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ABSTRACT

Terrorism, Immigration and Asylum Approval*

Using the universe of individual asylum cases in the United States from 2000-2004 and a difference-in-differences research design, we test whether Sept. 11, 2001 decreased the likelihood that applicants from Muslim-majority countries were granted asylum. Our estimates suggest that the attacks resulted in a 4 percentage points decrease in the likelihood that applicants from Muslim-majority countries are granted asylum. The estimated effect is larger for applicants who share a country of origin with the Sept. 11, 2001 attackers. These effects do not differ across judge political affiliation. Our findings provide evidence that emotions affect the decisions of judges.

JEL Classification: D74, K4, K37, P48

Keywords: courts, crime, immigration, judicial decision, sentencing and terrorism

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1 Introduction

How do emotions such as anger, fear, or sadness influence peoples' decision making? An extensive body of research assesses the part extraneous information and emotions play in how people make evaluations and judgements. However, relatively little analysis has investigated the effect of catastrophic events such as Sept. 11, 2001 on decision making.

One of the founding principles of the United States is equal and fair treatment under the law. The influence of emotions and extraneous information on judges' decision making is a deviation from this principle and a miscarriage of justice. Moving from the macro to the micro, the individuals applying for asylum (asylees) are doing so in order to flee persecution from their country of origin on the grounds of race, religion, nationality, belonging to a particular social group, or political opinion. Asylees who are deported unjustly face poverty, violence, torture, and/or death. Clearly, these are high stakes decisions that require the evaluation and removal of underlying judicial prejudice.

Our empirical strategy investigates how Sept. 11, 2001 changed grant rates of asylees from Muslim-majority countries in American immigration courts. Conceptually, Islamic terrorism may affect judges' decision making through several different mechanisms. Emotions have been shown to not only color the immediate and related judgments and evaluations, but also those that are unrelated and occur much later (Lerner et al. (2015)). According to this mechanism, the Sept. 11, 2001 attacks represent a negative event and as a consequence we might expect to see lower asylum grant rates. If cases of asylees from Muslim-majority countries create emotional cues related to the attacks, we might expect the decline in asylum grant rates to be even larger for asylees from those countries.[1] It is worth emphasizing here that we are not attempting to identify the causal effect of the September 11, 2001 attacks on Muslim asylees but rather the **differential** causal effect between non-Muslim and Muslim asylees. Our proposed negative emotional cue mechanism should effect all asylees and attempts to identify the causal effect of the attacks on Muslim asylees with non-Muslims in the control group would bias estimates towards zero. We acknowledge that judges' asylum grant rates for all asylees are impacted but that the effect is more pronounced for those from Muslim-majority countries and it is this differential effect that we aim to identify.

[1] Other possible channels include judges updating their perceptions of the frequency of Islamic terrorism or terror-related changes in local economic conditions. See Section 3 for more details.

In this paper, we employ a difference-in-differences research design using the entire universe of asylum applications from January 2000–September 2004. Our difference-in-differences analysis compares asylees from Muslim-majority countries and those not from Muslim-majority countries, before and after Sept. 11, 2001. We also conduct this analysis for asylees from countries associated with the attacks (Egypt, Lebanon, United Arab Emirates, and Saudi Arabia) to provide further evidence that the effect is specific to Sept. 11, 2001. Our estimates suggest that asylees from Muslim-majority countries are about 4 percentage points less likely to receive asylum than those not from Muslim-majority countries. We also provide evidence that asylees from attack associated countries are between 6.7–13.6 percentage points less likely to receive asylum than those not from countries associated with Sept. 11, 2001. Additionally, we repeat the analysis for the March 11, 2004 attacks that occurred in Madrid, Spain. Our estimates suggest that asylees from Muslim-majority countries are also about 4 percentage points less likely to receive asylum after the attacks.

We also explore a triple-difference research design aimed at testing whether or not the differential effect of Sept. 11, 2001 on asylum grant rates for asylees from Muslim-majority countries varies by the party of the President of the United States at the time of a judge's appointment. Our results suggest that Democrat appointed judges do not respond to the attacks in a different way than do Republican appointed judges.

In order to address concerns that the composition of the applicant pool changes endogenously in response to the attacks, we restrict our sample to windows of 90, 180, 270, and 365 days around Sept. 11, 2001. We exploit evidence that the case backlogs and processing wait times are much longer than these windows. This suggests that compositional changes are unlikely to be responsible for our estimated effects. Our findings are also robust to case-level and temperature controls, and judge and city fixed effects as well as the choice of estimation method. We control for legal representation, detention status at the time of hearing, and whether the application was in response to removal proceedings.

This paper contributes to two main branches of the behavioral economics and decision theory literature. First, we expand upon the work done examining the impacts of emotional shocks on decision making by considering an emotional shock that is more broadly applicable and larger in scale. Most relevant is the work done by Beland and Brent (2018), Card and Dahl (2011) and Eren and Mocan (2018) who examine the effects of negative emotional shocks on domestic violence and juvenile sentencing,

respectively. Second, we contribute to a growing literature examining the impact of extraneous information on the decisions of judges (Danziger et al. (2011)). In this sense our work is closely related to Heyes and Saberian (2019) who examine the impact of pollution and temperature on immigration judge asylum decisions, and Philippe and Ouss (2018) who explore the impact of crime on jury decision making. Additionally, Alesina and La Ferrara (2014) investigate racial bias in capital sentencing in the U.S. using variation in sentence reversals, finding evidence of bias against minority defendants. Anwar et al. (2012) examine the impacts of racial composition of juries in Florida, finding bias in the conviction rates of black defendants when there were no black jury members. Shayo and Zussman (2011) find that Arab and Jewish small claims judges are biased in favor of their own groups, and that this bias is strongly related to the intensity of nearby terrorism in the previous year. Cohen and Yang (2019) exploit random case assignment in federal sentencing in the U.S. to find that Republican-appointed judges deliver harsher sentences to black defendants and the difference between white and black defendants grows with judge discretion. Lastly, our work is very closely related to McConnell and Rasul (2019), who examine the spillover effects of the Sept. 11, 2001 attacks on differential sentencing of hispanic and black defendants in the U.S. finding that hispanic defendants experienced worse outcomes as a consequence. Our work, however, differs from the existing literature that examines judge or jury decision making in several key ways: first, we examine the effect of a single catastrophic Islamic terrorist attack rather than changes in the intensity of terrorism or criminal activity; second, at this time in the U.S. there had been essentially no Islamic terror attacks committed and so the social division generated by these attacks is significantly less ingrained than in an ongoing conflict context; last, we are examining the decisions of immigration judges rather than those of small claims judges or juries where the decision makers and stakes of the decisions are quite different.

Previous empirical work has focused on examining the effect of terrorism on many outcomes which, although closely related, differ from ours. We contribute here by documenting the effects of terrorism on asylum approval rates for applicants from Muslim-majority countries and countries associated with the attacks of Sept. 11, 2001. Dreher et al. (2017) estimate the impact of changes in immigrant stocks on the probability of terror attacks in the host country, finding no evidence that terrorism is imported from Muslim countries or other countries with significant terrorism.[2] Cornelissen

[2] Avdan (2014) finds that terrorism in Europe results in migration restriction for

and Jirjahn (2012), Dávila and Mora (2005), Kaushal et al. (2007) document the impacts of Sept. 11, 2001 on labor market outcomes of Arabs and Muslims.[3] Our work also ties in with studies examining the effect of terrorism on changes in ethnic attitudes (Ratcliffe and von Hinke Kessler Scholder (2015)), self-identification among Arab and Islamic Americans (Mason and Matella (2014)) and integration and assimilation (Bisin et al. (2008), Elsayed and De Grip (2018), Gould and Klor (2016)).

