

How do Parole Boards Respond to Large, Societal Shocks? Evidence from the 9/11 Terrorist Attacks

Brendon McConnell* Kegen Teng Kok Tan† Mariyana Zapryanova‡

May 14, 2024

Abstract

We provide the first evidence of the impact of 9/11 on outcomes for Muslims in the US criminal justice system. We focus on parole outcomes of Black Muslim men in the state of Georgia, and find large post-9/11 declines in the likelihood of being granted parole and a subsequent 23% relative increase in prison time for Muslim inmates. These impacts persisted for several years after 9/11 and were larger for inmates with higher levels of recidivism risk. We argue that these effects reflect unwarranted disparities driven by the decision-making of parole board members post-9/11.

*Department of Economics, City University of London. Email: brendon.mcconnell@gmail.com.

†Department of Economics, University of Rochester. Email: ttan8@ur.rochester.edu.

‡Department of Economics, Smith College. Email: mzapryanova@smith.edu.

We are grateful to Doctor Tim Carr at the Georgia Department of Corrections for sharing the administrative prison files with us. We are thankful to Steve Hayes at the Georgia Board of Pardons and Paroles for many useful conversations regarding the parole process in Georgia. We benefited from useful comments and suggestions from seminar participants at Smith College, George Mason University Law School, Washington and Lee University, and the University of Wisconsin-Madison, as well as conference participants at the 2022 Southern Economics Association Conference, Spring 2023 NBER Law and Economics Meeting, 2023 American Law and Economics Association Meeting, 2023 European Meeting of the Econometric Society, 2023 Society of Labour Economics, 2023 UW-Madison JPGI Alumni Conference, and 2023 Conference on Empirical Legal Studies. We thank Monica Deza, Steven Durlauf, Peter Grajzl, Robert Kaestner, Stéphane Mechoulan, Mike Mueller-Smith, Imran Rasul, and Lucie Schmidt for their feedback. Priscilla Liu provided excellent research assistance.

1 Introduction

Disparities in the U.S. criminal justice system have long been debated. The topic has, in recent years, intensified and broadened to cover policing, bail, sentencing, and reentry. The primary focus of the debate has centered on disparate treatment by race, yet there is evidence from both the U.S. and other countries of disparities across other dimensions including ethnicity, gender and religion. Despite the multitude of studies, it is still unclear to what extent large disparities in outcomes arise due to discriminatory practices versus underlying unobserved differences across groups.

In this paper we focus on disparities in parole outcomes, a key aspect of prison reentry that allows prison inmates to be released before their sentences are fully served. Our interest lies in investigating religious-based disparities – specifically disparate parole outcomes for Black Muslim inmates. Our focal point is the time period around a large, societal event – the 9/11 terrorist attacks. Whilst the 9/11 attacks impacted a wide swathe of outcomes and led to changes in many dimensions of life in the US, including large-scale legal changes, policing responses, military engagement, and numerous psychological effects (Davis, 2007; Woods, 2011), the US Muslim population was particularly affected. Affected outcomes include the labor market (Davila and Mora, 2005; Kaushal et al., 2007), victimization (Singh, 2002), criminalization (Kaufman, 2019), discrimination (Sheridan, 2006), societal resentment and reservations (Panagopoulos, 2006), and assimilation (Gould and Klor, 2016). We conceptualize the attacks as an exogenous shock to the level of animosity towards Muslims.

We estimate the impact of such a shock to animosity towards Muslims in a regression-adjusted difference-in-differences (DD) framework, conditioning on a rich set of relevant control variables. We interpret our DD estimates as the causal effects of the parole board response to the 9/11 terrorist attacks on parole outcomes for Muslim inmates. We provide a battery of evidence in support of the key identifying assumptions—namely the parallel trends assumption and the stability of group composition before and after 9/11—that underpin this causal interpretation.

Our study is the first to investigate the impact of the criminal justice system response to 9/11 on Muslims in the US. We use administrative records from the Department of Corrections in the state of Georgia, which importantly contain information on self-reported religion.¹ We focus on Black, male, parole-eligible inmates as the overwhelming majority (94.3%) of Muslim

¹Religion is collected as a part of the process of being admitted into the Georgia State Correctional System and is reported once the convicted felon is transferred from the court to one of the state's diagnostic and classification prisons. For our core analysis, we restrict our sample to those sentenced prior to 9/11/01, thus ruling out endogenous reporting of Muslim religion status in response to the attacks.

inmates in our data are Black and male.

We document a substantial *short-run* change in the parole outcomes of Muslim inmates in the aftermath of 9/11, detailing impacts for those who came up for parole within a one year window around the attacks. At the extensive margin, we find a 12 percentage point (17%) reduction in the probability of receiving parole. This translates to roughly 200 more days in prison, a 23% increase. To benchmark this effect, our DD estimate is of a similar magnitude to the difference in prison time between serial offenders (8 or more prior convictions) and first-time offenders. These estimates are all statistically significant.

We next examine the extent to which the large short-run impacts we document persist over the longer term. We implement an event study design and consider parole outcomes for the years 1998-2005. In contrast to other outcomes for Muslims, which seem to exhibit large short term effects and substantial fade out (e.g., homicide rates against Muslims (Gould and Klor, 2016)), we document a *substantial degree of persistence* for the parole outcomes of Muslim inmates. Within our event study framework, we cannot reject equality of the short-run estimates (0-1 years post-9/11) with the longer-run estimates (3-4 years post-9/11).

We further conduct a series of analyses to consider a series of potential explanations for our findings. We examine changes in inmate behavior while in prison, finding no statistical changes for Muslim inmates in the post-9/11 period. We conduct sensitivity analyses to examine the importance of raters – those who prepare the parole file for the parole board – again finding no impact of accounting for rater identity. We then move to consider factors that may play a role for Muslim inmates’ life post-release, which the parole board should factor in to parole decisions. We examine the unemployment rate in inmates’ home county, to proxy for local job opportunities, finding no significant changes in the local labor market to which Muslim inmates review post-9/11 would return. We use the full names of the inmates in our sample along with a Muslim-sounding name classifier to create an index of how Muslim inmates names sound. Such informational content of inmates’ names may reflect the extent to which these individuals would face discrimination once released into the post-9/11 environment. This is not likely the channel driving our core results – not only do our sample of Muslim inmates have names that do not differ substantively from their non-Muslim counterparts, there is no statistical difference in the Muslim-sounding name index for Muslim inmates reviewed by the parole board post-9/11.

Taking account of (i) this battery of null results along key dimensions of behavior within prison, and (ii) the factors that may influence reintegration in life outside of prison, we suggest that the most likely cause of the unwarranted disparities that we document for Muslim inmates

post-9/11 is the response of the parole board itself.

While it is both challenging and beyond the scope of this paper to parse the various potential sources of discrimination, we attempt to shed light on the nature of the discrimination by constructing a measure of ex-ante recidivism risk and conduct a heterogeneity analysis of our core parole board outcomes based on this risk measure. We document that parole outcomes for Muslim inmates post-9/11 worsen with ex-ante recidivism risk. For inmates with low predicted recidivism risk, the change in outcomes in the post-9/11 period is muted, whereas for those with high ex-ante recidivism risk, we find more pronounced effects post-9/11. We interpret this as evidence that our short-run estimates are unlikely to be driven by an unconditional discrimination mechanism, but rather are sensitive to the underlying recidivism risk of inmates.

Our work contributes to three key strands of the literature. First, we contribute to the growing empirical literature on the influence of extraneous events and factors on the application of justice.² Within this literature, attention has nearly exclusively focused on the decision-making of judges (Brodeur and Wright, 2019; Eren and Mocan, 2018; Philippe and Ouss, 2018; Arnold et al., 2022), prosecutors (Bielen and Grajzl, 2021; McConnell and Rasul, 2021), and juries (Bindler and Hjalmarsson, 2019). Ours is the first study that sheds light on impact of extraneous factors on the decision-making process of parole boards. The decisions of parole board members are not only different from those of judges or juries, where the degree of discretion and stakes of the decisions are quite different, but also are highly impactful given the wide use of discretionary parole boards in the US criminal justice system.³

Second, by providing the first evidence of how parole board decision making was impacted by the shock of the terrorist attacks of 9/11, we contribute to a body of empirical work that has studied the impact of terrorism on various societal outcomes. For criminal-justice outcomes, scholars have documented impacts on hate crime (Ivandic et al., forthcoming), federal criminal sentences (McConnell and Rasul, 2021)⁴, asylum approvals (Brodeur and Wright, 2019; Emeriau, 2023), and civil cases (Shayo and Zussman, 2011).⁵

Finally, with our focus on disparate outcomes for Muslim inmates, we expand the scope of

²See Ludwig and Mullainathan (2021) for a review.

³In 2019 alone, over 400,000 individuals were released on parole in the US, 43% of these via the discretion of a parole board (Oudekerk and Kaeble, 2019).

⁴In contrast to McConnell and Rasul (2021), the current paper focuses only on Black prisoners and looks at the Muslim-non-Muslim differences. Given that Muslim prisoners are a small fraction of the total Black prison population, it is not surprising that McConnell and Rasul (2021) find that Black inmates collectively are not impacted.

⁵We provide a more detailed summary of the studies in the relevant literature that looks at criminal-justice outcomes in Table A1. In addition, examples of other economic outcomes that the literature has found to be affected by terrorism include labor market (Cornelissen and Jirjahn, 2012; Glover, 2021; Kaushal et al., 2007), housing market (Lepage, 2023), macroeconomy (Abadie and Gardeazabal, 2003; Blomberg et al., 2004), assimilation (Gould and Klor, 2016).

the literature studying disparities in the US criminal justice system. Existing evidence from the US has largely focused on examining *racial bias* in the decision-making of police officers (Feigenberg and Miller, 2022; Goncalves and Mello, 2021), judges (Arnold et al., 2018), juries (Flanagan, 2018), prosecutors (Sloan, 2019), parole boards (Anwar and Fang, 2015; Mechoulan and Sahuguet, 2015), and parole officers (LaForest, 2022). Whilst some papers focus on religious bias in the context of the criminal justice system in India (Ash et al., 2021) and the Netherlands (Bielen and Grajzl, 2021), our paper is the first to focus on religion as a basis for discrimination rather than race or ethnicity in the US criminal justice system.

The rest of the paper is organized as follows. Section 2 outlines the parole process in Georgia and describes the data. Section 3 describes presents our empirical strategy. Section 4 presents our main results, while Section 5 discusses the documented disparities and explores factors inside and outside of prison that might have changed post 9/11. Section 6 concludes.

2 The Parole Process in Georgia and Data

2.1 Institutional Setting

The state of Georgia releases prisoners from prison using a discretionary parole system, where releases are granted on the assessment and full discretion of a parole board. The Georgia parole board consists of five members appointed by the governor to seven-year term subject to confirmation by the State Senate.⁶ The parole board system in Georgia is by no means an exception across the other 33 states that operate discretionary parole systems (see Table A2).⁷ For instance, state parole boards on average consist of 7 members who collectively or as a small group make parole decisions.

Sentenced felons in Georgia are transferred from the court to a diagnostic prison where they go through a battery of tests and diagnostic questionnaires before being assigned to a prison. Importantly for our study, prisoners self-report their religion during this diagnostic process. Most parole-eligible inmates become statutorily eligible for parole release after serving one-third of their prison sentence (O.C.G.A. §42-9-45). Prior to the prisoner's review by the parole board, a pre-parole investigation is conducted by a parole hearing examiner, a role we also refer to as rater, and comprises of the rater interviewing the prisoner and gathering information about the

⁶It is important to note that there were no changes in the parole board composition during our main estimation sample period (Godfrey et al., 2022). The stable board composition helps us isolate the impact of the terrorist attacks without concerns of picking up any effects on parole outcomes due to changes in parole board composition.

⁷In addition, Zapryanova (2020) shows that Georgia's prison population appears to be representative of that nationwide.

prisoner's personal information and criminal record. The rater then uses the Parole Decisions Guidelines Grid System (hereafter the grid) along with the prisoner's risk score and current offense crime severity level, to determine the recommended prison time, also known as the grid recommendation.⁸ Once the prisoner is rated according to the grid, the rater compiles prisoner's parole file and writes a summary discussing its contents.⁹

The parole file contains all records from the diagnostic prison, importantly for us, including the religion of the inmate, as well as all pre-parole records, including pre-parole investigation reports, the grid score, severity level and recommendation, and the rater's summary of the content of the parole file. The parole file is sent, sequentially in a randomized order, to each of the five parole board members, one at a time.¹⁰ After reviewing the file, each member marks their decision on a ballot on the parole file. A parole decision is reached if three out of five board members vote the same way (O.C.G.A. §42-9-42).

2.2 Data and Sample Selection Criteria

Our data are sourced from rich administrative internal records of the Georgia Department of Corrections (GDC), and include a record of all prisoners admitted in prison in Georgia from 1980 to 2008. We observe detailed information on the prisoner demographic characteristics, including prisoner's self-reported religion at admission to prison, and on the parole board decision-making process. In addition, we observe the date on which each prisoner was rated by the grid.¹¹ This date is the earliest date on which the parole file is complete and passed to the parole board for a vote.

We make several sample restrictions and our final estimation sample consists of 4,832 prisoners, of which 222 are Muslim. First, we exclude prisoners who were rated within 180 days prior to 9/11 in order to avoid contaminating our control group with prisoners who might have been rated prior to 9/11 but whose parole file was ultimately reviewed by the board after 9/11. Second, we base our sample on prisoners rated by the Georgia parole board within a 365 day window around 9/11. Third, we ensure that all defendants in our sample have been sentenced prior to 9/11 to

⁸For more details on the grid, please refer to the "Parole Decision Guidelines Grid for Pre-2008 cases" found at <https://pap.georgia.gov/parole-consideration/parole-consideration-eligibility-guidelines>.

⁹The raters are not pivotal actors in the parole process in Georgia and do not have discretion in the evaluation or take an active role in the decision-making process, except for the fact that they prepare the parole file. In general, they do not have a discretion on what is included or not in the parole file except for the fact that they write a summary of the parole file for the parole board members.

¹⁰We do not observe the actual votes because the board members' votes are classified as confidential state secrets under Georgia law (see O.C.G.A. section 42-9-53).

