No Backlash to Value-Aligned Policies: Reassessing Public Responses to High-Profile Immigration Reforms

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Abstract

Do salient pro-immigration reforms provoke public backlash? Despite widespread beliefs that such reforms would incite counterproductive opposition, evidence on immigration policy changes' impact on voter attitudes remains ambiguous. We address this issue comprehensively using an unexpected-event-during-survey design, assessing the effects of major recent U.S. immigration policies on incumbent evaluations and policy preferences across all available representative surveys. Specifically, we examine verifiably high-profile policy announcements with clear executive responsibility attributions, including pro-immigration measures (Obama's DACA and DAPA) and anti-immigration measures (Trump's "Muslim" and "Green Card" bans). Our analysis reveals neither DACA nor DAPA triggered immediate backlash against immigration or the incumbent. At the same time, immigration bans had no effects or may have even increased public support of immigration. These findings suggest policymakers might have limited cause for concern regarding public backlash against value-aligned policies, even on contentious issues like (unauthorized) immigration.

Introduction

Immigration is once again transforming global politics. From Donald Trump's election victory and intensified enforcement in the United States to the rise of the far-right in Europe, public backlash against immigration is said to reshape political landscapes with far-reaching consequences beyond immigration policy alone. Indeed, many otherwise immigration-friendly politicians and researchers across the political spectrum have repeatedly attributed the rise of far-right populism and xenophobia on both sides of the Atlantic to voter backlash against immigration and a corresponding political failure to adequately restrict it (e.g., Pevnick, 2024). Despite limited empirical evidence, these popular arguments–often based on isolated instances of alleged immigration backlash–have been highly influential among policymakers. Contrary to the widespread beliefs that such reforms would incite counterproductive voter backlash, as informed by the growing academic literature on immigrant group threat and thermostatic politics, the degree to which these policies influence voter attitudes has remained ambiguous, especially in the short term (e.g., Kustov, 2023; Van Hauwaert, 2023).

This ambiguity may stem from previous backlash studies relying on aggregate year-to-year data, which obscures individual-level awareness of policy changes and responsibility attribution–key elements for understanding policy feedback effects. To address this issue, our paper investigates whether and to what extent nationally salient US pro-immigration (and anti-immigration) reforms with clear executive responsibility can provoke an immediate negative reaction among the general public and various voter subgroups against the incumbents or policy support. Examining policies' immediate and short-term effects on public opinion is important because these effects can influence people's behaviors, how elites interpret public opinion, and whether longer-term backlash develops over time.

To that end, we analyze the most high-profile recent immigration policies in the United States (see Figure 1). In particular, we focus on two policies regarding unauthorized populations where voters are particularly negative and polarized: the Deferred Action for Childhood Arrivals (DACA) and the Deferred Action for Parents of Americans and Lawful Permanent

Residents (DAPA). We also complement this evidence by examining the potential effects of similarly salient recent anti-immigration measures, including the DACA rescission, as well as the "Muslim" and "Green Card" bans introduced by President Donald Trump.

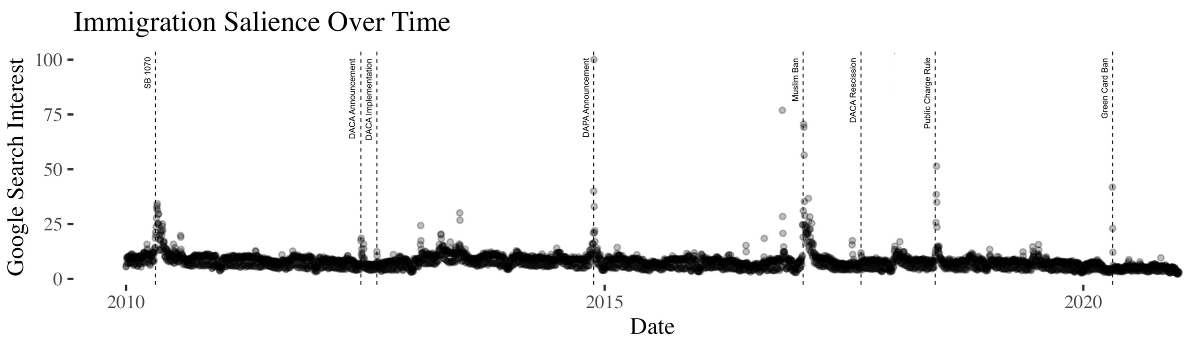

Figure 1: Daily Google Search on "Immigration" Over Time.

DACA, introduced in 2012 by President Barack Obama, provided temporary protection from deportation and work authorization to eligible young people who came to the U.S. as children. DAPA, announced in 2014 but never implemented due to legal challenges, aimed to extend similar benefits to certain undocumented parents of U.S. citizens and lawful permanent residents. Although DACA targets a relatively sympathetic immigrant population and now enjoys broad voter support (Jacobs, 2023), it faced significant opposition at the time of its announcement in 2012 (see Figure I12). In fact, it faced harsh criticism from Republican politicians and many voters who viewed it as an illegal usurpation of the authority of Congress. Many observers thus also believed that DACA could provoke and have provoked a significant backlash among the public and contribute to the rise of anti-immigration sentiment and support for restrictionist policies. On his first 2016 campaign trail, Donald Trump capitalized on this criticism, regularly promising to end the program, which he subsequently did after assuming office, further contributing to the perception of a strong counter-productive backlash against pro-immigration policies (e.g., McHugh, 2018).

Our paper examines whether and how voters actually respond to significant immigration reforms by assessing the impacts of these policies on voters' evaluations of incumbents and policy preferences using all of the publicly available, high-quality survey data. Specifically, we employ an unexpected-event-during-survey research design and apply it to 10 large-scale, nationally representative U.S. surveys in the case of DACA and DAPA, with 6 surveys for anti-immigration policies. This approach allows us to capture the immediate effects of these policy announcements on public opinion, minimizing the influence of confounding factors.

Our analysis reveals that neither DACA nor DAPA has triggered significant voter backlash against immigration or incumbents. At the same time, the anti-immigration measures of DACA rescission and immigration bans had no effects on incumbent evaluations but may have decreased public opposition to immigrants and immigration. These findings suggest that while voters may indeed retaliate against *certain* pro-immigration measures by voting for anti-immigration parties and candidates, they can also accept such measures as legitimate, particularly when these policies are programmatic and value-aligned–that is, designed and implemented according to transparent principles, clearly communicated objectives, and focused explicitly on serving broader public interests in alignment with prevailing preferences.

The results of this study have important implications for policymakers, indicating that they might have limited cause for concern regarding public backlash against programmatic policies, even on such politically charged and high-profile issues as (unauthorized) immigration. Our findings contribute to the ongoing debates on policy feedback (Mettler and SoRelle, 2018; Béland et al., 2022; Patashnik, 2023) and the link between immigration policy and opinion (Van Hauwaert, 2023; Kustov, 2025), challenging the prevailing narrative of common anti-immigration backlash and highlighting the potential for public acceptance of selective pro-immigration reforms. In the end, public backlash to pro-immigration or other policies is neither automatic nor inevitable. The extent of backlash, if any, depends on the design of these policies and how effectively they align with broader public interest and values (see Appendix K), rather than the scope or the type of immigration involved.

Public Response to Salient Immigration Reforms: Backlash, Stability, and Legitimation

The concept of "public backlash" refers to an adverse reaction of the public to the advancement of certain social groups and policies by a significant segment of the population that may ultimately be counterproductive to this advancement (Thomas, 2008; Bishin et al., 2015; Abrajano and Hajnal, 2015; Norris and Inglehart, 2019; Patashnik, 2023). Importantly, backlash arguments are more than just about some people disagreeing with some policy changes. In a large society with many different opinions, it is impossible for everyone to accept any single change. Any consistent backlash argument is thus not just about the consequences of policy changes per se but rather about the *net* negative voter responses to these changes. In other words, backlash arguments are *counterfactual* arguments saying that pushing for too much change too fast may lead to less progress over time due to relatively stronger opposition from those who disagree with the cause (Kustov, 2023).

In the context of immigration, backlash is often conceptualized as an adverse public response to the rising physical presence of immigrants in terms of ethnic demographic change, an idea often dubbed as "group threat" (Pottie-Sherman and Wilkes, 2017; Kaufmann and Goodwin, 2018). However, this focus on immigration-induced demographic change as opposed to policy change may overlook a significant aspect of the issue since immigrant presence is neither necessary nor sufficient for backlash (Solodoch, 2021a). In fact, as can be seen from the reforms like DACA that we examine in this paper, many important pro-immigration advancements may not involve any immediate demographic change at all. At the same time, focusing on the effects of clearly identifiable policy changes is arguably more useful for informing policymaking than focusing on demographic changes co-determined by numerous factors beyond policy (for a discussion, see Clemens and Lewis, 2022).

Public backlash can also be understood as an extreme form of "thermostatic" public opinion (Wlezien, 1995; Van Hauwaert, 2023), where people react to prominent policy changes by adjusting their stated opinions about those changes in the direction of their actual preferences. In the case of immigration, where most people have skeptical attitudes, a high-profile pro-immigration reform could thus lead to more people voicing anti-immigration opinions and potentially voting for anti-immigration candidates to reverse the changes they disagree with. While thermostatic backlash may not necessarily indicate a fundamental shift in individuals' underlying preferences themselves, it can still be counter-productive to pro-immigration changes if it brings success to more anti-immigration politicians and parties who reverse those changes.

However, public policies can also generate their own support by altering people's incentives or by sending authoritative institutional signals about societal norms (Pierson, 1993; Lenz, 2012; Ura, 2014; Béland et al., 2022). For instance, in the case of legalizing gay marriage, most studies have not detected backlash effects and instead found that these legal advances increased their public acceptance (Bishin et al., 2015; Flores and Barclay, 2016; Abou-Chadi and Finnigan, 2019; Aksoy et al., 2020). Regarding immigration, while some cross-national evidence suggests thermostatic backlash effects to certain pro-immigration policies (Van Hauwaert, 2023), other evidence contradicts this (Kustov, 2023).

Despite its wide scope, previous policy feedback literature on immigration has been limited by its focus on aggregate analysis of time-series cross-sectional policy and opinion data. This approach obscures whether citizens are aware of policy changes and if they understand who is responsible for those changes, which is problematic because policy feedback requires salient policies and clear responsibility attribution (Soss and Schram, 2007; Hamel, 2024). Moreover, evidence suggests that only a minority of people are knowledgeable about the policy process (Kustov and Landgrave, 2025), and responsibility attribution can be particularly challenging for complex immigration legislation, especially under conditions of divided or coalition governments (Brown, 2010; Fortunato et al., 2021).

Similarly, much of the existing research on anti-immigration backlash has prioritized long-term trends across years or electoral cycles. Although it is widely understood that immigration attitudes tend to be stable over time (Kustov, Laaker, et al., 2021), short-term reactions among certain segments of the population may still arise, with some individuals reacting negatively and changing their stated preferences in response to salient policy announcements. Even if fleeting, such reactions could have outsized political implications, potentially shaping political behaviors such as increased protest participation. Conversely, if a significant policy reform does not provoke an immediate backlash, this absence suggests that the policy event itself is unlikely to generate systematic backlash over the long term.[1]

To address these limitations, we examine high-profile, stand-alone immigration reforms with clear responsibility attributions in the US context, where congressional gridlock has shifted legislation to presidential executive orders (Pierce and Bolter, 2020). We specify hypotheses regarding the possible backlash effects of major pro-immigration reforms (DACA and DAPA by the Obama Administration) and their anti-immigration counterparts ("Muslim Ban" and "Green Card Ban" by the Trump Administration). Investigating short-term dynamics thus complements long-term perspectives by testing whether immediate backlash forms a meaningful first step in broader backlash patterns or if, as some evidence suggests, immigration attitudes remain resistant to even highly salient interventions, both in the short term and over time (Kustov, Laaker, et al., 2021; Kustov, 2023).

All in all, according to *the thermostatic backlash hypothesis*, salient pro-immigration reforms like DACA should immediately increase anti-immigration preferences and support for anti-immigration politicians while decreasing support for pro-immigration incumbents like Barack Obama. Conversely, salient anti-immigration reforms like the "Muslim Ban" should decrease anti-immigration preferences and increase support for pro-immigration politicians while decreasing support for anti-immigration incumbents like Donald Trump.[2]

[1] Of course, a policy may have unanticipated long-term or indirect effects that generate public backlash later, particularly on novel issues with unsettled public opinion. However, all else being equal, the absence of short-term backlash reduces the likelihood of systematic long-term backlash, even if it does not rule it out entirely. For example, while one might argue that contemporary immigration politics reflects a delayed backlash to the 1965 Immigration and Nationality Act, such claims are difficult to substantiate or falsify.

[2] Unfortunately, to our knowledge, no comprehensive publicly available survey data exists regarding the latest 2021-2025 pro-immigration and anti-immigration actions of the Biden and second Trump administrations.

However, given the general stability of public preferences toward immigration (Kustov, Laaker, et al., 2021), a reasonable alternative expectation can be that even salient immigration policy changes may not significantly affect immigration preferences. Public opinion on immigration tends to remain relatively stable due to deeply rooted individual attitudes, group identities, and cultural values, which change slowly over time. Consequently, immediate shifts in immigration attitudes following high-profile policy events may be limited.

At the same time, both pro- and anti-immigration reforms might alter underlying social norms and legitimize these changes across some of the electorate, counteracting any backlash-induced shifts in anti-immigration preferences (Valentim, 2024). In other words, according to *the legitimation hypothesis*, voters' reactions to salient pro-immigration changes could be neutral to net positive, while their reactions to anti-immigration policies could be neutral to net negative.

Legitimation is particularly likely when a policy proposal, even on a generally controversial issue like immigration, resonates with broader pre-existing public attitudes and values. For instance, studies of same-sex marriage legalization indicate that policy shifts toward greater inclusivity often occurred in contexts where public acceptance had already begun to grow, reflecting changing social norms and values regarding equality and civil rights (e.g., Flores and Barclay, 2016). Similarly, policies perceived as "demonstrably beneficial"—those whose rationale is intuitively clear and understandable to most people without extensive explanation—may also garner greater acceptance (Kustov, 2025). By contrast, reforms perceived as demonstrably harmful or contrary to broadly held values–such as fairness or non-discrimination–may provoke backlash, even among voters generally cautious toward immigration.

Below, we begin by discussing our data, design, and analysis for understanding the immediate public opinion effects of pro-immigration reforms under Barack Obama followed by the anti-immigration reforms under Donald Trump.

Study 1: The Effects of Pro-Immigration Policies

Data and Design

Our main identification strategy is an unexpected-event-during-survey-design (UESD) (Muñoz et al., 2020), which leverages unexpected events during unrelated surveys to identify the causal effects of the phenomena underlying these events. To test our hypotheses about the effects of pro-immigration reforms, we compare the attitudes of respondents interviewed shortly before and after the DACA announcement (*DACA-A*, 2012-06-15), DACA implementation (*DACA-I*, 2012-08-15), and DAPA announcement (*DAPA-A*, 2014-11-20) across multiple surveys. These event dates serve as our treatment variables, coded as 1 if the respondent in the relevant survey is interviewed after each date and 0 otherwise.

We analyze three outcome types across the surveys: incumbent evaluations (*IE*), anti-immigration attitudes (*AIA*), and Tea Party support (*TPS*). *IE* is measured with multiple items approximating approval (explicit or implicit), favorability, vote intention, preferences, and confidence perceptions of Barack Obama. *AIA* is measured with multiple items measuring support for reducing immigration, increasing deportations, anti-immigration "show me your papers" bill (Arizona SB 1070), in addition to differentially negative evaluations of Latinos vis-a-vis white people. *TPS* is measured with multiple items approximating support for the Tea Party movement. *TPS* is a theoretically relevant outcome since prior research identifies *TPS* is primarily a function of group-based attitudes (specifically against immigrants) rather than small government conservatism (Barreto et al., 2011). All outcomes are standardized as z-scores. See Table 1 for a catalog of the surveys, independent variables, temporal domains, and outcomes included in our analysis. See Appendix Section A for methodological details on each survey. See Section B for more outcome measurement details.

Under the standard UESD assumptions, we estimate the following linear model:

$$Y_i = \alpha + \beta_1 Policy_i + \sum_{k=1}^{k} \beta_{k+1} X_i^k + \varepsilon_i$$

Table 1: Surveys and outcomes included in the analysis

Survey	Target Population	N	Policy Treatment	Outcome Measures (+Category)
Fox May-Jun '12	National Registered Voters	2732	DACA Announcement	Obama Approval (IE), Obama Vote Intent (IE), Tea Party Support (TPS)
GSS '12	National Adult Population	1302	DACA Announcement	Reduce Immigration (AIA)
Pew Jun. '12	National Population	2013	DACA Announcement	Obama Approval (IE), Obama Favorability (IE), Obama Vote Intent (IE), Deportations (AIA), SB 1070 Support (AIA), Tea Party Support (TPS)
IAT '12	Unrepresentive, Self-Selected	21024	DACA Announcement, DACA Implementation	Obama Approval (IE), Obama Favorability (IE), Obama D-Score (IE)
TAPS Jun. '12	National Adult Population	1693	DACA Announcement	Obama Approval (IE), Obama Favorability (IE), Obama Vote Intent (IE), Obama Confidence (IE), Dem-GOP Approval (IE), White - Latino FT (AIA)
Gallup Tracking Poll (2008-2016)	National Adult Population	1.3m	DACA Announcement, DACA Implementation, DAPA Announcement	Obama Approval (IE)
TAPS Aug. '12	National Adult Population	1707	DACA Implementation	Obama Approval (IE), Dem-GOP Approval (IE)
TAPS Nov. '14	National Adult Population	1522	DAPA Announcement	Dem-GOP Approval (IE), Tea Party Support (TPS)
IAT '14	Unrepresentative, Self-Selected	16207	DAPA Announcement	Obama Approval (IE), Obama Favorability (IE), Obama D-Score (IE)
CES '14	National Registered Voters	48853	DAPA Announcement	Tea Party Support (TPS)

Note: IE = incumbent evaluations, AIA = anti-immigration attitudes, TPS = Tea Party support

Y_i is the outcome of interest (*IE*, *AIA*, *TPS*) for respondent i. $Policy_i$ is an indicator equal to 1, 0 otherwise, if i is interviewed either after the DACA announcement, DACA implementation, or DAPA announcement. $\sum_{k=1}^{k} \beta_{k+1} X_i^k$ are k covariates measuring some permutation of age, gender, race, college-educated, income, ideology, partisanship, and geography across the surveys (see Section C for control covariate availability across surveys). ε_i are robust errors. If the backlash hypothesis is supported, coefficients should be negative for the *IE* outcome, and positive for *AIA* and *TPS* outcomes.

We validate UESD assumptions to show our post-*policy* coefficients are relatively insulated from bias. The core UESD identifying assumption is *ignorability*. Treatment should be independent of potential outcomes conditional on random sampling (or consistency in survey self-selection regardless of treatment). Thus, respondents interviewed pre- and post-*policy* should be compositionally similar. Section D shows respondent characteristics are largely balanced pre/post-*policy* except for in the IAT '14 and CES '14 datasets.[3] Therefore, in the aggregate, our statistical conclusions are unlikely to be perturbed by omitted variable bias and the ignorability assumption is well-supported.

Another UESD identification assumption is *excludability*: differences between respondents interviewed pre/post-*policy* should be the sole consequence of the *policy*. "Treatment" is not just the policy, but collateral media attention. However, outside DACA/DAPA, there are no punctuated moments of media attention over immigration or the respective policies in the year(s) these policies were implemented other then the media attention that occurred immediately after the policy announcement/implementation (Section E2), suggesting the absence of simultaneous immigration-related events that may motivate immigration policy backlash. Moreover, given our *IE* outcomes focus on evaluations of President Obama, it is important to demonstrate Obama's salience does not increase near the moment DACA/DAPA are announced or implemented. Indeed, Google search data shows there are no punctuated moments of increased Obama salience that could reshape mass evaluations of him around DACA/DAPA (Figure E3), further validating the excludability assumption.

[3]Note, we adjust for this imbalance in our main estimates.

We further validate the excludability assumption by ruling out if our post-*policy* effects are driven by pre-*policy* outcome time trends. We subset our data to the pre-policy period and assess the "effect" of being interviewed after the median pre-*policy* date and find identify largely null effects on our outcomes of interest (Section F). Therefore, our main estimates are unlikely to be perturbed by secular attitudinal trends, anticipatory effects, or other events, but rather, isolate the effect of the immigration policies of interest.

For each outcome (*IE*, *AIA*, *TPS*), we present a study-adjusted random-effects Hartung-Knapp meta-analytic estimate across the surveys. The Hartung-Knapp approach is optimal since it adjusts standard errors in the presence of effect heterogeneity across studies (preventing Type I error) (IntHout et al., 2014). "Study-adjusted" means effect coefficients are averaged within each survey study, preventing artificial standard error deflation.

Analysis and Results

Figure 2 visualizes all 30+ of our before and after comparisons across various outcomes, the overall evidence is not consistent with the idea that high-profile immigration reforms cause counterproductive voter backlash. The standardized meta-analytic coefficient of *DACA-A*, *DACA-I*, and *DAPA-A* on pro-*IE*, *AIA*, and *TPS* is 0.004 (SE = 0.02, p = 0.5), -0.05 (SE = 0.04, p = 0.31), and 0.01 (SE = 0.03, p = 0.8). The meta-analytic coefficient signs for *IE* and *AIA* are in the opposite direction of the backlash hypothesis. Thus, high-profile pro-immigration policies like DACA or DAPA do not appear to motivate backlash *on average*. Effect size research posits a standardized effect of 0.1 is substantively negligible (Cohen, 2013). By extension, under an equivalence test, coefficients are deemed negligible if their 95% CIs are within ± 0.1 SD (Lakens et al., 2018). All meta-analytic coefficients are within ± 0.1 SD, so they are both statistically insignificant and *substantively negligible* under the equivalence test.