Last, our work complements studies documenting the consequences of transnational terrorism on fear, uncertainty, and behavioral responses to those emotions. We mostly relate to the contributions of Becker and Rubinstein (2011) who argue that terrorism may lead to intense fear of future dangers and Brodeur (2018) who provides empirical evidence that terror attacks in the U.S. decrease consumer confidence.

The paper is structured as follows. Section 2 provides background for the the entry of asylum seekers into the U.S. and the role judges play in the asylum process. In Section 3, we provide a conceptual framework and review the literature on behavioral biases. Section 4 describes the data used and provides summary statistics. Section 5 outlines our empirical strategy and model specifications. Section 6 presents our results. The last section concludes.

2 Asylum and Immigration Judges

In this section, we first briefly describe how immigration judges are hired, by whom, and what their qualifications and backgrounds look like. We then provide an overview of the asylum process in the United States.

2.1 Immigration Judges and the Executive Office for Immigration Review

Asylum adjudication is carried out by the Executive Office for Immigration Review (EOIR) which was created in 1983 after a restructuring of the Department of Justice (DOJ). Its creation separated asylum adjudication from the enforcement of immigration laws (formerly the Immigration and Naturalization Service (INS) which is now part of the Department of Homeland Security (DHS)). The EOIR is tasked with carrying out immigration court hearings, administrative hearings, and appellate reviews and

countries who experience attacks, but does not result in the erosion of the humanitarian principles backstopping asylum recognition.

[3]See Abadie and Gardeazabal (2003), Blomberg et al. (2004), and Crain and Crain (2006) for the macroeconomic consequences of terrorism.

does so with delegated authority of the Attorney General. The Director of the EOIR reports directly to the Deputy Attorney General (Department of Justice (2018a)).

Formally, the Attorney General makes immigration judge appointments however the hiring process is conducted by the EOIR. The requirements for becoming an immigration judge include being a U.S. citizen or national in possession of a law degree with at least 7 years of post bar legal experience who is authorized to practice law as an attorney in the U.S. Applicants are also required to submit a writing sample to demonstrate their ability to author legal documents (Department of Justice (2018b)).

We manually collected judge characteristics for all the judges in our dataset (see Section 4 for more details) and use those with full enumeration as controls. Unfortunately, the data is incomplete as we are missing complete biographical information for about 10 percent of our judges. Of the judges for which we have complete information, about 50 percent had previously worked for the INS in some capacity, often as trial attorneys. Around 45 percent of judges had previously worked in legal aid, nearly the same proportion as having worked for a firm or in private practice. These background characteristics are not mutually exclusive and it is common for judges to, for example, have worked for the INS and also have worked for a firm or legal aid organization.

Immigration courts are often not staffed with law clerks to provide additional research assistance or bailiffs (unless the hearing are taking place at a detention center) and judges are required to manage their own recording of proceedings (National Association of Immigration Judges (2011)). These resource issues are compounded by a heavy workload—making hundreds of decisions each year and hearing several times that many—and hundreds of thousands of backlogged cases.[4] Perhaps as a result of these conditions, the EOIR and DOJ are not able to fill vacancies in a timely manner and there have been concerns with the temperament, quality, and performance of judges from Federal courts and the Attorney General (Cable News Network (CNN) (2006); Liptak (2005); National Association of Immigration Judges (2011)).

2.2 The Asylum Process

In the United States, refugees refer to those who submit applications for refugee status from outside of the United States while asylees refer to those

[4]There are currently over 800,000 cases that are pending but undecided. In our sample, there were approximately 150,000-200,00 pending cases (TRAC (2018)).

who submit applications from within the United States.

Among asylees, there are two types of applications—affirmative and defensive. Affirmative applications are filed directly with the United States Citizenship and Immigration Service (USCIS) at the initiative of the applicant (regardless of whether or not the individual entered legally or illegally). Defensive applications are those that are filed in response to removal (deportation) proceedings. Removal proceedings may occur as a result of criminal convictions, determination of illegal entry into the United States, or possession of insufficient documentation at border crossings. As such, defensive applicants are often detained at the time of the hearing, but can be released with a bond, though this is infrequent (United States Government Accountability Office: Report to Congressional Requesters (2008)). In our analysis, we control for whether the application is affirmative or defensive.

Applicants may file for asylum or a withholding of removal (or both), regardless of whether the application is affirmative or defensive. Asylum eligibility requires that the applicant be fleeing persecution based on race, religion, nationality, belonging to a particular social group, or political opinion. Additionally, eligibility requires the application must be filed within one year of arriving in the United States barring exceptional circumstances preventing this or a material change in circumstances. Immigration judges are required to determine whether or not the fear of persecution is "well-founded" and if the applicant is ineligible for asylum, whether or not the applicant qualifies for a withholding of removal. Withholding of removal requires that the applicant is "more likely than not" to be persecuted upon return to their country of origin. The same criteria of "more likely than not" also applies to withholding of removal due to the Conventions Against Torture (Miller et al. (2015); Ramji-Nogales et al. (2007)).

Affirmative applications are first vetted by asylum officers employed by UCSIS who can grant asylum themselves but commonly do not. Asylum officers are randomly assigned within one of the eight regional UCSIS offices. Affirmative applications that are not granted asylum by asylum officers are then moved to immigration judges. Defensive applications enter the system at the immigration judge stage. Asylees are randomly assigned an immigration judge within the court their application is referred to. Asylees who were arrested are typically assigned the court closest to the location of the arrest. Asylees filing affirmatively upon entering the U.S. are typically assigned the court closest to the port of entry used. Asylees who did not apply at their time of entry and who have not been arrested are typically

assigned to the closest court to their address (Miller et al. (2015); Refugee, Asylum, and International Operations Directorate (2016)).[5]

Applicants are provided with an interpreter for hearings, but legal representation is not provided. Applicants may obtain legal representation themselves and in our analysis we control for this.[6] Asylum cases are adversarial, with immigration judges hearing the applicant and their legal representation (if present) as well as a trial attorney from the United States Immigration and Customs Enforcement Agency representing the government (Ramji-Nogales et al. (2007)).

After a judgment is rendered, both parties have the ability to appeal the decision to the Board of Immigration Appeals. The majority of decisions are not appealed and among those that are appealed, the vast majority confirm the decision of the immigration judge (Miller et al. (2015)). Further appeals can be made to the U.S. Court of Appeals but this is exceedingly rare.

To sum up, asylees apply for refugee status from within the United States, either in response to being deported or proactively, to escape persecution in their own country. Their cases are heard by randomly assigned immigration judges who are institutionally independent of immigration enforcement, typically in the court closest to their physical location at the time of the application. Hearings are adversarial and asylees must provide their own legal representation. In our empirical analysis, we will control for whether applications are affirmative or defensive, whether or not the asylee has legal representation, and whether or not the asylee was detained. We also control for judge gender, experience, and the party in control of the White House when they were appointed.

3 Conceptual Framework

The fundamental directive immigration judges are given is to evaluate the eligibility and assess the likelihood of an asylee's persecution upon return to their country of origin.[7] Nonetheless, immigration judges have significant

[5]Applicants have the ability to request a change of venue, which can be approved at the immigration judge's discretion. The other party must have an opportunity to respond that there must be "good cause" for the request to be approved. Cases are not restarted upon being transferred. Requests purely meant to delay proceedings and requests occurring after an initial hearing on the merits of the application are strongly disfavored (Office of the Chief Immigration Judge (2018)).

