Taste-Based Discrimination and the Labor Market Outcomes of Arab and Muslim Men in the United States

by

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ABSTRACT

Between 2001 and 2014, more than 6,500 American soldiers died while serving in Afghanistan and Iraq. Drawing on data from the Defense Manpower Data Center, which contains information on each of these soldier's home state as well as the exact date on which he or she died, we estimate the relationship between home-state fatalities and the labor market outcomes of first- and second generation Arab and Muslim men working in the United States. Because home state does not influence when, where or how the U.S. military deploys its soldiers, news of a soldier's death can be thought of as producing a temporary, state-specific shock to the degree of prejudice faced by Arab and Muslim men working in the United States. We find that home-state fatalities are essentially unrelated to wages and employment status. However, they are negatively related to hours of work, especially among Arab and Muslim men in occupations that require intense interactions with customers or clients. We argue that these results are consistent with customer taste-based discrimination but inconsistent with statistical models of discrimination.

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1. Introduction

Members of ethnic and racial minorities in the United States earn, on average, substantially less than white males with similar observable characteristics (Black et al. 2006; Biddle and Hamermesh 2013; Wilson and Rodgers 2016). For instance, Wilson and Rodgers (2016) found that black men earned 23 percent less than white men after adjusting for age and educational attainment; Black et al. (2006) found that Hispanic men earned 9 percent less than white men after adjusting for age and educational attainment.

One potential explanation for these earnings gaps is taste based: employers, customers or coworkers are prejudiced towards ethnic and racial minorities, which translates into lower relative wages, hours, and earnings (Becker 1957, 1971). Statistical discrimination (Phelps 1972; Aigner and Cain 1977) and difficult-to-measure skill differentials (O'Neill 1990; Neal and Johnson 1996) represent alternative explanations with distinct policy implications.

Previous researchers have struggled to isolate the effects of taste-based discrimination from those of statistical discrimination and difficult-to-measure skill differentials (Charles and Guryan 2011, 2013). In an influential review of the discrimination literature, Charles and Guryan (2011) argued that the most promising strategy for gaining a better understanding of why members of minority groups are treated differently than white men in the labor market would be through exploiting exogenous variation in prejudice. However, identifying such variation has proven to be exceedingly difficult.[1]

[1] Specifically, Charles and Guryan (2011) wrote:

> …most empirical exercises in the literature simply do not convincingly test between discrimination models. An approach that we believe offers some promise would be to analyze directly the two quite different mechanisms that the two types of models emphasize. Ideally, this direct evidence would employ persuasive research designs, including exogenous variation in prejudice or in information.

This is the first study to exploit plausibly exogenous variation in prejudice. Between 2001 and 2014, more than 6,500 American soldiers died while serving in Afghanistan and Iraq, two countries with majority-Muslim populations. Drawing on data from the Defense Manpower Data Center, we observe each soldier's home state as well as the exact date on which he or she died. Because home state does not influence when, where or how the U.S. military deploys its soldiers, we argue that news of a soldier's death can be thought of as producing a temporary, state-specific shock to the degree of prejudice faced by Arab and Muslim men working in the United States.

Our data are drawn from the Current Population Survey (CPS) Outgoing Rotation Groups (ORG), which contain information on the respondent's country of origin and his or her parents' country of origin. We focus on first- and second-generation Arab and Muslim men (hereafter referred to as Arab and Muslim men). After merging the CPS data with information on fatalities among U.S. soldiers serving in Afghanistan and Iraq, we estimate a model that controls for labor market conditions at the state-level and nation-wide shocks to the labor market outcomes of Arab and Muslim men.

We find little evidence that events in Afghanistan and Iraq influenced the labor force participation and employment status of Arab and Muslim men in the United States. However, we find a strong, negative relationship between hours worked by Arab and Muslim men and the number of U.S. soldiers from their state of residence who died over the course of the interview month, a result that is consistent with taste-based models of labor market discrimination. Specifically, an additional home-state fatality is, on average, associated with 0.2 fewer hours worked per week, but this effect does not appear to be permanent: one month after

the fatalities occurred, the estimated effect on usual hours worked and hours of work last week is small and statistically insignificant.

During the period under study, we observe 163 state-month combinations with 6 or more fatalities. The negative relationship between home-state fatalities and usual hours of work is driven by these particular state-month combinations. The estimated effect of 5 or fewer home-state fatalities, while negative, is small and statistically insignificant at conventional levels, while the estimated effect of 6 or more home-state fatalities is substantial in terms of magnitude. Specifically, experiencing 6 or more home-state fatalities is associated with 1.7 fewer hours worked last week by Arab and Muslim men, or a 4.0 percent reduction. When, following Kilbourne et al. (1994), we restrict the sample to Arab and Muslim men who were employed in jobs that required intense interactions with customers or clients (e.g., cashiers, dentists, salesmen, and waiters), we find an almost 8 percent reduction in hours worked last week when 6 or more soldiers from their state died during the interview month. By contrast, there is little evidence of a relationship between fatalities and hours of work among Arab and Muslim men in occupations that did not require intense interactions with customers or clients.

Finally, the results described above are robust to several changes in specification. For instance, they are robust to including (or not including) state-specific linear trends and they are robust to restricting the sample to the fourth wave of the ORG sample. When we exclude men from countries such as Indonesia and Malaysia, there is still a strong negative relationship between hours of work and fatalities. However, there is no evidence of a relationship (either positive or negative) between fatalities and the hours worked by men from Western European countries such as France, Germany, Spain, and the United Kingdom. This last result suggests that we have identified the effect of changing tastes as opposed to labor market conditions.

Because home-state fatalities should contain no information about the productivity of Arab and Muslim men working in the United States, we conclude that our results cannot be explained by models of statistical discrimination.

2. Background

2.1. Discrimination against Arabs and Muslims living in the United States

Discrimination researchers have, over last several decades, been focused on documenting and explaining the experiences of black Americans. However, as noted by Altonji and Blank (1999), examining the experiences of other racial and ethnic groups could yield important insights.

A clear majority of Americans (59%) believe that Muslims living in the United States face substantial discrimination (Pew Research Center 2016), and there is evidence, albeit anecdotal, that the wars in Afghanistan and Iraq generated, or at least intensified, feelings of animus towards Arab Americans and Muslims (Elias 2006; Greenhouse 2010).[2] There is also evidence, albeit descriptive, that these wars contributed to workplace frictions. The number of workplace complaints filed by Muslims spiked after the September 11th terrorist attacks, fell from 2002 to 2004, and then steadily increased through 2010, the year in which combat operations in Iraq officially ended (Greenhouse 2010; Hymowitz and Green 2016).

