Avoiding Prejudice: Islamophobia and the Labor Supply of Arabs and Muslims in the U.S.*

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Abstract

This paper provides direct evidence that members of a disfavored group adjust their economic behavior in response to prejudice. We exploit plausibly exogenous, localized increases in anti-Arab and anti-Muslim sentiment generated by combat fatalities of U.S. service members from a given state during the wars in Iraq and Afghanistan between 2001 and 2014. Linking detailed records on military fatalities with survey data, we estimate the causal effect of these shocks on the labor supply of Arab and Muslim men residing in the affected states. Following a home-state fatality, Arab and Muslim men reduce their weekly hours of work by approximately one percent relative to the baseline sample mean. The reduction is twice as large among the self-employed and concentrated in occupations requiring frequent customer or co-worker interaction. These patterns are consistent with avoidance-based sorting predicted by prejudice-based models of discrimination. The timing of the response coincides with short-lived increases in anti-Muslim hate crimes, confirming that the estimated effects reflect shifts in prejudice rather than broader economic conditions. Our findings provide direct evidence that prejudice can distort market outcomes by inducing targeted individuals to withdraw from interactions with prejudiced market participants.

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1 Introduction

Ever since Becker's (1971) seminal work, economists have relied heavily on prejudice-based models to analyze market discrimination against disfavored groups.[1] In these models, some subset of market actors—employers, customers, or coworkers—harbor animus toward a group, generating equilibria in which its members may receive lower returns than otherwise identical individuals. Whether such discrimination arises, and how large it is, depends on equilibrium sorting: the avoidance of transactions between prejudiced actors—especially the most biased—and members of the disfavored group.

Theory suggests that sorting occurs on both sides of the market. Prejudiced actors experience disutility from contact with the disfavored group and will avoid such interactions unless compensated; in turn, disfavored individuals seek to transact with the least prejudiced actors they can.[2] Previous work shows that unexplained wage gaps across U.S. labor markets—between Blacks and Whites (Charles and Guryan, 2008) and between White men and women (Charles et al., 2025)—align more closely with the prejudice of the marginal rather than average discriminator, as predicted by prejudice-based models. These patterns, however, provide only indirect evidence of the avoidance-based sorting mechanism. This paper provides *direct* evidence on whether individuals respond to rising prejudice by avoiding economic transactions with those who hold it.

An ideal test of avoidance-driven sorting would identify an exogenous shock that shifts prejudice against a disfavored group across markets and over time, and then assess whether, relative to unaffected groups, members of that group reduce market interactions following the shock—especially where the shock is larger or adjustment easier. We implement this test by examining how Arabs and Muslims in the United States respond to localized, plausibly exogenous shocks to prejudice.

Arabs and Muslims are among the fastest-growing minority groups in the U.S., and evidence suggests they face substantial bias. A 2016 Pew survey found that 59 percent of

[1] Statistical discrimination models (Phelps 1972; Aigner and Cain 1977) emphasize imperfect information. Other explanations attribute group differences to unobserved skill variation (O'Neill 1990; Neal and Johnson 1996).

[2] The most prejudiced person with whom they interact is the "marginal discriminator," whose prejudice sets the equilibrium level of discrimination.

Americans believe Muslims face "a lot" of discrimination (Pew Research Center, 2016).[3] Amer and Hovey (2012) report that more than half of Arab Americans exhibited symptoms of major depression in 2004, attributing this to "profiling, discrimination, and biased anti-Arab media" (p. 415). Other surveys document similar experiences of bias and prejudice.[4] Yet systematic study of discrimination against this group remains limited relative to the large literature on Blacks, Hispanics, and other minorities, leaving open both whether Arab/Muslim discrimination exists and what form it takes.[5] Examining how Arabs and Muslims (henceforth, "Arabs/Muslims") respond to shocks to prejudice provides a direct test of the avoidance mechanism and helps distinguish prejudice-based mistreatment from statistical discrimination.

Between 2001 and 2014, more than 6,500 U.S. soldiers died while serving in Afghanistan and Iraq, both majority-Muslim countries. Using data from the Defense Manpower Data Center (DMDC), we observe the exact date and home state of each fatality. We argue that a soldier's death plausibly triggered a temporary rise in anti-Arab and anti-Muslim sentiment within the home state, as residents learned of the loss of one of their own in a conflict with a Muslim-majority country. Such sentiment shifts could have led Arabs and Muslims in that state to reduce or avoid interactions with non-Muslims during periods of heightened prejudice. Because the deployment and timing of combat fatalities are not related to the states of origin of the soldiers, these events should not influence broader labor market conditions.[6] Thus, any observed change in labor market choices between Arabs and Muslims in the home state of the fallen soldier can be interpreted as a response to an exogenous shock to local prejudice.

Our empirical analysis combines individual labor-market data from the Current Population Survey (CPS) with detailed information on U.S. military fatalities in Afghanistan and Iraq. The CPS identifies first- and second-generation Arab and Muslim men (our primary

[3] Respondents were asked, "In the U.S. today, is there a lot of discrimination against Muslims or not?"

[4] See data from the Center for American Progress (Ali et al., 2011).

[5] See Bayer et al. (2025) for a review of groups studied in discrimination literature.

[6] Operational needs and world events determine where, when and how U.S. military units are deployed (Lyle 2006; Engel et al. 2010; Cesur et al. 2013). Soldiers can indicate a preference for a particular division, but subsequent assignments (for instance, to a particular brigade or company) are, conditional on rank and occupation, independent of personal characteristics and preferences, including the state in which he or she was recruited (Lyle 2006; Engel et al. 2010; Cesur et al. 2013).

study group) based on respondents' and parents' countries of birth. We link these records to fatalities of soldiers from each respondent's state of residence and estimate how Arab/Muslim men's hours worked respond to these localized, time-specific shocks to prejudice.

Across a range of specifications, we find a strong negative relationship between hours worked by Arab/Muslim men and the death of a U.S. soldier from their state in the weeks preceding their CPS interview. Arab/Muslim men worked, on average, 0.4 fewer hours per week when exposed to at least one home-state fatality three weeks before the CPS reference week—roughly a one-percent decline relative to the sample mean. This estimate, consistent with the avoidance prediction of prejudice-based models, reflects the average response across all Arab/Muslims in our sample, combining individuals who differ in how easily they can adjust their hours and in how much their jobs require interaction with non-Muslims. Consistent with the idea that self-employed workers can more readily adjust (Farber, 2005), we find larger effects among the self-employed—0.68 fewer hours worked under similar exposure. Likewise, effects are larger for occupations that require frequent client contact, such as cashiers, dentists, salesmen, and waiters.

Our results are robust to a range of specification checks. Including or excluding region-by-month fixed effects has no qualitative effect. Excluding men from countries such as Indonesia and Malaysia—Muslims who may be less easily identifiable—does not alter the findings. Finally, we find no relationship between home-state fatalities and hours worked by immigrant men from European countries, suggesting the results reflect changes in anti-Arab and anti-Muslim sentiment rather than general labor-market shocks. Together, these findings provide direct evidence that avoidance-based sorting contributes to the observed labor market responses to prejudice.

Besides its contribution to the general discrimination literature, our work extends the limited economic literature on discrimination against Arabs and Muslims in the U.S., which has focused mainly on outcomes surrounding the September 11, 2001 attacks. Studies such as Dávila and Mora (2005) and Kaushal et al. (2007) document short-term wage declines for Arabs and Muslims relative to Whites and other immigrants.[7] These analyses, however,

[7]See also Wang (2016); Ratcliffe and von Hinke Kessler Scholder (2015); Åslund and Rooth (2005); Helly (2004).

rely on national time-series variation over short windows, making it difficult to separate discrimination from broader post-9/11 shocks. Our study exploits localized, precisely timed variation in prejudice to overcome this limitation.

In the next section, we present a simple theoretical overview to frame our analysis. In Section 3, we discuss the data and details of the empirical framework used in the paper. Section 4 presents the results and Section 5 concludes.

2 Theoretical Overview

2.1 Outline of Decision Problem

Setup To motivate our empirical analysis, we develop a theoretical framework that builds on the standard prejudice-based model, retaining its essential features and focusing on the relationships central to this paper. We consider Arab/Muslim individuals i operating in labor markets m. In each period t, which we take to be a week, individuals choose how much to participate in market activity, denoted by weekly hours $h_{it} \geq 0$. We assume that a Muslim who works in a given week must match (i.e., interact) with a non-Muslim, indexed by j. Lower values of h_{it} therefore correspond to greater avoidance of cross-group labor market interactions, while not working ($h_{it} = 0$) implies that the individual avoids such interactions altogether in that week. Let there be a standard convex cost of supplying hours, $C_i(h)$, with $C_i'(h) > 0$ and $C_i''(h) > 0$.