[6]Unfortunately we have no information about languages spoken or indications of whether or not the offered interpreter was taken.

[7]The lack of research resources and time constraints they face may make them reliant on existing reports produced by the State Department (Miller et al. (2015)).

discretion in adjudicating cases—the definitions of terms like "persecution" used in establishing eligibility for asylum are vague or left up to individual judges as is the estimation of probabilities of persecution.

There are thus several channels through which Islamic terror attacks could influence immigration judges' decision making. We first focus our attention on unconscious or behavioral mechanisms. We then discuss other mechanisms through which Islamic terrorism could affect judges' decisions.

3.1 Behavioral Biases

The institutional setting and context in which immigration judges operate may make them more likely to bring their policy preferences and personal biases to bear on the cases they examine, using them to filter case facts, regardless of legal merit.

There is substantial evidence that emotions influence decision making in economics, psychology, and neuroscience (see Lerner et al. (2015) for a an overview). This is relevant in our context since negative emotions may lead to pessimistic evaluations (Johnson and Tversky (1983)).[8] This is complemented by findings that emotions persist, continuing to influence decisions that are unrelated to the emotion (e.g., Han et al. (2007)). Again, this is important for our context as if emotions persist, then those pessimistic evaluations can occur in a context entirely divorced from that which generated the emotion. There is a burgeoning empirical literature documenting this concept. For example, Lerner et al. (2003) conduct a field experiment in the aftermath of Sept. 11, 2001 where participants are treated with news articles inducing fear or anger. They find that those affected by fear viewed the world with greater risk and those with anger with less risk. Important for our research, those affected by anger supported more harsh treatment of suspected terrorists than those affected by fear.

Two other relevant studies are Eren and Mocan (2018) and Philippe and Ouss (2018). Philippe and Ouss (2018) examine the effect of news coverage of unrelated crime on juror sentences and find an increase in sentence duration. They also explore whether or not this is something specifically related to crime or whether or not crime is another form of bad news, finding that other forms of bad news that may affect mood do not influence juror decisions. Eren and Mocan (2018) explore the effect of unexpected football team losses on juvenile court judges in Louisiana and find that

[8] Danziger et al. (2011) find that the order in which parole judges review cases and the timing of food breaks influences judge decision making, though whether the mechanism is the break, food, or possible change in mood is unclear.

emotional shocks result in harsher sentencing of black juveniles.[9]

To the extent that judge characteristics determine which emotion was felt by the attack itself, the attacks may influence judge behavior in different ways. These potentially heterogenous emotional responses combined with the importance of immigration judge policy preferences are key reasons for our collection and use of experience and appointing political party as controls and factors that may generate heterogenous treatment effects.

3.2 Other Mechanisms

Terrorism and the associated media coverage could shift the perceptions of the frequency of Islamic terrorism. This is a cognitive effect known as the "availability heuristic", which is a mental shortcut in which a person's perceived frequency of an event is influenced by how easily they can call to mind an instance of that event (Tversky and Kahneman (1973)). While the change in perceived frequency is an unconscious decision, the decision to rely on "gut feelings" and avoid challenging or updating those perceptions is conscious. This effect could also interact with the country of origin, as Islamic terrorism could alter the perceived frequency of individuals from Muslim countries being terrorists. These two consequences of Islamic terrorism offer an alternative, conscious mechanism that explains changes in judges' decision making, specifically for asylees from Muslim-majority countries.

In Section 1 we referenced the body of existing research documenting the consequences of terrorism on assimilation, integration, and labor markets. If immigration judges are influenced by local conditions then this existing research suggests a mechanism other than emotion through which judges' decision making could be altered. Miller et al. (2015) suggest that judges can be influenced by local conditions, drawing on other research indicating that judges' decisions in other contexts correlate strongly with local attitudes; that opposition to immigration is tied to local labor market conditions; and that immigrants are a fiscal burden on their local governments.

[9] Card and Dahl (2011) find that unexpected football team losses cause an increase in domestic violence mediated by a change in mood. They describe a 'gain-loss' utility framework where sport team losses result in fluctuations around a rational reference point.

4 Data

Our data on asylum decisions is administrative, case-level data containing the universe of asylum cases in the United States occurring between January 2000 and September 2004. In total, we have 269,270 asylum decisions made by 262 immigration judges across courts based in 43 cities. The dataset contains information about the date and location of hearing, the judge responsible for the case, whether or not an applicant had legal representation, the nationality of the applicant, whether or not the application was in response to deportation proceedings. We merge this data with information about the share of the population that is Muslim in the applicant's country of origin.[10] Additionally, we are able to determine whether or not an applicant was detained in an institution during their hearing. The dataset contains information not only on cases where some form of asylum is denied or approved, but also cases that are withdrawn or dismissed. In our analysis, we exclude dismissed and withdrawn cases as they may be systematically different than pure denials or approvals, leaving us with 140,417 decisions. These applicants are less likely to have legal representation (69%) and less likely to be filing defensively (21%). Our estimates are robust to their inclusion and treatment as denials (see Section 6).

Additionally, we collected biographical information about each judge in the sample. Our primary data source for collecting judges' characteristics is the judge reports from the Transactional Records Access Clearinghouse (TRAC) which is a data collection, research, and data warehouse based out of Syracuse University. Their website contains reports for judges each year in which the judge decides at least 100 cases. The judge information was collected from "a variety of official sources including press releases, testimony, other biographical information released by the Department of Justice, and responses received to specific TRAC inquiries" (TRAC (2008)). Unfortunately, the 100 decision per year threshold in the TRAC data leaves 30 of the 262 judges missing. We used internet searches, relying on DOJ and other government documents or newspaper articles, to compile information about these judges. We were able to obtain gender for all judges and year of appointment for all but four. Unfortunately, information on the backgrounds of these judges was not as readily available and as such, we

[10]Data about the share of the population that is Muslim in each country is drawn from 2010 estimates by the Pew Research Center's Forum on Religion and Public Life and we define a country as Muslim-majority if greater than 50% of their population is Muslim. Note that our definition of Muslim-majority countries would not change if we were using 1990 instead of the 2010 estimates.

do not use this information in our analysis.

We collected information about judge gender, year of appointment, and backgrounds prior to appointment. We used the year of appointment to construct variables indicating the amount of experience a judge has at the time of Sept. 11, 2001. Last, we construct a dummy variable for which political party held the Presidency at the time of the judge's appointment. This variable serves as a measure of a particular judge's ideology as judges are hired by the executive branch of the U.S. government.

There is a large degree of variation in asylum approval rates between courts. For instance, in 2001, the court in San Francisco, California approved 65 percent of cases while the court in Philadelphia, Pennsylvania approved 29 percent of cases. There is also a large degree of variation in approval rates within courts: approval rates in 2001 ranged from 6 percent to 93 percent in New York, New York for judges who adjudicated at least 200 cases. Importantly for our analysis, cases are randomly assigned to immigration judges within a court. In the absence of random case assignment our identification strategy would be threatened by sorting of Muslim cases to judges less (or more) likely to grant asylum.

Table 1 provides summary statistics across our sample on asylum outcomes and case characteristics for the U.S. as a whole. Each of the variables contained in Table 1 are binary variables and thus the means presented represent the share of cases for which the statement is true. Over 90 percent of asylees have a legal representative and 9 percent were detained. Over the sample, about 38 percent of cases were granted asylum. Just under 37 percent of applications are defensive (in response to removal proceedings) and just over 63 percent are affirmative (no removal proceedings being pursued). About 22 percent of asylees have a Muslim-majority country of origin and just under 2 percent share a country of origin with a perpetrator of the Sept. 11, 2001 attacks.