Previous researchers interested in documenting labor market discrimination against Arabs and Muslims have typically used terrorist attacks as their source of identifying variation, which,

[2] The Pew Research Center survey was conducted in January of 2016. Respondents were asked, "In the U.S. today, is there a lot of discrimination against Muslims or not?" Fifty-nine percent answered this question in the affirmative. Amer and Hovey (2012) found that, in 2004, over half (50.1 percent) of Arab Americans exhibited the symptoms of major depression, a result the authors attributed to "profiling, discrimination, and biased anti-Arab media" (p. 415). Kira et al. (2008) found a strong positive correlation between time spent watching news about the Iraq War and post-traumatic stress disorder (PTSD) among Iraqi refugees living in the United States.

for reasons discussed below, may not isolate taste-based discrimination per se. For instance, Dávila and Mora (2005) examined the wages of first- and second-generation Arab men living in the United States over the period 2000-2002. They found that, controlling for observable factors such as educational attainment and experience, the wage gap between Arab and white men was on the order of 15 percent in 2000. By 2002, this gap had expanded significantly, a result Dávila and Mora (2005) attributed to "the vast amount of media coverage following 9-11 specifically focused on the Middle East" (p. 590).

Kaushal et al. (2007) took a similar approach to examining the effects of the September 11th terrorist attacks on the employment and earnings of Arab and Muslim men in the United States, but used non-Arab and non-Muslim immigrants as a control group.[3] These authors found that the attacks were associated with an immediate 9-percent reduction in the wages of Arab and Muslim men as compared to the wages of other immigrants. Four years after the attacks, there was little evidence that Arab and Muslim men were earning less than non-Arab and non-Muslim immigrants.

The results of Dávila and Mora (2005) and Kaushal et al. (2007) are consistent with the hypothesis that taste-based labor market discrimination against Arab and Muslim men increased after September 11, 2001. However, as noted by Dávila and Mora (2005), it is possible that the September 11th terrorist attacks revealed new information about the productivity of Arab and Muslim workers living in the United States.[4] It is also possible that there was a post-September

[3] Specifically, their control group was composed of first- and second-generation immigrants who were not from the following countries: India, Mexico, Central America, the Caribbean, and what the CPS categorizes as "other Africa." The authors excluded immigrants from Mexico, Central America, and the Caribbean to ensure that the treated and comparison groups had similar observables.

[4] While Kaushal et al. (2007, p. 304) described their results as "consistent with prejudice-based labor market discrimination," Dávila and Mora (2005, p. 593) acknowledged that their results could have been due to statistical discrimination.

11th shock to labor market conditions facing Arab and Muslim men that was not discriminatory in nature.[5]

2.2. Distinguishing between taste-based and statistical discrimination

The canonical model of taste-based discrimination was developed by Becker (1971 [1957]). He posited that some individuals incur a psychic cost when interacting with members of racial or ethnic minorities, which is added to the costs (and benefits) of labor market transactions. For instance, in the case of an employer who is prejudiced against blacks, the total cost of hiring a black worker would equal the sum of the wage and the employer's physic cost. In the case of customer discrimination, black sellers receive a lower price, offsetting the cost incurred by white customers from interacting cross-racially.

If the degree of prejudice presumably varies across employers, the wage gap will, in equilibrium, be determined by the most prejudiced employer with whom blacks interact as opposed to the average level of prejudice (Charles and Guryan 2011). Likewise, the equilibrium price received by black sellers is determined by the tastes of the marginal, as opposed to the average, customer. As the fraction of blacks in the market increases, sorting away from the most prejudiced employers is more difficult to accomplish, and the racial/ethnic wage gap will, according to the Becker model, increase (Charles and Guryan 2008).

Models of statistical discrimination, do not assume prejudice on the part of employers or their customers. Employers maximize profits by hiring the most qualified workers at prevailing

[5] In addition to Dávila and Mora (2005) and Kaushal et al. (2007), several other studies have explored the effects of the terrorist attacks on Arab and Muslim immigrants. For instance, Wang (2016) found that Arab and Muslim immigrants to the United States were less likely to enter self-employment after 9/11. Ratcliffe and von Hinke Kessler Scholder (2015) explored the effects of the 2005 London bombings on housing prices and segregation. See also Åslund and Rooth (2005) and Helly (2004).

wages, but, for some reason, predicting the productivity of black job candidates is more difficult than predicting the productivity of white job candidates. If black and white workers are, on average, equally productive, black workers whose productivity is above the mean will be paid less that their equally qualified white counterparts, while black workers whose productivity is below the mean will be paid more than their white counterparts (Aigner and Cain 1977).

From an empirical perspective, distinguishing between tasted-based and statistical models of discrimination has proven to be extremely challenging because they often produce similar predictions. Charles and Guryan (2011) noted that "most tests in the literature, purporting to support or reject a particular type of model, are based on findings that are potentially explicable by some version of the other type of discrimination model." In fact, only a handful of studies provide direct evidence that can be used to distinguish between the two classes of discrimination models.

Autor and Scarborough (2008) studied the effect of introducing a computer-scored personality test at a firm on the productivity of black hires and the proportion of black hires. Using arguably exogenous variation in when this test was introduced across over 1000 establishments, they found a 10 percent increase in employee tenure among both black and white works, but the test did not impact the fraction of new hires who were black. This latter result suggests that improved information about a worker's quality increased productivity among hired workers, which is consistent with the predictions of the standard statistical discrimination model.

Using data on racial sentiment from the General Social Survey (GSS), Charles and Guryen (2008) created a state-based prejudice index among whites. Their results were consistent with the predictions of the Becker model. Specifically, they found that the black-white wage gap was positively related to the degree of prejudice of the marginal white, but essentially unrelated

to average levels of prejudice among whites.[6] They also found that the black-white wage gaps was positively related to the proportion of blacks in the state.

3. Data

Our data on causalities were obtained from the Defense Manpower Data Center (DMDC) and merged with CPS data.[7] The DMDC data include the exact date of death and home state for servicemen who died while serving in Afghanistan and Iraq. For servicemen who were injured, we know the date when the injury took place, but we do not know anything about the seriousness of the injury. The DMDC data were merged with CPS data based on the month in which the respondent was interviewed and his home state.

Our primary sample is composed of first- and second-generation Arab and Muslim men between the ages of 18 and 55 who were interviewed by the CPS ORG between October of 1997 and August of 2014. These men were born in an Arab or a majority-Muslim country, or one of their parents was born in an Arab or majority-Muslim country. In addition, we constructed a sample composed of first- and second-generation immigrants from Western European countries to be used for falsification tests.[8]

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[6] Charles and Guryen (2008, p. if pp. 779-780) argued that, if p is the percentage of blacks in the workforce, the prejudice of the marginal white could be measured by the p^{th} percentile of the prejudice distribution.