Each working Muslim receives realized hourly earnings W_{ij}, which decline monotonically with the anti-Muslim prejudice of their non-Muslim labor market match, d_{mj}. Define

$$W_i(d) \equiv \mathbb{E}[\, W_{ij} \mid d_{mj} = d \,]$$

as the expected hourly wage conditional on prejudice level d.

Matching and Marginal Match Matching occurs as follows: after paying a fixed weekly search cost ϕ_i, a Muslim individual i observes the prejudice levels of N randomly drawn non-Muslims in market m and matches with the least prejudiced among them. The prejudice level of this expected match is therefore

$$d_m^* = \min\{\, d_{1m}, d_{2m}, \ldots, d_{Nm} \,\}. \tag{1}$$

We refer to d_m^* as the *marginal match*—the prejudice level of the least-prejudiced non-Muslim available to a Muslim worker in market m. In spirit, the marginal match is directly analogous to the *marginal discriminator* in Becker (1971) framework: it governs equilibrium payoffs and is determined by sorting and search within the market (see Charles and Guryan (2008)).

Equilibrium Conditions Each week, person i decides whether to participate in the labor market, and if so, for how many hours. His expected payoff when deciding is

$$U_i(h_{it}) = W_i(d_m^*)\, h_{it} - C_i(h_{it}) - \phi_i. \tag{2}$$

Let

$$V_i(d_m^*) \equiv \max_{h \geq 0} \{W_i(d_m^*)h - C_i(h)\}$$

denote the indirect utility from working. Individual i participates if and only if

$$V_i(d_m^*) \geq \phi_i. \tag{3}$$

Expression (3) defines the participation, or extensive, margin: the individual works (interacts with a non-Muslim at all that week) only if the value of employment exceeds the fixed search cost ϕ_i.[8] Let the indicator N_{it} denote non-participation (that is, $h_{it} = 0$). The probability of non-participation is

$$\Pr(N_{it} = 1) = \Pr[\, \phi_i > V_i(d_m^*) \,] = 1 - F_\phi(V_i(d_m^*)), \tag{4}$$

where $F_\phi(\cdot)$ is the cumulative distribution function of search costs.

If there is an interior solution to person i's hours decision, (3) holds, and optimal hours

[8]Formally, the setup is equivalent to a model with a random outside option θ_i and a fixed search cost ϕ, where participation requires $V_i(d_m^*) - \phi \geq \theta_i$. Normalizing the outside option to zero and allowing ϕ_i to vary across individuals preserves all comparative statics while simplifying exposition.

h_i^* equate marginal cost and marginal benefit:

$$C_i'\big(h_i^*(d_m^*)\big) = W_i(d_m^*). \tag{5}$$

2.2 Effect of Changes in the Marginal Match, d_m^*

Main Effect From (4), the effect of a change in the marginal match's prejudice on the probability of non-participation (total avoidance) is

$$\frac{\partial \Pr(N_{it} = 1)}{\partial d_m^*} = -\, V_i'(d_m^*)\, f_\phi(V_i(d_m^*)) \ \geq\ 0, \tag{6}$$

because $V_i'(d_m^*) = h_i^*(d_m^*)W_i'(d_m^*) < 0$.

The effect on non-participation may be zero rather than strictly positive for some individuals. Intuitively, someone already not working will continue not to work if the value of working declines, so the only interesting case is for those already employed. For a worker with positive hours, an increase in the prejudice of the marginal match induces withdrawal from the labor market only if the decline in $V_i(d_m^*)$ is large enough to push it below ϕ_i. Thus, moderate or transitory increases in the marginal match's prejudice may leave participation unchanged; only large or persistent increases are likely to generate withdrawal from work.

By contrast, the predicted effect of a prejudice shock on hours for those already working is unambiguously negative. From (5),

$$\frac{\partial h_i^*}{\partial d_m^*} = \frac{W_i'(d_m^*)}{C_i''(h_i^*)} < 0, \tag{7}$$

since $W_i'(d) < 0$ and $C_i''(h) > 0$.

Differences Across Jobs Thus far, we have assumed systematic differences across individuals only in their search cost ϕ_i. Another plausible source of heterogeneity is the nature of individuals' jobs. One dimension is the extent to which a Muslim worker's effective wage is affected by the prejudice of their non-Muslim match. For example, Muslims employed in jobs involving frequent or intense client or co-worker interactions are likely to experience larger reductions in effective wages from interacting with prejudiced non-Muslims than are those in jobs with limited cross-group contact.

To represent this simply, we relax the assumption of a common wage function and instead write individual effective wages as

$$W_i(d) = \bar{W} - \alpha_i d,$$

where $\alpha_i \in [0, 1]$ indicates the degree of client or co-worker interaction in worker i's job.

A second source of job heterogeneity is the flexibility of hours. Jobs with fixed, required hours offer little scope for adjustment short of quitting whereas, at the other extreme, self-employment permits greater week-to-week hours variation. Rather than identical cost functions of hours across jobs, individuals may therefore face different cost functions $C_i(h)$, with those in more flexible jobs having flatter marginal costs of hours (smaller $C_i''(h)$). To capture this simply, we assume

$$C_i(h) = \varphi_i h^2,$$

where larger $\varphi_i > 0$ denotes less flexibility in Muslim i's job.

Heterogeneous Responses Combining the two expressions above for effective wages and cost of hours, the equilibrium condition for work hours implies

$$\frac{\partial h_i^*}{\partial d_m^*} = \frac{W_i'(d_m^*)}{C_i''(h_i^*)} = -\frac{\alpha_i}{2\varphi_i}. \tag{8}$$

From (8), the magnitude of the negative effect of increases in the marginal match's prejudice on work hours depends on the second-order derivatives:

$$\frac{\partial^2 h_i^*}{\partial d_m^* \partial \alpha_i} = -\frac{1}{2\varphi_i} < 0, \quad \text{and} \quad \frac{\partial^2 h_i^*}{\partial d_m^* \partial \varphi_i} = \frac{\alpha_i}{2\varphi_i^2} > 0. \tag{9}$$

Hours respond more strongly the easier it is to adjust them (the smaller is φ_i) and the more interactive the job (the larger is α_i). Evidence consistent with this latter prediction would be especially supportive of our interpretation that hours reductions in response to prejudice shocks reflect a desire to avoid cross-group interactions. Although other mechanisms could in principle reduce hours, including a possible reduction in job availability for

Muslims after a shock for some reason, explanations of this sort cannot account for the stronger effects we predict and observe among workers in highly interactive or flexible jobs. It is also of independent interest that the heterogeneity across these dimensions provides a direct test of the avoidance mechanism central to our framework.

Beyond these central predictions, the model also implies that participation should be relatively unresponsive to moderate or temporary changes in the prejudice of the marginal match, since only large or persistent shifts would induce complete withdrawal from work. We test these predictions using exogenous shocks to the distribution of prejudice among non-Muslims that shift the prejudice of the marginal match within a market.

Shocks to d_m^* We earlier defined the marginal match as the least prejudiced non-Muslim encountered during a search, but did not link this term with overall prejudice in the market.

Suppose the prejudice of non-Muslims in market m, d_{mj}, follows a distribution with mean μ_D^m and standard deviation σ_D^m. Results from canonical search models (Mortensen and Pissarides 1994; Burdett and Judd 1983; Burdett and Mortensen 1998; Rogerson et al. 2005) and from the theory of order statistics (David and Nagaraja, 2003) suggest that the expected minimum of a finite number of random draws is, in general, well approximated by a linear function of the mean and variance, so assuming this approximation holds,[9]

$$d_m^* = \mu_D^m - \lambda\,\sigma_D^m, \qquad \lambda > 0, \tag{10}$$

where λ summarizes search intensity.[10]

The effect on d_m^* of a shock z to the prejudice distribution is

$$\frac{d\,d_m^*}{dz} = \frac{d\mu_D^m}{dz} - \lambda\,\frac{d\sigma_D^m}{dz}. \tag{11}$$

We focus on shocks that raise prejudice in the market. Such prejudice-increasing shocks can take several forms, most of which will increase the prejudice of the marginal match.

[9] This approximation holds for distributions such as the normal, logistic, and exponential (see David and Nagaraja 2003, Chapters 3–4).

[10] This formulation illustrates Becker's (1971) insight about the marginal discriminator: the prejudice level driving expected outcomes rises with average prejudice in the market but is mitigated by search and sorting (cf. Charles and Guryan 2008).

For example, shocks that shift the entire distribution rightward (raising μ_D^m while leaving σ_D^m unchanged) should unambiguously increase d_m^*. Similarly, a shock that raises prejudice mainly in the lower part of the distribution, reducing the number of tolerant individuals, raises μ_D^m and lowers σ_D^m, thereby increasing d_m^*. Only shocks that shift the upper tail of the distribution—the already most prejudiced—make the effect on d_m^* ambiguous. Increasing prejudice among these individuals raises both the mean and the variance, which have opposite effects on the expected minimum draw. Although the variance increase offsets part of the mean effect, it adds little weight to the lower (tolerant) tail from which Muslim workers draw their matches, so even in this case the prejudice of the marginal match should rise.