Around 72 percent of applications were heard by judges appointed by a Democrat. Applications were heard by female judges around 36 percent of the time. Just over 35 percent of applications were heard by a judge who had between 0 and 5 years of experience. Judges between 6 and 10 years of experience heard just over 46 percent of applications while those with over 10 years of experience handled the remaining 22 percent or so.

5 Identification Strategy

The objective is to estimate the impact of terrorism on judges' decisions when granting asylum. We rely on two empirical models.

Difference-in-Differences – In our main empirical analysis, we implement a difference-in-differences (DD) research design by comparing the asylum grant rates of asylees from Muslim-majority countries and asylees not from Muslim-majority countries, before and after Sept. 11, 2001. In our main specification, we estimate

$$y_{itjc} = \alpha + \beta Muslim_i + \delta Post911_t + \\ \gamma Muslim_i \times Post911_t + X_i'\psi + temperature_{ct}\theta + \lambda_j + \tau_c + \varepsilon_{itjc} \quad (1)$$

where y_{itjc} is a binary variable that equals one if a judge j grants asylum for case i on date t in city c. $Muslim_i$ is a binary variable that equals one if the asylee is from a Muslim-majority country and zero otherwise. $Post911_t$ is a dummy for whether case i was judged after the attacks. Our coefficient of interest in this model is γ. It shows the effect of DD interaction term, $Muslim_i \times Post911_t$. X_i' is a vector of case specific variables including whether or not the individual is detained at the time of the hearing, has legal representation, and is filling affirmatively or defensively. $temperature_{ct}$ measures the mean temperature from 6am to 4pm in city c on date t and is included following Heyes and Saberian (2019). λ_j and τ_c are judge and city fixed effected, respectively.

We also estimate Equation (1) replacing $Muslim_i$ with a binary variable $Associated_i$ that equals one if the asylee is from a country associated with the Sept. 11, 2001 attacks and zero otherwise. Associated countries are defined as the countries of origin of the 19 attackers: one from Egypt, one from Lebanon, two from United Arab Emirates, and 15 from Saudi Arabia. This alternative specification test whether the effect of Sept. 11, 2001 is larger/smaller for this subgroup of asylees. We hypothesize that the effect is larger since the behavioral biases discussed in Section 3 could be magnified for asylees from these countries. Again, the coefficient of interest is the DD interaction term, this time between $Associated_i$ and $Post911_t$. We exclude Muslim-majority countries other than those associated with the attack, making the comparison between asylees from attack-associated countries and those from non-Muslim-majority countries.

Additionally, we estimate Equation (1) replacing $Post911_t$ with a binary variable $Post311_t$ that equals 1 if the hearings are heard after the March 11, 2004 terror attacks conducted in Madrid, Spain. The coefficient of interest in these regressions is the interaction $Muslim_i \times Post311_t$.

We consider the treatment as being applied at the country level with Muslim-majority countries as being treated and non-Muslim-majority countries as untreated. We thus cluster our standard errors at the country of asylee origin level. We have 204 clusters in total.

Our identification assumption is that in the absence of the Sept. 11, 2001 attacks, the change in the grant rates of Muslim-majority countries and other countries would not be different, conditional on controls. We visually demonstrate that this parallel trends assumption holds for the pre-treatment period. Figure 1 plots the monthly average asylum grant rate over our entire sample (January 2000 to September 2004) for asylees from Muslim-majority countries and asylees not from Muslim-majority countries. The vertical line represents the Sept. 11, 2001 attacks. Figure 2 plots the analog for asylees from countries associated with the attacks. In Figure 1, we can see that prior to the attacks, the grant rates for both groups were trending upwards. Asylees from Muslim-majority countries were much more likely to be granted asylum than those from non-Muslim countries, but that there is a convergence in the asylum grant rates in the aftermath of Sept. 11, 2001. In fact, prior to the attacks, the monthly average for asylees from Muslim-majority countries was never below 40% and was above 50% for several months. After the attacks, however, only two months broke 50% and several months fell below 40%. For those from non-Muslim countries it appears there is a leveling off of the growth in grant rates after the attacks with the average asylum grant rate in most months coming in around 35%.

Switching to Figure 2, we can see that asylees from countries associated with the attacks were much more likely to be granted asylum both prior to the attacks and after the attacks. However, just as in Figure 1 the gap shrinks after the attacks. For asylees from associated countries, prior to the attacks, the average asylum grant rate for several months broke 80% and none were below 50%. After the attacks, several months fell below 50% and only two months broke 70%. As with Figure 1, there appears to be an upward trend in the average monthly asylum grant rate prior to the attacks. Afterward, the growth rate levels out for asylees from non-associated countries and appears to be negative for those from associated countries. The results for asylees from associated countries are noisier than those for non-associated countries, possibly because of the smaller number

of observations for asylees from associated countries.

In both figures, the trends for asylees from Muslim-majority countries and associated countries appear to track the trends for asylees not from Muslim-majority or associated countries very closely. More formally, Appendix Table A1 shows the estimation of Equation 1 with the inclusion of a linear time trend interacted with a dummy variable indicating if the asylee is from a Muslim-majority country. The full suite of controls are also included and standard errors are clustered at the country of origin. The results indicate that there is no significant difference in the pre-treatment trends for asylum approval rating between asylees from Muslim-majority countries and those not from Muslim-majority countries. Together, these provide evidence that our choice of control groups represent appropriate counterfactuals prior to the Sept. 11, 2001 attacks and would continue to do so afterward. We delay our discussion of our placebo treatment to Section 6.

Given the large degree of heterogeneity in judge grant rates, it is crucial for our identification that there is no sorting of cases for asylees from Muslim-majority countries to judges that are particularly lenient or severe. Appendix Table A2 presents the results from a regression of a judge stringency measure (created by calculating the leave-one-out mean grant rates for each judge) on a dummy variable that equals 1 if an asylee is from a Muslim-majority country and 0 otherwise, while controlling for court fixed-effects. If cases are randomly assigned, the point estimate should be close to zero and that is exactly what we find.

There may be concerns that Sept. 11, 2001 and March 11, 2004 resulted in a change in the composition of asylum applicants. This would be an issue if the change is more pronounced for Muslim applicants, possibly in anticipation of potential backlash. This is not an issue for our main specification since we restrict the sample only to cases occurring six months before and after Sept. 11, 2001. The Immigration and Nationality Act requires that a decision be made on applications within 180 days of filing, though in practice backlogs are much longer than this. According to the Transactional Records Access Clearinghouse, a data gathering, distribution, and research organization housed at Syracuse University, the average wait time for the entire U.S. across all nationalities ranged from a low of 380 days in 2000 to a high of 422 days in 2003 (TRAC (2018)), while the average processing time ranged from 234 days in 2001 to 283 days in 2004 (TRAC (2018)). As a consequence, the hearings in our sample will correspond to applications filed before Sept. 11, 2001 and March 11, 2004 thus avoiding any potential

bias from compositional changes in the applicant pool. We also note that due to data limitations, we are only able to conduct 3 month and 6 month windows around March 11, 2004.[11]