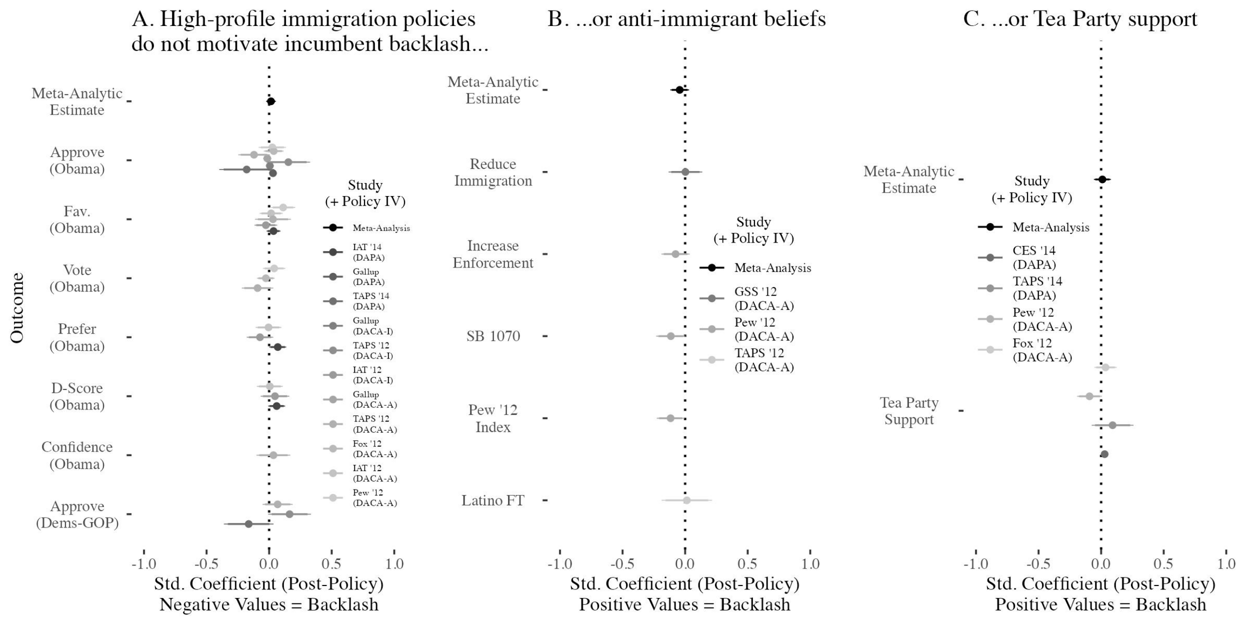

Figure 2: DACA and DAPA did not cause backlash. X-axis is the standardized *post-policy* coefficient, y-axis is the outcome. Panels A-C characterize outcomes related to incumbent backlash, anti-immigration attitudes, and Tea Party support. Color denotes study and policy (i.e., DACA announcement, DACA implementation, DAPA announcement) analyzed. 95% CIs displayed from HC2 robust SEs. Random-effects Hartung-Knapp meta-analytic estimates are displayed and are study-adjusted to prevent artificial SE deflation. Estimates using surveys targeting representative populations are population-weighted.

Heterogeneous Effects by Partisanship and Ideology

We evaluate if our null result is masking countervailing effects conditional on the major political dispositions favoring/disfavoring pro-immigration policies related to partisanship (Ollerenshaw and Jardina, 2023).[4] The meta-analytic coefficients for *DACA-A*, *DACA-I*, and *DAPA-A* on pro-*IE*, *AIA*, and *TPS* are statistically null for both Democrats/Liberals (Figure G8) and Republicans/Conservatives (Figure G9). These results are powerful evidence against the backlash hypothesis, because they suggest individuals who are disposed toward opposing (supporting) pro-immigration incumbents (e.g., Obama) and immigrants have already decided their position on the outcomes of interest prior to the introduction of immigration policies. Anecdotal evidence of backlash to pro-immigration policies may reflect existing attitudinal predispositions instead of attitudinal *changes* among the mass public.

[4] In the IAT surveys, where partisanship is not available, we use liberal-conservative political ideology instead.

Robustness

Given our *post-policy* coefficients are ITT effects, the absence of backlash effects may be due to weak treatment reception. We believe this is unlikely for several reasons. The two policies we examine are highly salient such that the mass public may have plausibly "received-the-treatment." DACA and DAPA were announced with afternoon and primetime evening speeches by President Barack Obama and led to large, discontinuous, increases in immigration-related media coverage and information-seeking (Figure E2-E6). The public is more likely to indicate immigration is the most important issue, at least after the DACA announcement, implying the public "received-the-treatment" (Table H2). A survey fielded immediately *post-DACA* demonstrates many Americans were following the policy closely, more than other salient issues (Syria, Egypt, Colorado wildfires, the EU economic crisis, the Sandusky scandal) and second only to the economy and the 2012 election. Two surveys fielded immediately *post-DAPA* demonstrate most Americans have heard about the policy and were following the policy more or just as closely than other salient issues (e.g., Ebola, ISIS, Michael Brown's murder, see Figure H11). In sum, our auxiliary evidence suggests large segments of the American public perceived DACA and DAPA and "received-the-treatment" such that if there were ITT effects (in either direction), we should observe them in the data.

Furthermore, one can argue that the prospect of weak treatment reception is *not a shortcoming*, but an advantageous feature of our externally valid research design. Exposure to information concerning pro-immigration policies does not occur in a vacuum (e.g., within the confines of a controlled survey experimental manipulation), but rather in a noisy political space that may otherwise attenuate the prospect of backlash effects. By using a UESD to assess attitudinal responses to "real-world" events, our evidence accounts for this noisy political space and thus better reflects the reality of public attitudes in response to pro-immigration policies (compared to lab or survey experiment designs).

Study 2: Evaluating Anti-Immigration Policies

Study 1 provides robust data-driven evidence using the UESD strategy, but it is limited in that it only evaluates the effect of pro-immigration, not all kinds of policies. To address this limitation, Study 2 amasses several different data sources to evaluate the effect of three high-profile anti-immigration policies implemented during the Trump administration on incumbent evaluations and anti-immigration attitudes: (1) the Muslim Ban (*MB*, 2017-01-27), (2) the DACA rescission (*DACA-R*, 2017-09-05), and (3) the green card ban (*GCB*, 2020-04-22). We focus on these policies for three reasons. First, they were nationally salient, with sharp increases in attention to the respective policies and immigration around the moment these policies are implemented. Second, like DACA and DAPA with Obama, these policies possess a clear connection of responsibility to Donald Trump given they are all executive orders, allowing us to effectively assess the link between anti-immigration policies and incumbent evaluations. Third, these policies have clear moments of policy onset which allows for an effective comparison between respondents in available surveys interviewed before and after the policy. Other high-profile anti-immigration policies during the Trump administration, like the child separation policy, were comprised of a series of events and were noticed by the public and media well after policy onset[5]. As a result, the onset of increased public attention to the policy is not particularly clear nor amendable to a credible identification strategy.

Muslim Ban

Context

In December 2015, Donald Trump announced during a rally in Mount Pleasant, South Carolina, his intention to impose a "total and complete shutdown of Muslims entering the United States" if elected president.[6] He renewed this proposal after the ISIS-inspired Pulse

[5] https://www.upi.com/Top_News/US/2019/01/17/Audit-finds-thousands-more-migrant-kids-were-separated-from-families/5671547743793

[6] https://www.c-span.org/video/?401762-1/presidential-candidate-donald-trump-rally-mount-pleasant-south-carolina&start=1830

nightclub massacre in June 2016, continuing to emphasize it throughout his presidential campaign. Following his inauguration, Trump signed an executive order on January 27, 2017, banning entry into the U.S. for citizens from Iran, Iraq, Libya, Somalia, Sudan, Syria, and Yemen for at least 90 days, regardless of valid non-diplomatic visas.

The Muslim Ban sparked immediate and widespread condemnation from Democratic elites, international actors, and approximately 1,000 career U.S. diplomats. Republicans were divided, with some, including Senators John McCain, Lindsey Graham, and Susan Collins, criticizing the policy's implementation. Immediate legal challenges prompted Trump to issue a revised executive order on March 6, 2017, clarifying that it did not explicitly target Muslims and exempted green card holders. Although this revision was temporarily blocked by the U.S. District Court for Hawaii, the Supreme Court reinstated it on June 26, 2017.[7]

The Muslim Ban quickly became highly salient among the public. A Pew poll conducted shortly after its implementation found nearly 80% of respondents heard about the policy "a lot" (Figure J16). Google Trends data similarly demonstrate that searches related to the Muslim Ban and immigration sharply increased to record levels immediately following the policy's announcement, suggesting minimal anticipation by the public (Figure J17).

The policy also prompted substantial public protest, particularly at major U.S. airports. Data from the Crowd Counting Consortium at Harvard's Ash Center indicate a sharp, discontinuous increase in the number and size of protests, averaging 25,000 protesters per day immediately following implementation (Figure J13, Panels A-B). Given prior research linking protests to shifts in public opinion by increasing issue salience (Reny and Newman, 2021), such widespread demonstrations may have influenced public attitudes toward Trump and immigration policy.

[7] See `https://www.aclu-wa.org/pages/timeline-muslim-ban`.

Data and Design

We use several datasets to assess the consequences of the Muslim Ban (*MB*) on incumbent evaluations (*IE*) and anti-immigration attitudes (*AIA*). To assess the relationship between *MB* implementation and *IE*, we use three cross-sectional nationally representative surveys consistently measuring *Trump favorability* shortly before and after the *MB*. These are the ABC ($N = 1005$, fielded 01/12/2017-01/15/2017), PRRI ($N = 1013$, 01/18-01/22), and Pew ($N = 1503$, 02/07-02/12) polls. *Trump favorability* is a 4-point scale item from "very unfavorable" to "very favorable." We estimate the effect of being interviewed in the Pew poll post-*MB*, a binary indicator, on *Trump favorability* adjusting for and without common control covariates across the surveys (age, gender, race, college-educated, partisanship). All covariates are rescaled between 0-1. The PRRI poll serves as a temporal placebo. If *Trump favorability* levels in the PRRI poll (1/18-1/22) are statistically similar to *favorability* levels in the ABC poll (1/12-1/15), we can be more confident our post-*MB* estimates are not a function of unobserved pre-*MB* temporal trends (e.g., a Trump inauguration or Women's March effect). Indeed, we find *Trump favorability* is the same between 1/12-1/15 (ABC poll) and 1/18-1/22 (PRRI poll), suggesting unobserved temporal trends affecting *Trump favorability* do not drive our results (Figure 3, Panel A). Moreover, our results are unlikely to be driven by compositional shifts between surveys since respondent characteristics are balanced between respondents interviewed before and after the *MB* (Figure J18).

To assess the relationship between *MB* implementation and *AIA*, we focus on two outcomes: 1) Muslim Ban support and 2) anti-Arab attitudes. For the Muslim Ban support outcome, we use four nationally representative CBS polls consistently measuring *MB support* between December 2015-February 2017 (fielded 12/2015, $N = 1011$; 06/2016, $N = 1001$; 07/2016, $N = 1600$; and 02/2017, $N = 1019$).[8] *MB support* is a binary indicator based on a respondent

[8] *MB support* is measured in the *exact same way* across these polls over time. This is important since prior research shows question wording can drastically affect support for the Muslim Ban (source: `https://www.washingtonpost.com/news/politics/wp/2017/02/02/do-americans-support-trumps-immigration-action-depends-on-whos-asking-and-how/`).

reporting "should ban" in response to a question asking respondents if they think "the U.S. should temporarily ban all Muslims...from entering the United States" as opposed to "should not ban." We estimate the effect of being interviewed in the final CBS poll (which was post-*MB*), a binary indicator, on *MB support* adjusting for and without common control covariates across the surveys (age, gender, race, college-educated, partisanship). All covariates are rescaled between 0-1. Again, we find *MB support* is stable in the CBS polls between December 2015 and July 2016 (Figure 3, Panel B), so we can be more confident unobserved pre-*MB* temporal trends are not driving our post-*MB* coefficient estimates. One issue is that there is a significant amount of time elapsed between the first and final CBS poll fielded post-*MB* (07/2016) and the final CBS poll fielded pre-*MB* (02/2017), so intervening temporal factors may explain our results. However, we believe the risk of intervening temporal factors is limited given Google Trends Search data demonstrates the Muslim Ban was not at all salient between July 2016 and February 2017 until the Muslim Ban was actually implemented through Trump's executive order (Figure J15). Finally, our results are unlikely to be driven by compositional shifts between surveys since respondent characteristics are balanced between respondents interviewed before and after the *MB* in the CBS polls measuring *MB support* (Figure J19).

For the anti-Arab attitude outcomes, we use data from the 2016 Project Implicit Arab Implicit Association Test (A-IAT) survey,[9] an opt-in survey that we subset to U.S. non-Muslim[10] adults who completed the entire survey ($N = 30608$, 84 daily respondents on average). Unlike the CBS polls measuring *MB support*, the A-IAT is opt-in and unrepresentative. Relative to the CBS polls, the A-IAT has a younger (31yo vs. 48yo), more woman (63% vs. 51%), more

[9]Source: `https://osf.io/t8u7p/`

[10]We focus on non-Muslims because Muslims may have a different interpretative framework concerning the Muslim Ban and their attitudes toward Arab Muslims. Moreover, since the A-IAT is an opt-in survey, it may overrepresent Muslims. Indeed, we find a) Muslims hold much more positive attitudes toward Arab Muslims relative to non-Muslims; b) the effect of the *MB* on reducing anti-Arab attitudes is smaller for Muslims relative to non-Muslims because their attitudes concerning Arab Muslims have less space to travel than non-Muslims (Table J4); c) Muslims are overrepresented in the A-IAT, composing 7% of the overall sample. The exclusion of Muslim respondents should not affect representativeness given only 1% of the US population is Muslim (see: `https://www.pewresearch.org/short-reads/2018/01/03/new-estimates-show-u-s-muslim-population-continues-to-grow`).

college-educated (49% vs. 30%), more white (70% vs. 64%) and more liberal (58%) sample on average. However, the lack of representativeness may be inconsequential or advantageous for three reasons. First, although the A-IAT sample may be less likely to hold anti-Arab attitudes relative to the general population (except on the dimension of race, since white people are more antipathic toward Arab Muslims),[11] this may also mean our estimates concerning the effects of the Muslim Ban on anti-Arab attitudes in the A-IAT are underestimated relative to a representative sample due to ceiling effects on the extent to which pro-Arab Muslim attitudes can travel among a sample positively predisposed toward Arab Muslims. Second, prior research demonstrates exposure to external stimuli in unrepresentative surveys generates similar effects as exposure to external stimuli in representative surveys (Coppock, 2019; Roman and Thompson, 2024). Third, if we identify consistent attitudinal responses between the nationally representative CBS polls and the A-IAT (which we end up identifying), we can be more confident survey representativeness does not fully explain our results.

We measure three anti-Arab attitude outcomes. The first two are *anti-Arab bias* and *anti-Arab favorability*, which are explicit measures of anti-Arab attitudes. *Anti-Arab bias* is on a 1-7 scale from “I strongly prefer Arab Muslims to Other People” to “I strongly prefer Other People to Arab Muslims.” *Anti-Arab favorability* is the difference in two items measuring warmth toward “other people” and “Arab Muslims” on a 1-10 scale from “extremely cold” to “extremely warm.” The third outcome is an *anti-Arab D-score*, a normalized measure from -2-2 of how quickly respondents associate negative (positive) phrases to Arab Muslim (other) names relative to associating positive (negative) phrases to other (Arab Muslim) names. Higher values suggest implicit bias against Arab Muslims (i.e., associating negative attributes to Arab Muslims) (Greenwald and Lai, 2020). Given indirect measurement, the *D-score* may be less influenced by impression management to be perceived as pro-Muslim post-Muslim Ban (Greenwald and Lai, 2020). Therefore, we can assess relatively quick,

[11] Indeed, auxiliary analyses shows younger, college-educated, women, and liberal respondents are more less likely to hold anti-Arab attitudes while white respondents are more likely to hold anti-Arab attitudes relative to non-whites (Table J6).

negative, emotional responses (i.e., System 2 responses) to Arab Muslims in addition to more deliberate evaluations of Arab Muslims (i.e., System 1 responses) (Greenwald and Lai, 2020). Although the IAT is not insulated from introspection, the modest correlation between the *D-score* and explicit bias suggests the IAT measures attitudes that are difficult to manipulate. Therefore, the *D-score* is valuable since we can demonstrate the adoption of positive attitudes toward Arab Muslims post-Muslim Ban may not be impression management. The *D-score* is well-established and associated with objective covariates characterizing subordination (Ratliff and Smith, 2021).

We estimate the effect of taking the A-IAT post-*MB* (01/27/2017), a binary indicator, on the three aforementioned outcomes adjusting for and without controls (age, woman, white, college-educated, ideology, non-metro residence, religiosity, state indicators for Texas, New York, Illinois, California, and Florida) for subsample bandwidths that are 1-30 days before and after the Muslim Ban. All covariates are rescaled between 0-1.

Like Study 1, our identification strategy using the A-IAT data is consistent with an unexpected-event-during-survey design (UESD). The core UESD identification assumption is *ignorability*: self-selection into taking the survey should be independent of potential outcomes and treatment (Muñoz et al., 2020). Given the motive to self-select into taking the A-IAT may shift over time, we expect respondent characteristics in temporal bandwidth subsamples that are further from the Muslim Ban to be increasingly imbalanced. Indeed, we generally find support for the *ignorability assumption* in the form of respondent covariate balance before and after the Muslim Ban in bandwidth subsamples closer to the onset of the Muslim Ban, but increased imbalance further from the Muslim Ban onset (Figure J21). Therefore, we focus on and primarily interpret post-*MB* coefficient estimates using the 6 day bandwidth A-IAT subsample, which includes a large number of respondents ($N = 2071$) and relatively limited covariate imbalance on only 2/12 covariates (age, college-educated).

Additionally, we provide evidence against the possibility our post-*MB* coefficients using A-IAT data are driven by pre-*MB* outcome trends by showing the placebo effect of taking the 2016 A-IAT after the median pre-*MB* date (January 14th) on anti-Arab attitudes while censoring all post-*MB* data is statistically null (Muñoz et al., 2020) (Figure J22). These null temporal placebo effects suggest pre-*MB* factors (e.g., the Women's March, possible anticipatory effects concerning the Muslim Ban, Trump's other immigration executive orders such as the sanctuary city ban on January 25th) are not driving our results.

Moreover, we provide evidence our post-*MB* coefficients are not driven by unobserved factors inherent to being interviewed before and after the calendar day the Muslim Ban is implemented (January 27th). We leverage A-IAT data three years prior and after (2014-2016, 2018-2020) the year the Muslim Ban was implemented and show being interviewed after January 27th in years when the Muslim Ban was not implemented is not associated with shifts in anti-Arab attitudes (Figure J23), implying our results are not driven by an unobserved temporal trend inherent to the month of January.

Finally, we demonstrate our post-*MB* coefficients are not driven by an external generalized prosocial attitudinal trend toward marginalized groups. We leverage other Project Implicit surveys on anti-disabled, anti-old, anti-Asian, anti-women, anti-indigenous, anti-Black, and anti-LGBT attitudes and demonstrate respondents surveyed after the Muslim Ban are not more likely to hold positive or negative attitudes toward other marginalized groups (Table J3). These findings suggest shifts in attitudes toward Arab Muslims after the Muslim Ban are not a function of a general external attitudinal trend concerning marginalized groups, but rather, the Muslim Ban.

Results

Consistent with the effects of pro-immigration policies on *IE* in Study 1, Trump's *favorability* is stable before and after the Muslim Ban between January 12th-February 12th (Figure 3, Panel A).[12] The post-*MB* effect on *Trump favorability* is precisely 0 ($p = 0.84$). If we apply the same equivalence test as in Study 1 ($\pm$ 0.1 standard deviations), the standardized

[12]For more information on the estimation strategy we use across the various surveys, see Section J.2.1

post-*MB* effect on Trump favorability is also substantively negligible as it is within $\pm$ 0.1 (β = -0.006 standard deviations, SE = 0.03).

Conversely, we find evidence suggesting the Muslim Ban motivated backlash against the policy itself, both by undercutting support for the policy and motivating support for the group disparately affected by the policy (i.e., Arab Muslims). The post-*MB* effect on *MB support* is -15 percentage points (Figure 3, Panel B), a substantively large 33% of the outcome standard deviation and 44% of the pre-*MB* outcome mean.

Likewise, honing in on the 6-day bandwidth A-IAT subsample pre/post-*MB*, we find respondents interviewed post-*MB* were less likely to report *anti-Arab bias* and *anti-Arab favorability* by 0.02 ($p < 0.01$) and 0.01 points ($p < 0.05$) on the 0-1 outcome scale (Figure 4, Panels A-B), but do not possess lower levels on the *D-score* ($p = 0.71$) (Figure 4, Panel C). These statistically significant coefficients are equivalent to 14% and 9% of the respective outcome standard deviations, and possess standardized confidence intervals outside the negligible effect equivalence test threshold of $\pm$ 0.1 standard deviations that we outline in Study 1. Although explicit anti-Arab Muslim attitudes decline post-*MB* (*anti-Arab bias*, *favorability*), implicit anti-Arab attitudes do not (the *D-score*). These results are still meaningful in terms of the reductions in anti-Arab attitudes because although people may still be implicitly biased against Arab Muslims in the same way pre-*MB*, they understand that explicitly expressing anti-Arab bias became less socially acceptable after the Muslim Ban.

Heterogeneous Effects by Partisanship and Ideology

We assess if the Muslim Ban had heterogeneous effects on *Trump favorability*, *MB support*, and *anti-Arab attitudes* by partisanship and ideology (in the A-IAT survey, since partisanship is not measured in it). *Trump favorability* was largely stable among Democrats pre/post-*MB*, which was already very low at baseline pre-*MB*. However, Trump favorability increased post-*MB* for Republicans while declining among independents, resulting in a net-zero shift

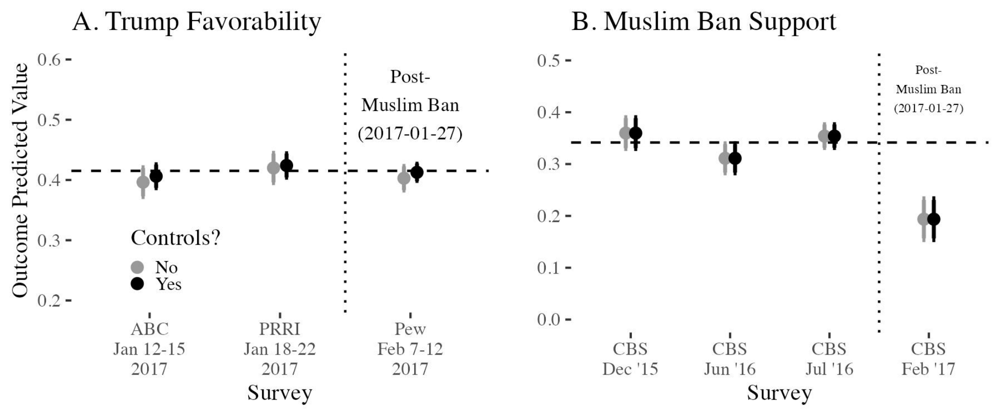

Figure 3: There was no incumbent backlash to the Muslim Ban, but some suggestive evidence of pro-immigration policy backlash. Panels A/B characterize predicted values of Trump favorability and Muslim Ban support (y-axis) across several surveys (x-axis) fielded near the moment the Muslim Ban was signed by President Donald Trump (2017-01-27, denoted by dotted vertical line) adjusting and not adjusting for control covariates (denoted by color: age, gender, race, college-education, partisanship). Dashed horizontal line characterizes average of pre-Muslim Ban outcome level. Vertical line denotes pre/post-Muslim Ban. Estimates are population-weighted. 95% CIs displayed from HC2 robust SEs.

