific sources of insecurity and concern that the paper identifies.

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7 Appendix A: Descriptive Statistics and Diagnostics Tables and Figures

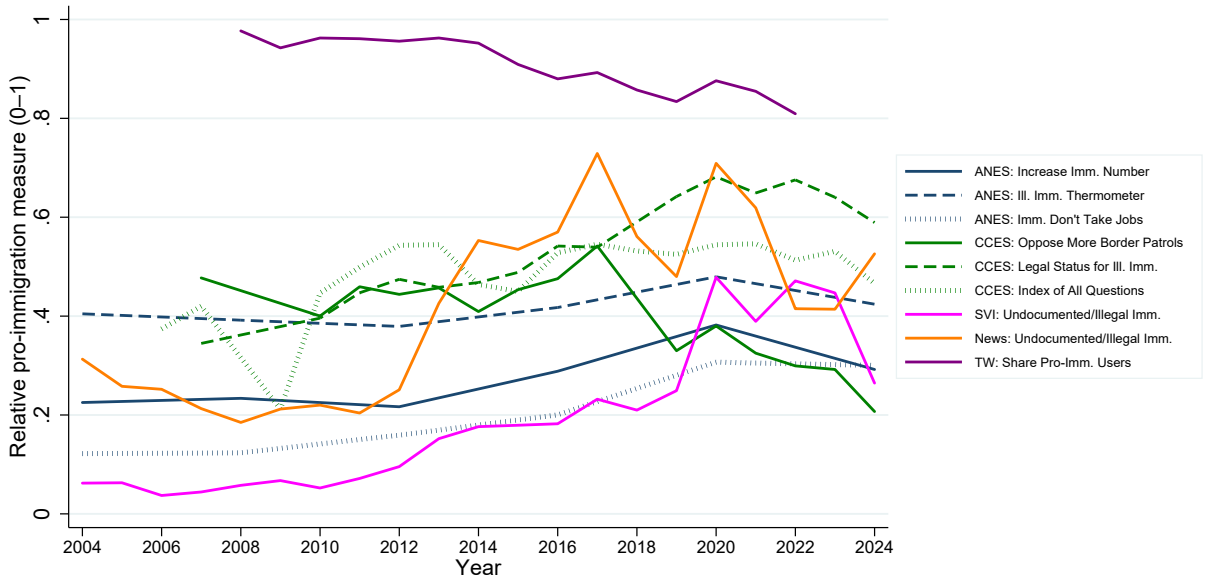


Figure 1: Immigration Sentiment Over Time

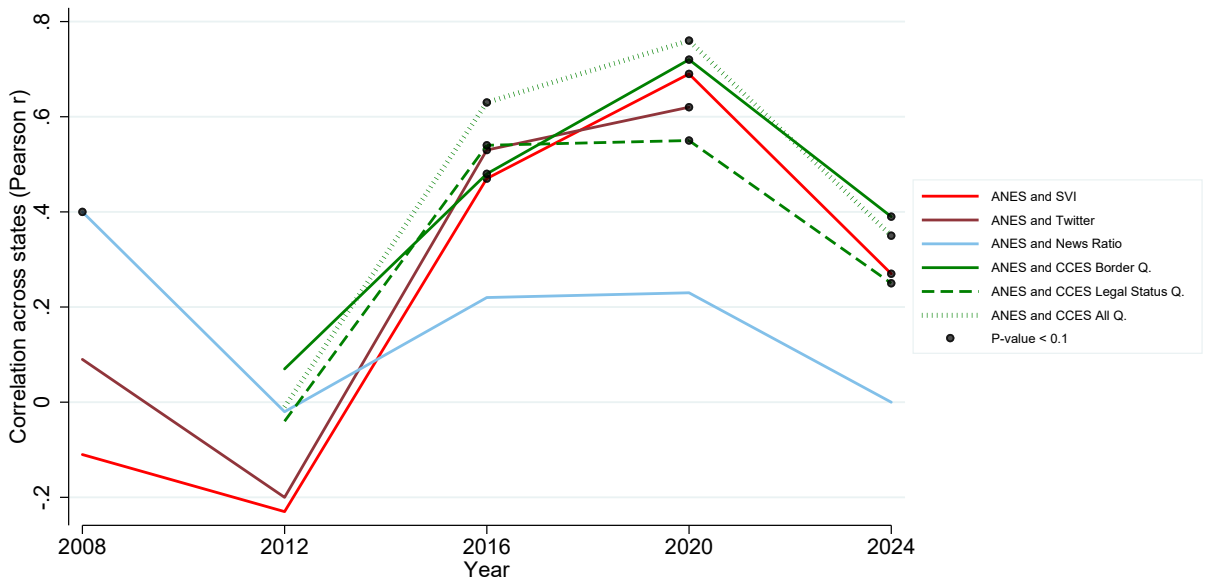


Figure 2: State-level Correlation of ANES (Increase Immigration Level) and Other Measures

