Is Discrimination Scarring? Effects of the September 11, 2001 Terror Attacks on New Entrants in the Labor Force

Jonathan Presler

Department of Economics, Syracuse University

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Abstract

In this paper, I look at whether new entrants to the labor force who are Americans of Middle Eastern, Afghan, or Pakistani descent faced scarring effects due to discrimination following the September 11, 2001 terror attacks. I break from the difference-in-difference framework common in the discrimination literature revolving around terror attacks, and I utilize an event-study model similar to those employed in the recession literature. This allows me to estimate not only initial effects due to discrimination, but also a period of recovery and the existence and duration of any scarring effects. I provide weak evidence of the existence of such scarring effects due to discrimination and discuss possible channels of these scarring effects. I conclude with potential avenues for refining the precision of my estimates in future work.

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1 Introduction

After the September 11, 2001 terror attacks, American attitudes towards Arabs worsened.[1] I utilize the attacks as an exogenous shock in attitudes towards American workers of Middle Eastern, Afghan, and Pakistani descent. A literature has developed around this question, and finds significant labor market effects due to increased discrimination. More recent work finds that younger Middle Eastern, Afghan, and Pakistani workers were most affected by discrimination (Rabby and Rogers 2011). This is intuitive, as younger workers have less experience and have lower attachment to the labor force. In fact, we see that new graduates are much more likely to be affected by the business cycle and experience "scarring" effects when graduating during a recession (Kahn 2010, Genda et al 2010, Oreopoulos et al 2012).

In this paper, I look at whether new entrants to the labor force who are Americans of Middle Eastern, Afghan, or Pakistani descent experienced scarring wage effects due to discrimination following the September 11, 2001 terror attacks. I break from the difference-in-difference framework common in the discrimination literature revolving around terror attacks, and I utilize an event-study model similar to those employed in the recession literature. This allows me to estimate not only initial effects due to discrimination, but also a period of recovery and whether there are any scarring effects, as well as their duration. The duration of scarring effects due to graduating in a recession can be quite large; Oreopoulos et al (2012) find mean effects lasting 10 years in Canada and Kahn (2010) finds effects lasting at least 17 years in the US.

Lower wages paid to workers due to discrimination are obviously detrimental to the welfare of the targeted workers, but they can also be detrimental to society as a whole.

[1] In a Gallup article written in September of 2001, Jeffrey Jones discusses changing American attitudes towards Arabs following the attacks. Jones reports that "the American public has generally held somewhat negative views of Arabs even before the recent terrorist attacks on September 11. Polls conducted since the attacks show that a significant minority of Americans report having become less positive toward Arabs. Additionally, about three out of 10 Americans say that in the last two weeks, they have heard negative comments about Arabs living in the United States, and about half to 60% are willing to support increased security measures aimed specifically at Arabs in the United States."

When labor is misvalued,[2] it is not likely to be optimally allocated, and the economy operates less efficiently. This is the first paper (to my knowledge) to look at recession-like scarring effects due to discrimination and it provides weak evidence of the existence of such scarring effects for Middle Eastern, Afghan, and Pakistani Americans entering the labor market after the September 11, 2001 terror attacks. I discuss possible mechanisms behind these scarring effects and provide possible avenues for refining estimate precision in future work. By understanding the potentially long-term effects of discrimination, future work can develop an estimate of the social costs of discrimination on the economy, in addition to the explicit harm caused to those discriminated against.

2 Literature Review

2.1 Discrimination

I begin by discussing three main theories of discrimination. I then introduce some work identifying discrimination effects and conclude by reviewing the literature related to the September 11, 2001 terror attacks specifically.

Becker's (1957) work on discrimination and his resulting model is perhaps the most famous model of discrimination in economics. The model relies on the assumption that employers have a "taste" for discrimination; that is, prejudiced employers lose some utility by hiring minority workers. Because there is a range of discriminatory attitudes among people, this creates a spectrum of magnitudes of utility loss experienced by employers, where unprejudiced employers experience no such utility loss when employing minority workers.

However, prejudiced employers may be willing to hire minority labor if it is cheap enough. The employer's decision depends upon the marginal productivity of the worker and compares

[2] Note that labor is misvalued only under some models of discrimination. Under models of statistical discrimination, the employer's expected value of labor is the driver of discrimination; likewise, if customers have a taste for discrimination, then otherwise equal minority workers may have lower productivity than majority workers.

that to the utility loss the employer endures through employing the minority worker. For example, if all workers are equally productive (regardless of race) having marginal products of 10, then a prejudiced employer will be willing to hire a minority worker for 7 only if the employer's utility loss from hiring the worker is less than 3.

If the employer is indifferent whether to hire the minority worker, then the employer is said to be the "marginal discriminator," and sets the wage price for minority workers in the market. This price setting by the marginal discriminator follows from the profit-maximizing nature of firms and assumed homogeneity of workers; this can be seen in a simple equilibrium setting in Figure 2 in Charles and Guryan (2008).[3]

We now look at the comparative statics of Becker's model, examining changes in the proportion of minority workers and shifts in taste for discrimination. An increase in the proportion of minority workers relative to majority workers in the population decreases minority worker wages, ceteris paribus. This is because the number of openings for minority workers in firms less prejudiced than the old marginal discriminator is no longer sufficient given the supply of minority labor; a new, more discriminating marginal discriminator now sets a lower wage.[4]

There are two related shifts in taste for discrimination that we can consider. The first is an increase in the taste for discrimination held by current discriminators. This is equivalent to an increase in the utility loss discriminating employers face when employing minority workers. The result is a downward shift in demand for minority labor, decreasing the wage of minority workers.[5] Related to this, we could also consider an increase in the proportion of prejudiced employers; as before, this equivalently shifts the demand for minority labor left. The increase in number of prejudiced employers means fewer employers endure no utility loss when employing minority workers.[6]

[3] For convenience, I have reproduced this figure in the appendix. See Figure A1.
[4] See the shift from S_2 to S_1 in Figure A1.
[5] This is the shift in Figure A1 from D to D'.
[6] This is not explicitly drawn in Figure A1, but can be seen as a shift in point B; The movement of B may also alter the slope of D.

I now move on to Phelps (1972), in which he develops the concept of "statistical discrimination." Statistical discrimination does not rely on an alteration of the utility function, as does Becker's model, but instead requires employers to maximize expected utility with imperfect information regarding potential employees. Phelps provides an example of discrimination which illustrates the employer's possible perspective. In this example,[7] a traveler is staying at a hotel in an unfamiliar town. The traveler plans to eat dinner, but has no prior knowledge of the restaurants in the town, although he knows that the hotel has a restaurant. Of course, the traveler would like to maximize the quality of his meal, but obtaining the information required to find the best meal at the best price in the town would be costly (this was before online reviews reduced this cost significantly). The traveler has a rule never to eat in hotel restaurants, but as a result it can be said that the traveler discriminates against hotel restaurants. However, the traveler is maximizing expected utility if the traveler's prior experience is that hotel restaurants are, on average, worse than other restaurants, and obtaining information regarding which restaurants (hotel or otherwise) are worth eating in is costly.

We now consider how this works in an employment example with the model itself. Given two apparently equally qualified candidates, one majority and one minority candidate, the employer decides which to hire. The expected profit maximizing employer may opt to hire the majority worker for one of two reasons.

The first reason is similar to the one above. If unobservable characteristics are said to be correlated with race (or gender, orientation, etc.), and these unobservables are a part of the production function, then an expected profit maximizing employer will take this into account during the hiring process. Fang and Moro (2011) give an apt example:[8] an employer is deciding between two young applicants, one male and one female. Given no other observable differences, the employer may opt to hire the male candidate over the

[7] See Phelps 1972, page 659 for the story I discuss here.

[8] Chapter 5 of the Handbook of Social Economics, Volume 1A. "Theories of Statistical Discrimination and Affirmative Action: A Survey." Page 135.

female candidate because the employer believes the female candidate has lower labor force attachment due to "a higher propensity to engage in child-rearing." For a similar reason, the employer who has hired both may decide to invest more specific human capital into the male rather than the female employee, exacerbating the issue in the future by giving the male both this perceived superiority (due to greater expected labor force attachment) along with additional credentials.

The second reason the employer may decide to hire the minority candidate is due to the employer's experience and the variance in the measure of ability, through the interview process for example. In this example, the hiring manager is a member of the majority, and is deciding between a majority and minority candidate of apparently equal quality, based on information gathered in the interview process. The manager may believe his or her assessment of the majority candidate to be more accurate than the assessment of the minority candidate, perhaps due to cultural differences. Thus if the candidates appear to be above average, the profit maximizing manager will hire the majority candidate, as the variance associated with the assessment of the minority candidate is higher and there is a greater chance the minority candidate is worse than the majority candidate. If the candidates appear to be below average, and the manager plans to hire one of them, then the inaccuracy of the measurement means the profit maximizing manager thinks there is a greater chance the minority worker is better than the majority worker, and will hire the minority worker. It may appear at first that these balance out, but this is not the case; minority workers are preferred in worse jobs, and majority workers are preferred in better ones.

There is a viciousness to Phelps' model that does not exist in Becker's. Becker's model relies on an employer's taste for discrimination - a direct modification of the utility function. We might hope that increased enlightenment of prejudiced employers either through increased exposure to minority groups or simply changing attitudes over generations would bring tastes for discrimination toward zero. Phelps, however, presents a model for discrimination that relies not on malicious intent, but expected utility maximization and imperfect information. Statistical discrimination can result in a viscous cycle if employers believe the unobservables from being a minority make them worse candidates, thereby making employers less willing to hire them and exacerbating the difference in opportunities (resources) available to the groups of workers.