[7] Aggregate data on casualties can be obtained from the Defense Casualty Analysis System, or DCAS. They are available at: https://www.dmdc.osd.mil/dcas/pages/main.xhtml. We obtained micro-level data on casualties directly from DMDC, including information on home state.

[8] The Western European countries were: The Azores, Belgium, Denmark, England, Finland, France, Iceland, Ireland, Italy, The Netherlands, Northern Ireland, Norway, Portugal, Scotland, Spain, Sweden, Switzerland, and Wales. The Arab and majority-Muslim countries were: 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 explore the sensitivity of our results to excluding men from Indonesia, Malaysia, Pakistan, and other Middle Eastern countries.

Table 1 provides descriptive statistics for the samples analyzed below. We have information on a total of 15,932 Arab and Muslim men, most of whom (83 percent) were in the labor force when interviewed by the CPS. Of those participating in the labor force, 93% were employed at an average wage of 22 dollar per hour. Arab and Muslim respondents reported usually working an average of 43.08 hours per week, and 42.64 hours the week prior to being interviewed. Although respondents in the Western European sample worked comparable hours, they made 9% more per hour, were more likely to be a U.S. citizen, and were much more likely to have been born in the United States than Arab and Muslim men.

In Table 2 we report the total number of fatalities among U.S. soldiers serving in Afghanistan and Iraq during the period 1997-2014. Appendix Table 4 provides analogous information for soldiers wounded in action. The first troops were not deployed to Afghanistan until October, 2001 (Tucker 2010). Before that date, fatalities were zero in every year. After the invasion of Iraq, in March of 2003, total fatalities increased sharply and remained above 500 until 2011. Non-fatal injuries were much more common than fatalities. For instance, there were 887 fatalities in 2004 and almost 300,000 soldiers were wounded in action that same year.

4. Identification strategy

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).

To estimate the effects of casualties in Afghanistan and Iraq on the labor market outcomes of Arab and Muslim men working in the United states, we estimate the following baseline regression:

$$y_{ism} = \alpha + \sum_{j=-2}^{2} \partial_j^F Fatalities_{sm+j} + \sum_{j=-2}^{2} \partial_j^W Wounded_{sm+j} + \boldsymbol{\beta}' \boldsymbol{X}_{ism} + v_s + w_m + \theta_s m + \varepsilon_{ism}$$

,

where *i* indexes individuals, *s* indexes states, and *m* indexes months (m = 1…203). Our independent variable of interest, $Fatalities_{sm}$, is equal to the number of U.S. soldiers from state *s* who died in month *m*. Its coefficient, ∂_0^F, represents the effect of an additional home-state fatality on the labor market outcome under study, y_{ism}. Two leads and two lags of $Fatalities_{sm}$ are also included on the right-hand side of the estimation equation. $Wounded_{sm}$ is equal to the number of U.S. soldiers from state *s* who were wounded while serving in month *m*.

The controls, represented by the vector $\boldsymbol{X}_{ism}$, are listed in Table 1, along with descriptive statistics and definitions. They include individual characteristics (age indicators, educational attainment, ethnicity, race, marital status, U.S. citizenship, and length of time spent in the United States) as well as state-level factors (population and the unemployment rate).[9] State fixed effects, represented by v_s, ensure that identification is based on within-state variation in the number of casualties, while the 203 month fixed effects, represented by w_m, account for any new information about the productivity of Arab and Muslim men that might be gained from events in Afghanistan and Iraq. In other words, identification comes entirely from variation in home-state

[9] Three indicators for educational attainment (high school graduate, some college, and college graduate) are included. The omitted category is composed of men without a high school education. Three indicators for time spent in the United States (1-5 years, 6-10 years, and 10 or more years) are included. The omitted category is composed of men whose parents were born outside the United States.

casualties (both fatalities and wounded) net of new information available across all U.S. employers. For instance, if employers viewed Arab 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 decision 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. News of home-state casualties was presumably disseminated through personal interactions with family members and friends of the deceased, local events such as funerals and parades, and through media reports.[10] State-specific monthly linear trends account for smoothly evolving within-state changes in labor market outcomes of Arab and Muslim men.

5. Graphical evidence

Figures 1-5 were constructed by regressing the labor market outcomes of Arab and Muslim men on the number of soldiers from state *s* who died in month *m*. Two leads and lags of home-state fatalities were also included on the right-hand side of these regressions as well as two leads and lags of home-state wounded, state fixed effects, and month fixed effects. In Figures 1-3, we explore the effects of home-state fatalities on labor force participation, employment status and log wages. Figures 4 and 5 were constructed by regressing hours of work (i.e., usual hours of work and hours worked last week) on home-state fatalities.

[10] 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 of this coverage include: Bell (2004), Posada (2005), Rotstein (2006), Kirkland (2007), Roeder (2007), Schwach (2007), Warrenoct (2007), Bynum (2008), Mitchell (2008), and Coleman (2012).

Figures 1-3 provide little evidence that labor force participation, employment status and wages were related to events in Afghanistan and Iraq. In contrast, there is strong evidence that fatalities among U.S. soldiers serving in Afghanistan and Iraq served as state-specific shocks to hours worked by Arab and Muslim men. The estimated coefficients of the two leads are small and statistically indistinguishable from zero in Figures 4 and 5, while home-state fatalities that occurred over the course of the interview month are negatively and significantly related to both usual hours worked by Arab and Muslim men and hours worked in the previous week. Specifically, an additional fatality is, on average, associated with 0.29 fewer hours worked per week and 0.27 hours worked in the last week. These effects, however, do not appear to be permanent: the estimated coefficients of $Fatalities_{sm-1}$ and $Fatalities_{sm-2}$ are small and not statistically significant at conventional levels.

6. Results

6.1. Labor force participation and employment

We turn now to estimates of the fully specified regression model, which includes state-specific linear time trends and the controls listed in Table 1. In general, estimates of the fully specified model are very similar to the bare-bones estimates discussed in the previous section, suggesting that we have identified a valid natural experiment.

In Table 3, we begin by examining the labor force participation and employment decisions of Arab and Muslim men. Estimates of ∂^F are, without exception, small and statistically insignificant at conventional levels. As a falsification test, we also report estimates of the relationship between home-state fatalities and these outcomes among men from Western European countries, who, presumably, were not subject to discrimination stemming from the

events in Afghanistan and Iraq. These estimates are also small, although both leads of ∂^F are significant in the labor force participation equation. Finally, estimates of the relationship the effect of home-state wounded, ∂^W, which are not reported in Table 3, are consistently small and statistically insignificant at conventional levels.