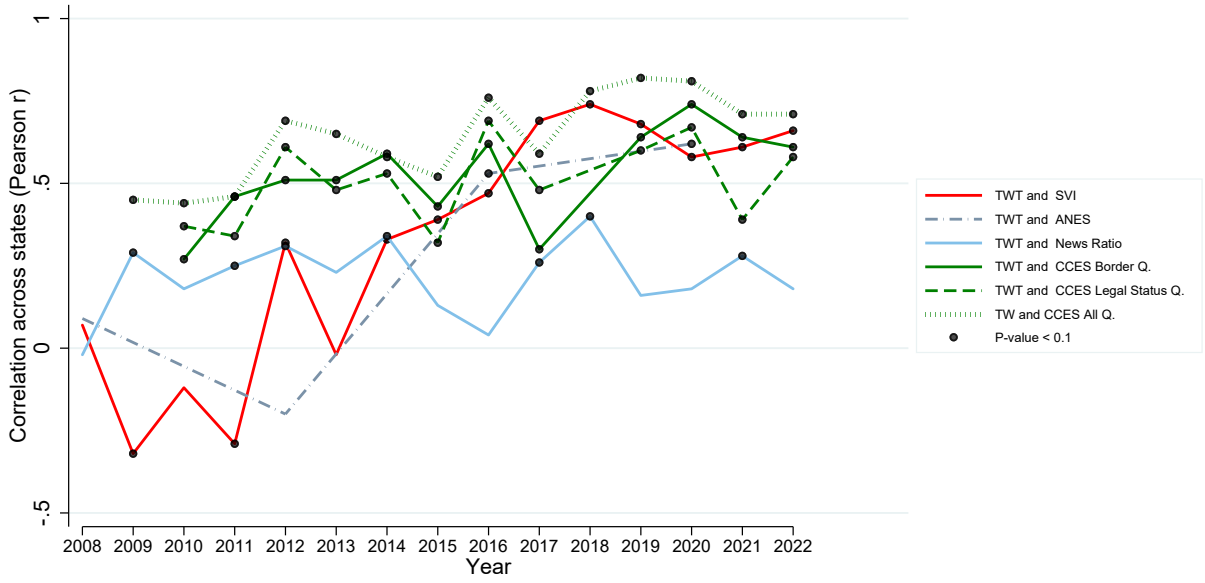


Figure 3: State-level Correlation of Twitter Sentiment and Other Measures

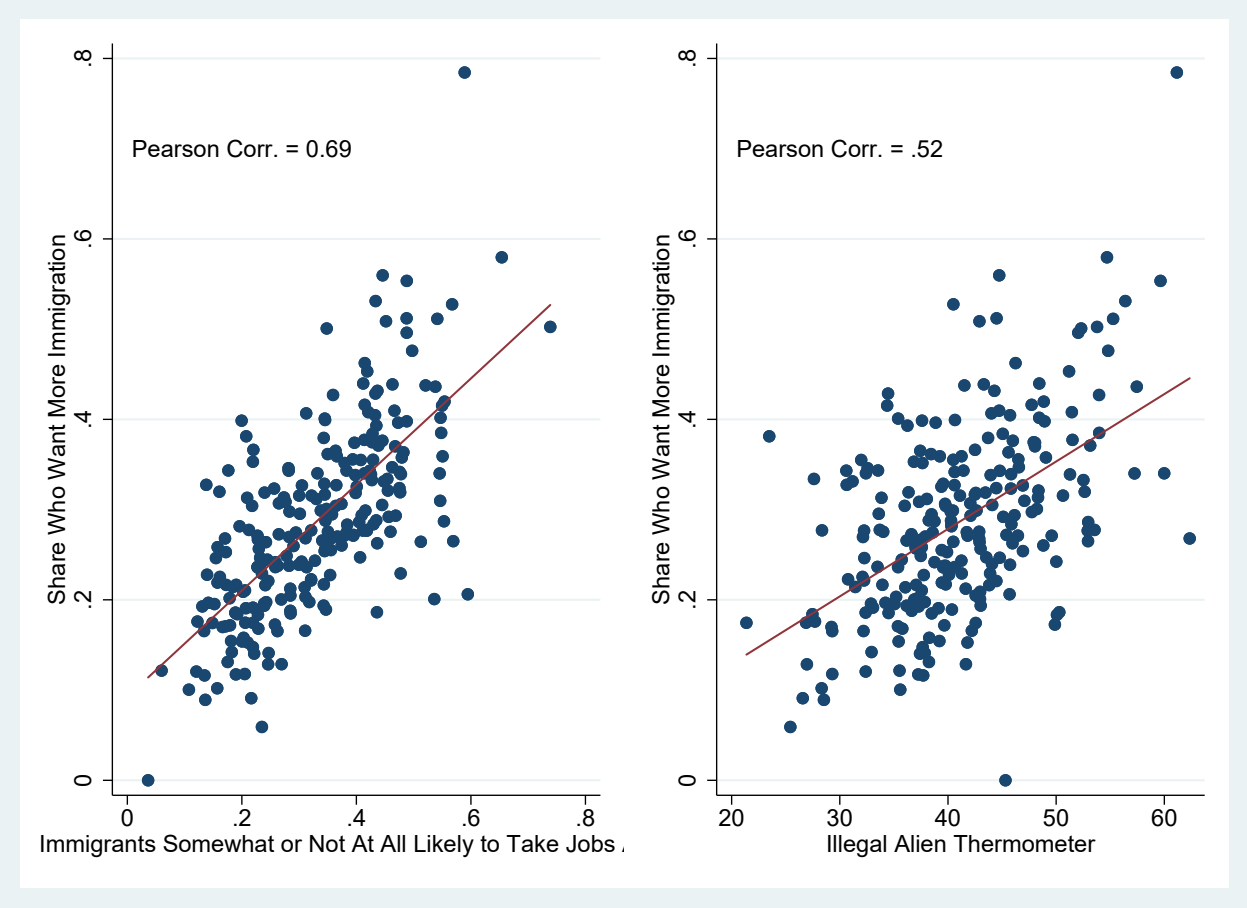


Figure 4: State-level Correlation of ANES Measures

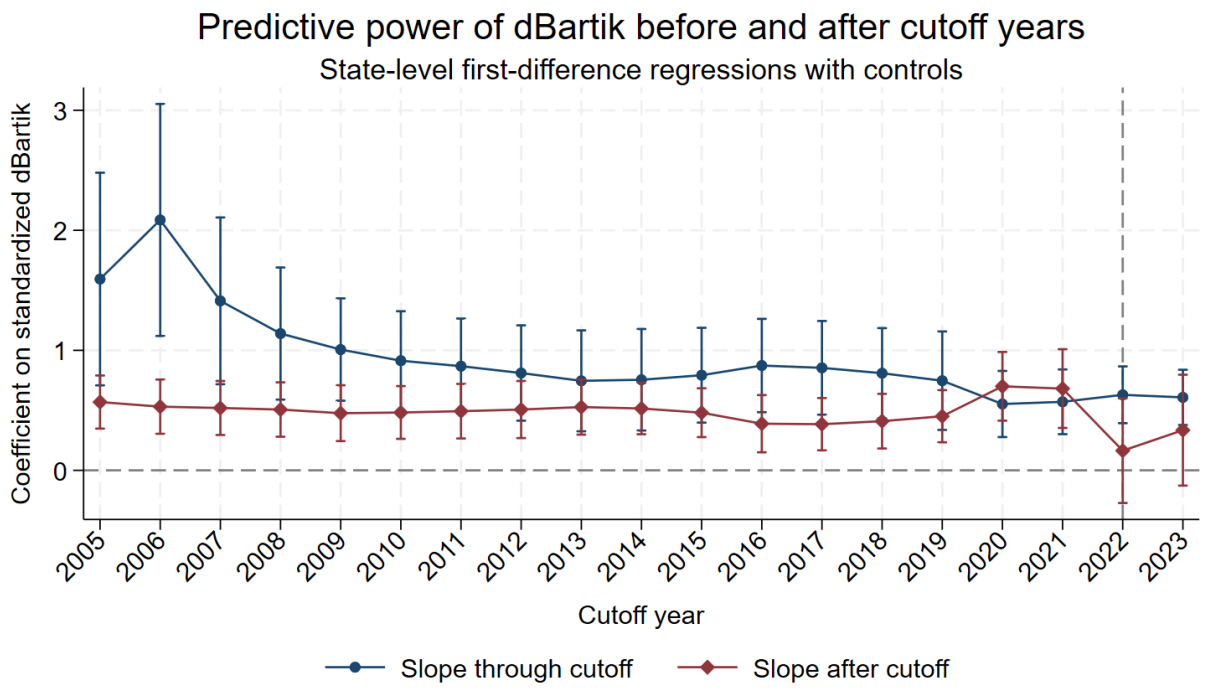


Figure 5: Immigration Sentiment Over Time

Notes: Each point shows the estimated coefficient from a regression of changes in log employment on changes in the Bartik shock, using data before or after the indicated cutoff year. Vertical lines denote 95% confidence intervals. Regressions include year fixed effects and demographic controls. Standard errors are clustered at the state level.

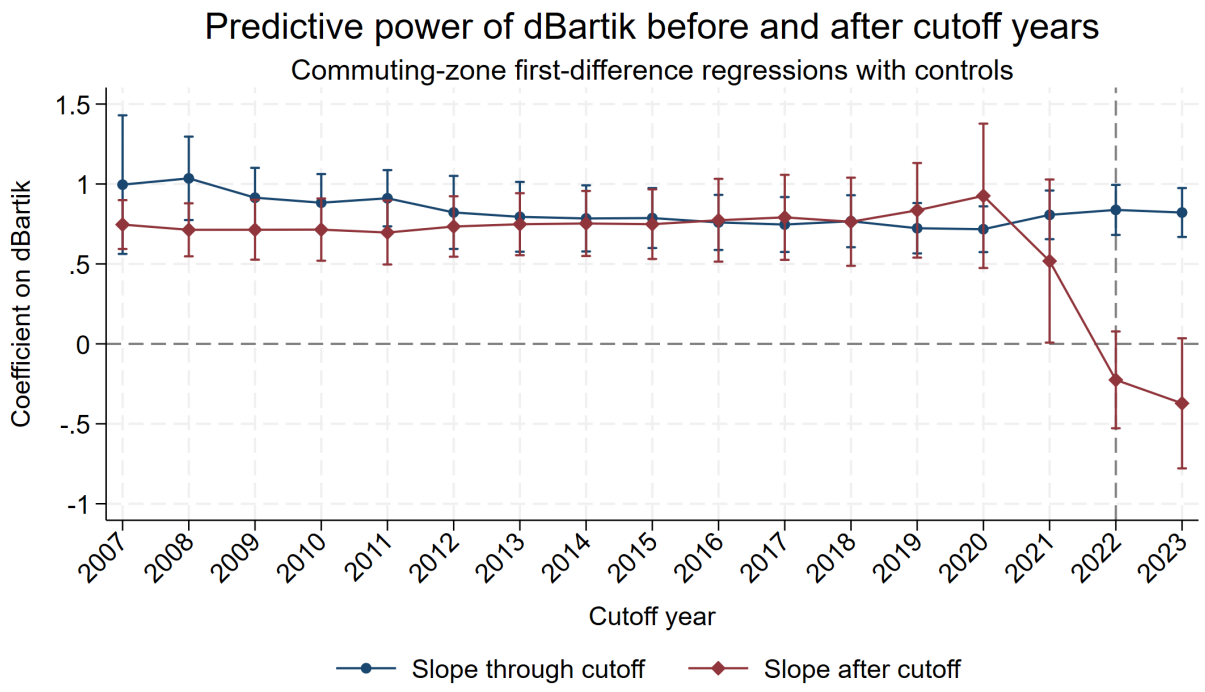


Figure 6: Immigration Sentiment Over Time

Notes: Each point shows the estimated coefficient from a regression of changes in log employment on changes in the Bartik shock, using data before or after the indicated cutoff year. Vertical lines denote 95% confidence intervals. Regressions include year fixed effects and demographic controls. Standard errors are clustered at the czone level.

Table A1: ANES Responses by Employment Status

Variable	Unemployed	Employed	Tstatistic	N
LESS THAN COLLEGE				
Imm. Take Jobs (1=Very likely; 4 = Not at all)	2.47	2.59	-3.008	7514
Ill. Alien Thermometer (0-negative; 100-positive)	40.26	38.92	1.291	7313
Imm. Levels (1-Decrease; 3-Increase)	2.69	2.66	1.149	7461
WITH COLLEGE ED.				
Imm. Take Jobs (1=Very likely; 4 = Not at all)	2.92	3.07	-2.421	5512
Ill. Alien Thermometer (0-negative; 100-positive)	48.11	46.3	.998	5387
Imm. Levels (1-Decrease; 3-Increase)	3.01	2.99	.378	5479

Notes: Sample restricted to respondents with native-born parents. Entries are unweighted means by employment status, shown separately by education group. t-statistics are reported.

Table A2: CCES Responses by Employment Status: Less Than College

Variable	Unemployed	Employed	Tstatistic	N
LESS THAN COLLEGE				
Grant legal status	.58	.53	11.783	97212
Do not build the wall	.59	.5	3.776	3412
More guest workers	.19	.19	-.058	5148
Oppose more border patrols	.42	.35	18.117	97213
Identify and deport	.55	.49	6.619	24114
Sanction employers	.44	.36	10.54	27629
Allow police to question	.61	.57	5.558	41003
Reduce legal immigration	.61	.54	10.015	35607
Prohibit services to ill. Imm.	.7	.65	4.534	13053
Build a wall	.59	.49	18.869	54602
Increase border funding	.16	.17	-.128	2799
Deny automatic citizenship	.68	.6	8.065	13053
No Syrian regugees	.61	.59	.567	1604
Muslim ban	.72	.74	-.676	1604
Legal status for children	.76	.69	9.994	29573
Prison for illegal reentry	.55	.44	8.729	10369
Deny asylum	.44	.35	7.446	10385
Other question (excl. border)	.02	-.1	20.002	120335
All questions	.03	-.09	22.019	120337
Border and legalization average	.04	-.08	18.688	97232
Defund police for not reporting	.59	.5	12.571	31931

For detailed question descriptions see the appendix. Sample is restricted to first-, second, or third-generation but not immigrant citizens or immigrant non-citizens. The sample is further restricted to respondents who identity as Democrats or Republicans (including leaners). All estimates are unweighted.

Table A3: CCES Responses by Employment Status: With College

Variable	Unemployed	Employed	Tstatistic	N
WITH COLLEGE ED.				
Grant legal status	.59	.63	-5.961	85474
Do not build the wall	.73	.64	1.826	2619
More guest workers	.3	.36	-2.266	5268
Oppose more border patrols	.48	.47	2.555	85474
Identify and deport	.59	.63	-2.74	22724
Sanction employers	.37	.39	-2.477	24649
Allow police to question	.65	.68	-2.645	35470
Reduce legal immigration	.75	.72	3.097	32132
Prohibit services to ill. Imm.	.72	.75	-2.418	10202
Build a wall	.73	.67	7.172	49145
Increase border funding	.33	.31	.537	1598
Deny automatic citizenship	.62	.68	-3.3	10202
No Syrian regugees	.67	.75	-2.21	2469
Muslim ban	.82	.85	-1.024	2469
Legal status for children	.8	.79	.922	27250
Prison for illegal reentry	.66	.58	3.252	10355
Deny asylum	.57	.51	2.69	9271
Other question (excl. border)	.14	.15	-1.628	101898
All questions	.15	.15	-.421	101898
Border and legalization average	.12	.14	-1.942	85487
Defund police for not reporting	.73	.67	5.565	29479

For detailed question descriptions see the appendix. Sample is restricted to first-, second-, or third-generation but not immigrant citizens or immigrant non-citizens. The sample is further restricted to respondents who identity as Democrats or Republicans (including leaners). All estimates are unweighted.