In the next section, we discuss the exogenous shock to the prejudice distribution that we exploit to test the theoretical predictions outlined earlier.

3 Data, Summary Statistics, and Empirical Framework

3.1 Data and Summary Statistics

Our empirical analysis exploits a plausibly exogenous shock that increases prejudice in a market, thereby raising the prejudice of the marginal potential match for Muslim workers. Because the shock is orthogonal to other determinants of hours of work, changes in hours following the shock can be interpreted as evidence of avoidance of prejudice.

The shock arises from local exposure to combat fatalities among U.S. service members deployed in the Iraq and Afghanistan wars, which generated sharply timed increases in anti-Muslim sentiment in the deceased soldiers' home areas. This variation in local exposure provides a natural experiment in market-level prejudice.

To measure the shock, we compile administrative micro data on combat fatalities from the Department of Defense's Defense Manpower Data Center (DMDC), which records the home county and state of each U.S. service member killed in Iraq or Afghanistan, along with the exact date of death.[11] Using the DMDC micro data, we calculate, for each state and

[11] These detailed micro data are preferable for our purposes to the aggregate casualty data available from the Defense Casualty Analysis System (DCAS) at `https://www.dmdc.osd.mil/dcas/pages/main.xhtml` . The DMDC data also report injuries with exact injury dates but without severity information. We focus on fatalities in this paper.

week during the wars in Afghanistan and Iraq (2001–2014), the total number of fatalities of U.S. service members whose home of record is that state. These state–week fatality counts constitute our measure of the prejudice shock. The empirical analysis relates the labor supply of Arab and Muslim men in each state and reference week to the magnitude and timing of these fatality shocks.

Labor-supply outcomes are drawn from the Current Population Survey (CPS). Each CPS respondent reports labor-market information for the "reference week," defined as the week containing the 12th day of the month. Because we know the exact date of each fatality, we can align labor-supply outcomes in the CPS precisely with whether a state experienced a fatality shock before, during, or after that reference week. Our final analysis sample merges the DMDC fatality-shock data with CPS micro data by respondents' home state and interview month, thereby linking each respondent to the timing of fatality shocks in their state relative to the week of their reported labor-market activity.

Table 1 reports total fatalities of U.S. service members deployed in Afghanistan and Iraq during 1997–2014.[12] Troops were not deployed to Afghanistan until October 2001 (Tucker, 2010). As the first column shows, fatalities were zero in every year before that date. After the invasion of Iraq in March 2003, total fatalities rose sharply—from 520 in 2003 to 887 in 2004 and roughly 1,000 by 2007. From this peak, fatalities fell to an average of about 480 per year over the next four years. After 2011, as the wars wound down, fatalities declined steadily, reaching about 50 per year by 2014.

Operational needs and world events determine where, when, and how U.S. military units are deployed (Lyle 2006; Engel et al. 2010; Cesur et al. 2013). Although soldiers may express preferences for particular divisions, subsequent assignments—conditional on rank and occupation—are effectively independent of personal characteristics and of the state in which they were recruited (Lyle 2006; Engel et al. 2010; Cesur et al. 2013). Under these rules, the timing and location of combat fatalities should be largely random across soldiers' home states and over time.

The second column of table 1 shows that fatalities occurred among soldiers from nearly every state. From the invasion of Iraq in 2003 through 2011, there was never a year in

[12] Appendix table A.1 provides analogous information for soldiers wounded in action.

which more than one state was completely spared a casualty, and in many years at least one fatality occurred in every state. Even after 2011, when overall deaths fell, more than half of all states still experienced fatalities. The third column shows that combat deaths occurred in virtually every week of the conflict. Finally, the last column reports that roughly one-quarter of fatalities occurred during the CPS "reference week," precisely what would be expected if fatalities were randomly distributed across weeks within each month—consistent with our identifying assumption in the analysis below.

Our primary analysis sample consists of first- and second-generation Arab and Muslim men ages 18–55 who were interviewed in the CPS between October 1997 and August 2014.[13] We link these respondents to the state-by-week fatality data described above using their state of residence and interview month. Men are classified as Arab or Muslim if they were born in, or have at least one parent born in, an Arab or majority-Muslim country.[14]

The first column of table 2 presents descriptive statistics for this primary sample. For comparison, the second column reports corresponding statistics for an alternative sample of first- and second-generation immigrants from European countries used in our falsification tests.[15]

We have information on We 68,612 Arab and Muslim men, most of whom (84 percent) were in the labor force when interviewed. Among those participating in the labor force, 94 percent were employed at an average wage of $22 per hour. Respondents reported working an average of 42.7 hours in the CPS reference week.[16] Although respondents in the European

[13] We mainly focus the analysis on Arab and Muslim men because the labor force participation of Arab and Muslim women has been historically low, averaging about 57% during the period we analyze. For completeness we show the effects on labor supply of Arab and Muslim women in section 4.

[14] The Arab and majority-Muslim countries are Afghanistan, Algeria, Bangladesh, Egypt/United Arab Rep., Indonesia, Iran, Iraq, Jordan, Kuwait, Lebanon, Libya, Malaysia, Morocco, Pakistan, Saudi Arabia, Sudan, Syria, Turkey, United Arab Emirates, Yemen, Palestine, and "other Middle East." Below, we examine the sensitivity of our results to excluding men from Indonesia, Malaysia, Pakistan, and other Middle Eastern countries.

[15] The European countries are Denmark, Finland, Iceland, Norway, Sweden, England, Scotland, Wales, United Kingdom, Ireland, Northern Ireland, Belgium, France, Netherlands, Switzerland, Greece, Italy, Portugal, Azores, Spain, Austria, Czechoslovakia, Germany, Hungary, Poland, Romania, Bulgaria, Albania, Yugoslavia, Bosnia and Herzegovina, Croatia, Macedonia, Serbia, Kosovo, Montenegro, Estonia, Latvia, Lithuania, other USSR/Russia, Ukraine, Belarus, Moldova, and Europe n.e.c.

[16] CPS respondents are interviewed once a month for four consecutive months, are "dormant" for eight months, and are then interviewed again for four consecutive months. Interviews are conducted during the week containing the 19th of the month (one week earlier in December). Respondents are asked about employment status, usual hours, and hours worked "last week," which typically refers to the week containing

sample worked comparable hours, they earned roughly 7 percent more per hour, were more likely to be U.S. citizens, and were much more likely to have been born in the United States than Arab and Muslim men.

3.2 Empirical Setup

Our empirical analysis tests how exogenous increases in prejudice—proxied by fatality shocks among service members from a state—affect labor supply, which we interpret as evidence of avoidance behavior. We estimate the following regression for Arab and Muslim men:

$$y_{ist} = \alpha + \sum_{w=-4}^{4} \vartheta_w \text{Fatalities}_{stw} + \mathbf{X}'_{ist}\beta + \nu_s + \omega_t + \varepsilon_{ist}. \tag{12}$$

Where i indexes individuals, s indexes states, t indexes CPS survey months ($t = 1, \ldots, 203$), and w indexes weeks relative to the CPS reference week (week 0, typically the week containing the 12th day of the month). The independent variable of interest, Fatalities_{stw}, equals one if one or more U.S. service members from state s died in week w of month t. Its coefficient, ϑ_w, captures the effect of exposure to at least one home-state fatality on hours, y_{ist}, w weeks before or after the reference week.[17] Our main sample focuses on employed individuals, but to assess effects on participation, we also estimate the regression unconditional on working.

The vector of controls, $\mathbf{X}_{ist}$, includes individual characteristics (age indicators, educational attainment, ethnicity, race, marital status, U.S. citizenship, years in the United States, and occupation fixed effects) and state-level factors (state-year population and unemployment rate).[18] In some specifications, we also include in the control vector measures for the number of soldiers from the home state injured in a state-week.State fixed effects (ν_s) ensure that identification is based on within-state variation in exposure to home-state fatalities, while 203 survey-month fixed effects (ω_t) capture any new information common across markets that could affect perceptions of Arab and Muslim workers. Thus, identification comes

the 12th of the month. Respondents report earnings and wages when they are part of the "Outgoing Rotation Group" in their fourth and eighth interviews.

[17]During the study period, there are 1,132 state–week combinations with at least one soldier fatality.

[18]Controls include three indicators for educational attainment (high school, some college, and college graduate), indicators for identifying as Hispanic and non-Hispanic Black, and three indicators for years in the United States (fewer than 5, 5–10, and more than 10 years). The omitted group consists of men whose parents were born outside the United States.

entirely from differences in home-state fatality exposure net of aggregate shocks.[19] We cluster the standard errors at the state level and we use CPS person-weights in all regressions.