Triple Differences – We supplement our main analysis by investigating how the effect estimated in Equation 1 varies across immigration judges appointed during a Republican or Democrat presidency, employing a triple-differences (DDD) research design. We estimate

$$\begin{aligned} y_{itj} = \alpha + \beta Muslim_i + \delta Post911_t + \\ + \zeta Muslim_i \times JudgeCharj + \theta Post911_t \times Democrat_j + \\ \kappa Muslim_i \times Post911_t \times Republican_j + \\ \gamma Muslim_i \times Post911_t + X_i'\omega + V_j'\eta + temperature_{ct}\theta + \tau_c + \epsilon_{itj} \end{aligned} \quad (2)$$

where V_j' is a vector of judge specific variables including gender, and dummies for between 0–5 years, 6–10 years and more than 10 years of experience as of Sept. 11, 2001. $Democrat_j$ is a binary variable that equals one if the judge was appointed during a Democrat presidency and zero if appointed during a Republican presidency. For example, we compare the difference in asylum grant rates for asylees from Muslim-majority countries (treated) to asylum grant rates for asylees from other countries (control), across Republican (treated) and Democrat (control) judges, before (control) and after Sept. 11, 2001 (treated). Our coefficient of interest in this model is κ, which shows the effect of the DDD interaction term, $Muslim_i \times Post911_t \times Democrat_j$. We do not estimate Equation (2) for asylees from countries associated with the attacks due to sample size concerns. All other components are defined as in Equation (1).

6 Results

In this section, we first estimate the effect of Sept. 11, 2001 on granting rates for applicants from Muslim-majority countries and countries associated with the attacks. We then estimate heterogeneous treatment effects—exploring how the estimated effect of Sept. 11, 2001 for Muslim-majority countries and associated countries varies across location and judge characteristics. We conclude this section with robustness checks.

[11] We are seven days short of fully having 6 month windows around March 11, 2004.

6.1 Difference-in-Differences

Table 2 presents OLS estimates of Equation (1) for asylees from a Muslim-majority country whose case was heard within 180 days before or after Sept. 11, 2001. What clearly emerges is that Sept. 11, 2001 is associated with a large decrease in the likelihood applicants from Muslim-majority countries are granted asylum. As noted in Section 5, we restrict our sample around the attacks on Sept. 11, 2001 in order to help address concerns about changes in the composition of applicants, perhaps reflecting strategic behavior in response to the attacks. Consequently, the time period is March 15, 2001 to March 9, 2002 (180 days before and after). The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The sample size is 25,832 observations (i.e., cases). As mentioned in Section 5, we report standard errors clustered by asylee country of origin in parentheses. Appendix Table A3 presents the estimated coefficients of the control variables.

Column 1 presents the simple differences of an applicant being from a Muslim-majority country and having their case heard after Sept. 11, 2001. We show that prior to the attacks, applicants from Muslim-majority countries, in comparison to other applicants, were significantly more likely to receive asylum (coeff. of 0.122 and std. dev. of 0.031). The dummy $Post911_t$, which indicates whether the case was heard after Sept. 11, 2001, is negative and statistically significant at the 1% level. The estimate suggests that the attacks decreased the likelihood to receive asylum by about 2 percentage points (pp).

Column 2 adds our coefficient of interest, γ, in the third row. We find that the Sept. 11, 2001 attacks resulted in a 4.3 percentage point decrease in the likelihood that applicants from Muslim-majority countries are granted asylum. Column 3 adds to the model applicant controls, while column 4 includes applicant and temperature controls along with judge and city fixed effects. Applicant controls include detention status, application type (affirmative or defensive), and legal representation, while temperature is the mean temperature in the city of the hearing from 6am to 4pm on the day of the hearing. [12] Our estimates are remarkably stable (4.3–4.4pp throughout) and are statistically significant at conventional level. The inclusion of temperature controls, and judge and city fixed effects slightly increase the precision of our estimate, which is statistically significant at the 5% level

[12] As discussed in Section 5, we control for these additional variables to ensure that our estimates are not picking up compositional changes in the pool of applicants in response to the events of September 11, 2001.

in column four (our preferred specification).

Table 3 presents the results of Equation (1) for asylees from a country associated with the Sept. 11, 2001 attacks whose case was heard within 180 days before or after the attacks. The structure of the table is the same as in Table 2. Once again, the attacks result in a large decrease in the likelihood of granted asylum and in fact the estimates here are much larger. As before, column 1 presents the simple differences of an applicant having their case heard after the attacks or of being from an associated country. We find that those from an associated country are significantly more likely to receive asylum. We also find a very similar estimate of the effect of an applicant's case being heard after Sept. 11, 2001.

The estimates in columns 2–4 suggest that the Sept. 11, 2001 attacks caused a significant decline in the likelihood of applicants from associated countries to receive asylum. The estimates range from 6.7pp to 13.6pp and are statistically significant at the 10% level in our preferred specification. It is important to emphasize that because there are relatively few asylees from attack associated countries, our estimates are relatively imprecise.

Table 4 contains the OLS estimates of Equation (1) for asylees from a Muslim-majority country whose case was heard within 180 days of the March 11, 2004 attacks in Madrid. The structure of the table is the same as Tables 2 and 3. Unlike our estimates for the Sept. 11, 2001 attacks, our estimates in column 1 of the effects of applicants being from Muslim-majority countries or for having a case heard after the attacks are not statistically significant at the 1%, 5%, or 10% levels. Given the trends seen in Figure 1, the gap that asylees Muslim-majority countries had over asylees from other countries has greatly eroded by the time the Madrid attacks happen. Columns 2–4 again suggest that we find an effect of the Madrid attacks of between 3.4pp and 4pp. Our estimates are robust to the inclusion of applicant and temperature controls and, judge and city fixed effects. These estimated effects are very similar in magnitude to those of the Sept. 11 attacks.[13] It may be surprising that the magnitude of the effect of the Madrid attacks on immigration judges is so similar to that of the Sept. 11 attacks which took place on American soil. One possibility is that immigration judges in America became more sensitive to terrorism

[13]We also estimate this specification for the Madrid attacks with the treatment definition being changed to asylees sharing a country with the attackers (Morocco and Tunisia). Our estimated coefficients for the interaction term are negative but not statistically significant at the 10% level. However, the Madrid attacks were perpetrated by individuals linked to only two countries and thus we are quite underpowered for this analysis.

related shocks, perhaps in part due to the political climate and perception of the risks and threat of terrorism post Sept. 11, 2001.

6.2 Robustness Checks and Placebo Test

Table 5 explores the sensitivity of our findings to alternative choices of pre- and post-Sept. 11, 2001 periods. The sample window varies from 3 months (90 days) to 12 months (365 days) before and after Sept. 11, 2001. Columns 1–4 correspond to applicants from Muslim-majority countries and columns 5–8 correspond to applicants from countries associated with the attacks. Each column includes all applicant and temperature controls along with judge and city fixed effects. Column 2 is the same as column 4 of Table 2 and column 6 is a repetition of column 4 in Table 3. The results suggest two main things: (1) the estimated effect size is in the same ballpark across these different windows; and (2) the sample size issues mentioned in the previous section have a dramatic effect on the precision of the estimates for attack associated countries across different windows.

Additionally, Appendix Tables A4 and A5 present Logit and Probit results for Equation (1) for asylees from Muslim-majority countries whose case was heard within 180 days before or after Sept. 11, 2001. These tables are replications of Table 2. The estimates provided are marginal effects and are nearly identical to those found in Table 2. Once again we find an effect of a magnitude around 4pp that is statistically significant at the 5% level and that is robust to adding or removing applicant and temperature controls, and judge and city fixed effects. These tables present a 6 month (180 day) window, but the results are robust across all windows.