Table A4: Growth Rates Summary Statistics

	Growth Rates, Base 1990	Growth Rates, Base 2000
Mean	0.28	0.13
S.D.	0.58	0.39
IQR	0.51	0.25
1/HHI industries x years	272.69	161.92
1/HHI industry groups	34.49	19.61
Largest Employment Share	0.01	0.02
Largest Employment Share, Industry Group	0.08	0.13
# shocks (industry x years)	1085.00	855.00
# industries	217.00	171.00
Industry Groups	76.00	50.00

The table presents the distribution of national industry-specific growth rates across industries and time periods from base year to 2004, 2008, 2012, 2016, and 2020. All statistics are weighted by the average industry employment shares across states in the base year. Columns differ by whether 1990 or 2000 industry categories and base years were used.

8 Appendix B: Main Results Tables

Table B1: Bartik Shocks and Immigration Attitudes (first difference, every 4 years)

	ANES					CCES			
	(1) ANES (Number)	(2) ANES (Jobs)	(3) ANES (Therm)	(4) SVI Ratio	(5) News Ratio	(6) CCES (Border)	(7) CCES (Legalize)	(8) CCES All	(9) Twitter
D.Bartik	1.196^{+}	1.403**	0.632	0.944*	1.098*	0.154	-0.0920	0.324	0.447*
	(0.691)	(0.424)	(0.488)	(0.389)	(0.451)	(0.359)	(0.212)	(0.306)	(0.195)
D.Avgerage Age	-0.139	0.868	0.129	0.624	0.552^{+}	0.436	0.0480	-0.0388	-0.218
	(0.645)	(0.554)	(0.779)	(0.594)	(0.316)	(0.701)	(0.441)	(0.401)	(0.232)
D.Share Single	-0.318	1.040	1.929*	0.265	0.200	0.741	0.0673	0.139	-0.299
	(0.912)	(0.704)	(0.911)	(0.647)	(0.279)	(0.664)	(0.438)	(0.393)	(0.250)
D.Share White	1.484*	2.462***	1.854*	-0.432	-0.636	-0.671	-0.643	-1.067*	-0.290
	(0.691)	(0.563)	(0.833)	(0.843)	(0.540)	(0.585)	(0.452)	(0.487)	(0.269)
D.Share Male	0.134	0.582^{+}	-0.439	-0.237	-0.148	0.0104	-0.0810	0.0864	-0.0690
	(0.245)	(0.303)	(0.290)	(0.185)	(0.135)	(0.210)	(0.166)	(0.169)	(0.118)
D.Sh. College Ed.	-0.377	-0.0483	-0.517	1.262*	-0.130	0.756^{+}	0.0198	0.116	0.339^{+}
	(0.610)	(0.664)	(0.681)	(0.524)	(0.387)	(0.393)	(0.219)	(0.306)	(0.201)
N	159	159	159	132	192	102	102	102	153
R-Squared	0.335	0.230	0.270	0.539	0.585	0.691	0.541	0.335	0.694

Standard errors in parentheses

Standard errors clustered at the state level

Includes year fixed effects, share college-educated, white, male, single and average age

Years covered: 2004, 2008, 2012, 2016, 2020

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B2: Industry-Level Equivalent Regression

Panel A. State Level							
	(1) ANES Number	(2) ANES Therm	(3) ANES Jobs	(4) Twitter	(5) CCES Legalize	(6) CCES Border	(7) CCES All
D.Bartik	1.747*	0.277	1.629**	0.826***	-0.0798	0.134	0.281
	(0.791)	(0.422)	(0.504)	(0.225)	(0.184)	(0.311)	(0.266)
Observations	102	102	102	102	102	102	102
R^2	0.431	0.068	0.334	0.779	0.541	0.691	0.335
Panel B. Industry-Level							
	(1) ANES Number	(2) ANES Therm	(3) ANES Jobs	(4) Twitter	(5) CCES Legalize	(6) CCES Border	(7) CCES All
D.Bartik	1.747***	0.277	1.629***	0.826***	-0.0798	0.134	0.281
	(0.498)	(0.397)	(0.309)	(0.232)	(0.121)	(0.311)	(0.234)
Observations	434	434	434	434	434	434	434
R^2	0.313	0.038	0.427	0.349	0.006	.	0.005

Standard errors in parenthesis (clustered at the state level in Panel A and industry group level in Panel B). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Controls include state fixed effects, share college-educated, white, male, single and average age in Panel A and the same (in industry-equivalent way) in Panel B.

Table B3: State-level results–Shorter Time intervals

Panel A. 2-year intervals, even years.						
	(1)	(2)	(3)	(4)	(5)	(6)
	SVI Ratio	News Ratio	CCES (Border)	CCES (Legalize)	CCES All	Twitter
D.Bartik	1.628*	-0.228	-0.800	0.847	0.409	-0.277
	(0.668)	(0.369)	(0.760)	(1.025)	(0.415)	(0.168)
N	342	432	204	204	306	357
R-Squared	0.208	0.394	0.213	0.155	0.303	0.699
Panel B. 2-year intervals, all years						
	(1)	(2)	(3)	(4)	(5)	(6)
	SVI Ratio	News Ratio	CCES (Border)	CCES (Legalize)	CCES All	Twitter
D.Bartik	1.155	-0.673	-0.474	0.630	0.309	-0.164
	(0.697)	(0.454)	(0.674)	(0.659)	(0.334)	(0.148)
N	647	816	408	408	612	663
R-Squared	0.263	0.372	0.323	0.186	0.452	0.703

Both panels report two-year first-difference regressions. Panel A uses even years only; Panel B uses all available years. Outcomes are the non-ANES measures only: Google search, news sentiment, CCES Border, CCES Legalize, CCES All, and Twitter. All specifications include year fixed effects and controls for changes in average age and the shares single, male, white, and college-educated. Standard errors in parentheses, clustered at the state level. + p<0.1 * p<0.05 ** p<0.01 *** p<0.001.

Table B4: CZ-level results (Twitter and CCES)

Panel A. Even and Leap Years												
	Twitter Pro-Imm Share			CCES (Border)			CCES (Legalize)			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
D.Bartik	0.110+	0.0417	0.306	-0.224+	-0.316	-1.206	0.0646	0.238	0.401	-0.129	-0.149	-0.222
	(0.0648)	(0.139)	(0.343)	(0.126)	(0.379)	(1.507)	(0.112)	(0.327)	(1.278)	(0.109)	(0.212)	(0.567)
Observations	4423	2278	931	2084	1123	280	2084	1123	280	2848	1714	580
R^2	0.269	0.228	0.066	0.223	0.125	0.119	0.093	0.083	0.160	0.544	0.210	0.087
Panel B. All Years												
	Twitter Pro-Imm Share			CCES (Border)			CCES (Legalize)			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
D.Bartik	0.110+	0.142	0.229	-0.224+	-0.277	-1.366+	0.0646	0.0656	0.779	-0.129	-0.143	0.0415
	(0.0648)	(0.128)	(0.205)	(0.126)	(0.267)	(0.748)	(0.112)	(0.222)	(0.664)	(0.109)	(0.176)	(0.259)
Observations	4423	4275	3799	2084	1650	802	2084	1650	802	2848	2512	2108
R^2	0.269	0.208	0.145	0.223	0.172	0.151	0.093	0.091	0.071	0.544	0.264	0.162

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Panel A includes state-year and CZ fixed effects, share single, male, white, college-educated, and average age. Panels B and C include state and year indicators, share single, male, white, college-educated, and average age. Only CZs with at least 30 respondents or X/Twitter users per CZ included.

Table B5: Individual-Level Results

Panel A. ANES State-Level Shocks

	(1) Number	(2) Jobs	(3) Therm
D.Bartik	0.181***	0.184***	0.174***
	(0.0367)	(0.0403)	(0.0357)
Observations	18338	18464	18096
R^2	0.115	0.125	0.148

Panel B. CCES State-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	-0.00346	-0.00156	0.00712	-0.00256	0.000423	0.00391	0.00170	-0.00295	0.0204+
	(0.00441)	(0.00418)	(0.00597)	(0.00395)	(0.00349)	(0.00441)	(0.00900)	(0.00761)	(0.0116)
Observations	456919	456919	446991	456945	456945	447017	561196	561196	518231
R^2	0.088	0.088	0.089	0.103	0.103	0.100	0.126	0.126	0.131

Panel C. CCES CZ-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	0.00824+	0.00646	0.00791+	0.00283	0.00369	0.00464	0.00865	0.00794	0.0138*
	(0.00453)	(0.00466)	(0.00409)	(0.00462)	(0.00427)	(0.00337)	(0.00649)	(0.00635)	(0.00579)
Observations	452870	452870	442998	452896	452896	443024	556372	556372	513674
R^2	0.095	0.095	0.096	0.110	0.110	0.107	0.136	0.136	0.142

Standard errors in parenthesis (clustered at the state level in panels A and B and CZ in panel C). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Panels A and B include state and year indicators, and state share single, male, white, college-educated, and average age. Panel C includes CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. Columns (1), (4), (7) include all years, columns (2), (5), (8)–2-year intervals, columns (3), (6), (9)–4-year intervals.

Table B6: Individual-Level Results–Only Natives

Panel A. ANES State-Level Shocks

	(1) Number	(2) Jobs	(3) Therm
D.Bartik	0.191***	0.224***	0.210***
	(0.0545)	(0.0323)	(0.0476)
Observations	15071	15176	14852
R^2	0.118	0.124	0.115

Panel B. CCES State-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	-0.00535	-0.00274	0.00849	-0.00426	-0.00128	0.00277	0.000274	-0.00463	0.0202
	(0.00434)	(0.00473)	(0.00691)	(0.00350)	(0.00293)	(0.00421)	(0.00919)	(0.00827)	(0.0125)
Observations	424676	424676	415170	424692	424692	415186	522410	522410	481648
R^2	0.086	0.086	0.088	0.105	0.105	0.102	0.125	0.125	0.131

Panel C. CCES CZ-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	0.00828+	0.00449	0.00811*	0.00333	0.00261	0.00378	0.00843	0.00633	0.0137*
	(0.00472)	(0.00459)	(0.00397)	(0.00465)	(0.00437)	(0.00349)	(0.00666)	(0.00636)	(0.00583)
Observations	422101	422101	412633	422117	422117	412649	519376	519376	478753
R^2	0.094	0.094	0.095	0.112	0.112	0.109	0.135	0.135	0.142

Standard errors in parenthesis (clustered at the state level in panels A and B and CZ in panel C). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Panels A and B include state and year indicators, and state share single, male, white, college-educated, and average age. Panel C includes CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. Columns (1), (4), (7) include all years, columns (2), (5), (8)–2-year intervals, columns (3), (6), (9)–4-year intervals.