¹¹We do not observe the exact date on which the parole board makes a final decision. However, we use the rate date as the earliest date on which the parole file is ready to be reviewed by the board. Because of this potentially critical issue, when constructing our analysis sample, we build in a buffer period to ensure that those rated pre-9/11 are reviewed by the parole board pre-9/11.

rule out judicial responses. This allows us to focus solely on the parole board response to 9/11. To ensure sample balance, we implement an additional constraint – for those rated pre-9/11, these inmates must be sentenced prior to 9/11/2000. This is to maintain comparability across the groups. We present a schematic outlining these sample restrictions in Figure A1. Finally, we restrict our sample to Black male parole-eligible inmates with non-missing admission, release, sentence and rate dates.¹² We do this for two reasons. First, almost all Muslims in our data are Black males.¹³ Second, there is a large literature documenting racial and gender disparities in criminal justice outcomes. By restricting our sample to Black male inmates, we are able to focus solely on our key treatment variable – Muslim religion status – without complications from sampling variation leading to different proportions of other protected characteristics across both treatment and time that could drive differences in potential confounders.

In Table 1 we present summary statistics of our estimation sample and the results of a series of balance tests. Column (3) and Column(6) show the *p*-values for a null of no difference in means across the two periods, for non-Muslim and Muslim inmates respectively. Non-Muslim inmates have slightly more prior convictions post-9/11 and Muslim inmates are less likely to be married in the post-9/11 sample. Column (7) presents the *p*-value of the difference-in-differences across the control variables. We note that the difference-in-differences are not statistically different from zero for nearly all characteristics. The *p*-values never fall below .05, and the coefficients are small whenever there is statistically significant imbalance (less than high school degree and married). Combined with the fact that we are testing multiple hypotheses and the non-significance of the joint test, we believe that our balance holds.

3 Empirical Approach and Identification

Standard cross-sectional regression approaches for estimating parole outcome disparities are plagued by concerns regarding selection bias (Baron et al., 2023). To circumvent such concerns, we implement a DD strategy, thereby partialling out any omitted variables that are common across periods and that may be correlated with both Muslim status and parole board outcomes. Under the assumption of (i) parallel trends and (ii) stable group composition, such an approach enables us to estimate the causal effect of the parole board response to the 9/11 terrorist attacks

¹²Our main results are qualitatively similar when White inmates are also included in the estimation sample.

¹³Of individuals sentenced in the decade running up to the 9/11 attacks, Black male inmates represent 94.3% of the Muslim inmate population, compared to 56.3% of the non-Muslim inmate population.

Table 1: Balance Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Non-Muslim			Muslim			
	Pre-9/11	Post-9/11	p-value: Difference	Pre-9/11	Post-9/11	p-value: Difference	p-value: DD
Sample Size	2293	2316		112	110		
Education:							
≤ High School	.649	.674	[.077]	.688	.591	[.135]	[.065]
High School	.249	.235	[.278]	.196	.245	[.381]	[.272]
Some College	.0868	.0777	[.263]	.0982	.155	[.209]	[.148]
College	.0153	.0134	[.592]	.0179	.00909	[.573]	[.664]
Social Class:							
On welfare	.0946	.0976	[.734]	.0893	.127	[.365]	[.411]
Occasionally Employed	.0781	.0674	[.162]	.0625	.0727	[.763]	[.545]
Min. Living Standard	.468	.494	[.082]	.438	.409	[.670]	[.427]
Middle Class	.34	.325	[.293]	.384	.373	[.864]	[.959]
Unknown	.0192	.016	[.407]	.0268	.0182	[.667]	[.790]
I.Q. Score	94 (22.7)	95.2 (19.8)	[.066]	96 (18.9)	96.1 (21.6)	[.984]	[.693]
Has Children	.674	.67	[.791]	.705	.618	[.171]	[.197]
Married	.111	.103	[.379]	.179	.0818	[.032]	[.052]
Prior Convictions	2.05 (2.52)	2.3 (2.63)	[.001]	2.7 (2.97)	2.57 (2.84)	[.751]	[.340]
Age at Sentencing	30.9 (9.59)	30.8 (9.59)	[.734]	29.5 (8.77)	28.8 (7.65)	[.559]	[.628]
Risk Score	11.1 (3.8)	11.5 (3.77)	[.000]	12.2 (3.68)	12.3 (3.64)	[.819]	[.513]
Severity Level	2.87 (1.69)	2.8 (1.59)	[.139]	2.94 (1.61)	2.7 (1.62)	[.274]	[.454]
Sentence Length	1977 (1512)	2054 (1759)	[.111]	2052 (1524)	2049 (1697)	[.989]	[.718]
Major Offense Group:							
Violent/Sexual	.242	.21	[.010]	.268	.218	[.390]	[.761]
Property	.332	.339	[.633]	.313	.391	[.223]	[.273]
Drugs/DUI	.343	.367	[.091]	.348	.273	[.226]	[.118]
Other	.0833	.0846	[.871]	.0714	.118	[.237]	[.257]
Joint Test			[.003]			[.577]	[.455]

Notes: Means and standard deviations (in parentheses for continuous covariates) are shown. *p*-values are based on OLS regressions with Eicker-Huber-White standard errors.

towards Muslim inmates. To make progress on this topic, we implement a specification of the form:

$$y_{it} = \alpha_1 Post_t + \alpha_2 Muslim_i + \beta(Post_t \times Muslim_i) + \gamma X'_i + \pi_t + \epsilon_{it}, \quad (1)$$

where y_{it} is the parole outcome of interest for prisoner i rated at time t , $Post_t$ is an indicator that takes the value 1 for prisoners with parole files prepared and ready to be reviewed by the board after 9/11, and 0 otherwise, and $Muslim_i$ is an indicator for Muslim religion status. Importantly for our setting, the religion of the inmates was recorded prior to 9/11 because it was collected during the prisoners' intake from the court into the Georgia State Correctional System, thus ruling out endogenous recording of treatment status as a function of 9/11.

We condition on a rich set of covariates, the most important of which are a series of dummy variables for each of the 21 guidelines cells used as part of the determination of recommended prison time in Georgia.¹⁴ These guidelines combine a prisoner's parole risk score and the severity level of the crime that placed the inmate behind bars. We additionally control for the sentence length received from the sentencing judge, dummies for having children, being married, age at sentencing deciles, education categories, quartiles of Culture Fair IQ test score, indicators for the most serious offense committed, socio-economic status, and dummies for number of prior convictions. The fixed effects, π_t , capture any rating month-specific unobservables. The error term is ϵ_{it} . We use Eicker-Huber-White standard errors throughout. We note that there are no district or area fixed effects, and no parole board fixed effects – the files of all inmates are reviewed by the same parole board throughout the full sample period.¹⁵

3.1 Identification

The key identifying assumption underpinning our empirical approach is that Muslim and non-Muslim inmates experience common trends in parole board outcomes. Taking into account the recent critique to canonical pre-trends testing made by Roth (2022), we provide a battery of evidence using multiple approaches in support of parallel trends in our setting.¹⁶

We first implement a set of placebo DD regressions. We shift all key dates one year back in time, and re-estimate Equation 1, with the sole difference that now the $Post_t$ term takes value zero for the period 11 September 1999–10 September 2000, and one for the period 11 September 2000–10 September 2001. We present the results in Table B1 and find no significant

¹⁴Note that in our estimation sample we do not observe any prisoners a crime severity category of 8.

¹⁵Our main specification is robust to inclusion of rater fixed effects that account for any time-invariant heterogeneity of the ways raters prepare the parole files.

¹⁶We discuss these results in Section B.1.3.

placebo DD estimates. Next, we provide graphical evidence of the lack of existence of pre-trends by presenting the raw, underlying data for three years prior to our estimation sample – the calendar years of 1998-2000. We cannot reject the null of equality of trends in any case based on Figure B1. Finally, In Figure B2 we implement the honest difference-in-differences approach of Rambachan and Roth (2022), in order to create worst-case treatment effect bounds for potential violations of the parallel trends assumption, based on pre-trends. Taken together, the evidence we present here is strongly supportive of parallel trends in parole board outcomes for non-Muslim and Muslim inmates in the period prior to 9/11.

Given that we are using repeat cross-sectional data for our empirical analysis, we also provide evidence for a second identifying assumption – that the composition of the two groups is stable across the pre and post periods – in Appendix B.2 (Blundell and Dias, 2009). First, we perform a series of balance tests that show almost no differences in observable characteristics across the two periods for non-Muslim and Muslim inmates and significant difference-in-differences across the observables (see Table 1).¹⁷ Second, we do not find evidence for the potential of strategic reordering of when Muslim and non-Muslim inmates appear before the parole board in the aftermath of 9/11 by implementing various duration model regressions (see Table B2). This is important as it provides suggestive evidence that endogenous changes in the timeline of the parole process is highly unlikely and could not by itself explain our main results.¹⁸

One potential threat to our identification strategy is that we do not observe Muslim-status over the duration of inmates’ prison spells, in a setting where in-prison religious conversions are potentially common (Boddie and Funk, 2012; Hamm, 2007; Kusha, 2016). If 9/11 lead to changes in both the number of in-prison conversions and the composition of who converts, this could result in selection bias. That means that even if 9/11 had no causal effect on parole board decision making, differential composition across the treatment and control groups in the pre and post periods could lead us to detect changing outcomes using our DD approach. We address this concern in Appendix B.3, we implement a sensitivity analysis of our key DD parameters based on a proxy for conversion rate likelihood. We sequentially remove facilities by their Muslim prisoner concentration rank. We start at the very top of the distribution of this concentration rank, and move down until we have deleted 50% of our sample. As seen in Figure B3, the parameter estimates are extremely stable across all sample specifications, leading us to conclude

¹⁷We present balance tests for our longer-term results in Table B3. Note that while rare education categories (non-High School) show some statistically significant differences, they are generally small in magnitude. The joint tests excluding education are not statistically significant.

¹⁸Furthermore, the timing for when a Muslim inmate’s file is prepared does not seem to have changed significantly after 9/11, suggesting that raters are not strategically reordering their rating dates to penalize Muslims (see Figure C2).

that differential conversions to Islam are not a credible threat to our identification strategy.

4 Results

Table 2: Means, Differences, and Difference-in-Differences

	(1)	(2)	(3)	(4)
	Parole Granted	Days Parole	Days Prison	Sample Size
Non-Muslim Inmates				
Pre-9/11	.71 [.454]	961 [1,253]	869 [722]	2,293
Post-9/11	.744 [.437]	990 [1,414]	902 [855]	2,317
Raw Difference	.0336** (.0131)	29.3 (39.3)	33.5 (23.3)	4,610
Conditional Difference	.0116 (.0113)	-52.8*** (15.8)	42.3*** (15.7)	4,610
Muslim Inmates				
Pre-9/11	.723 [.449]	1,065 [1,402]	813 [511]	112
Post-9/11	.636 [.483]	848 [1,215]	1,029 [1,049]	110
Raw Difference	-.0869 (.0627)	-218 (176)	216* (111)	222
Conditional Difference	-.108* (.0554)	-245*** (79.6)	244*** (79.4)	222
DD Estimates: Post-9/11×Muslim				
Raw DD	-.12* (.0638)	-247 (180)	182 (113)	4,832
Conditional DD	-.12** (.0566)	-192** (81.2)	202** (81)	4,832
Conditional DD/ $\bar{Y}_{0,PRE}$	-.169** (.0797)	-.2** (.0845)	.232** (.0932)	4,832

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard deviations are presented in brackets, and Eicker-Huber-White standard errors in parentheses. “Raw DD” represents the unconditional regression estimates. The following controls are included in the “conditional DD” regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

We present our main results in Table 2.¹⁹ The DD coefficients we estimate are large, statistically significant, and economically meaningful. We find that the parole outcomes of Muslim inmates in Georgia were negatively impacted as a consequence of 9/11. Post 9/11, Muslim inmates are 12 percentage points less likely to be granted parole. Expressed in terms of the

¹⁹In order to probe our results, we conduct several sets of sensitivity analyses. In the Appendix C.1, we provide evidence that our results are robust to the inclusion of prisoner characteristics (Figure C1), rater fixed effects (Figure C2), and the width of the exclusion window which insures that individuals rated pre-9/11 are seen by the parole board pre-9/11 (Figure C3). In addition, in Appendix C.2 we explore heterogeneity of our main results by crime severity level (Table C1) and risk score group (Table C2).

pre-9/11, non-Muslim average (which we denote by $\bar{Y}_{0,PRE}$), this is a 17% reduction in the probability of receiving parole. The effect is statistically significant at the 5% significance level. This lower likelihood of parole translates in 194 fewer days on parole. Given that the average parole period for non-Muslim inmates in the pre-9/11 period is 961 days (or 2.6 years), our DD estimates amounts to a 20% reduction in time on parole. Finally, we present results for days in prison. In line with the parole time results, we find Muslim inmates spend over half a year longer in prison if their file is reviewed by the parole board after 9/11, a 23% increase compared to the reference sub-sample of non-Muslim inmates reviewed prior to 9/11. In relation to the literature, we note that our effect size is comparable to other studies that investigate the role of major terrorist attacks on Muslims or other minority groups (see Table A1). These outcomes include asylum status, criminal sentencing, and prosecution charges and range from 12-30% in effect size (Bielen and Grajzl, 2021; Brodeur and Wright, 2019; Emeriau, 2023; McConnell and Rasul, 2021).

To better comprehend the magnitude of this effect, we benchmark our DD estimates with the estimates from other control variables. Our estimate of the increase in days in prison for Muslim inmates whose cases were reviewed post-9/11 (202 days) is of the same magnitude as the average conditional difference in time served between a serial offender (eight or more prior convictions) and a first-time offender (192 additional prison days).

In order to further gauge the magnitude of our findings and test whether our results are an artifact of the small number of Muslim prisoners in our sample, we conduct a permutation test exercise involving random assignment of treatment status across inmates.²⁰ We run 500 placebo experiments, where we randomly assign Muslim status across inmates, and then conduct our baseline DD analysis.²¹ We present these results in Figure C5. We observe that our actual DD estimate on prison time is more positive and significant than all 500 placebo estimates. The estimates for parole granted and days on parole are more negative in magnitude and more statistically significant than almost all of the placebo estimates. These results make clear the (statistical) significance of our findings and ease concerns that our findings might be driven by the relatively small number of Muslim inmates in our estimation sample.