Becker appears to allude to this idea, when he writes that "discrimination and prejudice are not usually said to occur when someone prefers looking at a glamorous Hollywood actress rather than at some other woman; yet they are said to occur when he prefers living next to whites rather than next to Negroes."[9] However, Phelps defines this as statistical discrimination and develops a model under which the harms to the minority group are apparent. If we take Becker's example of a big name Hollywood actor, we can look to current discussions around minority actors looking to land leading roles and directors pointing out the lack of big name minority actors[10] to see this sort of discrimination in which race appears to be part of, or correlated with, a piece of the production function for the industry.

Black (1995) creates an equilibrium search model in which he employs Becker's concept of "taste" for discrimination. Black simplifies the continuum of prejudice to have employers of two types: those who will and those who will not hire minority workers. Adding the continuum of prejudice (as in Becker) adds complexity without clear benefit or additional insight.[11] There are also two types of workers: majority and minority, and these firms and individuals are placed into a search framework. The key concept driving the wage differential in Black's model is that, in a search framework, it is costly to search for employment. Because some firms will not hire minority workers, and minority workers cannot determine which employers are prejudiced before they apply,[12] the search costs of minority workers are greater. Higher search costs imply a lower reservation wage for minority workers, and this

[9] See page 13 of Becker 1971.

[10] For example, see: http://www.latimes.com/business/hollywood/la-fi-ct-hawaii-five-0-asian-actors-20170708-story.html

[11] As we will see, minority wages will be less than majority wages, so we can think of the employers who are not willing to hire the minority workers as those whose utility loss endured when employing minority workers is greater than that experienced by the marginal discriminator.

[12] Firms do not advertise their prejudice, if for no other reason than that it is illegal in the US and penalties are high enough such that a profit maximizing firm will not wish to face them.

is a function of the proportion of prejudiced firms in the market.

This is a search model, and search costs are experienced by both minority and majority workers - thus marginal product is greater than wages for all workers and all firms have some monopsonistic power. However, because wages are lower for minority workers, unprejudiced firms are more profitable in the market. To deal with this, Black assumes a distribution of entrepreneurial talent and endogenizes the proportion of prejudiced firms in the market. This is one of the attractive elements of this model; in Becker's model, if we assume a perfectly competitive market, prejudiced employers should be forced out of the market over time. Black's model gives a way for wage differentials due to discrimination to persist; the cost for prejudiced firms is that more entrepreneurial skill is necessary for the firm to survive than if it were unprejudiced.

Recall that, in Becker's model, when the proportion of minority workers in the market increases, the wages of minority workers decrease because the marginal discriminator is more discriminating. In Black's model, however, the increase in proportion of minority workers decreases the pool of workers prejudiced firms will hire, thereby decreasing their profitability and ultimately the proportion of prejudiced firms in the market. As a result, an increase in minority workers actually increases minority wages in this model. Which scenario is more realistic is ambiguous, but empirically testable. Under Becker's model, an increase in the proportion of minority workers is a simple supply increase, which necessarily reduces the price. However, we could also imagine that an increased minority presence makes majority individuals more likely to interact with those in the minority, thereby reducing taste for discrimination through acquaintanceship or even friendship.

Another key insight in both Black and Becker is that when discrimination exists, there will be economic segregation[13] in the market. However, the number of minority workers can be low enough in Becker's model such that there is no economic discrimination. In Black's

[13] Economic segregation is where minority and majority workers work at different places. Because some firms are unwilling to hire minority workers at the current price, these firms will be entirely majority worker, and all minority workers will be constrained within a subset of firms in the market. Economic discrimination is when there is a wage differential.

model, however, there is always both economic segregation and economic discrimination.

I now shift to some empirical work on discrimination, which deals with effects of discrimination, often in the labor market.

I provide a brief discussion of Charles and Guryan (2008), in which they attempt to identify effects of discrimination through an empirical analysis of Becker's model. To do this, they use the General Social Survey to construct a one-dimensional measure of discrimination, and then match this with CPS earnings data from the 1970s through the early 2000s. They find that, in general, the predictions of Becker's model hold.

The biggest sticking point, at least for me, is the method of determining the "marginal discriminator," which is what Becker's analysis hinges upon. Charles and Guryan note that if we assume all firms to be of equal size, and if the minority group makes up $p\%$ of the population, then the p^{th} quantile of discriminators would be the marginal discriminator. This stretches credulity for two reasons. The first is that firms must be of equal size, which is certainly not true. The question is whether it is true enough, and it appears that if discriminating firms are smaller than non-discriminating firms, then the assumption may also hold true.[14] This adds plausibility to the assumption. The second and more problematic reason is brought up in Black (1995), which is that the number of prejudiced firms is endogenous. This is because they must be more efficient operators in order to stay in the market and maintain discriminatory behavior. It is also apparent that increasing the quantile that measures the marginal discriminator in tandem with the percentage minority mechanically recreates Becker's result that an increase in the population of the minority reduces their wages, so this is not a finding of the paper but of the modeling technique used.

Charles and Guryan try to assuage these concerns by noting that the tenth percentile is significant in determining black wages, whereas the median and ninetieth are not. This provides stronger evidence that the marginal discriminator is likely important for the observed wage differentials, even if they do not convincingly identify the marginal discriminator. The

[14] This is not discussed in Charles and Guryan (2008), but is the result of my own thinking on the issue.

paper provides the first attempt to empirically test Becker's model, and sheds some light on the issue, despite difficulties identifying the marginal discriminator itself. Their evidence also suggests that other discrimination mechanisms are at play as well, as they are able to explain only about a quarter of the wage gap, leaving most of the observed wage differentials to be the result of observable differences and statistical discrimination.[15]

More specifically applicable to this paper, I now discuss some of the work in the discrimination literature related to the September 11, 2001 terror attacks specifically, and terror attacks more broadly. This literature utilizes changing attitudes in response to a terror attack as a shock to discrimination felt by Muslims, Arabs, or a related group. I will begin with a focus on work relating to labor market effects, and note some of the work dealing with other discrimination effects.

Dávila and Mora (2005) were the first, to my knowledge, to look at the effects discrimination resulting from the September 11, 2001 terror attacks. Dávila and Mora cite increased complaints of employment discrimination against Arab workers in the US as well as increased complaints of religious discrimination filed by Muslim Americans with the EEOC as evidence of changing attitudes bleeding into the workplace, potentially creating measurable effects. Income data from 2000 and 2002 comes the American Community Survey (ACS), and Dávila and Mora are interested in the effects on young men age 25-40 of Arab, Afghani, Iraqi, or Pakistani descent.[16]

The empirical strategy Dávila and Mora employ first involves the estimation of a standard earnings function, which includes dummy variables for people of Middle Eastern, African Arab, or another potentially affected racial group, with non-Hispanic whites as a comparison. They estimate the effect and significance of being in each of these groups in 2000 and 2002, and use a difference-in-differences framework to test the significance of the change. In most of their models they find a significant difference, and so Dávila and Mora use a Juhn-Murphy-

[15] This does not diminish the impact of their findings. If a quarter of the black-white wage gap is explainable by taste for discrimination, that is enough to be disturbing and motivation for policy change.

[16] Note that in reviewing this and subsequent papers, the target group varies. I attempt to match the descriptors used by the authors of each of these papers.

Pierce (JMP) decomposition to see whether the change is due to observable differences or unobservable differences, and quantile regression to determine where the greatest effects were felt. They interpret unobservable differences in their JMP decomposition as discrimination and find that most of the effect is due to discrimination; the quantile regression shows that Middle Eastern Arab men from across the income spectrum were affected.

Kaushal et al (2007) is the most cited paper in this literature, with Dávila and Mora a close second. Kaushal et al employ a simple difference-in-difference earnings model over the period of September 1998 through September 2004, excluding September 2001. They are interested in the effect on Arabs and Muslims in the US, and use country of birth (either the individual or a parent) as a proxy. Their main source of data is the Current Population Survey Outgoing Rotation Groups (CPS-ORG).

In addition to finding a wage decline between of 9-11% as a result of the discrimination, Kaushal et al also attempt to determine reasons for this decline. They examine worker mobility both in terms of geography as well as in terms of occupation and industry. They found that after the terror attacks, Arab and Muslim men were more likely to switch from high to low wage industries; Kaushal et al also found that Muslim and Arab men were less likely to move between states after the attacks, which limits potential wage gains from mobility, and may contribute to the measured wage differentials.

It is reasonable to expect that some parts of the country are more or less tolerant of Arabs and Muslims than other parts of the country, and so Kaushal et al try to control for this; they use FBI hate crime statistics as a measure for religious and ethnic tolerance.[17] I remain unconvinced that this is either a good proxy or even a desirable control variable. I argue that both variation in labor market outcomes (their dependent variable) and changes in hate crimes are outcomes of underlying sentiments; we want to measure and understand the labor market effects of discrimination. I find it unlikely that those who are inclined to commit hate

[17] Kaushal et al acknowledge the measurement error inherent in these statistics. The reporting of these numbers to the FBI is not mandatory, and so certain states or jurisdictions simply provide no information to the FBI. There is also variation in how jurisdictions define, record, and report hate crimes in practice.

crimes are also the same people directly responsible for labor market discrimination. To use Becker's model as an illustration, the marginal discriminator sets the labor market outcomes, and I would argue that someone committing a hate crime is much more discriminatory than the marginal discriminator - much more of an outlier in terms of ethnic or religious intolerance. To assume that an increase in hate crimes is proportional to a change in the marginal discriminator is not an assumption I am readily willing to accept. We saw in Charles and Guryan (2008) that the tenth percentile discriminator was significant in affecting wages, but the effect of the ninetieth percentile discriminator was indistinguishable from zero. Based on the results in Kaushal et al showing the difference between high and low hate crime states in terms of labor market outcomes, it appears that the inclusion of this variable is important for the magnitude of the results Kaushal et al report, although not the sign.