Table B7: Individual-Level Results–Only Immigrants

Panel A. ANES State-Level Shocks

	(1) Number	(2) Jobs	(3) Therm
D.Bartik	0.193+	0.000172	0.0934
	(0.110)	(0.0701)	(0.104)
Observations	3216	3237	3189
R^2	0.081	0.076	0.214

Panel B. CCES State-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	0.0192	0.0186	-0.00104	0.0275+	0.0264	0.0238	0.0230	0.0258	0.0369
	(0.0126)	(0.0111)	(0.0187)	(0.0141)	(0.0179)	(0.0179)	(0.0193)	(0.0200)	(0.0270)
Observations	32243	32243	31821	32253	32253	31831	38786	38786	36583
R^2	0.098	0.098	0.099	0.071	0.071	0.069	0.117	0.117	0.115

Panel C. CCES CZ-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	0.00647	0.0387+	0.0179	0.0150	0.0455**	0.0465**	0.0349	0.0610*	0.0577*
	(0.0217)	(0.0215)	(0.0191)	(0.0178)	(0.0172)	(0.0154)	(0.0262)	(0.0258)	(0.0226)
Observations	30769	30769	30365	30779	30779	30375	36996	36996	34921
R^2	0.135	0.135	0.135	0.113	0.113	0.111	0.161	0.161	0.158

Standard errors in parenthesis (clustered at the state level in panels A and B and CZ in panel C). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Panels A and B include state and year indicators, and state share single, male, white, college-educated, and average age. Panel C includes CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. Columns (1), (4), (7) include all years, columns (2), (5), (8)–2-year intervals, columns (3), (6), (9)–4-year intervals.

Table B8: Individual-Level Results CCES Other Outcomes

Panel A. 1-year Bartik Change. CCES Questions

	(1) Oppose More Border Patrols	(2) Grant Legal Status	(3) Border and Legalization Average	(4) All Questions	(5) Other Questions (excl. Border)	(6) - 1 × Allow Police to Question	(7) - 1 × Sanction Employers	(8) - 1 × Identify and Deport	(9) - 1 × Reduce Legal Migration	(10) - 1 × Defund Police For Not Reporting
D.Bartik	0.00828+	0.00333	0.0119	0.00843	0.00741	0.00725	-0.00755	0.00539	-0.00820	-0.0114
	(0.00472)	(0.00465)	(0.00761)	(0.00666)	(0.00691)	(0.0103)	(0.0118)	(0.0163)	(0.00619)	(0.00880)
Observations	422101	422117	422212	519376	519371	204262	157686	126267	180865	166436
R^2	0.094	0.112	0.115	0.135	0.132	0.117	0.099	0.116	0.099	0.097

Panel B. 1-year Bartik Change. CCES Questions-Continued

	(1) - 1 × Prohibit Services to Ill. Imm.	(2) - 1 × Build a Wall	(3) More Guest Workers	(4) - 1 × Deny Automatic Citizenship	(5) Legal Status to Children	(6) - 1 × No Syrian Refugees	(7) - 1 × Muslim Ban	(8) Do not Build the Wall	(9) Prison For Reentry
D.Bartik	0.0909*	-0.0173**	0.0150	0.0893*	-0.0117	-0.0207	0.00503	-0.0114	-0.0131
	(0.0356)	(0.00642)	(0.0198)	(0.0411)	(0.0157)	(0.0244)	(0.0241)	(0.0301)	(0.0253)
Observations	65693	214559	40371	65693	77911	12404	12404	16110	54996
R^2	0.080	0.121	0.101	0.096	0.051	0.085	0.061	0.077	0.098

Panel C. 4-year Bartik Change. CCES Questions

	(1) Oppose More Border Patrols	(2) Grant Legal Status	(3) Border and Legalization Average	(4) All Questions	(5) Other Questions (excl. Border)	(6) - 1 × Allow Police to Question	(7) - 1 × Sanction Employers	(8) - 1 × Identify and Deport	(9) - 1 × Reduce Legal Migration	(10) - 1 × Defund Police For Not Reporting
D.Bartik	0.00810*	0.00378	0.0122*	0.0137*	0.0142*	-0.00440	0.00228	-0.00371	0.000507	0.00577
	(0.00397)	(0.00349)	(0.00592)	(0.00582)	(0.00616)	(0.00622)	(0.00785)	(0.0149)	(0.0104)	(0.00987)
Observations	412633	412649	412744	478753	478748	204262	148218	126267	180865	166436
R^2	0.095	0.109	0.115	0.142	0.137	0.117	0.101	0.116	0.099	0.097

Panel D. 4-year Bartik Change. CCES Questions-Continued

	(1) - 1 × Prohibit Services to Ill. Imm.	(2) - 1 × Build a Wall	(3) More Guest Workers	(4) - 1 × Deny Automatic Citizenship	(5) Legal Status to Children	(6) - 1 × No Syrian Refugees	(7) - 1 × Muslim Ban	(8) Do not Build the Wall	(9) Prison For Reentry
D.Bartik	0.0253	-0.0238*	0.0364+	0.0269	-0.00322	0.0221	0.0191	0.0598*	0.0190
	(0.0265)	(0.00933)	(0.0208)	(0.0279)	(0.00925)	(0.0227)	(0.0183)	(0.0298)	(0.0122)
Observations	65693	205091	30903	65693	77911	12404	12404	16110	54996
R^2	0.079	0.124	0.107	0.096	0.051	0.085	0.061	0.077	0.098

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All specifications include CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated.

Table B9: Heterogeneity Based on Factors that May Affect Labor Market Competition

Panel C. CCES CZ-Level Shocks

	Education Level		Employment Status		Union Membership		Industry Imm. Share	
	(1) dy=1	(2) dy=4	(3) dy=1	(4) dy=4	(5) dy=1	(6) dy=4	(7) dy=1	(8) dy=4
D.Bartik	0.0162*	0.00901	0.0153	0.0204**	-0.00464	-0.0135+	0.00813	0.0135*
	(0.00816)	(0.00833)	(0.00937)	(0.00772)	(0.00705)	(0.00696)	(0.00662)	(0.00582)
No HS × D.Bartik	0	0						
	(.)	(.)						
High School Graduate × D.Bartik	-0.0000679	0.00579						
	(0.00503)	(0.00603)						
Some College × D.Bartik	-0.00746	0.0118*						
	(0.00484)	(0.00600)						
2-Year × D.Bartik	-0.00354	0.00719						
	(0.00562)	(0.00630)						
4-Year × D.Bartik	-0.0122*	0.00608						
	(0.00504)	(0.00615)						
Post-Grad × D.Bartik	-0.0217***	-0.0145*						
	(0.00539)	(0.00660)						
Full-Time × D.Bartik			0	0				
			(.)	(.)				
Part-Time × D.Bartik			0.00511+	0.000118				
			(0.00308)	(0.00378)				
Temporarily Laid Off × D.Bartik			-0.0242**	-0.0299*				
			(0.00828)	(0.0117)				
Unemployed × D.Bartik			0.00197	0.00803*				
			(0.00389)	(0.00391)				
Yes, Currently × D.Bartik					0	0		
					(.)	(.)		
Yes, Formerly × D.Bartik					0.00988*	0.0231***		
					(0.00429)	(0.00461)		
No, Never × D.Bartik					0.0157***	0.0299***		
					(0.00369)	(0.00403)		
Industry Imm. Share							-0.00775***	-0.00993***
							(0.00174)	(0.00168)
Industry Imm. Share × D.Bartik							0.00415**	0.00418**
							(0.00133)	(0.00144)
Observations	519376	478753	290477	271620	508880	477895	519376	478753
R^2	0.135	0.142	0.131	0.136	0.136	0.142	0.135	0.142

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All specifications include CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. In each column, the first row shows the coefficient on the omitted group (whose coefficient is shown as 0), and the other coefficients show the slope difference relative to the omitted group. The outcome is the average of all the CCES immigration policy questions. Odd columns–1 year Bartik shocks; Even columns–4-year Bartik shocks

Table B10: Heterogeneity Based on Factors that May Affect Welfare Concerns

Panel C. CCES CZ-Level Shocks	spending_welfare		spending_healthcare		spending_education		spending_infrastructure		spending_police		spending_vs_tax	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
D.Bartik	-0.0585***	-0.0768***	-0.0515***	-0.0641***	-0.0346**	-0.0282*	-0.00439	0.0173	0.0733***	0.103***	-0.0383***	-0.0156*
	(0.0119)	(0.0129)	(0.0108)	(0.0120)	(0.0108)	(0.0118)	(0.0123)	(0.0126)	(0.0119)	(0.0126)	(0.00900)	(0.00741)
Greatly increase × D.Bartik	0	0	0	0	0	0	0	0	0	0		
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)		
Slightly increase × D.Bartik	0.0131**	0.0209***	0.0453***	0.0711***	0.0203***	0.0328***	0.000842	-0.000690	-0.0346***	-0.0405***		
	(0.00420)	(0.00620)	(0.00398)	(0.00574)	(0.00363)	(0.00590)	(0.00385)	(0.00557)	(0.00428)	(0.00632)		
Maintain × D.Bartik	0.0716***	0.106***	0.123***	0.166***	0.0805***	0.102***	0.0102*	0.00766	-0.103***	-0.125***		
	(0.00460)	(0.00694)	(0.00468)	(0.00702)	(0.00450)	(0.00644)	(0.00425)	(0.00623)	(0.00405)	(0.00628)		
Slightly decrease × D.Bartik	0.112***	0.150***	0.118***	0.158***	0.106***	0.133***	0.00415	-0.0103	-0.212***	-0.286***		
	(0.00546)	(0.00905)	(0.00626)	(0.00871)	(0.00837)	(0.0116)	(0.00910)	(0.0131)	(0.00753)	(0.0104)		
Greatly decrease × D.Bartik	0.107***	0.135***	0.0952***	0.119***	0.0991***	0.120***	0.0321**	0.0223	-0.248***	-0.345***		
	(0.00518)	(0.00779)	(0.00737)	(0.0106)	(0.00727)	(0.0105)	(0.0123)	(0.0204)	(0.00852)	(0.0127)		
Spending cuts over tax increase =0 × D.Bartik											0	0
											(.)	(.)
Spending cuts over tax increase =1 × D.Bartik											0.0643***	0.0402***
											(0.00374)	(0.00392)
Observations	188877	188877	188825	188825	188766	188766	188697	188697	188673	188673	271834	237993
R^2	0.317	0.317	0.281	0.280	0.250	0.249	0.163	0.163	0.211	0.209	0.241	0.259