For instance, if employers viewed Muslim men as more of a risk after a particularly violent encounter between U.S. troops and Iraqi insurgents, this would be captured by the month-by-year fixed effects. Because home-state casualties are determined by deployment decisions made by the U.S. military and events overseas, they can be thought of as, in effect, randomly assigned and should not contain information about the productivity or skills of Arab and Muslim men working in the United States.

As a robustness/falsification exercise to our main analysis, we also examine how exposure to war fatalities affects the behavior of European immigrant. For these men, we do not expect labor-supply responses to home-state fatalities, since these events should not generate negative sentiment toward them and thus no corresponding desire among them to avoid labor-market interactions.

It is also possible that periods with multiple fatalities may produce especially large shocks to prejudice, with correspondingly larger hours-avoidance reactions among Arabs and Muslims. Having confirmed, that fatalities do not affect the hours worked by European immigrants, we estimate regressions of the same form as (12) using the pooled CPS sample including both Arab/Muslim and European men given by:

$$y_{ist} = \alpha + \sum_{w=-4}^{4} \vartheta_w \text{Fatalities}_{stw} + \gamma A_i + \sum_{w=-4}^{4} \delta_w \text{Fatalities}_{stw} \times A_i + \mathbf{X}'_{ist}\beta + \nu_s + \omega_t + \varepsilon_{ist} \quad (13)$$

where the notation and variables are the same as in equation 12 and A_i is an indicator that equals one when the individual is from an Arab or Muslim country.[20] In this regression, we

[19] It might be noted that the distribution of U.S. fatalities during the wars was not uniform across service branches (Duncan et al., 2019). Thus, even after conditioning for state and month-by-year fixed effects, time-varying state socio-economic characteristics may have impacted the number of enlistees from each state, the branch they choose to enlist in, and thus number of home-state fatalities. To assess this issue, we examined the relationship between home-state fatalities and the characteristics of a sample of men, aged 18-55, drawn from the American Community Survey (ACS) for the years 2000-2013. In regressions that include state and year fixed effects, we find no relationship between men's characteristics in the state and combat fatalities, providing support for the assumption that the exposure to fatalities can be thought of as randomly assigned. This is consistent with the findings of Karol and Miguel (2007). The results of this analysis are available upon request.

[20] We estimate a fully interacted model, where the individual controls in vector $\mathbf{X}_{ist}$ and the state and month-by-year fixed effects are also interacted with A_i.

allow the treatment to vary by intensity by introducing two new indicator variables: one denotes whether a soldier from state s died in week w; the second indicates whether two or more soldiers from state s died in week w, which

4 Results

In this section, we present our empirical results. Before turning to the labor supply and avoidance evidence, we assess the credibility of our argument that exogenous shocks to state–time fatalities generate an increase in prejudice in the affected market.

Hate Crimes If service-member fatality shocks increase anti-Muslim sentiment, some members of the broader community might respond with hate-motivated criminal acts against Arabs and Muslims. Thousands of law enforcement agencies collect and submit to the FBI, under the Uniform Crime Reporting (UCR) system, data on crimes motivated by racial, religious, ethnic, identity, and other types of prejudice. We assemble these "hate crime" statistics by state and week. We then estimate regressions that relate anti-Muslim hate crimes in a state-week to a series of indicators for whether there was a combat fatality of a service member from that state in specific weeks before and after the week the hate crime is reported. The regressions control for state–year population and include a rich set of state and time fixed effects.

Figure 1 presents our results graphically and we report the estimates in Appendix Table A.2. The two series depicted for each week are hate crimes against Muslims (solid confidence intervals) and hate crimes against other religious groups (dotted confidence intervals). As the figure shows, we find—reassuringly—that the number of hate crimes against Muslims in a given week is not affected by fatality shocks that occur *after* that week. In contrast, we find that hate crimes against Muslims in a state are higher by a (weakly) statistically significant amount three to four weeks after a fatality shock. This is consistent with our main assumption. The second column of Appendix Table A.2. shows no statistically significant effect on hate crimes against other religious groups.[21] These patterns strengthen our

[21] Previous research has shown that local war fatalities may have a salient impact on affected communities. For instance, Karol and Miguel (2007) found that home-state casualties from the Iraq War were associated

confidence that fatalities in wars against majority-Muslim countries affect anti-Muslim bias.

News of home-state fatalities almost certainly reached the fallen service member's community some time after the event. Even in the highly unlikely case that the family was informed immediately or shortly after the death, the information would be disseminated to the broader home state through interactions with friends and loved ones of the deceased and, especially, through events such as funerals, memorials, and other public observances—all of which would create a lag between the event and exposure to news about it.[22] The date of the fatality, which we treat as the date when the exogenous event occurs, may thus be systematically (and mechanically) too early. The hate-crime results, showing effects three weeks after the event, hint at this possibility.

To assess this issue, using the exact names of service members from the DMDC and information on home county, we identify funeral dates for 80% of the fallen soldiers in our sample. Figure 2 plots the distribution of days between the death and the funeral or memorial. The figure shows that, on average, funerals and memorials (presumably a key information vehicle for the event) occurred 11.8 days after the fatality. Below, we show some results that treat the date of the fatality shock as the date of death, and other results that set the treatment date to the funeral date.

Baseline Estimates Figure 3 and Appendix Table A.3 present our baseline estimates of the effect of shocks on hours worked for the Arab/Muslim and European immigrant samples. The figure was constructed by regressing the reported hours worked by Arab/Muslim respondents "last week" (i.e., during the reference week) on an indicator for whether at least one soldier from state s died during the reference week of survey month t, denoted "Ref Week." The regressions include similarly constructed home-state fatality indicators for the weeks preceding (and following) the reference week, state fixed effects, and month-by-year fixed effects. The top panel shows results with shocks measured relative to the date of death;

with a lower vote share going to President Bush during the 2004 election. Christensen (2017) found that county-level casualties from the Iraq and Afghanistan wars were associated with decreased enlistments.

[22]During the period under study, the public was intensely interested in events in Afghanistan and Iraq, and local media outlets often covered the funerals of soldiers killed overseas. Examples include: Bell (2004), Posada (2005), Rotstein (2006), Kirkland (2007), Roeder (2007), Schwach (2007), Warrenoct (2007), Bynum (2008), Mitchell (2008), and Coleman (2012).

the bottom panel shows estimates measuring the shock date by the date of the funeral.

The top panel shows that Arab and Muslim men worked, on average, 0.50 fewer hours when exposed to at least one home-state fatality three weeks before the reference week—about one percent reduction relative to the mean number of hours worked by Arab and Muslim men. Estimates of the relationship between home-state fatality indicators and hours worked by European men are small and statistically insignificant, suggesting that the negative relationship among Arab and Muslim men is not driven by a change in local labor market conditions that affected all workers similarly. These patterns lend credence to the avoidance interpretation.

Since the prejudice shock may induce an extensive-margin response (i.e., the choice to work zero hours), analyzing hours worked would result in an endogenously selected sample. In the first column of Appendix Table A.3, we estimate the results unconditional on employment, where unemployed people take zero for hours worked last week. The effect is similar in magnitude to our preferred specification, suggesting that temporary increases in prejudice have little effects on the decision to work.

As discussed above, there is reason to suppose that the community received news of the fatality event some time after the date of death. Panel (b) of the figure is based on regressions that relate reported hours of work to the date of the funeral. Strikingly, we find similar pattern of reduced hours for Muslims (alone), but the effect now appear one week after the funeral (0.42 fewer hours), which is precisely what one would expect given our discussion about when news spreads to the broader community.

Robustness and Alternative Specifications In Table 3, we assess how sensitive our results are to various specification choices. Reading across the table, it is striking how stable our baseline results are across different specifications. In particular, the key estimate of a reduction in hours following a fatality event three weeks prior is essentially unaffected by whether we do not use survey weights, whether the models include region×month fixed effects, or whether the regressions control for persons wounded in action.

One final sensitivity result addresses concerns about our definition of who is Arab/Muslim. Up to this point in the analysis, we have used a broad definition of Arab and Muslim men.

We assess how sensitive our results are to this definition by exploring two alternatives. Our main results consider first-generation immigrants from Arab and Muslim countries. For one alternative definition, we exclude first- and second-generation men from Indonesia, Malaysia, and Pakistan, and men from countries under the category "other Middle East." For the second alternative definition, we experiment with a broader definition by adding first- and second-generation immigrant men from India to the treatment group. Although India is not a majority-Muslim country, there is anecdotal evidence that men of Indian heritage, especially Sikhs, faced discrimination in the aftermath of the terrorist attacks on September 11, 2001, and during the Iraq and Afghanistan wars. As figure 4 shows, the results are remarkably stable across these different specifications.[23]

In our view, these baseline results provide strongly supportive evidence of the arguments laid out previously in the paper. It is worth stressing that being forced to use states rather than, say, counties mitigates against our finding any effect whatsoever. By our argument, the unfortunate news of a fatality would be particularly noted and reacted to by persons in the part of the state where the deceased soldier is from. Other non-Muslims elsewhere in the state are less likely to know about—and thus less likely to respond to—the event. That we nonetheless estimate a statistically significant hours response, blending the behavior of these two populations, strongly suggests that these estimates are an underestimate of the response among the most directly affected Muslims.

