



Do social movements influence decision makers? Evidence from U.S. immigration courts

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HIGHLIGHTS

- Studies how protests influence decisions in U.S. immigration courts.
- Uses the 2006 immigration reform protests as a natural experiment.
- Protests reduced removal decisions by about 2.4%.
- Effect appears to last up to three weeks.
- Impact dissipates by week four, indicating only temporary judicial response.

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ABSTRACT

Can modes of collective action, including protests, demonstrations, and marches influence court decisions, even without affecting the law or formal institutions? Here, I assess the influence of protests on the results of removal proceedings in U.S. immigration courts. Using a difference-in-differences design and administrative immigration court data, I compare removal proceedings outcomes around the time of the 2006 immigration reform protests. I find some evidence of a small and temporary influence on removal decisions. The probability of removals appears to decrease by approximately 2 percentage points for up to 3 weeks following the protests' onset. There is little evidence that the overall effect of mobilization has medium or long term influence on outcomes. The effect varies across case, location and judge characteristics.

1. Introduction

Social movements use protests, demonstrations and marches to signal collective attitudes and objectives, articulate grievances and perceived power imbalances, aiming to effect social, political or economic change (Fisher et al., 2019; Hollyer et al., 2019; Amenta et al., 2010; Wouters and Walgrave, 2017). Protest is core to social movements' behavior differentiating them from special interest or pressure groups (King et al., 2007; Tilly, 1999), and has historically coincided with important shifts in policy and public opinion. The civil rights (Wasow, 2020; Mazumder, 2018; Isaac and Christiansen, 2002), environmental (Kitschelt, 1986), and pro-democracy movements (Markoff, 1999; Ketchley and El-Rayyes, 2021; Bursztyjn et al., 2021), among others have employed protests to influence public preferences and opinion, and facilitate large scale socioeconomic and political change.

Social, economic and political outcomes depend on the actions of multiple agents involved in policy design and implementation, each with

distinct motivations and mandates. Legislators for example determine policy frameworks, while expert decision-makers are appointed to administer them, enforcing predetermined regulations while evaluating often complex circumstances. Understanding the mechanisms driving the influence of collective action requires examining its effect at different stages of the policy design and implementation process. A large and growing body of research characterizes the influence of protest on policy design and legislator selection (King et al., 2007; Kitschelt, 1986; Madestam et al., 2013; McAdam and Su, 2002). Less is known about the direct influence of protest on policy implementation in general, and the application of the law in particular. Can protests influence the interpretation and application of rules, impacting on outcomes even without affecting regulations or formal institutions?

Here I examine the influence of protests on judicial decisions, focusing on the outcomes of removal proceedings in US immigration courts. To this end, I use the natural experiment created by the Spring 2006 US immigration reform protests, when millions of people participated

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in a series of nationwide marches in opposition to a proposed reform of the immigration system that would impose stricter penalties on undocumented immigrants and make it a criminal offense to provide them with assistance. To estimate the immediate influence of the protests on removal proceedings outcomes, I use administrative immigration court decisions data along with information on the timing and location of the marches. As the timing of the protests is orthogonal to the timing of immigration court hearings, I exploit the staggered occurrence of protests across the country, to employ a difference-in-differences design comparing removal proceedings outcomes before and after the protests' onset, across courts that were and were not exposed to a nearby protest. I find some evidence that the 2006 immigration reform protests had a small short term influence on removal decisions. Estimates from a stacked difference-in-differences design suggest that the probability of removal decisions is lower by around 2.4 percentage points in courts experiencing nearby protests, for up to three weeks following the protests' onset. Any influence the protests exerted appears to be temporary, as the effect dissipates by week 4. Results are broadly similar when employing alternative estimators for difference-in-differences designs with staggered treatment adoption (Roth et al., 2023). There is some weak evidence of a non-linear relationship between the magnitude of local protests and the probability of removals, but the influence of the protests turns insignificant when accounting for the distance from the border, while the estimated overall medium to long-term influence of aggregate participation and marches is not stable across specifications.

There is growing evidence linking protest occurrence and salience, to social and political attitudes and behavior. A series of studies use data from the Latino National Survey that was fielded while the 2006 immigration reform marches were ongoing, to show that temporal and spatial proximity to the protests influenced Latinos' interest in politics, and their attitudes towards the government and undocumented immigration (Wallace et al., 2014; Branton et al., 2015; Carey et al., 2014; Barreto et al., 2009; Pantoja et al., 2008; Rim, 2009). Similarly, Pavlic (2021) finds that individual political attitudes in Chile were influenced by proximity to university student protests, Andrews et al. (2016) argue that proximity to civil rights protests encouraged positive views of the movement among white southerners in the early 1960s, while Madestam et al. (2013) show that participation in the Tea Party marches in 2009 improved the Republican Party's local performance in the 2010 midterm elections. The effect of protest on social and political attitudes and behaviors can persist over time. Mazumder (2018) documents that white Americans from counties that experienced civil rights protests in the 1960s are more likely to support affirmative action and identify as Democrats in the present, while Hungerman and Moorthy (2023) show that lower local participation in the original Earth Day in 1970 in the US, is associated with weaker support for environmental causes 20 years later.

Protests can move public opinion and outcomes by increasing the salience of contentious issues. The 2020 Black Lives Matter marches for example increased the salience of anti-racist discourse (Dunivin et al., 2022), contributed to a reduction in the use of lethal force by the police (Campbell, 2023), and are related to lower police favorability and greater perceptions of discrimination against African Americans in some segments of the population (Wright et al., 2023). Collingwood et al. (2018) document a positive shift in public opinion towards immigration following the signing into law of executive order 13,769 which restricted entry to the United States for individuals originating from seven Muslim-majority countries, attributing the change to the protests against the policy, while Jiménez-Sánchez et al. (2022) associate mass protests in Spain during the 2019 International Women's Day with increased interest in gender equality. The frequency of protests on the liberal-conservative spectrum is associated with the election shares for Republicans and Democrats (Gillion and Soule, 2018), while protest occurrence is related to the distribution of public spending (Jaynes, 2002), property market prices around protest locations (Collins and Margo,

2007), and the valuation of firms linked to the subjects of the protest (Acemoglu et al., 2018).

The points raised earlier link protests to social, economic and political attitudes and conditions. How might the influence of protest on attitudes and the ambient social and political environment translate to variation in court decisions? The "legal model" of judicial decision-making posits that judges solely review facts and apply legal reasoning to reach a verdict with reference to precedents and statutes (Spamann and Klöhn, 2016). The legal model does not permit for the influence of exogenous shocks external to case characteristics. It further cannot explain the large systematic variation in decision-making across judges and jurisdictions applying the same law (Weisselberg and Dunworth, 1993; Ramji-Nogales et al., 2007; Rehaag, 2012; Ryo, 2016; Ludwig and Mullainathan, 2021). In response, researchers have proposed multiple alternative and complementary frameworks of judicial decision-making, emphasizing the influence of decision makers' individual characteristics and attitudes (Sunstein et al., 2007; Segal and Spaeth, 2002), their strategic concerns (Epstein and Jacobi, 2010), and the use of heuristics (Simon, 1998).

Attitudinal models of judicial decision-making highlight the role of judges' perceptions, attitudes and individual characteristics (Segal and Spaeth, 2002). In these models, evidence, precedents and statutes are interpreted through the lens of judicial agents' political orientation and ideology (Pinello, 1999; Zorn and Bowie, 2010), gender (Steffensmeier and Hebert, 1999; Boyd and Nelson, 2017), and religion (Bornstein and Miller, 2009; Mehmood et al., 2023) among others. The attitudinal model then implies that salient expressions of public discontent can exert direct influence on the attitudes and perceptions of judicial actors, impacting on their assessment of available evidence, their interpretation of legal principles, and ultimately their judgments (Sunstein et al., 2007; Segal and Spaeth, 2002; Keith et al., 2013).

In strategic models, judicial agents make decisions optimizing some objective function (Posner, 1993; Epstein and Jacobi, 2010; Epstein and Knight, 2013). Decisions can be aimed at improving agents' professional and reputational status (Miceli and Coşgel, 1994; Posner, 1993), promoting their political views (Richards and Kritzer, 2002; Clark et al., 2022), or aspiring to shape the development and interpretation of the law (Epstein and Knight, 2013; Rosenthal and Talmor, 2022). In this context, public protests can influence decision-making by increasing the salience of contentious issues and raising awareness of previously neglected concerns, encouraging decision makers to proactively address them (Hollyer et al., 2019; Amenta et al., 2010), or by prompting responsive action aimed at mitigating unrest (Fisher et al., 2019).

Psychological models of judicial decisions explore the influence of cognitive fallacies, biases and use of heuristics (Rachlinski and Wistrich, 2017; Peer and Gamliel, 2013; Berthet, 2022). Chen et al. (2016) find negative autocorrelation in US judicial asylum decisions, a pattern consistent with the gambler's fallacy, while Kip Viscusi (1999) finds systematic errors in a sample of judges evaluating small-probability, large-loss events. Judicial decisions can follow patterns consistent with anchoring effects (Bystranowski et al., 2021), hindsight bias (Guthrie et al., 2002; Oeberst and Goeckenjan, 2016) and have been shown to co-vary with judges' emotions (Eren and Mocan, 2018). Exposure to protests then can reinforce or alleviate biases, and interfere with heuristics impacting on decision-making.

Drawing from the literature on judicial decision-making, I explore various mechanisms that can plausibly drive a relationship between decisions and protest exposure. To assess whether effects are driven by protest salience (Tilly, 1999; Wouters and Walgrave, 2017), I compare the overall effect of the protests across immigration courts with differential exposure as approximated by the counts of participants and marches respectively. To assess whether results align with attitudinal and strategic models of judicial decision-making (Segal and Spaeth, 2002; Clark et al., 2022), I test the effect's heterogeneity across case characteristics, immigration judge characteristics, and regional

characteristics, that can influence both judicial discretion and sensitivity to external pressure impacting on the magnitude and direction of the protest's effects. Using the fact that the marches were predominantly attended by members of Latino communities, plausibly focusing the perceived aims of the protests on improving conditions for Latino immigrants, I compare protest influence across cases concerning Latino and non-Latino individuals. I further test whether the effect varies systematically across cases based on the respondent's custody status. To gain some insight into whether protest influence varies across judges' backgrounds, I compare the prevalence of removal decisions across relatively more and less lenient judges, and across judges that are likely Hispanic and not. Finally, I examine how the effect varies with the local population, across immigration courts in majority Republican and Democrat regions, and with the distance from the Mexico-US border. The results suggest that the effect is driven by a combination of mechanisms aligning with attitudinal and strategic models of judge behavior, as protests update judges' understanding of public sentiment and highlight the reputational costs of their decisions. I find that the probability of removal decisions declines more among stricter immigration judges, and in courts in areas with Republican majorities. One plausible interpretation is that stricter judges strategically manage their reputations in response to local political sentiment. Because they tend to issue harsher decisions on average, they may have greater scope to moderate their rulings without undermining their perceived credibility. By contrast, relatively more lenient judges may already operate near a constraint on acceptable leniency, limiting their ability to adjust their behavior. At the same time, the protests lower the probability of removals for cases concerning Latinos, but the size of the effect decreases with the overall magnitude of the protests, and reverses for Latino respondents that have been detained, suggesting that increased protest visibility, institutional constraints, and hardening plausibly due to backlash to the protest, may limit judicial responsiveness to external pressure.

The rest of the paper is structured as follows: the next section presents some background information on the immigration court system and the 2006 immigration reform protests. Section 3 discusses the data and the empirical approach, while Section 4 presents the results. Section 5 discusses and concludes.

2. Background

2.1. Immigration courts and removal proceedings

The US immigration system, the associated institutions, and the day-to-day handling of cases by immigration courts are described in detail by Ramji-Nogales et al. (2007), Baum (2009–2010), Eagly (2015), Ryo (2016), Hausman et al. (2023) and Shiff (2022) among others. Here I summarize some features that are relevant to the subsequent analysis. At the time of the 2006 immigration reform protests, there were 52 Federal immigration courts in different geographical locations referred to as “base cities” (Eagly, 2015), employing over 200 immigration judges who handled around 200,000 cases per year. Immigration judges are attorneys, appointed and supervised by the US Attorney General. They do not have life tenure and can be removed or reassigned at the Attorney General's discretion (Ryo, 2016). When adjudicating removal proceedings, immigration judges decide whether to sustain the charges as presented by the government or terminate the case if no proper grounds for removal are cited. If the charges are sustained, the case results in a removal. However, respondents have the right to apply for a relief or voluntary departure. In this case, immigration judges are also called to decide on the relief application (Eagly, 2015). Within each court, cases are randomly assigned to immigration judges who review them in the order they were received (Chen et al., 2016; Ramji-Nogales et al., 2007). Immigration judges have the authority to order the removal of non-residents and residents who are charged with alleged immigration offenses (Eagly, 2015). They are allowed substantial leeway

while exercising their authority, evidenced by the variation in outcomes across immigration courts and judges (Ryo, 2016). For example, in their comprehensive analysis of asylum granting decisions Ramji-Nogales et al. (2007) show that Colombian applicants had approval rates ranging from 5% to 88% across judges in a Miami immigration court, while the approval rate for Indian applicants ranged from 3% to 84% in a San Francisco immigration court. Similarly Ryo (2016) identifies heterogeneity in the outcomes of bond hearings for detained undocumented immigrants.

2.2. The 2006 immigration reform protests

In December 2005 the US House of Representatives passed the Border Protection, Anti-Terrorism, and Illegal Immigration Control Act (H.R.4437) (Rep. Sensenbrenner, 2006). The bill's most contentious provisions increased the severity of penalties for undocumented immigrants, reclassifying their presence in the U.S. from a civil to a felony offense, and criminalized any activity assisting undocumented individuals to enter or remain in the country (Fetzer, 2006). The bill had significant adverse ramifications for many communities and was opposed by community leaders, civil society and professional organizations (McKeon, 2006; Philadelphia Bar Association, 2006; American Civil Liberties Union, 2006; Barreto et al., 2009; VARA, 2006).

In opposition to H.R.4437 a series of protests were organized from February to May 2006. Over 250 marches and demonstrations took place in 44 states. Between 3 and 5 million protesters attended the marches, with the largest in Chicago and Los Angeles (Bada et al., 2006b). The protests emerged through hybrid mechanisms, combining spontaneity with elements of central direction. Established immigrant rights organizations, labor unions faith-based groups, and Spanish-language media networks played critical roles (Bada et al., 2006b; Rim, 2009), while diffusion of information through the media allowed the movement to grow without having a unique command center. The first large scale march occurred on March 10, 2006, in Chicago, attended by up to 300,000 participants. On March 25 and April 10, marches in Los Angeles were attended by an estimated 500,000 to 1 million individuals. The movement culminated on May 1, 2006, with the nationally coordinated “Day Without Immigrants.” The May 1 action combined demonstrations, school walkouts, work stoppages, and consumer boycotts designed to demonstrate immigrants' positive economic impact and power. While not a general strike, the withdrawal of labor from agricultural, construction and service industries was intended to highlight immigrant labor's importance. Protests were overwhelmingly peaceful featuring strong intergenerational participation.