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All specifications include CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. In each column, the first row shows the coefficient on the omitted group (whose coefficient is shown as 0), and the other coefficients show the slope difference relative to the omitted group. The outcome is the average of all the CCES immigration policy questions. Odd columns–1 year Bartik shocks; Even columns–4-year Bartik shocks

Table B11: Heterogeneity Based on Factors that May Affect Cultural Concerns

Panel C. CCES CZ-Level Shocks

	Ideology		Polit. Leaning		Race		Immigration Status	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D.Bartik	-0.00976+	-0.0157**	0.000494	0.00944+	0.00808	0.0219***	0.00678	0.0168**
	(0.00553)	(0.00590)	(0.00619)	(0.00549)	(0.00674)	(0.00590)	(0.00658)	(0.00590)
Very Liberal × D.Bartik	0	0						
	(.)	(.)						
Liberal × D.Bartik	-0.0128***	0.00700*						
	(0.00266)	(0.00299)						
Moderate × D.Bartik	0.00971***	0.0162***						
	(0.00248)	(0.00366)						
Conservative × D.Bartik	0.0421***	0.0406***						
	(0.00261)	(0.00471)						
Very Conservative × D.Bartik	0.0397***	0.0341***						
	(0.00268)	(0.00453)						
Not Sure × D.Bartik	0.0247***	0.0367***						
	(0.00417)	(0.00460)						
Conservative Views × D.Bartik			0.101***	0.0594***				
			(0.00314)	(0.00432)				
White × D.Bartik					0	0		
					(.)	(.)		
Black × D.Bartik					-0.00690*	-0.0433***		
					(0.00330)	(0.00472)		
Hispanic × D.Bartik					0.00914*	-0.0273***		
					(0.00368)	(0.00485)		
Other × D.Bartik					0.0110**	0.0137**		
					(0.00410)	(0.00514)		
Non-native × D.Bartik							0.0130***	-0.0323***
							(0.00390)	(0.00416)
Native, recent imm. ancestors × D.Bartik							0.00170	-0.000900
							(0.00199)	(0.00242)
Native, no recent imm. ancestors × D.Bartik							0	0
							(.)	(.)
Observations	518079	478382	517709	477086	519376	478753	556372	513674
R^2	0.353	0.371	0.252	0.270	0.135	0.142	0.136	0.141

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All specifications include CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. In each column, the first row shows the coefficient on the omitted group (whose coefficient is shown as 0), and the other coefficients show the slope difference relative to the omitted group. The outcome is the average of all the CCES immigration policy questions. Odd columns–1 year Bartik shocks; Even columns–4-year Bartik shocks

Table B12: CZ-level results (Twitter and CCES) – Symmetry

	Twitter Pro-Imm Share			CCES (Border)			CCES (Legalize)			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
D.Bartik	0.100	0.143	0.228	-0.248+	-0.446	-1.331+	0.0609	0.322	0.374	-0.139	-0.146	0.0317
	(0.0663)	(0.127)	(0.210)	(0.128)	(0.381)	(0.793)	(0.115)	(0.325)	(0.642)	(0.109)	(0.172)	(0.261)
D.Bartik x (D.Bartik>0)	-0.0946	-0.129	0.00171	-0.260*	0.284	0.527	-0.0409	-0.432	1.609	-0.126	-0.0192	-0.00216
	(0.0644)	(0.0932)	(0.0253)	(0.107)	(0.277)	(1.104)	(0.0959)	(0.275)	(0.981)	(0.0892)	(0.110)	(0.0346)
Observations	4423	4275	3799	2084	1650	802	2084	1650	802	2848	2512	2108
R^2	0.269	0.208	0.145	0.225	0.173	0.152	0.093	0.092	0.071	0.544	0.264	0.160

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All specifications control for state (average trends) and time effects, and time-variant CZ characteristics (share single, male, white, college-educated, and average age). Columns (1), (4) (7) include all years, columns (2), (5), (8)–2-year intervals, columns (3), (6), (9)–4-year intervals. Only CZs with at least 30 respondents or X/Twitter users per CZ included.

Table B13: Individual-Level Results Bartik Change – Symmetry

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	0.0206**	0.0121+	-0.0132+	0.00755	0.00687	0.00711	0.0162+	0.0166+	-0.00303
	(0.00725)	(0.00641)	(0.00703)	(0.00611)	(0.00644)	(0.00617)	(0.00946)	(0.00984)	(0.0107)
D.Bartik x (D.Bartik>0)	-0.0122*	-0.00885	0.0269***	-0.00417	-0.00498	-0.00420	-0.00762	-0.0115	0.0202+
	(0.00566)	(0.00569)	(0.00724)	(0.00461)	(0.00553)	(0.00678)	(0.00729)	(0.00799)	(0.0112)
Observations	422101	422101	412633	422117	422117	412649	519376	519376	478753
R^2	0.094	0.094	0.095	0.112	0.112	0.109	0.135	0.135	0.142

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All specifications include CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. Columns (1), (4), (7) include all years, columns (2), (5), (8)–2-year intervals, columns (3), (6), (9)–4-year intervals.

9 Appendix C: Additional Details on Data and Descriptive Statistics

9.1 Additional Google Trends Discussion

9.1.1 Search Volume Index (SVI)

Google Trends data are anonymized and aggregated search volume indices from a random sample of actual search requests made on Google search engine[15]. The SVI measures the relative popularity of a query for a specific time and geography. The SVI is calculated by dividing the number of searches for a specific term i, in geographic area g by the total number of searches in geographic area g and time t. The resulting ratio is then scaled between 0 and 100. The scaling occurs by diving the share of inquiries for term i in a particular geography g and time t by the fraction of search volume for term i to the total Google searches (FAQ Google Trends, 2023; Stephens-Davidowitz, 2014; Choi and Varian, 2012). That is,

$$\frac{\frac{(\text{Google searches including term } i)_g}{(\text{Total Google searches})_{(g,t)}}}{\frac{(\text{Google Searches including the term } i)}{(\text{Total Google searches})_{\max}}} \tag{9}$$

9.1.2 Privacy Threshold

It is important to note that Google Trends has an undisclosed privacy threshold. If total searchers for a term are below that threshold, no index is reported. The privacy threshold is based on absolute numbers so smaller geographies are more likely to be missing an index (Stephens-Davidowitz, 2014). Repeated searches from the same person over a short period of time is not used for creating the index (FAQ Google Trends, 2023).

9.1.3 Normalization

The highest volume of the index is scaled to be 100, and different regions that show the same search interest for a term do no always have the same total search. When the data is at finer geography levels than the whole country (e.g., state or media markets levels), the denominator of Equation (9) remains the total number of searchers in the country (e.g., the U.S.) at time t, while the geographic level for the numerator becomes the finer state or media market level. Then, they are normalized so that the highest scoring geography has 100.

[15]Because a random sample of actual search requests is pulled for a query, downloaded SVIs at different times are not necessarily identical. In fact, we have noticed minor differences in downloaded SVIs every few hours and have used this knowledge to produce bootstrapped standard errors. SVIs are also rounded to the nearest integer, so pulling multiple samples and averaging provide more precise estimates ((Stephens-Davidowitz and Varian, 2015)).

9.1.4 Interpreting the Data

If Google Trends shows that a search term has decreased through time, it does not necessarily mean that there are fewer searchers now than there were previously. It means that there are fewer searches, as a percent of all searches, than there were previously. Similarly, if geographic area g_1 scores higher than geographic area g_2 for a search term, it does not mean that g_1 makes more total searches for the term than g_2 It means that, as a percent of total searches, there are relatively more searches in g_1 than g_2 on that term. Otherwise, bigger places with more searches would always score higher ((Stephens-Davidowitz and Varian, 2015)).

Repeated searches from the same person over a short period of time are not used for creating the index. Caution should be exercised when choosing search terms, as single-word queries may be too broad or open to misinterpretation. It is recommended to use multiple words or phrases to specify the query. Additionally, when comparing search trends across different geographic areas or time periods, it is essential to keep in mind that the data may be affected by external factors, such as news events, cultural shifts, or changes in search behavior. Therefore, it is necessary to consider these factors when interpreting the data.

In the main text, we show how the ratio of the SVI for "undocumented immigrants" to the SVI for "illegal immigrants." changes over time. Figure C1 complements that measure by plotting the two monthly search series directly, from January 2004 through December 2024, on a common scale. This makes it possible to see not only how the ratio changes over time, but also the underlying levels of relative search interest for each term.

Figure C1 shows that "illegal immigrants" has been consistently much more popular as a search term throughout the sample period. Search interest in that term is higher in every part of the sample and features several sharp spikes, including in the mid-2000s, around 2016–2018, and again near the end of the sample. By contrast, "undocumented immigrants" remains at a much lower level throughout, especially in the earlier years. At the same time, the figure also shows a gradual increase in the relative popularity of "undocumented immigrants," particularly from the mid-2010s onward. Thus, while the more hostile term continues to dominate in absolute search intensity, the alternative term becomes more visible over time.

This pattern is important for interpreting the ratio used in the main analysis. Changes in that ratio do not reflect a situation in which "undocumented immigrants" overtakes "illegal immigrants" as the dominant term. Rather, they reflect shifts at the margin in relative language use, against a backdrop in which "illegal immigrants" remains the much more common search term. The figure therefore helps clarify both why the ratio is informative and why it should be interpreted as a measure of relative terminology rather than a measure of absolute search intensity.