Treatment Intensity and Heterogeneous Responses Thus far, we have focused on exposure to at least one home-state fatality without distinguishing between low- and high-intensity exposures. Yet it seems reasonable that periods with multiple fatalities may produce especially large shocks to prejudice, with correspondingly larger hours-avoidance reactions among Arabs and Muslims. Besides the intensity of the shock, we also discussed in the theory section that, for shocks of a *given* intensity, the Arab/Muslim avoidance response might vary along two dimensions: (a) whether Muslims were self-employed; and (b) whether they worked in highly interactive sectors. Table 4 presents results that explore these issues.[24]

[23] See appendix table A.4 for full regression estimates.

[24] Appendix Table A.5 presents the corresponding results for women and shows no evidence that women's labor supply responds to prejudice shocks.

The table introduces two new indicator variables: one denotes whether a soldier from state s died in week w; the second indicates whether two or more soldiers from state s died in week w. During the period under study, we observe a total of 1,132 state–week combinations in which one soldier died, and 272 state–week combinations in which two or more soldiers died. We estimate this model using the pooled sample as in equation 13.

The first column of table 4 shows regressions that include the two indicators described above (and their leads and lags) on the right-hand side of our main regression model. The findings provide evidence that hours worked by Arab and Muslim men were more responsive to high-intensity exposure. Specifically, one home-state fatality three weeks before the reference week is associated with 0.46 fewer hours worked. By contrast, the estimated coefficient of the indicator for two or more fatalities is larger: exposure to two or more home-state fatalities three weeks before the reference week is associated with 0.54 fewer hours worked.[25]

In the second column, we report results restricting the sample to Arab and Muslim men who were self-employed. Consistent with the idea that self-employed workers can more easily adjust their hours of work, we find much larger reductions in response to the state fatality shock. For example, exposure to a multiple-fatality week produces a reduction in hours among self-employed Muslims that is twice as large as in the overall sample. These results strongly confirm the prediction in the theoretical section.

Based on descriptions in the *Dictionary of Occupational Titles* (DOT), Kilbourne et al. (1994) created a list of occupations that required intense interactions with clients or customers. Occupations were considered interactive if two criteria were met: workers had to provide a service while engaged in face-to-face contact with clients or customers, and the provision of this service had to occur for a "substantial portion of work time" (p. 716).[26]

[25] We cannot reject the hypothesis that the coefficients of the one-fatality indicator and the two-or-more-fatalities indicator are equal at the 0.05 level.

[26] The full list of interactive occupations is: chiropractors, clergy, dentists, librarians, nurses, optometrists, osteopaths, physicians and surgeons, recreation and group workers, religious workers, social and welfare workers, teachers, therapists and healers not elsewhere classified, library attendants and assistants, attendants in physicians' and dentists' offices, baggagemen in transportation, cashiers, receptionists, ticket agents, salesmen and sales clerks not elsewhere classified, auto service and parking attendants, taxicab drivers and chauffeurs, baby-sitters, attendants (in hospitals, professional and personal service not elsewhere classified, and recreation and amusement), hairdressers and cosmetologists, midwives, porters, practical nurses, and waiters and waitresses. In addition to the occupations identified by Kilbourne et al. (1994), we included supervisors of sales jobs, insurance sales, real estate sales, financial services sales, advertising sales, retail sales, and salesperson occupations in the interactive category.

Following Kilbourne et al. (1994), we split the sample using their three-digit SOC occupation codes. About 21% of those who were employed reported an occupation that required intense interactions with customers or clients (n = 57,642). Examples include cashiers, chiropractors, dentists, hairdressers, parking attendants, salesmen, social workers, and waiters. The remaining CPS respondents (n = 218,543) reported occupations that did not require intense interaction.[27]

The third column of the table presents estimates for Arab and Muslim men working in interactive occupations. Consistent with our argument that, if Arabs and Muslims anticipate negative interactions with their co-workers or clients, those whose jobs require more interaction should more sharply avoid interactions at work, we find substantial hours changes for this group. The results show that the death of two or more soldiers three weeks before the reference week is associated with 1.63 fewer hours worked, or 3.8 percent relative to the mean.

5 Conclusion

This paper offers new evidence that prejudice shapes economic behavior not only through the decisions of those who hold it but also through avoidance by its targets. We show that when anti-Muslim sentiment rises in a local market—triggered by the death of a U.S. soldier from that state in wars against Muslim-majority countries—Arab and Muslim men reduce their labor supply. The magnitude and timing of the response align closely with increases in anti-Muslim hate crimes, supporting the interpretation that these shocks heighten local prejudice rather than shift economic fundamentals. The effects are largest among the self-employed and in highly interactive occupations, where individuals have both the flexibility and incentive to reduce exposure to discriminatory interactions.

These findings extend the Beckerian framework of prejudice-based discrimination by providing direct behavioral evidence of avoidance-driven sorting. They imply that prejudice can amplify economic inequality not only through lower wages but also through voluntary withdrawal from market engagement. More broadly, our results highlight how seemingly

[27] Among Arab and Muslim (European) men 34% (19%) were employed in high interactive occupations.

distant geopolitical events can reverberate through local labor markets, altering participation decisions and shaping the economic trajectories of minority communities in subtle but meaningful ways.

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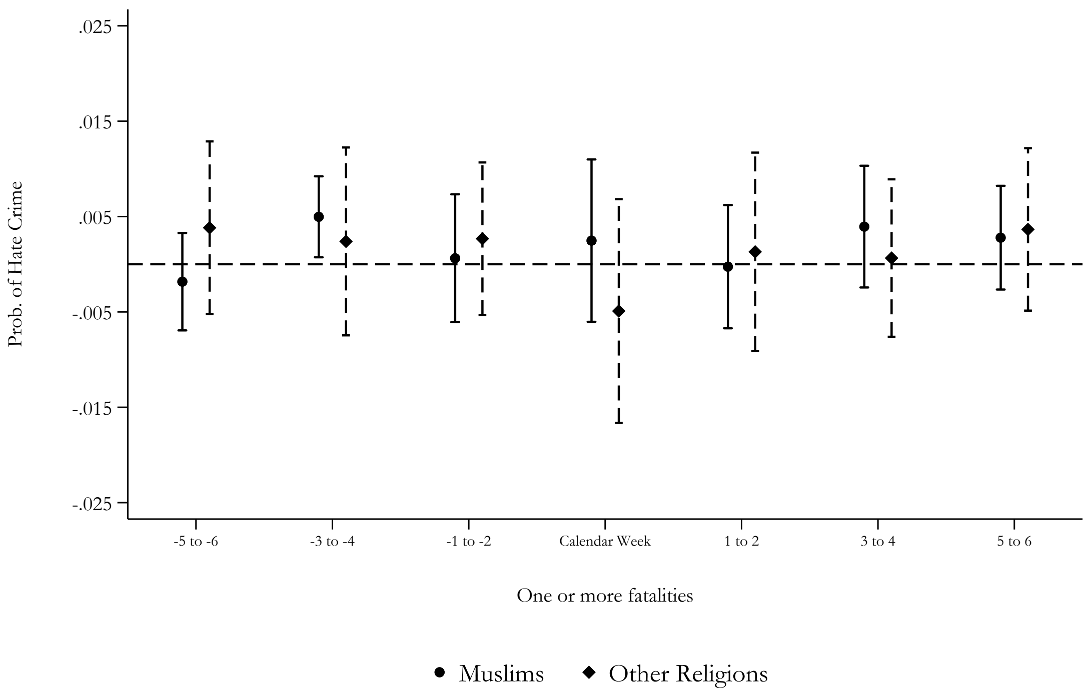
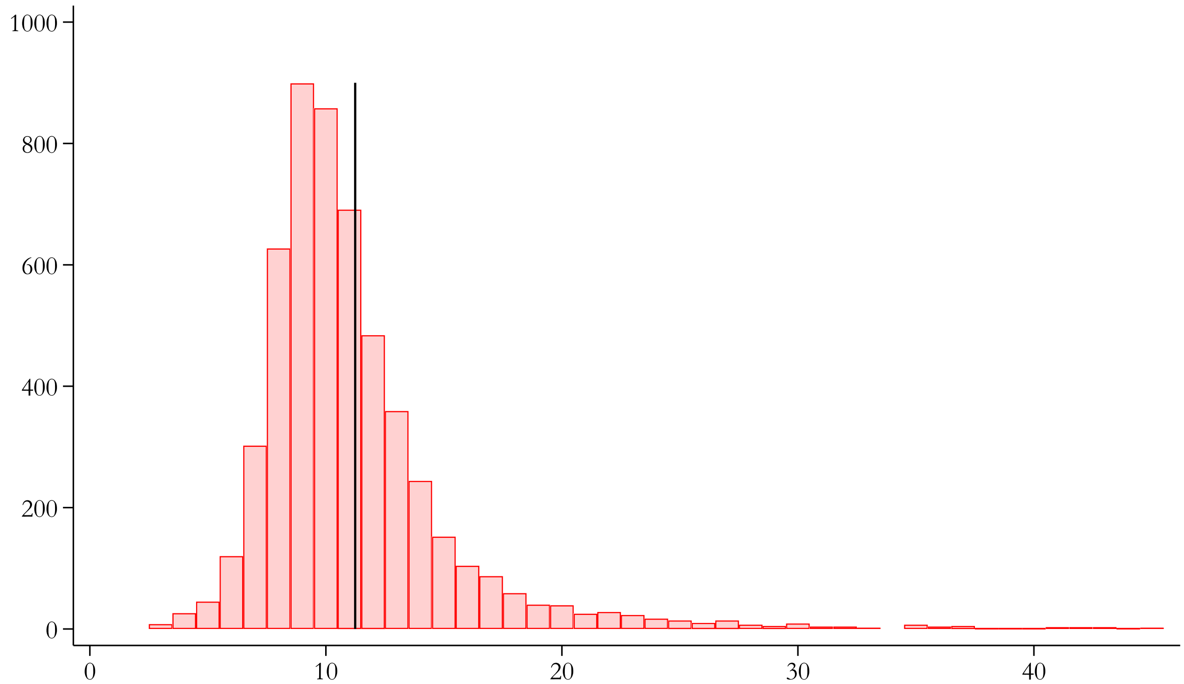
Figures and Tables