²⁰Given that this exercise follows the same approach as one would use to conduct randomization inference (RI), we display the RI *p*-value in brackets in the legend of each graph.

²¹In Figure C4 we present further evidence from an extension of the permutation test approach.

4.1 Longer-Run Effects

We next assess the extent to which our main DD estimates persist. To do so, we expand our sample by considering years from 1998 to 2005. We maintain similar sampling rules for defining the extended sample.²² We estimate a dynamic version of Equation (1) as follows:

$$y_{it} = \alpha Muslim_i + \sum_{\substack{t=1998, \\ t \neq 2000}}^{2004} \beta_t (Period_t \times Muslim_i) + X'_i \gamma + \theta_t + \pi_t + \epsilon_{it}, \quad (2)$$

where $Period_t$ denotes a year that starts on 9/11 of a given calendar year, t , and runs until 9/10 of the following calendar year, and θ_t are period fixed effects. We present the resulting estimates in the form of event study graphs in Figure 1.

For all three outcomes, we document a striking persistence of the short-run effects we detail in Section 4, namely the large declines in parole grants and days on parole and the corresponding increase in days in prison. For days paroled and days in prison, the long-run effects (i.e., estimates for the year 2004/2005) are statistically significantly different from zero at conventional levels (the associated p -values are respectively .027 and .059). For all outcomes, we cannot reject the null that the short- and long-run effects are equal. The persistence of the effects against Muslims we document contrasts with studies that have documented short-term impacts of 9/11 on hate crimes (Gould and Klor, 2016) and labor market outcomes (Kaushal et al., 2007).

5 Discussion

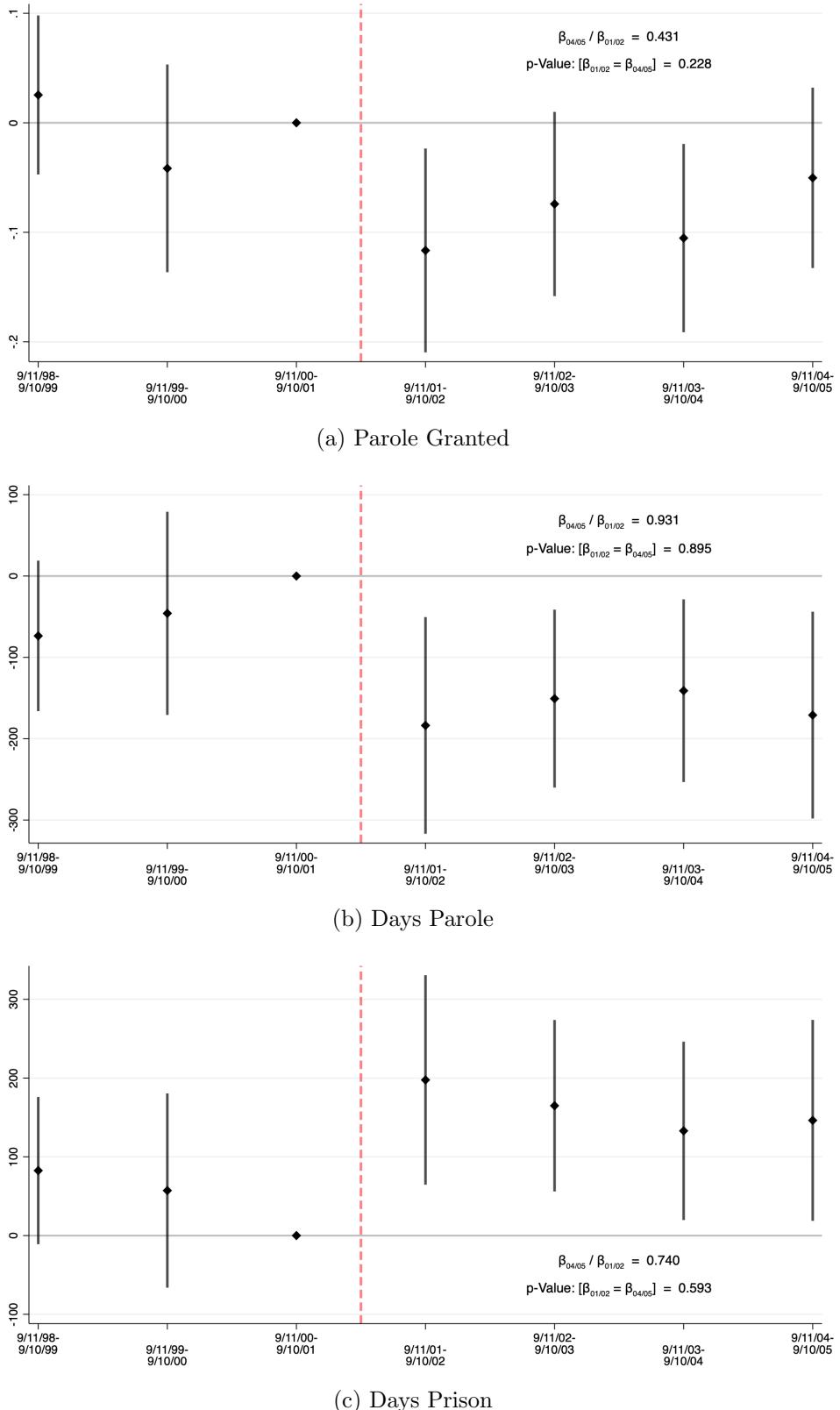
The size and persistence over time of our estimates begs the question: what drives these results? To begin, we perform a Juhn, Murphy, and Pierce style decomposition and show that observable characteristics are unable to explain the disparity generated by 9/11 (see Figure C6). While disparities do not always entail discrimination if there are characteristics that may be observable to the parole board but not to the econometrician, the decomposition and the setting provide strong support for discrimination underlying our findings.²³ The results so far suggest that the new disparity in parole outcomes generated by 9/11 is likely driven by discrimination on the part of the parole board. Inmate characteristics do not appear to explain the results and other in-prison processes also do not seem to have changed.

To shed light on the kind of animus that might underlie parole board choices, we investigate

²²Specifically, we implement the same sample selection procedure that we do for our core sample for each year in our extended sample. This does mean that for later years, we may include inmates who are sentenced post-9/11.

²³In Table C3, we do not find statistically significant evidence that in-prison disciplinary outcomes have changed in response to 9/11.

Figure 1: Dynamic Effects for Parole Outcomes



Notes: The lines indicate the 90% confidence intervals of the point estimates, represented as diamonds. The time variable, displayed on the x -axis, shows the date range of when the inmate was rated. The omitted time period is the year prior to 9/11/01, and comprises inmates rated between 9/11/00 and 9/10/01. Regression specifications include the follow control variables: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

the extent to which our results vary by recidivism risk.²⁴ Based on the predicted recidivism risk, we compute recidivism risk quartiles. Using these quartiles, we conduct a further set of heterogeneity analysis – we estimate a triple difference version of our baseline specification, where the third difference is recidivism risk quartile. We plot the quartile-specific DD estimates against quartile-specific baseline recidivism risk. The plots can be found in Figure 2. We find suggestive evidence in support of heterogeneous effects by recidivism risk factors, where days on parole (in prison) are statistically significantly lower (higher) for high-risk Muslims compared to low-risk Muslims post-9/11. Parole probability is not significant due to wider standard errors but the pattern of the point estimates is similar.

5.1 Discrimination

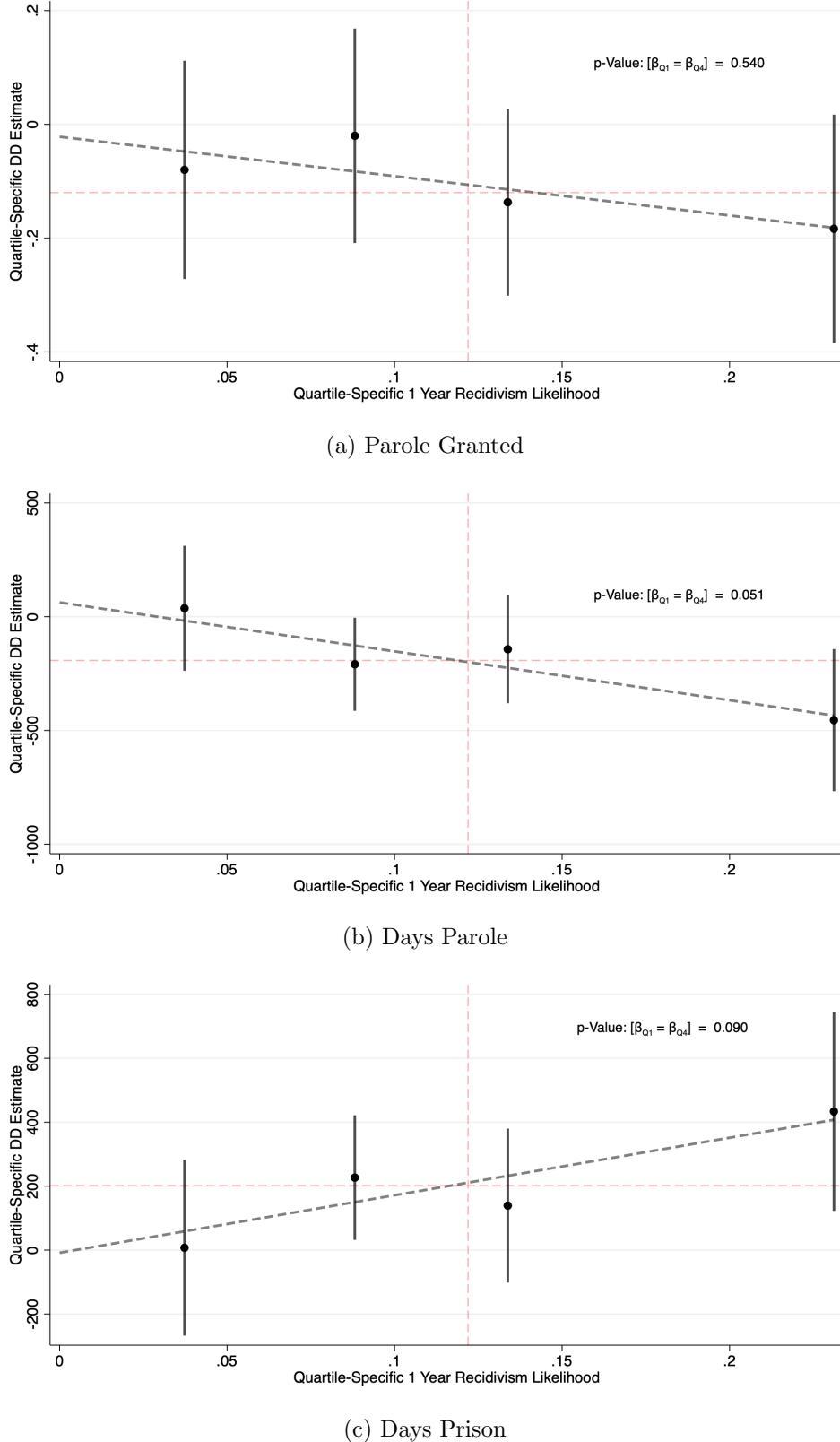
Understanding the type of discrimination at play is challenging and difficult. There are three major categories of discrimination that have been discussed in the literature. The first two are widely known in economics, and consist of taste-based (or preference-based) discrimination, as well as statistical-based discrimination (Lang and Spitzer, 2020). The third, that has had much more visibility in the other social sciences, is institutional discrimination (Small and Pager, 2020). An important commonality, across all types of discrimination, is how notoriously difficult it is to identify and quantify discrimination. On top of the ever-pervasive omitted variables problem that plagues any comparison between groups (Heckman, 1998), further issues such as the well-known inframarginality problem (Ross and Yinger, 2002) are also formidable challenges.

In our setting, the institutional component is unlikely to be at play as the exogenous shock of 9/11 did not immediately alter institutional details of the parole decision making process. Increases in religious disparities of parole outcomes could be due to more prejudicial views regarding Muslim inmates by parole board members after 9/11. Such views would depend on the parole board members being able to observe the religious status of inmates (Rose, 2023), which they do in the rating files that they receive. Taste discrimination may also be heterogeneous with respect to other characteristics of Muslim inmates, and persistent, hence consistent with our heterogeneity by recidivism risk results (Brock et al., 2012; Arnold et al., 2018).

On the other hand, a statistically-based explanation would posit that parole board members update their beliefs regarding the relative recidivism risks of Muslim inmates after 9/11. The

²⁴We employ a LASSO logit model using the adaptive lasso selection method to predict recidivism risk based on a rich data of observable crime-related and inmate socio-demographic characteristics. We show the full set of predictors as well as those selected by the LASSO logit model in Figure C7. Table C4 presents the one-year recidivism risk for different groups of former inmates who were released at least one year prior to 9/11.

Figure 2: Sub-group DD Estimates Versus Sub-group Baseline Recidivism Risk



Notes: Each point represents the quartile-specific DD estimate, and the solid lines the 90% confidence intervals. The lines of best fit are based on OLS (thick, dashed line), where each point is inverse weighted by the variance of the quartile-specific DD estimate. Within the graph, a *p*-value is presented based on a test of equality of parameter estimates for the first and fourth quartiles. Regression specifications include the follow control variables: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

parole board members could have raised their expectations of recidivism risk for Muslim inmates if they believed that the broader social climate for Muslims post-9/11 worsened, making it more likely for Muslim inmates to recidivate.²⁵ This includes potentially social discriminatory factors such as anti-Muslim sentiment in the labor market, policing rates, harshness of judges, and others, all contributing to heightened recidivism risk. If the conditions Muslim inmates face upon release are persistently worse than before 9/11, this would be consistent with our results. We search for evidence that such concerns apply to our sample by looking for changes in labor market outcomes in the county of release in response to 9/11 (Appendix C.7).²⁶ We also perform an analysis of the names of Muslims in our sample (Appendix C.8) to see how salient their Muslim identity might be in a post-9/11 world.²⁷ Our results, which document no differences in home-county unemployment rate (Table C5) and no differences in the Muslim-soundingness of Muslim inmates' names who are reviewed post-9/11 (Table C6), suggest that neither of these dimensions are likely important drivers of the unwarranted disparities we document in Section 4.