6.2. Wages

In Table 4, we report estimates of the relationship between home-state fatalities and the wages of Arab and Muslim men. Consistent with the notion that wages of Arab and Muslim men are not sensitive to events in Afghanistan and Iraq, the estimate of ∂_0^F is small and statistically insignificant at conventional levels, although the estimate of ∂_{-2}^F is negative and significant. Likewise, estimates of the relationship between home-state fatalities and the wages of Western European men are small and statistically insignificant.[11]

6.3. Hours of work

In Table 4, we report estimates of the relationship between home-state fatalities and hours of work based on the fully specified regression model. An additional home-state fatality is associated with 0.21 fewer hours usually worked by Arab and Muslim men. Replacing usual hours of work with hours worked last week produces a similar estimate: an additional home-state fatality is associated with 0.20 fewer hours worked in the past week. The estimated effects on hours of work one and two months after the fatalities occurred are small and statistically insignificant, suggesting that there is a quick rebound to normal. Estimates of the relationship between home-state fatalities and the hours of Western European men are small and statistically

[11] Estimates of ∂^W, which are available in Appendix Table 1, are generally small and statistically insignificant.

insignificant, suggesting that the negative relationship between home-state fatalities among Arab and Muslim men is not driven by local labor market conditions.[12]

6.4. Alternative samples of Arab and Muslim men

Up to this point in the analysis, we have used a very broad definition of first- and second-generation of Arab and Muslim men. In columns (1) through (3) of Table 5, we test whether the estimates of the relationship between home-state fatalities and hours of work are sensitive to using a more restrictive definition. Specifically, we exclude first- and second generation men from Indonesia, Malaysia, Pakistan and men from countries under the category of "other Middle east".

The results are similar to those discussed above. The estimated relationship between home-state fatalities and usual hours of work is similar to the estimate reported in Table 4 and statistically significant at the 1 percent level. The estimated relationship between home-state fatalities and hours worked last week is a little smaller than that reported in Table 4 and significant at the 10 percent level.

In columns (4) through (6) of Table 5, we restrict the sample to the fourth wave of the CPS sample, just before households are dropped from the survey for the first time. This ensures that every respondent is observed only once. The results are similar to those discussed above: the estimated effects of home-state fatalities on usual hours of work and hours of work last week are statistically significant and of comparable magnitude to the baseline estimates.

[12] In Appendix Table 1, we report estimates of the relationship between home-state wounded and hours of work, ∂^WThe estimates of ∂_0^W are small and statistically insignificant at conventional levels suggesting that employers, customers or clients reacted to the death of soldiers, but did not react to non-fatal injuries suffered in combat.

6.5 Estimates by Type of Employment

It is possible that Arab and Muslim workers were themselves choosing to supply fewer hours of work in response to home-state fatalities, perhaps because they were afraid to mingle with the general public on their commutes to and from work. If this were the case, then the estimates of ∂^F in Tables 4 and 5 could not be interpreted as evidence of discrimination on the part of customers and/or employers.

In an effort to address this issue, we split the sample into Arab and Muslim men who were self-employed versus employees with the idea that those belonging to the first group could more easily adjust their hours of work (Farber 2005) but should have been subject to the same fear of retaliation after the occurrence of fatalities in Afghanistan or Iraq.

The results are reported in Table 6. Although the estimated effect of home-state fatalities on usual hours of work is of similar magnitude for self-employed Arab and Muslim men vs. employees, it is not statistically distinguishable from zero when the sample is restricted to the self-employed. Moreover, the estimated effect of home-state fatalities on hours worked last week is significantly smaller in the self-employed subsample, suggesting that the negative relationship between home-state fatalities and hours of work documented in Tables 4 and 5 was caused by discriminatory behavior on the part of customers and/or employers.

6.6. Non-linear estimates

We have, for the sake of convenience, up to this point in the analysis been forcing the effects of home-state fatalities to be linear. In Table 7 we replace $Fatalities_{sm}$ with two indicators. The first is equal to 1 if 1-5 soldiers from state *s* died while serving in Afghanistan and Iraq in month *m* (and equal to 0 otherwise). The second is equal to 1 if 6 or more soldiers

from state *s* died while serving in Afghanistan and Iraq in month *m* (and equal to 0 otherwise). Two lags and two leads of these indicators are also included on the right-hand side of the estimating equation. Similar indicators with two lags and leads were included for wounded soldiers in state *s* in month *m*. During the period under study, we observe a total of 3,149 state-months with non-zero values for $Fatalities_{sm}$. Of these, $Fatalities_{sm}$ took on a value of 6 or greater 163 times, or 5 percent of the time.

The results suggest that the negative relationship between home-state fatalities and hours of work is driven by the state-month combinations in which 6 or more soldiers died. The estimated coefficients of the indicator for 1-5 fatalities in state *s* and month *m* is generally small and statistically insignificant. In contrast, the estimated coefficients of the indicator for 6 or more fatalities are much larger and statistically significant. Arab and Muslim men reported working 1.4 fewer usual hours when 6 or more soldiers from their state of residence died during the interview month; they reported working 1.7 fewer hours last week when 6 or more soldiers from their state of residence died during the interview month. This first estimate represents a 3 percent reduction in the usual hours of work, while the second represents a 4 percent reduction in hours worked last week.

6.7. Estimates by occupation type

Having demonstrated a negative relationship between home-state fatalities and hours of work, we are now in a position to further explore potential mechanisms. Again, we are interested in whether this relationship can be ascribed to customer or employer discrimination.

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).[13]

Following Kilbourne et al. (1994), we split the sample of Arab and Muslim men using their 3-digit SOC occupation codes. About 32% of those who were employed reported an occupation that required intense interactions with customers or clients (n=3,959). Examples of such occupations include cashiers, chiropractors, dentists, hairdressers, parking attendants, salesmen, social workers and waiters. The remainder of Arab and Muslim CPS respondents (n = 8,263) reported an occupation that did not require intense interactions. We label these latter occupations, "non-interactive". If customers and clients reacted to reports of home-state fatalities by avoiding contact with Arabs and Muslims, then we should observe larger estimates of ∂^F for interactive occupations.

In Table 8, we report regression estimates of the non-linear regression model by occupation type. The results provide strong evidence that the effects of home-state fatalities on hours of work were largest among Arab and Muslim men in interactive occupations as defined by Kilbourne et al. (1994), and that the state-month combinations with more than 6 fatalities appear to be driving the relationship been home-state fatalities and hours of work. Experiencing 6 or more home-state fatalities is associated with 2.3 fewer hours usually worked, or an almost 5

[13] 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, retails sales, and salespersons occupations in the interactive category.

percent reduction relative to the mean. Experiencing 6 or more home-state fatalities is associated with 3.3 fewer hours worked in the last week, an almost 8 percent reduction relative to the mean.