9.2 Additional Details on Twitter

Hashtag terms for assigning sentiment target:

Anti-Trump/Pro-Immigrant: "#antitrump #resist #liar #notmypresident #resistance #impeachtrump #notrump #nobannowall #resisttrumptuesdays #theresistance #nevertrump #dumptrump #lovetrumpshate #boycotttrump #fucktrump #trumpprotest #trumpmemes #trumpisajoke #daca #abolishice #familiesbelongtoghether #faketrumpemergency #trumpshutdown #nowall #indicttrump #trumplies #trumpliesmatter #cult45 #moroninchief #dumpchump #closethecamps #daca #defundhate #statueofliberty #publiccharge #smartnews #beto2020 #warren2020 #dontdeportmelania #racistinchief #derangeddonald #racist #racism #xenophobia #moscowmitch #standwithiraqirefugees #iceraids #trumpcamps #keepfamiliestogether #publicchargerule #25thamendmentnow #saynotoxenophobia #powerofinclusion #trumpisawhitesupremacist #detention #humanrights #asylumseekers #knowyourrights #undocumented #cuccinelliresign #stephenmiller #deportmelania #refugeeswelcome #bernie2020 #concentrationcamps #dontlookaway #trumpisaracist #jewsagainstice #neveragainisnow #tuckfrump #enoughisenough #racisttrump"

Pro-Trump/Anti-Immigrant: "#maga #fakenews #buildthatwall #buildthewall #makeamericagreatagain #protrump #trumpsupporters #trumptrain #trumppence #republicansfortrump #blacksfortrump #latinosfortrump #womenfortrump #americafirst #draintheswamp #trump2020 #enforceourborders #illegalimmigrants #illegals #illegalimmigration #illegalaliens #maga2020 #kag2020 #dems #recallgavinnewsom #wethepeople #closetheborder #sanctuarycities #sanctuarystate #wakeupamerica #socialism #liberalismisamentaldisorder #finishthewall #deportthemall #democratsaredestroyingamerica #openborders #sendthemback #noamnesty"

9.3 CCES Immigration Questions

At different points in time, CCES asked respondents to indicate agreement with the following statements (with occasional slight variation from year to year). The two questions asked most often are in bold. In parenthreses, we specify the years the questions were asked. In brackets, we specify the corresponding column names in Table B8.

What do you think the U.S. government should do about immigration?

1. **Increase the number of border patrols on the US-Mexican border**. (2007, 2010-2017,2019-2021) In 2011, the statement was: increase border presence. [Oppose More Border Patrols]

2. **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. (2007, 2010-2017,2019-2021) [Grant Legal Status]

3. Allow police to question anyone they think may be in the country illegally. (2010,2011-2014,2017) [Allow Police Questioning]

4. Fine US businesses that hire illegal immigrants. (2007,2010,2012-2014,2017) [Sanction Employers]

5. Identify and deport illegal immigrants. (2014,2016,2017) [Identify and Deport]

6. Increase spending on border security by $25 billion, including building a wall between the U.S. and Mexico. (2007,2017,2018,2020-2021) [Build a Wall]

7. Overturn President Trump's order to use $6 billion of defense funds to pay for the construction of a wall. (2019) [Do Note Build the Wall]

8. Withhold federal funds from any local police department that does not report to the federal government anyone they identify as an illegal immigrant. (2017-2021). In 2017, the statement was: require local police departments to report to the federal government anyone they identify as an illegal immigrant [Defund Police for Not Reporting]

9. Prohibit illegal immigrants from using emergency hospital care and public schools. (2012-2013) [Prohibit Services to Ill. Imm.]

10. Deny automatic citizenship to American-born children of illegal immigrants. (2012,2013) [Deny Automatic Citizenship]

11. Increase the number of guest workers allowed to come legally to the U.S. (2007,2010,2016,2017) [More Guest Workers]

12. Reduce legal immigration by 50 percent over the next 10 years by eliminating the visa lottery and ending family-based migration.(2018-2020) [Reduce Legal Immigration]

13. Provide legal status to children of immigrants who are already in the United States and were brought to the United States by their parents. Provide these children the option of citizenship in 10 years if they meet citizenship requirements and commit no crimes. (DACA). (2021) [Legal Status to Children]

14. Admit no refugees from Syria (2016) [No Syrian Refugees]

15. Ban Muslims from immigrating to the U.S.(2016) [Muslim Ban]

16. Grant legal status to DACA children, spend $25 billion to build the border wall, and reduce legal immigration by eliminating the visa lottery and ending family-based migration. (2018) [unused, since it is pro-immigration in terms of legalizing DACA, but anti-immigration in terms of border enforcement and eliminating lottery]

17. Send to prison any person who has been deported from the United States and reenters the United States. (2018) [Prison for Reentry]

We use the same set of questions indicative of more Republican vs Democratic leanings, but not directly related to immigration, as Djourelova (2023):

1. Allow abortion (x-1)
2. Oppose Gay marriage
3. Repeal Affordable Care Act
4. Oppose affirmative action
5. Support military intervention
6. Spending cuts over tax increase
7. Sales tax of income tax
8. Jobs over environment
9. Economy worse under Obama

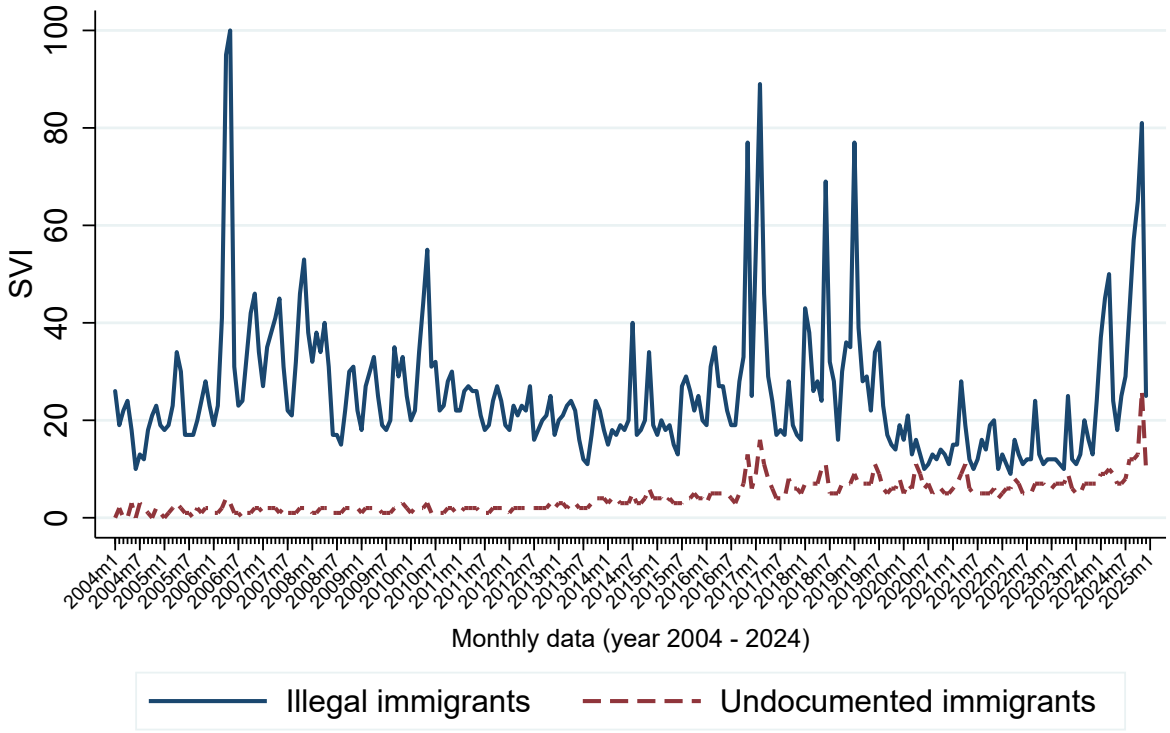


Figure C1: Trends in Attitudes Towards Immigrates, Google Trends 2004 – 2024

Table C1: Summary Statistics by Attitudes on Levels of Immigrants

Variable	(1) Increase	(2) Same	(3) Decrease	(4) Increase Vs. Same	(5) Increase Vs. Decrease	(6) Same Vs. Decrease
Female	0.503	0.515	0.536	-0.012	-0.033***	-0.021***
	(0.500)	(0.500)	(0.499)	(0.009)	(0.009)	(0.007)
Age (years)	44.348	46.582	48.066	-2.233***	-3.718***	-1.484***
	(17.996)	(17.512)	(17.387)	(0.317)	(0.312)	(0.232)
Less than high school degree	0.097	0.104	0.137	-0.007	-0.040***	-0.033***
	(0.296)	(0.305)	(0.343)	(0.005)	(0.006)	(0.004)
High school degree	0.033	0.032	0.037	0.001	-0.004	-0.005**
	(0.178)	(0.175)	(0.189)	(0.003)	(0.003)	(0.002)
Some college	0.203	0.259	0.259	-0.056***	-0.055***	0.001
	(0.403)	(0.438)	(0.438)	(0.008)	(0.008)	(0.006)
Bachelor's degree	0.266	0.211	0.142	0.055***	0.124***	0.069***
	(0.442)	(0.408)	(0.349)	(0.007)	(0.007)	(0.005)
Advanced degree	0.198	0.115	0.065	0.082***	0.133***	0.051***
	(0.398)	(0.320)	(0.246)	(0.006)	(0.005)	(0.004)
Employed	0.647	0.629	0.600	0.018**	0.047***	0.029***
	(0.478)	(0.483)	(0.490)	(0.009)	(0.009)	(0.006)
Married	0.520	0.558	0.572	-0.037***	-0.052***	-0.015**
	(0.500)	(0.497)	(0.495)	(0.009)	(0.009)	(0.007)
Never married	0.342	0.274	0.227	0.067***	0.114***	0.047***
	(0.474)	(0.446)	(0.419)	(0.008)	(0.008)	(0.006)
Divorced, separated, or widowed	0.138	0.168	0.200	-0.030***	-0.062***	-0.032***
	(0.345)	(0.374)	(0.400)	(0.007)	(0.007)	(0.005)
White alone	0.646	0.689	0.783	-0.043***	-0.137***	-0.094***
	(0.478)	(0.463)	(0.413)	(0.008)	(0.008)	(0.006)
Black alone	0.124	0.128	0.097	-0.005	0.027***	0.032***
	(0.329)	(0.334)	(0.296)	(0.006)	(0.005)	(0.004)
Asian alone	0.042	0.031	0.012	0.011***	0.030***	0.019***
	(0.201)	(0.173)	(0.109)	(0.003)	(0.002)	(0.002)
Hispanic	0.146	0.117	0.076	0.029***	0.070***	0.041***
	(0.353)	(0.321)	(0.265)	(0.006)	(0.005)	(0.004)
Union membership in household	0.170	0.156	0.165	0.014**	0.005	-0.009*
	(0.376)	(0.363)	(0.372)	(0.007)	(0.007)	(0.005)
Observations	3,315	8,585	9,322	24,931	24,931	24,931

Source: Authors' calculations from the American National Election Studies, 1992 – 2020.
Notes: Columns (1) – (3) report the relative frequencies for everyone who believe immigration levels should increase, stay the same, or decrease. Columns (4) – (6) report the difference in the relative frequencies between the three attitudinal groups. Numbers in parentheses are standard deviations for Columns (1) – (3) and standard errors for Columns (4) – (6). All estimates except for sample sizes are weighted. *** p≤0.01; ** p≤0.05; *** p≤0.1.