Rabby and Rodgers (2011) continue in a similar vein to Kaushal et al in that they estimate a difference-in-difference earnings model with CPS-ORG data. The focus of Rabby and Rodgers is on younger Muslim men age 16-25. The idea is to see the effects on men who more closely resemble terrorists (or at least American perceptions of terrorists). Additionally, they define three nested target groups in which the broadest group is similar to that used in Kaushal et al, again with the idea of determining whether the profile of the most affected workers matches the terrorist profile they define. The narrowest group (most likely to be the target of discrimination) are Middle Eastern Arab countries, and the middle group adds individuals from Iran, Afghanistan, Pakistan, Egypt and Morocco to the narrowest group.

As expected, Rabby and Rodgers find the greatest effects among the youngest workers, and diminishing but still significant effects as they broaden their target group. They also find that low skill workers are most affected by the event, which is in contrast with Dávila and Mora. Additionally, they test whether the 2005 London bombings affected labor market outcomes for Muslims in the US and find no effect, which is consistent with other cross-border effects that have been studied.

For example, Shannon (2012) uses a difference-in-differences approach similar to Kaushal et al (2007) in order to test whether there were similar labor market effects felt by Canadian Muslims. Shannon finds no effects. Braakmann (2010) looks at the effect of 9-11 on Muslims in Europe, also using a difference-in-differences framework. He begins by replicating prior work in the US, and then uses the same time period to see if there are effects in the UK. He further tests whether a country's direct involvement matters by using both the 2005 London bombings and the 2004 Madrid bombings as events to see whether labor market outcomes in the UK were affected. However, Braakmann finds no effect in the UK for any of the three terror attacks - suggesting either that the US is fundamentally different from the UK or Europe, or that his data is insufficient to measure an effect.

The effect of terror attacks on other, non-earnings outcomes that may be affected by discrimination have also been examined. Moussa (2013) uses a difference-in-differences approach to see whether academic achievement for Arab students in the NYC public school system declined following the attacks. Moussa finds significant negative effects in standard measureable educational outcomes: test scores, grade retention, and special education designation. Johnston and Lordan (2012) use a difference-in-differences approach to find that increased discrimination in the UK as a result of the 2001 US terror attacks led to decreased health outcomes for Muslims in the UK. Using a difference-in-differences approach (as seems to be standard in this literature), Hole and Ratcliffe find that increased discrimination in the UK resulting from the 2005 London bombings decreased happiness and mental health in adolescent Muslim girls, but found no effect for Muslim boys.

Some work has also been done regarding spillover effects. For example, Wang and Wang (2011) look at intermarriage between immigrants and US natives, finding an increase in intermarriage of about 2% after the terror attacks for Hispanic immigrants. They also find that an immigrants employment opportunities are increased after intermarriage with a native, and suggest that this benefit sparked the increase, as Hispanics saw negative labor market effects after the attacks. However, if this is the motivation, it is surprising that they do not find a similar increase amongst Muslim immigrants, and it is unclear that the terror attacks were really the event sparking this increase.

2.2 "Scarring" Effects

A newer literature looks at the effects of luck, in terms of graduation timing, on lifetime earnings. Theory is ambiguous on the length of the effect we might expect to see, but the general finding is that there is a "scarring" effect for individuals graduating during economic downturns, and those individuals have a long-term earnings decline. For example, Oreopoulos et al (2012) finds wage depression lasting 10 years, and Kahn (2010) finds even longer scarring effects. There seems to be a measurable effect, but the exact cause of these depressed earnings is unknown. It also appears that the length of the effect varies with worker skill level (Oreopoulos 2012). I go through a number of the proposed theories[18] before giving a brief discussion of some specific work and methodology in the literature.

As mentioned before, the wage effect is ambiguous; I begin by discussing why the effect might only be short-term (effects limited primarily to the recessionary period). If the labor market behaves according to neoclassical models of supply and demand, we expect that wages will rise and fall with shifts in the demand for labor. Wages would therefore be lower in a recessionary period when demand for labor is lower, but wages would increase as the economy recovers and labor demand increases.

We might think this a simplistic view, but we can see a similar short-term wage effect in a search model with the assumption that there are diminishing marginal returns to experience. This means that workers who obtain a poor match during a recessionary period when search costs are high can move to another job during a growth period; the diminishing marginal returns to experience means that as they gain experience at the new firm, the gap between the worker and the counterfactual is reducing with time. Sufficient diminishing marginal returns to experience means that the wage effect remains short lived.

I now turn to theories for why the wage effects of graduating in a recession could be

[18] Kahn (2010) and Oreopoulos et al (2012) provide good discussions of possible reasons for the observed effect, and much of my discussion of the theory will come from their work.

persistent. If graduating in a recession causes a difference in the development of human capital, then this could account for wage scarring. We can get this difference in human capital development via a couple plausible mechanisms.

Becker (1967) discusses the value of early investment in human capital because early investment means individuals gain returns on that capital over a longer period. Workers graduating in a recession may have a harder time finding a job (longer period unemployed) or be developing human capital that is less useful, due to a poor match (general human capital in a separate industry or specific human capital in a worse firm). Later in their career, once a good match is developed, the employee may no longer be worth investing in due to lower future benefits that result from the employee being older.

Consider graduates entering the labor market during a recessionary period in which it is difficult for employees to find employment. The search length takes longer, as there are fewer job openings, thereby reducing wage offers and frequency of offers - and with that, reducing reservation wage as well as quality of the job match that is ultimately accepted. If we compare these workers to those who graduate in an expansionary period, we'd expect wages to be lower for those graduating in a recession even when we control for experience, as more of that experience will be from positions held at bad matches. If workers of different ability have different search times, and low skill workers have disproportionately higher search costs as a result, we would expect low skill workers to lose more experience relative to the counterfactual of entering in an expansionary period. This is in addition to the average reduction in experience that stems from longer average search periods.

Assumptions about employer learning can also lead to scarring effects. If employers are slow to learn a worker's quality and that worker had a bad match, then the employee will spend more time undervalued. A spectrum of effect lengths based on worker skill level can result from the assumption that employers learn about higher skill workers faster than lower skill workers.

Of similar effect to the employer learning, but a different mechanism, is the effect of contract renegotiation. If contracts are perfectly renegotiated (as assumed in the neoclassical model), there is no long-term loss due to graduating in a recession. However, if contract renegotiation is not perfect, then employees stagnate for longer periods of time at recession-level wages than neoclassical theory suggests. Without any option to renegotiate wage contracts, we could expect permanent wage scarring. It is plausible that jobs that do not require high levels of skill are less likely to incorporate frequent wage renegotiations, thereby causing the spectrum of effects observed by worker skill level.

Oreopoulos (2008) develops a search model in which high ability workers receive better and more frequent job offers than do low ability workers. In addition, job mobility reduces with age (alternatively, search costs increase with age). These two assumptions in a search framework model the observed effects of wage scarring that varies in severity by skill level. The increasing search costs as workers age means that some low skill workers who graduate in a recession and are poorly matched end their job search before finding a better one, resulting in permanent scarring for low skill workers and a spectrum of scarring effect periods for those of different skill levels. Reasons for an increase in search costs with age are intuitive, including lower mobility due to home ownership or family constraints such as a spouses job or childrens schooling. Thus the initial match becomes more important, as older workers' search costs prevent them from searching as intensively as younger ones.

I now look at some of the identification strategies that have been used in the literature. The methods I discuss have many similarities, and are similar to an event-study research design.

Kahn (2010) uses the NLSY79 to estimate recessionary effects on wages over the 1980s. She uses an augmented Mincer earnings equation that includes the unemployment rate at college graduation and observation. Her Mincer model controls for standard things like potential experience and its square. Kahn interacts potential experience with the unemployment rate at graduation in order to see how this unemployment changes over time. By using potential experience, we see how long the effects of graduation unemployment rates effect each cohort. She finds significant and sizeable negative wage effects for those graduating during an economic downturn for the entire period of her sample (17 years after graduation), providing strong evidence of mean-level scarring effects in the US over this period.

Oreopoulos et al (2012) uses a large matched data set between Canadian university students, longitudinal income tax data, and firm payroll records to examine recessionary effects on wages in a more granular way than before. They estimate a model similar to Kahn in that they modeled an earnings equation using the unemployment rate at year of graduation. Rather than the standard Mincer model, they used fixed effects for things like potential experience, observation year, year of graduation, and location. They then look at wages for each cohort and plot the development of these wages over time, in a modified event-study framework that allows them to look at lagged effects up to 10 years into the future. Their detailed university data also allows them to separate the students by skill level and examine heterogeneity of effect by worker skill. They find that the highest skilled workers suffer very little, and the lowest skilled workers face permanent scarring as a result of graduating during a recession; the mean scarring effect is 10 years. It is possible that the difference in mean effect between Oreopoulos et al (2012) and Kahn (2010) can be explained by differences in wage equality between the US and Canada.