Appendix Tables A6 contains the results of a placebo test using Equation (1) for asylees from Muslim-majority countries and associated countries whose case was heard within 180 days before or after Sept. 11, **2000**. Columns 1–4 correspond to applicants from Muslim-majority countries and columns 5–8 correspond to applicants from countries associated with the attacks. Because there was no attack on Sept. 11, 2000, the DD interaction term should be zero. In fact, we see that these estimated effects are not significantly different from zero and are unaffected by including or excluding applicant and temperature controls and, judge and city fixed effects. While this table presents a 6 month (180 day) window, the results are robust across all windows. A second placebo test using Sept. 11, **2002** yields the same conclusions.

Appendix Table A7 produces the results of a placebo test using Equa-

tion (1) for asylees from Muslim-majority countries whose case was heard within 180 days of March 11, **2003**. Again, we expect to see that the DD interaction term should be 0 as there is no attack on March 11, 2003. We do in fact see that the estimated coefficient is not statistically different from 0 and robust to the inclusion or exclusion of controls and fixed effects.

Appendix Table A8 presents the same results as Table 2 but includes withdrawn and dismissed cases. The estimated coefficient of interest is in the same direction and very close to that presented in Table 2. Our other estimates using dismissed and withdrawn cases are similarly unchanged from those excluding them.

Appendix Table A9 provides estimates for Equation (1) raising the threshold for determining that a country is a Muslim-majority country from 50% to 75%. The estimates have the same direction and similar magnitude as those in Table 2 and the estimate in our preferred specification (column 4) is statistically significant at the 10% level.

6.3 Triple-Differences

Table 6 presents the results of Equation (2) for asylees from Muslim-majority countries. As in the previous DD analysis, the first two differences remain before and after Sept. 11, 2001 and whether or not the applicant is from a Muslim-majority or associated country. The third difference is whether or not the case is heard by a judge who was appointed during a Democrat presidency. As in Appendix Table 5, we allow the sample window to vary from 3 months (90 days) to 12 months (365 days) before and after Sept. 11, 2001. All estimates include applicant, judge and temperature controls and city fixed effects with standard errors clustered at the asylee's country of origin.

Our results suggest that the differential effect of Sept. 11, 2001 on Muslim-majority country asylees does not vary by the appointing political party of the judge. Estimates are not statistically significant across any of the windows around the attacks and the point estimates of the coefficients are quite small. These results are in contrast with recent work by Cohen and Yang (2019) who find that the appointing political party of federal judges influences judge decision making. It is worth emphasizing here, however, that we are measuring differences in judge responses to Sept. 11, 2001 along political affiliation and not differences in asylum approval along political affiliation.

7 Conclusion

Using the universe of asylum applications in the U.S. we investigated the effect of Islamic terrorism on granting rates for asylees from Muslim-majority countries in comparison to those from other countries. We find asylees from Muslim-majority countries were 4.4 percentage points less likely to be granted asylum in the aftermath of Sept. 11, 2001. For asylees from countries associated with the Sept. 11, 2001 attacks (Egypt, Lebanon, Saudi Arabia, United Arab Emirates) the effect is even more pronounced—they were 6.7–13.6 percentage points less likely to be granted asylum than those from other countries, however these estimates are relatively imprecise due to the small number of asylees from attack associated countries. We also find that the events of March 11, 2004 resulted in a 4.1% reduction in the likelihood that asylees from Muslim-majority countries were granted asylum. These results are robust to applicant and temperature controls, judge and city fixed effects, as well as alternative estimation methods. Additionally, we do not find evidence of differences in the differential effects of Sept. 11, 2001 on asylees from Muslim-majority countries across judge political affiliation.

Our results are consistent with the emerging literature documenting the influence of emotions on decision making. These findings are of interest to policy makers in evaluating the performance of immigration judges and in preparing for potential consequences of terrorism for the justice system. Additionally, these findings are of interest in contributing to the research enumerating the consequences of terrorism and in particular the legacy of Sept. 11, 2001.

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Figure 1: Average Monthly Relief Rates: Muslim-Majority Countries v. Other Applicants

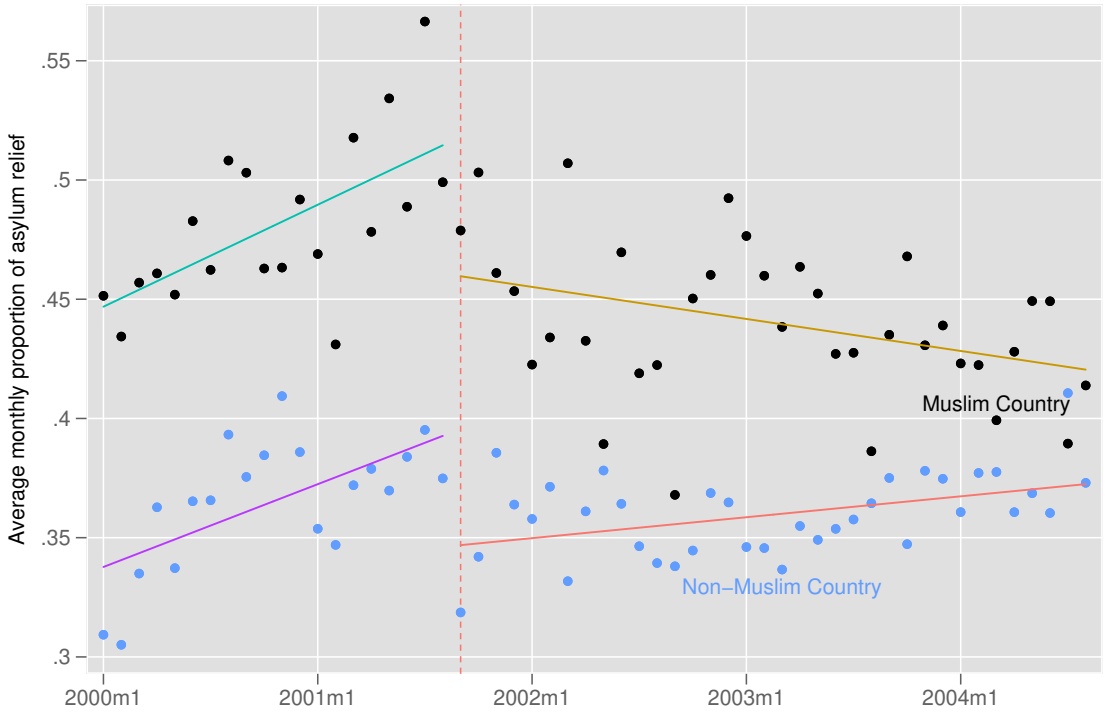

Figure 2: Average Monthly Relief Rates: Attack Associated Countries and Non-Attack Associated Countries