Figure 1:
The Effect of Fatalities on Hate Crimes



Notes: These figures show the coefficients and 90% confidence intervals from regressions estimating the effect of fatalities on hate crimes against Muslims or other religions. The controls include state-year population as well as state and week-by-year fixed effects. The standard errors are clustered at the state level. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on hate crimes is drawn from the Uniform Crime Reporting program published by the Federal Bureau of Investigation.

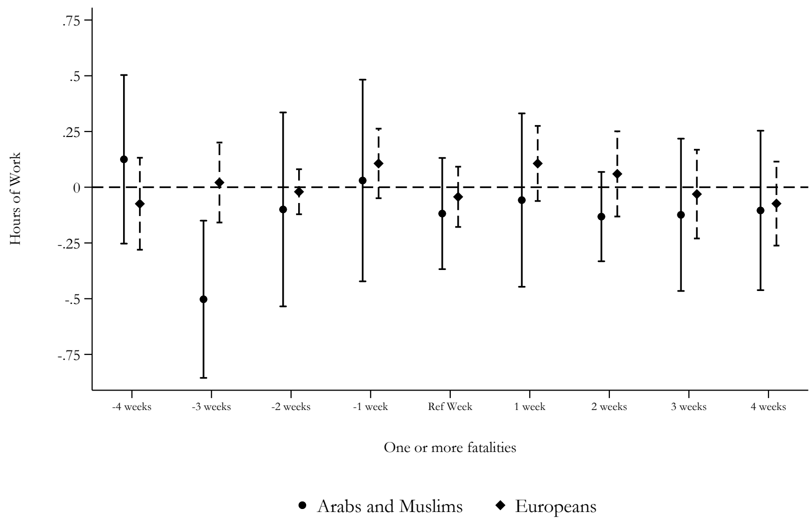
Figure 2:
Frequency of Days between Fatality Date and Funeral/Memorial



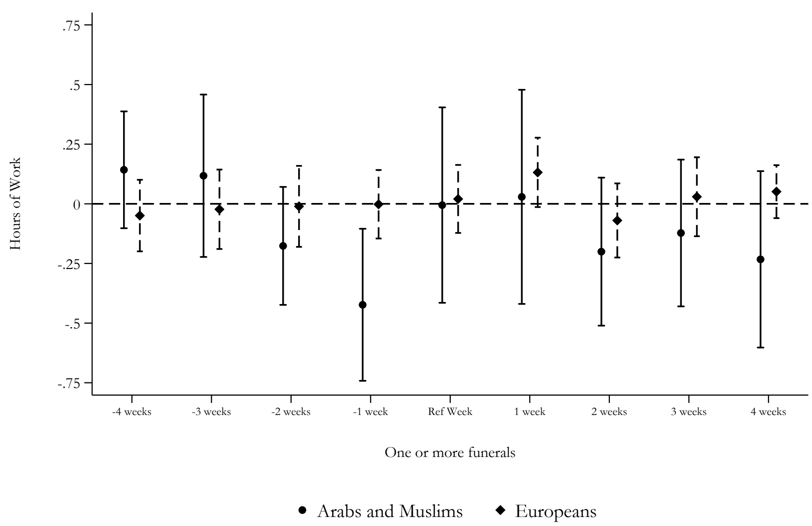
Notes: The data on funeral dates were collected by the authors. The vertical black line indicates an average of 11.2 days between the fatality and funeral/memorial.

Figure 3:
The Effect of Fatalities on Hours Worked Last Week of Men

(a) Fatalities

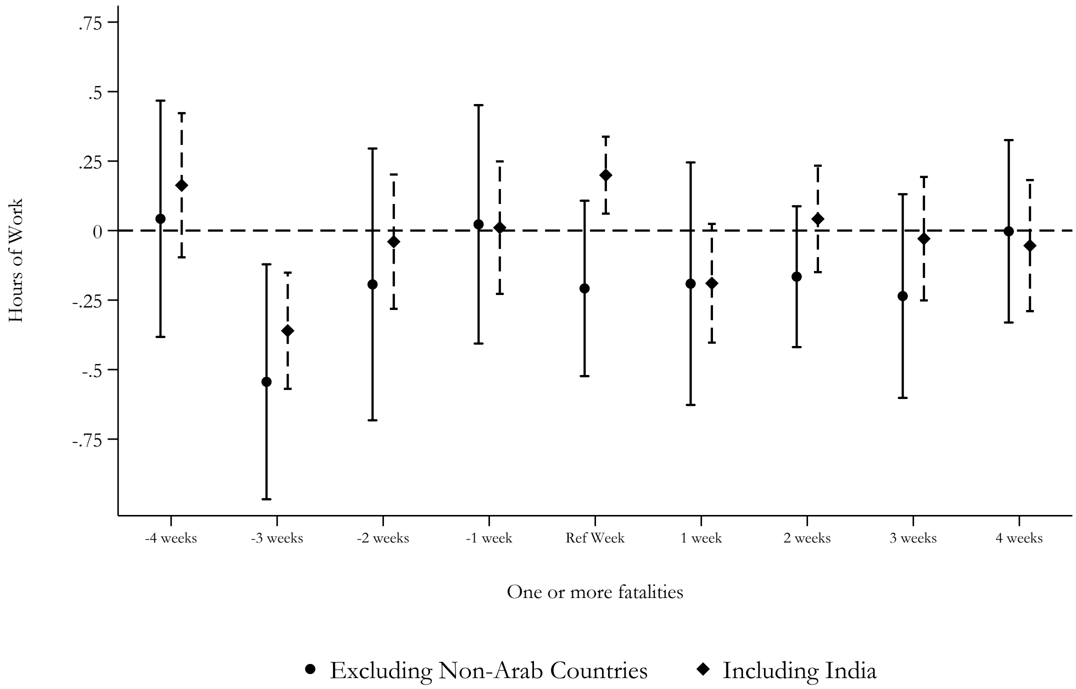


(b) Funerals



Notes: These figures show the coefficients and 95% confidence intervals from regressions estimating the effect of fatalities on hours worked last week of Arabs and Muslims or Europeans. Panel (a) uses the date of fatality and panel (b) uses the date of the funeral. The controls include state-level factors (state-year population and unemployment rate), indicators for age, educational attainment (high school, some college, and college graduate), ethnicity and race (Hispanic and non-Hispanic Black), time spent in the United States (below 5 years, 5-10 years, and more than 10 years), as well as state, survey month-by-year, and occupation fixed effects. The standard errors are clustered at the state level. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on labor supply is and data on labor supply is drawn from the Current Population Survey (CPS). The sample is restricted to CPS respondents age 18-55 observed in 1997-2014.