Genda et al (2010) looks at the effects of recessions in separate countries, specifically the US and Japan. I will focus on their work in the US, as they use the CPS-ORG for their analysis, which is the same data I use in this paper. They estimate an earnings equation that could be seen as a hybrid between Kahn (2010) and Oreopoulos et al (2012), including unemployment rates at graduation and observation, standard Mincer controls like education and potential experience, and for some fixed effects like region, observation year, and a time trend. Their interest is in the coefficient on college graduation employment rate and whether there is a significant effect on earnings during the sample period, and how this changes over time. To do this, they interact with dummies for each additional three years of potential experience an individual obtains. They find a strong significant effect of unemployment at time of graduation on wages for both high school graduates and high school dropouts in the US, but the effects for both groups become statistically insignificant in the long run (10-12 years of experience) that matches previous studies of college students. Surprisingly, they find that high school dropouts recover more quickly than graduates, recovering after 3 years. This could be due to the already low wages unskilled workers may be receiving for their work.

In this paper, I look for recession-like effects on cohorts graduating soon after a shock to discrimination in order to examine potential scarring effects due to this discrimination. Some of the mechanisms discussed previously may be more relevant to this context than others, so I now relate the two literatures.

One possible mechanism by which recessions may affect wage is the availability of high-wage jobs. High wage jobs are tied to the business cycle,[19] and the availability of these jobs does not appear to be linked to a shock to discrimination. Thus if the lack of available high-wage jobs is the driver for scarring effects in a recession, the continued availability of these jobs when a small subset of the workforce is facing increased discrimination would imply that we should observe no scarring effects.

One key, mechanical difference between the recession and discrimination literatures is that I do not have a variable, such as unemployment levels, that provides variation over time. Instead, all individuals enter the labor force either before or after the shock has occurred. If I wanted to consider a spectrum of discriminatory attitudes, this might be the biggest argument in favor of a mechanism like in Kaushal et al (2007), where they use hate crimes as a measure of discrimination. More convincing than hate crimes might be survey responses dealing with racial or religious tolerance, as this allows a closer approximation to the marginal discriminator (as discussed in Charles and Guryan 2008). However, the shock to discrimination provided by the response to the September 11, 2001 terror attacks provides me with the necessary variation.

[19] See Oreopoulos (2012), page 5.

A key assumption in relating these literatures is that attitudes regarding race and religion revert to pre-shock levels slowly over time, similar in effect and duration to the recovery following a recession. If the shock is either permanent or quick, my approach will not be useful. A permanent shock may still be identified, but the mechanisms of scarring would be different - scars would develop simply because the target group faces more discrimination, not due to mechanisms like increasing search costs with age. It is also likely that the length of time firms have to adjust means that older workers would also be affected, more than simply new entrants to the labor force. If the recovery is too quick, there will be few new workers affected and the scarring effects we see will be minimal both because of the small group and because the time loss is short enough that new entrants can find better matches without facing a large additional cost.

3 Empirical Strategy and Methods

3.1 Yearly Effects

I begin by attempting to reproduce some of the effects seen in the discrimination literature around the September 11, 2001 terror attacks. I argue that wage stickiness means that only a small portion of Middle Eastern, Afghan, and Pakistani workers in the US will be observably affected by increased labor market discrimination. In order to provide evidence for my hypothesis, I need to show effects on labor market outcomes for Middle Eastern, Afghan, and Pakistani workers that are low relative to the group that theory says should be most affected.

I follow precedent and use a difference-in-difference framework, estimating the following model:

$$y_{it} = \beta_0 + \beta_1 sept_t + \beta_3 MEAP_i + \beta_4 MEAP_i * sept_t + \beta_5 X_{it} + month_t + state_{it} + \varepsilon_{it} \quad (1)$$

where y_{it} is the outcome of interest (the log of real hourly, weekly, and annual earnings). $sept_t$ indicates whether the observation took place before or after September 11, 2001. $MEAP_i$ indicates whether the individual is of Middle Eastern, Afghan, or Pakistani descent. X_{it} is a set of control variables which includes bins for age, education, and race, as well as indicators for whether an individual is married or foreign-born. Variables $month_t$ and $state_{it}$ control for month and state fixed effects respectively. I estimate this model once including all individuals of Middle Eastern, Afghan, and Pakistani descent working in the US, and another time restricting only to those born in the US.

The broader literature typically uses both immigrants and US-born workers in their target group, so it is important to look at how the inclusion of immigrants affects the estimated effects of discrimination. The inclusion of immigrants is useful for prior work because it increases sample power. However, because I am interested in labor market entry, I restrict most of my analysis to US-born workers in order to mitigate the chance that workers enter the labor force in another country where increased discriminatory effects resulting from the September 11, 2001 terror attacks are not present. The literature looking at cross-country effects of terror attacks points to insignificant or non-existent discrimination effects for attacks occurring across national borders (see Braakmann 2010 or Shannon 2012).

When I move to the analysis of cohorts, I follow the recession literature and utilize an event-study framework. As a result, most of my analysis will expand upon the short-period difference-in-difference framework[20] used in nearly all of the discrimination literature tied to terror attacks. I attempt to bridge this gap by also providing an event-study depiction of the lead-up to and aftermath of the terror attacks in terms of earnings differentials between Middle Eastern, Afghan, and Pakistnai workers. Again, I estimate this once including all

[20] Here, I refer to work like Kaushal et al (2007) or Rabby and Rogers (2011). Kaushal et al (2007) uses a period of three years before and after the attack in their difference-in-difference model. In this paper, I am still interested in the periods before and after the 2001 terror attacks, but I use a longer period of ten years before and after the attacks. I allow the effect to change for each year (cohort) in order to analyze the event effect, as well as the recovery. Analyzing the recovery is a key part of this model choice both here and in the recession literature. In this paper, I will often refer to this short-period difference-in-difference model simply as a difference-in-difference model.

individuals of Middle Eastern, Afghan, and Pakistnai descent working in the US and again restricting my sample only to those born in the US. I utilize the following model:

$$y_{it} = \beta_0 + \beta_1 MEAP_i + \sum_t year_{it} + \sum_t \delta_t(year_{it} * MEAP_i) + \beta_2 X_{it} + state_{it} + \varepsilon_{itc} \quad (2)$$

Again, y_{it} is the labor market outcome of interest. $MEAP_i$ is an indicator for whether the individual is of Middle Eastern, Afghan, or Pakistani descent. $year_{it}$ is a set of dummies for each year. δ_t is the estimate of interest, showing the loss experienced by the target group relative to the comparison each year. X_{it} is a set of controls: bins for age, education and race, as well as indicators for married or foreign-born. $state_{it}$ controls for state fixed effects. The model shows the loss experienced by individuals each year relative to American workers in my comparison group.

3.2 Cohort "Scarring" Effects

The primary thrust of my paper is to estimate possible scarring effects for Middle Eastern, Afghan, and Pakistani Americans entering the labor market after the September 11, 2001 terror attacks. I expect a sharp drop in cohort earnings for Middle Eastern, Afghan, and Pakistani Americans entering the labor force immediately after the attacks. My expectation is that the 2002 cohort would see the lowest income (largest scarring effect), and 2003 would be similarly low. Kaushal et al (2007) find limited evidence that labor market effects dissipated by 2004, so we might expect the 2004 cohort to be at or close to pre-attack levels, after which cohorts would earn similar wages. If we believe discrimination to be decreasing over time,[21] then we might see a trend in which earnings differentials between Middle Eastern, Afghan, and Pakistani Americans increase relative to majority workers.

The terror attacks provided a shock to discrimination among many Americans towards people of Middle Eastern, Afghan, and Pakistani descent. I would like to measure the effects

[21] This belief is supported by work like Charles and Guryan (2008), where they find that taste for discrimination is lower in younger cohorts.

of the discrimination shock to the counterfactual, and in setting up my research design I follow Jacobson et al (1993). I define the loss faced by Arab workers to be the difference between their labor market outcomes before and after the terror attacks. The theoretical model is thus:

$$\mathbb{E}\big[y_{itc}|D_{ic} = 1, X_{itc}\big] - \mathbb{E}\big[y_{itc}|D_{ic} = 0, X_{itc}\big] \tag{3}$$

where y_{itc} is the labor market outcome for Arab individual i observed at time t who entered the labor market with cohort c. D_{ic} indicates whether or not the September 11, 2001 terror attacks occurred yet, and equals 1 if it had and zero otherwise. X_{itc} is a set of control variables including education.

Clearly, equation 3 is not something I can estimate. As such, I use a second-best, which entails following a comparison group before and after the terror attacks in order to see whether Arabs were disproportionately affected relative to this comparison. This is even more useful in this context, as a discrimination paper, because part of my estimation process will be to compare the group facing discrimination to others. For discrimination to be present, we expect to see lower wages received by the target of that discrimination. The model I estimate compares Middle Eastern, Afghan, and Pakistani Americans to a comparison group of Americans as follows:

$$\begin{aligned} y_{itc} = \beta_0 + \beta_1 MEAP_i + \sum_c ELF_{ic} + \sum_c \delta_c (ELF_{ic} * MEAP_i) + \beta_3 X_{it} \\ + \beta_3 (X_{it} * MEAP_i) + yse_t + \theta_t + year_t + \beta_2 state + \varepsilon_{itc} \end{aligned} \tag{4}$$

where y_{itc} is the labor market outcome for individual i observed at time t who entered the labor market with cohort c. $MEAP_i$ indicates whether the individual is of Middle Eastern, Afghan, or Pakistani descent. ELF_{ic} is a set of dummy variables indicating the year of entry into the labor force. δ_c^k are the coefficients of interest and measures the loss experienced by a Middle Eastern, Afghan, or Pakistani American entering the labor force in a given year, relative to the comparison group in that year. In order to identify this for the whole period of interest (1991-2011), I also include the 1990 cohort and use this as the "left out" group. X_{it} are controls for age, education, marital status, and race. yse_t is a set of dummies for the number of years since entry into the labor force. θ_t controls for a time trend polynomial,[22] which is included in some specifications. *year* controls for observation year fixed effects and *state* controls for state fixed effects.