Additionally, we note that even if the data does not support a statistical discrimination mechanism, it is difficult to rule out the presence of *inaccurate* statistical discrimination. The parole board may have incorrect priors about Muslim recidivism risk that are inconsistent with the data and is distinct from taste-based discrimination as a motivation. However, to tease these forms of discrimination apart would require data on the beliefs of parole board members which we do not have.

6 Conclusion

Using administrative data from the state of Georgia, and a difference-in-differences approach, we provide the first evidence of how Muslims in the criminal justice system were affected by the terrorist attacks of 9/11. Outcomes worsen for some Muslims reviewed for parole in the aftermath of 9/11 – these inmates are 17% less likely to be granted parole, and consequently spend 200 additional days in prison, on average – a 23% increase from baseline. The effects on parole outcomes are strongly persistent up to 2005, 4 years after the attacks. We further document that the Muslim inmates reviewed post-9/11 with higher ex-ante recidivism risk experience the largest falls in parole likelihood, and greatest increase in prison time. Taken together with the

²⁵We note that even if discrimination were statistical-based, this is arguably an unjustifiable penalty on the Muslim inmates as it reflects broader social discrimination in the decision making as a factor of consideration for parole.

²⁶We take this approach instead of a mediation analysis to avoid the risk of bad controls.

²⁷Figure C8 provides initial evidence that there is little difference in the names of Muslim and non-Muslim inmates.

dynamic effects, we suggest that risk-based factors likely played a sizeable role in the responses of the parole board.

In terms of the external validity of our findings, we believe our results are generalizable beyond the specific context of study. First, group-based decision-making occurs at other stages of the criminal justice system, notably the U.S. Supreme Court. Group-based decision-making also occurs in other aspects of life – in the labor market, both in the private sector where multiple interviewers may decide on whom to hire, in academia, where members of the department rank job market candidates, and in sport, where, for example, a fixed group of judges will score gymnasts, figure skaters or boxers. Second, the parole system in Georgia operates in a similar way to other parole systems in states that use discretionary parole as their main mechanism of releasing inmates from prison.

Our work has policy implications for the optimal design of criminal justice systems. Specifically, our findings point to a potentially under-appreciated aspect of parole boards – their flexibility to respond to external, post-sentencing events. If there are large, external shocks – such as the one that we study in this work – the parole board has leeway to change their decision-making in response, in a potentially discriminatory fashion. This is very different from a purely judicial-based system, where there is little scope to respond to external shocks post-sentencing.

It is worth noting that even if the board made choices based on real increases in recidivism risk among Muslim inmates, these increases in risk may be driven by broader discrimination that in turn may be undesirable. Our paper highlights the open question of whether the criminal justice system should reflect and buttress potential societal discrimination as a part of their duty. Any debate regarding how to reform the criminal justice system, in order to make it both more efficient and more equitable, should thus consider the nuanced role of a parole board system, given the response that we document in this paper.

References

- ABADIE, A. AND J. GARDEAZABAL (2003): “The economic costs of conflict: A case study of the Basque Country,” *American economic review*, 93, 113–132.
- ANWAR, S. AND H. FANG (2015): “Testing for racial prejudice in the parole board release process: Theory and evidence,” *The Journal of Legal Studies*, 44, 1–37.
- ARNOLD, D., W. DOBBIE, AND P. HULL (2022): “Measuring racial discrimination in bail decisions,” *American Economic Review*, 112, 2992–3038.
- ARNOLD, D., W. DOBBIE, AND C. S. YANG (2018): “Racial bias in bail decisions,” *The Quarterly Journal of Economics*, 133, 1885–1932.
- ASH, E., S. ASHER, A. BHOWMICK, D. L. CHEN, T. DEVI, C. GOESSMANN, P. NOVOSAD, AND B. SIDDIQI (2021): “Measuring gender and religious bias in the indian judiciary,” *Center for Law & Economics Working Paper Series*, 2021.
- BARON, E. J., J. J. DOYLE JR, N. EMANUEL, P. HULL, AND J. P. RYAN (2023): “Racial Discrimination in Child Protection,” *National Bureau of Economic Research*.
- BIELEN, S. AND P. GRAJZL (2021): “Prosecution or Persecution? Extraneous Events and Prosecutorial Decisions,” *Journal of Empirical Legal Studies*, 18, 765–800.
- BINDLER, A. AND R. HJALMARSSON (2019): “Path dependency in jury decision making,” *Journal of the European Economic Association*, 17, 1971–2017.
- BLOMBERG, S. B., G. D. HESS, AND A. ORPHANIDES (2004): “The macroeconomic consequences of terrorism,” *Journal of monetary economics*, 51, 1007–1032.
- BLUNDELL, R. AND M. C. DIAS (2009): “Alternative approaches to evaluation in empirical microeconomics,” *Journal of Human Resources*, 44, 565–640.
- BODDIE, S. C. AND C. FUNK (2012): “Religion in prisons: A 50-state survey of prison chaplains,” in *Pew Forum*, Pew Research Center Washington, DC.
- BROCK, W. A., J. COOLEY, S. N. DURLAUF, AND S. NAVARRO (2012): “On the observational implications of taste-based discrimination in racial profiling,” *Journal of Econometrics*, 166, 66–78.
- BRODEUR, A. AND T. WRIGHT (2019): “Terrorism, immigration and asylum approval,” *Journal of Economic Behavior & Organization*, 168, 119–131.

- CHATURVEDI, R. AND S. CHATURVEDI (2023): “It’s All in the Name: A Character-Based Approach to Infer Religion,” *Political Analysis*, 1–16.
- CORNELISSEN, T. AND U. JIRJAHN (2012): “September 11th and the earnings of Muslims in Germany—The moderating role of education and firm size,” *Journal of Economic Behavior & Organization*, 81, 490–504.
- DAVILA, A. AND M. T. MORA (2005): “Changes in the earnings of Arab men in the US between 2000 and 2002,” *Journal of Population Economics*, 18, 587–601.
- DAVIS, D. W. (2007): *Negative liberty: Public opinion and the terrorist attacks on America*, Russell Sage Foundation.
- EMERIAU, M. (2023): “Victim or Threat? Shipwrecks, Terrorist Attacks, and Asylum Decisions in France,” *American Journal of Political Science*.
- EREN, O. AND N. MOCAN (2018): “Emotional judges and unlucky juveniles,” *American Economic Journal: Applied Economics*, 10, 171–205.
- FEIGENBERG, B. AND C. MILLER (2022): “Would eliminating racial disparities in motor vehicle searches have efficiency costs?” *The Quarterly Journal of Economics*, 137, 49–113.
- FLANAGAN, F. X. (2018): “Race, gender, and juries: Evidence from North Carolina,” *The Journal of Law and Economics*, 61, 189–214.
- GLOVER, D. (2021): “Job search and intermediation under discrimination: Evidence from terrorist attacks in France,” *Working paper*.
- GODFREY, J., K. T. K. TAN, AND M. ZAPRYANOVA (2022): “The Effect of Parole Board Racial Composition on Prisoner Outcomes,” *Working paper*.
- GONCALVES, F. AND S. MELLO (2021): “A few bad apples? Racial bias in policing,” *American Economic Review*, 111, 1406–41.
- GOULD, E. D. AND E. F. KLOR (2016): “The long-run effect of 9/11: Terrorism, backlash, and the assimilation of Muslim immigrants in the West,” *The Economic Journal*, 126, 2064–2114.
- HAMM, M. S. (2007): “Terrorist Recruitment in American Correctional Institutions: An Exploratory Study of Non-traditional Faith Groups; Final Report,” Indiana State University, Department of Criminology, <https://www.ojp.gov/pdffiles1/nij/grants/220957.pdf>.

HECKMAN, J. J. (1998): “Detecting Discrimination,” *The Journal of Economic Perspectives*, 12, 101–116.

IVANDIC, R., T. KIRCHMAIER, AND S. J. MACHIN (forthcoming): “Jihadi attacks, media and local hate crime,” *Journal of Law and Economics*.

JUHN, C., K. M. MURPHY, AND B. PIERCE (1993): “Wage inequality and the rise in returns to skill,” *Journal of political Economy*, 101, 410–442.

KAUFMAN, S. B. (2019): “The criminalization of Muslims in the United States, 2016,” *Qualitative Sociology*, 42, 521–542.

KAUSHAL, N., R. KAESTNER, AND C. REIMERS (2007): “Labor market effects of September 11th on Arab and Muslim residents of the United States,” *Journal of Human Resources*, 42, 275–308.

KUSHA, H. R. (2016): *Islam in American prisons: black Muslims' challenge to American penology*, Routledge.

KUZIEMKO, I. (2013): “How should inmates be released from prison? An assessment of parole versus fixed-sentence regimes,” *The Quarterly Journal of Economics*, 128, 371–424.

LAFOREST, M. (2022): “Racial Bias, Gender Bias, and the Effects of Parole Officers on Reentry,” *Working Paper*.

LANG, K. AND A. K.-L. SPITZER (2020): “Race Discrimination: An Economic Perspective,” *The Journal of Economic Perspectives*, 34, 68–89.

LEPAGE, L.-P. (2023): “Discrimination and sorting in the real estate market: Evidence from terrorist attacks and mosques,” *European Economic Review*, 153, 104386.

LUDWIG, J. AND S. MULLAINATHAN (2021): “Fragile algorithms and fallible decision-makers: lessons from the justice system,” *Journal of Economic Perspectives*, 35, 71–96.

MCCONNELL, B. AND I. RASUL (2021): “Contagious Animosity in the Field: Evidence from the Federal Criminal Justice System,” *Journal of Labor Economics*, 39, 739–785.

MECHOULAN, S. AND N. SAHUGUET (2015): “Assessing racial disparities in parole release,” *The Journal of Legal Studies*, 44, 39–74.

OUDKERK, B. AND D. KAEBLE (2019): “Probation and parole in the United States, 2019,” *Washington, DC: US Department of Justice*.

- PANAGOPOULOS, C. (2006): “The Polls-Trends: Arab and Muslim Americans and Islam in the aftermath of 9/11,” *Public Opinion Quarterly*, 70, 608–624.
- PHILIPPE, A. AND A. OUSS (2018): ““No hatred or malice, fear or affection”: Media and sentencing,” *Journal of Political Economy*, 126, 2134–2178.
- RAMBACHAN, A. AND J. ROTH (2022): “A More Credible Approach to Parallel Trends,” Tech. rep., Working Paper.
- RENAUD, J. (2019): “Grading the parole release systems of all 50 states,” *Prison Policy Initiative*, https://www.prisonpolicy.org/reports/parole_grades_table.html.
- ROSE, E. K. (2023): “A Constructivist Perspective on Empirical Discrimination Research,” *Journal of Economic Literature*, 61, 906–23.
- ROSS, S. L. AND J. YINGER (2002): *The Color of Credit: Mortgage Discrimination, Research Methodology, and Fair-Lending Enforcement*, The MIT Press.
- ROTH, J. (2022): “Pretest with Caution: Event-Study Estimates after Testing for Parallel Trends,” *American Economic Review: Insights*, 4, 305–22.
- SHAYO, M. AND A. ZUSSMAN (2011): “Judicial ingroup bias in the shadow of terrorism,” *The Quarterly journal of economics*, 126, 1447–1484.
- SHERIDAN, L. P. (2006): “Islamophobia pre-and post-September 11th, 2001,” *Journal of interpersonal violence*, 21, 317–336.
- SINGH, A. (2002): ” We are Not the Enemy”: *Hate Crimes Against Arabs, Muslims, and Those Perceived to be Arab Or Muslim After September 11*, vol. 14, Human Rights Watch.
- SLOAN, C. (2019): “Racial bias by prosecutors: Evidence from random assignment,” in *ICCJ 2019: International Conference on Criminal Justice June*, 25–26.
- SMALL, M. L. AND D. PAGER (2020): “Sociological Perspectives on Racial Discrimination,” *The Journal of Economic Perspectives*, 34, 49–67.
- WOODS, J. (2011): “The 9/11 effect: Toward a social science of the terrorist threat,” *The Social Science Journal*, 48, 213–233.
- ZAPRYANOVA, M. (2020): “The effects of time in prison and time on parole on recidivism,” *The Journal of Law and Economics*, 63, 699–727.