Marches were primarily attended by individuals of Latino descent (Branton et al., 2015; Wallace and Zepeda-Millán, 2020; Wallace et al., 2014). The relatively greater mobilization of Latino communities has been attributed to provisions of HR4437 that explicitly referred to Latino immigrants, the emphasis the bill placed on the policing of the US-Mexico border, the penetration of Spanish-language media and the effectiveness of Latino community organizations (Rim, 2009). As the protests progressed, their framing shifted to emphasize immigrant embeddedness in the country, with many participants displaying U.S. rather than country-of-origin symbols. The bill was not approved by the Senate and was not signed into law.

3. Data and empirical approach

3.1. Data

I use administrative immigration court decisions data from the Executive Office for Immigration Review (EOIR), an agency operating under the Department of Justice. The data became publicly available following Freedom of Information Act requests and have been used by Ramji-Nogales et al. (2007), Eagly (2015), Chen et al. (2016), Hausman

et al. (2023), Kim and Semet (2020), and Spamann (2022) among others. EOIR data are updated every month. I use the June 2023 release,¹ containing information from over 9 million cases from 1953 to 2023. The database includes information on case, proceedings and hearing characteristics. This includes the immigration court with jurisdiction over the case, the immigration judge hearing the case, the date of the hearings, and the case outcome. It also reports on the nationality and custody status of the respondent. To construct the final dataset I follow the steps described in Eagly (2015), Thorley and Mitts (2019) and Kim and Semet (2020). The exact procedure and variables used are presented in detail in Appendix A.

I remove from the analysis cases that do not reflect judicial decisions. Specifically, I follow Eagly (2015) and Spamann (2022) to exclude administrative case closures² and instances where a case was resolved through the respondent's initiative, instead focusing on cases where a decision to remove or not was reached by the judge.³ I further remove stipulated removals that are the result of deliberation between the immigration judge and the respondent. Each unique EOIR case entry includes information on possibly multiple proceedings and hearings. Following Eagly (2015) and Thorley and Mitts (2019), in the main analysis I restrict the data to the first proceeding of each case that was decided by a judge, effectively focusing on a single proceeding per case.⁴ In robustness tests I also show estimates when using all proceedings. I use information from active immigration courts (Eagly, 2015), excluding observations from courts that handled fewer than 500 cases in 2005 and 2006. Occasionally courts will merge, split or close and when this happens records are transferred to a new court. I focus on cases heard by courts that were continuously operational from 2006 to 2023. I further remove observations by immigration judges that heard less than 50 cases in the same period. Finally, I filter out cases involving nationals from countries with fewer than 50 observations during that time.

Table 1 summarizes some characteristics of cases adjudicated between 1 January and 1 June that encompasses the samples for all analyses reported later. During this period, there were 88,846 proceedings decided by 227 judges in the 41 immigration courts examined here. Around 76% of the cases concerned nationals of Spanish majority-speaking countries of the American continent, while the majority of cases (81%) resulted in a removal decision. About 17% of the cases concerned individuals that were detained and 11% concerned cases involving a criminal indictment.

Data on protests come from the “2006 Immigrant Rights Protests - Mapping American Social Movements” database (Bada et al., 2006b,a), reporting information for 268 protests that occurred in 163 cities and 44 states, between 14 February and 1 May 2006, collected from local and national news media reports. In addition to the date and the city where a protest occurred, the database reports upper and lower bound participation estimates, as well as the news outlet reporting on the protest. For some of the subsequent analysis, I use the midpoints of the participation

Table 1
Sample descriptive characteristics.

Variable	Average	St.Dev.
Removal	0.81	0.39
Latino	0.76	0.43
Detained	0.17	0.37
Criminal indictment	0.11	0.31
In-person hearing	0.95	0.22
Representation	0.30	
Cases per judge	1056	1160
Cases per court	5514	4165
Immigration Judges	227	
Immigration Courts	41	
Cases	88,846	
Protest participation (upper)	19,139.39	80,906.53
Protest participation (lower)	13,359.8	55,079.45
Events by location	2.55	1.95
Population in protest counties	801,045	1,127,288
Share of foreign born population in protest counties	0.14	0.09
Share of Hispanic population in protest counties	0.19	0.17

Note: The top panel presents descriptive characteristics of proceedings where a decision was reached 1 January 2006 and 1 June 2006. Removal, Latino, Detained, Criminal indictment, In-person hearing and Representation are all binary. Cases per judge and Cases per court are the counts of cases heard by judges and courts respectively. Protest participation (upper) and Protest participation (lower) refer to the upper and lower bounds of protest attendance as reported in Bada et al. (2006a).

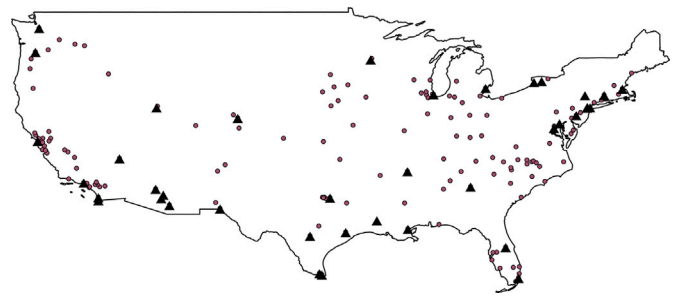


Fig. 1. Immigration courts (triangles) and protests (circles).

intervals to proxy protest magnitude. The protests attracted on average 19,139 participants, with the largest march occurring in Chicago on May 1 and attracting 500,000-1,000,000 protestors. The smallest recorded protest occurred in Wilmington attended by just 12 participants. The recorded locations experienced on average 2.5 protests each. The average share of foreign born and Hispanic population in the counties where protests took place were 14% and 19% respectively. Average population was over 800,000 individuals. For the courts included in the sample, in year 2005 the share of removals ranged from 42% (New York) to 97% (Oakdale) with a median of 82% (Salt Lake City). Fig. 1 illustrates the distribution of the immigration courts and protests in space. Fig. BF.1 in the appendix shows the cumulative distributions of protest participation and protest events over time, while Table BT.2 in the appendix shows the count of protests across locations.

3.2. Empirical approach

To assess the influence of the 2006 immigration reform protests on removal proceedings outcomes, I employ a difference-in-differences approach exploiting the staggered occurrence of marches in the proximity of immigration courts. To this end, I compare removal proceedings outcomes across hearings conducted in the weeks before and after the start of the protests, in immigration courts that were and were not exposed to a nearby protest. As hearing dates are determined some time in advance

¹ EOIR retrospectively revises case data. Therefore case information may vary across releases.

² Until 2012 immigration judges could not grant an administrative closure if either party were opposed (ACLU, 2022). Administrative closures at the time of the 2006 immigration reform protests therefore represent the judge's decision conditional on DHS approval, or they are the outcome of combined decision-making by the judge and the DHS. Furthermore, the timing of administrative closure is uncertain as guidance suggests that “Where at all possible, issues involving administrative closure should be resolved in advance of individual calendar hearings and not at hearings” (ACLU, 2022). I nevertheless show estimates including administrative closures in robustness tests.

³ This includes decisions marked as “Remove”, “Exclude”, “Deport”, “Voluntary Departure”, “Admit”, “Relief Granted”, “Terminated” and “Final Grant of EOIR 42B/SUSP”.

⁴ For the rest of the analysis I use the terms “case” and “proceeding” interchangeably.

(GAO, 2017), the timing of protest occurrence is orthogonal to the timing of the hearings.⁵

An extensive literature in econometrics documents the challenges in interpreting the estimates from Two-Way Fixed Effects (TWFE) estimation of difference-in-differences designs with staggered treatment adoption, arising from comparing across units that are treated at different points in time (Borusyak et al., 2021; de Chaisemartin and D'Haultfœuille, 2020, 2023; Roth et al., 2023; Sun and Abraham, 2021; Goodman-Bacon, 2021). In staggered adoption designs, the TWFE estimator is a weighted average of all possible 2×2 difference-in-differences estimators. When treatment effects vary over time, weights can be negative (Goodman-Bacon, 2021). To address the ensuing bias I present estimates from a stacked difference-in-differences analysis (Cengiz et al., 2019; Baker et al., 2022; Bessen et al., 2023). The stacked DiD approach avoids comparisons between earlier- and later-treated units by approaching each event as a separate sub-experiment with single treatment and control groups during the event window. In each sub-experiment treated units are compared against never- or not-yet-treated units that are “clean controls”. To carry out the stacked difference-in-differences analysis, I start by creating separate datasets for each cohort of immigration courts that experienced their first nearby protest event on week s , keeping observations in a 10-week event window $[s - 5, s + 4]$ (i.e., hearings occurring in the 5 weeks before and the 4 weeks after the week of the first protest for cohort s). To each of these cohort-specific datasets, I add observations from hearings occurring on the same calendar dates in never-treated courts (i.e., those courts that did not experience any nearby protest) and in not-yet-treated courts (i.e., those courts that experience the first nearby protest after 4 weeks). These observations are clean controls, as they are not subject to a protest event during the time window. Table BT.1 in the Appendix shows the courts by cohort of treatment. To average across sub-experiment specific effects I stack the cohort-specific datasets in relative time and estimate:

$$Y_{itcs} = \alpha_{cs} + \gamma_{ts} + \beta T_{itcs}^d + X_i + \epsilon_{itcs} \quad (1)$$

The outcome Y is a binary variable indicating whether removal was ordered following hearing i . t , c and s index the week of the hearing, the immigration court with jurisdiction over the case, and the sub-experiment respectively. α_{cs} are court by sub-experiment effects, differencing out time invariant court characteristics that may influence outcomes (Ramji-Nogales et al., 2007), accounting for the fact that immigration courts in the control group may appear in more than one sub-experiments. γ_{ts} are time by sub-experiment effects, capturing time-specific variation common across all cases (Baker et al., 2022). Matrix X_i contains a set of case characteristics, including the respondent's country of national origin, immigration judge specific effects, and variables indicating hearings concerning respondents who are detained, hearings concerning respondents who are criminally indicted, in-person hearings, and hearings where the respondent had legal representation.⁶

The treatment variable T^d is binary, equal to one for cases heard after the first protest that occurred within d miles of a court's base city. For the baseline analysis, I set $d = 100$ miles, to account for courts outside city limits, the size of average local media markets (Gurun and Butler, 2012; Gentzkow, 2006; Peress, 2014), and potential spillovers across locations. In robustness tests I also show estimates when $d = 25$ and 50 miles. Coefficient β captures the difference in the probability of removals across courts that have, and have not experienced a protest, before and after the protest. The parallel trends assumption requires that in the

absence of the protests, the evolution of removal decisions would have been similar across all immigration courts irrespective of the eventual treatment status. While this assumption cannot be tested directly, I use the leading terms in an event study specification to gain some insight into the existence of pre-treatment trends and assess the evolution of the effect over time. I use the stacked dataset to estimate:

$$Y_{itcs} = \alpha_{cs} + \gamma_t + \sum_{w=-5, w \neq -1}^4 \beta_w I_{icsw}^d \times T_{itcs}^d + \sum_{w=-5, w \neq -1}^4 \delta_w I_{icsw}^d + X_i + \epsilon_{itcs} \quad (2)$$

Each I_{icsw}^d is binary, indicating hearings that took place on week w before or after the first protest that occurred within d miles of the immigration court with jurisdiction over the case. Coefficients β_w capture the change in the probability of the outcome between courts that were and were not exposed to the protests on week w relative to the week before the first protest ($w = -1$) which is omitted.

A threat to the validity of the Difference-in-Differences approach described earlier comes from the possibly endogenous nature of the protests. It is plausible that unobserved general public attitudes towards H.R.4437, immigrants and immigration, may simultaneously drive protest occurrence and determine removal outcomes, biasing estimates. To assess this threat I discuss the organization and focus of the protests, in juxtaposition with public opinion. While large, the 2006 protests did not attain the public engagement and sustained involvement characterizing a broad social rights movement (Johnson and Hing, 2007; Rim, 2009). Participants came primarily from Latino communities of recent immigrants, while the engagement of other disadvantaged or immigrant communities was low, and their disposition towards the protestors and their objectives was sometimes negative (Johnson and Hing, 2007). Whilst protests were to some extent coordinated nationally (Bloemraad et al., 2011), information on the provisions of H.R.4437, was mostly targeted to immigrant communities (Bloemraad et al., 2011), and was communicated primarily through Spanish language media, grassroots immigration organizations, hometown associations and family networks (Bloemraad et al., 2011; Wang et al., 2011; Lazos Vargas, 2008). The limited mobilization beyond immigrant communities suggests that anticipation and awareness of the marches was not diffused across the general population. The wide distribution of marches in space and the extent of participation surprised both organizers and observers, suggesting they were unexpected and spontaneous (Wang et al., 2011; Lazos Vargas, 2008). It does not then seem that protest occurrence reflects underlying attitudes towards immigrants and immigration. To further investigate the salience of immigration at the time around the protests, Fig. BF.5 in the Appendix uses data from Google Trends to show the distribution of searches for “immigration” over time and across states. There does not appear to be a systematic increase in people searching for “immigration” in the time leading to the protests. Interest in immigration in many cases appears to pick up only after the protests

Table 2
Protests and the outcome of removal proceedings.

	(1)	(2)
T^{100}	-0.027** (0.009)	-0.024*** (0.007)
Date×Sub-exp. FE	Yes	Yes
Court×Sub-exp. FE	Yes	Yes
Case Controls		Yes
Dependent variable mean	0.86	0.86
Observations	112,789	112,789

Note: Estimates from the stacked difference-in-differences design described in Eq. (1). The dependent variable is binary, indicating a removal decision. The treatment variable T^{100} is binary, equal to one for cases heard after the first protest that occurred within 100 miles of a court's base city. Standard errors clustered at the immigration court level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

⁵ The EOIR database records the date on which the Department of Homeland Security issued the charging document to the individual. For the proceedings examined here, the mean and median time between the charging document's issuance and the first hearing were 156 and 91 days respectively. Similarly, the mean and median times between the issuance date and the hearing date when the first decision was made were 315 and 155 days respectively.

⁶ Table BT.3 reports the means of the observable characteristics by treatment group.