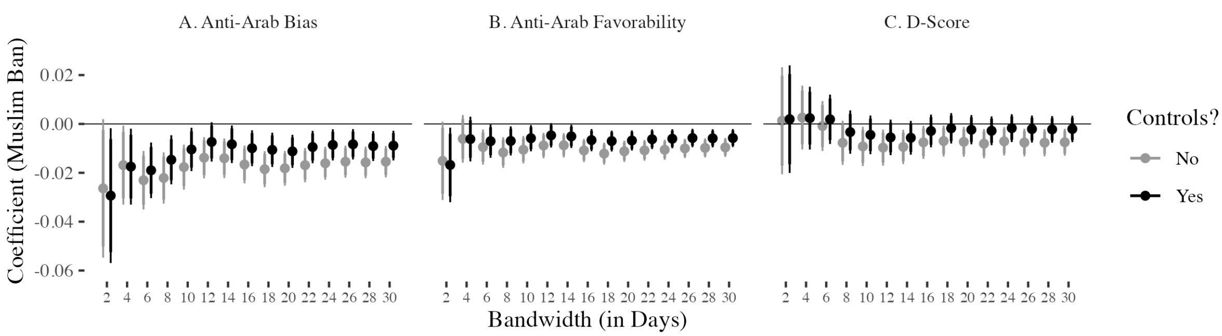

Figure 4: Anti-Arab attitudes decreased post-Muslim Ban. Each panel characterizes a different outcome. The x-axis is the bandwidth (in days), the y-axis is the post-*MB* coefficient. Color denotes the inclusion/exclusion of control covariates. 95% CIs displayed from HC2 robust SEs.

in favorability post-*MB* on average (Figure J24). Conversely, the decline in *MB support* post-*MB* occurs among *both* Democrats and Republicans (Figure J25). Therefore, it appears that although Republicans may have liked Trump's commitment to his campaign promise to implement the Muslim Ban, the policy itself was so unpopular they were willing to distance themselves from it. Consistent with our findings demonstrating the *MB* did not have a heterogeneous influence on *MB support* by partisanship, we also do not find the post-*MB* influence on anti-Arab attitudes is heterogeneous by political ideology in the A-IAT survey (Table J5).

DACA Rescission

Context

On September 5, 2017, Donald Trump announces a memo rescinding DACA, immediately triggering several lawsuits. Trump's memo deferred implementation of the rescission for six months to allow Congress time to legislatively pass protections for undocumented immigrants covered by DACA. Although Congress failed to act by March 5, 2018, several U.S. district courts ordered an injunction preventing the phase-out of DACA. The Supreme Court eventually ruled that the rescission of DACA was arbitrary and capricious under the Administrative Procedure Act on June 18, 2020.

Trump's DACA rescission was salient. Google Trends Search data shows search interest in immigration- and DACA-related terms discontinuously increased after the DACA rescission (Figure J26). Importantly, the salience of many of these search terms was at their highest point in the 30 days before and after the DACA rescission. Moreover, the sharp, discontinuous increase in salience implies the public did not anticipate an increase in DACA rescission-related salience, suggesting our effects are not driven by pre-rescission anticipatory factors. Like the Muslim Ban, the DACA rescission resulted in several protests against the policy throughout the nation. Regression-discontinuity-in-time estimates using data from the Crowd Counting Consortium show the DACA rescission led to an immediate increase in the number of pro-DACA protests, with 29 protests per day and 4600 protesters participating per day on average throughout the nation (Figure J13, Panels C-D).

Data and Design

We assess the effect of Trump's DACA rescission (9/5/2017, *DACA-R*) on incumbent evaluations in the form of the public's approval of Trump's job as president (*approval*). We use a compilation of approval topline data from several surveys by FiveThirtyEight. This data is high frequency, with 5 topline polls sampling 23000 respondents per day on average. The data are at the topline-level.

We use a regression discontinuity-in-time (RDiT) design to evaluate the discontinuous effect of *DACA-R* on *approval*. We use the mean-squared optimal bandwidth approach by Calonico et al. (2014). For the sake of thoroughness, we present RDiT estimates using multiple kernels (uniform, triangular, epanechnikov) and polynomials (0, 1, 2, 3) for the running variable (days to *DACA-R*). Given some survey toplines contain more information in the FiveThirtyEight data as a result of survey sample size differences, we weigh our RDiT estimates by topline survey sample size. We use a RDiT design to assess the effect of *DACA-R* on *approval* for several reasons. First, the design is less susceptible to secular pre-treatment attitudinal trends given our quantity of interest (the discontinuous *DACA-R* coefficient) is a discontinuous effect. Second, descriptive statistics fitting loess models on each side of the moment *DACA-R* is implemented shows there is a pre-*DACA-R* increase in Trump's *approval* (Figure 5, Panel A), which could bias difference-in-means estimates characteristic of our other estimation strategies (e.g., the unexpected-event-during-survey design).

Results

We find limited evidence *DACA-R* discontinuously affected Trump's *approval* (Figure 5, Panel B). RDiT estimates using a running variable polynomial equal to 0 or 1 suggests *DACA-R* increased Trump's approval. But, this result is not robust to alternative RDiT specifications. Post-*DACA-R* RDiT estimates that are better at accounting for, for example, positive pre-*DACA-R* *approval* trends using running variables with polynomials equal to 2 or 3 are statistically insignificant. We largely interpret these results as consistent with our other findings suggesting immigration policies have a limited influence on incumbent evaluations.

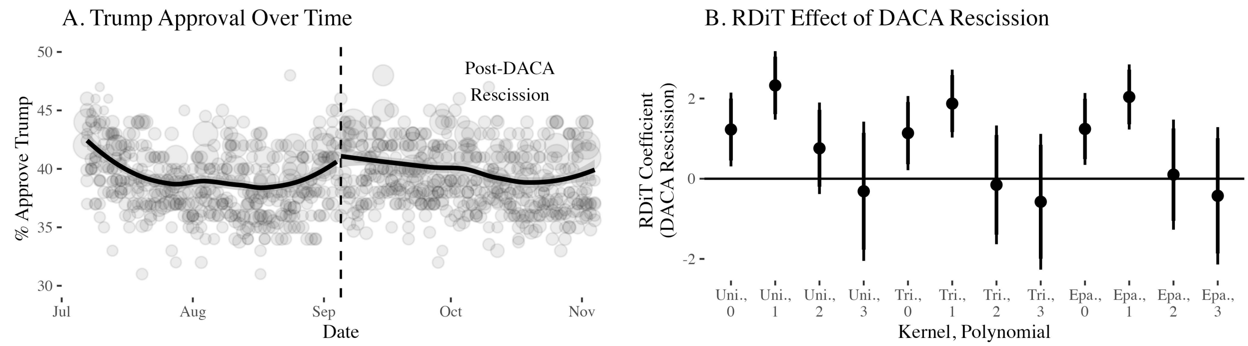

Figure 5: There was no shift in Trump approval after the DACA rescission. Panel A characterizes Trump approval over time using 538 approval topline data from several surveys (survey sample size denoted by dot size) two months before and after the DACA rescission. Dashed vertical line denotes moment of DACA rescission (2017-09-05). Loess models fit on each side of the DACA rescission. Panel B characterizes regression discontinuity-in-time estimates assessing the immediate effect of the DACA rescission on Trump approval (y-axis) by different kernel and polynomial specifications using the mean-squared optimal bandwidth approach by Calonico et al. (2014). Estimates weighted by survey sample size. 95% CIs displayed from robust SEs.

Green Card Ban

Context

On April 22, 2020, Donald Trump implemented an executive order suspending the issuance of green cards to immigrants abroad, even if they have family members who are green card holders or U.S. citizens in the United States. Trump justified the policy on the basis that US jobs needed to be protected amid an employment crisis brought on by the COVID-19 pandemic.[13] Originally, the policy was meant to be in place for 60 days, but on June 20 it was extended until after the end of the Trump presidency when President Joe Biden revoked it on February 24, 2021.[14]

Trump's green card ban was relatively salient. Google Trends Search data shows immigration-related search terms discontinuously increased and peaked to their highest level the moment Trump announces the green card suspension in the 30 days before and after

[13] https://www.vox.com/policy-and-politics/2020/4/21/21229286/trump-immigration-ban-executive-order-coronavirus

[14] https://www.dw.com/en/us-president-joe-biden-reverses-trump-green-card-ban/a-56682561

the suspension (Figure J27). However, unlike the DACA rescission or Muslim Ban, there was no commensurate protest activity against the policy the moment it was implemented, which could either reflect the limited impulse to protest the policy among the mass public or constraints on outside movement during the COVID-19 pandemic (Figure J13, Panels E-F).

Data and Design

We evaluate the influence of Trump's Green Card Ban (*GCB*, 04/22/2020) on incumbent evaluations and anti-immigration attitudes using data from the Democracy Fund+UCLA Nationscape survey (NS). The NS is an online sample continuously fielded between June 2019-January 2021 by LUCID ($N = 465,000$, $N = 870$ mean daily respondents). The NS is a high quality sample. LUCID targeted census quotas on several socio-demographic covariates in addition to filtering out repeat respondents. The NS was also weighted to several government benchmarks to ensure representativeness. Indeed, evidence shows the NS produced similar statistics as gold standard nationally representative surveys (Tausanovitch et al., 2019).

We measure incumbent evaluations with three outcomes. *Trump approval* is a binary indicator if the respondent indicates they "strongly approve" or "somewhat approve" of the way Donald Trump is handling his job as opposed to somewhat or strongly disapproving. *Trump favorability* is a binary indicator if a respondent reports they have a "very favorable" or "somewhat favorable" impression of Donald Trump as opposed to "somewhat unfavorable" or "very unfavorable." *Trump vote* is a binary indicator equal to 1 if a respondent reports they would vote for Trump over Biden in the general election.

We measure anti-immigration attitudes with six outcomes. *Wall* is a binary indicator equal to 1 if a respondent agrees the government should build a wall on the southern US border. *Deport* is a binary indicator equal to 1 if a respondent agrees the government should deport all undocumented immigrants. *No citizenship* is a binary indicator equal to 1 if a respondent disagrees with creating a pathway to citizenship for all undocumented immigrants.

No DREAM is a binary indicator equal to 1 if a respondent disagrees with creating a pathway to citizenship for undocumented immigrants brought to the US as children. *Undocumented Unfavorability* is a binary indicator equal to 1 if a respondent reports they have a "somewhat unfavorable" or "very unfavorable" impression of undocumented immigrants as opposed to somewhat or very favorable. *Latino Unfavorability* is a binary indicator equal to 1 if a respondent reports they have a "somewhat unfavorable" or "very unfavorable" impression of Latinos as opposed to somewhat or very favorable.

Our identification strategy is again an unexpected-event-during-survey design (UESD). We compare outcome levels between respondents interviewed before and after the *GCB* announcement while adjusting or not adjusting for various sociodemographic covariates (age, white, woman, college-educated, income, partisanship, ideology). Since we cannot be certain all respondents "received-the-treatment" by learning about the GCB, our post-*GCB* coefficient is an intent-to-treat (ITT) estimate and likely an under-estimate of the GCB effect on the truly treated.

Given the NS is based on an online panel, the key identifying UESD assumption (ignorability) is that potential outcomes and self-selection into the survey are independent of treatment. Using several subsample bandwidths 2-30 days before and after the GCB (in 2-day intervals), we find evidence in support of the ignorability assumption particularly for subsample bandwidths 22-30 days post-*GCB*, where there is usually imbalance on only 1/8 control covariates (Figure J28). Since the 22 day bandwidth subsample is the first subsample with only 1 imbalanced covariate and a relatively large sample size, we focus on interpreting post-*GCB* estimates using this subsample for the sake of brevity.

We also rule out the possibility our results may be driven by pre-*GCB* outcome trends (e.g., the onset of COVID in March 2020). We censor post-*GCB* data in the NS and evaluate the placebo effect of being interviewed 22 days pre-GCB relative to 23-44 days pre-GCB. We do not find the temporal placebo has an effect on our outcomes of interest, implying our post-*GCB* coefficient estimates are unlikely to be the product of a preexisting outcome trend

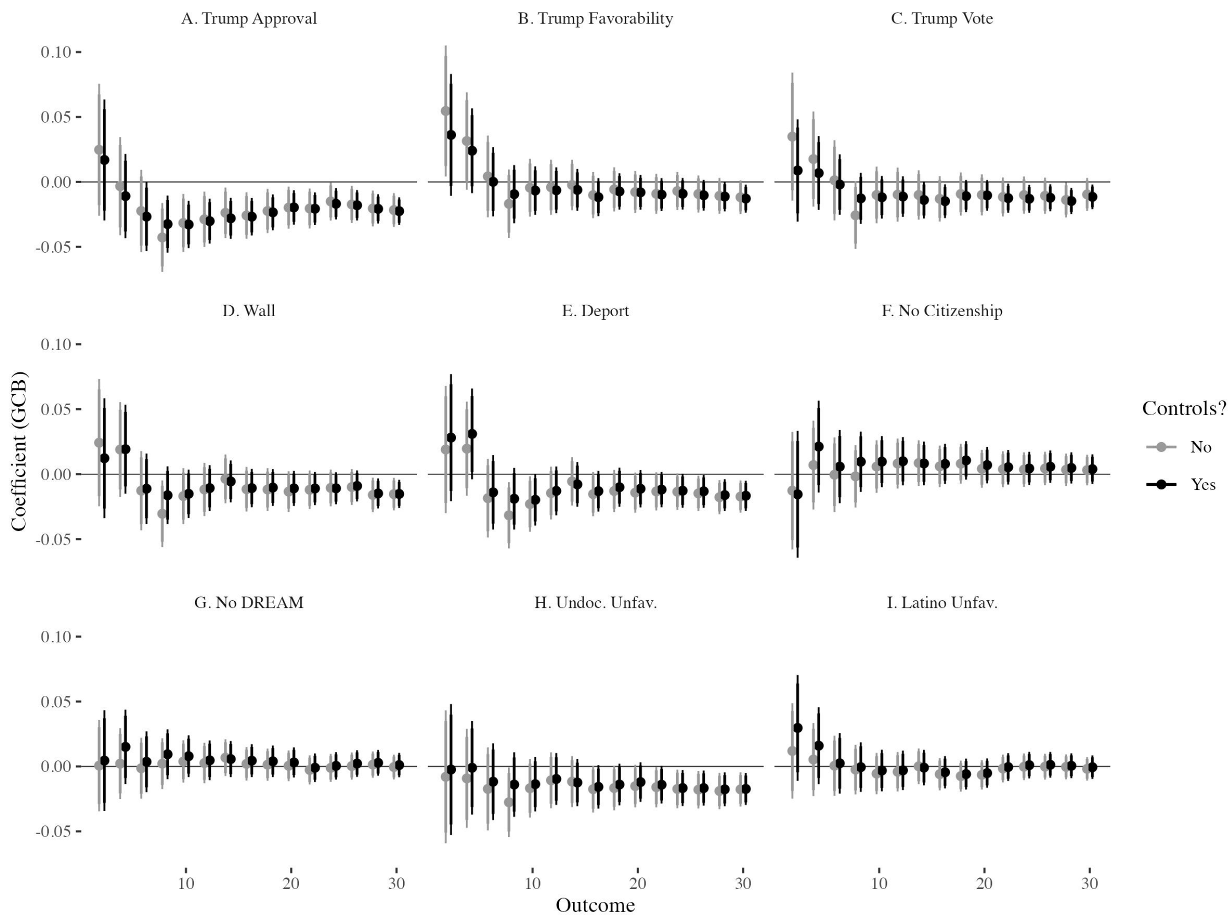

Figure 6: Positive incumbent evaluations and some anti-immigration attitudes decline after Trump's green card ban. Each panel characterizes a different outcome. The x-axis is the bandwidth (in days), the y-axis is the post-*GCB* coefficient. Color denotes the inclusion/exclusion of control covariates. 95% CIs displayed from HC2 robust SEs.

(Figure J29).

Moreover, if our post-*GCB* coefficient is either positive or negative with respect to anti-immigration attitudes, this may be due to unobserved trends motivating politically liberal/conservative policy preferences or positive/negative attitudes toward other marginalized social groups. We rule out the possibility our post-*GCB* coefficient is a function of unobserved attitudinal trends concerning liberal policy preferences aor marginalized groups by demonstrating being interviewed post-*GCB* does not shift attitudes on a number of immigration-irrelevant policies (carbon caps, gun control, taxes, abortion, health care) or social groups (Asians, Black people, Muslims, LGBT people) (Figure J30).

Results

Figure 6, Panels A-C characterize post-*GCB* coefficients across sample bandwidths between 2-30 days (in two day intervals). We find some evidence, at the outset, that Trump's Green Card Ban may have generated incumbent backlash. The post-*GCB* coefficient adjusting for controls in the 22-day bandwidth subsample is negative and statistically significant for the *Trump approval* ($\beta = -0.02$, $p < 0.01$) and *Trump vote* outcome ($\beta = -0.01$, $p < 0.05$), but not the *Trump favorability* outcome $\beta = -0.01$, $p = 0.13$).[15] However, these effects are *substantively small* and within the ± 0.1 SD equivalence test threshold outlined in Study 1 for negligible effects. The standardized post-*GCB* coefficient for the *Trump approval* and *vote* outcome is -0.04 (SE = 0.01) and -0.03 (SE = 0.01) respectively, with 95% confidence intervals within ± 0.1 SD.

We identify similar results concerning anti-immigration attitudes. The post-*GCB* coefficient adjusting for controls in the 22-day bandwidth subsample is statistically insignificant for the *no citizenship*, *no DREAM*, and *Latino unfavorability* outcomes (Figure 6, Panels F, G, I), but there appears to be a pro-immigration backlash in the form of a statistically significant reduction in support for the *wall* ($\beta = -0.01$, $p < 0.10$), deporting undocumented immigrants ($\beta = -0.01$, $p < 0.10$), and *undocumented unfavorability* ($\beta = -0.01$, $p < 0.10$, see Figure 6, Panels D, E, H). Yet, again, these are substantively small coefficients within the ± 0.1 SD equivalence test threshold from Study 1. The standardized versions of the aforementioned coefficients are -0.02 (SE = 0.01), -0.02 (SE = 0.01), and -0.03 (SE = 0.01), with 95% confidence intervals within ± 0.1 SD.

Consistent with our other inquiries, we largely interpret our findings concerning the consequences of the GCB as evidence that, to the extent we can identify incumbent or pro-immigration backlash with a well-powered survey fielded continuously at the daily-level, backlash is subdued and substantively limited.

[15] However, the post-*GCB* coefficient when the outcome is *Trump favorability* is approaching statistical significance and is negatively signed like when the outcome is *Trump approval* or *vote*.

Discussion and Conclusion

This study challenges the common perception that significant relaxations of existing immigration restrictions would inevitably face voter resistance and ultimately be counterproductive. By examining most of the available high-quality public opinion and policy data in the aftermath of DACA and DAPA, two high-profile pro-immigration reforms concerning unauthorized immigrants, alongside several anti-immigration policies, we provide a data-driven assessment of such concerns.

Our results indicate that these pro-immigration reforms have not been counterproductive in terms of increasing anti-immigration attitudes or voting intentions against the pro-immigration incumbent in the short term. Despite high opposition to immigration among voters in general and possible backlash to poor immigration management or border control (Solodoch, 2021b), targeted pro-immigration reforms that liberalize particular individuals and groups that voters support may further legitimize freer immigration in the electorate. Given that most immigration is already restricted, the evidence suggests that the selective relaxations of these restrictions or the selective legalization of immigrants without legal status generally do not cause voter backlash. These findings contribute to the literature on policy feedback and backlash (Patashnik, 2023), thermostatic immigration opinion (Van Hauwaert, 2023; Kustov, 2023), as well as generalize previous research on the legitimation of immigration through "demonstrably beneficial" policymaking aimed at advancing broad national interest (Kustov, 2021; Kustov, 2025).

However, it is important to acknowledge that immigration reforms are rarely independent of prior voter behavior. One could argue that the absence of backlash effects is due to DACA and DAPA being relatively conservative and mindful of their possible effects within the existing political equilibrium (Chou et al., 2021) or simply due to the fact that voters tend to be more sympathetic towards specific groups migrants who have already been here for a long time like DREAMers (Margalit and Solodoch, 2022). Even with these considerations in mind, our evidence challenges the common claim that pro-immigration policy advancements may have been counterproductive or conducive to the rise of xenophobia or populism, at least when it comes to the limited liberalization of some immigration. To the extent that some argue excessively lax immigration policies contributed to the election of Donald Trump in 2016 and 2024, our findings suggest that targeted programmatic reforms like DACA or DAPA were not the cause.

It is important to note that the absence of the immediate backlash against salient yet relatively moderate and programmatic pro-immigration reforms is a modest standard. This does not imply that these or most other pro-immigration reforms enjoy widespread support among citizens or lead to broader public acceptance of immigration in the longer-term, or a more open immigration system in general. For example, while detailed survey data may be limited, policies like New York City's "right to shelter"–or salient decisions to retain these policies amid a significant inflow of migrant arrivals without work permits–might have created tensions between the interests of existing residents and newcomers, potentially contributing to the relatively larger 2024 right-wing shift in the area (e.g., see Ketcham and Di Martino, 2023). However, if backlash fails to materialize in the short term, it is unlikely to emerge in the long term (Claassen and McLaren, 2022). Moreover, if backlash does not arise in response to policies improving the status of undocumented workers, it is even less likely to do so for policies streamlining legal immigration tracks–such as skilled, family-based, and other popular but still heavily restricted categories (Kustov, 2025).

Conversely, implementing anti-immigration policies may not be a winning political strategy when such policies are not programmatic or are not aligned with broader public interest or values, even among those who oppose immigration. In this regard, we show that the Muslim ban was deeply unpopular. While it may not have provoked an electoral backlash against the Trump administration, it did shift public opinion in a more pro-immigration direction. Although we lack fine-grained evidence, this was likely true for the extremely unpopular family separation policies of the first Trump administration, which may have further pushed Americans toward pro-immigration views (also see Patashnik, 2023). This is further supported by recent evidence of a "reverse backlash" effect, where anti-immigration policies prompt a pro-immigration response (Dennison and Kustov, 2023). In the case of DACA, in particular, we know that the policy has managed to generate sustained support over time, despite initial criticism and subsequent political challenges (Jardina and Ollerenshaw, 2022) (also see Figure I12).

Although we do not identify substantial shifts in mass opinion toward immigrants or incumbents after the policies we analyze *on average*, it does not mean there is no room for attitudes to shift among relevant subgroups explicitly targeted by pro- or anti-immigration policies. For example, Gutierrez and Roman (2024) find that although the DAPA announcement did not shift Obama approval on average, it did increase his approval among the Latino population, who disparately serve to benefit from the policy. Moreover, although favorability toward Trump did not shift post-Muslim Ban on average, it is possible the American Muslim population reduced their favorable opinions toward Trump after the policy (which, unfortunately, we cannot effectively estimate due to lack of measurement of Muslim identification or small sample sizes). Future research should explore other sources of heterogeneity where attitudes may be more likely to shift in response to specific immigration policies.