Online Appendix

A Literature, Sample Selection and the Parole Process in Georgia

Table A1: Impact of terrorists events on criminal-justice outcomes

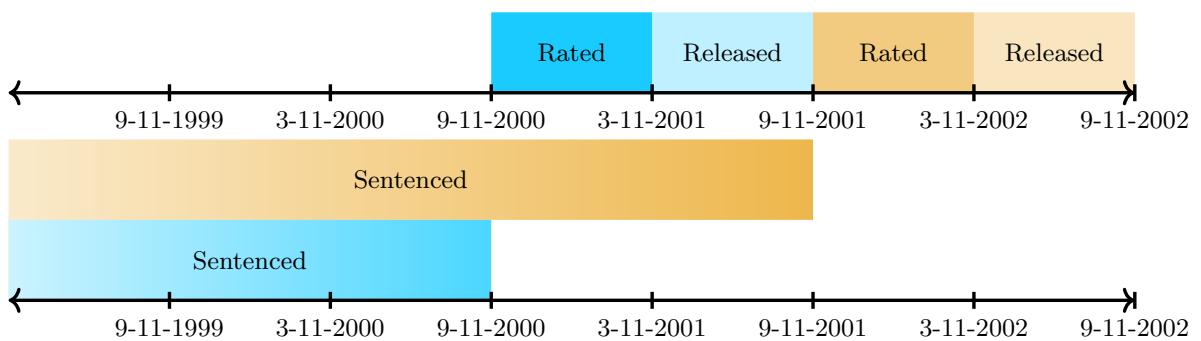
Author (Year)	Size of Ef- fect	Outcome	Treatment Group	Event	Country
Emeriau (2024)	-12.20%	Probability of granting refugee sta- tus	Any asylum seekers	Migrant shipwrecks and terrorist attacks	France
McConnell and Rasul (2021)	-30.40%	Probability of a down- ward depa- ture from sentencing guidelines	Hispanic	9/11 Terror- ist attacks	US
Brodeur and Wright (2019)	-19%	Probability of granting asylum	Asylum seekers com- ing from Muslim- majority countries	9/11 Terror- ist attacks	US
Bielen and Grajzl (2021)	20.76%	Probability that a charge against suspect is prosecuted	Muslim	An extremist murdering a filmmaker known for his critique of Islam	Netherlands

Table A2: Discretionary Parole Board Systems in US and Georgia

Parole System Characteristic	Mean	Std. dev.	US		Georgia
			Min	Max	
Would mandate face-to-face hearings	0.561	0.446	0	1	0
Would provide method to challenge incorrect information	0.273	0.452	0	1	0
Prohibits input from prosecutors	0.621	0.434	0	1	0.5
Prohibits input from crime survivors	0.727	0.282	0	1	1
Would allow input from applicant, family, community, employers, prison admin	0.545	0.289	0	1	0
Employs presumptive parole policies	0.197	0.248	0	0.5	0
Does not deny parole for subjective reasons	0.727	0.282	0	1	0.5
Would mandate yearly reviews	0.409	0.404	0	1	0
Would provide case managers to assist individuals	0.227	0.397	0	1	0
Would provide individuals with access to all records	0.333	0.389	0	1	0
Would incorporate parole guidelines	0.333	0.27	0	1	1
Would require parole board to file yearly report to an oversight committee	0.394	0.496	0	1	0
Would have meaningful appeal process	0.53	0.432	0	1	0
Prison Policy Initiative overall score	38	29	0	83	42
Number of members on the Parole Board	7	2.25	3	13	5

Notes: Data on the number of members on the Parole Board in each state was collected from each state's Parole Board website. We were not able to retrieve information on the size of the Parole Boards in Alaska and Maryland. All other data comes from Renaud (2019). Overall score is a weighted average of each of the characteristics calculated by Prison Policy Initiative. All other characteristics are graded on the scale 0-0.5-1, where 0 stands for no, 0.5 for partially, and 1 for yes. The states included in the US average are states that offer discretionary parole, namely, Alabama, Alaska, Arkansas, Colorado, Connecticut, Hawaii, Idaho, Iowa, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New York, North Dakota, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, West Virginia, and Wyoming.

Figure A1: Sample Selection Schematic



Notes: This figure shows a timeline of the sample restrictions applied to the treated group (orange) and control group (blue). All prisoners in the treated group are sentenced before 9-11-2001, rated between 9-11-2001 and 3-11-2002, and released between 3-11-2002 and 9-11-2002. All prisoners in the control group are sentenced before 9-11-2000, rated between 9-11-2000 and 3-11-2001, and released between 3-11-2001 and 9-11-2001.

B Identifying Assumptions

In this section, we present supportive evidence for (i.) the common trends assumption and (ii.) stability of group composition over time. These are the two core identifying assumptions of a repeat cross section difference-in-differences approach.

B.1 Parallel Trends

We provide three pieces of evidence in support of the parallel trends assumption inherent in our DD approach. Each piece of evidence approaches the topic of parallel trends from a different perspective. Each piece of evidence provides support that the parallel trends assumption holds for our empirical specification in the sample period under consideration.

B.1.1 Placebo DDs

We first implement a set of placebo DD regressions. We shift all key dates one year back in time, and re-estimate Equation 1, with the sole difference that now the $Post_t$ term takes value zero for the period 11 September 1999–10 September 2000, and one for the period 11 September 2000–10 September 2001. We present the results in Table B1. Given the absence of any significant placebo DD parameters, we consider the placebos as the first piece of evidence in support of the parallel trends assumption.

Table B1: Parole Board Decisions and Prisoner Outcomes

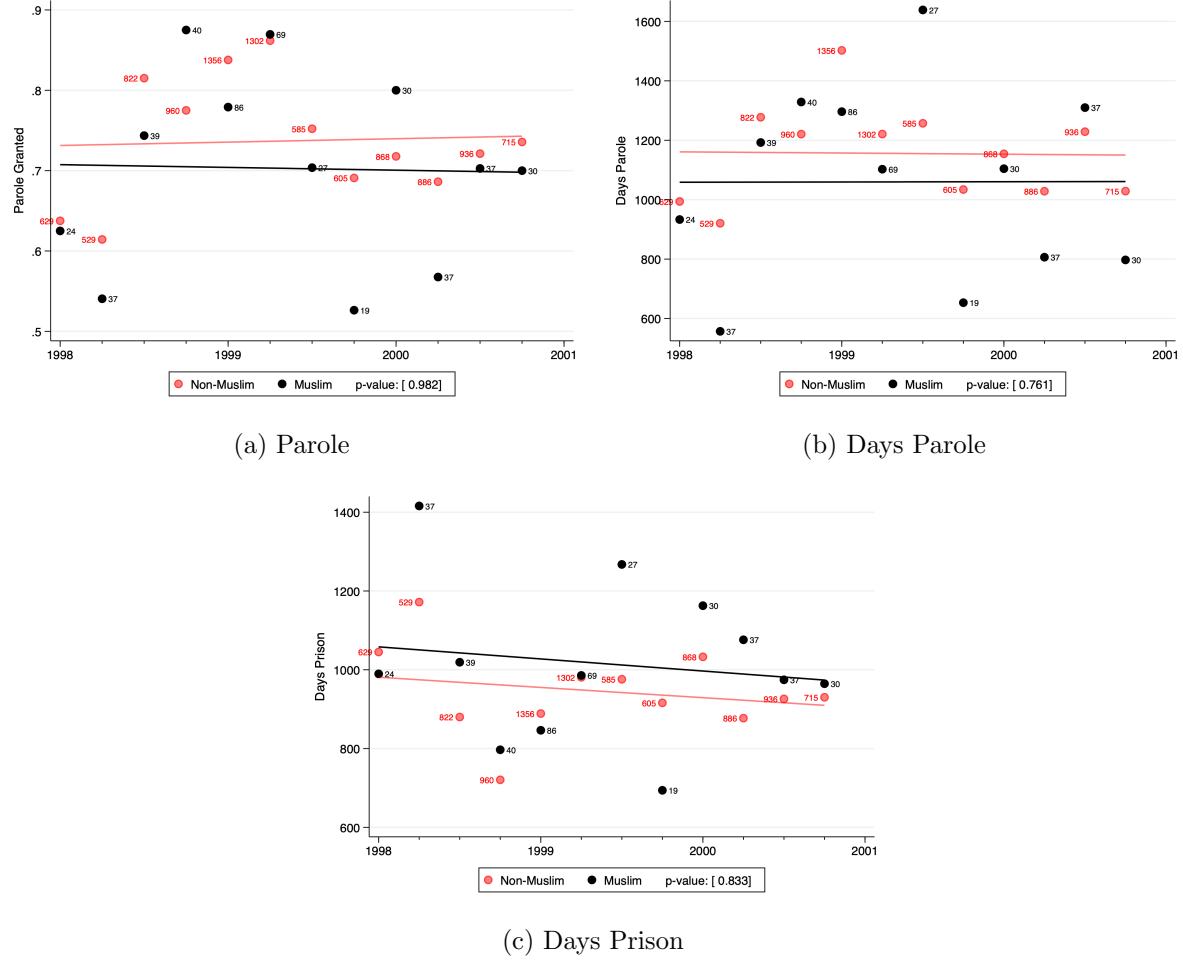
	(1)	(2)	(3)
	Parole Granted	Days Parole	Days Prison
Post-9/11×Muslim	.0416 (.0552)	49.1 (74.9)	-59.4 (74.1)
$\bar{Y}_{0,PRE}$.714	1011	882
(Post-9/11×Muslim) / $\bar{Y}_{0,PRE}$.0583 (.0773)	.0486 (.0741)	-.0673 (.084)
Adjusted R^2	.319	.873	.606
Observations	5,031	5,031	5,031

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. The following controls are included in all regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

B.1.2 Trends in the Raw Data

Next, in Figure B1, we provide graphical evidence of the existence of pre-trends by presenting the raw, underlying data for three years prior to our estimation sample – the calendar years of 1998-2000. We present the p -values from a test of equality of trends. We cannot reject this null of equality of trends in any case – the smallest p -value is .76.

Figure B1: Raw Pre-Trends



Notes: The p -value presented in the legend of each graph is based on a test of equality of trends between Muslim and non-Muslim inmates at the individual level using pooled data, with Eicker-Huber-White standard errors.

B.1.3 Honest Difference-in-Differences

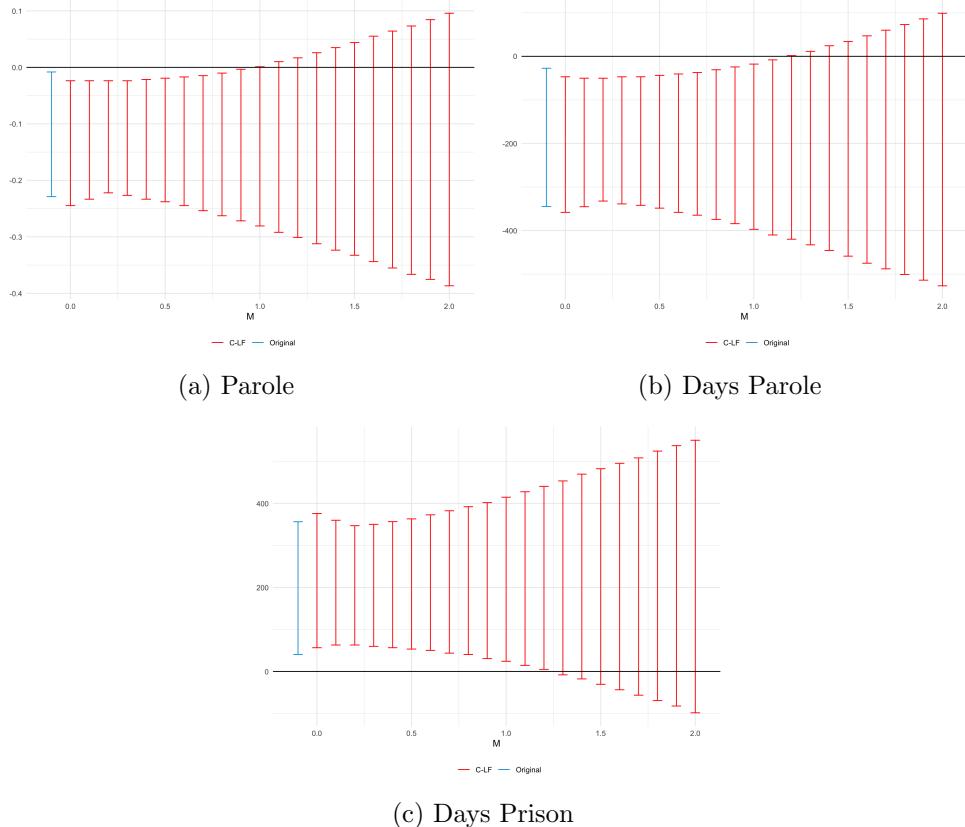
Finally, we implement the honest difference-in-differences approach of Rambachan and Roth (2022), in order to create worst-case treatment effect bounds for potential violations of the parallel trends assumption, based on pre-trends.

In order to operationalize this approach, we use data on those who come before the parole board between September 11, 1999 and September 10, 2002 , and create 3 periods: 1. An initial period of those up for parole between September 11, 1999 and September 10, 2000 – the year prior to the pre-period used in the main analysis, 2. the pre-period of those inmates reviewed between September 11, 2000 and September 10, 2001 and 3. the post-period of September 11, 2001 and September 10, 2002. We then implement a continuous treatment and binary treatment version of our core DD model, but based on the extended data and a 3 period approach, as follows:

$$y_{it} = \alpha_0 Muslim_i + \sum_{j=1, \neq 2}^3 \alpha_j Period_j + \sum_{j=1, \neq 2}^3 (\beta_j Period_j \times Muslim_i) + X'_i \gamma + \pi_m + \epsilon_{it}, \quad (3)$$

The graphical outputs from the Rambachan and Roth (2022) approach, where we use the Relative Magnitude approach for bounding, are presented in Figure B2. For all three outcomes, the “breakdown value” of \bar{M} – the factor of the pre-trends at which the bounds on the estimated treatment effect overlap with zero – exceeds 1. This means that even if post 9/11 violations of parallel trends were as large as any pre-period violations, the confidence set for the treatment effects would not include zero.

Figure B2: Honest Difference-in-Differences



Notes: The blue band (“Original”) is the 90% confidence interval of the DD treatment effect estimate. The red bands (“C-LF”) are the robust 90% confidence intervals for the Rambachan and Roth (2022) Relative Magnitude-based bounds. These vary with the x-axis – \bar{M} – which designates factors of the maximum pre-treatment violation of parallel trends. Thus a confidence interval that does not intersect 0 when $\bar{M} = 1$ informs us that when we allow any parallel trend violations in the post-period to be as large as the maximum pre-treatment violation, the 90% confidence intervals for the bounded treatment effect do not include zero.

B.2 Stable Group Composition

B.2.1 Duration Analysis

We implement a series of duration model regressions, where the key duration variable is the time from prison admission to rate date. We present these in Table B2. The purpose of this analysis is twofold. First, this analysis contributes to the evidence in support of the assumption of group stability required for identification when using repeat cross-sectional data. Second, and arguably more importantly, it could show evidence that there is no *strategic reordering* of Muslim inmates post-9/11. There might be a concern that the parole board reorders inmates in order to see more Muslim inmates straight after 9/11, earlier than expected. If this were the case, we would expect the duration from prison entry to rating date to be *shorter*, not longer. In addition, we had a secondary strategic reordering concern regarding the board bringing forward cases of Muslim inmates with more serious offenses, or offenses more closely aligned with the terrorist attacks. If this were the case, we would find evidence of this imbalance of inmate characteristics in our balancing exercise (Table 1). Although the small sample size is a challenge to precisely estimate the results in Table B2, we believe they provide suggestive evidence that strategic reordering is unlikely.