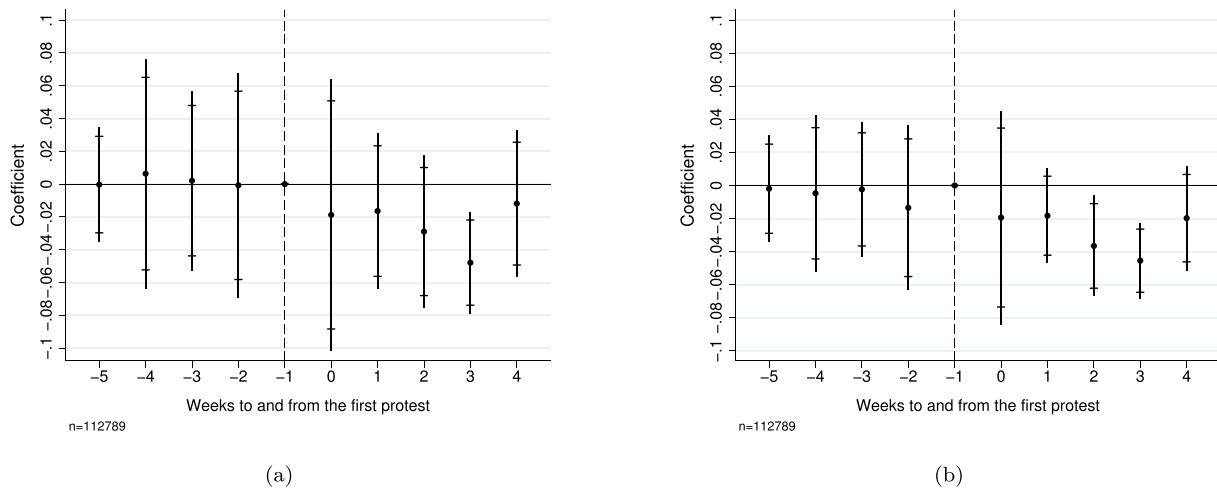


Fig. 2. Protests and the outcome of removal proceedings. Estimates from the stacked event study specification in Eq. (2). The dependent variable is binary, equal to one for a removal decision. The model in Fig. 2(a) does not include case controls. The model in Fig. 2(b) adds the full set of case controls. Dots are point estimates of the difference in the probability of the outcome in week $\{-5, \dots, -2, 0, \dots, 4\}$ relative to week $\{-1\}$. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

take place. To provide some further insight into whether attitudes towards immigration changed in the time leading to the protests, Section B.6 in the Appendix presents an analysis using data from the 2006 Social Capital Community Survey (SCCS) (Putnam, 2022).

4. Results

4.1. Protests and removal proceedings outcomes

Table 2 reports stacked difference-in-differences estimates from Eq. (1), comparing the probability of removal decisions before and after the first protest occurring within 100 miles of a court's base city. There is evidence that protest exposure has quantitatively small but statistically significant influence on the outcomes of removal proceedings. The model controlling only for time $\times$ sub-experiment and court $\times$ sub-experiment fixed effects (column 1) suggests that exposure to a nearby protest is related to a 2.7 percentage point decrease in the probability of a removal decision. Introducing case controls slightly attenuates the effect to 2.4 percentage points (column 2). Given a baseline removal rate of 86%, this corresponds to a 2.7% percent decline in removals, and suggests that approximately 433 fewer removals among the 18,033 post-treatment cases can be attributed to the protests.⁷

Fig. 2 shows estimates from the event study specification in Eq. (2), capturing the change in the probability of removals on each of the 5 weeks leading to and following the first march that occurred within 100 miles of the immigration court with jurisdiction over a case, relative to the week prior to the first march. Coefficients on the lead indicators are in all cases close to zero and statistically insignificant, lending some support to the parallel trends assumption. Estimates on the lag indicators are consistent with the results reported in Table 2. For the model in Fig. 2(a) which accounts only for time and court $\times$ sub-experiment effects, the probability of removals decreases by around 4 percentage points in the four weeks following the first nearby protest. Adding the full set of controls slightly varies the size of the estimates which now range from 3.7 to 4.7 percentage points. Importantly any influence the protest exerts appears to be temporary, decreasing over time with a clear tendency of reverting to zero by the fourth week after the protests.

4.2. Protest attendance and frequency

The analysis so far relies on the variation in the timing of the first protest occurring in the neighborhood of immigration courts. It does not say much about the importance of the crowd the protests attracted, or whether the frequency of the marches mattered for outcomes in the medium to long run (Wouters and Walgrave, 2017; Tilly, 1999). To assess if the overall size or the frequency of protests matters for removal outcomes, I compare between courts that were more and less exposed to the protests, before and after the entire protest period, estimating:

$$Y_{itc} = \alpha_c + \gamma_t + \beta P_t \times M_c^d + X_t + \epsilon_{itc} \quad (3)$$

where P_t is binary, equal to one for cases heard after the end of the protests (after 1 May 2006) and zero for hearings that took place before the start of the protests (before 1 February 2006). The sample includes hearings that took place between 14 January and 1 June 2006 (i.e., in the 30 days before the first and the 30 days after the last) protest as reported in Bada et al. (2006a), excluding hearings during the entire protest period (14 February - 1 May 2006). γ_t are date specific effects and M_c^d is a measure of protest exposure intensity. I approximate exposure intensity with protest size and frequency. Specifically, M_c^d is either (i) the sum of attendance in all protests that occurred within a $d = 100$ mile radius from the base city of court c , or (ii) the sum of marches within a $d = 100$ mile radius from the base city of court c . Similar dose-response approaches have been used extensively to assess the impact of multivalued treatments on various outcomes (Cook et al., 2023; Lindo et al., 2020). The parallel trends assumption for the canonical difference-in-differences specification with a binary treatment extended to the continuous treatment case, requires that in the absence of the protests the development of judicial decisions would have been similar across all courts irrespective of the eventual protest intensity they faced. To gain some insight on the validity of this assumption, Fig. BF.2 in the Appendix plots removal decisions over time, for immigration courts in different quartiles of the protest attendance and protest frequency distributions.⁸

⁸ It is important to note however that Callaway et al. (2021) show that when the treatment is multivalued the parallel trends assumption suffices only for identifying the average treatment effect on the treated for a given dose level. In the present case, this suggests that identifying average treatment effect of

⁷ Equivalently treatment reduces the outcome by 0.06 standard deviations.

Table 3
Protest intensity and removal proceedings outcomes.

	(1) Participation	(2) Participation	(3) Frequency	(4) Frequency
$P \times Q_2$	-0.030 (0.018)	-0.016 (0.010)	-0.003 (0.019)	0.007 (0.010)
$P \times Q_3$	-0.027 (0.027)	-0.023** (0.010)	-0.005 (0.024)	-0.011 (0.012)
$P \times Q_4$	0.034 (0.025)	0.025 (0.020)	-0.003 (0.038)	0.005 (0.025)
Date FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
Case Controls		Yes		Yes
Dependent variable mean	0.81	0.81	0.81	0.81
Observations	34,774	34,774	34,774	34,774

Note: Estimates from the difference-in-differences design described in Eq. (3). The dependent variable is binary, indicating a removal decision. P is binary indicating cases decided after 1 May 2006. In columns 1 and 2, Q_2 , Q_3 and Q_4 are binary, indicating cases heard by courts in the 2nd, 3rd and 4th quartile of the marches participation distribution. In columns 3 and 4, they indicate cases heard in the corresponding quartiles of the marches frequency distribution. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Columns 1 and 2 of Table 3 show estimates when M_c^d enters Eq. (3) flexibly in bins to avoid imposing a functional form on the relationship between intensity and outcomes. There is some evidence that the probability of removals decreases with participation in nearby marches. For the model accounting for the full set of controls, the probability of removals is lower by 2.3 percentage points for cases heard by courts in the 3rd quartile of the participation distribution respectively, relative to cases heard by courts in the 1st quartile. The coefficient on the interaction with the 4th quartile indicator is positive, albeit statistically insignificant. I do not find evidence suggesting a relationship between the count of nearby marches and proceedings outcomes (columns 3 and 4). Table BT.4 in the appendix repeats this analysis, this time scaling protest participation and events by the population within 100 miles of the court's base city. The results do not suggest a statistically significant relationship between removals and intensity⁹. A possible interpretation is that the largest protests generated spillover effects that extended beyond the 100 mile radius used here. If these events received extensive national coverage, they may have influenced judges in areas classified as controls, thereby attenuating estimated effects in the highest participation categories.

4.3. Heterogeneity across case characteristics

The 2006 immigration reform protest marches were attended primarily by individuals of Latino descent (Branton et al., 2015; Rim, 2009). Did the protests' focus on Latino communities improve outcomes for Latinos¹⁰ awaiting a decision on removal proceedings, perhaps through attitudinal or strategic mechanisms? Columns 1 and 2 in Table 4 and Fig. 3(a) report estimates from Eqs. (1) and (2) for cases concerning

some exposure intensity m requires the evolution of removal outcomes in courts that received m to be similar to courts that would have received m had they been treated. To identify the average treatment effect across all units, Callaway et al. (2021) propose a stronger parallel trends assumption requiring the average change in outcomes over time across units had they received dose m , is the same as the average change in outcomes for those units that did in fact receive m .

⁹ Fig. BF.6 shows estimates from a version of Eq. (3) that replaces the Post-protest indicator with a series of week dummies to assess pre- and post-protest trends. Estimates do not suggest significant pre-protest trends in removals. Nevertheless, it appears that the overall effect of protest is to increase the probability of removals in the post-protest period.

¹⁰ For this analysis I define Latino individuals as the nationals of Spanish-speaking majority countries of the American continent. These include Argentina, Bolivia, Chile, Colombia, Costa Rica, Cuba, Dominican Republic, Ecuador, El

Latino and non-Latino individuals. The coefficient on T^{100} is negative but statistically insignificant for the Latino sample while it suggests that removals decrease by 3.5 percentage points for the non-Latino sample. Event study estimates are similar suggesting that the probability of removals decreases by up to about 8 percentage points in the first 3 post-protest weeks for the non-Latino sample (Fig. 3(a)). The result for the Latino sample is likely due to the significant variation in the distribution of Latino respondents originating from different countries across courts, along with the strength of local protests.¹¹ To further assess this, columns 3 and 4 interact the treatment indicator with the count of participants for the entire protest duration within a 100 mile radius, for the Latino and non-Latino cases respectively. Accounting for participation suggests that the probability of removals decreases by about 3 percentage points for the Latino sample, but the influence of the protest decreases as participation increases. There are at least two plausible interpretations for this result. First, it points to diminishing marginal influence of the protest. Courts may respond strongly to initial attention but larger protests may induce fatigue or institutional hardening and backlash that counteract the initial effect. Second, the result may be the outcome of selection effect, as larger protests may occur in regions with already higher removal rates.

An individual's custody status may signal their unobserved characteristics and predispose decision makers positively or negatively towards them. Estimates in columns 5 and 6 suggest that the effect of the protests is insignificant in cases concerning immigrants that have been detained, but the protests appear to reduce the probability of removal by about 1.8 percentage points for the non-detained. Results are mirrored by event study estimates (Fig. 3b). To see whether the influence of the treatment varies across Latino and non-Latino cases by detainment status I estimate Eq. (1) interacting the treatment and detained indicators. Estimates in columns 7 and 8 of Table 4 suggest that while protests appear to decrease the probability of removal among non-detained Latino respondents, they tend to increase the probability of removal for the detained. On the other hand, custody status does not appear to influence the probability of removal for the non-Latino sample. Results are likely due to the fact that custody status signals underlying case characteristics that influence judges, plausibly counteracting the protest. I also interact participation with the treatment variable for the samples of detained Latino and Latino non-detained cases. The coefficient on the interaction term is insignificant for the former (0.002, se = 0.0005) but positive and significant for the later (0.009; se = 0.002). As earlier this can be compatible with backlash against the group primarily represented in the protests. Fig. BF.7 in the Appendix shows event study estimates for the groups of Latino detained and non-detained cases. It appears that a divergence occurs at around the 3rd post-protest week when the probability of removal increases for the detained but decreases for the non-detained.

4.4. Heterogeneity across judge characteristics

To assess how protest exposure might influence decisions through its interactions with judge characteristics, I begin by assessing whether judges with Hispanic backgrounds responded differently to the 2006 protests. I infer judges' likely ethnicity using a probabilistic analysis of their surnames, employing approaches described in Imai and Khanna (2016), Imai et al. (2022) and Rosenman et al. (2023). As this analysis cannot capture the complexity of individual identities, multiracial backgrounds, or the social and familial dimensions of ethnicity, the resulting classification should be interpreted as a proxy rather than a definitive measure of ethnic identity. Based on these probabilities, I construct a

Salvador, Guatemala, Honduras, Mexico, Nicaragua, Paraguay, Peru, Panama, Uruguay, and Venezuela.

¹¹ Indeed when removing the Miami court that heard 83% of cases concerning Cubans from the sample, or when introducing nationality-by-court effects the result turns significant (Table BT.5 in the Appendix shows these results).

Table 4
Heterogeneity across case characteristics.