There are several advantages to using an event-study framework over a short-period difference-in-difference model[23] for this paper. A short-period difference-in-difference framework would allow me to determine whether there is a drop in earnings for cohorts after the terror attack, but it would not allow me to analyze the recovery. This is why the event-studies and modifications of the framework are common in the recession effects literature. By expanding the difference-in-difference over a longer period, and allowing the effect to vary by cohort, the event-study framework allows us to look at each cohort individually (rather than grouped pre and post) in order to see how it compares to the period before and after. Specifically, I divide workers into cohorts for each year 1991 through 2011 in order to develop a picture ten years before and ten years after the terror attacks. This gives me a window in which to examine trends and argue whether any changes in labor market outcomes are attributable to the event, as well as shed some light on the duration of estimated scarring effects.

In using this research design, there are two key assumptions I make. The first is that market prices adjust quickly to new information. In this case, the specific event is the September 11, 2001 terror attacks, which resulted in a shock to discrimination against people of Middle Eastern, Afgan, and Pakistani descent. The mechanism through which discrimination plays out may affect the speed at which wages adjust to the new state of the world. If this increases employer's taste for discrimination, we would expect immediate effects. If discrimination is

[22] I include model specifications that include a linear time trend, cubic time trend, and no time trend.
[23] I mention this short–period difference-in-difference framework because this is the most widely used model in the discrimination literature dealing with terror attacks.

instead statistical in nature, it may take more time for companies to measure and then react to changing opinions of Middle Eastern, Afghan, and Pakistani individuals by customers (for example).

I also assume that the comparison group is not affected by the event. It seems likely that my preferred comparison group is, for the most part, unaffected by increased discrimination due to the terror attacks. I will discuss the choices I make in constructing comparison groups in the later part of Section 3.3, and I conduct some robustness checks of my target and comparison groups to check for spillover effects in Section 5.1.

3.3 Data

I use the Current Population Survey Outgoing Rotation Groups (CPS-ORG) as my primary data source. This gives me large sample sizes, which are important because I am looking at a subset of the US population which is about 0.5% of the total US population.[24]

The CPS-ORG provide important demographic information; however, it is limited in the detail it gives regarding race and religion - key pieces of information considering the questions in this paper. The racial qualifiers do not include anything that identifies an individual as Middle Eastern, Afghan, or Pakistani, and the Census Bureau is forbidden by law to ask individuals about what their religion is, eliminating the possibility of using Muslims as a proxy. What the CPS-ORG do provide is information on birthplace and parental birthplace, and so I use these to construct my target group identifier in a manner similar to Kaushal et al (2007) and Rabby and Rodgers (2011).

To create this identifier, I define the Middle East[25] according to the regions used in the CPS, and I also include countries listed in the North Africa region. In constructing the target group, I exclude Israel and include both Afghanistan and Pakistan. Thus the countries in my target group are as follows: Afghanistan, Algeria, Cyprus, Egypt/United Arab Republic,

[24] 2006-2010 American Community Survey Briefs conducted by the US Census Bureau

[25] The definition of what countries are included in the Middle East has a contentious history; see Davison (1960) for a brief history of the term and some of its early evolutions.

Iran, Iraq, Jordan, Kuwait, Lebanon, Libya, Middle East (not specified), Morocco, Northern Africa, Pakistan, Palestine, Saudi Arabia, Sudan, Syria, Turkey, United Arab Emirates, and Yemen. Other countries I would include in this region, but do not appear in CPS data, are Bahrain, Oman, Qatar, Tajikistan, and Tunisia. My identifier, which I label as people from the Middle East, Afghanistan, and Pakistan, is coded such that anyone with a parent born in one of the specified countries is considered to be of Middle Eastern, Afghan, or Pakistani descent.

It is important to point out that, unlike most prior work in the discrimination literature dealing with terror attacks, I do not include immigrants themselves in my analysis. I do this for several reasons, which I discuss below.

My model deals with discrimination related to time of entry into the labor force. The discrimination effects are tied to being in the US at time of entry, if not existentially then at least in magnitude. Including immigrants in my model introduces the possibility that the individual entered the labor market in another country where he did not face discriminatory effects tied to the September 11, 2001 terror attacks, and thus does not face the same scarring effects.

The inclusion of immigrants also raises concerns regarding differences in wage effects due to differences in education systems; restricting to US born individuals nearly ensures that they attend US schools and reduces variance in schooling opportunities and efficacy. It also solves or at least mitigates language issues that might take precedent over education level when an immigrant searches for work.

There is also a concern that immigrants could respond to discrimination by changing their decision whether to move to the US. If not the magnitude, then the make-up of who decides to come to the US may change, and some who are present may opt to leave. By restricting to individuals born in the US, I have a lag of at least 16 years between citizen birth and entry into the labor force for a high school dropout. While parents may decide to leave in response to discrimination, I argue that having older children who have grown up in the US and not in the parents' native country means parents would be much less likely to leave. Certainly migration within the US is a possible response to increased discrimination, but Kaushal et al (2007) find evidence of reduced migration for Arab and Muslim men after the terror attacks.[26]

The comparison group I use is individuals born in the US with parents not born in India (or the target group). I exclude individuals of Indian descent because there is evidence of increased discrimination towards them after the terror attacks.[27]

Notice that there are many people of Middle Eastern, Afghan, or Pakistani descent who are not detected by this method. Thus there are some members of the comparison group who faced discrimination similar to those in my target group. Similarly, only one parent must be born in one of the aforementioned countries, so some of the individuals in my target group may not have faced discrimination following the terror attacks. Both of these concerns bias my results towards zero.

I also limit my sample to men age 18-55. Limiting to men does several things for me. First, it allows greater comparability with other papers in the literature, which look at men because they are more likely to be working than women, particularly given the target group. Second, this paper deals with discrimination, and I do not wish to confound my results measuring racial and ethnic discrimination effects with a possible gender discrimination story.

Other important variables included in the CPS-ORG data include: wage, employment, hours worked, gender, age, race, education, marital status, citizenship status, immigration year, and state of residence. All wage information is deflated to 2002 dollars[28] using Bureau of Labor Statistics CPI tables.

For the labor force entry dates and human capital accumulation dates I make several

[26] Reduced mobility in turn may be a wage reducing effect, as willingness to move increases job opportunity and thus potential wage. I do not have a way of controlling for this, but if mobility were reduced after the attacks, then my results would be biased upwards.

[27] For example, among the first hate crime murders following the terror attacks was that of Balbir Singh Sodhi, an Indian. See the New York Times, September 17, 2001.

[28] 2002 dollars are chosen to be consistent with Kaushal et al (2007) to enable easier comparisons.

assumptions that it is important to discuss. This constructed variable is similar to "potential experience" as discussed by Card (1999). I assume that the individual begins her education at age 6 and does not repeat or skip any grades, nor does she take a break from schooling until it is complete. These assumptions may introduce noise into these constructed variables; the direction of the bias depends upon whether more individuals who entered the labor force before the terror attacks are measured as doing so after or vice-versa. My argument is that individuals who are held back or skip grades are insignificant compared to the number of individuals that take a break from school either between high school and college or college and graduate school. This means I likely have more individuals in the before groups than the after groups: individuals who are denoted as completing their education prior to the attacks but do not, and individuals marked as completing their education just after the attacks but actually complete it once the effects have diminished. My guess is that people do not generally take such long breaks to move from the first group to the last, so that the number of people doing this would be small; this would bias my results toward zero.

4 Results

4.1 Yearly Effects

The main results in this paper are created in contrast to those developed in much of the other work done in this literature. As such, I begin by presenting estimates akin to these estimates and then move on to cohort effects.

Table 2 provides difference-in-difference estimates over the time period September 1998 through September 2004 excluding September 2001.[29] I estimate Table 2 using Model 1. The first column shows estimates constructed similarly to those in much of the literature, including both US-born and foreign-born individuals in the target group. Notice that the

[29] This period is chosen to match that in Kaushal et al (2007), which is a similar window to that used in other work in the literature as well, such as Dávila and Mora (2005).

effect is modest and statistically insignificant.[30] The second column shows estimates constructed with only US-born individuals. These results suggest that foreign-born individuals seem to be driving the sign of the aggregate effect in the first column. I argued previously in section 3.3 that looking at US-born individuals will lead to lower bias. Admittedly, this table also suggests that US-born individuals are less sensitive to discrimination than foreign-born individuals, thus my results should be taken as a lower bound for discriminatory effects on foreign-born workers of Middle Eastern, Afghan, and Pakistani descent in the US. Overall, the effect at the aggregate is modest and insignificant for both samples when we consider the aggregate population.

We now begin to modify the short-period difference-in-difference model and consider what happens each year over the period 1994-2011. Here, I am looking at yearly effects, not cohort effects, in order to see if any individual year might be driving the results, and whether the choice of window is important in the use of a difference-in-difference framework. For this portion of my results, I am restricted to starting at 1994 because this is when the Census Bureau began collecting information used to define the target and comparison groups. Figure 1 depicts the estimated loss each year over the period 1994-2011 for Middle Eastern, Afghan, and Pakistani workers in the US compared to US-born workers not of Indian descent in terms of log(real annual wages). To create this figure, 2012 is included in the sample, and is used as the "left out" year.