Table C2: Summary Statistics by Attitudes on Whether Immigrants Take Jobs Away, ANES 2004 – 2020

Variable	(1) Not at all	(2) Somewhat	(3) Extremely or Very	(4) Not at all Vs. Somewhat	(5) Not at all Vs. Extremely or Very	(6) Somewhat Vs. Extremely or Very
Female	0.510	0.513	0.520	-0.004	-0.011	-0.007
	(0.500)	(0.500)	(0.500)	(0.009)	(0.010)	(0.008)
Age (years)	46.518	47.032	49.078	-0.513	-2.560***	-2.046***
	(17.370)	(17.764)	(17.265)	(0.329)	(0.334)	(0.296)
Less than high school degree	0.069	0.074	0.123	-0.005	-0.054***	-0.049***
	(0.254)	(0.261)	(0.328)	(0.005)	(0.006)	(0.005)
High school degree	0.042	0.047	0.058	-0.005	-0.016***	-0.011***
	(0.201)	(0.211)	(0.234)	(0.004)	(0.004)	(0.004)
Some college	0.231	0.248	0.250	-0.016**	-0.018**	-0.002
	(0.422)	(0.432)	(0.433)	(0.008)	(0.008)	(0.007)
Bachelor's degree	0.270	0.227	0.143	0.043***	0.127***	0.084***
	(0.444)	(0.419)	(0.350)	(0.008)	(0.008)	(0.007)
Advanced degree	0.187	0.128	0.066	0.059***	0.121***	0.062***
	(0.390)	(0.334)	(0.248)	(0.007)	(0.006)	(0.005)
Employed	0.664	0.626	0.574	0.037***	0.090***	0.053***
	(0.472)	(0.484)	(0.495)	(0.009)	(0.009)	(0.008)
Married	0.551	0.548	0.547	0.003	0.003	0.000
	(0.497)	(0.498)	(0.498)	(0.009)	(0.010)	(0.008)
Never married	0.310	0.288	0.265	0.022***	0.045***	0.023***
	(0.463)	(0.453)	(0.441)	(0.008)	(0.009)	(0.007)
Divorced, separated, or widowed	0.139	0.164	0.188	-0.025***	-0.049***	-0.024***
	(0.346)	(0.370)	(0.391)	(0.007)	(0.007)	(0.006)
White alone	0.630	0.725	0.739	-0.095***	-0.109***	-0.015*
	(0.483)	(0.447)	(0.439)	(0.009)	(0.009)	(0.007)
Black alone	0.110	0.100	0.129	0.010*	-0.019***	-0.029***
	(0.313)	(0.300)	(0.335)	(0.006)	(0.006)	(0.005)
Asian alone	0.041	0.035	0.014	0.006*	0.027***	0.021***
	(0.199)	(0.184)	(0.118)	(0.004)	(0.003)	(0.003)
Hispanic	0.169	0.101	0.074	0.068***	0.095***	0.027***
	(0.375)	(0.301)	(0.262)	(0.006)	(0.006)	(0.005)
Union membership in household	0.175	0.163	0.144	0.013*	0.031***	0.018***
	(0.380)	(0.369)	(0.351)	(0.007)	(0.007)	(0.006)
Observations	3,471	5,609	4,673	24,931	24,931	24,931

Source: Authors' calculations from the American National Election Studies, 2004 – 2020.
Notes: Columns (1) – (3) report the relative frequencies for everyone who believe that it is not at all likely, somewhat likely, or extremely likely or very likely for immigrants to take jobs away. Columns (4) – (6) report the difference in the relative frequencies between the three attitudinal groups. Numbers in parentheses are standard deviations for Columns (1) – (3) and standard errors for Columns (4) – (6). All estimates except for sample sizes are weighted. *** p≤0.01; ** p≤0.05; *** p≤0.1.

Table C3: Congruency Between Attitudes Towards Immigration, ANES 2004-2020

	Increase		Same as now		Decrease	
	Count	Percent	Count	Percent	Count	Percent
Somewhat likely	1,320	33.05%	4,084	50.31%	2,476	33.91%
Not at all likely	2,147	53.76%	2,348	28.93%	524	7.18%
Extremely or Very likely	527	13.19%	1,685	20.76%	4,302	58.92%
Total	3,994	100.00%	8,117	100.00%	7,302	100.00%

Notes: Odd columns from (1) to (5) report the relative frequencies (counts) in every cell, while even columns from (2) to (6) report the percentages. Column (1) reports the number of respondents who believe that levels of immigration to the U.S. should increase and that immigrants are somewhat, not at all, or extremely or very likely to take jobs away in the first three rows, respectively. Column (3) reports the number of respondents who believe immigration levels should stay and that immigrants are somewhat, not at all, or extremely or very likely to take jobs away across the first three rows, respectively. Column (5) reports the number of respondents who believe immigration levels should decrease and that immigrants are somewhat, not at all, or extremely or very likely to take jobs away across the first three rows, respectively. The final row in Columns (1), (3), and (5) report total column counts. Columns (2), (4), and (6) report the relative percentages for the corresponding cells in Columns (1), (3), and (5). Column (7) reports the total row counts. All estimates are unweighted.
Source: Authors' calculations from the American National Election Studies, 2004-2020.

10 Appendix D: Additional Results: Fixed-Effects Specifications

In this appendix we present results of location-level fixed-effects specifications to complement the main first-difference results. At the state level, we start with the following fixed-effects panel model:

$$y_{lt} = \alpha_l + \tau_t + \omega B_{lt} + X'_{lt}\lambda + \varepsilon_{lt}, \tag{10}$$

where y_{lt} is the measure of immigration attitudes in state l at time t, α_l is a state fixed effect, capturing all time-invariant characteristics of state l, τ_t is a year fixed effect capturing shocks common to all states in year t, B_{lt} is the Bartik labor demand shock, and X_{lt} is a vector of other time-varying state-level controls. The coefficient ω is our parameter of interest, measuring the effect of a one-unit labor demand shock on the attitude outcome.

The CZ-level fixed-effects specification is:

$$y_{zt} = \alpha_z + \alpha_l * \tau_t + \omega B_{zt} + X'_{zt}\lambda + \varepsilon_{zt}, \tag{11}$$

where y_{zt} is the attitude outcome in CZ z (which is located in state l) at time t, α_z is a CZ fixed effect, and $\alpha_l * \tau_t$ denotes a state-by-year fixed effect (for state l that contains CZ z at time t). By including $\alpha_l * \tau_t$, we compare each CZ to other CZs within the same state and year, absorbing any statewide economic or policy shocks in year t. More specifically, the coefficient ω in the Bartik shock B_{zt} (constructed analogously at the CZ level) is then identified as a deviation from CZ-level average of CZ relative to other CZs in the same state-year in response to labor demand changes.

Table D1: Bartik Shocks and Immigration Attitudes (FE, every 4 years)

	ANES					CCES			
	(1) ANES (Number)	(2) ANES (Jobs)	(3) ANES (Therm)	(4) SVI Ratio	(5) News Ratio	(6) CCES (Border)	(7) CCES (Legalize)	(8) CCES All	(9) Twitter
Bartik	1.577**	1.441**	1.146*	1.715**	0.569	0.151	-0.138	0.312	1.020***
	(0.551)	(0.485)	(0.470)	(0.616)	(0.370)	(0.371)	(0.241)	(0.239)	(0.246)
Avgerage Age	0.305	0.834	0.572	0.320	0.936**	-0.249	-0.149	-0.232	-0.0657
	(0.495)	(0.601)	(0.650)	(0.867)	(0.295)	(0.485)	(0.351)	(0.223)	(0.209)
Sh. College Ed.	0.492	0.369	0.195	1.573*	0.139	0.466	-0.270	-0.0507	0.560*
	(0.524)	(0.604)	(0.463)	(0.731)	(0.441)	(0.382)	(0.227)	(0.188)	(0.237)
Share Male	0.207	0.328	-0.432+	-0.475+	0.105	0.0671	-0.0433	0.0561	0.0615
	(0.307)	(0.328)	(0.241)	(0.240)	(0.162)	(0.186)	(0.227)	(0.133)	(0.150)
Share White	1.509*	1.911**	1.947**	-0.740	-1.135*	-0.775+	-0.519	-0.743*	-0.505+
	(0.587)	(0.576)	(0.610)	(0.781)	(0.506)	(0.459)	(0.413)	(0.304)	(0.272)
Share Single	0.509	1.241	1.517	-0.128	0.407	-0.00480	-0.234	-0.213	-0.185
	(0.811)	(0.817)	(0.914)	(0.955)	(0.354)	(0.542)	(0.407)	(0.247)	(0.274)
Constant	-29.88	-62.34+	-17.07	1.555	-29.38+	6.514	19.64	13.99	7.497
	(30.70)	(33.07)	(27.43)	(43.19)	(15.42)	(21.07)	(19.44)	(13.35)	(13.03)
N	216	216	216	182	240	153	153	153	204
R-Squared	0.702	0.682	0.667	0.714	0.813	0.873	0.944	0.911	0.930

Standard errors in parentheses

Standard errors clustered at the state level

Includes state and year fixed effects, share college-educated, white, male, single and average age

Years covered: 2004, 2008, 2012, 2016, 2020

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D2: Industry-Level Equivalent Regression (Fixed Effects)