	(1) Latino	(2) non-Latino	(3) Latino	(4) non-Latino	(5) Detained	(6) non-Detained	(7) Latino	(8) non-Latino
T^{100}	-0.013 (0.009)	-0.035* (0.020)	-0.030*** (0.010)	-0.034 (0.022)	-0.009 (0.009)	-0.018* (0.009)	-0.020** (0.009)	-0.035* (0.020)
$T^{100} \times Participation$			0.007*** (0.002)	-0.001 (0.002)				
$T^{100} \times Detained$							0.034*** (0.013)	-0.005 (0.024)
Date×Sub-exp. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court×Sub-exp. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable mean	0.89	0.71	0.89	0.71	0.85	0.85	0.89	0.71
Observations	92,716	20,067	92,716	20,067	18,530	94,218	92,716	20,067

Note: Estimates from the stacked difference-in-differences design in 1. The dependent variable is binary, indicating a removal decision. The treatment variable T^{100} is binary, equal to one for cases heard after the first protest that occurred within 100 miles from a court's base city. *Participation* is the sum of participants in all protests that occurred within 100 miles of a court's base city. *Detained* is binary for cases concerning respondents that have been detained. Columns 1 and 2 use the samples of Latino and non-Latino immigrants respectively. Columns 3 and 4 show estimates when interacting the treatment indicator with protest participation within 100 miles of the base city of the immigration court, for the samples of Latino and non-Latino immigrants respectively. Columns 5 and 6 use the samples of detained and not-detained immigrants. Columns 7 and 8 show estimates when interacting the treatment indicator with the detained cases indicator for the Latino and non-Latino samples respectively. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

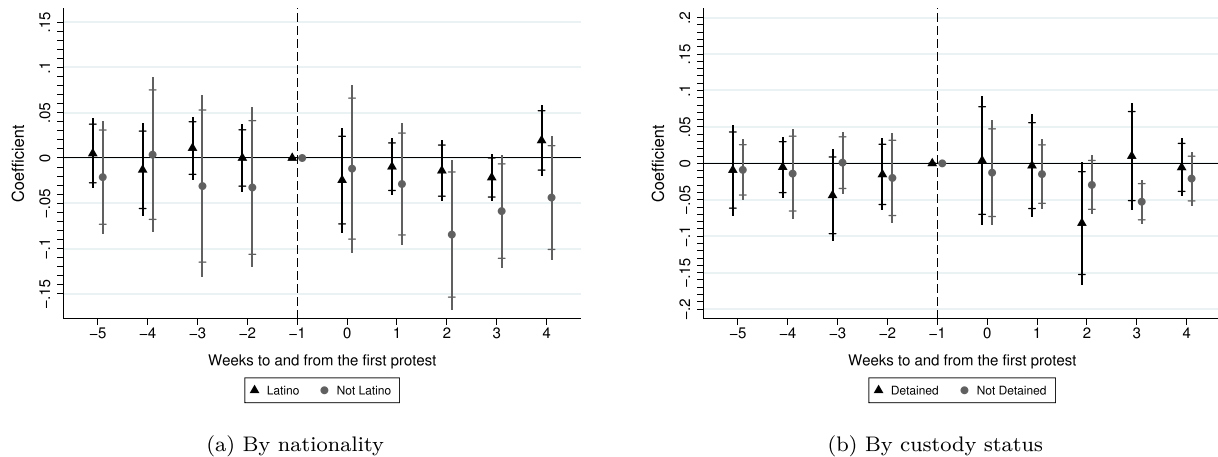


Fig. 3. Estimates from the stacked event study specification in Eq. (2). The dependent variable is binary, equal to one for a removal decision. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

binary indicator for Hispanic name origin, equal to 1 for immigration judges whose highest predicted probability corresponds to the Hispanic category. I then estimate the models in equations 1 and 2 for the samples of Hispanic and non-Hispanic judges. This analysis is subject to the caveat that the sample contains only a small number of Hispanic judges. Estimates in columns 1 and 2 of Table 5 and Fig. BF.8 in the Appendix, suggest that the effect is entirely due to non-Hispanic judges. This is consistent with the hypothesis that the predominantly Latino protests plausibly increased the scrutiny placed on judges who were perceived to be distant from the affected population. Non-Hispanic judges may have therefore faced stronger incentives to adjust their decision-making in response. In contrast, Hispanic judges already perceived as aligned with the interests of the protestors had less incentive to do so. At the same time, judges of non-Hispanic backgrounds may update their beliefs more strongly in response to visible mobilization of Latino communities compared to judges with Hispanic backgrounds who are already (perceived to be) aligned with the protestors.

I further examine whether the effect varies with judges' prior strictness. To this end, I construct a measure of strictness, as the share of removal decisions made by each judge before January 1 2006, and estimate equations 1 and 2 for the samples of judges in the lower half of the

strictness distribution (the lenient) and the judges in the upper half (the strict). Estimates in columns 3 and 4 of Table 5 suggest that the effect is entirely driven by the strict group. One plausible interpretation is that stricter judges have greater flexibility to adjust decisions in a more lenient direction following a salient shock like the protests. On the other hand, already lenient judges may operate closer to a boundary of acceptable leniency, and therefore have less opportunity for granting relief. At the same time, the protests may have updated judges' beliefs about public opinion, issue salience and the reputational costs of their decision-making. Interpreting public protests as evidence of greater scrutiny, strict judges are plausibly incentivized to adopt more lenient position either to align with the public sentiment or to strategically manage their reputation.

4.5. Heterogeneity across regional characteristics

The salience and perceived consequences of the protest may vary with the size of the local population. Courts located in more populous areas plausibly face greater media coverage and public scrutiny. If judges strategically respond to changes in perceived public attention, the effect of protest exposure may differ systematically across

Table 5
Heterogeneity across judge characteristics.

	(1) Hispanic	(2) Non-Hispanic	(3) Lenient	(4) Strict
T^{100}	0.046 (0.089)	-0.022*** (0.009)	-0.010 (0.016)	-0.040*** (0.009)
Date×Sub-exp. FE	Yes	Yes	Yes	Yes
Court×Sub-exp. FE	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes
Dependent variable mean	0.94	0.83	0.64	0.93
Observations	29,245	83,526	28,062	84,721

Note: Estimates from the stacked difference-in-differences design described in Eq. (1). The dependent variable is binary, indicating a removal decision. The treatment variable T^{100} is binary, equal to one for cases heard after the first protest that occurred within 100 miles of a court's base city. Columns 1 and 2 use the sample of cases adjudicated by immigration judges that are likely to be of hispanic origin and not, respectively. Columns 3 and 4 use the sample of cases adjudicated by immigration judges at the 1st and 2nd, and the 3rd and 4th quartiles of the strictness distribution respectively. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

regions at different points of the population distribution. Column 1 of Table 6 regresses removal decisions on the treatment indicator and its interactions with indicators for courts located in base cities in the second through fourth quartiles of the 2010 population distribution within a 100-mile radius, using data from EPA (2010). There is some evidence that the influence of protest exposure increases with the surrounding population. The probability of removal decisions at courts in the third quartile of the population distribution is about 1.5 percentage points lower compared to courts in the first quartile. Nevertheless the coefficient on the fourth quartile interaction term is insignificant.

The partisan orientation of the local population may influence participation and directly or indirectly condition courts' responses to the protest. To assess whether this is the case, I estimate Eq. (1) separately for courts located in Democratic and Republican majority areas. To approximate the partisan leaning of the local population, I calculate the share of votes cast for the Democratic and Republican candidates in the 2004 Presidential election, in counties within a 50 mile radius from the court's base city. Columns 2 and 3 of Table 6 show the estimates. The influence of the protest appears to be concentrated in Republican majority regions. The result agrees with earlier interpretations attributing the effect on the potential scrutiny afforded to decisions. In Republican majority regions, where approximately 92% of decisions are removals, judges may have more scope to align with public opinion, compared to those based in courts in Democratic majority regions.¹²

Finally, column 4 regresses removal decisions on the treatment indicator and its interactions with indicators for courts located in base cities in the second through fourth quartiles of the distance from the US-Mexico border distribution. The effect although negative, turns statistically insignificant.

4.6. Robustness

4.6.1. Alternative estimators

To assess the results' sensitivity to the estimation procedure, Fig. 4 shows results from four estimators that have been developed to aggregate heterogeneous treatment effects in settings with staggered treatment adoption (de Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2023), with and without controls.¹³ Results are generally consistent with

¹² I further estimate models interacting the treatment indicator with a dummy variable indicating judges in the lower half of the strictness distribution for courts in Republican and Democratic majority areas. The coefficient on the interaction term is insignificant in both cases.

Table 6
Heterogeneity across local characteristics.

	(1) Population	(2) Democrat	(3) Republican	(4) Distance
T^{100}	-0.020** (0.010)	-0.014 (0.012)	-0.033*** (0.009)	-0.011 (0.008)
$T^{100} \times Q_2$	0.002 (0.008)	Yes		-0.130 (0.009)
$T^{100} \times Q_3$	-0.015* (0.009)			-0.0130 (0.010)
$T^{100} \times Q_4$	-0.002 (0.013)			0.017 (0.01)
Date×Sub-exp. FE	Yes	Yes	Yes	Yes
Court×Sub-exp. FE	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes
Dependent variable mean	0.86	0.82	0.92	0.86
Observations	112,789	65,495	47,285	112,789

Note: Estimates from the stacked difference-in-differences design in Eq. (1). The dependent variable is binary, indicating a removal decision. The treatment variable T^{100} is binary, equal to one for cases heard after the first protest that occurred within 100 miles from a court's base city. All models include the full set of controls. The dependent variable is binary, indicating a removal decision. In column 1, Q_2 , Q_3 and Q_4 are binary, indicating cases heard by courts in the 2nd, 3rd and 4th quartile of the population distribution. Columns 2 and 3 use the samples of immigration courts located in Democrat and Republican majority regions, according to the 2004 presidential elections respectively. In column 4 Q_2 , Q_3 and Q_4 are binary, indicating cases heard by courts in the 2nd, 3rd and 4th quartile of the distance from the border distribution. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the stacked difference-in-differences estimates reported earlier. For the models without controls, pre-treatment coefficients are close to zero and statistically insignificant, with the exception of one coefficient for the estimators of Borusyak et al. (2023) and Sun and Abraham (2021). Post-treatment effect sizes are very similar across estimators and consistent with those shown in Fig. 2, suggesting that the probability of removals decreases by nearly 1–4 percentage points in the weeks following the protests' onset. When including controls, the Callaway and Sant'Anna (2021) effects are significant and of comparable magnitude to the stacked approach for weeks 1–4. Effects from the Borusyak et al. (2023) estimator are significant for all post-protest and two pre-protest periods. The Sun and Abraham (2021) estimator recovers significant treatment effects for the second, third and fourth post-protest weeks, while effects from the de Chaisemartin and D'Haultfoeuille (2020) approach are significant for weeks 1–4.

4.6.2. Sensitivity to distance threshold

The analysis so far relies on distinguishing between treatment and control immigration courts based on a 100-mile distance threshold. To test how this matters for the results I start by shrinking the threshold to 25 and 50 miles. Table BT.8 and Fig. BF.9 in the Appendix show the corresponding estimates from Eqs. (1) and (2) respectively. Estimates are comparable to the baseline results in Table 2. Setting the threshold for treatment at 25 or 50 miles suggests that protest exposure is related to a 1 percentage point decrease. Estimates from Eq. (2) are broadly similar to the baseline results, pointing to a small and temporary influence from protests. Columns 3 and 4 of Table BT.8 show estimates from Eq. (3), where the treatment is the inverse distance squared weighted total participation and sum of events respectively. The probability of removal in courts in the third quartile of the participation distribution is about 2 percentage points lower, relative to courts in the first quartile.¹⁴

¹³ Tables BT.6 and Table BT.7 show the exact estimates.

¹⁴ These estimates are also illustrated in Fig. BF.10.

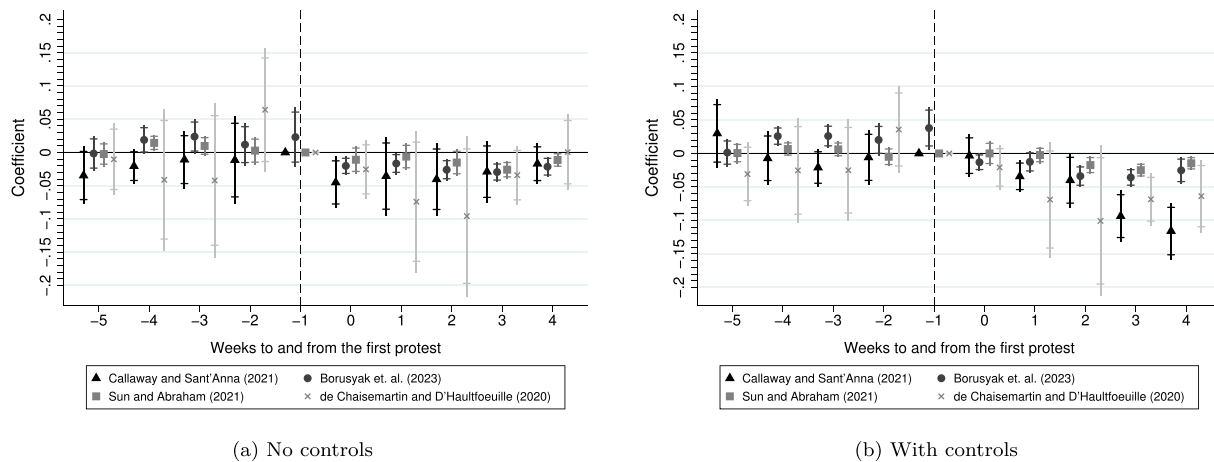


Fig. 4. Effect estimates from four alternative estimators: (i) Callaway and Sant'Anna (2021) (triangle), (ii) Borusyak et al. (2023) (circle), (iii) Sun and Abraham (2021) (square), and (iv) de Chaisemartin and D'Haultfoeulle (2020) (cross). The dependent variable is binary, indicating a removal decision. Bars and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

Table BT.9 shows estimates from Eq. (3) to assess whether the overall effect of the protest varies with distance, interacting the post-protest indicator with participation (in hundreds of thousands) and events within 25 – 50, 50 – 100, 100 – 200 and over 200 miles from the court's base city. Estimates suggest that the effect does not exhibit a clear monotonic distance decay pattern: An additional 100,000 protestors within 25 – 50 miles reduces the probability of removal by 0.011 relative to the omitted band, while the 100 – 200 and 200+ bands show similar negative changes. In contrast, the corresponding interactions when exposure is approximated by the count of protest events are small and statistically insignificant (column 2).

Fig. BF.14 shows estimates from Eq. (1) when iteratively omitting a single court. Results are comparable to the baseline and stable suggesting that findings are not driven by a single court.

4.6.3. Sensitivity to the choice of proceedings

The main analysis accounts only for the first proceeding on which the judge reached a decision. Allowing multiple proceedings per case delivers estimates similar to the baseline (column 1 of Table BT.10 and Fig. BF.11).

Before 2006 non-citizens could not be granted permanent resident status if they were “arriving non-citizens” and “in removal proceedings”. While this regulatory bar was rescinded by the United States Citizenship and Immigration Services (USCIS) and EOIR in 2006 (NILA, 2022), applicants were still in danger of removal, if they received the final removal order before the USCIS decided on their relief application. It is possible then, that during the transitory period that spans the time examined here, immigration judges terminated cases to allow undocumented immigrants to seek relief with the USCIS. If this were the case, reported results could, at least in part, be due to a policy change unrelated to the protests. To assess whether this is the case, I estimate the baseline specifications excluding terminations. Estimates in Table BT.10 and Fig. BF.11 are comparable to those reported earlier, suggesting that the probability of removals decreases by 1.9 percentage points.

The main analysis excludes administrative closures as until 2012 immigration judges could not grant an administrative closure if either party were opposed (ACLU, 2022). Including administrative closures does not qualitatively change the result, but lowers its magnitude to 2 percentage points (column 3 of Table BT.10).

Protests took place in waves. The first wave comprised marches in mid February to late March, the second in early April, and the third on the first of May (Wang et al., 2011). Some of the second and third wave marches occurred in cities that had already experienced a protest.