7. Conclusion

Previous researchers interested in documenting labor market discrimination against Arabs and Muslims have typically used terrorist attacks as their source of identifying variation. For instance, Dávila and Mora (2005) found that, controlling for observable factors such as educational attainment and experience, the wage gap between Arab and white men expanded significantly after the terrorist attacks of September 11, 2001. Kaushal et al. (2007) found that these attacks were associated with an immediate 9-percent reduction in the wages of Arab and Muslim men as compared to the wages of other immigrants, but 4 years after the attacks, there was little evidence that Arab and Muslim men were earning less than non-Arab and non-Muslim immigrants.

The results of Dávila and Mora (2005) and Kaushal et al. (2007) are consistent with the hypothesis that taste-based labor market discrimination against Arab and Muslim men increased after September 11, 2001. However, it is possible that the September 11th terrorist attacks revealed new information about the productivity of Arab and Muslim workers living in the United States (Dávila and Mora 2005). Moreover, it is impossible to rule out the possibility that there was a post-September 11th shock to labor market conditions facing Arab and Muslim men that was non-discriminatory in nature.

Between 2001 and 2014, more than 6,500 American soldiers died while serving in Afghanistan and Iraq. Drawing on data from the Defense Manpower Data Center, which contains information on each of these soldier's home state as well as the exact date on which he

or she died, we estimate the relationship between home-state fatalities and the labor market outcomes of first- and second generation Arab and Muslim men working in the United States. We argue that, because home state does not influence when, where or how the U.S. military deploys its soldiers, news of a soldier's death can be thought of as producing a temporary, state-specific shock to the degree of prejudice faced by Arab and Muslim men working in the United States.

We find that home-state fatalities are unrelated to wages and employment status. In contrast, an additional home-state fatality is, on average, associated with 0.4 fewer hours worked per week. When, the sample is restricted to Arab and Muslim men who were employed in jobs that required intense interactions with customers or clients (e.g., cashiers, dentists, salesmen, and waiters), an additional fatality is associated with 0.9 fewer hours worked per week.

During the period under study, we observe 163 state-month combinations with 6 or more fatalities. The negative relationship between home-state fatalities and hours of work is driven by these particular state-month combinations. The estimated effects of 5 or fewer fatalities, while negative, are small and statistically insignificant at conventional levels, while experiencing 6 or more fatalities is associated with a 3-4 percent reduction in hours of work. Arab and Muslim men in jobs that require interacting with customers experienced an almost 5 percent reduction in usual hours worked when 6 or more soldiers from their state died during the interview month, and they experienced an almost 8 percent reduction in hours worked last week when 6 or more soldiers from their state died during the interview month. Because home-state fatalities should contain no information with regard to the productivity of Arab and Muslim men working in the United States, these results cannot be explained by models of statistical discrimination.

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Figure 1. Fatalities and Labor Force Participation