Figure 4:
The Effect of Fatalities on Hours Worked Last Week of Men
Alternative Sample Definitions



Notes: This figure show the coefficients and 95% confidence intervals from regressions estimating the effect of fatalities on hours worked last week using the date of fatality and alternative sample definitions. The controls include state-level factors (state-year population and unemployment rate), indicators for age, educational attainment (high school, some college, and college graduate), ethnicity and race (Hispanic and non-Hispanic Black), time spent in the United States (below 5 years, 5-10 years, and more than 10 years), as well as state, survey month-by-year, and occupation fixed effects. The standard errors are clustered at the state level. We define non-Arab countries as Bangladesh, Indonesia, Malaysia, and "other Middle East". Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on labor supply is drawn from the Current Population Survey (CPS). The sample is restricted to CPS respondents age 18-55 observed in 1997-2014.

Table 1:
Descriptive Statistics on War Fatalities, by Year

	Total Fatalities	# States with at least 1 Fatality	# Weeks with at least 1 Fatality	% of Total Fatalities in in Reference Week
1997	0	0	0	0.00%
1998	0	0	0	0.00%
1999	0	0	0	0.00%
2000	0	0	0	0.00%
2001	11	10	6	45.45%
2002	49	24	20	24.49%
2003	520	50	46	21.15%
2004	887	50	52	26.16%
2005	923	49	52	19.39%
2006	907	50	52	22.71%
2007	999	50	52	20.62%
2008	458	49	51	24.67%
2009	454	48	52	20.93%
2010	552	48	52	25.91%
2011	461	49	52	20.82%
2012	311	42	52	23.47%
2013	128	35	40	23.44%
2014	50	28	24	34.00%

Notes: This table shows descriptive statistics of fatalities by year. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System.

Table 2:
Descriptive Statistics for Current Population Survey Sample

	Arabs and Muslims	Europeans
Age	35.97 (10.52)	38.26 (10.59)
High school	0.19 (0.40)	0.25 (0.43)
Some college	0.25 (0.43)	0.28 (0.45)
College	0.49 (0.50)	0.41 (0.49)
Married	0.57 (0.49)	0.58 (0.49)
U.S. citizen	0.63 (0.48)	0.78 (0.41)
1-5 years in U.S.	0.13 (0.34)	0.07 (0.25)
6 or more years in U.S.	0.69 (0.46)	0.39 (0.49)
Born in the U.S.	0.18 (0.38)	0.50 (0.50)
Labor Force Participation	0.84 (0.37)	0.89 (0.32)
Sample Size	68,612	275,523
	Conditional on labor force participation	
Hours Worked	42.47 (13.95)	43.23 (13.08)
Sample Size	52,281	226,186
Employed	0.93 (0.25)	0.95 (0.22)
Sample Size	57,626	245,722
Self-employed	0.18 (0.39)	0.15 (0.36)
Sample Size	57,300	245,192

Notes: This table shows descriptive statistics of Current Population Survey (CPS) respondents. The sample is restricted to CPS respondents age 18-55. Standard deviations are in parentheses. Means are weighted with person-level basic monthly survey weights.

Table 3:
The Effect of Fatalities on Hours Worked Last Week of Men
Robustness to Control Variables

	Baseline Results	Alternative Control Variables			
Fatality 4 weeks after RW	-0.10	0.04	-0.11	-0.09	-0.10
	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)
Fatality 3 weeks after RW	-0.12	-0.12	-0.16	-0.11	-0.16
	(0.17)	(0.18)	(0.16)	(0.17)	(0.16)
Fatality 2 weeks after RW	-0.13	-0.09	-0.14	-0.15	-0.15
	(0.10)	(0.12)	(0.10)	(0.10)	(0.10)
Fatality 1 week after RW	-0.06	-0.02	-0.05	-0.05	-0.04
	(0.19)	(0.19)	(0.19)	(0.20)	(0.19)
Fatality reference week (RW)	-0.12	-0.10	-0.18	-0.12	-0.19
	(0.12)	(0.12)	(0.13)	(0.12)	(0.13)
Fatality 1 week before RW	0.03	0.02	0.01	0.02	-0.00
	(0.23)	(0.23)	(0.22)	(0.22)	(0.22)
Fatality 2 weeks before RW	-0.10	-0.09	-0.13	-0.07	-0.10
	(0.22)	(0.20)	(0.21)	(0.21)	(0.21)
Fatality 3 weeks before RW	-0.50***	-0.35**	-0.46***	-0.50***	-0.46***
	(0.18)	(0.17)	(0.17)	(0.18)	(0.17)
Fatality 4 weeks before RW	0.13	0.07	0.16	0.12	0.15
	(0.19)	(0.19)	(0.18)	(0.19)	(0.17)
Observations	52,280	52,280	52,280	52,280	52,280
Survey Weights	YES	NO	YES	YES	YES
Region x Month FE	NO	NO	YES	NO	YES
Wounded in Action	NO	NO	NO	YES	YES
Mean Y - Baseline	44.38	44.38	44.38	44.38	44.38

* Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: This table shows the coefficients and standard errors in parenthesis from regressions estimating the effect of fatalities on hours worked last week of Arabs and Muslims using the date of fatality. The controls in the baseline model include state-level factors (state-year population and unemployment rate), indicators for age, educational attainment (high school, some college, and college graduate), ethnicity and race (Hispanic and non-Hispanic Black), time spent in the United States (below 5 years, 5-10 years, and more than 10 years), as well as state, survey month-by-year, and occupation fixed effects. The standard errors are clustered at the state level. Regressions are weighted with person-level basic monthly survey weights. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on labor supply is drawn from the Current Population Survey (CPS). The sample is restricted to CPS respondents age 18-55 observed in 1997-2014.

Table 4:
The Effect of Fatalities on Hours Worked Last Week of Men (Fully Interacted Model)
Intensity of Exposure

	Full Sample	Self Employed	High-Interactive Occupations
1 fatality 2 wks. after RW	-0.11	-0.37	-0.05
	(0.19)	(0.73)	(0.33)
>= 2 fatalities 2 wks. after RW	-0.28	-0.75	-0.06
	(0.28)	(0.69)	(0.49)
1 fatality 1 week after RW	-0.18	-1.47***	-0.04
	(0.17)	(0.55)	(0.42)
>= 2 fatalities 1 week after RW	-0.01	-1.46	0.61
	(0.34)	(0.92)	(0.49)
1 fatality reference week (RW)	0.00	0.46	-0.13
	(0.12)	(0.56)	(0.50)
>= 2 fatalities reference week (RW)	-0.22	0.36	-0.64
	(0.30)	(0.47)	(0.86)
1 fatality 1 week before RW	-0.04	-0.12	0.96
	(0.24)	(0.68)	(0.68)
>= 2 fatalities 1 week before RW	-0.14	0.20	0.77
	(0.31)	(0.62)	(0.51)
1 fatality 2 wks. before RW	0.00	-0.60	0.18
	(0.22)	(0.51)	(0.50)
>= 2 fatalities 2 wks. before RW	-0.32	-1.13**	0.06
	(0.25)	(0.54)	(0.40)
1 fatality 3 wks. before RW	-0.46*	-0.77	-0.62
	(0.24)	(0.57)	(0.38)
>= 2 fatalities 3 wks. before RW	-0.54**	-1.81**	-1.63***
	(0.26)	(0.68)	(0.45)
1 fatality 4 wks. before RW	0.32	0.04	0.68*
	(0.24)	(0.55)	(0.36)
>= 2 fatalities 4 wks. before RW	-0.07	-1.48*	0.18
	(0.34)	(0.84)	(0.41)
Observations	276,185	43,288	57,642
Mean Y - Baseline	44.09	48.66	45.14

* Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: This table shows the coefficients from inter and standard errors in parenthesis from regressions estimating the effect of fatalities on hours worked last week of Arabs and Muslims using the date of fatality. The controls in the baseline model include state-level factors (state-year population and unemployment rate), indicators for age, educational attainment (high school, some college, and college graduate), ethnicity and race (Hispanic and non-Hispanic Black), time spent in the United States (below 5 years, 5-10 years, and more than 10 years), as well as state, survey month-by-year, and occupation fixed effects. The standard errors are clustered at the state level. Regressions are weighted with person-level basic monthly survey weights. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on labor supply is drawn from the Current Population Survey (CPS). The sample is restricted to CPS respondents age 18-55 observed in 1997-2014.

Appendix

A Supplemental Figures and Tables

Table A.1:
Descriptive Statistics on Wounded in War, by Year

	Total Wounded	# States with at least 1 Wounded	# Weeks with at least 1 Wounded	% of Total Wounded in in Reference Week
1997	0	0	0	0.00%
1998	0	0	0	0.00%
1999	0	0	0	0.00%
2000	0	0	0	0.00%
2001	32	22	5	75.00%
2002	72	28	22	6.94%
2003	2350	51	47	25.11%
2004	7933	51	52	27.69%
2005	5930	51	52	25.21%
2006	6250	51	52	22.14%
2007	5965	51	52	22.03%
2008	2496	50	52	23.12%
2009	2468	51	52	25.41%
2010	5153	51	52	25.01%
2011	4848	51	52	21.76%
2012	2841	51	52	19.92%
2013	1296	49	52	32.02%
2014	407	45	40	31.70%

Notes: This table shows descriptive statistics of fatalities by year. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System.