In other areas, the second and third waves marked the first occurrence of a march. It can be argued that first occurrences in later waves were anticipated, plausibly influencing the timing of the hearings or biasing decisions. To address this possibility I show estimates when focusing on protests taking place in February and March, repeating the stacked DiD analysis using the February-March sub-experiments. Estimates are similar to the baseline (column 4 in Table BT.10 and Fig. BF.11).

4.6.4. Accounting for judges hearing cases in multiple courts

Immigration judges may hear cases in more than one court. To account for this and test whether it impacts results, I first repeat the baseline analysis assigning the treatment according to the immigration judge's home court. In this case T^{100} is binary, indicating hearings after the first protest occurring within 100 miles of the court handling the majority of cases the judge heard in 2005 and 2006. Estimates in column 1 of Table BT.11 and Fig. BF.12 are similar to the baseline, suggesting that the probability of removals is lower about 2 percentage points after the start of the protest period, while the effect appears to dissipate by week 4. I also repeat the analysis when removing cases heard by judges not in their home court. Estimates are again comparable to the baseline (column 2).

4.6.5. By date

I repeat the analysis defining treatment status according to the exact date of the first protest rather than the week of the protest. To this end, I construct separate datasets for each cohort of courts that were treated on date s , this time keeping observations in a 60-day event window $[s - 30, s + 29]$. I then stack cohort datasets and estimate Eqs. (1) and (2). Estimates in Table BT.12 are comparable to those shown earlier: the probability of removals is about 2.6 percentage points lower for cases heard in the 30 days following the start of the protest. At the same time Fig. BF.13 suggests that the effect is concentrated across days 4–28 following the protests.

4.6.6. Placebo tests

I test the relationship between the probability of removal and a series of placebo protests. I assume a hypothetical protest period beginning on 14 February and concluding on 1 May for each year spanning from 2007 to 2019. For every individual year within this range, I compile stacked data sets transposing the cohort treatment dates, and estimate Eqs. (1) and (2). Fig. 5 shows the estimated influence of the 2006 immigration reform protests for each of the placebo tests from Eq. (1). All placebo estimates are in absolute value smaller than the true effect, the majority is very close to zero, while only one estimate is statistically significant in

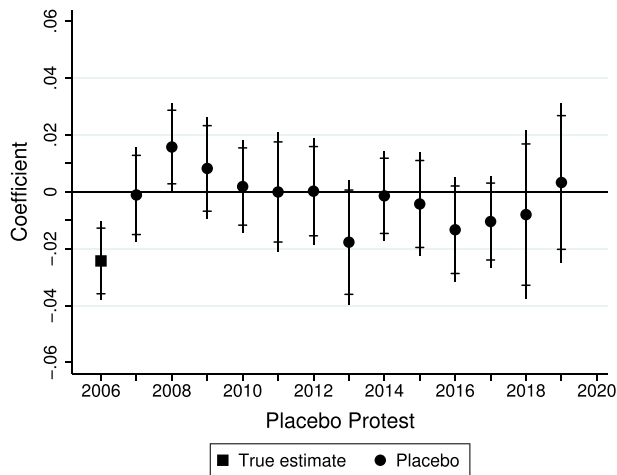


Fig. 5. Placebo estimates. Each point represents an estimate from Eq. (1) testing for the effect of a hypothetical protest occurring between 14 February and 1 May each year. All specifications include the full set of covariates.

conventional levels. Fig. BF.15 in the Appendix shows the corresponding estimates from Eq. (2) for every year in 2007–2019. With very few exceptions, estimates on post-treatment coefficients are close to zero and statistically insignificant.

5. Discussion and conclusion

Social movements frequently employ forms of collective action including protests, demonstrations and marches. Do these modes of activism influence outcomes close to the activists' objectives? A growing literature demonstrates how protests can impact the public's attitudes and behaviors (Branton et al., 2015; Xu and Guo, 2023), and legislative decision-making (King et al., 2007; Madestam et al., 2013; Soule et al., 2006). Here, I diverge from existing work on policy formation, to study the influence of protests on the application of the law. To this end, I focus on the outcomes of removal proceedings in US immigration courts around the time of the 2006 protests against the immigration reform proposed by the Border Protection, Anti-terrorism, and Illegal Immigration Control Act. Comparing removal decisions in immigration courts before and after the protests, I find some evidence that the protests had a small and temporary effect on removal outcomes: the probability of removals decreases by approximately 2 percentage points for up to 3 weeks after the onset of local xprotests, corresponding to approximately 433 fewer removals in the post protest period out of a total of over 18,000.

The results suggest that protests may have an immediate albeit small and temporary influence on court outcomes, even without producing a corresponding change in the law, policy, or institutions. This can be plausibly attributed to protests' ability to increase the salience of contentious issues, and shift public attitudes and behaviors. Tilly (1999) argues that the strength of social movements is proportional to their size, their commitment, and the unity of their messaging.¹⁵ Assuming that the strength of a social movement can be approximated by its impact on outcomes, results presented here can be used to comment on this hypothesis. I find contradictory evidence on the influence that the size of the protests as measured by the number of participants has on

outcomes. This relationship is not linear as implied by Tilly (1999), instead weakening as participation increases, and even turning positive for some specifications. On the other hand I do not find evidence that commitment as approximated by the frequency of protests influences outcomes. A plausible explanation for this pattern may be sought in protest-related disruption that increases with their size and each additional march, raising objections against the practices employed by activists (Xu and Guo, 2023; Wouters, 2019; Ketchley and El-Rayyes, 2021). The 2006 protests projected their message and demands with unity, further strengthened by the large participation of individuals with Latino descent. Nevertheless, estimates suggest that while protests decreased the probability of removals for Latinos, greater participation attenuated the effect, suggesting that disruption and institutional hardening might counteract the initial impact.

While the result is generally robust to various perturbations of assumptions, it is important that it is caveated. For instance, the non-responsiveness of removals in the baseline specification when focusing on cases concerning Latinos may be indicative of underlying violations of the exclusion restriction. At the same time, without data to approximate judges' or community ties, it is not possible to attribute any observed effects of increased accountability to local immigrant communities. The identification approach itself is better suited to smaller, localised protests that may have impacted their immediate environs, rather than larger protests that had country-wide impact. Furthermore, results assessing the overall, medium to long-term influence of the aggregate participation and marches are not stable across specifications.

Findings allude to a heterogeneous impact of protest characteristics across agents with different incentives and mandates. While re-election minded legislators may be sensitive to large and frequent protests, this does not appear to be the case with officials tasked with implementing the law. Overall, while results suggest that exposure to protests may influence judges' interpretation of evidence and judgments as suggested by the large literature on the extralegal determinants of judicial outcomes (Segal and Spaeth, 2002; Epstein and Jacoby, 2010), the small magnitude and transient nature of the effect are encouraging for the stability of judicial decision-making.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Building the dataset

The EOIR database is delivered in a series of.csv files. To construct the final dataset I use information contained in the following files: "A_tblCase.csv", "A_TblCaseIdentifier.csv", "B_tblProceeding.csv", and "tbl_schedule.csv". Table "A_tblCase" contains case information with each case uniquely identified. Table "B_tblProceeding" contains information on (possibly multiple) proceedings for each case, while "tbl_schedule" contains hearing-specific information (possibly multiple hearings per proceeding). To construct the final dataset I employ the following steps:

1. I merge *A_tblCase* and *B_tblProceeding* using the unique case identifier ("idncase"). I discard cases that do not appear on the proceedings table and proceedings that do not appear in the case table (i.e., observations that are not matched across the two tables).
2. To identify stipulated removals and discard them from the dataset I use information in table *A_TblCaseIdentifier*. For stipulated removals, variable "case_id" of *A_TblCaseIdentifier* takes values "SR" and "SD". I use the unique case identifier ("idncase"), to merge

¹⁵ Tilly (1999) also refers to a fourth factor, "worthiness" interpreted as their credibility as it manifests through the endorsement of moral authorities (Wouters and Walgrave, 2017).

- table *A_tblCaseIdentifier* on the combined table produced by step 1, and remove cases where “*case_id*” equals “SR” or “SD”.
- Table *tbl_schedule* contains hearings information by case and proceeding. I add the hearings information merging table *tbl_schedule* using the unique case and proceeding identifiers.
 - Information on case type is reported in variable “*case_id*” of table *A_tblCase*. I keep observations coded as “REM” (Removal), “DEP” (Deportation) and “EXC” (Exclusion). I remove observations coded “AOC” (Asylum only Case), “CDR” (Continued Detention Review), “CFR” (Credible Fear Review), “CSR” (Claimed Status Review), “DCC” (Departure Control), “DDC” (DD Appeal), “NAC” (NACARA Adjustment), “REC” (Rescission), “RFR” (Reasonable Fear Review), and “WHO” (Withholding Only).
 - I retain hearings under immigration courts that were in operation between 2006 (the time of interest) and 2023 (the time of the EOIR data release used here). I remove cases and hearings under immigration courts that were shut down between 2006 and 2023. I also remove cases in courts that appear operational in 2006 but in fact were not. These appear because EOIR transfers cases heard by immigration courts that were closed or merged to other courts. The main results are similar when repeating the analysis using information from the main hearing location of each base court.
 - I remove proceedings that do not reflect judge decisions (Eagly, 2015). I use information reported in variables “*dec_code*” and “*other_comp*” in table *B_tblProceeding*, along with information in variable “*case_id*” of table *A_tblCase*. Variable “*dec_code*” reports the decision of the immigration judge, depending on the case type. Variable “*other_comp*” takes values when there is an administrative decision. For the main analysis I remove decisions with “*other_comp*” equal to “A” (Administrative Closing - Other), “B” (Administrative closing - Failure to Appear), “C” (Change of Venue), “F” (Failure to Prosecute (DHS cases only)), “H” (Haitian), “O” (Other Administrative Completion), “P” (Temporary Protected Status), “T” (Transfer), “X” (Zero Bond), and “Y” (Prosecutorial Discretion - Admin Close); and those with “*dec_code*” equal to “I” (Prosecutorial Discretion-Terminated), “J” (Jurisdiction Transferred to the BIA), or “O” (Other). Category “U” (Dismissed by IJ) starts appearing in the data after 2019, so it is not accounted for in the present analysis. In robustness tests, I show estimates including administrative closures (decisions marked with “*other_comp*” equal to “A”).
 - To construct the binary dependent variable, I use information from variables “*dec_code*” table *B_tblProceeding*. I set the dependent variable equal to 1 when “*case_type*” is “RMV” or “EXC” or “DEP”, and “*dec_code*” is “X” (Remove), or “E” (Exclude), or “D” (Deport), or “V” (Voluntary Departure). I set the dependent variable equal to zero when “*case_type*” is “RMV” or “EXC” or “DEP”, and “*dec_code*” is “A” (Admit), “R” (Relief Granted), “T” (Terminated), or “Q” (Final Grant of EOIR 42B/SUSP). In robustness tests I also show estimates when removing Terminations.
 - To select the earliest proceeding with a decision, I use information on the hearings dates. The EOIR database includes multiple date variables at the case, proceeding and hearing levels. Variable “*latest_hearing*” in table *A_tblCase* reports the last hearing for the entire case, that may include later proceedings. Variable “*comp_date*” in table *B_tblProceeding* reports the date the immigration judge rendered a decision on the proceeding. Variable “*adj_date*” reports the date of the planned hearing. To identify the earliest decision for each case, I first rank all proceedings by their completion date. I keep the proceeding with the earliest completion date. I take the latest hearing date (“*adj_date*”) as the date of the hearing. Results are broadly similar when using the last hearing of a case.

Appendix B. Additional results

B.1. Courts by cohort of treatment

Table BT.1

Courts by cohort of treatment.

Cohort	Courts
1	Baltimore, Elizabeth, Newark, New York, Philadelphia, Arlington
2	Portland
3	Chicago
4	Los Angeles, San Francisco
5	Atlanta, Dallas, Denver, Detroit, Eloy, Florence Hartford, Houston, Phoenix, Ulster
6	Boston, El Paso, El Paso - Detained, Imperial, Las Vegas, San Diego, Tucson
7	Fort Snelling, Krome, Miami, Orlando, Salt Lake City
8	Batavia, Buffalo, Harlingen, Memphis, Port Isabel, Seattle, San Antonio
9	New Orleans
No protest	Oakdale

Note: The table shows the courts belonging in each treatment cohort.

B.2. Protests by location

Table BT.2

Count of protests by location.

Protest Count	Location
9	Los Angeles
7	San Diego
5	Dallas, El Paso, New York, Phoenix, Tucson
4	Bakersfield, Denver, Detroit, Fresno, Houston, Las Vegas, Watsonville
3	Atlanta, Boston, Chicago, Miami, Philadelphia, Sacramento, Salem, San Francisco Washington
2	Albuquerque, Anchorage, Aurora, Birmingham, Chapel Hill, Charlotte, Chattanooga, Concord, Costa Mesa, Eugene, Farmersville, Fort Lauderdale, Fort Myers, Hartford, Kansas City, Knoxville, Louisville, Madison, Milwaukee, Oakland, Orlando, Portland OR, Riverside, San Antonio, San Jose, Santa Cruz, Santa Fe, Seattle, Trenton, Wilmington NC, Yakima
1	Albertville, Allentown, Arlington, Austin, Belle Glade, Berkeley, Birdville, Blacksburg, Boise, Boulder, Burlington, Charleston, Cicero, Cleveland, Colorado Springs, Columbia NC, Columbia SC, Columbus, DeKalb, Des Moines, Durham, Elgin, Fitchburg, Fort Wayne, Fort Worth, Garden City, Georgetown, Goshen, Grand Junction, Grand Rapids, Greenville, Haltom City, Harlingen, Harrisburg, Hemet, Homestead, Hood River, Huntersville, Huntington Park, Indianapolis, Jackson, Joliet, Jonesborough, Kennewick and Pasco, Kensington, Kern County, Lake Worth, Lenoir Hickory, Lexington, Lincoln, Little Rock, Los Fresnos, Lumberton, Madera, Medford Rogue Valley, Memphis, Monterey, Morehead, Moreno Valley, Nashville, New Heaven, New Orleans, Norfolk, Northern Virginia, Omaha, Pajaro, Palm Springs, Pensacola, Petaluma, Pittsburgh, Plant City, Portland ME, Providence, Raleigh, Richmond CA, Richmond VA, Rochester, Salinas, Salisbury, Salt Lake City, San Bernardino, San Ysidro, Santa Ana, Santa Barbara, Santa Rosa, Sarasota, Siler City, Sioux Falls, Smithfield, South Bend, South Sioux City, St. Louis, St. Paul, Stamford, Stockton, Storm Lake, Tampa, Tarran County, Toledo, Tracy, Tulsa, Tyler, Union City, Urbana Champaign, Walla Walla, West Palm Beach, Wichita, Wilmington DE

Note: The table shows the count of protests reported in Bada et al. (2006a) by location.