Table B2: Duration Analysis – (Rate Date - Admission Date)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unconditional				Full Covariate Set			
	OLS	Cox	Gompertz	Weibull	OLS	Cox	Gompertz	Weibull
Post-9/11×Muslim	.0913 (.142)	-.134 (.134)	-.146 (.153)	-.0119 (.233)	.147 (.127)	-.173 (.14)	-.221 (.135)	-.251 (.222)
$\bar{Y}_{0,PRE}$ (days)	304	304	304	304	304	304	304	304
Observations	4,826	4,826	4,826	4,826	4,826	4,826	4,826	4,826

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses, regular p-values in brackets. The outcome variable in all cases is the duration from prison admission date until rate date – the start of the parole process. The exceptions to this are columns 1 and 5, where the outcome variable is the natural log of duration. Logs were taken to deal with the extreme right skew of the data. For these proportional hazard models we present coefficients and not hazard rates. For the Cox proportional hazard model, the Gompertz and the Weibull based models, a negative coefficient means a lower hazard rate, and thus a longer duration. For the Log Logistic and Weibull based models, we specify gamma frailty. The following controls are included in all regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

B.2.2 Balance

In Table B3 we present the results of a series of balance tests for the longer-run results.

Table B3: Longer-term Balance Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	
	9/11/1998-9/10/1999			9/11/1999-9/10/2000			9/11/00-9/10/01 (Base Period)			9/11/2001-9/10/2002			9/11/2002-9/10/2003			9/11/2003-9/10/2004			9/11/2004-9/10/2005		
	C	T	p	C	T	p	C	T	C	T	p	C	T	p	C	T	p	C	T	p	
Sample Size	3,748	198		2,548	80		2,291	112	2,318	110		2,799	133		3,060	107		2,762	130		
Education:																					
≤ High School	.668	.53	[.0025]	.652	.575	[.112]	.649	.688	.674	.591	[.0601]	.681	.526	[.00201]	.679	.701	[.815]	.68	.569	[.017]	
High School	.241	.222	[.47]	.246	.188	[.971]	.249	.196	.235	.245	[.273]	.224	.188	[.753]	.222	.15	[.714]	.225	.254	[.117]	
Some College	.0803	.202	[.00861]	.0856	.225	[.0232]	.0869	.0982	.0777	.155	[.134]	.0815	.248	[.0011]	.0859	.131	[.489]	.0807	.162	[.127]	
College	.0107	.0455	[.0912]	.0169	.0125	[.648]	.0153	.0179	.0134	.00909	[.677]	.0136	.0376	[.299]	.0127	.0187	[.835]	.0148	.0154	[.934]	
Social Class:																					
On Welfare	.0835	.116	[.307]	.0891	.0875	[.942]	.0947	.0893	.0975	.127	[.414]	.103	.128	[.478]	.0908	.0841	[.997]	.115	.0846	[.505]	
Occasionally Employed	.0667	.0909	[.222]	.0891	.138	[.14]	.0781	.0625	.0673	.0727	[.57]	.0625	.0902	[.205]	.0592	.103	[.136]	.0644	.0462	[.814]	
Min. Living Standard	.485	.429	[.755]	.451	.438	[.804]	.468	.438	.494	.409	[.432]	.467	.429	[.919]	.499	.467	[.972]	.484	.462	[.873]	
Middle Class	.346	.359	[.55]	.347	.313	[.235]	.34	.384	.325	.373	[.931]	.35	.338	[.37]	.334	.318	[.372]	.32	.392	[.652]	
Unknown	.0187	.00505	[.192]	.0243	.025	[.826]	.0192	.0268	.016	.0182	[.739]	.0171	.015	[.626]	.0167	.028	[.852]	.0159	.0154	[.699]	
I.Q. Score	93.6	99	[.158]	93.6	98.7	[.257]	94	96	95.2	96.1	[.608]	92.4	97.4	[.268]	86.6	82.2	[.109]	91.7	98.2	[.0707]	
	(20.1)	(20.2)		(20.7)	(16.1)		(22.7)	(18.9)	(19.8)	(21.6)		(24.9)	(20.9)		(32)	(37.7)		(22.8)	(17.8)		
Has Children	.667	.641	[.362]	.669	.75	[.46]	.674	.705	.67	.618	[.211]	.627	.669	[.856]	.67	.654	[.475]	.667	.731	[.65]	
Married	.114	.0859	[.0234]	.115	.0875	[.0396]	.111	.179	.103	.0818	[.0482]	.0975	.158	[.819]	.107	.0841	[.0517]	.106	.108	[.122]	
Prior Convictions	2.15	2.81	[.962]	2.15	3.09	[.465]	2.05	2.7	2.3	2.57	[.357]	2.08	2.92	[.614]	2.16	3.21	[.416]	2.32	2.82	[.574]	
	(2.66)	(3.16)		(2.57)	(3.28)		(2.52)	(2.97)	(2.63)	(2.84)		(2.58)	(2.94)		(2.67)	(3.38)		(2.68)	(2.7)		
Age at Sentencing	30.9	30.3	[.452]	31.3	30.9	[.406]	30.9	29.5	30.8	28.8	[.699]	30.6	30.8	[.131]	30.9	32	[.0664]	32	31.7	[.471]	
	(9.14)	(9.01)		(9.48)	(7.92)		(9.59)	(8.77)	(9.59)	(7.65)		(9.66)	(8.56)		(9.58)	(10.1)		(9.75)	(9.36)		
Risk Score	9.38	8.94	[.253]	9.92	9.28	[.569]	9.89	8.81	9.45	8.7	[.533]	10	9.4	[.39]	9.72	8.54	[.916]	9.36	9.23	[.0434]	
	(4.01)	(4.14)		(3.88)	(4.53)		(3.8)	(3.68)	(3.77)	(3.64)		(3.88)	(3.83)		(3.85)	(4)		(3.79)	(4.04)		
Severity Level	2.82	2.82	[.682]	2.99	2.71	[.212]	2.87	2.94	2.8	2.7	[.489]	3.06	3.03	[.641]	3.06	2.7	[.0681]	3	3.12	[.733]	
	(1.82)	(1.84)		(1.74)	(1.88)		(1.69)	(1.61)	(1.59)	(1.62)		(1.73)	(1.74)		(1.71)	(1.84)		(1.69)	(1.75)		
Sentence Length	2263	2215	[.512]	2060	2006	[.447]	1977	2052	2054	2049	[.797]	2242	2272	[.82]	2536	2066	[.0307]	2494	2618	[.888]	
	(1727)	(1742)		(1757)	(1671)		(1512)	(1524)	(1759)	(1697)		(2016)	(2264)		(2079)	(1818)		(2217)	(2168)		
Major Offense Group:																					
Violent/Sexual	.28	.308	[.721]	.264	.275	[.842]	.241	.268	.21	.218	[.681]	.304	.316	[.726]	.227	.206	[.467]	.236	.269	[.812]	
Property	.334	.389	[.14]	.323	.438	[.0544]	.332	.313	.339	.391	[.283]	.309	.331	[.447]	.288	.495	[.00088]	.312	.338	[.527]	
Drugs/DUI	.306	.242	[.283]	.338	.237	[.102]	.344	.348	.367	.273	[.153]	.319	.263	[.359]	.398	.206	[.00141]	.37	.308	[.282]	
Other	.0795	.0606	[.873]	.0754	.05	[.632]	.0834	.0714	.0846	.118	[.266]	.0682	.0902	[.392]	.0876	.0935	[.652]	.0811	.0846	[.695]	
Joint Test		[.038]			[.247]						[.774]			[.186]			[.222]			[.5]	
Joint Test																					
Exc. Education		[.108]			[.203]						[.816]			[.652]			[.108]			[.503]	

Notes: Means and standard deviations (in parentheses for continuous covariates) are shown. We present the *p*-values for the DD term from an OLS regression with Eicker-Huber-White standard errors.

B.3 Differential Conversions to Islam as a Threat to Identification

Recent research suggests that prison conversions are common among Muslim inmates (Boddie and Funk, 2012; Hamm, 2007; Kusha, 2016), which our measure for religion would not capture. This could be a potential threat to our identification strategy, as our results could be driven by differential rates of in-prison conversion to Islam in the post-9/11. If fewer conversions occur post-9/11, and Muslim converts in our control group pre-9/11 were discriminated against, then parole outcomes may improve in the post-9/11 control group where there may have been fewer converts, negatively biasing our DD estimates for parole outcomes. If in the wake of 9/11, some would-be converters to Islam do not convert, then we will have a differential composition of converters in our non-Muslim-at-entry control group. Recall, we observe religious affiliation at the point of entry into prison, but we do not observe in-prison conversion.²

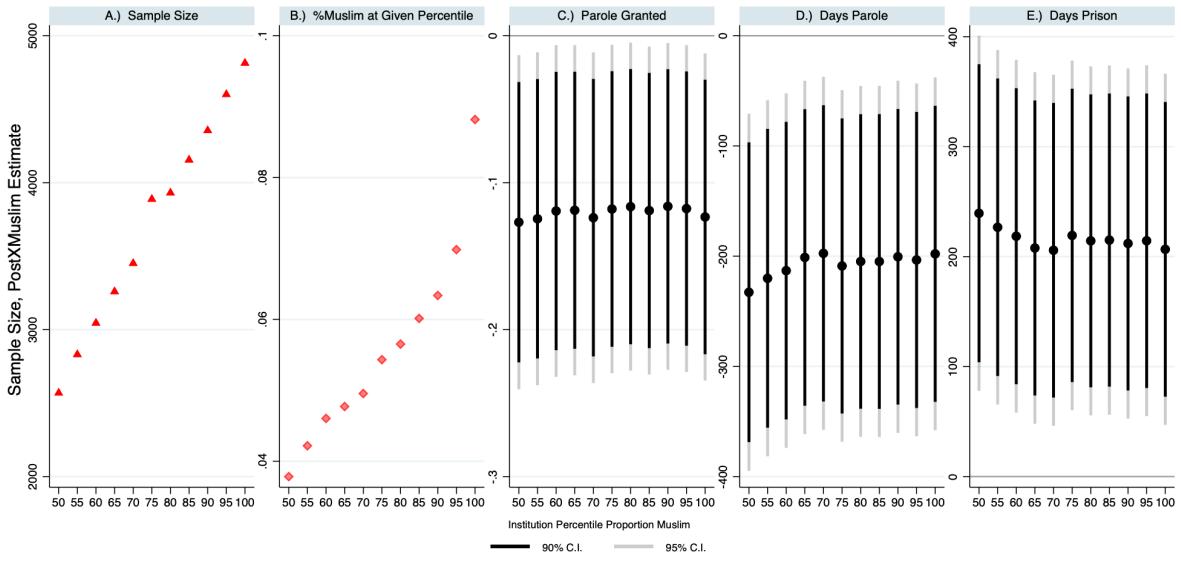
In order to address this data shortcoming we take the following approach. First, we assume that if an inmate who is non-Muslim at entry decides to convert to Islam, the inmate is more likely to do so at a high Muslim-at-entry concentration prison than a low concentration prison. This assumption is based on several considerations – a higher Muslim concentration prison will both (i) provide more opportunities for social interactions that spur a conversion and (ii) provide a more supportive network for practice of the religion (Hamm, 2007).

Accordingly, we construct the proportion of Muslim inmates at each prison facility from the stock of inmates in the 9/11 period and then create (inmate-weighted) percentiles of the proportion of Muslim inmates.³ We create these percentiles for the non-Muslim-at-entry control group. We next run a series of regressions for our three main outcome variables, sequentially removing non-Muslim-at-entry inmates in the top p% of prison facilities by proportion of Muslim inmates, reporting the results of this sensitivity exercise in Figure B3 below. The first panel of the figure displays the sample size for the regression, once we remove non-Muslim-at-entry inmates from the top p% of prisons. The lower the percentile, the more we reduce the sample. The second panel displays the average proportion of % Muslim at the given percentile. The remaining three panels display DD estimates and respective confidence intervals for our three main outcomes. We can remove non-Muslim-at-entry inmates at the top 50% of Muslim concentration prisons, reducing our working sample size by half, and see no statistically or economically meaningful change in our parameter estimates. We take these results as strong evidence that differential conversion to Islam in the post-9/11 period is not driving our core findings.

²Furthermore, inmates' religion is reported only for their last imprisonment spell.

³The mean proportion of Muslim inmates for our core sample is .042. The maximum is .103.

Figure B3: DD Estimates by Muslim Prison Composition



Notes: Panel A presents the total sample size when using data less than percentile p . Panel B presents the value of %Muslim inmates in the institution for the given percentile p . Panels C, D and E display the point estimate, 95% and 90% confidence intervals for Post-9/11 \times Muslim when we exclude the top p percentiles of institutions by %Muslim inmates. Thus, for example, the estimates corresponding to percentile 80 are based on our core estimation sample where prisons with the top 20% of proportion Muslim inmates have been removed. The DD regression specifications that yield parameter estimates in Panels C, D and E include the following control variables: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

C Robustness and Ancillary Results

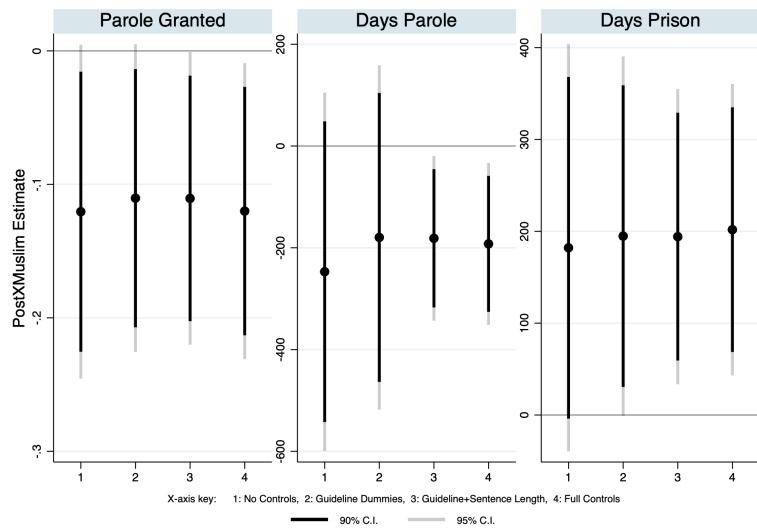
C.1 Robustness checks

In order to probe our results, we conduct several sets of sensitivity analyses. First, in Figure C1, we progressively include more covariates starting from specification 1 (no control variables) and finishing with specification 4 (full set of controls, and our baseline specification). The coefficients are extremely stable across specifications. Reassuringly, the DD estimates are stable to the inclusion of the controls.