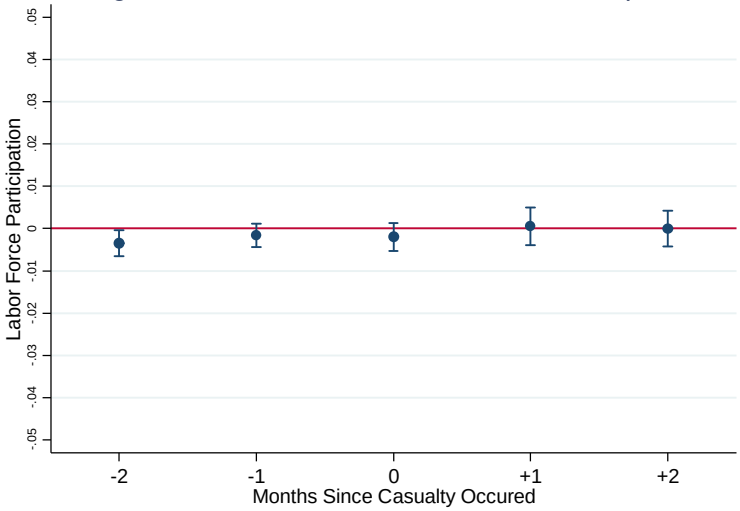

Figure 2. Fatalities and Employment

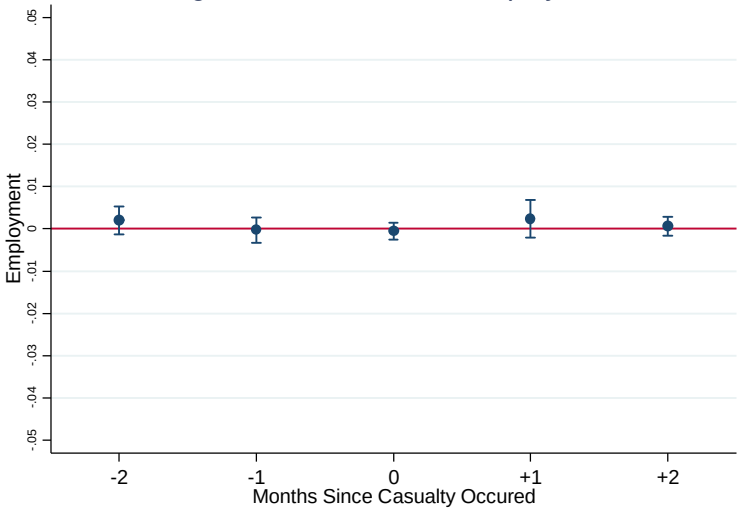

Figure 3. Fatalities and Wages

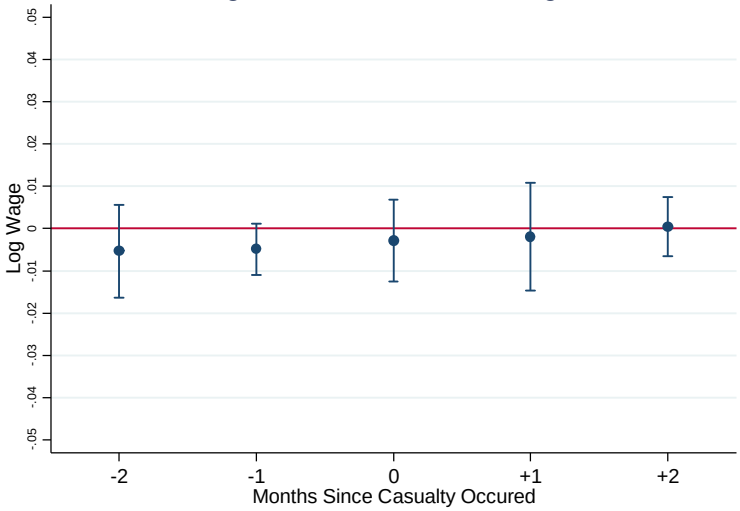

Figure 4. Fatalities and Usual Hours of Work

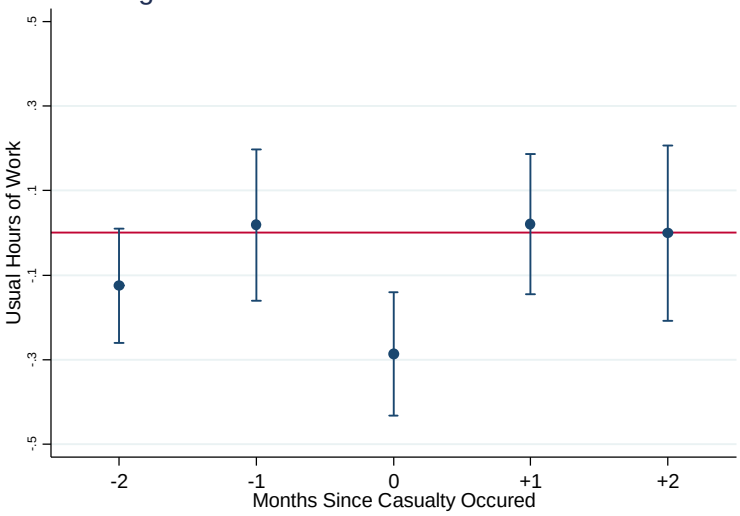

Figure 5. Fatalities and Hours of Work Last Week

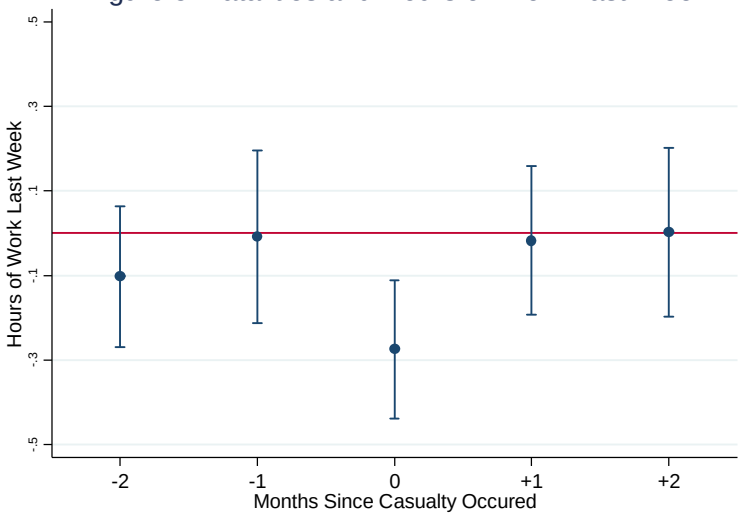

Table 1. Descriptive Statistics for CPS Sample, 10/1997 through 8/2014

	Arab and Muslim Immigrants	Western European Immigrants
Age	36.10	39.22
	(10.48)	(10.32)
High school	18.6%	24%
Some college	24%	27%
College	50%	42%
Married	57%	60%
Hispanic	1%	5%
Black	4%	2%
U.S. citizen	62%	80%
1-5 years in U.S.	19%	6%
6-10 years in U.S.	14%	4%
Above 10 years in U.S.	49%	27%
Born in the U.S.	18%	63%
Unemployment rate	6.44	5.94
	(2.30)	(2.15)
Labor force participation	83%	89%
Sample Size	15,932	32,467
	Conditional on labor force participation	
Employment	93%	95%
Sample Size	13,161	28,823
Usual hours of work	43.08	44.08
	(12.73)	(11.05)
Sample size	12,222	27,410
Hours of work last week	42.64	43.61
	(13.67)	(12.63)
Sample size	11,842	26,557

Notes: Samples are composed of male CPS respondents between the ages 18 and 55. Standard deviations are in parentheses.

Table 2. Fatalities by Year, 1997-2014

	State-level Statistics				
Year	Total	Mean	S.D.	Min	Max
1997	0	0	0	0	0
1998	0	0	0	0	0
1999	0	0	0	0	0
2000	0	0	0	0	0
2001	11	0.22	0.46	0	2
2002	49	0.96	1.46	0	7
2003	516	10.12	10.46	0	56
2004	887	17.39	19.84	0	111
2005	921	18.06	18.37	0	81
2006	905	17.75	17.62	0	90
2007	999	19.59	22.01	0	127
2008	458	8.98	10.05	0	49
2009	451	8.84	9.61	0	45
2010	551	10.80	10.41	0	47
2011	461	9.04	10.93	0	56
2012	311	6.10	7.83	0	46
2013	128	2.51	3.09	0	15
2014	49	0.96	1.43	0	8

Source: Defense Manpower Data Center

Table 3. Fatalities, Labor Force Participation and Employment

	Arabs and Muslims		Western Europeans	
	Labor force participation	Employed	Labor Force participation	Employed
	(1)	(2)	(3)	(4)
Number of fatalities:				
two months before interview	-0.0021	0.0024	0.0029**	0.0004
	(0.001)	(0.002)	(0.001)	(0.001)
one month before interview	-0.0012	-0.0003	-0.0023*	0.0000
	(0.001)	(0.001)	(0.001)	(0.001)
month of interview	-00007	0.0001	-0.0008	0.0001
	(0.002)	(0.001)	(0.002)	(0.001)
one month after interview	0.0018	0.0021	0.0001	-0.0003
	(0.002)	(0.002)	(0.001)	(0.001)
two months after interview	-0.0003	0.0003	-0.0011	-0.0007
	(0.002)	(0.001)	(0.001)	(0.001)
Mean of dependent variable	0.83	0.93	0.89	0.95
Sample size	15,932	13,161	32,467	28,823

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

Notes: Samples are composed of male CPS respondents between the ages 18 and 55, 1997-2014. Each column represents the results from a separate OLS regression. Controls at the individual levelinclude age, educational attainment, ethnicity, race, marital status, U.S. citizenship, and length of time spent in the U.S. Regressions also include the state population and unemployment rate, state fixed effects, month fixed effects, and state-specific linear time trends. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 4. Fatalities, Wages, and Hours of Work

	Arabs and Muslims			Western Europeans		
	log hourly wage	usual hours work	hours of work last week	log hourly wage	usual hours work	hours of work last week
	(1)	(2)	(3)	(4)	(5)	(6)
Number of fatalities:						
two months before interview	-0.0048**	-0.1113*	-0.0759	-0.0002	-0.0276	0.0115
	(0.002)	(0.060)	(0.079)	(0.002)	(0.047)	(0.054)
one month before interview	-0.0021	0.0215	-0.0112	0.0013	-0.0032	-0.0605
	(0.003)	(0.078)	(0.089)	(0.002)	(0.070)	(0.070)
month of interview	0.0046	-0.2070***	-0.2034**	0.0018	-0.0537	-0.0780
	(0.0046)	(0.058)	(0.090)	(0.002)	(0.041)	(0.062)
one month after interview	-0.0001	0.0380	0.0053	0.0024	-0.0735	-0.0886
	(0.003)	(0.053)	(0.068)	(0.002)	(0.058)	(0.064)
two months after interview	0.0042*	-0.0997	-0.0855	0.0025	0.0124	0.0240
	(0.002)	(0.083)	(0.084)	(0.002)	(0.042)	(0.050)
Mean of dependent variable	2.87	43.09	42.64	3.00	44.08	43.61
Sample size	10,103	12,222	11,842	23,569	27,410	26,557

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

Samples are composed of male CPS respondents between the ages 18 and 55, 1997-2014.