Notice the spike in 1999 which heavily influences the estimates over the 1998-2004 period previously shown. Aside from this spike, there does not seem to be much difference in annual earnings when looking at the period year by year. If we restrict to only US-born workers, there is even an observed spike after the attacks in 2002.

Similar results are shown in Figures 2 and 3, which depict the loss in terms of log(real weekly earnings) and log(real hourly wage) respectively. I am unable to detect any

[30] This contrasts with the results of Kaushal et al (2007). I attribute this to their inclusion of hate crimes per capita for each state as a control variable, to which the estimates are sensitive. I argue that this is inappropriate and that hate crimes are, like wage differentials, outcomes of discrimination rather than controls.

drop in wages causally linked to the September 11, 2001 terror attacks when looking at the whole set of workers.

4.2 Cohort "Scarring" Results

I now move on to my main results and look at labor market effects on each cohort over the period 1991-2011. Those entering the labor market are the individuals for which we most expect to see labor market effects from discrimination shocks. Figure 4 depicts the relative wages received by individuals of each cohort between 1991 and 2011.[31] To construct Figure 4, I estimate Model 4 by including cohorts from 1990 through 2011 and allowing 1990 to be the "left out" cohort. In this initial specification I do not include a time trend, interaction terms, or years since entering the labor force; additional specifications will be discussed later in the results. The earnings measures for the target group of Middle Eastern, Afghan, and Pakistani Americans is quite noisy, but there does appear to be a drop after the terror attacks. There remains a question of what causes the apparent spike prior to the attacks, and there is a concern of other similar drops such as that between 1994 and 1996.

Figure 4 shows estimates of the earnings effect on real annual income due to being in a given cohort and being a member of the target group or not. As mentioned, the results are noisy. However, it is striking to see a drop of nearly $\$7,000$ in real annual income for Americans of Middle Eastern, Afghan, or Pakistani descent relative to the comparison[32] between 2001 and 2002, which does not appear to recover to pre-shock levels. As reference, we can see in Table 1 that this would be a drop in about 17% of annual income. Keep in mind that this is a persistent loss for the 2002 cohort relative to the 2001 cohort - these are long term effects, for which the outcomes are measured over the following ten years in the sample period.

Figure 5 simply reframes Figure 4 into the loss in real annual income experienced by

[31] The estimations presented in this paper include the use of CPS earnings weights. Their exclusion results in a trend similar to what is presented here, although effect magnitudes are diminished in some cases.

[32] The comparison is American workers not in the target group and not of Indian descent.

Middle Eastern, Afghan, and Pakistani Americans relative to other US-born workers of non-Indian descent. This loss is how I will frame future figures in this paper. Again, I include the years 1990 - 2011 in my sample and 1990 is the "left out" year allowing me to plot effects from 1991 through 2011. Figure 4 shows that the comparison group is relatively smooth, and the noise is generated primarily from the target group. This trend continues in the other models, and so little information about the comparison group is missing when we focus on the loss, while making the difference (the discrimination effect) easier to see.

I consider seven model specifications and test the sensitivity of the model to them. These are shown in Figure 6. Model 1 is my baseline, which I used previously and will continue to use in my other analysis. It estimates Equation 4, but does not include the interaction of the control variables, a time trend control, or a control for years since entering the labor market. Model 2 includes interactions of the control variables, and we can see that while these estimates do not share the same magnitude as the other models, they do share a similar shape and display a similar drop after the attacks (as well as a similar increase prior). Model 3 adds a linear time trend to Model 1. Model 4 changes this linear time trend to a cubic time trend. Model 5 instead uses month fixed effects for each month in the sample. Model 6 replaces the fixed effects of model 5 with state by month fixed effects. Model 7 is identical to Model 1, with the exception that it adds a control for years since entering the labor market. Notice that the model which is most noticeably different is model 2, which interacts the control variables. I argue that differences in the effect of controls, such as education, faced by the target group are an aspect of the discrimination I wish to measure and thus opt not to include them in my preferred model. Otherwise, Model 7 is the most different, but is nonetheless similar to the others. Given the lack of apparent difference in the other specifications, I opt for Model 1 due to its simplicity. Notice also that the magnitude of the drop, around $\$7,000$ in real annual income, is consistent across all model specifications.

To this point I have focused on real annual income, and it would be natural to expect other outcomes to share a similar effect if discrimination is present. Figures 7, 8, and 9 depict alternative labor market outcomes: real hourly wage, real weekly earnings, and probability of employment[33] respectively. We see a consistent drop in labor market outcomes for the 2002 cohort, with slight improvements over 2002 seen by the 2003 and 2004 cohorts. Specifically, we see a drop of \$1.75 in the hourly wage from 2001 to 2002; the drop is quite large, and almost unbelievable in size. However, it appears to recover slightly in 2003 and somewhat further in 2004, which would support the expected pattern. As we move to cohorts more recent than that, however, we see less of this expected pattern and more noise, making it harder to rely on any single estimate alone. Real weekly earnings drop by a little over \$100 per week, which is a little smaller than expected if the \$7,000 drop in real annual earnings is to be believed. We also see a drop in the probability of being employed of about 25% from 2001 to 2002. This continues in 2003 and returns to pre-recession levels in 2004 as expected. Again, we see another smaller decline afterwards that calls into doubt the precision of our estimates. Notice that for each outcome, individuals do not, except in the case of employment probability, make it back up to 2000 and 2001 levels before 2004 at the earliest. As before, one difficulty is in explaining the increase prior to the drop, but I will point out that this increase is asynchronous among the various labor market measures, but synchronous in the drop after 2001.

Figures 7 and 9 depict results close to the predicted outcome. Wages and probability employed are relatively flat, relative to the comparison, in the years leading up to 2001. They drop sharply in 2002, recover slightly in 2003, and return close to pre-event levels in 2004. However, additional noise before and after may cast doubts on the validity of these results and this interpretation.

[33] The probability of whether an individual is employed is estimated with a probit model.

5 Robustness Checks

5.1 Validity of the Target Group

There are a few concerns we might have with the current analysis. The first is that after the terror attacks, broad American attitudes towards foreigners of any type worsened, and as a result any effects I observe are not specifically targeted at Middle Eastern, Afghan, and Pakistani Americans, but at all foreigners and their descendants. I test this by constructing the loss felt by three groups, relative to the comparison group of all other US-born workers not in the target group and not of Indian descent.

The first group is all second generation immigrants. This is the broadest group, and includes all people whose parents immigrated to the US. If there is broad discrimination against all foreigners and their descendants rather than a targeted one against Middle Eastern, Afghan, and Pakistani Americans, we might expect to see an effect here. Figure 10 shows that there is no such effect.

I narrow this group to only second generation immigrants whose parents are not from English-speaking countries. I define “English speaking countries” using tier 4 student visa requirements for the UK.[34] This is the list of countries from which students applying for a visa to study in the UK do not need to provide proof of English language knowledge, as it is assumed that the applicant is a native speaker. Countries on this list are: Antigua and Barbuda, Australia, the Bahamas, Barbados, Belize, Canada, Dominica, Grenada, Guyana, Ireland, Jamaica, New Zealand, St Kitts and Nevis, St Lucia, St Vincent and the Grenadines, Trinidad and Tobago, UK, and the USA. Figure 11 shows that there may be a small effect after the terror attacks.

The third group I look at narrows to further exclude immigrants whose parents are from European countries as well as not native English speakers. Figure 12 shows that there may be a spillover effect to this group. This could point to a general xenophobia resulting from the

[34] https://www.gov.uk/tier-4-general-visa/knowledge-of-english

terror attacks or simply be the result of some other synchronous event affecting immigrants in America. It is important to note, however, that while there appears to be a drop in wages for the cohorts after the attacks (2001 and 2002), the wages remain above those of the comparison group.

Given that this third group seemed most likely to be affected by the terror attack, it is important to examine whether Middle Eastern, Afghan, and Pakistani Americans experienced a drop after the terror attacks relative to this group. If there is no observable effect here, then the effect we observe is likely not restricted to individuals of Middle Eastern, Afghan, or Pakistani descent. However, Figure 13 shows a similar pattern to that seen when we compare to the broader American workforce in Figure 4, in which we see an increase in wages for cohorts before the terror attacks and a sharp drop after.

The results I present in this paper are quite noisy, likely due to the sample size for those of Middle Eastern, Afghan, and Pakistani descent in each cohort. However, it is possible that some of this noise is due to variance in occupation type or education level. I examine these two possibilities next.

5.2 Race and Ethnicity as Part of the Production Function

If we have large heterogeneity in occupation type, we may be able to observe greater clarity by restricting to an occupation most likely to face discrimination. I expect that occupations which are more customer-facing would be more discriminating, as customers may have a preference for the type of person they deal with and thus race and ethnicity become a part of the production function. As such, I examine the subset of workers in occupations classified by the CPS as service and sales. Given the terror attacks likely did not have an effect on employer's perception of worker productivity in most industries, we would expect any result here to be evidence for statistical discrimination.

Figure 14 depicts the results. Surprisingly, the drop in wages disappears when we consider only this sector. This suggests that the discrimination shock resulting from the September 11, 2001 terror attacks likely did not affect statistical discrimination.

It is possible that wage differential we observe are due to occupation choice, and that there is low variation within occupation. Thus discrimination could shift people from higher wage sectors to lower wage sectors. Kaushal et al (2007) find evidence of movement from high-wage to low-wage professions by a similar target group after the terror attacks.