B.3. Participation and events over time

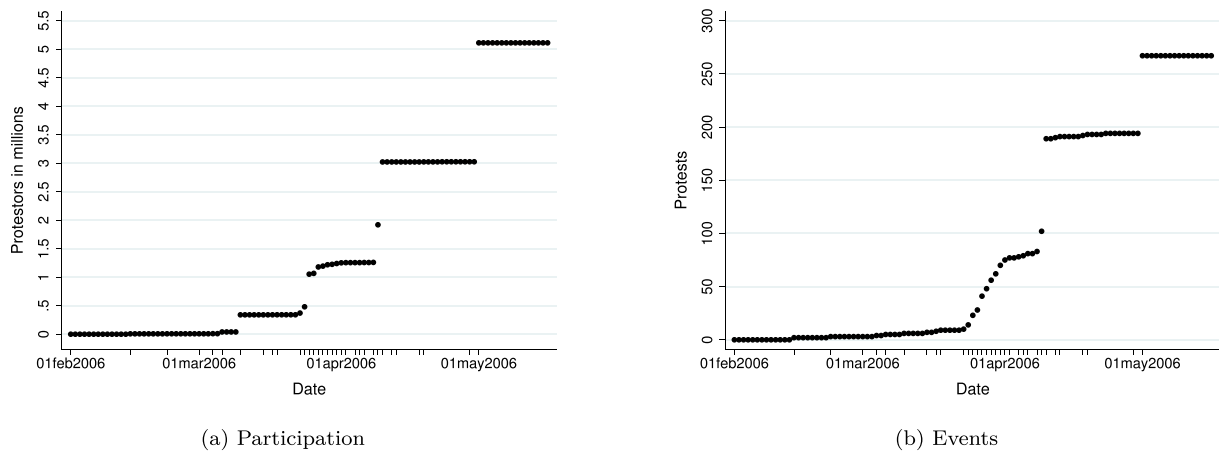


Fig. BF.1. Cumulative distribution of protest participation and protest events over time. Ticks on the horizontal axis indicate dates when marches took place according to the data in Bada et al. (2006a).

B.4. Case observable characteristics by treatment status

Table BT.3

Means for treated and control groups for the stacked dataset.

Variable	Control	Treated	pvalue
Sub-experiment 1			
Removal	0.86	0.78	0.00
Latino	0.81	0.74	0.00
Detained	0.16	0.20	0.00
Criminal indictment	0.09	0.13	0.00
In-person hearing	0.95	0.93	0.00
Representation	0.22	0.33	0.00

Note: Each of the “Control” and “Treated” columns reports the mean of the corresponding variable for the control and treated groups respectively. Columns p-value report the corresponding p-value from a test of means equality.

B.5. The evolution of removal decisions across courts in the months before and after the protests

The evolution of the share of removals in the months leading to the protest period is similar across courts with different protest exposure. It is worth noting that the probability of removals in the pre-protest period appears inversely related to eventual protest intensity: courts in the first quartile of exposure have the highest share of removals, while removals are lowest in courts in the fourth quartile of intensity metrics. This pattern appears to be a result of court-specific characteristics. Courts in lower quartiles are more likely to specialize in detained cases, and be located in areas with relatively lower populations. The combined effect of a higher share of removals for detained cases and smaller protests in less populated areas gives rise to the observed inverse relationship. This is unlikely to influence estimates as courts’ fixed characteristics are accounted for in the difference-in-differences estimates. Examining the evolution of removals in the post-protest period can contribute some insight into the potential influence of protest exposure on outcomes. The share of removals is unchanged for courts in the highest quartile of the

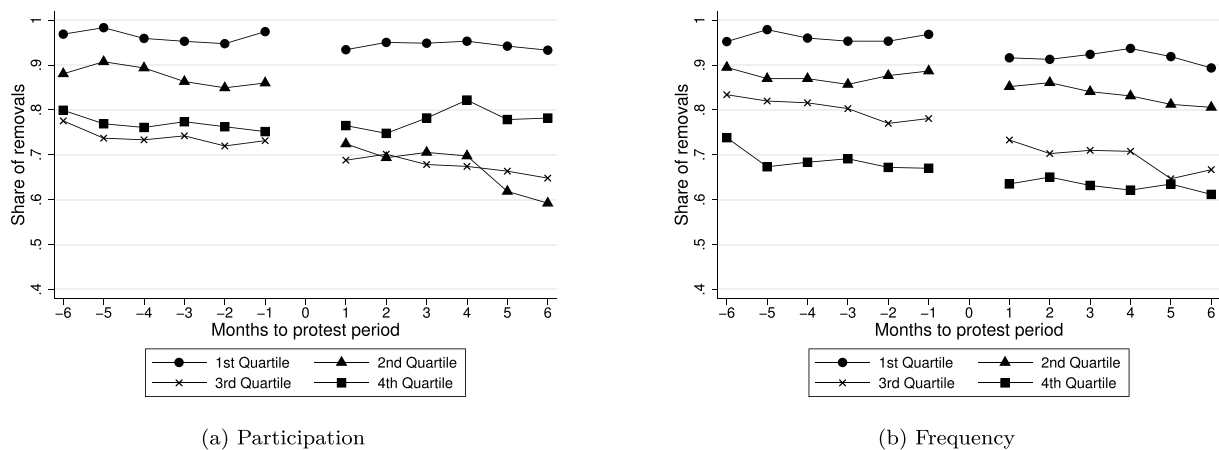


Fig. BF.2. The evolution of removal decisions. The figure shows the share of removal decisions in the 30 days leading up to and following the end of the protest period for immigration courts by quartiles of the reported participation distribution (a), and the frequency distribution (b). Negative values on the horizontal axes indicate the months leading up to the start of the protests (February 2006). Positive values measure the months since the end of the protests (May 2006).

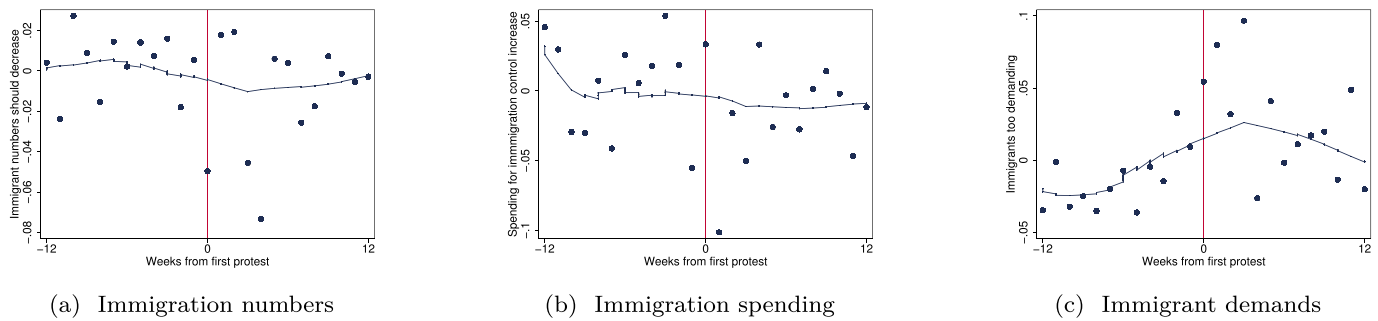


Fig. BF.3. The figure shows the evolution of attitudes towards immigrants and immigration over time. Points are the average residuals of a regression of each attitude on educational attainment, income, sex, employment status, and a set of state dummies. Vertical lines mark the time of the first protest.

attendance distribution, while there is a small decrease in the share of removals for courts in the first quartile. On the other hand, removals appear to be substantially lower for courts in the second and third quartiles, pointing to a potential non-linear relationship between removals and protest intensity.¹⁶

B.6. The evolution of immigration salience around the time of the protests

To provide some further insight into whether attitudes towards immigration changed in the time leading up to the protests, I use data from the 2006 Social Capital Community Survey (SCCS) (Putnam, 2022). The 2006 wave of the SCCS was fielded between January and August, and contains information from over 15,000 individuals residing across the US. The finest level of spatial resolution is the state. Along with standard socio-demographic data, the SCCS collected information on respondents' attitudes and opinions regarding immigrants and immigration. Respondents were asked: (A) "Would you like to see spending increased or decreased (for) tightening border security to prevent illegal immigration? (1) Decreased, (2) Neither increased nor decreased, (3) Increased"; (B) "Do you think the number of immigrants to America nowadays should be (1) Increased a lot, (2) Increased a little, (3) Remain the same, (4) Reduced a little, or (5) Reduced a lot?"; (C) "Immigrants are getting too demanding in their push for equal rights. (1) Disagree strongly, (2) Disagree somewhat, (3) Neither agree nor disagree, (4) Agree somewhat, (5) Agree strongly". I define a binary "Immigration spending" variable, equal to 1 for those stating they wish to see spending for tightening border security "Increased", a binary "Immigrant numbers" variable equal to 1 for those stating that the number of immigrants should be "Reduced a lot" or "Reduced a little", and a binary variable indicating those that "Agree" and "Somewhat agree" that "Immigrants are getting too demanding".

Fig. BF.3 shows the evolution of attitudes on immigration in relative time, in the 12 weeks before and after the first protest that occurred in a state. The pattern of change in attitudes over time does not suggest the public were becoming more or less pro-immigrant before the onset of the protest period. There is no discernible trend in attitudes towards immigrant numbers or immigration spending. At the same time, the share of those agreeing that immigrant numbers should decrease (Fig. BF.3a) and the share of those agreeing with increasing spending

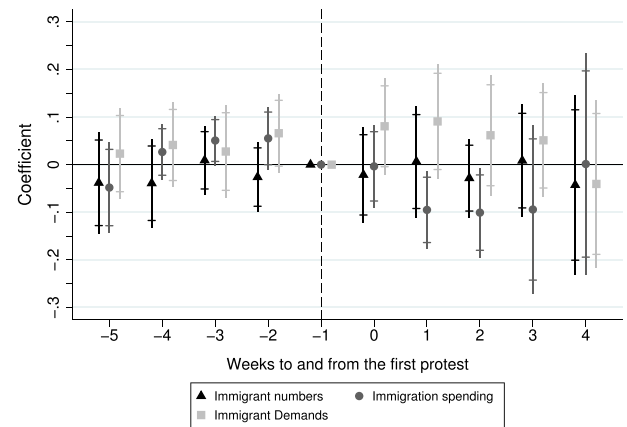


Fig. BF.4. Estimates from the stacked difference-in-differences design described in Eq. (2) for each of the 3 attitudinal outcomes. Bars and ticks show 95% and 90% confidence intervals respectively. Controls include age categories, sex, educational attainment, employment status, income categories, and a set of state-of residence dummies.

for immigration control (Fig. BF.3b) remain roughly similar before and after the first protest. Finally the belief that "Immigrants are getting too demanding" trends upward in the weeks before and after the protest (Fig. BF.3c).

To further assess the evolution of attitudes over time, test for pre-trends, and evaluate the influence of the protests on immigrant attitudes, I estimate stacked difference-in-differences designs as the one described in Eq. (2). I create separate datasets for each cohort of states that experienced their first protest in week s , keeping observations in a 10-week event window $[s - 5, s + 4]$ (i.e., individuals responding in the 5 weeks before and the 4 weeks after the week of the first protest for cohort s). To each of these cohort-specific datasets, I add the respondents who were surveyed on the same calendar dates in never-treated states, and in not-yet-treated states, and then stack the cohort-specific datasets in relative time. Models include controls for age categories, income categories, employment status, educational attainment and sex. Fig. BF.4 shows coefficient estimates. There does not appear to be systematic evidence of a change in attitudes towards immigrants or immigration in the time leading up to the protest, as all but one of the coefficients on the lead terms are statistically insignificant. On the other hand, there is some evidence of attitudes changing in the post-protest period as the probability that respondents support increasing spending for immigration control decreases by 9 and 10 percentage points in the first and second weeks after the protests' onset respectively. It should be noted though that given the state-level geographic resolution of the survey data, Fig. BF.4 provides evidence on pre-trends across treated and untreated states, but cannot rule out differential pre-trends within states, as in the main analysis.

¹⁶ It is important to note however that Callaway et al. (2021) show that when the treatment is multivalued the parallel trends assumption suffices only for identifying the average treatment effect on the treated for a given dose level. In the present case, this suggests that identifying average treatment effect of some exposure intensity m requires the evolution of removal outcomes in courts that received m to be similar to courts that would have received m had they been treated. To identify the average treatment effect across all units, Callaway et al. (2021) propose a stronger parallel trends assumption requiring the average change in outcomes over time across units had they received dose m , to be the same as the average change in outcomes for those units that did in fact receive m .



Fig. BF.5. Figures (a)-(j) show the distribution of Google searches for "Immigration" across states, over time, and by the first protest. Figure (k) shows the distribution of Google searches from states with no recorded protests. Data from Google Trends.

B.7. Pre-trends for treatment intensity models

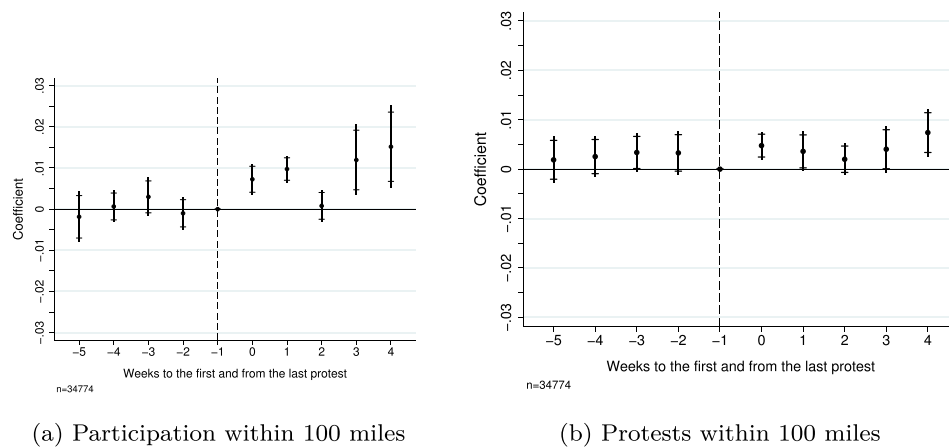


Fig. BF.6. Estimates of the β coefficients from $Y_{itc} = \alpha_c + \gamma_t + \sum_{w=-5, w \neq -1}^4 \beta_w I_{tc}^d \times M_c^d + \sum_{w=-5, w \neq -1}^4 \delta_w I_{tc}^d + X_t + \epsilon_{ict}$. The dependent variable is binary, equal to one for a removal decision. Fig. BF.6(a) and (b) focus on the count of participants and protest events within 100 miles of the immigration court's base city. All models include the full set of controls. Dots are point estimates of the difference in the probability of the outcome in week $\{-5, \dots, -2, 0, \dots, 4\}$ relative to week $\{-1\}$. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

B.8. Protest participation and event intensity

Table BT.4

Removals and overall protest intensity.