Second, we also investigate whether the raters could have played a role in our main results. While raters have somewhat limited scopes for impacting the parole outcome as their job is primarily to compile information for the board, we check to see if controlling for rater effects could change our estimates. We show that the inclusion of rater fixed effects allowing for changes to the rater effect post 9/11 do not affect our results (see Figure C2). Given the lack of any key decision-making roles for the raters, this is as expected.

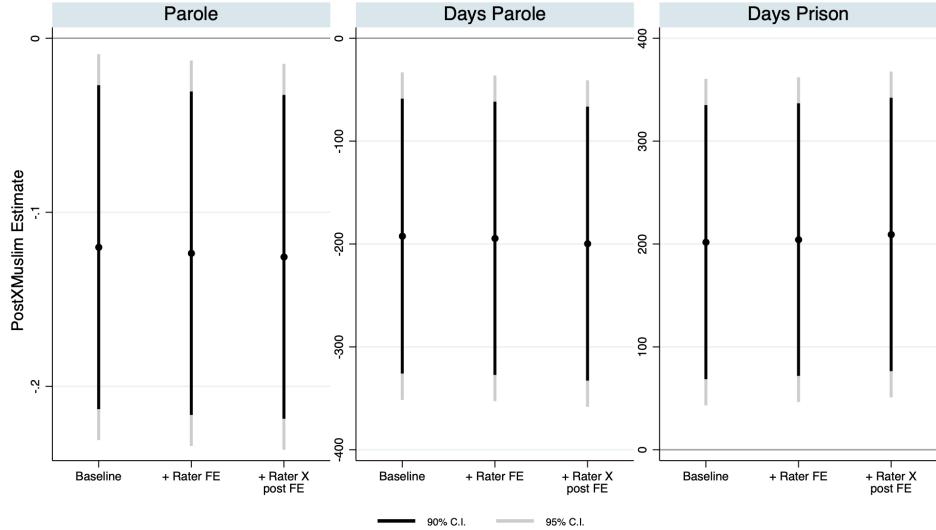
Finally, in Figure C3 we consider the sensitivity of our estimates to the width of the exclusion window that we specify, in order to ensure those individuals rated pre-9/11 are seen by the parole board pre-9/11. The wider the exclusion window, the smaller the sample size. The first panel of Figure C3 quantifies this intuition. The results are broadly stable until we force the exclusion window to be 8 months or wider, due primarily to the loss of sample size. We focus on cases that have a rate date within a $+/-1$ year window around 9/11. We enforce a buffer/exclusion window towards the end of the window, as the parole board is unlikely to see a case on the rate date. Our baseline is to specify a 6 month exclusion window, the cost of which is to effectively halve our sample size. The benefit is that we can be fairly certain that all cases with rate dates in the pre-period are indeed seen by the parole board in the pre-period. For cases that are seen after, we will misallocate post cases as pre cases. The consequence of this will be to attenuate our treatment effect. The estimates show precisely this – with too short an exclusion window we attenuated our treatment effect – many of the cases with rate dates close to, but before, 9/11/01 are likely reviewed by the majority of the parole board in the post-period. Parameter estimates are broadly stable and monotonically decrease for the parole outcomes and increase for the prison outcome. This pattern breaks when we exclude too large a proportion of our sample – Excluding more than 240 of the possible 365 days reduces the sample size sufficiently that parameter estimates become erratic.

Figure C1: Robustness of the DD Estimates to the Sequential Inclusion of Controls



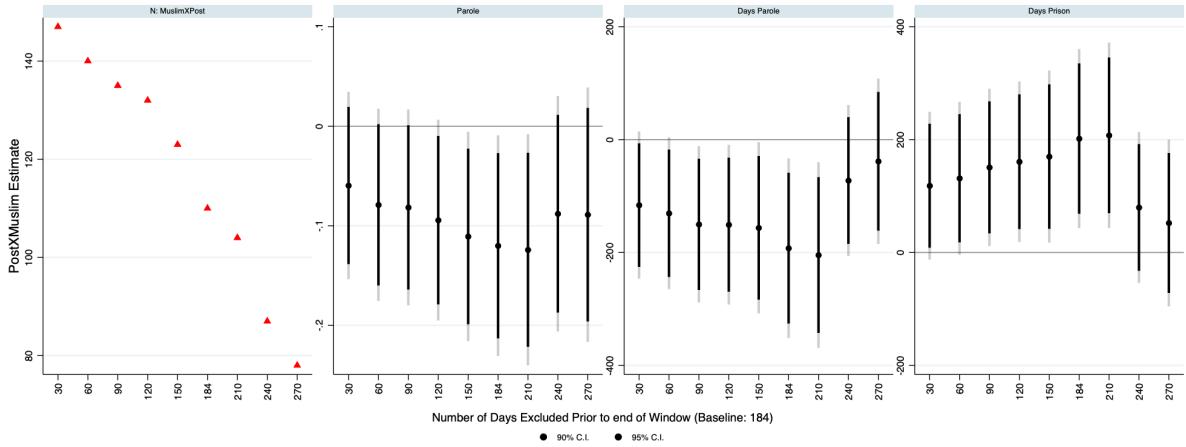
Notes: We present four, numbered specifications. Specification 1 includes no other controls, and thus yields unconditional DD estimates. Specification 2 includes a set of Parole Decision Guideline cell dummies. Specification 3 additionally includes as a control sentence length in days. Specification 4, our baseline specification includes sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

Figure C2: Rater sensitivity analysis



Notes: We present three different specifications for our core parole board outcomes. The points and lines are respectively coefficient estimates and confidence intervals. Specification 1, our baseline specification includes sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. Specification 2 is identical to specification 1, but includes rater fixed effects. Specification 3 is identical to specification 1, but includes rater-by-post period fixed effects. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

Figure C3: Robustness of the DD Estimates to Different Lengths of the Exclusion Window



Notes: Panel A presents the number of Muslim inmates in the post-9/11 period for each exclusion window. Panels B, C and D display the point estimate, 95% and 90% confidence intervals for Post-9/11 \times Muslim based on the exclusion restriction range shown on the x-axis. 184 days is our baseline choice. The DD regression specifications that yield parameter estimates in Panels B, C and D include the following control variables: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

C.2 Short Run Impacts – Treatment Effect Heterogeneity

Table C1: Parole Board Decisions and Prisoner Outcomes by Offense Severity

	(1)	(2)	(3)	(4)	(5)	(6)
	Low-Severity Offenses			High-Severity Offenses		
	Parole Granted	Days Parole	Days Prison	Parole Granted	Days Parole	Days Prison
Post-9/11 × Muslim	-.161** (.0647)	-181** (88.5)	177** (88.3)	-.0316 (.103)	-177 (177)	229 (180)
$\bar{Y}_{0,PRE}$.794	1056	671	.482	704	1404
(Post-9/11 × Muslim) / $\bar{Y}_{0,PRE}$	-.203** (.0815)	-.171** (.0838)	.264** (.131)	-.0656 (.213)	-.252 (.252)	.163 (.128)
Adjusted R^2	.161	.888	.355	.385	.765	.644
Observations	3,619	3,619	3,619	1,213	1,213	1,213

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. The offense severity level is one of the two inputs that form the parole board grid. Low-Severity Offenses are those with an offense severity level of 1-4. High Severity Offenses are those with an offense severity level of 5 and above. The following controls are included in all regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

We assess the extent to which our main DD estimates mask treatment effect heterogeneity. To do so, we split the sample into low and high severity offenses⁴ and re-estimate our baseline specification on the two sub-samples. We present our findings in Table C1 and we find that the large decline in parole grants we document in Table 2 is driven by Muslim inmates convicted of low severity offenses. Muslim inmates with low severity offense who are reviewed for parole post-9/11 are 16 percentage points, or 20%, less likely to be granted parole. This translates into an average prison sentence that is 26% longer. For the sub-group of inmates convicted of high severity offenses, our DD estimate for parole is considerably smaller, and statistically indistinguishable from zero. The extensive margin effects of 9/11 seem to be concentrated among the low severity inmates, while intensive margin effects seem more similar across inmate types. The difference in the point estimates for parole probability are interesting and large in magnitude but our small sample of Muslim inmates lead to relatively large standard errors so that we are unable to statistically distinguish the impacts across the two groups. We observe similar results when we explore heterogeneity based on grid risk score as shown in Table C2.

⁴Following Kuziemko (2013) we choose the low severity offenses as offense levels 1-4, and high severity offenses as 5 and above.

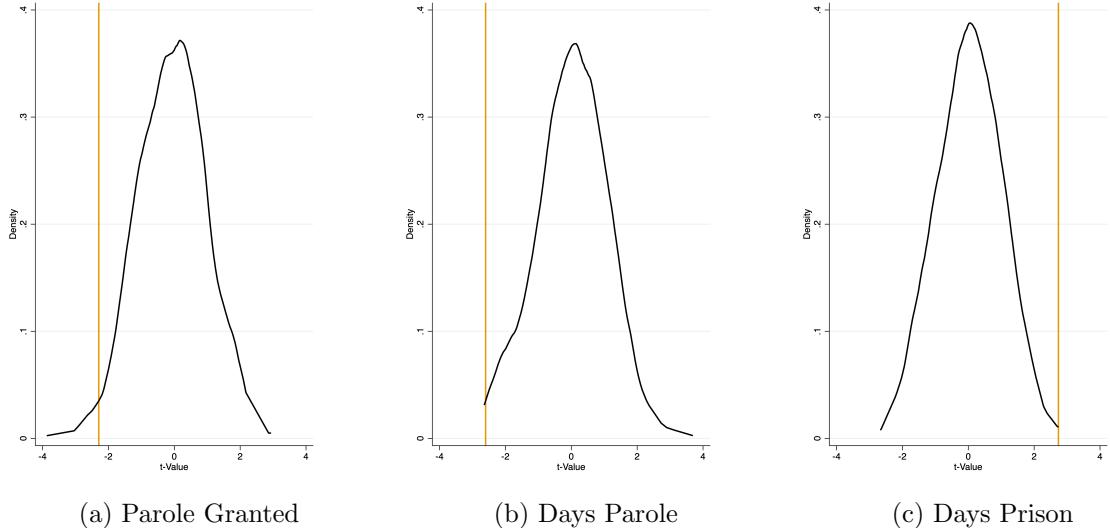
Table C2: Parole Board Decisions and Prisoner Outcomes by Grid Score Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Low Risk			Medium Risk			High Risk		
	Parole Granted	Days Parole	Days Prison	Parole Granted	Days Parole	Days Prison	Parole Granted	Days Parole	Days Prison
(Post-9/11 × Muslim)	-.162 (.154)	-103 (234)	145 (243)	-.0626 (.0788)	-165 (123)	162 (120)	-.177** (.0882)	-192* (114)	206* (113)
$\bar{Y}_{0,PRE}$.741	973	899	.742	978	820	.655	935	912
(Post-9/11 × Muslim) / $\bar{Y}_{0,PRE}$	-.219 (.207)	-.106 (.24)	.161 (.27)	-.0843 (.106)	-.169 (.126)	.197 (.146)	-.271** (.135)	-.206* (.122)	.226* (.124)
Adjusted R^2	.384	.775	.601	.291	.879	.528	.254	.82	.547
Observations	844	844	844	2,056	2,056	2,056	1,932	1,932	1,932

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. Success score, a measure of recidivism risk of an inmate, is one of the two inputs that form the parole board grid. Success scores are grouped in three groups: low risk (14 to 20 success points), medium risk (9 to 13 success points), and high risk (less than 8 success points). The following controls are included in all regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

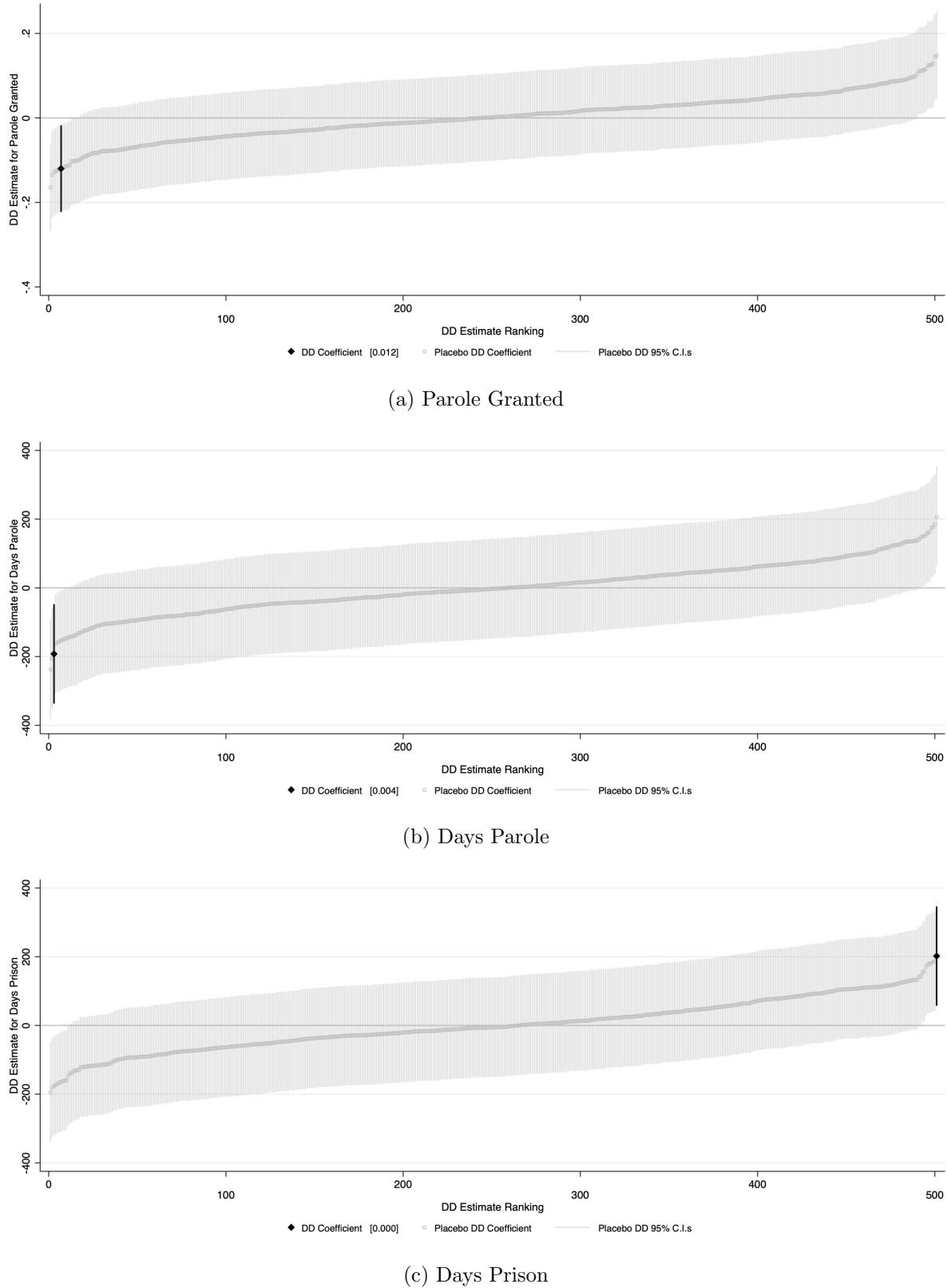
C.3 Permutation Test Results

Figure C4: Permutation t -Statistic Distribution



Notes: Each graph presents the t -statistic on the DD term from 10,0001 regressions – one regression based on true Muslim status, and 10,000 placebo regressions where Muslim status is randomly assigned across inmates. Random assignment of Muslim status is conducted to reflect the proportion of the sample who are Muslim and non-Muslim.