5.3 A Change in Observables

Given the small sample size of the target group, heterogeneity in educational attainment could also explain both the noise and the effect we observe. Figure 15 depicts the average years of education obtained by each cohort of Americans of Middle Eastern, Afghan, and Pakistani descent and compares these to the educational attainment of other American workers not of similar or Indian descent; it then relates this to cohort wage effects of the two groups. There is a definite correlation obvious in some years, such as the spike in 1998, but there appears to be less correlation immediately after 2001. In this time frame of interest, we see increasing educational attainment even as wages are dropping. It does not appear that education heterogeneity in our sample can explain the drop in wages experienced by the 2002 and 2003 cohorts. It is important to note that the main outlier in the pre-event trends, 1996 wages for the target group, is correlated with the lowest level of educational attainment we observe for Middle Eastern, Afghan, and Pakistani Americans during this period.

6 Conclusion

The results I present in this paper are quite noisy. Another dataset with increased power would provide additional precision and clarity to my results. In future work, I could follow Dávila and Mora (2005) and see whether I can get more precision using the ACS. Given the longitudinal nature of the survey, it may become simpler to map scarring effects for each cohort through time, similar to Oreopoulos et al (2012).

The most troubling part of my results for my story may be the increase we observe in each of the outcome variables leading up to the drop after 2001. What may be compelling is the asynchronous increases of these for different variables,[35] followed by a drop between 2001 and 2002 for all examined outcome variables: real annual income, real weekly income, real hourly wage, and probability of employment. We also do not see the expected recovery period where each successive cohort moves back towards pre-attack earnings levels. Later years may be affected by the financial crisis if the crisis affects Middle Eastern, Afghan, and Pakistani Americans differently than other Americans. However, I would expect to see a recovery earlier than the financial crisis, so I do not think this explains what we observe. I will now consider several possible explanations.

One thing to notice is the lack of apparent recovery after the attacks. It is possible that attitudes towards Middle Eastern, Afghan, and Pakistani workers did not improve after the attacks like others have suggested, but that this was a permanent or long-term shift in perspectives. I have been unable to find consistently measured opinion data for multiple periods both before and after the attacks that might shed light on whether or not this is the case, and so it remains a possibility.

It is also possible that future events continued to dampen the recovery. About when we might expect wage recovery to occur, the 2005 London attacks occurred, which may have served as a reminder of the September 11, 2001 terror attacks and reinforced prejudice against Middle Eastern, Afghan, and Pakistani Americans. I find this to be unlikely, due to the evidence showing no cross-border effects for terror attacks (and none, to my knowledge, showing the existence of significant cross-border effects).

[35] The lowest loss in terms of real annual income felt by Middle Eastern, Afghan, and Pakistani Americans was felt after the climb from 1999 to 2001 (see Figure 5); we see a similar pattern in real weekly earnings in Figure 8. For real hourly wage, in Figure 7, we see wages that are relatively flat from 1999 through 2001. In terms of the probability employed, we see a sharp increase in the probability between 1997 and 1998 (nearly no loss felt by the target group), which slowly tapers off until 2001. All of these outcomes share a sharp point estimate drop between 2001 and 2002, with varying recoveries. Afterwards, the recoveries seem somewhat random and do not follow the pattern we expect from theory.

There are a few reasons that the effects from the recession literature may not translate into this discrimination context, and they depend upon which mechanisms are driving recessionary scarring (which are unknown).

Oreopoulos et al (2012) find that scarring effects are greatly reduced for high skill workers. Average educational attainment for Americans of Middle Eastern, Afghan, and Pakistani descent is much higher than the general population; in 2001, Middle Eastern, Afghan, and Pakistani Americans received over a half year education more than their counterparts in the comparison group. This difference grows above a full year of education difference at several points within the sample (see Figure 15). Thus to the extent that education and skill are correlated, we may expect Middle Eastern, Afghan, and Pakistani Americans to recover more quickly from recessions or recession-like events, or even be insulated from their effects altogether. The lack of precision in these results allows the scarring effects to be interpreted as small or nonexistent, and the higher than average education of Middle Eastern, Afghan, and Pakistani Americans could help explain a lack of effect.

One possible mechanism by which recessions may affect wage in a manner that is not applicable to the discrimination setting is through the availability of high-wage jobs. High wage jobs are tied to the business cycle,[36] and the availability of these jobs does not appear to be linked to a shock to discrimination. Thus if the availability of high-wage jobs is the driver for scarring effects in a recession, the continued availability of these jobs when a small subset of the workforce is facing increased discrimination would imply that we should observe no scarring effects; this could be a reason for my lack of significant estimated effects.

It is possible that Middle Eastern, Afghan, and Pakistani Americans are a bad group to use to measure scarring effects from discrimination, simply because they are a relatively small fraction of the population. Becker's model predicts increased discrimination with the size of the group targeted by discrimination. Middle Eastern, Afghan, and Pakistani Americans are a very small group, representing only about 0.5% of the total US population.[37] Thus if

[36] See Oreopoulos (2012), page 5.
[37] 2006-2010 American Community Survey Briefs conducted by the US Census Bureau.

Becker's model is an accurate representation of discrimination, we may expect there to be enough employers in the market who do not discriminate against this group that wages would not fall. Instead, we would have economic segregation, but no discrimination. If Black's model is more accurate, however, we should see both. The existence of economic segregation is testable, but we would need more detailed information on where Middle Eastern, Afghan, and Pakistani workers are employed. However, given the significant effects measured by other work in this literature, I doubt this is the case.

Given the stickiness of wages, we cannot expect to see the sharp decline in wages predicted by Becker (1957) or Black (1995) in response to a shock to discrimination. I have provided weak evidence that effects may be more strongly felt by individuals newly entering the labor force and that there may be scarring effects. More work is needed using more or better data in order to find precise estimates of these effects and draw a more robust conclusion.

7 Tables and Graphs

Table 1: Summary Statistics

	Target Group	Comparison Group
Education:		
Less than High School	9.81	12.20
High School	14.30	26.90
Some College	32.93	31.20
4+ Years College	42.95	29.70
Age	26.39	27.76
Percent Married	24.69	36.97
Labor Market Outcomes:		
Prob. Employed	88.89	91.42
Real Annaul Income	\$39,248	\$37,259
Real Weekly Earnings	\$652	\$623
Real Hourly Wage	\$16.44	\$15.95
Race:		
Asian	77.09	80.33
Black	2.19	7.91
Hispanic	2.87	8.10
White	17.12	2.01
Other	0.73	1.64
No. of Observations:		
Employment Obs.	9,607	3,035,591
Wage Obs.	1,916	632,612

Notes: The target group consists of American workers of Middle Eastern, Afghan, and Pakistani descent. The comparison group consists of American workers not in the target group and not of Indian descent. I generated the summary statistics using the wage observations, as these outcomes are used more frequently in the paper; using the larger number of employment observations causes slight differences in the target group.

Table 2: Difference-in-Difference results with and without foreign-born workers

Variable	Foreign-born and US-born				Only US-born workers			
	Coef.	Std. Err.	t	P>t	Coef.	Std. Err.	t	P>t
log(real weekly earnings)	-0.010	0.026	-0.37	0.71	0.0928	0.091	1.02	0.31
log(real annual income)	-0.010	0.027	-0.36	0.72	0.0860	0.092	0.94	0.35
log(real hourly wage)	0.0071	0.030	0.24	0.82	0.1066	0.081	1.31	0.20

This table depicts difference-in-difference results over the time period September 1998 through September 2004 excluding September 2001, a period similar others used in the litereature (ex: Kaushal et al 2007). The first column shows estimates constructed akin to those in much of the literature, including both US-born and foreign-born individuals in the target group. The second column shows estimates constructed with only US-born individuals.

Table 3: Number of Target Group Observations by Cohort

Cohort	Wage Obs.	Employment Obs.
1991	60	346
1992	78	361
1993	71	386
1994	112	499
1995	89	494
1996	98	445
1997	85	441
1998	73	355
1999	95	439
2000	107	488
2001	96	502
2002	74	435
2003	79	452
2004	105	496
2005	87	414
2006	71	336
2007	80	428
2008	80	383
2009	66	322
2010	51	251
2011	66	310

One of the main concerns regarding my results are the noisy estimates. This table shows the number of observations of Middle Eastern, Afghan, and Pakistani Americans by cohort.