Table A.2:
The Effect of Fatalities on Hate Crimes

	Muslims	Other Religions
Fatality 5-6 weeks after Week 0	0.0028	0.0037
	(0.0032)	(0.0051)
Fatality 3-4 weeks after Week 0	0.0039	0.0006
	(0.0038)	(0.0049)
Fatality 1-2 weeks after Week 0	-0.0003	0.0013
	(0.0039)	(0.0062)
Fatality Week 0	0.0025	-0.0049
	(0.0051)	(0.0070)
Fatality 1-2 weeks before Week 0	0.0006	0.0027
	(0.0040)	(0.0048)
Fatality 3-4 weeks before Week 0	0.0050*	0.0024
	(0.0025)	(0.0059)
Fatality 5-6 weeks before Week 0	-0.0018	0.0038
	(0.0030)	(0.0054)
Observations	38,400	38,400
Mean Y - Baseline	0.0108	0.222

* Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: This table shows the coefficients and standard errors in parenthesis from regressions estimating the effect of fatalities on hate crimes against Muslims or other religions. The controls include state-year population as well as state and week-by-year fixed effects. The standard errors are clustered at the state level. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on hate crimes is drawn from the Uniform Crime Reporting program published by the Federal Bureau of Investigation.

Table A.3:
The Effect of Fatalities on Hours Worked Last Week of Men

	Arabs and Muslims			European Immigrants		
	Fatalities		Funerals	Fatalities		Funerals
	Including Zeros	Excluding Zeros		Including Zeros	Excluding Zeros	
Fatality 4 weeks after RW	-0.24	-0.10	-0.23	0.07	-0.07	0.05
	(0.23)	(0.18)	(0.18)	(0.12)	(0.09)	(0.06)
Fatality 3 weeks after RW	-0.05	-0.12	-0.12	0.06	-0.03	0.03
	(0.23)	(0.17)	(0.15)	(0.15)	(0.10)	(0.08)
Fatality 2 weeks after RW	-0.19	-0.13	-0.20	0.16	0.06	-0.07
	(0.13)	(0.10)	(0.15)	(0.16)	(0.10)	(0.08)
Fatality 1 week after RW	-0.05	-0.06	0.03	0.03	0.11	0.13*
	(0.23)	(0.19)	(0.22)	(0.12)	(0.08)	(0.07)
Fatality reference week (RW)	-0.24	-0.12	-0.01	-0.11	-0.04	0.02
	(0.17)	(0.12)	(0.20)	(0.10)	(0.07)	(0.07)
Fatality 1 week before RW	0.28	0.03	-0.42**	0.13	0.11	-0.00
	(0.22)	(0.23)	(0.16)	(0.10)	(0.08)	(0.07)
Fatality 2 weeks before RW	-0.00	-0.10	-0.18	-0.04	-0.02	-0.01
	(0.26)	(0.22)	(0.12)	(0.09)	(0.05)	(0.08)
Fatality 3 weeks before RW	-0.56**	-0.50***	0.12	0.10	0.02	-0.02
	(0.23)	(0.18)	(0.17)	(0.09)	(0.09)	(0.08)
Fatality 4 weeks before RW	0.08	0.13	0.14	0.03	-0.07	-0.05
	(0.17)	(0.19)	(0.12)	(0.15)	(0.10)	(0.07)
Observations	57,731	52,280	52,280	246,636	226,186	226,186
Mean Y - Baseline	40.92	44.38	44.38	41.01	44.05	44.05

* Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: This table shows the coefficients and standard errors in parenthesis from regressions estimating the effect of fatalities on hours worked last week of Arabs and Muslims or Europeans using the date of fatality or funeral. The controls include state-level factors (state-year population and unemployment rate), indicators for age, educational attainment (high school, some college, and college graduate), ethnicity and race (Hispanic and non-Hispanic Black), time spent in the United States (below 5 years, 5-10 years, and more than 10 years), as well as state, survey month-by-year, and occupation fixed effects. The standard errors are clustered at the state level. Regressions are weighted with person-level basic monthly survey weights. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on labor supply is drawn from the Current Population Survey (CPS). The sample is restricted to CPS respondents age 18-55 observed in 1997-2014.

Table A.4:
The Effect of Fatalities on Hours Worked Last Week of Men
Alternative Sample Definitions

	Excluding Non-Arab countries	Including Indian men
Fatality 4 weeks after RW	-0.00	-0.05
	(0.16)	(0.12)
Fatality 3 weeks after RW	-0.24	-0.03
	(0.18)	(0.11)
Fatality 2 weeks after RW	-0.17	0.04
	(0.13)	(0.10)
Fatality 1 week after RW	-0.19	-0.19*
	(0.22)	(0.11)
Fatality reference week (RW)	-0.21	0.20***
	(0.16)	(0.07)
Fatality 1 week before RW	0.02	0.01
	(0.21)	(0.12)
Fatality 2 weeks before RW	-0.19	-0.04
	(0.24)	(0.12)
Fatality 3 weeks before RW	-0.54**	-0.36***
	(0.21)	(0.10)
Fatality 4 weeks before RW	0.04	0.16
	(0.21)	(0.13)
Observations	44,133	108,053
Mean Y - Baseline	45.05	44.06

* Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: This table shows the coefficients from inter and standard errors in parenthesis from regressions estimating the effect of fatalities on hours worked last week of Arabs and Muslims using the date of fatality. The controls in the baseline model include state-level factors (state-year population and unemployment rate), indicators for age, educational attainment (high school, some college, and college graduate), ethnicity and race (Hispanic and non-Hispanic Black), time spent in the United States (below 5 years, 5-10 years, and more than 10 years), as well as state, survey month-by-year, and occupation fixed effects. The standard errors are clustered at the state level. Regressions are weighted with person-level basic monthly survey weights. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on labor supply is drawn from the Current Population Survey (CPS). The sample is restricted to CPS respondents age 18-55 observed in 1997-2014.

Table A.5:
The Effect of Fatalities on Hours Worked Last Week of Women (Fully Interacted Model)
Intensity of Exposure

	Full Sample	Self Employed	High-Interactive Occupations
1 fatality 2 wks. after RW	-0.17	-1.75*	-0.33
	(0.22)	(1.02)	(0.42)
>= 2 fatalities 2 wks. after RW	-0.04	-0.58	0.12
	(0.60)	(1.29)	(0.82)
1 fatality 1 week after RW	-0.39	-1.28	-0.31
	(0.28)	(0.95)	(0.42)
>= 2 fatalities 1 week after RW	-0.34	-0.05	-0.77
	(0.29)	(1.46)	(0.81)
1 fatality reference week (RW)	0.36	1.51	-0.19
	(0.29)	(1.12)	(0.38)
>= 2 fatalities reference week (RW)	-0.23	-0.21	-0.21
	(0.27)	(0.91)	(0.48)
1 fatality 1 week before RW	-0.23	-0.71	0.05
	(0.34)	(1.04)	(0.59)
>= 2 fatalities 1 week before RW	0.13	-0.70	0.25
	(0.35)	(1.00)	(0.50)
1 fatality 2 wks. before RW	0.10	-0.92	0.01
	(0.22)	(0.97)	(0.35)
>= 2 fatalities 2 wks. before RW	0.06	-0.26	0.77
	(0.32)	(1.23)	(0.63)
1 fatality 3 wks. before RW	0.07	-0.48	0.21
	(0.30)	(1.13)	(0.47)
>= 2 fatalities 3 wks. before RW	0.09	-0.62	-0.52
	(0.35)	(1.39)	(0.87)
1 fatality 4 wks. before RW	0.27	-1.39	0.02
	(0.23)	(0.99)	(0.44)
>= 2 fatalities 4 wks. before RW	0.12	-0.07	0.63
	(0.38)	(1.77)	(0.50)
Observations	228,896	21,215	83,396
Mean Y - Baseline	36.80	36.84	36.14

* Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: This table shows the coefficients from inter and standard errors in parenthesis from regressions estimating the effect of fatalities on hours worked last week of Arabs and Muslims using the date of fatality. The controls in the baseline model include state-level factors (state-year population and unemployment rate), indicators for age, educational attainment (high school, some college, and college graduate), ethnicity and race (Hispanic and non-Hispanic Black), time spent in the United States (below 5 years, 5-10 years, and more than 10 years), as well as state, survey month-by-year, and occupation fixed effects. The standard errors are clustered at the state level. Regressions are weighted with person-level basic monthly survey weights. Data on fatalities is drawn from the Defense Manpower Data Center Casualty Analysis System and data on labor supply is drawn from the Current Population Survey (CPS). The sample is restricted to CPS respondents age 18-55 observed in 1997-2014.