	(1) ANES Number	(2) ANES Therm	(3) ANES Jobs	(4) Twitter	(5) CCES Legalize	(6) CCES Border	(7) CCES All	(8) ANES Number	(9) ANES Therm	(10) ANES Jobs	(11) Twitter	(12) CCES Legalize	(13) CCES Border	(14) CCES All
Bartik	1.881*	0.556	1.531*	0.950***	-0.129	0.154	0.437							
	(0.823)	(0.399)	(0.642)	(0.237)	(0.227)	(0.380)	(0.335)							
Bartik								1.881***	0.556^{+}	1.531***	0.950***	-0.129	0.154	0.437*
								(0.394)	(0.287)	(0.285)	(0.203)	(0.146)	(0.230)	(0.189)
Observations	153	153	153	153	153	153	153	651	651	651	651	651	651	651
R^2	0.767	0.779	0.765	0.952	0.944	0.873	0.911	0.397	0.092	0.400	0.453	0.019	.	0.034

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D3: State-level results–Shorter Time intervals

Panel A. Fixed Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
	SVI Ratio	News Ratio	CCES (Border)	CCES (Legalize)	CCES All	Twitter
Bartik	2.074***	0.372	-0.263	-0.303	-0.246	1.030***
	(0.486)	(0.265)	(0.274)	(0.178)	(0.168)	(0.201)
N	775	912	663	663	816	765
R-Squared	0.537	0.769	0.757	0.845	0.894	0.941
Panel B. fixed-effects - 2-year intervals, even years						
	(1)	(2)	(3)	(4)	(5)	(6)
	SVI Ratio	News Ratio	CCES (Border)	CCES (Legalize)	CCES All	Twitter
Bartik	1.919***	0.235	0.0257	0.00144	-0.333*	1.109***
	(0.441)	(0.304)	(0.279)	(0.179)	(0.147)	(0.209)
N	406	480	306	306	408	408
R-Squared	0.627	0.762	0.806	0.929	0.905	0.947

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All panels include state-year effects, share single, male, white, college-educated, and average age.

Table D4: CZ-level results (Twitter and CCES)

Panel A. Fixed Effects

	Twitter Pro-Imm Share			Oppose More Border Patrols			Legalize Ill. Imm.			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bartik	0.414***	0.414***	0.520***	0.191***	0.155**	0.338***	0.00885	0.0132	0.0147	0.0642	0.0424	0.326**
	(0.0591)	(0.0611)	(0.0917)	(0.0476)	(0.0493)	(0.0843)	(0.0445)	(0.0490)	(0.0903)	(0.0499)	(0.0549)	(0.100)
Observations	4368	2230	944	2400	1622	797	2400	1622	797	2994	2130	797
R^2	0.895	0.906	0.907	0.785	0.768	0.818	0.882	0.897	0.895	0.883	0.857	0.822

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Panel A includes state-year and CZ fixed effects, share single, male, white, college-educated, and average age. Panels B and C include state and year indicators, share single, male, white, college-educated, and average age. Only CZs with at least 30 respondents or X/Twitter users per CZ included.

11 Appendix E: Base 2000 Industries

Table E1: Bartik Shocks and Immigration Attitudes (first difference, every 4 years)

	ANES					CCES			
	(1) ANES (Number)	(2) ANES (Jobs)	(3) ANES (Therm)	(4) SVI Ratio	(5) News Ratio	(6) CCES (Border)	(7) CCES (Legalize)	(8) CCES All	(9) Twitter
D.Bartik	0.616	1.093*	0.433	1.292**	0.750*	0.244	-0.0953	0.246	0.364*
	(0.593)	(0.437)	(0.308)	(0.448)	(0.324)	(0.315)	(0.169)	(0.269)	(0.146)
D.Avgerage Age	-0.449	0.851	0.104	0.620	0.256	0.464	0.0461	-0.0209	-0.271
	(1.115)	(0.619)	(0.816)	(0.632)	(0.270)	(0.740)	(0.457)	(0.412)	(0.256)
D.Share Single	-0.396	1.012	1.928*	0.188	0.000550	0.726	0.0745	0.125	-0.349
	(0.710)	(0.717)	(0.926)	(0.629)	(0.211)	(0.669)	(0.443)	(0.392)	(0.252)
D.Share White	1.200*	2.661***	1.946*	-0.338	-0.637	-0.661	-0.655	-1.060*	-0.269
	(0.574)	(0.589)	(0.855)	(0.769)	(0.407)	(0.583)	(0.456)	(0.489)	(0.274)
D.Share Male	0.122	0.566+	-0.443	-0.260	-0.112	0.00155	-0.0768	0.0730	-0.0700
	(0.296)	(0.302)	(0.289)	(0.181)	(0.112)	(0.206)	(0.167)	(0.168)	(0.116)
D.Sh. College Ed.	-1.100	-0.155	-0.553	0.971+	-0.172	0.693	0.0426	0.0716	0.296
	(0.819)	(0.651)	(0.660)	(0.517)	(0.333)	(0.438)	(0.217)	(0.330)	(0.214)
N	188	159	159	132	240	102	102	102	153
R-Squared	0.301	0.225	0.268	0.566	0.573	0.692	0.541	0.334	0.693

Standard errors in parentheses

Standard errors clustered at the state level

Includes year fixed effects, share college-educated, white, male, single and average age

Years covered: 2004, 2008, 2012, 2016, 2020

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E2: State-level results–Shorter Time intervals

Panel A. All Years						
	(1)	(2)	(3)	(4)	(5)	(6)
	SVI Ratio	News Ratio	CCES (Border)	CCES (Legalize)	CCES All	Twitter
Bartik	0.529***	0.277	0.607***	0.442***	0.568***	0.415***
	(0.0849)	(0.155)	(0.156)	(0.0914)	(0.127)	(0.115)
N	774	864	663	663	815	765
R-Squared	0.283	0.608	0.555	0.736	0.719	0.768
Panel B. 2-year intervals, even years.						
	(1)	(2)	(3)	(4)	(5)	(6)
	SVI Ratio	News Ratio	CCES (Border)	CCES (Legalize)	CCES All	Twitter
D.Bartik	0.929	-0.0827	0.0135	0.332	0.178	0.00303
	(0.467)	(0.318)	(0.774)	(0.938)	(0.358)	(0.151)
N	342	432	204	204	306	357
R-Squared	0.200	0.394	0.210	0.152	0.302	0.697
Panel C. 2-year intervals, all years						
	(1)	(2)	(3)	(4)	(5)	(6)
	SVI Ratio	News Ratio	CCES (Border)	CCES (Legalize)	CCES All	Twitter
D.Bartik	0.964	-0.518	0.0836	0.190	0.101	-0.0532
	(0.556)	(0.301)	(0.706)	(0.582)	(0.340)	(0.119)
N	647	816	408	408	612	663
R-Squared	0.263	0.371	0.323	0.184	0.452	0.703

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. All panels include state-year effects, share single, male, white, college-educated, and average age.

Table E3: CZ-level results (Twitter and CCES)

Panel A. Even and Leap Years												
	Twitter Pro-Imm Share			CCES (Border)			CCES (Legalize)			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
D.Bartik	0.0588	-0.00650	0.323	-0.0946	-0.0647	-1.159	0.0550	0.116	-1.054	-0.130+	-0.159	-1.044*
	(0.0466)	(0.103)	(0.200)	(0.0775)	(0.283)	(0.968)	(0.0772)	(0.232)	(0.733)	(0.0758)	(0.152)	(0.451)
Observations	4423	2278	931	2084	1123	280	2084	1123	280	2848	1714	580
R^2	0.269	0.228	0.067	0.223	0.124	0.123	0.093	0.083	0.165	0.544	0.210	0.097
Panel B. All Years												
	Twitter Pro-Imm Share			CCES (Border)			CCES (Legalize)			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
D.Bartik	0.0588	0.0661	0.137	-0.0946	-0.138	-0.726	0.0550	0.00525	-0.105	-0.130+	-0.194	-0.250
	(0.0466)	(0.0867)	(0.158)	(0.0775)	(0.207)	(0.442)	(0.0772)	(0.172)	(0.402)	(0.0758)	(0.128)	(0.211)
Observations	4423	4275	3799	2084	1650	802	2084	1650	802	2848	2512	2108
R^2	0.269	0.208	0.145	0.223	0.172	0.149	0.093	0.091	0.069	0.544	0.265	0.163

Standard errors in parenthesis (clustered at the CZ level). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Panel A includes state-year and CZ fixed effects, share single, male, white, college-educated, and average age. Panels B and C include state and year indicators, share single, male, white, college-educated, and average age. Only CZs with at least 30 respondents or X/Twitter users per CZ included.

Table E4: Individual-Level Results

Panel A. ANES State-Level Shocks

	(1) Number	(2) Jobs	(3) Therm
D.Bartik	0.181***	0.184***	0.174***
	(0.0367)	(0.0403)	(0.0357)
Observations	18338	18464	18096
R^2	0.115	0.125	0.148

Panel B. CCES State-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	0.0198+	0.0181*	-0.00193	0.0279**	0.0332*	0.0263+	0.0182	0.0245	0.0331
	(0.0108)	(0.00832)	(0.0146)	(0.0104)	(0.0141)	(0.0137)	(0.0188)	(0.0170)	(0.0199)
Observations	32243	32243	31821	32253	32253	31831	38786	38786	36583
R^2	0.098	0.098	0.099	0.071	0.071	0.069	0.117	0.117	0.115

Panel C. CCES CZ-Level Shocks

	Oppose More Border Patrols			Grant Legal Status			CCES All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
D.Bartik	0.00647	0.0387+	0.0179	0.0150	0.0455**	0.0465**	0.0349	0.0610*	0.0577*
	(0.0217)	(0.0215)	(0.0191)	(0.0178)	(0.0172)	(0.0154)	(0.0262)	(0.0258)	(0.0226)
Observations	30769	30769	30365	30779	30779	30375	36996	36996	34921
R^2	0.135	0.135	0.135	0.113	0.113	0.111	0.161	0.161	0.158

Standard errors in parenthesis (clustered at the state level in panels A and B and CZ in panel C). + p<0.1 * p<0.05 ** p<0.01 *** p<0.001. Panels A and B include state and year indicators, and state share single, male, white, college-educated, and average age. Panel C includes CZ and state-year fixed-effects, CZ share single, male, white, college-educated, and average age and individual controls–age, dummies for male, single, white, college-educated. Columns (1), (4), (7) include all years, columns (2), (5), (8)–2-year intervals, columns (3), (6), (9)–4-year intervals.