For list of controls see notes to Table 3. Standard errors, corrected for clustering at the state level, are in parentheses.

Table 5. Fatalities, Wages, and Hours of Work for Arab and Muslim Men Using Alternative Sample

	Excluding non-Arab Countries			Restricted to 4th ORG		
	log hourly wage	usual hours work	hours of work last week	log hourly wage	usual hours work	hours of work last week
	(1)	(2)	(3)	(4)	(5)	(6)
Number of fatalities:						
two months before interview	-0.0065	-0.1268*	-0.1495*	-0.0015	-0.1305	-0.1212
	(0.005)	(0.068)	(0.082)	(0.004)	(0.080)	(0.098)
one month before interview	-0.0032	0.0409	0.0818	0.0012	-0.0539	-0.0888
	(0.003)	(0.084)	(0.092)	(0.004)	(0.092)	(0.107)
month of interview	0.0033	-0.2139***	-0.1830*	-0.0029	-0.3125***	-0.2845***
	(0.004)	(0.062)	(0.093)	(0.006)	(0.079)	(0.101)
one month after interview	-0.0007	0.0004	-0.0013	-0.0035	0.1546	0.0862
	(0.004)	(0.075)	(0.098)	(0.004)	(0.095)	(0.116)
two months after interview	-0.0007	-0.1507	-0.1639*	0.0046	-0.1110	-0.0286
	(0.004)	(0.093)	(0.093)	(0.004)	(0.088)	(0.104)
Mean of dependent variable	2.87	43.11	42.69	2.87	43.19	43.65
Sample size	6,782	8,380	8,089	5,050	6,132	5,931

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

Notes: Samples in columns 1-3 are composed of male CPS respondents between the ages 18 and 55 exluding individuals from Indonesia, Malaysia, Pakistan, and "other Middle East". Samples in columns 4-6 included all Muslim and Arab men but are limited to respondents in the fourth wave of ORG. For list of controls see notes to Table 3. Standard Errors, corrected for clustering at the state level, are in parentheses.

Table 6. Fatalities and Hours of Work by Type of Employment

	Usual hours of work		Hours of work last week	
	Self-employed	Employees	Self-employed	Employees
	(1)	(2)	(3)	(4)
Number of fatalities:				
two months before interview	-0.0721	-0.0882	0.0076	-0.0708
	(0.194)	(0.065)	(0.235)	(0.081)
one month before interview	-0.1461	0.0749	-0.0467	0.0168
	(0.187)	(0.071)	(0.250)	(0.076)
month of interview	-0.2314	-0.2128***	-0.0875	-0.2566**
	(0.219)	(0.060)	(0.256)	(0.098)
one month after interview	0.0083	0.0524	0.0700	0.0077
	(0.249)	(0.046)	(0.222)	(0.065)
two months after interview	-0.1513	-0.0823	-0.0755	-0.0779
	(0.178)	(0.084)	(0.195)	(0.093)
Mean of dependent variable				
Sample size	2,090	10,116	2,017	9,809

* Statistically significant at the 10% level; ** at 5% level; *** at 1% level.
Samples are composed of Muslim and Arab male CPS respondents between the ages 18 and 55, 1997-2014. For list of controls see notes to Table 3.
Standard errors, corrected for clustering at the state level, are in parentheses.

Table 7. Fatalities, Wages and Hours of Work Allowing for Non-Linear Effects

	Log hourly wage (1)	Usual hours of work (2)	Hours of work last week (3)
Number of fatalities:			
1-5 fatalities two months before interview	0.0087	-0.1274	0.0611
	(0.012)	(0.316)	(0.380)
>=6 fatalities two month before	-0.0093	-0.2817	0.2844
	(0.032)	(0.582)	(0.615)
1-5 fatalities one month before	-0.0261	0.1040	0.1079
	(0.017)	(0.255)	(0.284)
>=6 fatalities one month before	-0.0372	-0.7168	-1.3758
	(0.025)	(0.680)	(0.926)
1-5 fatalities month of interview	0.0144	-0.1716	-0.3933
	(0.016)	(0.344)	(0.293)
>=6 fatalities month of interview	0.0206	-1.3525**	-1.6568***
	(0.028)	(0.543)	(0.492)
1-5 fatalities one month after	-0.0087	-0.1768	-0.2754
	(0.014)	(0.317)	(0.333)
>=6 fatalities one month after	0.0053	-0.0644	-0.3208
	(0.028)	(0.551)	(0.641)
1-5 fatalities two months after	0.0182	0.1256	0.0260
	(0.013)	(0.391)	(0.433)
>=6 fatalities two months after	0.0044	-0.9328	-0.5595
	(0.019)	(0.667)	(0.747)
Mean of dependent variable	2.87	43.09	42.64
Sample size	10,103	12,222	11,842

* Statistically significant at the 10% level; ** at 5% level; *** at 1% level.
Samples are composed of Muslim and Arab male CPS respondents between the ages 18 and 55, 1997-2014. For list of controls see notes to Table 3.
Standard errors, corrected for clustering at the state level, are in parentheses.

Table 8. Fatalities, Wages and Hours of Work by Occupation Type

	Usual hours of work		Hours of work last week	
	Interactive occupations (1)	Non-interactive occupations (2)	Interactive occupations (3)	Non-interactive occupations
Number of fatalities:				
1-5 fatalities two months before interview	0.3308	-0.4520	0.7734	-0.4624
	(0.600)	(0.348)	(0.620)	(0.433)
>=6 fatalities two month before	-0.0729	-0.3973	0.2821	0.1417
	(1.368)	(0.605)	(1.241)	(0.630)
1-5 fatalities one month before	-0.1688	0.2007	-0.2181	0.2352
	(0.518)	(0.327)	(0.562)	(0.370)
>=6 fatalities one month before	0.5307	-1.3238	-0.2422	-2.0312*
	(0.901)	(0.796)	(0.983)	(1.097)
1-5 fatalities month of interview	0.1961	-0.4301	-0.3499	-0.5505*
	(0.579)	(0.352)	(0.689)	(0.304)
>=6 fatalities month of interview	-2.3241**	-0.8702*	-3.3336***	-0.6207
	(1.130)	(0.438)	(1.123)	(0.482)
1-5 fatalities one month after	0.5407	-0.5026	-0.1141	-0.3349
	(0.780)	(0.359)	(0.640)	(0.422)
>=6 fatalities one month after	0.4240	-0.4674	-0.4756	-0.3322
	(1.151)	(0.641)	(1.477)	(0.817)
1-5 fatalities two months after	0.8628	-0.3163	0.2466	-0.2204
	(0.644)	(0.458)	(0.678)	(0.491)
>=6 fatalities two months after	0.0466	-1.7691**	-0.5916	-1.0538
	(1.090)	(0.795)	(1.232)	(0.914)
Mean of dependent variable	44.77	42.28	44.28	41.85
Sample size	3,959	8,263	3,820	8,022

* Statistically significant at the 10% level; ** at 5% level; *** at 1% level.
Samples are composed of Muslim and Arab male CPS respondents between the ages 18 and 55, 1997-2014. For list of controls see notes to Table 3.
Standard errors, corrected for clustering at the state level, are in parentheses.