In conclusion, our study provides empirical evidence that high-profile pro-immigration reforms, such as DACA and DAPA, have not triggered substantively significant immediate public backlash against incumbents or immigrants. These findings have important implications for policymakers, suggesting that they may have more leeway in implementing programmatic policies on politically charged issues like unauthorized immigration without fearing inevitable voter resistance, at least assuming those policies can be credibly perceived to benefit the public at large or be in line with public opinion otherwise. At the same time, policymakers should recognize that not all anti-immigration measures are inherently popular or immune to backlash, even when general immigration attitudes are relatively negative. However, further research is needed to understand the conditions under which immigration reforms can actively generate public support and lead to a better immigration system that benefits current and future citizens alike.

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Supplementary Material

Contents

A Survey Methodology

A.1 Fox May-Jun. '12

The Fox News data we use are three nationally representative polls of registered voters stacked together. A Fox May 13-15, 2012 poll ($N = 912$), Fox June 3-5, 2012 poll ($N = 913$), Fox June 24-26, 2012 poll ($N = 907$). The survey mode for these polls is mixed landline telephone and cellular. The survey organiation is Anderson Robbins Research/Shaw and Company Research. Interviews were conducted by Braun Research Inc. of Princeton, New Jersey. Braun research uses a probability landline sample combined with a cell phone frame. The survey is executed with a 6 call design over three nights. All interviews are completed with CATI technology and live interviewers. They select the youngest male/female available at the time of the call. The surveys are weighted to Census demographics for age, race/ethnicity, and gender.

In our main analysis, we compare the effects of being interviewed in the June 24-26, 2012 poll (after the announcement of DACA) relative to the June 3-5, 2012 poll (before the announcement of DACA) on our outcomes of interest. The May 13-15, 2012 poll is used for temporal placebo tests to rule out if our null result is potentially driven by a pre-DACA attitudinal trend in our outcomes of interest.

A.2 GSS '12

The 2012 General Social Survey is an independently drawn area probability sample of English-speaking (and Spanish-speaking) people 18 years of age or over, living in non-institutional arrangements within the United States. Participation is voluntary.

A.3 Pew Jun. '12

The Pew Jun. 2012 survey was a telephone poll fielded between June 7-17, 2012. It is a nationally representative adult sample ($N = 2013$). It is known as the June 2012 Voter Attitude Suvey. Interviews were conducted via landline and cell phone. The survey was conducted by Princeton Survey Research Associates International. Interviews were administered in English and Spanish by Princeton Data Source. Statistical results are weighted to correct demographic discrepancies. The sampling margin of error for the complete set of weighted data is $\pm$ 2.6 percentage points. Weighting was conducting along sex by age, sex by education, age by education, race/ethnicity, census region, population density and household telephone usage. The AAPOR RR3 response rate was 22%.

A.4 IAT '12/IAT '14

The IAT '12 and IAT '14 surveys are from the Project Implicit Presidential Implicit Association Test (IAT) data.[16] From 2003-present the President IAT was available on the Project Implicit demonstration website (https://implicit.harvard.edu/implicit/selectatest.html Click

[16]Data are publically available here: `https://osf.io/f38ag/`

on "President IAT" to try it yourself). The President IAT includes one standard IAT (Donald Trump (Barack Obama before March 27, 2017) (George W. Bush before 2009) vs. One of the previous US Presidents; Good vs. Bad), sets of explicit measures (such as attitudes toward each President), set of demographic questions (race, ethnicity age, political identity etc.), and debriefing questions about how respondents thought about their IAT score after the task.

From 2003 to the end of 2017, there are 891,800 session IDs created for President IAT, and the overall completion rate is around 46.6%. There were 530,238 respondents who completed the standard IAT part of the task, which is 59.5% of the total respondents.

For the purposes of our analysis, we subset the 2012 and 2014 President IAT data to all respondents who completed the entire survey and are residents of the United States. We do this since completion suggests respondents are highly attentive/motivated (which increases our chances of *not* identifying a null result, see Ternovski and Orr (2022)) and our policy treatments occur in the United States.

The IAT data are not population representative nor weighted to be population representative. For example, relative to the nationally representative 2012 General Social Survey of adults, the IAT sample is the IAT sample is younger (median age: 25 vs 47), more college-educated (41% vs. 29%), and more liberal (48% vs 30%). One might think the IAT data constitutes a population that is arguably less likely to backlash against the pro-immigration policies, given the younger, college-educated, and more liberal are more likely to support pro-immigration policies.[17] However, another possibility motivated by prior evidence is that those who are predisposed to be pro-immigration would be the most likely to backlash to pro-immigration policies since: a) mass public members with anti-immigration predispositions have already made up their mind concerning pro-immigration incumbents and immigrants (i.e., *ceiling effects*) (Hetherington and Suhay, 2011); b) mass public members with pro-immigration predispositions not only have more space to backlash in response to pro-immigration policies, but their predispositions may weaken in response to actual policy commitments where they are much more likely to be negatively affected by the policies they tend to be in support of (Hutchings et al., 2024).

Prior research shows the effects of external stimuli in representative samples are statistically indistinguishable from effects of external stimuli in unrepresentative samples (Coppock et al., 2018; Krupnikov et al., 2021). Indeed, prior research shows external events motivate downstream attitudinal shifts in similar ways for representative *and* unrepresentative survey samples (Ofosu et al., 2019; Roman and Thompson, 2022).

A.5 TAPS Jun. '12/Aug '12/Nov '14

The American Panel Survey (TAPS) is a mixed-mode phone and online survey fielded each month with different questions between December 2011-January 2018.[18] The target population

[17] Indeed, bivariate regression tests suggest the college-educated, younger, and more liberal are statistically less likely to support reducing immigration levels in the 2012 GSS.

[18] `https://wc.wustl.edu/american-panel-survey`

is the U.S. population of English-speaking adults. TAPS Jun '12, Aug '12, and Nov '14 have N = 1693, 1707, and 1522 respondents respectively. The sample of addresses was drawn from the U.S. Postal Service's computerized delivery sequence file (CDSF), which covers 97% of the physical addresses in all 50 states including P.O. boxes and rural route addresses. Homes that are vacant or seasonal are identified as are other categories that help to refine the efficiency of the sample to be mailed. Using data from available U.S. Census files plus from a variety of commercial data bases, such as White Pages, Experian, Acxiom, etc., MSG can add names to these addresses, match with landline telephone numbers, and—with some level of accuracy—tag on information regarding race/ethnicity, age of householder, whether there are people of a certain age in the household, presence of children, home ownership status, etc.

Based on recent experience with the recruitment of an online panel with the ABS frame, TAPS strata were designed to specifically break out young adults (ages 18-24) and Hispanics, in addition to the balance of the population. Young adults and Hispanics may be strategically oversampled because these groups have a tendency to under-respond to surveys. Four mutually exclusive strata were used:

- 18-24 year-old Hispanic adults
- All other Hispanic adults ages 25+ or age unknown
- 18-24 year-old non-Hispanic adults
- All other adults that are non-Hispanic or ethnicity unknown and ages 25+ or age unknown

The estimated yield from each of the above strata was 5.6%, 6.4%, 14.4% and 9.4%, respectively.

Within-household selection procedures vary by the mode in which the household responds to the initial contact.

A successful recruitment was counted only when a Profile Survey is completed.

TAPS calculated weights to make survey results generalizable to the U.S. population of English-speaking adults. Investigators received these weights as variables in delivered data files.

Adjustments were made to compensate for (a) selection probabilities altered by the stratified sample design and (b) within household selection probabilities associated with the random choice of a panel member from among all eligible adults residing in the household. These adjustments constituted the base weight that corrected the sample to approximate a simple random sample of the population of adults.

The following weight, $w1_{i|k}$, for mailing addresses i within stratum k is calculated as follows:

$$w1_{i|k} = (P_{i|k} \ / \ P_{tot}) \ (S_{tot} \ / \ S_{i|k})$$

Where

$P_{i|k}$ is the population or frame count within stratum k,

P_{tot} is the total population count from the frame,

$S_{i|k}$ is the sample count within stratum k, and

S_{tot} is the total recruited sample size.

The TAPS administrators also adjusted for the selection probability of the randomly selected adult within households. To adjust for this, they weighted each selected respondent, r, by the inverse of the number of eligible adults, A, ages 18 and older, enumerated as residing in household, h, and called this $w2_{r|h}$ and calculated as follows:

$$w2_{r|h} = A_h \ / \ 1$$

The base weight was the product of $w1_{i|k}$ and $w2_{r|h}$.

A.6 Gallup Tracking Poll (2009-2016)

Between Jan. 2009-Oct. 2016, Gallup sampled roughly 1000 U.S. adults per day about their approval of Barack Obama in addition to a number of political and socio-demographic covariates in the Gallup Daily Tracking Survey.

On any given evening, 200 Gallup interviewers conducted computer-assisted phone interviews with randomly selected respondents, aged 18 and older, including cellphone users and Spanish-speaking respondents from all 50 U.S. states and the District of Columbia. The survey includes standard demographics such as race, income, education, employment status, and occupation.

Gallup weights the data daily to compensate for disproportionalities in selection probabilities and non-response. Gallup weights the data to match targets from the U.S. Census Bureau by age, sex, region, gender, education, ethnicity, and race, as well as the population density of the self-reported location.

Landline and cellphone sampling frame description: Survey Sampling Inc. provides random-digit-dial (RDD) list-assisted landline sample and random-digit-dialing (RDD) wireless phone sample (consisting of all exchanges set aside for wireless phones) in non-overlapping frames. The random-digit-dial (RDD) list-assisted landline and wireless phone samples are stratified proportionately by U.S. Census region and by time zone within region.

Sample selection methodology: Gallup uses RDD list-assisted landline sample and RDD cellphone sample. When calling a landline telephone, Gallup uses random selection to choose respondents within a household based on the next birthday. Gallup treats cellphones as personal devices: The individual who answers the cellphone is the respondent.

Sample sizes: Gallup conducts 1,000 surveys with American adults, aged 18 and older, daily, 350 days annually. Five hundred respondents are asked the Well-being track survey, while the other 500 complete the Politics and Economy track survey. Certain variables, such as employment indicators and key demographics, are asked on both survey tracks.

Data weighting: Gallup calculates weights for the Daily tracking data that account for unequal selection probability, nonresponse, and post-stratification.

1. Gallup calculates selection probability and nonresponse weights to compensate for disproportionalities in probabilities of selection and response rate by sample frame. Gallup calculates these separately by time zone by region within the RDD landline phone sample and then within the wireless phone sample.

2. Gallup calculates selection probability weights to compensate for disproportionalities in probabilities of selection for respondents reached via a landline phone. Because Gallup only interviews one adult per landline household, these weights are based on the number of adults in the household.

3. Gallup calculates selection probability weights using the lambda compositing method for dual phone users. This compensates for disproportionalities in probabilities in selection for respondents who have both a landline and a wireless phone, and thus could be in both the landline and wireless phone sample frames, versus respondents with the possibility of being in only one sample frame. Gallup calculates the dual-user weights to account for the proportion of dual users from the landline versus the wireless phone samples. Each respondent's dual user status is based on whether their household has a landline phone and whether they personally have a cellphone.

4. Gallup uses an iterative proportional fitting (i.e., raking) algorithm to ensure the Daily tracking data match national targets of telephone status, Census region by age, gender by age, education, race by Hispanic ethnicity, and population density quintile of self-reported county.

   a. Gallup calculates post-stratification weights for telephone status using the latest available estimates from the National Health Interview Survey (NHIS) conducted by the National Center for Health Statistics to determine the individual-level target proportions by household telephone status. While this is an individual level weight and individual level weighting targets are used, each respondent's telephone status for this weight is based on their household's telephone status. This is done in order to match the method used to define individuals' telephone statuses by the NHIS.

   b. Gallup calculates demographic post-stratification weights based on targets from the Current Population Survey the U.S. Census Bureau conducts for the Bureau of Labor Statistics.

   c. Gallup calculates population density weights based on targets from the Decennial census.

5. Gallup trims the final weights to reduce variance.

6. Gallup calculates weights for each track separately and for the combined data.

A.7 CES '14

The 2014 CES survey was conducted online by YouGov. We use the post-election data in our analysis, which was fielded after the 2014 midterm election and around the time of DAPA's announcement (fielded between Nov 5, 2014 to Dec 6, 2014).

YouGov constructed a sampling frame of U.S. Citizens from the 2010 American Community Survey, including data on age, race, gender, education, marital status, number of children under 18, family income, employment status, citizenship, state, and metropolitan area. The frame was constructed by stratified sampling from the full 2010 ACS sample with selection within strata by weighted sampling with replacement (using the person weights on the public use file). Data on reported 2010 voter registration and turnout from the November 2010 Current Population Survey and on reported 2008 voter registration and turnout from the November 2008 Current Population Survey was matched to this frame using a weighted Euclidean distance metric. Data on religion, church attendance, born again or evangelical status, news interest, party identification and ideology was matched from the 2007 Pew U.S. Religious Landscape Survey. The target sample was selected by stratification by age, race, gender, education, and voter registration, and by simple random sampling within strata. Stratification and Matching The sample drawn for the CCES were chosen from the YouGov Panel, along with the MyPoints, Research Now, and SSI panels using a five-way cross- classification (age x gender x race x education x state). All respondents who completed the pre-election survey were re-invited to the post-election survey. The final set of completed pre-election interviews (numbering approximately 87,389, after quality controls were applied) were then matched to the target frame, using a weighted Euclidean distances metric.

For each team and the common content, the matched cases were then weighted to the sampling frame using entropy balancing. The matched cases and the frame were combined and the combined cases were balanced on multiple moment conditions. The moment conditions included age, gender, education, race, voter registration, ideology, baseline party ID, born again status, political interest, plus their interactions. The resultant weights were then post-stratified by age, gender, education, race, and voter registration status, as needed. Additionally, for the common content, the weights were post-stratified across states and statewide political races. Weights larger than 15 in the common content were trimmed and the final weights normalized to equal sample size. The team data weights were trimmed at 7.

B Outcome Details

B.1 Incumbent Evaluations (IE)

B.1.1 Pew 2012

Obama Approval: Do you approve or disapprove of the way Barack Obama is handling his job as President? Responses are 1) Approve, 2) Disapprove, 3) Don't know. Coded 1 if respondent indicates "approve," 0 otherwise.

Obama Vote Choice: Now, suppose the 2012 presidential election were being held TODAY. If you had to choose between [READ AND RANDOMIZE ROMNEY AND OBAMA] who would you vote for? Responses are 1) Barack Obama, the Democrat, 2) Mitt Romney, the Republican. Coded 1 if respondent indicates "Obama," 0 otherwise.

Obama Favorability: Is your overall opinion of Barack Obama very favorable, mostly favorable, mostly UNfavorable, or very unfavorable? Responses are 1) Very favorable, 2) Mostly favorable, 3) mostly unfavorable, 4) very unfavorable. Coded between 0-3 so 3 = very favorable and 0 = very unfavorable.

B.1.2 IAT 2012/2014

Prefer Obama: Which statement best describes you? 1) I strongly prefer Barack Obama to President X. 2) I moderately prefer Barack Obama to president X. 3) I slightly prefer Barack Obama to president X. 4) I like Barack Obama and President X equally. 5) I slightly prefer President X to Barack Obama, 6) I moderately prefer President X to Barack Obama, 7) I strongly prefer President X to Barack Obama. Coded so 6 = I strongly prefer Barack Obama to President X and 0 = I strongly prefer President X to Barack Obama. Recoded between 0-1. Note: President X is randomized in the IAT survey across George W. Bush, Bill Clinton, Thomas Jefferson, John F Kennedy, Abraham Lincoln, Richard Nixon, Ronald Reagan, and Franklin D. Roosevelt.

Obama Favorability: How warm or cold do you feel toward Barack Obama? 0 = cold, 5 = neutral, 10 = warm. Recoded between 0-1 where 1 = warm and 0 = cold.

Obama D-Score: The President IAT calculates normalized averages of how quickly respondents associate negative/positive attributes to President X/Obama relative to negative/positive attributes to Obama/President X in the form of a *D-score*. The *D-score* ranges from -2-2. Higher values suggest implicit bias in favor of Barack Obama over President X (i.e., associating negative attributes to President X more than Barack Obama) (Greenwald and Lai, 2020). The D-score is recoded between 0-1. Note: President X is randomized in the IAT survey across George W. Bush, Bill Clinton, Thomas Jefferson, John F Kennedy, Abraham Lincoln, Richard Nixon, Ronald Reagan, and Franklin D. Roosevelt.

B.1.3 Fox 2012

Obama Approval: Do you approve or disapprove of the job Barack Obama is doing as president? 1) Approve, 2) Disapprove. Coded 1 if respondent indicates "approve," 0 otherwise.

Obama Vote Choice: If the presidential election were held today, how would you vote if the candidates were 1) Democrat Barack Obama, 2) Republican Mitt Romney. Coded 1 if respondent indicates "Obama", 0 otherwise.

B.1.4 TAPS 2012/2014

Obama Approval (TAPS '12, DACA-A; TAPS '12, DACA-I; TAPS '14, DAPA-A): Do you approve or disapprove of the way the following are doing their jobs? President Obama? Respondents can indicate 1) Strongly approve, 2) somewhat approve, 3) somewhat disapprove, 4) strongly disapprove. Coded 1 if respondent indicates "strongly approve" or "somewhat approve," 0 otherwise.

Obama Favorability (TAPS '12, DACA-A): Rate each group or individual using the scale shown below: President of the United States. Respondents could choose from a 0-10 scale where 0 = cold, 5 = neutral, and 10 = warm. 10 = highest value, 0 = lowest. Recoded between 0-1.
Obama Vote Choice (TAPS '12, DACA-A; TAPS '12, DACA-I): Rate each group or individual using the scale shown below: President of the United States. Respondents could choose from a 0-10 scale where 0 = cold, 5 = neutral, and 10 = warm. 10 = highest value, 0 = lowest. Recoded between 0-1.

Obama Confidence (TAPS '12, DACA-A): What about President Obama? How much confidence do you have in him? 1) A great deal of confidence, 2) only some confidence, 3) hardly any confidence. Recoded between 0-2 where 2 = great deal of confidence and 0 = hardly any confidence.

Democrat - Republican Approval (TAPS '12, DACA-A; TAPS '12, DACA-I; TAPS '14, DAPA-A): Do you approve or disapprove of the way the following are doing their jobs? Democrats in Washington OR Republicans in Washington. Respondents can indicate 1) Strongly approve, 2) somewhat approve, 3) somewhat disapprove, 4) strongly disapprove. Democratic approval is coded 1 if respondent indicates "strongly approve" or "somewhat approve," 0 otherwise. Republican approval is coded 1 if respondent indicates "strongly approve" or "somewhat approve," 0 otherwise. We take the difference between Democratic and Republican approval to generate our measure of differential approval for Democrats in Washington vs. Republicans in Washington.

B.1.5 Gallup

Obama Approval: Do you approve or disapprove of the way Barack Obama is handling his job as president? 1) Approve, 2) Disapprove. Coded 1 if respondent indicates "approve," 0 otherwise.

B.2 Anti-Immigration Attitudes (AIA)

B.2.1 GSS '12

Reduce Immigration: Do you think the number of immigrants to America nowadays should be... 1) increased a lot, 2) increased a little, 3) remain the same as it is, 4) reduced a little, 5) reduced a lot. Coded 1 if respondent indicates "reduced a lot" or "reduced a little," 0 otherwise.

B.2.2 Pew '12

Support SB 1070: As you may know, two years ago the state of Arizona passed a law that requires police to verify the legal status of someone they have already stopped or arrested if they suspect that the person is in the country illegally. Do you approve or disapprove of Arizona's immigration law? 1) Approve, 2) Disapprove. Coded 1 if respondent indicates "approve," 0 otherwise.

Increase Enforcement: What should be the priority for dealing with illegal immigration in the U.S.? [RANDOMIZE; (one) better border security and stronger enforcement of our immigration laws; OR (two) creating a way for illegal immigrants already here to become citizens if they meet certain requirements] OR should BOTH be given equal priority? 1) Better border security and stronger enforcement of our immigration laws, 2) Creating a way for illegal immigrants already here to become citizens if they meet certain requirements, 3) Should BOTH be given equal priority. Coded 1 if respondent indicates "better border security and stronger enforcement of our immigration laws," 0 otherwise.

Pew '12 Index: An Additive index of *increase enforcement* and *support SB 1070*, rescaled between 0-1.

B.2.3 TAPS '12

Latino Feeling Thermometer: Rate each group of individuals using the scale shown below: Hispanics. Respondents could report between 0-10 where 0 = cold, 5 = neutral, and 10 = warm. We recode this variable between 0-1, where 1 = 10 (warm) and 0 = 0 (cold).

B.3 Tea Party Support (TPS)

B.3.1 Fox '12

Tea Party Support: Regardless of whether you've attended a Tea Party rally or event, do you consider yourself to be a part of the Tea Party movement, or not? 1) Yes, 2) No. Coded 1 if respondent indicates "yes," 0 otherwise.

B.3.2 Pew '12

Tea Party Support: From what you know, do you agree or disagree with the Tea Party movement, or don't you have an opinion either way? 1) Agree, 2) Disagree, 3) No opinion either way. Coded 1 if respondent indicates "agree," 0 otherwise.

B.3.3 TAPS '14

Tea Party Support: Do you consider yourself a supporter or opponent of the Tea Party movement, or neither? 1) Supporter, 2) Opponent, 3) Neither. Coded 1 if the respondent indicates "supporter," 0 otherwise.

B.3.4 CES '14

Tea Party Support: What is your view of the Tea Party movement? 1) Very positive, 2) Somewhat positive, 3) Neutral, 4) Somewhat negative, 5) Very Negative, 6) Dont know. Coded so 4 = very positive, 0 = very negative. Don't know responses are coded as neutral (2). Rescaled between 0-1.