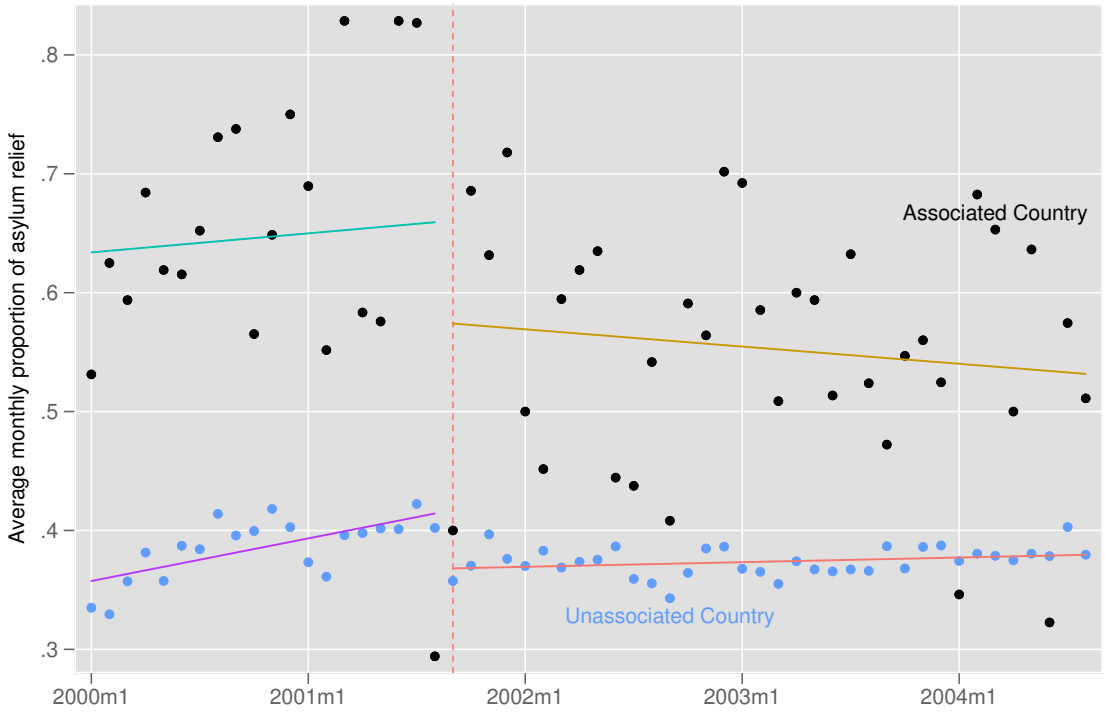

Table 1: Summary Statistics

	Mean	Std. Dev.
Outcomes		
Applicant Granted Any Relief	0.381	0.486
Applicants' Characteristics		
Asylee from Muslim-Majority Country	0.216	0.411
Asylee from Associated Country	0.016	0.127
Defensive Application	0.366	0.481
Has Legal Representation	0.901	0.289
Location is DOC, Detention Center or Prison	0.090	0.287
Judges' Characteristics		
Judge Appointed During Democrat Presidency	0.720	0.449
Judge is Female	0.357	0.479
0–5 Year of Experience	0.322	0.467
6–10 Years of Experience	0.462	0.499
More 10 Years of Experience	0.216	0.412
Observations	140,417	

Note: Authors' calculations. See Section 4 for more details. The time period is January 2000–August2004.

Table 2: Effect of Sept. 11, 2001 on Any Relief Granted: Muslim-Majority Countries

	(1)	(2)	(3)	(4)
Asylee from Muslim-majority country	0.122**	0.143***	0.126**	0.120***
	(0.048)	(0.052)	(0.053)	(0.032)
After Sept. 11, 2001	-0.024**	-0.014	-0.013	-0.005
	(0.012)	(0.011)	(0.013)	(0.009)
Muslim country × After Sept. 11, 2001		-0.043*	-0.043*	-0.044**
		(0.023)	(0.023)	(0.017)
Observations	25,821	25,821	25,821	25,819
R-squared	0.011	0.011	0.059	0.236
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows OLS estimates of Equation (1). The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Effect of Sept. 11, 2001 on Any Relief Granted: Attack Associated Countries

	(1)	(2)	(3)	(4)
Asylee from associated country	0.270***	0.343***	0.299***	0.244***
	(0.054)	(0.053)	(0.052)	(0.044)
After Sept. 11, 2001	-0.017	-0.014	-0.013	-0.005
	(0.012)	(0.011)	(0.013)	(0.010)
Associated country × After Sept. 11, 2001		-0.134***	-0.136***	-0.067*
		(0.024)	(0.020)	(0.039)
Observations	20,666	20,666	20,666	20,664
R-squared	0.006	0.007	0.059	0.245
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows OLS estimates of Equation (1). The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of March 11, 2004 on Any Relief Granted: Muslim-Majority Countries

	(1)	(2)	(3)	(4)
Asylee from Muslim-majority country	0.057	0.073	0.056	0.012
	(0.066)	(0.065)	(0.067)	(0.043)
After March 11, 2004	0.006	0.013	0.012	0.010
	(0.008)	(0.009)	(0.009)	(0.010)
Muslim Country × After March 11, 2004		-0.034**	-0.031*	-0.041**
		(0.016)	(0.017)	(0.016)
Observations	32,998	32,998	32,998	32,998
R-squared	0.002	0.003	0.062	0.253
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows OLS estimates of Equation (1). The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is September 13, 2003 to August 31, 2004. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of Sept. 11, 2001 on Any Relief Granted: Muslim-Majority Countries and Attack Associated Countries

	(1) 3 Months	(2) 6 Months	(3) 9 Months	(4) 12 Months	(5) 3 Months	(6) 6 Months	(7) 9 Months	(8) 12 Months
Asylee from Muslim-majority country	0.121***	0.120***	0.107***	0.099***				
	(0.032)	(0.032)	(0.033)	(0.035)				
After Sept. 11, 2001	-0.000	-0.005	-0.009	-0.015	-0.004	-0.005	-0.009	-0.013
	(0.013)	(0.009)	(0.012)	(0.013)	(0.012)	(0.010)	(0.013)	(0.013)
Muslim country × After Sept. 11, 2001	-0.034*	-0.044**	-0.036	-0.025				
	(0.018)	(0.017)	(0.025)	(0.028)				
Asylee from associated country					0.278***	0.244***	0.244***	0.242***
					(0.062)	(0.044)	(0.033)	(0.028)
Associated country × After Sept. 11, 2001					-0.098	-0.067*	-0.045	-0.072***
					(0.089)	(0.039)	(0.035)	(0.024)
Observations	12,289	25,819	38,925	54,212	9,843	20,664	31,037	43,219
R-squared	0.255	0.236	0.227	0.217	0.271	0.245	0.239	0.231
Applicant Controls	✓	✓	✓	✓	✓	✓	✓	✓
Temperature	✓	✓	✓	✓	✓	✓	✓	✓
Judge FE	✓	✓	✓	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: This table shows OLS estimates of Equation (1). The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. In columns 1 and 5, the time period is three months before and after Sept. 11, 2001. In columns 2 and 6, the time period is six months before and after Sept. 11, 2001. In columns 3 and 7, the time period is nine months before and after Sept. 11, 2001. In columns 4 and 8, the time period is twelve months before and after Sept. 11, 2001. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of Sept. 11, 2001 on Any Relief Granted: Muslim-Majority Countries by Judges Appointed during Democrat v. Republican Presidencies

	(1) 3 Months	(2) 6 Months	(3) 9 Months	(4) 12 Months
Muslim Country × After 9/11 × Democrat	-0.008	-0.029	-0.000	-0.004
	(0.064)	(0.044)	(0.045)	(0.045)
Observations	12,289	25,819	38,925	54,212
Applicant Controls	✓	✓	✓	✓
Temperature	✓	✓	✓	✓
City FE	✓	✓	✓	✓
Judge Controls	✓	✓	✓	✓

Note: This table shows OLS estimates of Equation (2). The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Applicant controls include application type, detention status, and legal representation. Judge controls include sex, experience, and political party of appointing administration. City fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. All pairwise interactions are included in the regressions, but omitted from the tables. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