	(1)	(2)
$P \times Q_2$	-0.015 (0.013)	
$P \times Q_3$	0.009 (0.011)	
$P \times Q_4$	0.035 (0.022)	
$P \times Q_2$		0.001 (0.013)
$P \times Q_3$		0.016 (0.022)
$P \times Q_4$		0.020 (0.014)
Date $\times$ Sub-exp. FE	Yes	Yes
Court $\times$ Sub-exp. FE	Yes	Yes
Case Controls	Yes	Yes
Observations	34,774	34,774

Note: Estimates from Eq. (3). The dependent variable is binary, indicating a removal decision. P indicates cases decided after the protest period. In column 1, Q_2 , Q_3 and Q_4 are binary, indicating cases heard by courts in the 2nd, 3rd and 4th quartile of protest intensity distribution, where intensity is defined as the ratio of protest participants within 100 miles over the population within 100 miles. In Column 2 Q_2 , Q_3 and Q_4 are binary, indicating cases heard by courts in the 2nd, 3rd and 4th quartile of event intensity distribution, where intensity is defined as the ratio of protest events within 100 miles over the population within 100 miles. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.9. Heterogeneity by case characteristics

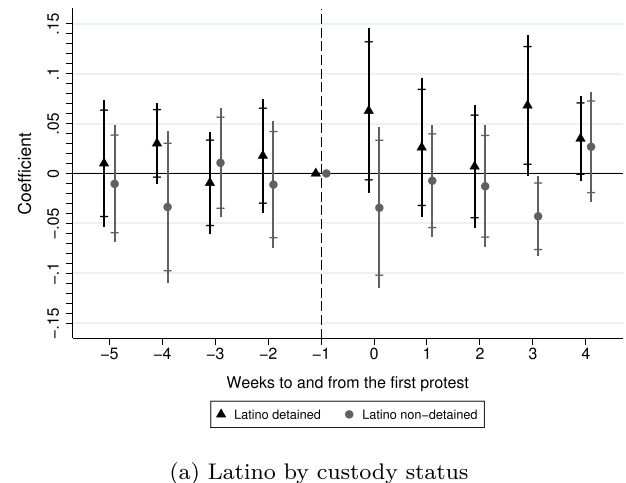


Fig. BF.7. Estimates from the stacked event study specification in Eq. (2) for the samples of Latino detained and non-detained cases. The dependent variable is binary, equal to one for a removal decision. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

Table BT.5
Protests and the outcome of removal proceedings.

	(1)	(2)
T^{100}	-0.015* (0.009)	-0.015* (0.009)
Date×Sub-exp. FE	Yes	Yes
Court×Sub-exp. FE	Yes	Yes
Case Controls	Yes	Yes
Observations	92,661	81,912

Note: Estimates from the stacked difference-in-differences design in 1. The dependent variable is binary, indicating a removal decision. T^{100} is binary equal to one for cases heard in the 30 days following the first protest. Columns 1 and 2 use the sample of cases concerning Latino individuals, adding court by immigrant nationality effects. Column 2 uses the sample of Latino individuals excluding cases in Miami and Orlando. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.10. Heterogeneity across judge characteristics

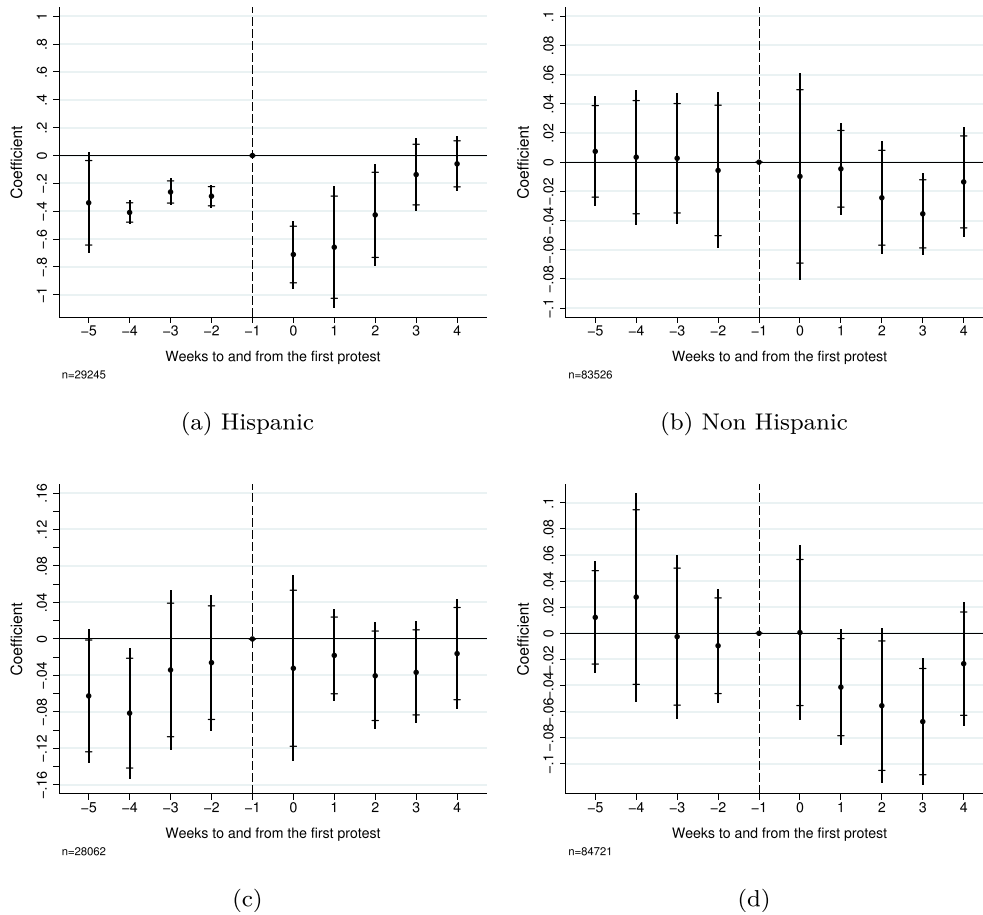


Fig. BF.8. Estimates from Eq. (3). The dependent variable is binary, equal to one for a removal decision. Fig. BF.8(a) and (b) focus on cases heard by Hispanic and non-Hispanic immigration judges respectively. Figs. BF.8(c) and (d) use information from cases heard by relatively lenient and strict immigration judges. All models include the full set of controls. Dots are point estimates of the difference in the probability of the outcome in week $\{-5, \dots, -2, 0, \dots, 4\}$ relative to week $\{-1\}$. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

B.11. Alternative estimators - detailed results

Table BT.6

Alternative estimators, no controls.

	(1)	(2)	(3)	(4)
T_{-5}	-0.035 (0.022)	-0.002* (0.013)	-0.002 (0.009)	-0.010 (0.028)
T_{-4}	-0.021 (0.013)	0.019* (0.011)	0.014** (0.006)	-0.041 (0.054)
T_{-3}	-0.011 (0.022)	0.024 (0.013)	0.010 (0.008)	-0.042 (0.059)
T_{-2}	-0.011 (0.034)	0.012 (0.016)	0.003 (0.011)	0.064 (0.047)
T_{-1}	0.000 (0.000)	0.023 (0.023)	0.000 (0.000)	0.000 (0.000)
T_0	-0.045** (0.020)	-0.020*** (0.007)	-0.011 (0.011)	-0.025 (0.022)
T_{+1}	-0.036 (0.030)	-0.017** (0.008)	-0.006 (0.010)	-0.074 (0.054)
T_{+2}	-0.040 (0.028)	-0.026*** (0.008)	-0.015 (0.010)	-0.096 (0.061)
T_{+3}	-0.029 (0.024)	-0.029*** (0.007)	-0.026*** (0.007)	-0.034 (0.023)
T_{+4}	-0.017 (0.015)	-0.021 (0.008)	-0.011 (0.006)	0.000 (0.029)

Note: Column 1: Callaway and Sant'Anna (2021); Column 2: Borusyak et. al. (2023) Column 3: Sun and Abraham (2021); Column 4: de Chaisemartin and D'Haultfoeuille (2020). Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.12. Sensitivity to distance threshold

Table BT.8

Sensitivity to the choice of distance thresholds.

	(1)	(2)	(3)	(4)
T^{25}	-0.009** (0.004)			
T^{50}		-0.008* (0.004)		
$Post \times Q_2$			-0.009 (0.012)	-0.003 (0.014)
$Post \times Q_3$			-0.021* (0.011)	-0.004 (0.014)
$Post \times Q_4$			0.014 (0.021)	-0.001 (0.023)
Date $\times$ Sub-exp. FE	Yes	Yes		
Court $\times$ Sub-exp. FE	Yes	Yes		
Date FE			Yes	Yes
Court FE			Yes	Yes
Case Controls	Yes	Yes	Yes	Yes
Dependent variable mean	0.86	0.86	0.81	0.81
Observations	142,402	134,190	34,774	34,774

Note: Columns 1 and 2 show estimates from the stacked difference-in-differences design described in Eq. (1). Columns 3 and 4 show estimates from Eq. (3). Q2-Q4 indicate cases heard in immigration courts at the 2nd to 4th quartile of the inverse distance squared weighted participation and events distribution respectively. In all cases the dependent variable is binary, indicating a removal decision. The set of case controls include immigration judge effects, non-citizen nationality effects, custody status effects and a binary variable indicating whether the respondent has legal representation. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table BT.7

Alternative estimators, with controls.

	(1)	(2)	(3)	(4)
T_{-5}	0.030 (0.026)	0.001** (0.011)	0.001 (0.008)	-0.031 (0.024)
T_{-4}	-0.007 (0.020)	0.026*** (0.007)	0.007 (0.006)	-0.025 (0.040)
T_{-3}	-0.021 (0.014)	0.026*** (0.009)	0.006 (0.006)	-0.025 (0.039)
T_{-2}	-0.006 (0.021)	0.020* (0.012)	-0.005 (0.007)	0.036 (0.033)
T_{-1}	0.000 (0.000)	0.038** (0.016)	0.000 (0.000)	0.000 (0.000)
T_0	-0.003 (0.016)	-0.013** (0.007)	0.000 (0.009)	-0.021 (0.017)
T_{+1}	-0.034*** (0.012)	-0.013 (0.008)	-0.002 (0.006)	-0.069 (0.044)
T_{+2}	-0.040* (0.021)	-0.034*** (0.008)	-0.017** (0.007)	-0.101* (0.057)
T_{+3}	-0.094*** (0.019)	-0.036*** (0.007)	-0.025*** (0.005)	-0.068*** (0.020)
T_{+4}	-0.116*** (0.022)	-0.025** (0.010)	-0.015*** (0.005)	-0.064*** (0.028)

Note: Standard errors clustered at the immigration court level in parentheses. Column 1: Callaway and Sant'Anna (2021); Column 2: Borusyak et. al. (2023) Column 3: Sun and Abraham (2021); Column 4: de Chaisemartin and D'Haultfoeuille (2020). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

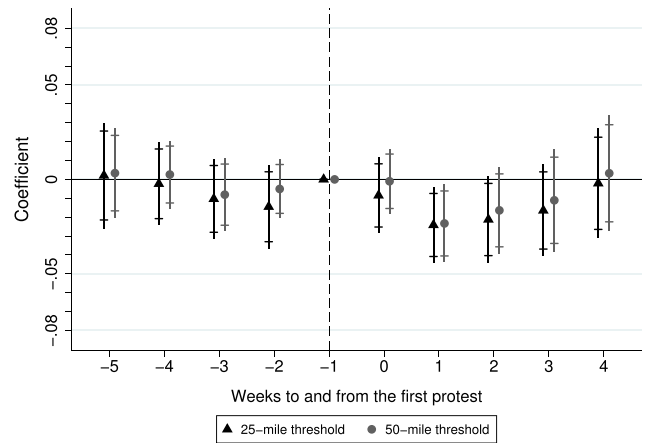
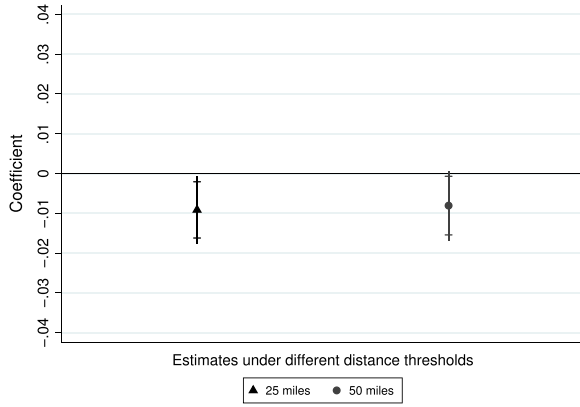
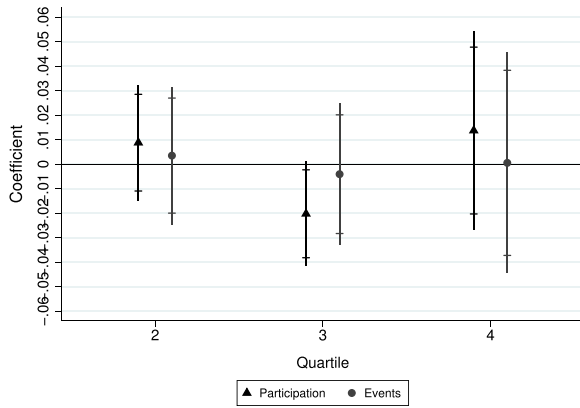


Fig. BF.9. Each marker indicates a different set of estimates from the stacked event study specification in Eq. (2). The dependent variable is binary, equal to one for a removal decision. All models include the full set of controls. Dots and triangles are point estimates of the difference in the probability of the outcome on week $w = \{-5, \dots, -2, \dots, 0, \dots, 4\}$ relative to week -1 , when the treatment threshold is set at 25 and 50 miles respectively. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.



(a) Estimates from equation 1 for 25 and 50 mile thresholds.



(b) Estimates from 3

Fig. BF.10. Visualized estimates reported in Table BT.8. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

Table BT.9

Distance and the influence of protest participation and protest events.