C Control Covariates Used Across Surveys

Table C1: Control and Balance Covariates At Use For Each Survey

Survey	Control/Balance Covariates At Use
Pew '12	Age, Woman, Married, White, Evangelical, College, Income, Unemployed, Ideology, Democrat, Independent, California, Florida, New York, Illinois, Texas, Pennsylvania
IAT '12/ IAT '14	Age, White, College, Woman, Catholic, Liberal, California, Pennsylvania, New York, Florida, Texas, Illinois
TAPS '12/ TAPS '14	Age, Woman, White, College, Income, Ideology, Democrat, Republican, California, Pennsylvania, New York, Florida, Texas, Illinois
Gallup	Age, Woman, White, Catholic, Married, Income, College, Ideology, California, New York, Texas, Florida, Illinois
Fox	Age, White, Woman, Evangelical, Income, College, Democrat, Republican, Northeast, Midwest, West
CES '14	Age, White, Woman, Married, Unemployed, College, Income, Partisanship, Ideology, Texas, Pennsylvania, California, New York, Illinois

D Balance Tests

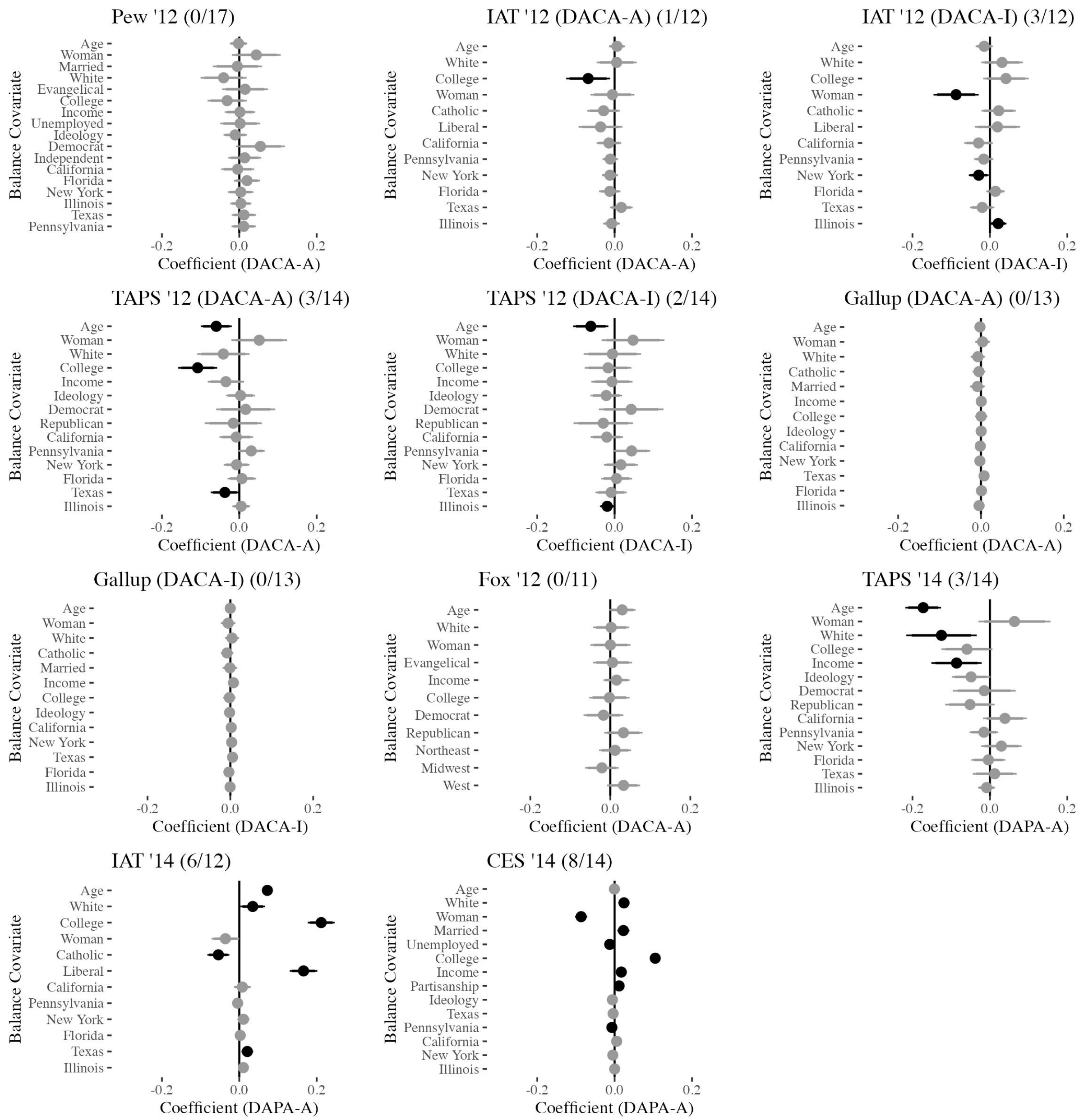

Figure D1: Covariate Balance Before and After DACA and DAPA Across Surveys. Each panel is from a different dataset (denoted by title), parentheses in title denotes how many covariates are imbalanced pre- and post-policy (black coefficients = statistically significant, grey otherwise). X-axis is the post-policy coefficient, y-axis is the balance covariate. All balance covariates rescaled between 0-1. 95% CIs displayed from HC2 robust SEs

E Ancillary Media Data

E.1 Mediacloud

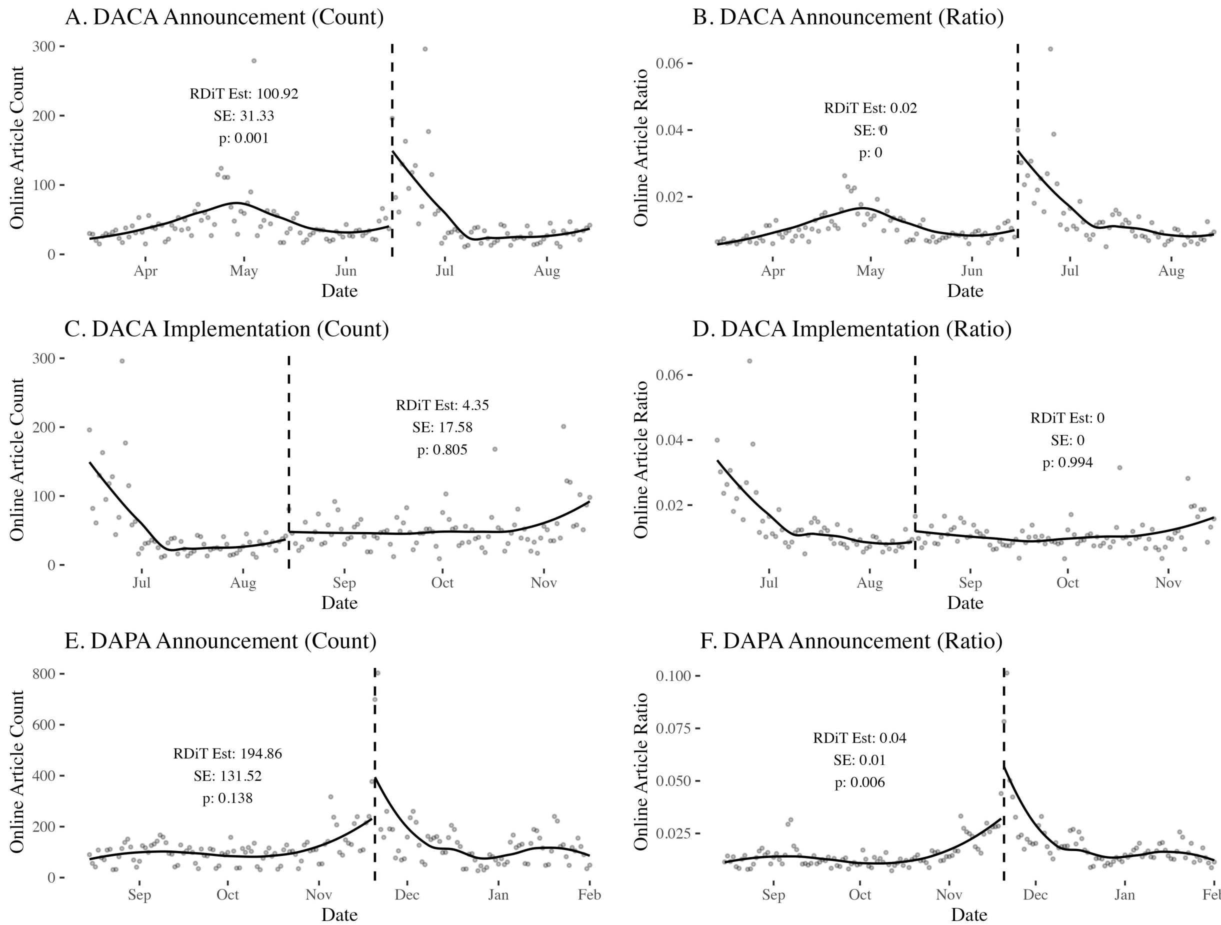

Figure E2: Immigration-related online news articles (y-axis) over time (x-axis). Panels A-B, C-D, and E-F characterize data near the moment near the DACA announcement, DACA implementation, and DAPA announcement. Annotations denote mean-squared optimal bandwidth regression-discontinuity-in-time estimates of the effect of the respective policies on the count and ratio of online immigration-related outcomes (Calonico et al., 2014).

E.2 Google Trends

E.2.1 Obama

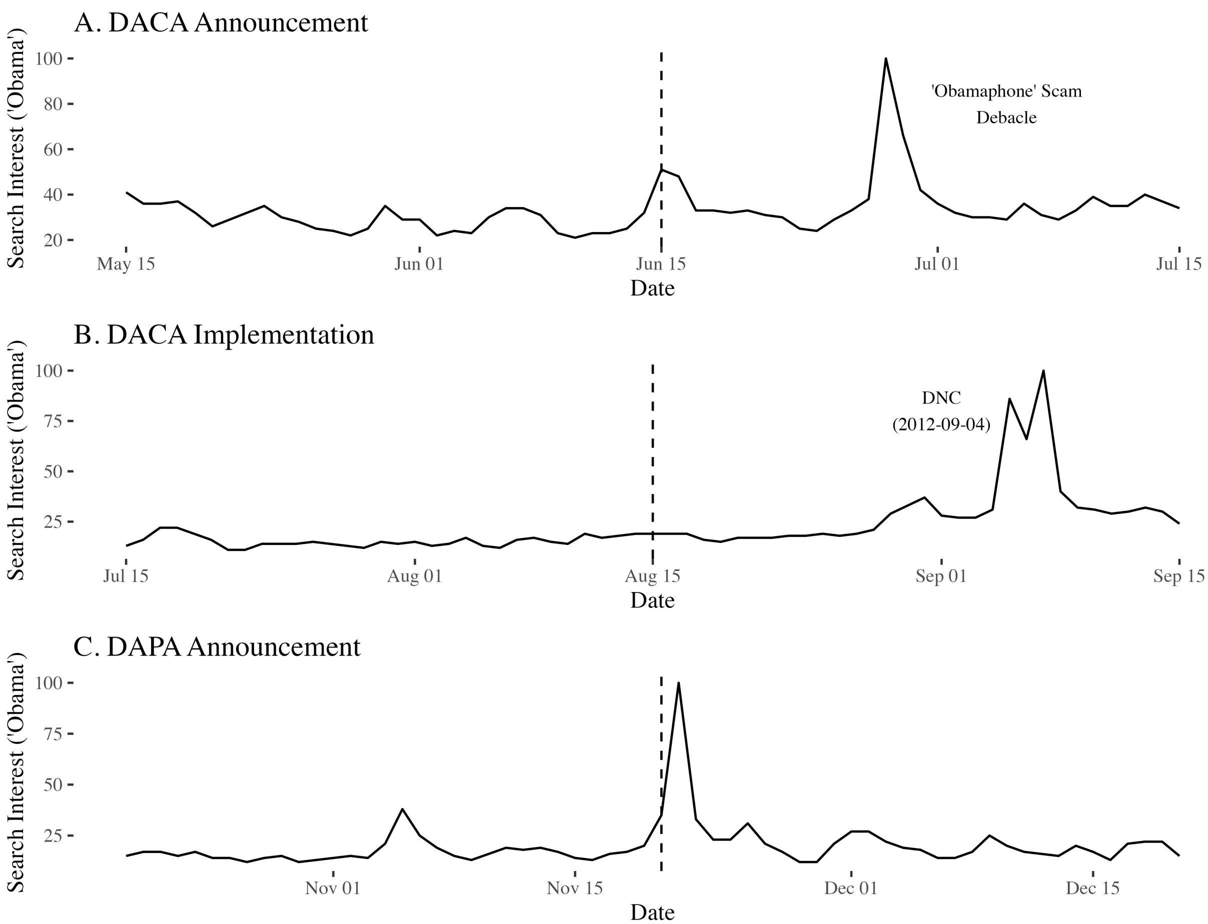

Figure E3: There were limited moments of punctuated attention to Obama around the time of the DACA announcement, DACA implementation, and DAPA announcement. X-axis is date, y-axis is Google Trends search interest in "Obama." Dashed vertical line denotes the onset of the DACA announcement (Panel A), DACA implementation (Panel B), and DAPA announcement (Panel C). The increase in Obama salience around the end of July 2012 on Panel A pertains to the "Obamaphone" scam, where people started searching about Barack Obama because they were receiving scam text messages that Barack Obama would provide them a free phone in return for personal information. The increase in Obama salience around September 2012 on Panel B pertains to the DNC, which is well after DACA's implementation.

E.2.2 DACA Announcement

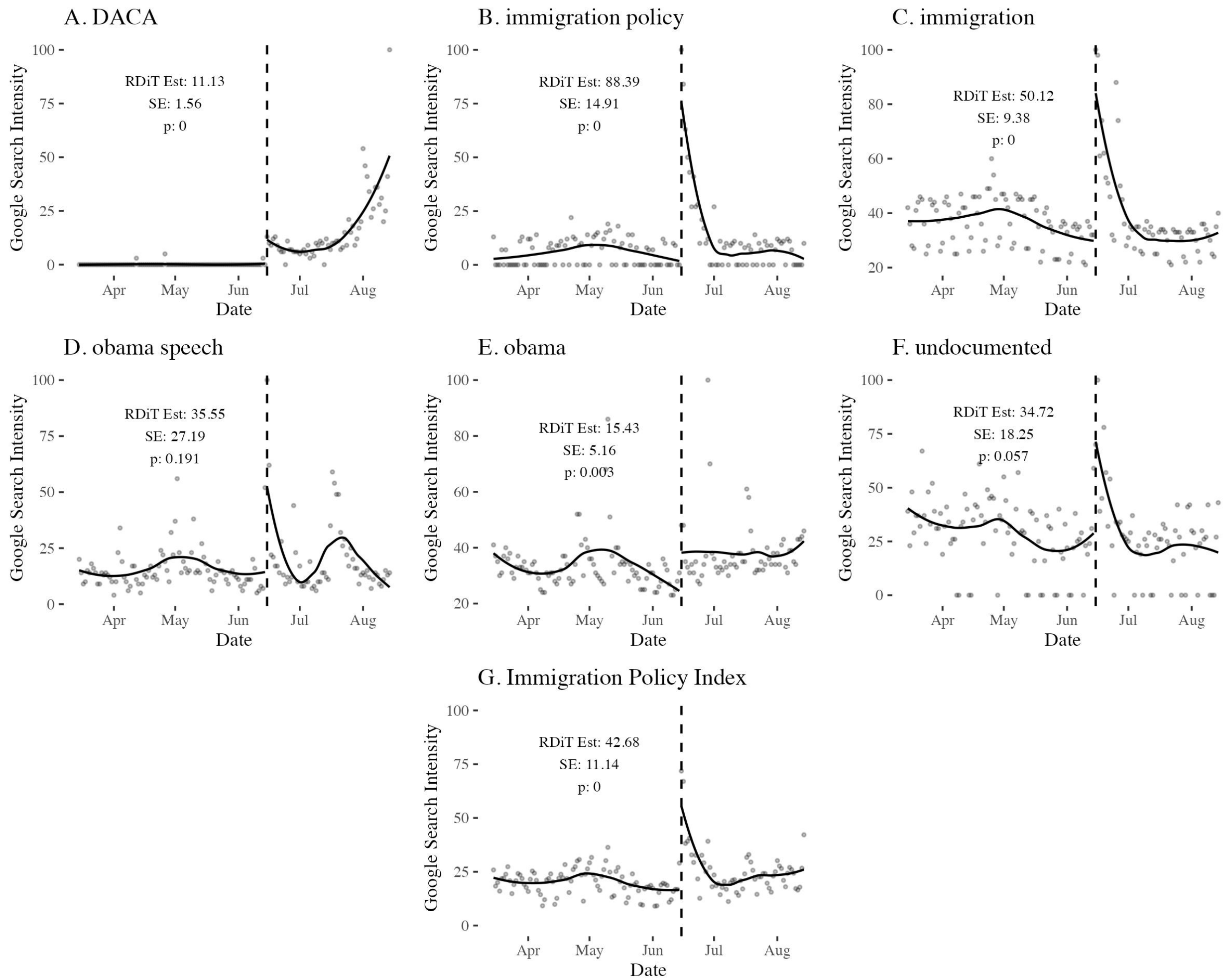

Figure E4: Search interest in immigration-related content (y-axis) over time (x-axis) around moment of DACA announcement. Each plot facet denotes Google search interest over different search times (specified on facet title). Immigration Policy Index is an average index of the other search terms. Annotations denote mean-squared optimal bandwidth regression-discontinuity-in-time estimates of the effect of the respective policies on the Google search intensity of the respective search terms (Calonico et al., 2014).

E.2.3 DACA Implementation

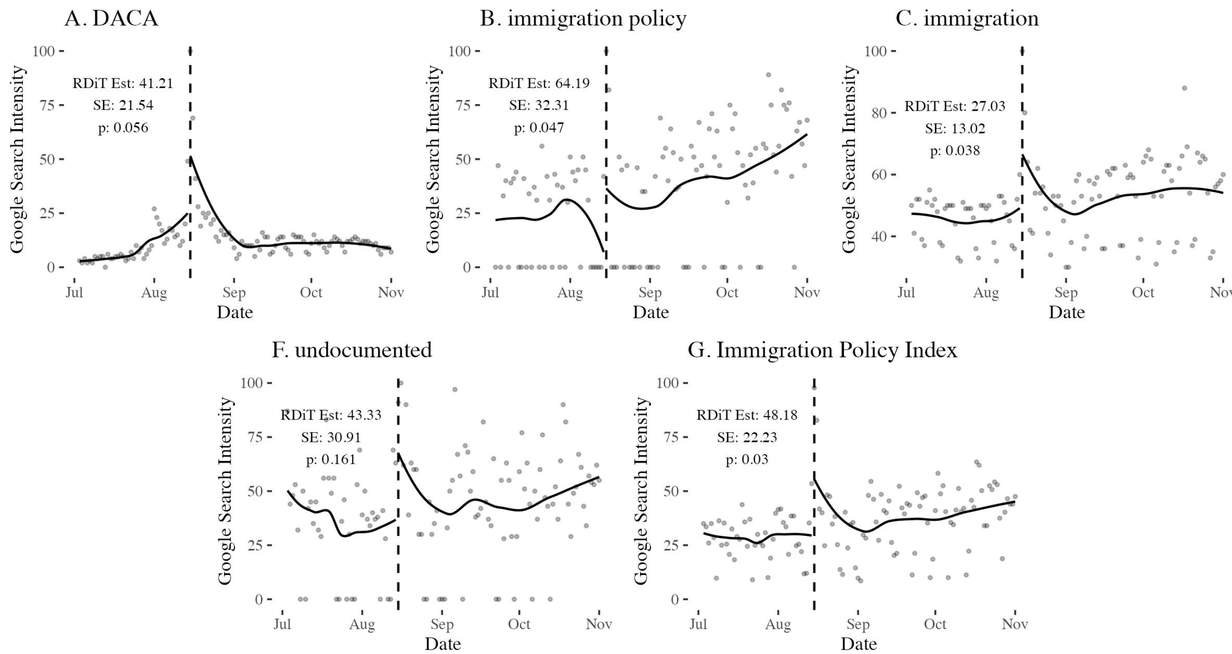

Figure E5: Search interest in immigration-related content (y-axis) over time (x-axis) around moment of DACA implementation. Each plot facet denotes Google search interest over different search times (specified on facet title). Immigration Policy Index is an average index of the other search terms. Annotations denote mean-squared optimal bandwidth regression-discontinuity-in-time estimates of the effect of the respective policies on the Google search intensity of the respective search terms (Calonico et al., 2014).

E.2.4 DAPA Announcement

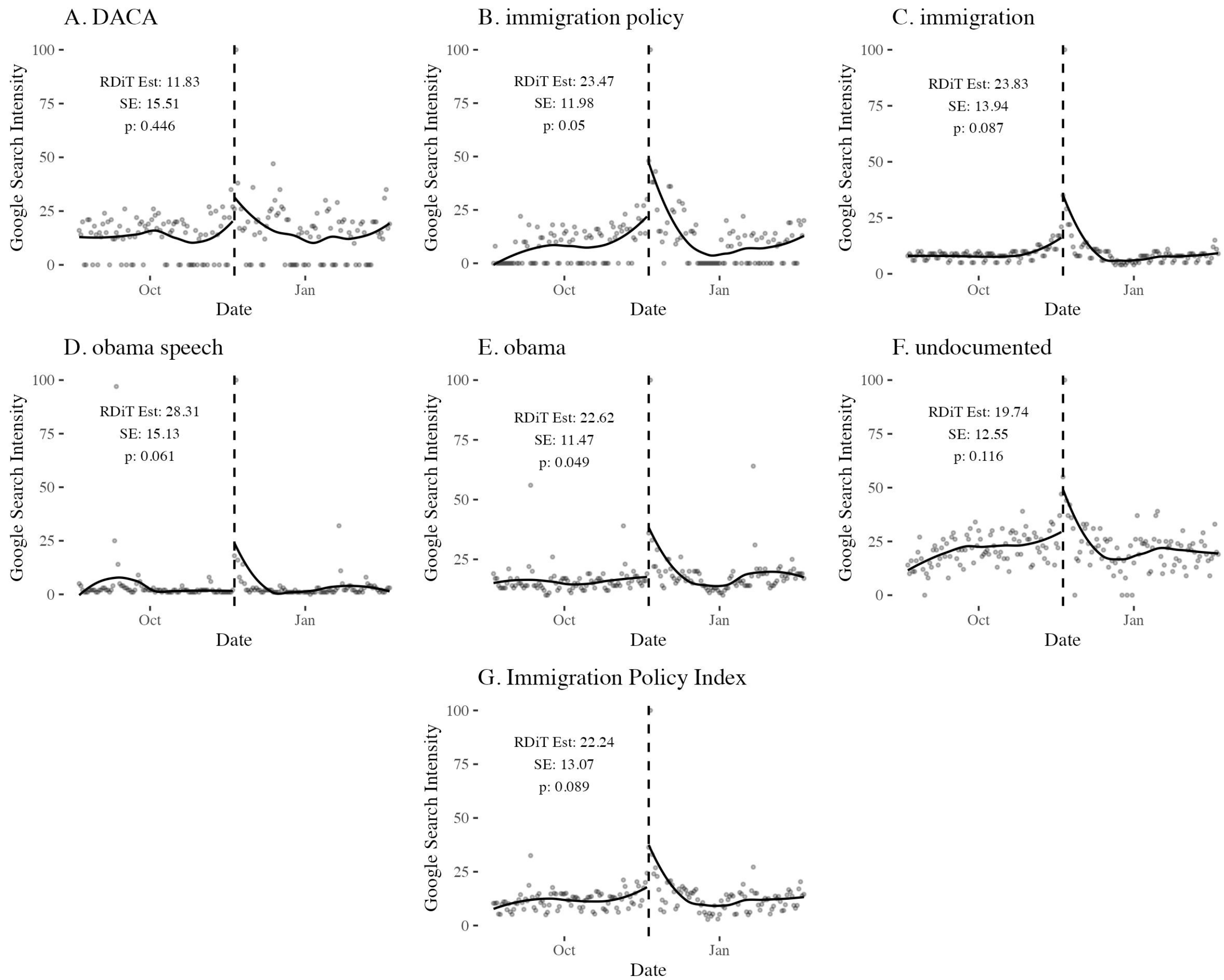

Figure E6: Search interest in immigration-related content (y-axis) over time (x-axis) around moment of DAPA announcement. Each plot facet denotes Google search interest over different search times (specified on facet title). Immigration Policy Index is an average index of the other search terms. Annotations denote mean-squared optimal bandwidth regression-discontinuity-in-time estimates of the effect of the respective policies on the Google search intensity of the respective search terms (Calonico et al., 2014).