Table Appendix 1. Fatalities and Labor Market Outcomes

	Labor force participation (1)	Employed (2)	Log hourly wage (3)	Usual hours of work (4)	Hours of work lase week (5)
Number of fatalities:					
two months before interview	-0.0021	0.0024	-0.0048**	-0.1113*	-0.0759
	(0.001)	(0.002)	(0.002)	(0.060)	(0.079)
one month before interview	-0.0012	-0.0003	-0.0021	0.0215	-0.0112
	(0.001)	(0.001)	(0.003)	(0.078)	(0.089)
month of interview	-00007	0.0001	0.0046	-0.2070***	-0.2034**
	(0.002)	(0.001)	(0.0046)	(0.058)	(0.090)
one month after interview	0.0018	0.0021	-0.0001	0.0380	0.0053
	(0.002)	(0.002)	(0.003)	(0.053)	(0.068)
two months after interview	-0.0003	0.0003	0.0042*	-0.0997	-0.0855
	(0.002)	(0.001)	(0.002)	(0.083)	(0.084)
Number of wounded:					
two months before interview	0.0018***	-0.0004	-0.0010	0.0153	0.0004
	(0.001)	(0.001)	(0.001)	(0.030)	(0.038)
one month before interview	-0.0000	-0.0002	0.0011	-0.0180	-0.0558**
	(0.001)	(0.000)	(0.001)	(0.022)	(0.024)
month of interview	0.0005	-0.0011**	-0.0001	-0.0025	0.0269
	(0.001)	(0.000)	(0.001)	(0.030)	(0.038)
one month after interview	-0.0014**	-0.0005	-0.0018	0.0148	0.0216
	(0.001)	(0.000)	(0.001)	(0.018)	(0.017)
two months after interview	0.0001	0.0005	-0.0003	0.0466*	0.0611**
	(0.001)	(0.000)	(0.001)	(0.025)	(0.029)
Mean of dependent variable	0.83	0.93	2.87	43.09	42.64
Sample size	15,932	13,161	23,569	12,222	11,842

* Statistically significant at the 10% level; ** at 5% level; *** at 1% level.
Samples are composed of Muslim and Arab male CPS respondents between the ages 18 and 55, 1997-2014. For list of controls see notes to Table 3.
Standard errors, corrected for clustering at the state level, are in parentheses.

Table Appendix 2. Fatalities and Log Hours of Work

	Arabs and Muslims		Western Europeans	
	Log usual hours of work (1)	log hours of work last week (2)	Log usual hours of work (3)	log hours of work last week (4)
Number of fatalities:				
two months before interview	-0.0021	-0.0044*	-0.0008	-0.0002
	(0.001)	(0.002)	(0.001)	(0.002)
one month before interview	-0.0003	-0.0012	0.0001	-0.0026
	(0.002)	(0.002)	(0.001)	(0.002)
- month of interview	-0.0037***	-0.0037*	-0.0007	-0.0013
	(0.001)	(0.002)	(0.001)	(0.002)
one month after interview	0.0015	-0.0004	-0.0017	-0.0022
	(0.002)	(0.003)	(0.001)	(0.002)
two months after interview	-0.0022	-0.0013	0.0007	0.0010
	(0.002)	(0.003)	(0.001)	(0.002)
Sample size	12,218	11,842	27,401	26,557

* Statistically significant at the 10% level; ** at 5% level; *** at 1% level.
Samples are composed of male CPS respondents between the ages
18 and 55, 1997-2014. For list of controls see notes to Table 3. Standard errors,
Standard errors, corrected for clustering at the statelevel, are in parentheses.

Table Appendix 3. Fatalities and Labor Market Outcomes Controlling for Past Enlistments

	Log hourly wage (3)	Usual hours of work (4)	Hours of work lase week (5)
Number of fatalities:			
two months before interview	-0.0049*	-0.0991	-0.0595
	(0.002)	(0.059)	(0.078)
one month before interview	-0.0020	0.0216	-0.0111
	(0.003)	(0.077)	(0.090)
month of interview	0.0040	-0.2170***	-0.2156**
	(0.004)	(0.055)	(0.083)
one month after interview	-0.0001	0.0473	0.0118
	(0.003)	(0.056)	(0.072)
two months after interview	0.0040*	-0.0883	-0.0588
	(0.002)	(0.082)	(0.081)
Mean of dependent variable	2.88	43.09	42.63
Sample size	8,804	10,670	10,333

* Statistically significant at the 10% level; ** at 5% level; *** at 1% level.
Samples are composed of male CPS respondents between the ages 18 and 55, 1997-2014. For list of controls see notes to Table 3.
Standard errors, corrected for clustering at the statelevel, are in parentheses.

Table Appendix 4. U.S. Soldiers Woulded in Action by Year, 1997-2014

	State-level Statistics				
Year	Total	Mean	S.D.	Min	Max
1997	0	0	0	0	0
1998	0	0	0	0	0
1999	0	0	0	0	0
2000	0	0	0	0	0
2001	534	10.47	39.04	0	254
2002	1,366	26.78	54.87	0	201
2003	36,534	716.35	1,393.09	0	9,144
2004	296,077	5,805.43	13,539.68	5	93,726
2005	258,865	5,075.78	8,810.19	2	54,864
2006	296,489	5,813.51	12,662.59	3	87,884
2007	333,929	6,547.63	13,405.14	0	91,948
2008	157,401	3,086.29	6,695.86	0	46,228
2009	141,184	2,768.31	5,703.27	1	38,608
2010	276,481	5,421.20	10,028.80	1	66,802
2011	286,469	5,617.04	11,491.54	1	78,994
2012	168,619	3,306.26	6,327.15	3	41,148
2013	82,632	1,620.24	2,955.48	0	18,796
2014	26,539	520.37	1,328.60	0	9,144

Source: Defense Manpower Data Center