	(1)	(2)
$P \times PA_{25-50}$	-0.011** (0.005)	
$P \times PA_{50-100}$	0.001 (0.006)	
$P \times PA_{100-200}$	-0.009** (0.004)	
$P \times PA_{200+}$	-0.008** (0.003)	
$P \times EV_{25-50}$		-0.010 (0.008)
$P \times EV_{50-100}$		-0.004 (0.005)
$P \times EV_{100-200}$		-0.008 (0.005)
$P \times EV_{200+}$		-0.006 (0.005)
Date×Sub-exp. FE	Yes	Yes
Court×Sub-exp. FE	Yes	Yes
Case Controls		
Observations	34,774	34,774

Note: The table shows estimates from Eq. (3). The dependent variable is binary, indicating a removal decision. P is binary indicating cases decided after 1 May 2006. PA_{x-y} is the sum of protest participants (in hundreds of thousands) in all protests that occurred within $[x, y]$ miles of a court. EV_{x-y} is the sum of protest events that occurred within $[x, y]$ miles of a court. Standard errors clustered at the court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.13. Sensitivity to the choice of proceedings

Table BT.10

Sensitivity to the choice of proceedings.

	(1)	(2)	(3)	(4)
T^{100}	-0.027*** (0.008)	-0.019*** (0.005)	-0.020** (0.009)	-0.026*** (0.008)
Date×Sub-exp. FE	Yes	Yes	Yes	Yes
Court×Sub-exp. FE	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes
Dependent variable mean	0.85	0.9	0.86	0.87
Observations	117,325	107,531	113,506	95,407

Note: Estimates from the stacked difference-in-differences design described in Eq. (1). The dependent variable is binary, indicating a removal decision. In column 1 the sample includes all proceedings. In column 2 it excludes terminations. In column 3 it includes administrative closures. In column 4 it focuses on the first wave of protests. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

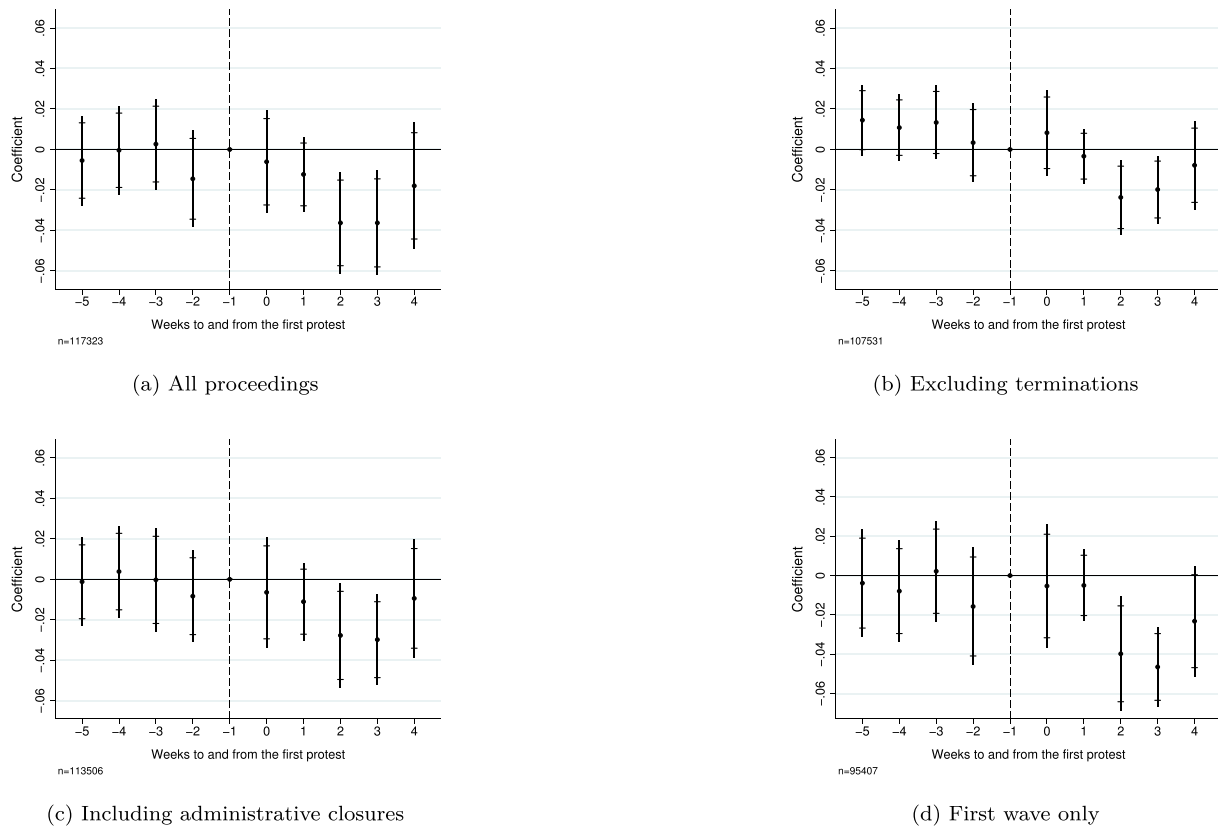


Fig. BF.11. Estimates from the stacked event study specification in Eq. (2). The dependent variable is binary, equal to one for a removal decision. The models include the full set of controls. Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

B.14. Accounting for judges hearing cases in multiple courts

Table BT.11

Protests and removal proceedings: accounting for judges hearing cases in more than one courts.

	(1)	(2)
T^{100}	-0.018*** (0.008)	-0.021*** (0.007)
Date×Sub-exp. FE	Yes	Yes
Court×Sub-exp. FE	Yes	Yes
Case Controls	Yes	Yes
Observations	108,750	99,037

Note: The table shows estimates from the stacked difference-in-differences design described in Eq. (1). Column 1 reports estimates when assigning the treatment according to immigration judges' base court. Column 2 excludes hearings by judges not in their base court. The dependent variable is binary, indicating a removal decision. The mean of the dependent variable is 0.86. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

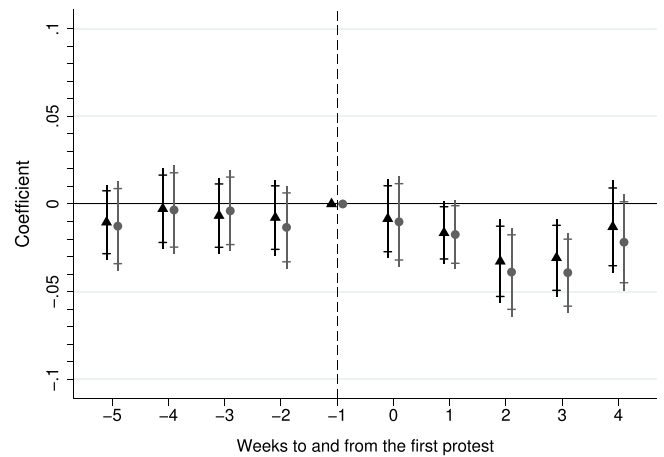


Fig. BF.12. Estimates from the stacked event study specification in 2, including the full set of controls. Estimates marked with triangles are from models assigning treatment according to the immigration judges' home court. Estimates marked with circles are after removing cases heard by immigration judges away from their home court. Standard errors are clustered at the immigration court level.

B.15. By date

Table BT.12
Protests and removal proceedings outcomes by date.

	(1)
T^{100}	-0.026*** (0.008)
Date×Sub-exp. FE	Yes
Court×Sub-exp. FE	Yes
Case Controls	Yes
Observations	103,742

Note: The table shows estimates from the stacked difference-in-differences design described in Eq. (1) including the full set of controls. The dependent variable is binary, indicating a removal decision. The mean of the dependent variable is 0.85. T^{100} is binary equal to one for cases heard in the 30 days following the first protest. Standard errors clustered at the immigration court level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

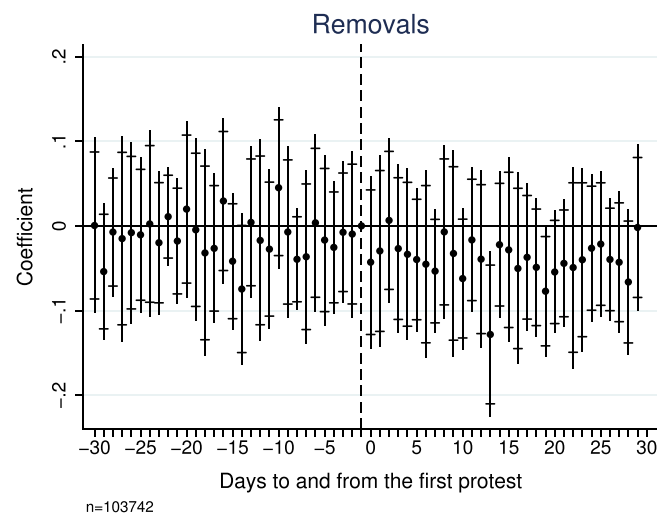


Fig. BF.13. OLS estimates from the stacked event study specification in Eq. (2), this time at the day level rather than the week level, and accounting for the full set of controls. The dependent variable is binary, equal to one for a removal decision. Dots are point estimates of the difference in the probability of the outcome on day $t = \{-30, \dots, -2, 0, \dots, 29\}$ relative to day -1 . Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

B.16. Omitting one court at the time

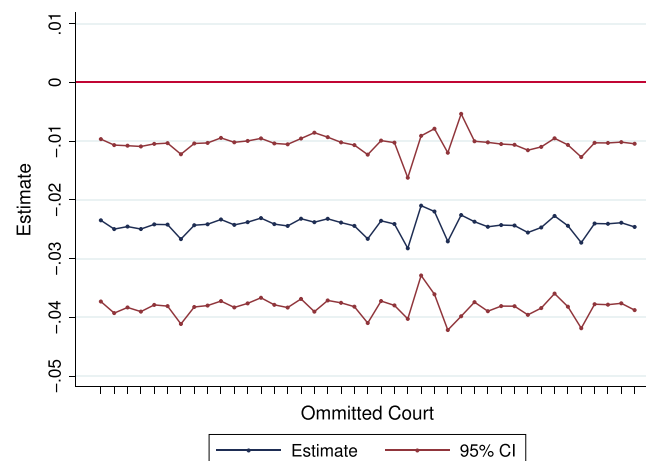


Fig. BF.14. Estimates from Eq. (1) when iteratively omitting a single immigration court. The blue line represents the estimates while the red lines show the 95% confidence interval. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

B.17. Placebos

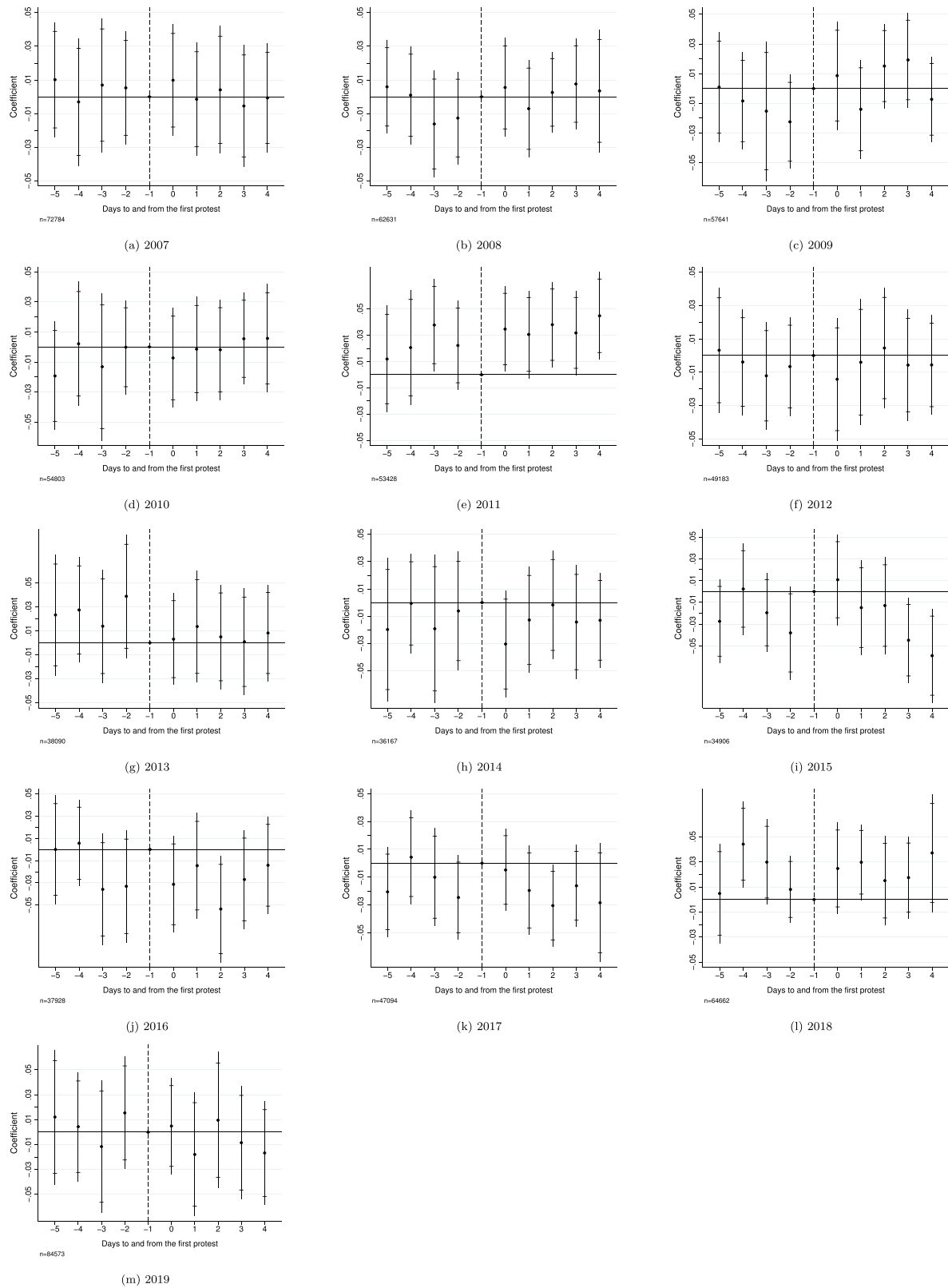


Fig. BF.15. Stacked event study estimates from placebo protests. Each figure shows estimates from the stacked difference-in-differences design in Eq. (2), assessing the effect of a hypothetical protest occurring in each year between 2007 and 2019. All models include the full set of covariates. Dots are point estimates of the difference in the probability of the outcome on week $w = \{-5, \dots, -2, \dots, 0, \dots, 4\}$ relative to week -1 . Lines and ticks are 95% and 90% confidence intervals respectively. Standard errors are clustered at the immigration court level.

Data availability

Data will be made available on request.

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