F Temporal Placebo Tests

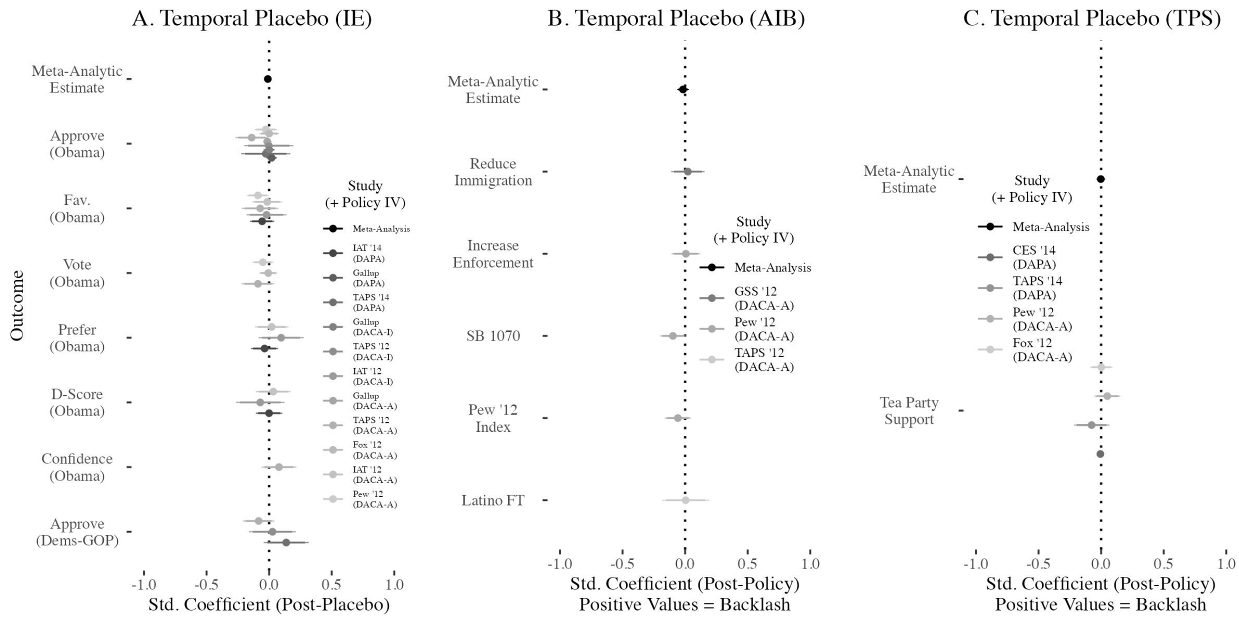

Figure F7: Assessing if there is a secular trend in the outcomes of interest pre-policy. X-axis is the standardized *post-placebo* coefficient (interviewed after the median pre-*policy* date relative to before the median pre-*policy* date, subsetting the data such that post-*policy* = 0), y-axis is the outcome. Panels A-C characterize outcomes related to incumbent backlash, anti-immigration attitudes, and Tea Party support. Color denotes study and policy (i.e., DACA announcement, DACA implementation, DAPA announcement) analyzed. 95% CIs displayed from HC2 robust SEs. Random-effects Hartung-Knapp meta-analytic estimates are displayed and are study-adjusted to prevent artificial SE deflation. Estimates using surveys targeting representative populations are population-weighted.

G Heterogeneity By Partisanship and Ideology

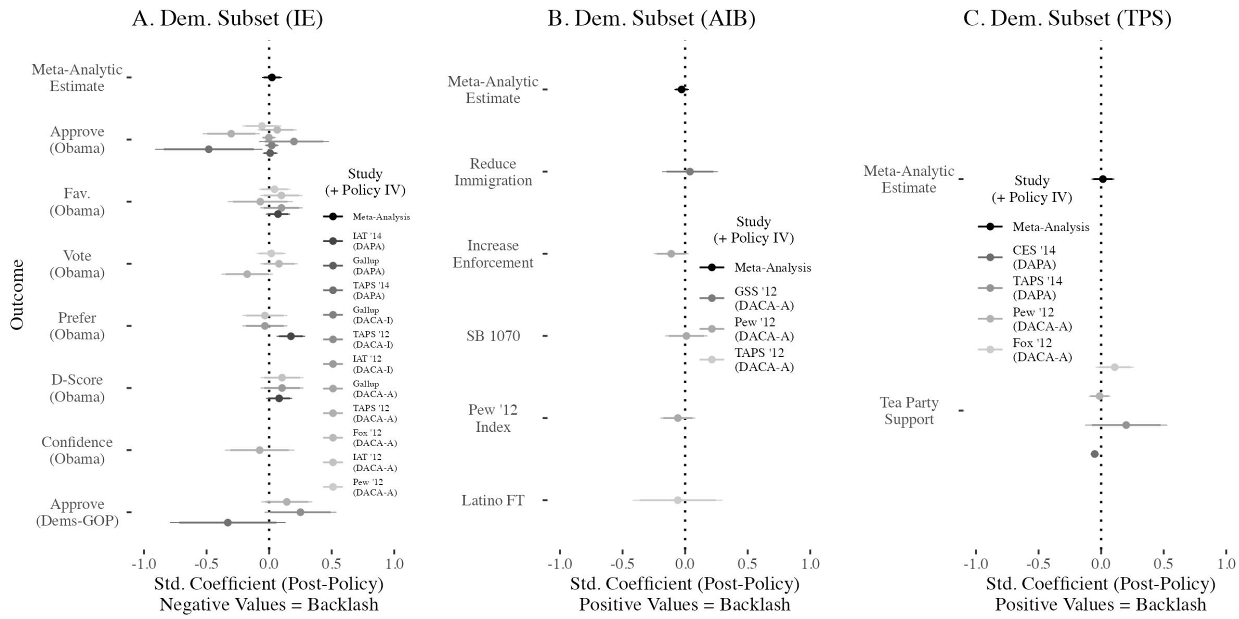

Figure G8: DACA and DAPA did not cause backlash among democrats and liberals. X-axis is the standardized *post-placebo* coefficient (interviewed after the median pre-*policy* date relative to before the median pre-*policy* date, subsetting the data such that post-*policy* = 0), y-axis is the outcome. Panels A-C characterize outcomes related to incumbent backlash, anti-immigration attitudes, and Tea Party support. Color denotes study and policy (i.e., DACA announcement, DACA implementation, DAPA announcement) analyzed. 95% CIs displayed from HC2 robust SEs. Random-effects Hartung-Knapp meta-analytic estimates are displayed and are study-adjusted to prevent artificial SE deflation. Estimates using surveys targeting representative populations are population-weighted.

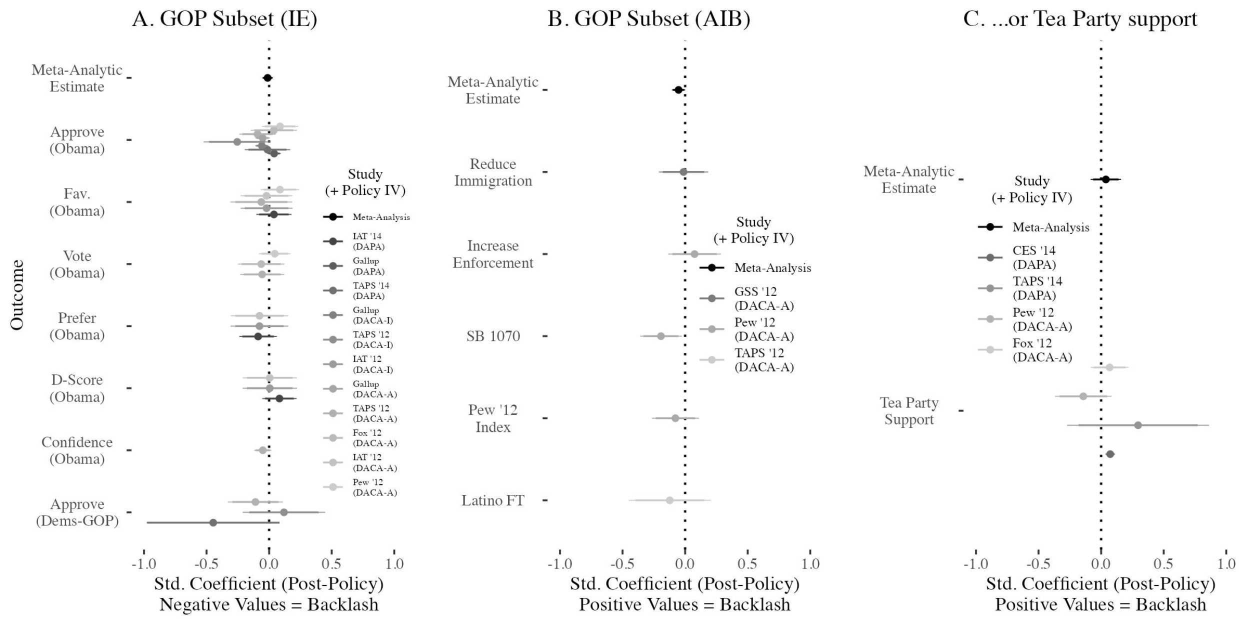

Figure G9: DACA and DAPA Did Not Motivate Backlash Among Republicans and Political Conservatives. X-axis is the standardized *post-placebo* coefficient (interviewed after the median pre-*policy* date relative to before the median pre-*policy* date, subsetting the data such that post-*policy* $= 0$), y-axis is the outcome. Panels A-C characterize outcomes related to incumbent backlash, anti-immigration attitudes, and Tea Party support. Color denotes study and policy (i.e., DACA announcement, DACA implementation, DAPA announcement) analyzed. 95% CIs displayed from HC2 robust SEs. Random-effects Hartung-Knapp meta-analytic estimates are displayed and are study-adjusted to prevent artificial SE deflation. Estimates using surveys targeting representative populations are population-weighted.

H Treatment Reception

H.1 Immigration = MIP Post-DACA

Table H2: Respondents were more likely to report immigration as the most important problem after DACA's announcement

	Immigration = MIP
	(1)
Post-DACA	0.06^{*}
	(0.02)
Age	-0.03
	(0.05)
Woman	-0.03
	(0.02)
Married	0.06^{**}
	(0.02)
White	-0.05
	(0.03)
Evangelical	-0.03
	(0.02)
College	-0.06^{**}
	(0.02)
Income	-0.03
	(0.03)
Unemployed	0.05
	(0.03)
Ideology	-0.08
	(0.05)
Democrat	-0.02
	(0.03)
Independent	0.02
	(0.04)
California	0.06
	(0.04)
Pennsylvania	-0.07^{**}
	(0.02)
New York	0.02
	(0.05)
Florida	0.01
	(0.04)
Texas	0.03
	(0.04)
Illinois	-0.08
	(0.05)
R^2	0.04
Num. obs.	2013

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

H.2 DACA = Followed Post-Announcement

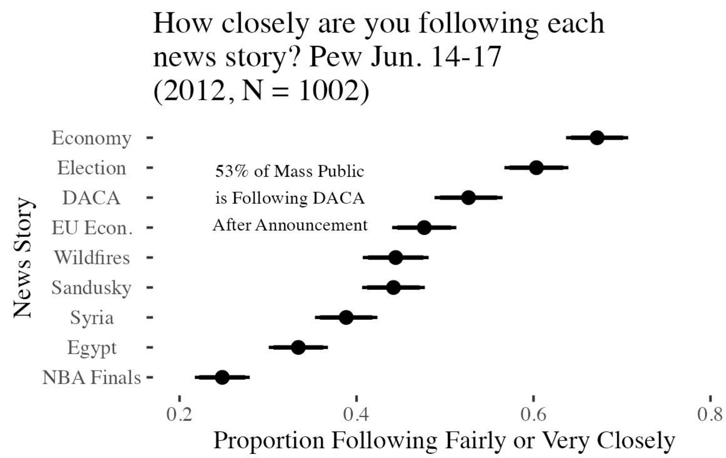

Figure H10: DACA was noticed by the majority of Americans. This plot characterizes the proportion of the mass public that indicates they are following DAPA fairly or very closely (relative to "not too" or "not at all" closely) immediately after DACA's announcement. Data are from a nationally representative Pew research poll. Estimates and 95% CIs are from 1000 bootstrap replicates.

H.3 DAPA = Followed Post-Announcement

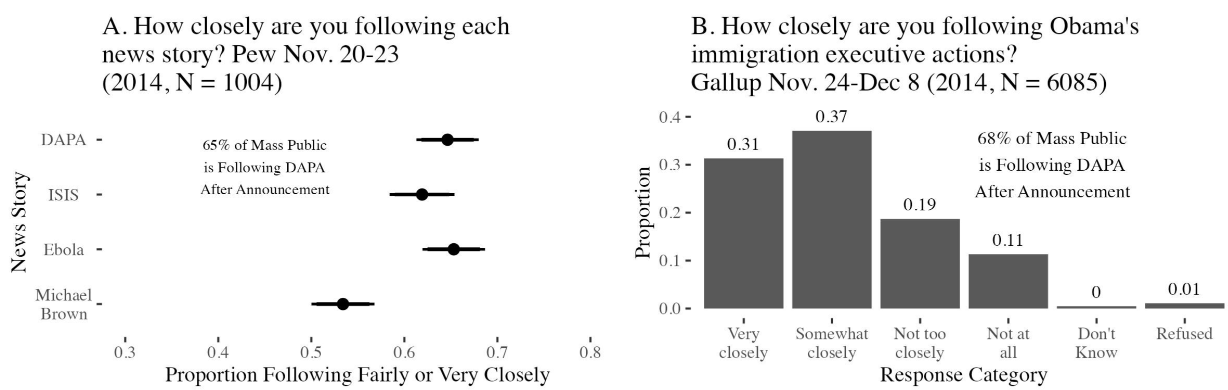

Figure H11: DAPA was noticed by the vast majority of Americans. Panel A characterizes the proportion of the mass public that indicates they are following DAPA fairly or very closely (relative to "not too" or "not at all" closely). Data are from a nationally representative Pew research poll. Estimates and 95% CIs are from 1000 bootstrap replicates. Panel B characterizes the proportion of people who are closely following Obama's executive actions "very" or "somewhat" closely relative to "not too" or "not at all" closely. Data are from a nationally representative Gallup poll.

I Attitudes Toward DACA Over Time

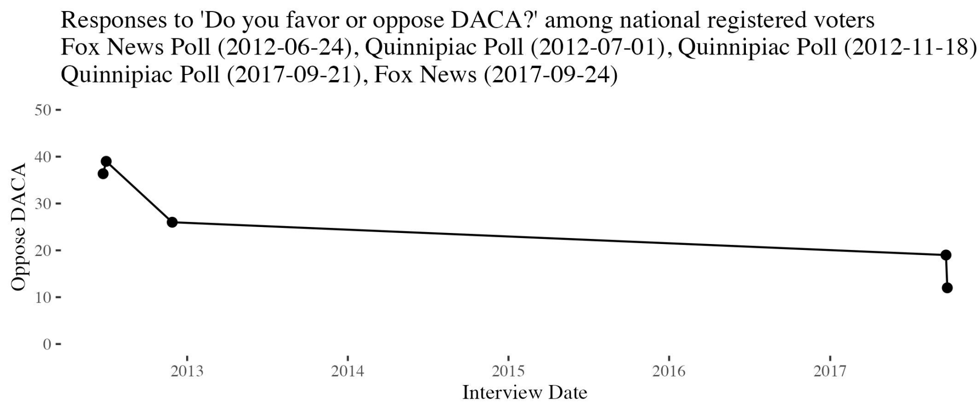

Figure I12: Opposition Toward DACA (x-axis) Over Time in 2012 (y-axis). Estimates are based on a common question asked across the survey datasets outlined in the title: "As you may have heard, President Barack Obama announced the government will stop deportation and grant work permits for certain illegal immigrants under the age of thirty who were brought to the United States as children. Do you favor or oppose this change to immigration policy?"

J Study 2: The Effects of Anti-Immigration Policies

J.1 Protest Activity

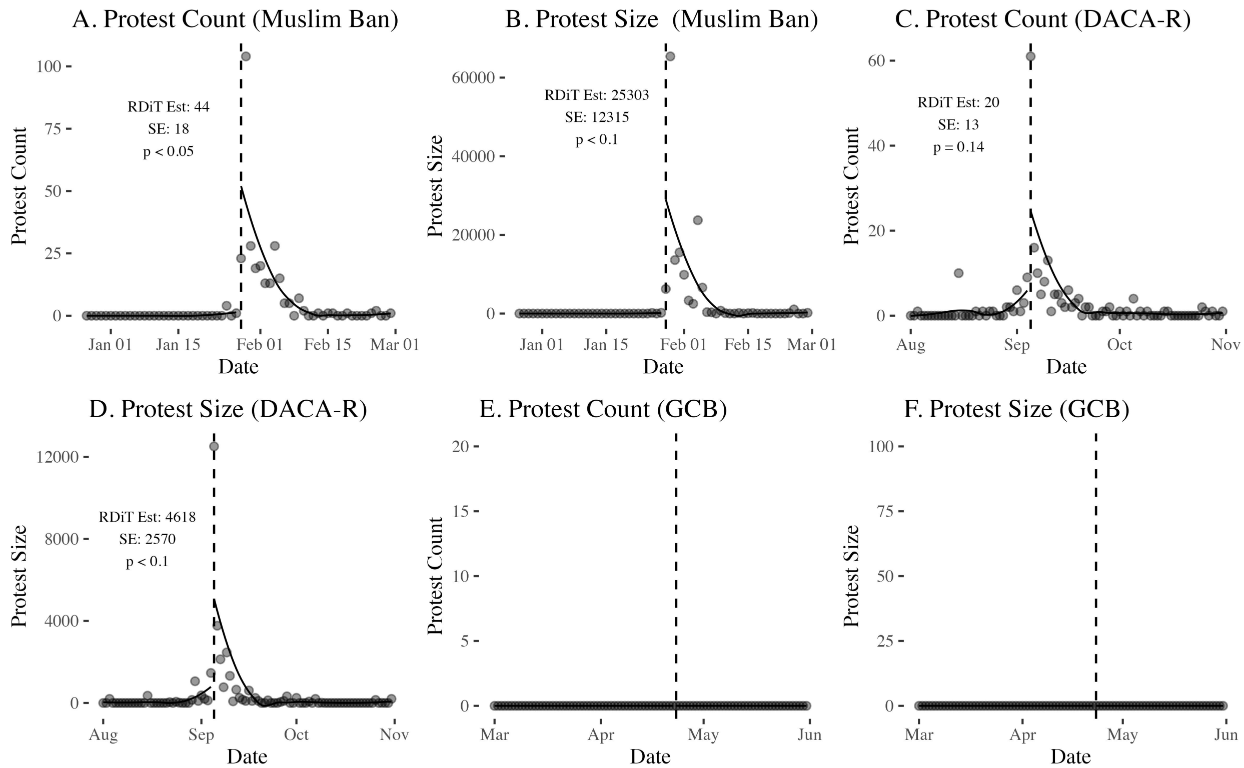

Figure J13: Protest activity around the moment of the Muslim Ban (Panels A-B), DACA Rescission (Panels C-D), and Green Card Ban (Panels E-F). The x-axis is the date, y-axis is either the number of daily protests against the anti-immigration policy or the number of daily people protesting against the anti-immigration policy (protest size). Data are from the Crowd Counting Consortium (CCC) at the Harvard Kennedy School's Ash Center for Democratic Govenrance (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). We identify protests against the Muslim Ban by identifying protests whose claims include the phrase "ban" in the text coded by the CCC. We identify protests against the DACA rescission by identifying protests whose claims include the phrase "DACA" in the text coded by the CCC. We attempt to identify protests against Trump's Green Card Ban by identifying protests whose claims include the phrase "green card" or "lawful permanent" in the text coded by the CCC. Dashed vertical line is the moment the policy is implemented. Annotations denote regression discontinuity-in-time estimates of the effect of policy onset on the count and size of protests using the Calonico et al. (2014) mean-squared optimal bandwidth appraoch (uniform kernel, running variable polynomial set to 1).

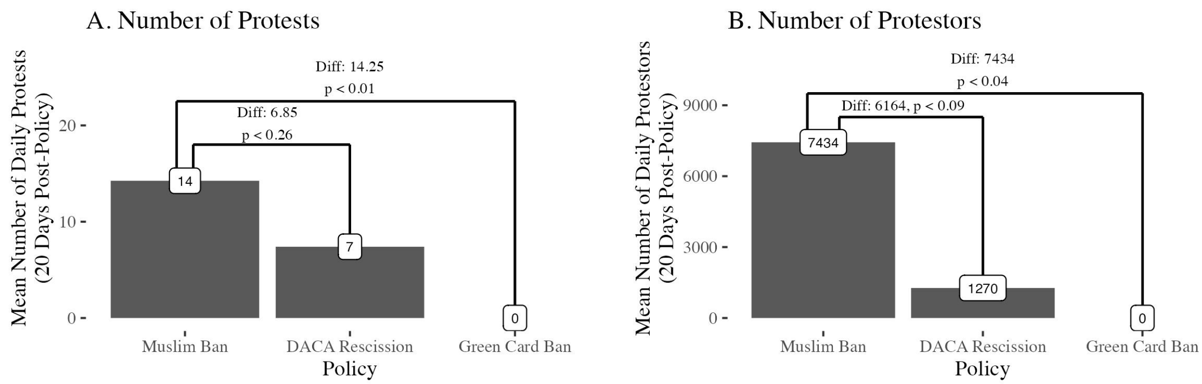

Figure J14: The Muslim Ban had more protest activity against it than the DACA recession or the Green Card Ban. The x-axis is the policy, the y-axis is the average number of daily protests in the 20 days after the policy (Panel A) or the average number of daily protestors against the policy in the 20 days after the policy (Panel B). We identify protests against the Muslim Ban by identifying protests whose claims include the phrase "ban" in the text coded by the CCC. Data are from the Crowd Counting Consortium (CCC) at the Harvard Kennedy School's Ash Center for Democratic Govenrance (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). We identify protests against the DACA rescission by identifying protests whose claims include the phrase "DACA" in the text coded by the CCC. We attempt to identify protests against Trump's Green Card Ban by identifying protests whose claims include the phrase "green card" or "lawful permanent" in the text coded by the CCC.