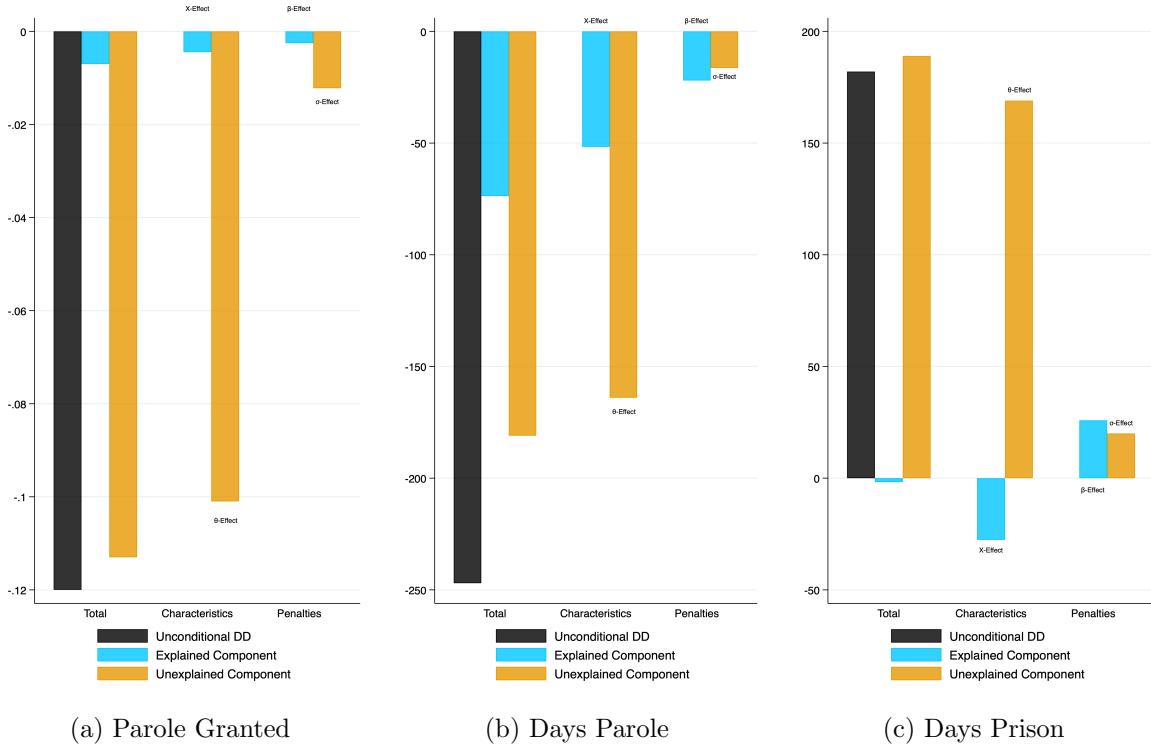
Figure C5: Permutation Test Results



Notes: Each graph presents the coefficient estimates and 95% confidence intervals from 501 regressions – one regression based on true Muslim status, and 500 placebo regressions where Muslim status is randomly assigned across inmates. Random assignment of Muslim status is conducted to reflect the proportion of the sample who are Muslim and non-Muslim. Given that the placebo regressions form the basis of how one would conduct randomization inference, we display randomization inference p -value in brackets at the bottom of each graphs. This is one way to reflect the finding that for our 3 outcomes (Parole Granted, Days Parole, Days Prison) only 6,2 and 0 of the 500 placebo estimates respectively exceed our true DD estimate. We present the density of t -statistics from our actual regression and 10,000 placebo regressions.

C.4 Decomposition of Parole Outcome Differentials

Figure C6: Juhn et al. (1993) Decomposition of Parole Outcome Differentials



Notes: The graphs show the key output from a Juhn et al. (1993) decomposition for our core analysis sample of +/-365 day window around 9/11/2001. We use non-Muslim inmates as the reference group, and pre-9/11 as the base period, which maps to our core DD specification. The procedure decomposes the unconditional difference-in-differences for our three main parole board outcomes. We use our standard set of control variables in this decomposition: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. For the total DD, the explained component represents what can be explained using our control variables, the unexplained component the remainder. We further decompose the unconditional DD into (i.) an explained characteristics term (based on changes in X – the X -effect), (ii.) an explained penalty term (based on changes in the penalties associated with X – the β -effect), (iii.) an unobserved quantities term, which reflects changes of Muslim inmates in the residual distribution of non-Muslim inmates (the θ -effect), and (iv.) an unobserved penalty term, which relates to changes in the residual variance (the σ -effect).

C.5 Disciplinary Outcomes

In order to understand the effect of 9/11 on parole board decision making, it is important for us to consider margins along which other actors may influence parole outcomes. Due to our empirical design, we rule out the role of sentences by focusing only on cases where sentencing occurs prior to 9/11. However, inmates may change their behavior post-9/11, or prison guards may apply disciplinary rules in a disparate manner post-9/11. If this were the case, then such behavioral changes would contribute to our estimated treatment effects.

We implement our main DD strategy using data on infractions that led to reports (incident was recorded, but did not result in a charge) or charges, and present the results in Table C3.⁵ We find no evidence of any statistically significant changes in disciplinary outcomes for Muslim inmates in the post-9/11 period. Although these results are noisy and we cannot reject large increases in infractions post 9/11, they provide suggestive evidence that disciplinary infractions whilst incarcerated are unlikely to explain the magnitude of our main results.⁶

Table C3: Disciplinary Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Disciplinary Count				Any Disciplinary (Binarized)			
	Reports		Charges		Reports		Charges	
	Total	Violent	Non-Violent	Total	Total	Violent	Non-Violent	Total
Post-9/11×Muslim	1.21 (.973)	.3 (.238)	1.19 (1.26)	1.5 (1.44)	.0339 (.0555)	.0361 (.0625)	.0157 (.0566)	.034 (.0555)
$\bar{Y}_{0,PRE}$	3.4	.441	4.23	4.69	.629	.215	.61	.63
$(Post-9/11 \times \text{Muslim}) / \bar{Y}_{0,PRE}$.354 (.286)	.679 (.539)	.282 (.298)	.319 (.307)	.0539 (.0883)	.168 (.291)	.0257 (.0928)	.054 (.0881)
Adjusted R^2	.21	.109	.194	.191	.156	.099	.154	.157
Observations	4,832	4,832	4,832	4,832	4,832	4,832	4,832	4,832

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. The following controls are included in all regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

⁵In order to increase power, we also re-estimate these results using the sample from a longer time horizon in order to increase the number of Muslim prisoners in the sample. We find similar conclusions using this approach – disciplinary outcomes are somewhat above zero, but are still not statistically significant.

⁶The data does not allow us to identify the source (prisoner own behavior, peers, or the prison guards) for these potential increases.

C.6 Ex-Ante Recidivism

Figure C7: Ex-Ante Recidivism Risk Predictors



Notes: Average marginal effects and corresponding 95% confidence intervals are presented from a LASSO-logit regression where the dependent variable is an indicator for recidivating in the year after release. We present the full set of potential explanatory variables. Those not selected by the LASSO are distinguished by an 'X'.

Table C4: Sub-Group Recidivism Risk

	(1)	(2)
	Recidivism Risk	Sub-Group Size
A.) Full Sample	0.122	15075
B.) Offense Severity		
Low	0.147	10848
High	0.056	4227
C.) Predicted Recidivism Quartile		
1	0.036	3700
2	0.088	3699
3	0.137	3700
4	0.230	3699

Notes: Predicted recidivism is based on 1 year recidivism probabilities for sample released 9/11/1998-9/10/2000

C.7 Local Labor Market Conditions

We apply our framework to county-level unemployment rates for the county that inmates are released to. We find that the counties that Muslim inmates were released to did not exhibit worse local labor market conditions. However, this does not preclude the possibility of discrimination against Muslims occurring, but rather a measure of overall economic conditions that Muslim inmates are facing.

Table C5: The Local Labor Market Conditions of Inmates Home County at the Time of Parole Decision

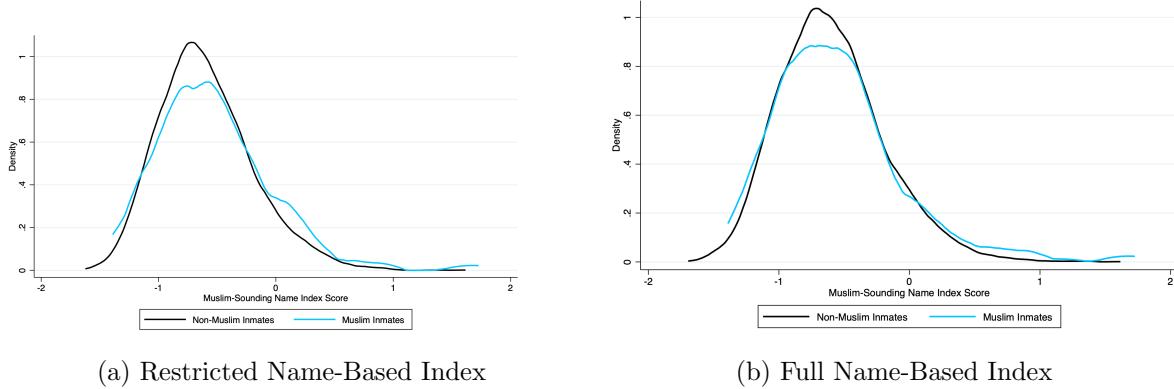
	(1)	(2)	(3)
	County Unemployment Rate (%)		
	Education Sub-Samples		
	Full Sample	Less Than High School	High School Graduate and Above
Post-9/11×Muslim	-.0596 (.107)	-.0507 (.133)	-.154 (.197)
$\bar{Y}_{0,PRE}$	4.26	4.3	4.19
(Post-9/11×Muslim) / $\bar{Y}_{0,PRE}$	-.014 (.0252)	-.0118 (.0309)	-.0367 (.0469)
Observations	4,784	3,160	1,624

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. The dependent variable is the county unemployment rate in the year the inmate's parole file is reviewed. Columns 2 and 3 present sub-sample analysis, where we split the full sample by highest level of education (less than High School, High School and above) The following controls are included in all regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.

C.8 Muslim-Sounding Name Analysis

We use the name-based classifier from Chaturvedi and Chaturvedi (2023) in order to assign a Muslim-sounding name score to each individual inmate. The Chaturvedi and Chaturvedi (2023) approach uses a training dataset from India, and a linear LVM classifier. We find that our sample of Muslim inmates do not have different sounding names than non-Muslims in general. This suggests that the likelihood of discrimination outside of prison is low, given that names are typically one of the major indicators for Islamic faith for men.

Figure C8: The Distribution of the Muslim-Sounding Name Index



Notes: We present the distribution of Muslim-sounding name scores using the name-based classifier from Chaturvedi and Chaturvedi (2023). Figure C8a presents scores based on restricted names (first name plus surname only) for non-Muslim and Muslim inmates separately. Figure C8b repeats the exercise, but based on full names (first, middle and last names).

Table C6: Muslim-Sounding Name Analysis

	(1)	(2)	(3)	(4)
	No Controls		Full Set of Controls	
	Restricted Name	Full Name	Restricted Name	Full Name
Post-9/11×Muslim	.0375 (.0675)	-.00039 (.0685)	.0383 (.068)	.0044 (.0689)
$\bar{Y}_{0,PRE}$	-.596	-.597	-.596	-.597
(Post-9/11×Muslim) / $\bar{Y}_{0,PRE}$	-.0629 (.113)	.00066 (.115)	-.0642 (.114)	-.00738 (.116)
Observations	4,832	4,832	4,832	4,832

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. The dependent variable is the underlying Muslim-sounding index score from the linear LVM classification exercise. Restricted Name indicates the use of a single first name and a surname only. Full Name indicates the use of all first, middle and last names. Columns 2 and 3 presents sub-sample analysis, where we split the full sample by highest level of education (less than High School, High School and above). The following controls are included in all regressions: sentence length in days, dummies for Parole Decision Guideline cell, rate date month, education level, IQ quintiles, having children, being married, age at sentencing deciles, major offense categories, social class groupings and number of prior convictions, where the maximum category is 8+. A +/-365 day window around 9/11/2001 is used for estimation. We exclude those rated within 180 days prior to 9/11/2001 (pre) and 9/11/2002 (post), the latter restriction to ensure sample balance. We require all inmates to be sentenced prior to 9/11/2000 (pre) or 9/11/2001 (post), the former restriction to ensure sample balance.