1 APPENDIX: NOT FOR PUBLICATION

Table A1: Test for Diverging Pre-Trends in Asylum Approval

	(1)
Muslim Country × Time	0.000
	(0.000)
Observations	25,819
R-squared	0.236
Applicant Controls	✓
Temperature	✓
Judge FE	✓
City FE	✓

Note: Coefficient is based on regressions of Equation (1) with the addition of a linear time trend and the interaction of the time trend and whether or not the asylee is from a Muslim-majority country. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Test for Random Assignment of Cases to Judges

	(1) Asylum Approval
Asylee from Muslim-majority country	0.000***
	(0.000)
Observations	140,428
R-squared	0.435
City FE	✓

Note: The dependent variable is a judge stringency measure created from leave-one-out mean grant rates for each judge. Coefficient presents results of regression of the judge stringency measure on a dummy variable that equals 1 if an asylee is from a Muslim-majority country and 0 otherwise. Court fixed-effects are included. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Full Estimation of the Effect of Sept. 11, 2001 on Any Relief Granted: Six Month Window

	(1)	(2)
Asylee from Muslim-majority country	0.120***	0.120***
	(0.032)	(0.032)
After Sept. 11, 2001	-0.005	-0.005
	(0.009)	(0.009)
Muslim country x After Sept. 11, 2001	-0.044**	-0.044**
	(0.017)	(0.017)
Defensive Application	-0.083***	-0.083***
	(0.026)	(0.026)
Has Legal Representation	0.213***	0.213***
	(0.034)	(0.034)
Location is DOC, detention centre, jail, or prison	-0.014	-0.014
	(0.031)	(0.031)
Mean temperature 6AM–4PM	0.000	0.000
	(0.000)	(0.000)
Observations	25,819	25,819
R-squared	0.236	0.236
Judge FE	✓	✓
City FE	✓	✓

Note: This table shows OLS estimates of Equation (1). The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Logit Estimation of the Effect of Sept. 11, 2001 on Any Relief Granted: Six Month Window

	Asylum Granted: Six Months Window			
	(1)	(2)	(3)	(4)
Asylee from Muslim majority country	0.118***	0.138***	0.122**	0.113***
	(0.044)	(0.047)	(0.049)	(0.029)
After Sept. 11, 2001	-0.024**	-0.015	-0.014	-0.006
	(0.012)	(0.011)	(0.013)	(0.009)
Muslim country × After Sept. 11, 2001		-0.039*	-0.040*	-0.039**
		(0.022)	(0.022)	(0.016)
Observations	25,821	25,821	25,821	25,725
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows Logit estimates of Equation (1). Marginal effects are reported. The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5: Probit Estimation of the Effect of Sept. 11, 2001 on Any Relief Granted: Six Month Window

	Asylum Granted: Six Months Window			
	(1)	(2)	(3)	(4)
Asylee from Muslim majority country	0.119***	0.139***	0.123**	0.114***
	(0.045)	(0.048)	(0.049)	(0.029)
After Sept. 11, 2001	-0.023**	-0.014	-0.014	-0.006
	(0.012)	(0.011)	(0.013)	(0.009)
Muslim country × After Sept. 11, 2001		-0.040*	-0.039*	-0.040**
		(0.022)	(0.022)	(0.016)
Observations	25,821	25,821	25,821	25,725
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows Probit estimates of Equation (1). Marginal effects are reported. The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A6: Placebo Test using Sept. 11, **2000**: Six Month Window

	Asylum Granted: Six Months Window							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Asylee from Muslim majority country	0.099*	0.108*	0.096*	0.104***				
	(0.058)	(0.059)	(0.055)	(0.039)				
After Sept. 11, 2000	0.013	0.017	0.014	0.031**	0.012	0.013	0.010	0.024**
	(0.011)	(0.013)	(0.012)	(0.013)	(0.011)	(0.011)	(0.010)	(0.011)
Muslim country × After Sept. 11, 2000		-0.016	-0.018	-0.025				
		(0.023)	(0.022)	(0.020)				
Asylee from associated country					0.267***	0.280***	0.253***	0.226***
					(0.094)	(0.103)	(0.089)	(0.046)
Associated country × After Sept. 11, 2000						-0.025	-0.030	-0.002
						(0.022)	(0.022)	(0.015)
Observations	24,680	24,680	24,680	24,679	24,680	24,680	24,680	24,679
R-squared	0.007	0.007	0.054	0.218	0.005	0.005	0.052	0.216
Applicant Controls			✓	✓			✓	✓
Temperature				✓				✓
Judge FE				✓				✓
City FE				✓				✓

Note: This table shows OLS estimates of Equation (1) for a placebo analysis. The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2000 to March 9, 2001. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A7: Placebo Test using March 11, **2003**: Six Month Window

	Asylum Granted: Six Months Window			
	(1)	(2)	(3)	(4)
Asylee from Muslim majority country	0.089**	0.102**	0.074*	0.055**
	(0.041)	(0.040)	(0.043)	(0.026)
After March 11, 2003	-0.000	0.005	-0.007	0.003
	(0.008)	(0.007)	(0.007)	(0.007)
Muslim country × After March 11, 2003		-0.024	-0.015	-0.024
		(0.020)	(0.020)	(0.018)
Observations	34,935	34,935	34,935	34,934
R-squared	0.006	0.006	0.054	0.215
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows OLS estimates of Equation (1) for a placebo analysis of the March 11, 2003 attacks in Madrid. The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is Sept. 12, 2002 to Sept. 7, 2003. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A8: Estimation of the Effect of Sept. 11, 2001 on Any Relief Granted including Withdrawals and Dismissals: Six Month Window

	(1)	(2)	(3)	(4)
Asylee from Muslim-majority country	0.112**	0.131***	0.131***	0.121***
	(0.048)	(0.048)	(0.044)	(0.034)
After Sept. 11, 2001	-0.020**	-0.013	-0.011	-0.008
	(0.008)	(0.009)	(0.009)	(0.008)
Muslim country × After Sept. 11, 2001		-0.039***	-0.038**	-0.036***
		(0.014)	(0.015)	(0.012)
Observations	48,369	48,369	48,369	48,340
R-squared	0.012	0.012	0.063	0.174
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows OLS estimates of Equation (1) including withdrawn and dismissed cases. The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A9: Varying the Threshold for Muslim-Majority Country
Full Estimation of the Effect of Sept. 11, 2001 on Any Relief Granted: Six Month Window

	(1)	(2)	(3)	(4)
Asylee from country with > 75% Muslim pop.	0.115**	0.129**	0.114**	0.110***
	(0.050)	(0.055)	(0.056)	(0.039)
After Sept. 11, 2001	-0.023**	-0.019	-0.017	-0.007
	(0.012)	(0.012)	(0.013)	(0.010)
Muslim 75 country × After Sept. 11, 2001		-0.028	-0.030	-0.034*
		(0.026)	(0.026)	(0.020)
Observations	25,821	25,821	25,821	25,819
R-squared	0.008	0.008	0.057	0.234
Applicant Controls			✓	✓
Temperature				✓
Judge FE				✓
City FE				✓

Note: This table shows OLS estimates of Equation (1). The Muslim-majority country definition is changed from 50% to 75%. The unit of observation is a case. The dependent variable is a dummy that equals one if asylum is granted and zero otherwise. The time period is March 15, 2001 to March 9, 2002. Applicant controls include application type, detention status, and legal representation. Judge and city fixed effects are included and temperature is the mean temperature on the day of the hearing from 6am–4pm. Standard errors are clustered by country of origin and are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1