J.2 Muslim Ban

J.2.1 Estimation Strategy

To evaluate the effects of the Muslim Ban on *Trump favorability*, *Muslim Ban support*, and *anti-Arab attitudes*, we estimate the following model:

$$Y_i = \alpha + \beta_1 Ban_i + \sum_{k=1}^{k} \beta_{k+1} X_i^k + \varepsilon_i$$

Where Y_i is the outcome of interest (Trump favorability, Muslim Ban support, anti-Arab bias, anti-Arab favorability, and the anti-Arab D-score), Ban_i is a binary indicator for being interviewed after the onset of the Muslim Ban (01/27/2017), $\sum_{k=1}^{k} \beta_{k+1} X_i^k$ are k control covariates, ε_i are robust errors. If backlash against the Muslim Ban occurs, we would expect β_1 to be *negative* with respect to the outcomes of interest.

J.2.2 Muslim Ban Salience

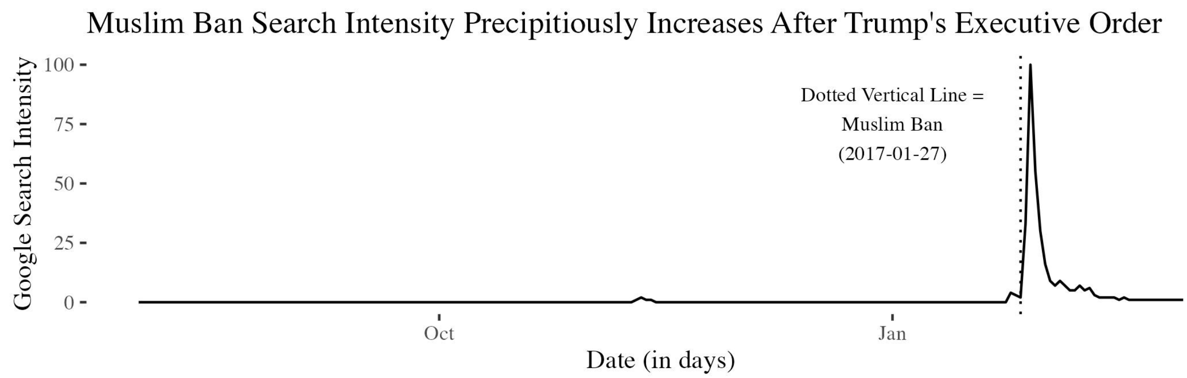

Figure J15: Google Trends data suggests interest in the Muslim Ban was unanticipated prior to Trump's executive order implementing the Muslim Ban. The x-axis is the date (between August 1, 2016-March 1, 2017), the y-axis is the normalized Google search intensity (0-100 scale). Dotted vertical line denotes moment the Muslim Ban was signed via executive order by President Donald Trump (2017-01-27).

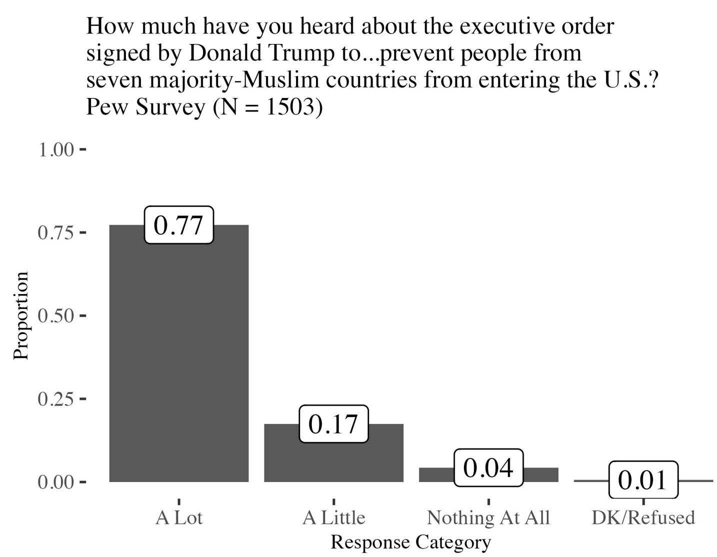

Figure J16: Nationally representative survey data shows nearly 80% of the American public have heard of the Muslim Ban to a significant degree near the moment it was implemented.. The x-axis is the response category, the y-axis is the proportion of people who reported each response category. Data are from a nationally representative Pew Research Center poll. Estimates are population-weighted to ensure representativeness.

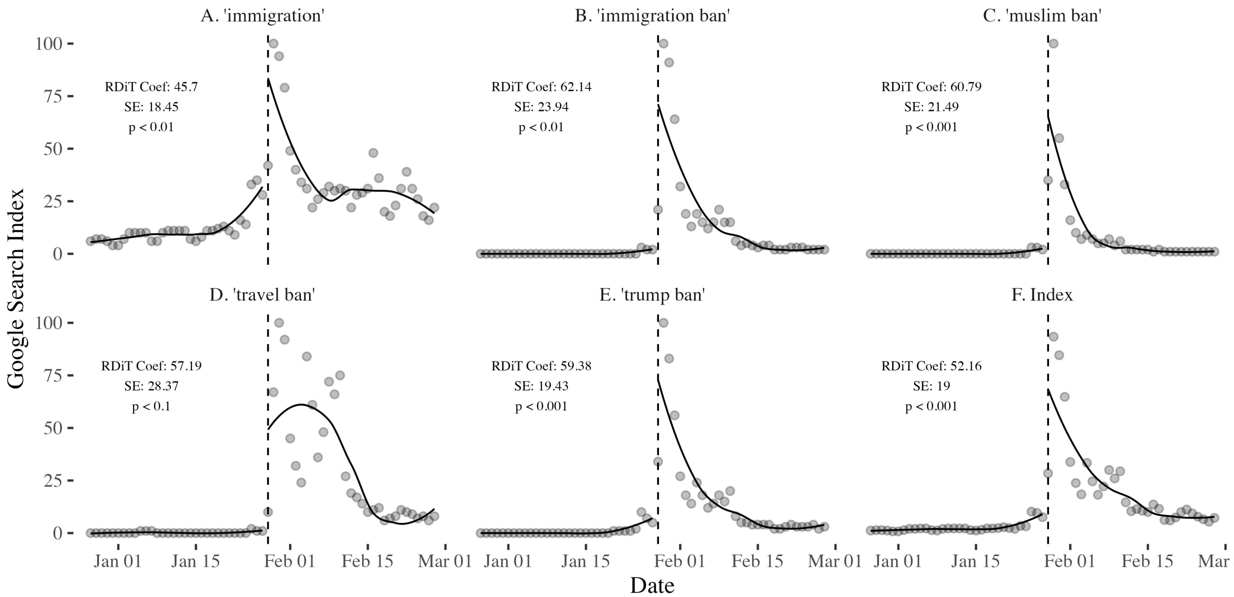

Figure J17: Google Trends data demonstrates search interest in Muslim Ban-related terms discontinously increased after the implementation of the Muslim Ban.. The x-axis is the day (30 days before and after the Muslim Ban), the y-axis is the measure of Google Search intensity (normalized between 0-100). Each panel characterizes a different search term denoted on the title. Panel F characterizes an average index of the search terms outlined on Panels A-F. Annotations denote regression discontinuity-in-time estimates using the Calonico et al. (2014) approach with a linear running variable (days to Muslim Ban) polynomial and uniform kernel.

J.2.3 Covariate Balance, Favorability Outcome

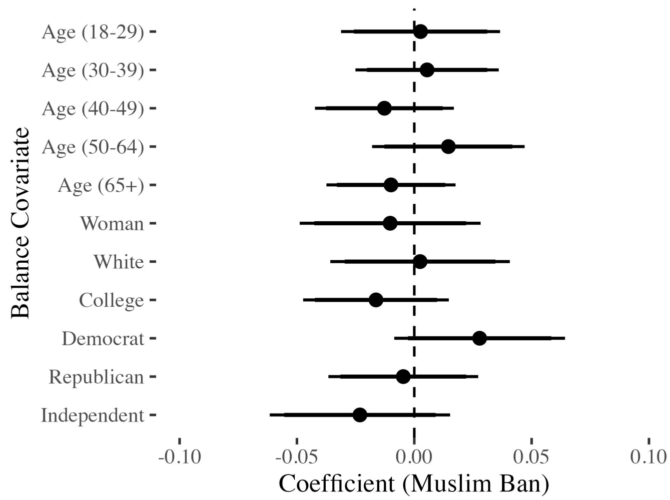

Figure J18: Covariates are balanced between respondents interviewed about their favorability toward Trump before and after the Muslim Ban. X-axis is the post-*MB* coefficient, y-axis is the balance covariate. Respondents interviewed before the Muslim Ban are from the ABC (1/12-1/15) and PRRI (1/18-1/22) polls. Respondents interviewed after the Muslim Ban are from the Pew (2/7-2/12) poll. 95% CIs displayed derived from HC2 robust SEs.

J.2.4 Covariate Balance, Muslim Ban Support Outcome

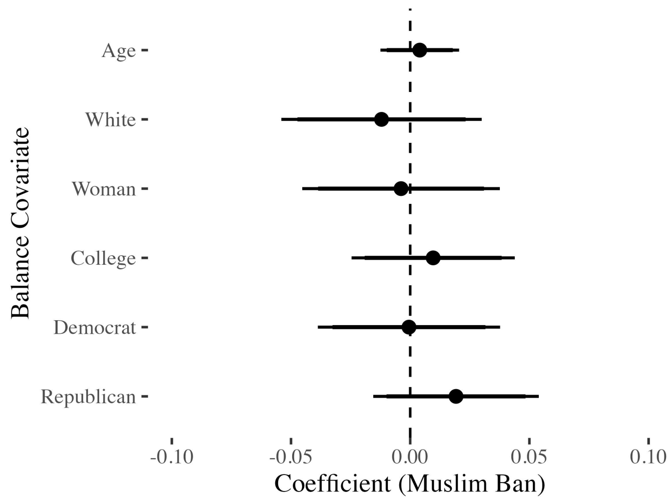

Figure J19: Covariates are balanced between respondents interviewed about their support of the Muslim Ban before and after the Muslim Ban. X-axis is the post-MB coefficient, y-axis is the balance covariate. Respondents interviewed before the Muslim Ban are from the CBS 12/2015, 01/2016, and 07/2016 polls. Respondents interviewed after the Muslim Ban are from the CBS 02/2017 poll. 95% CIs displayed derived from HC2 robust SEs.

J.2.5 Descriptive Statistics, A-IAT

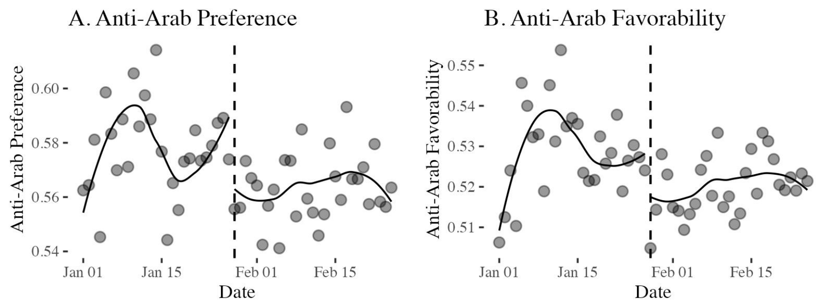

Figure J20: Anti-Arab attitudes (y-axis) over time (x-axis) 30 days before and after the Muslim Ban. X-axis is the date, y-axis is the outcome. Dashed vertical line denotes the implementation of the Muslim Ban. Loess lines are fit on each side of the moment the Muslim Ban is implemented.

J.2.6 Covariate Balance, A-IAT

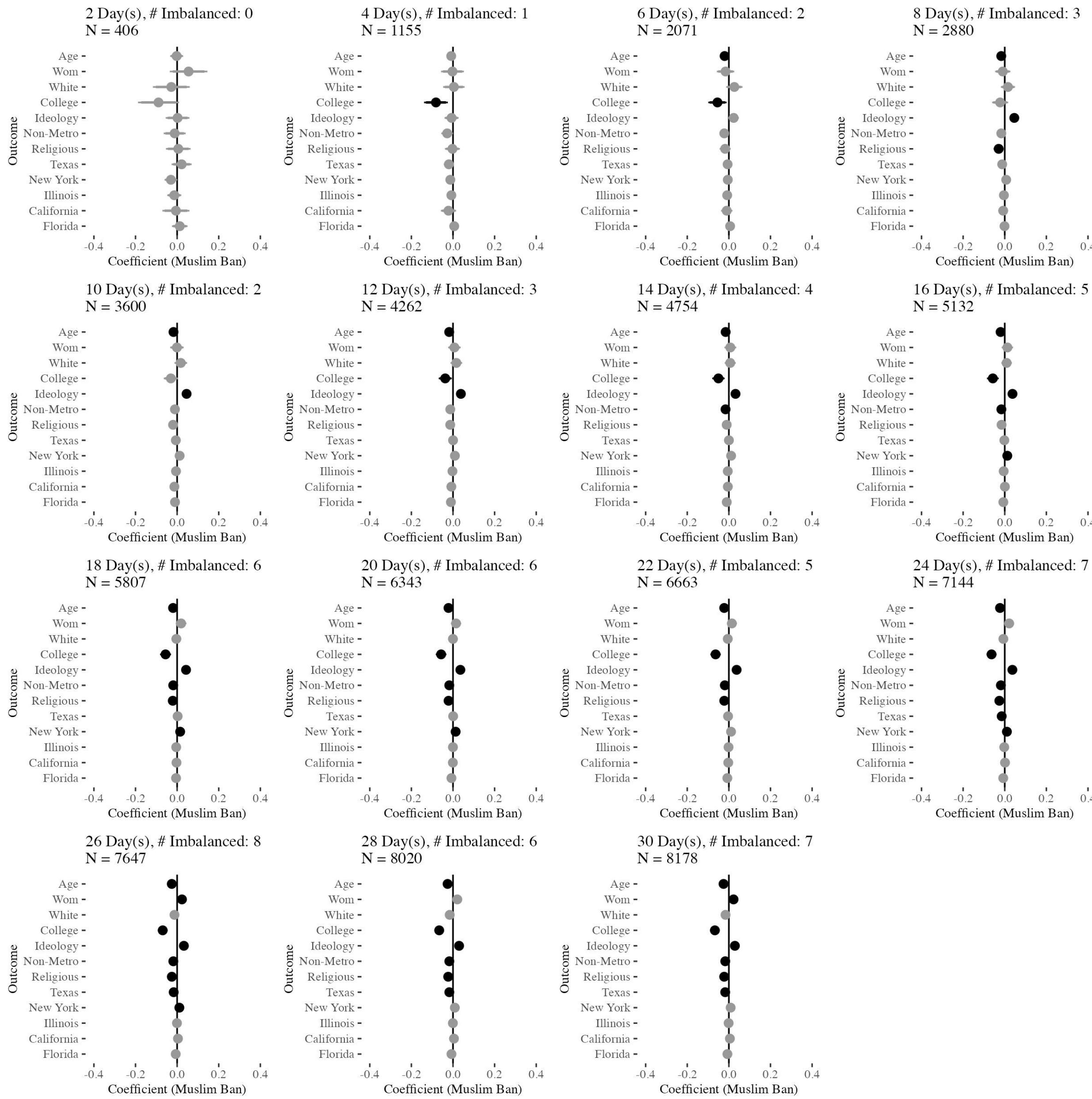

Figure J21: Covariates are balanced between respondents interviewed about their support of the Muslim Ban before and after the Muslim Ban (A-IAT Data). X-axis is the post-MB coefficient, y-axis is the balance covariate. Each panel characterizes a different bandwidth (in days) sample pre/post-MB and the plot title also characterizes the number of imbalanced covariates for each bandwidth A-IAT subsample (in addition to the sample size for each bandwidth subsample). Statistically significant coefficients are colored black, grey otherwise. 95% CIs displayed derived from HC2 robust SEs.

J.2.7 Temporal Placebo, A-IAT

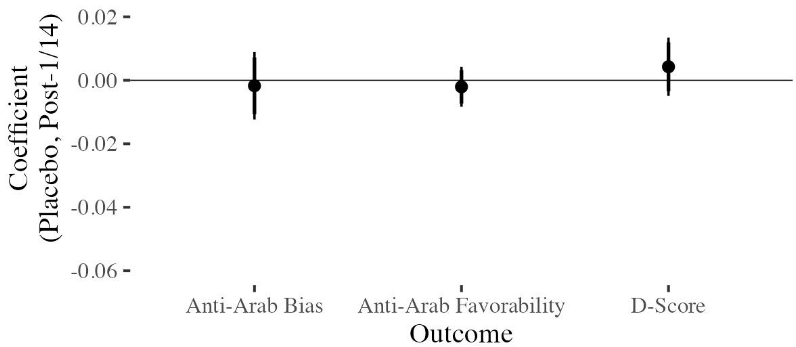

Figure J22: Temporal placebo test characterizing effect of taking the A-IAT after the median pre-*MB* date (January 14th). 95% CIs displayed derived from HC2 robust SEs.

J.2.8 Prior and Post-Year Temporal Placebo, A-IAT

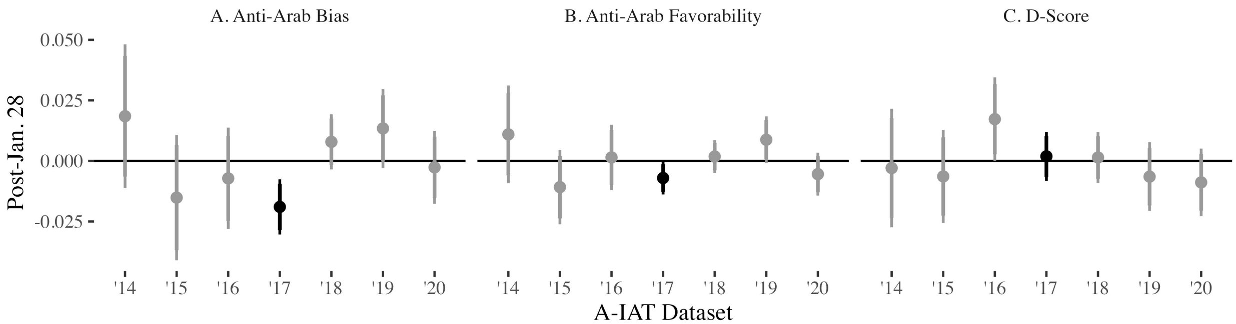

Figure J23: Temporal placebo test characterizing effect of taking the A-IAT after the calendar day of the Muslim Ban (January 28) in the years prior and after to 2017. X-axis characterizes the A-IAT dataset at use. Y-axis characterizes the "effect" of being interviewed after January 28th. Dark coefficients are coefficients from the 2017 A-IAT (the year the Muslim Ban occurred), grey coefficients are coefficients from other A-IAT years (years the Muslim Ban did not occur). Estimates use A-IAT data 6 days before and after January 28th for each respective year. 95% CIs displayed derived from HC2 robust SEs.

J.2.9 Irrelevant-Group Falsification Tests, A-IAT

Table J3: The Muslim Ban does not shift attitudes toward non-Muslim, non-Arab, groups

Post-Ban Coef.	SE	p-value	Outcome	Group	N
0.00	0.01	0.83	Bias	Anti-Disabled	1326
0.01	0.00	0.24	Unfavorability	Anti-Disabled	1342
0.00	0.01	0.87	D-Score	Anti-Disabled	1321
0.00	0.01	0.94	Bias	Anti-Old	2534
0.00	0.00	0.32	Unfavorability	Anti-Old	2605
0.01	0.01	0.21	D-Score	Anti-Old	2592
0.01	0.01	0.64	Bias	Anti-Asian	682
0.02	0.02	0.38	D-Score	Anti-Asian	705
0.17	0.06	0.01	Associate	Anti-Woman (Career)	3044
-0.00	0.01	0.87	D-Score	Anti-Woman (Career)	3038
0.02	0.09	0.79	Associate	Anti-Woman (Science)	1607
-0.01	0.01	0.65	D-Score	Anti-Woman (Science)	1614
0.01	0.02	0.35	Bias	Anti-Native Am.	505
0.01	0.03	0.75	D-Score	Anti-Native Am.	527
-0.00	0.00	0.49	Bias	Anti-Black	8227
-0.00	0.01	0.97	D-Score	Anti-Black	8296
0.00	0.01	0.86	Bias	Anti-LGBT	2966
0.01	0.01	0.21	D-Score	Anti-LGBT	3022

Note: Consistent with the main estimates assessing the effect of the Muslim Ban on anti-Arab attitudes, the data we leverage from the respective Project Implicit datasets on bias against disabled, old, Asians, women, Native Americans, Black people, and LGBT people are subsamples 6 days before and after the onset of the Muslim Ban. HC2 robust SEs reported.

J.2.10 Heterogeneity by Muslim self-identification

Table J4: The post-*MB* effect is weaker among Muslims (6-day bandwidth subsample)

	Anti-Arab Bias (1)	**Anti-Arab Fav.** (2)
Ban x Muslim	0.03	$0.03^{\dagger}$
	(0.03)	(0.01)
Muslim Ban	-0.02^{**}	-0.01^{*}
	(0.01)	(0.00)
Muslim	-0.14^{***}	-0.06^{***}
	(0.02)	(0.01)
Ideology	-0.14^{***}	-0.09^{***}
	(0.01)	(0.01)
Age	0.07^{**}	0.03^{*}
	(0.02)	(0.01)
Woman	0.00	−0.00
	(0.01)	(0.00)
White	0.02^{**}	0.02^{***}
	(0.01)	(0.00)
College	0.01	0.01
	(0.01)	(0.00)
Non-Metro	−0.01	0.00
	(0.01)	(0.01)
Religious	-0.02^{*}	-0.02^{*}
	(0.01)	(0.01)
Texas	−0.01	$-0.01^{\dagger}$
	(0.01)	(0.01)
New York	−0.01	$-0.02^{\dagger}$
	(0.01)	(0.01)
Illinois	−0.00	−0.00
	(0.01)	(0.01)
California	−0.01	0.01
	(0.01)	(0.00)
Florida	−0.02	−0.01
	(0.02)	(0.01)
R^2	0.15	0.15
N	2166	2179

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$

J.2.11 Muslim Ban Results by Partisanship and Political Ideology

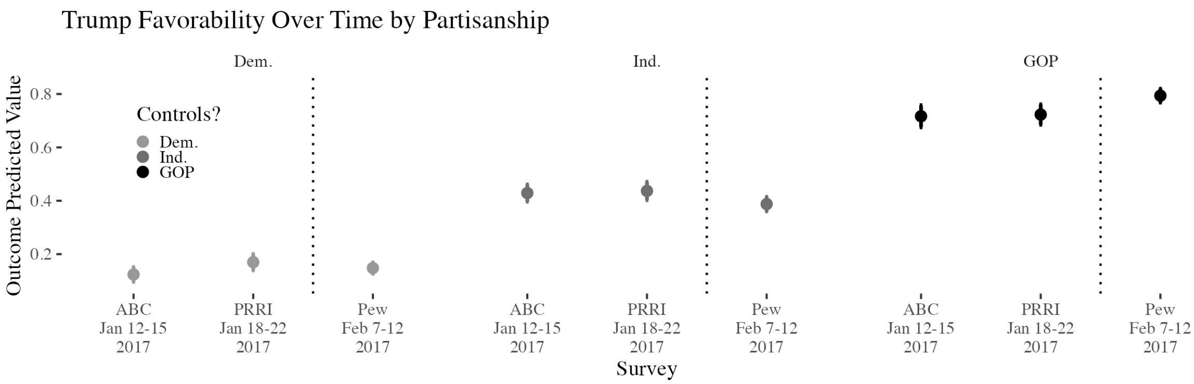

Figure J24: Both Democrats and Republicans reduce their support for the Muslim Ban after it is implemented. The x-axis is the survey period, the y-axis is the predicted value of support for the Muslim ban by party (black = GOP respondents, grey = Democrat respondents) adjusting for covariates (age, white, woman, college-educated). Dotted vertical line denotes surveys fielded before and after the Muslim ban. All estimates are population-weighted. 95% CIs displayed from HC2 robust SEs.

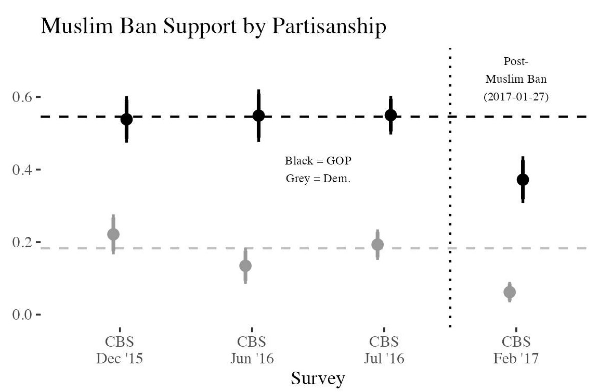

Figure J25: Republicans increase their support for Trump after the Muslim Ban, but independents reduce their support after the Muslim Ban. The x-axis is the survey period, the y-axis is the predicted value of Trump favorability by party (denoted by the plot facets) adjusting for covariates (age, white, woman, college-educated). Dotted vertical line denotes surveys fielded before and after the Muslim ban. All estimates are population-weighted. 95% CIs displayed from HC2 robust SEs.

Table J5: The post-*MB* effect on anti-Arab attitudes does not appear heterogenous by political ideology (A-IAT data, 6 day bandwidth subsample)

	Anti-Arab Bias (1)	**Anti-Arab Fav.** (2)	**D-Score** (3)
Ban x Ideology	0.03	0.01	0.02
	(0.02)	(0.01)	(0.02)
Muslim Ban	-0.04^{*}	-0.02	-0.01
	(0.02)	(0.01)	(0.01)
Ideology	-0.16^{***}	-0.11^{***}	-0.07^{***}
	(0.02)	(0.01)	(0.01)
Age	0.07^{**}	0.04^{**}	0.10^{***}
	(0.02)	(0.01)	(0.02)
Woman	0.00	0.00	-0.01^{*}
	(0.01)	(0.00)	(0.01)
White	0.02^{**}	0.02^{***}	0.00
	(0.01)	(0.00)	(0.01)
College	0.01	0.01	-0.00
	(0.01)	(0.00)	(0.01)
Non-Metro	-0.01	0.00	-0.01
	(0.01)	(0.01)	(0.01)
Religious	-0.02	-0.02^{**}	-0.01
	(0.01)	(0.01)	(0.01)
Texas	-0.01	-0.02^{*}	-0.01
	(0.01)	(0.01)	(0.01)
New York	-0.02	-0.02^{*}	-0.01
	(0.01)	(0.01)	(0.01)
Illinois	-0.01	-0.00	-0.00
	(0.01)	(0.01)	(0.01)
California	-0.00	0.00	-0.00
	(0.01)	(0.00)	(0.01)
Florida	-0.01	-0.01	-0.00
	(0.02)	(0.01)	(0.01)
R^2	0.11	0.13	0.04
Num. obs.	2032	2045	2059

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

J.2.12 Correlation Between Respondent Characteristics and Anti-Arab Attitudes

Table J6: Relationship between demographic/political characteristics and anti-Arab attitudes

	Anti-Arab Bias	**Anti-Arab Fav.**	**D-Score**
	(1)	(2)	(3)
Age	0.06^{***}	0.03^{***}	0.14^{***}
	(0.01)	(0.00)	(0.01)
College	-0.00	-0.00	-0.01^{***}
	(0.00)	(0.00)	(0.00)
Woman	-0.01^{***}	-0.00^{*}	-0.01^{***}
	(0.00)	(0.00)	(0.00)
White	0.02^{***}	0.01^{***}	0.00^{**}
	(0.00)	(0.00)	(0.00)
Ideology	-0.16^{***}	-0.09^{***}	-0.07^{***}
	(0.00)	(0.00)	(0.00)
R^2	0.12	0.11	0.06
Num. obs.	30191	30327	30385

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

J.3 DACA Rescission

J.3.1 DACA Rescission Salience

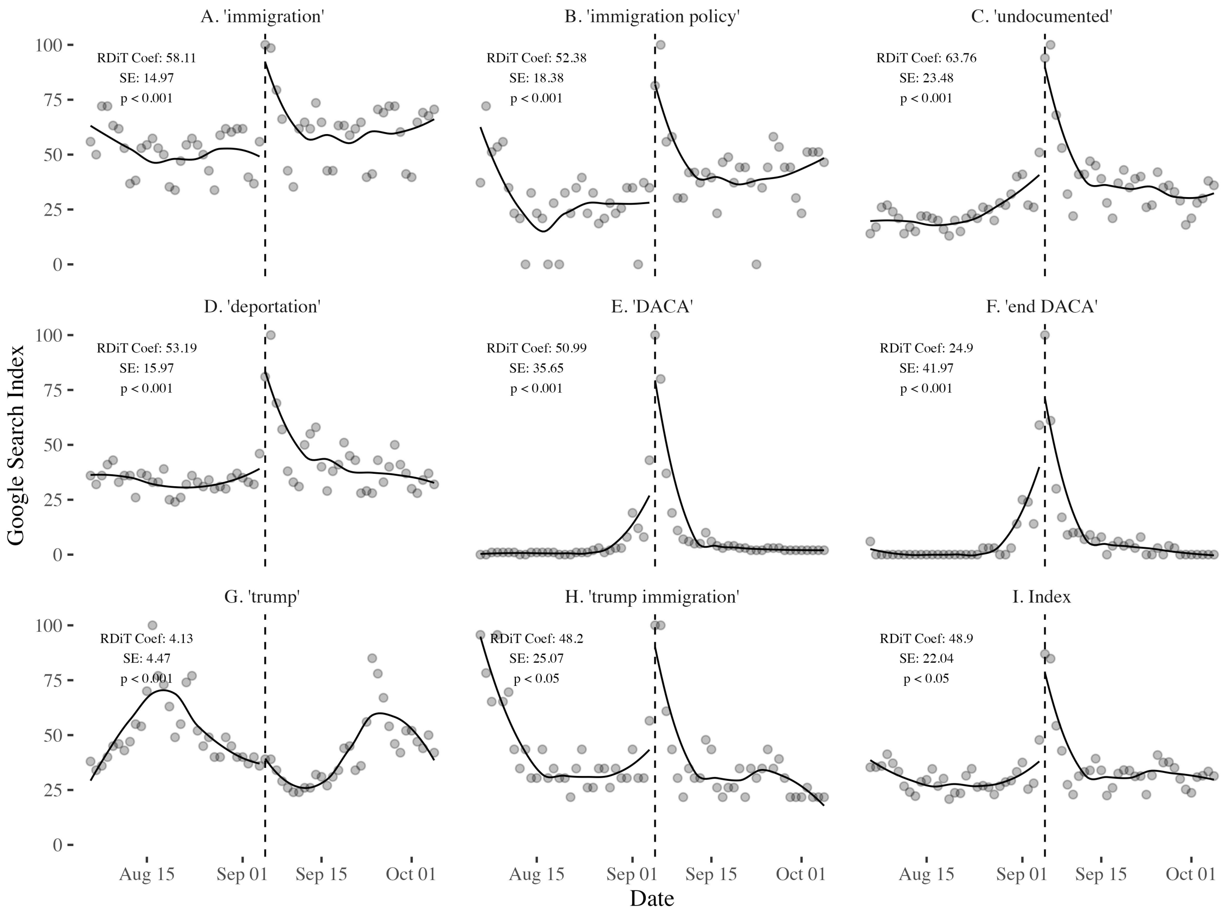

Figure J26: Google Trends data demonstrates search interest in immigration-related terms discontinuously increased after the implementation of the DACA rescission.. The x-axis is the day (30 days before and after the Muslim Ban), the y-axis is the measure of Google Search intensity (normalized between 0-100). Each panel characterizes a different search term denoted on the title. Panel F characterizes an average index of the search terms outlined on Panels A-E. Annotations denote regression discontinuity-in-time estimates using the Calonico et al. (2014) approach with a linear running variable (days to DACA rescission) polynomial and uniform kernel.

J.4 Green Card Ban

J.4.1 Green Card Ban Salience

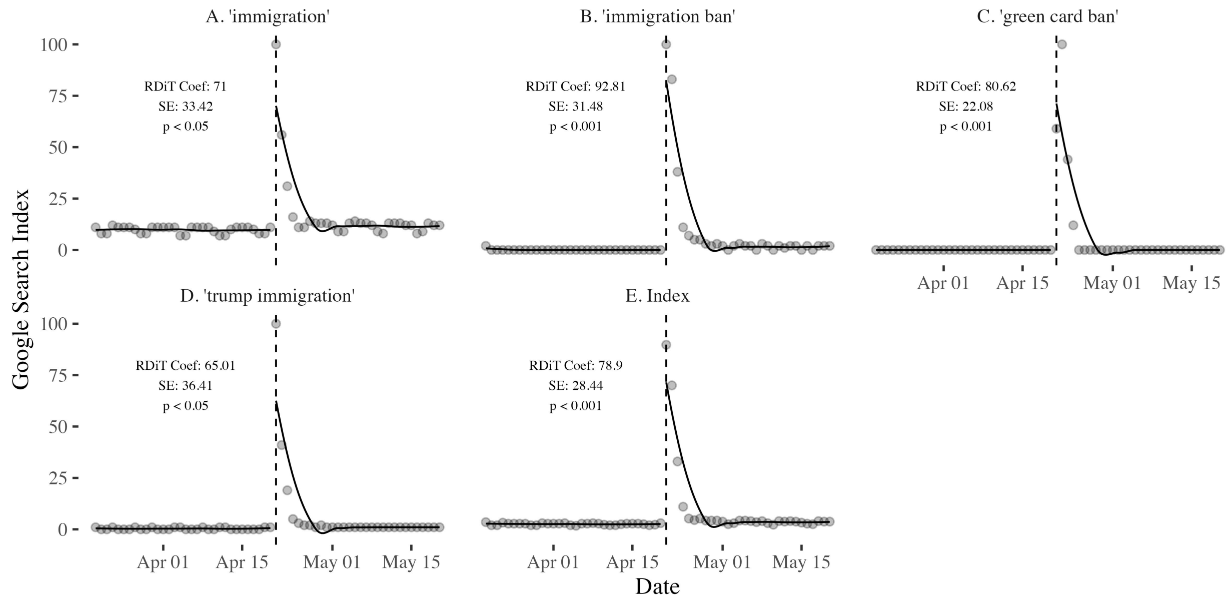

Figure J27: Google Trends data demonstrates search interest in immigration-related terms discontinuously increased after the implementation of the Green Card Ban.. The x-axis is the day (30 days before and after the Green Card Ban), the y-axis is the measure of Google Search intensity (normalized between 0-100). Each panel characterizes a different search term denoted on the title. Panel F characterizes an average index of the search terms outlined on Panels A-E. Annotations denote regression discontinuity-in-time estimates using the Calonico et al. (2014) approach with a linear running variable (days to Green Card Ban) polynomial and uniform kernel.

J.4.2 Balance Test

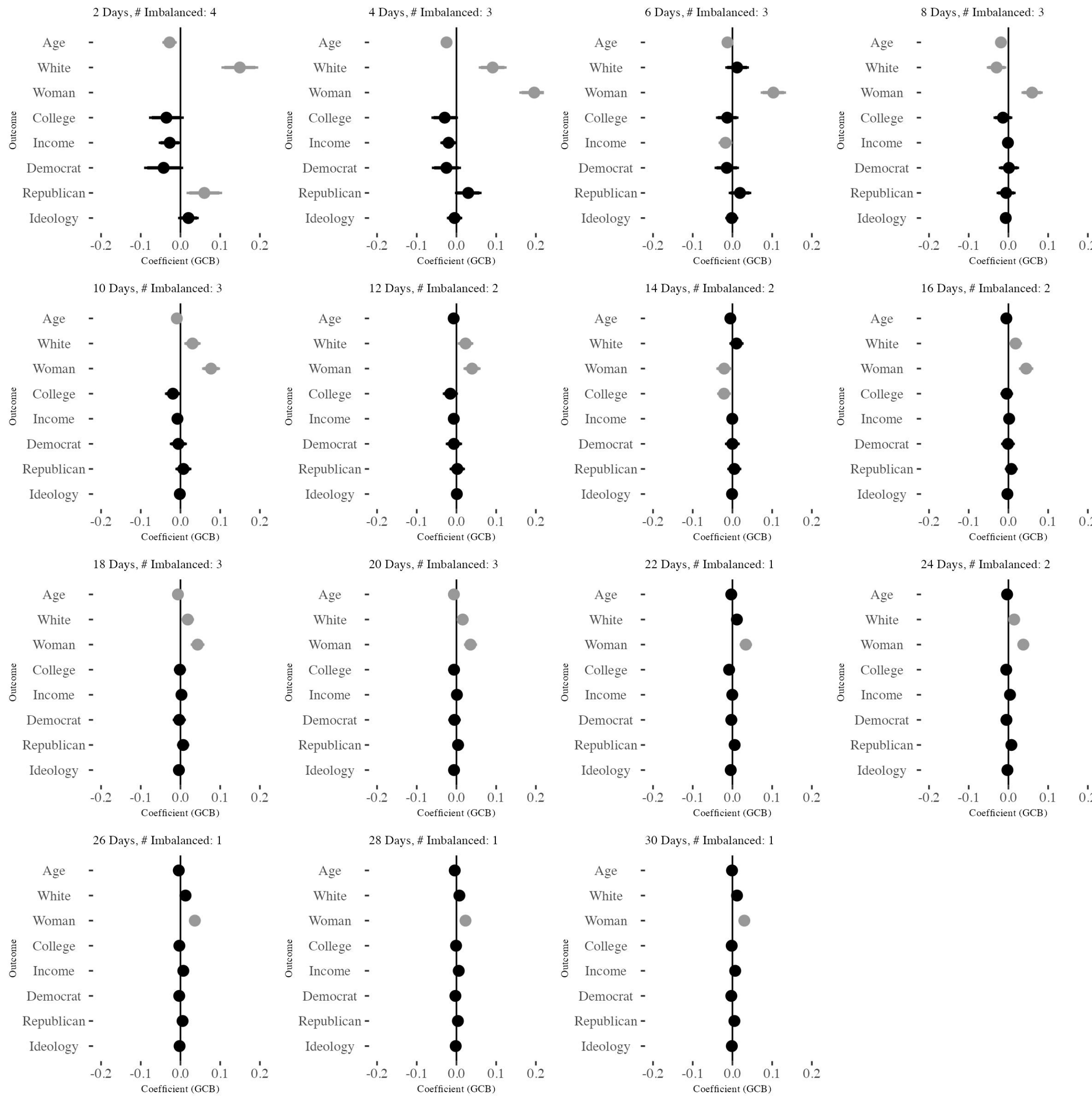

Figure J28: Covariate balance between respondents interviewed before and after Trump's Green Card Ban. X-axis is the post-*GCB* coefficient, y-axis is the balance covariate. Panel title notes the subsample bandwidth in addition to the number of imbalanced covariates. 95% CIs displayed derived from HC2 robust SEs.

J.4.3 Temporal Placebo Test

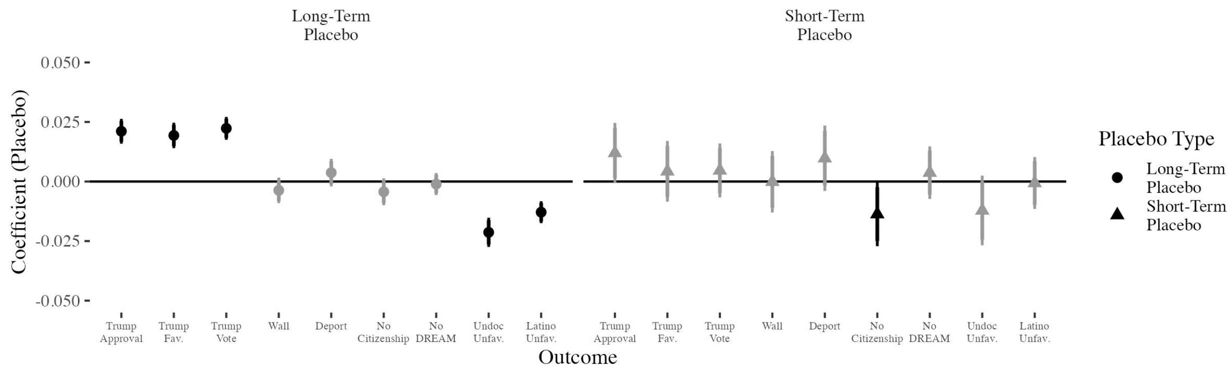

Figure J29: Temporal placebo tests assessing outcome trends prior to Trump's Green Card Ban. X-axis is the outcome of interest, y-axis is the post-*placebo* coefficient. Each facet characterizes estimates evaluating very long-term pre-GCB outcome trends and relatively shorter-term pre-GCB outcome trends. The post-placebo indicator for assessing very long-term pre-GCB outcome trends is a binary indicator equal to 1 if the respondent is interviewed after the median pre-GCB date in the Nationscape data (12/04/2019). The post-placebo indicator for assessing relatively shorter-term pre-GCB outcome trends is a binary indicator equal to 1 if the respondent is interviewed 22 days immediately before the GCB relative to 23-44 days before the GCB. Grey coefficients are statistically insignificant, black coefficients are statistically significant. All temporal placebo tests are covariate-adjusted. 95% CIs displayed derived from HC2 robust SEs.

J.4.4 Falsification Tests

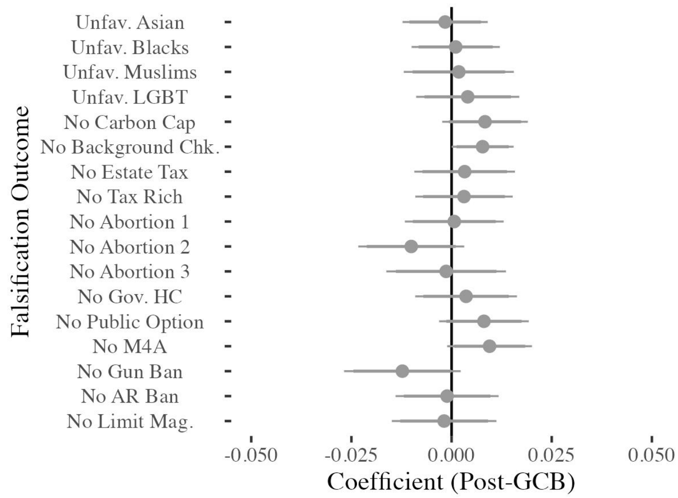

Figure J30: The Green Card Ban did not shift treatment-irrelevant attitudes. Y-axis is a falsification outcome of interest, x-axis is the post-*GCB* coefficient. Nationscape data at use is the 22-day bandwidth subsample. 95% CIs displayed derived from HC2 robust SEs.

K Baseline Preferences Across Policies

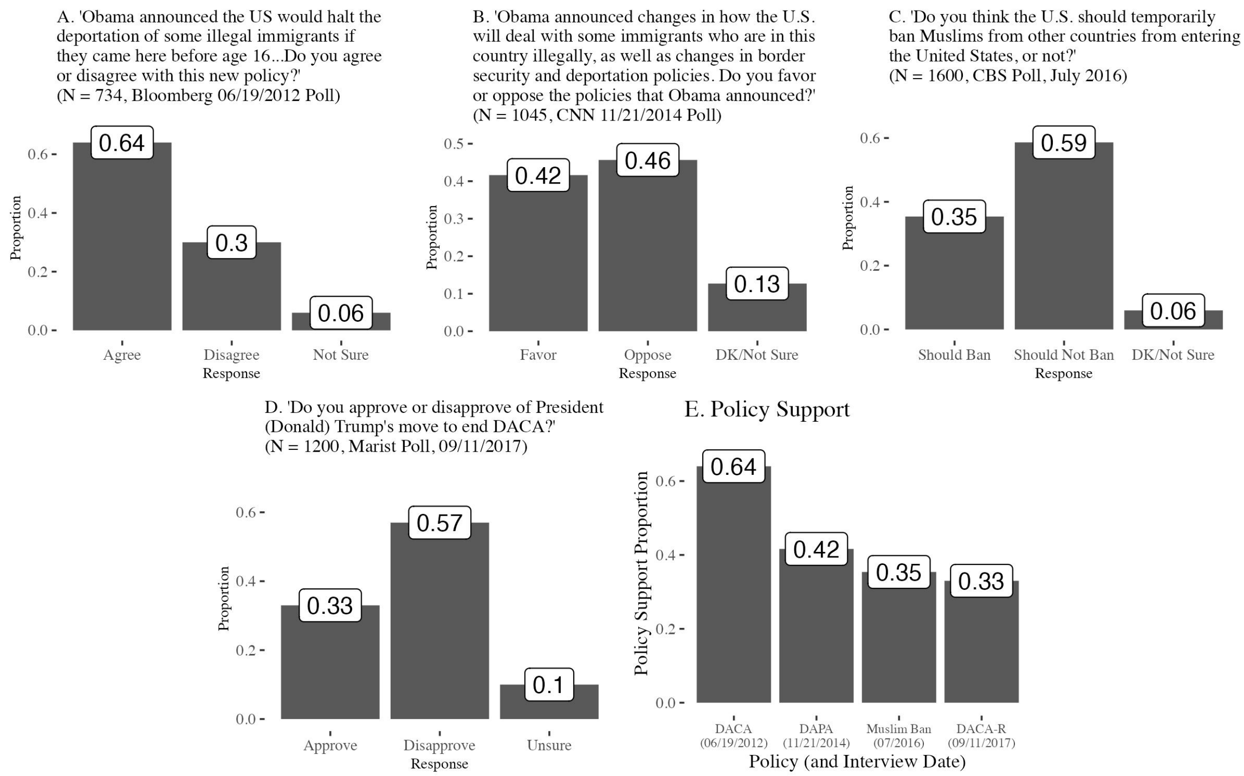

Figure K31: Baseline preferences across policies. Panels A-D characterize support for DACA, DAPA, the Muslim Ban, and the DACA rescission respectively. The title denotes the survey item respondents are responding to (in addition to details on sample size and the time of poll), the x-axis denotes the available responses to the survey, and the y-axis denotes the proportion of respondents indicating each response. Panel E characterizes policy support on the y-axis across the relevant policies on the x-axis. Data on support for the Green Card Ban are not available.