Figure 1: Event Study with entire population observed from 1994-2011: log(real annual wage)

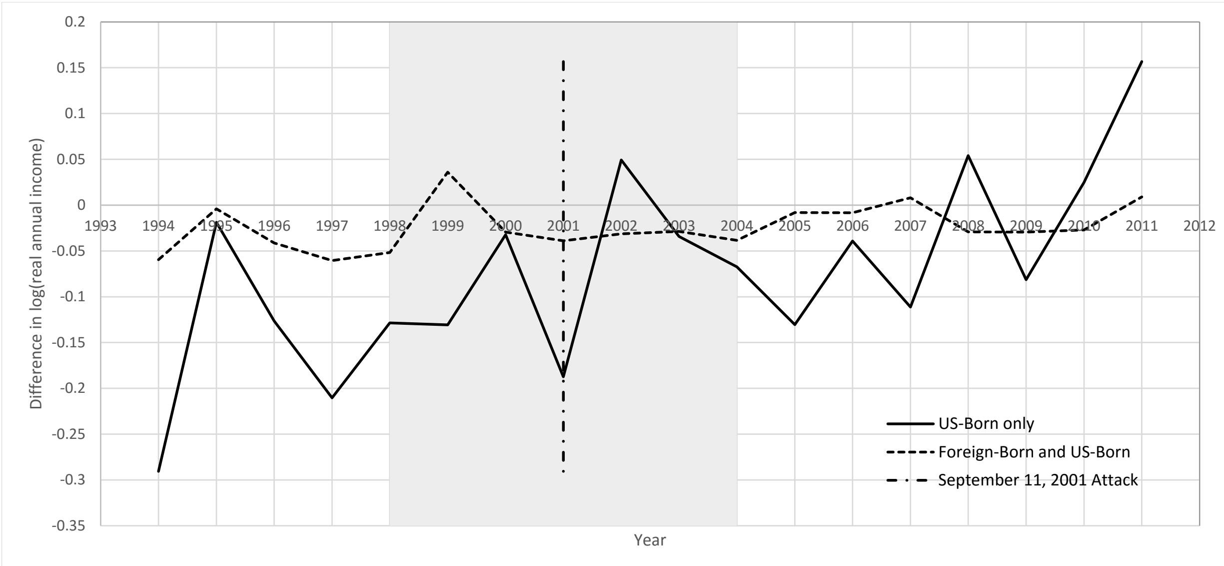
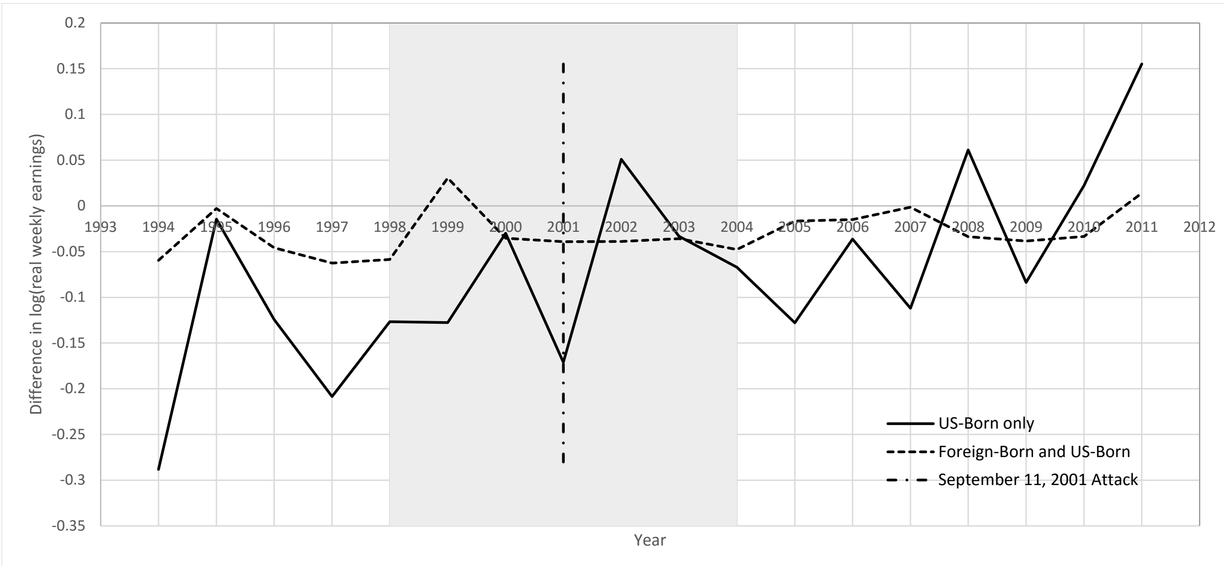


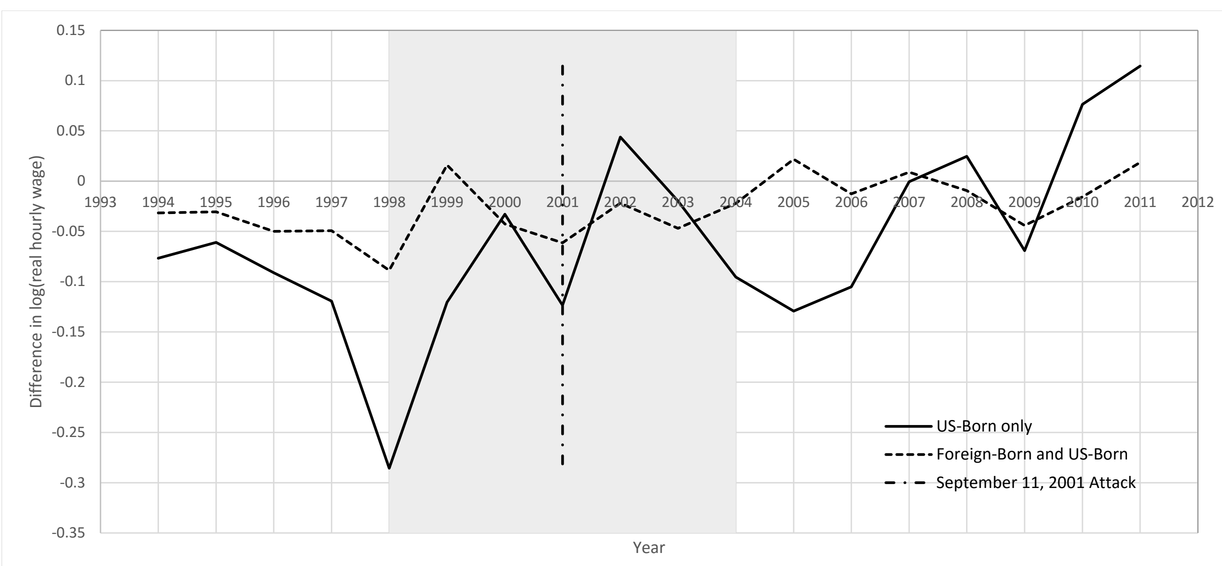
Figure 1 depicts the estimated loss each year over the period 1994-2011 for Middle Eastern, Afghan, and Pakistani workers in the US compared to US-born workers not of Indian descent in terms of log(real annual wages). The period starts at 1994 because this is when the Census Bureau began collecting information used to define the target and comparison groups. See estimates in Table A4 for coefficient estimates.

Figure 2: Event Study with entire population observed from 1994-2011: log(real weekly earnings)



Similar to Figure 1, Figure 2 depicts the the estimated loss each year over the period 1994-2011 for Middle Eastern, Afghan, and Pakistani workers in the US compared to US-born workers not of Indian descent in terms of log(real weekly earnings). Estimates in Table A5.

Figure 3: Event Study with entire population observed from 1994-2011: log(real hourly wage)



Similar to Figures 1 and 2, Figure 3 depicts the estimated loss each year for the period 1994-2011 for Middle Eastern, Afghan, and Pakistani workers in the US compared to US-born workers not of Indian descent in terms of log(real hourly wage). Estimates in Table A6.

Figure 4: Event Study by Cohort: Real Annual Income

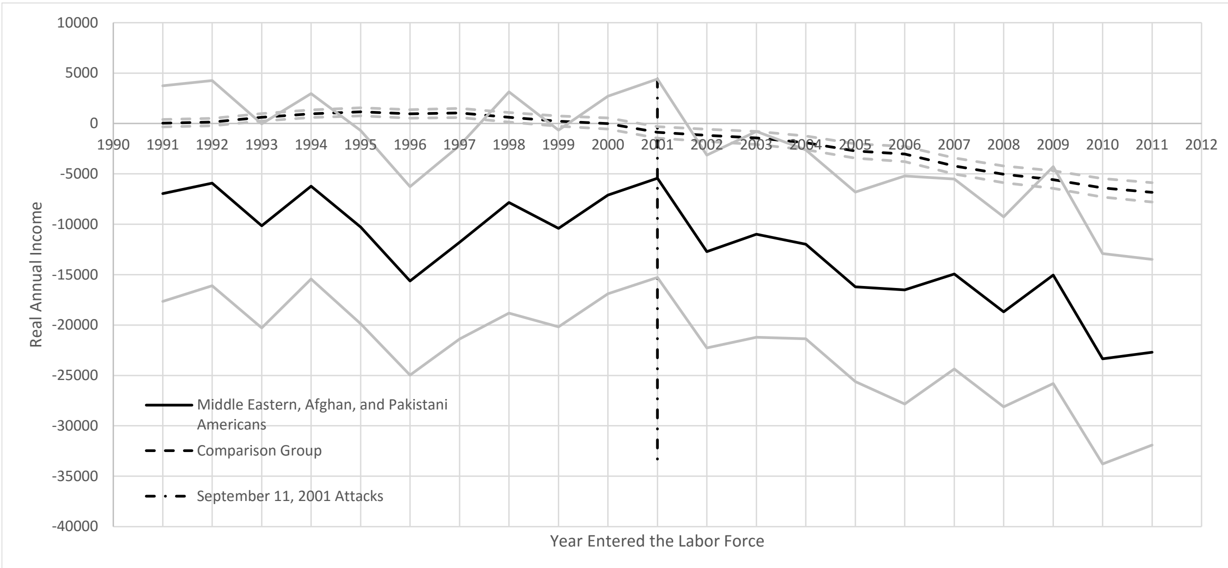


Figure 4 depicts the relative wages received by individuals of each cohort from 1991 to 2011. 90% confidence intervals are shown in grey, using robust standard errors. Estimates are calculated using Model 4. Included in the sample are cohorts from 1990 through 2011, but 1990 is the "left out" cohort. See Tables A1 and A2 in the appendix for point estimates and additional standard errors.

Figure 5: Graph of loss by cohort: Real Annual Income

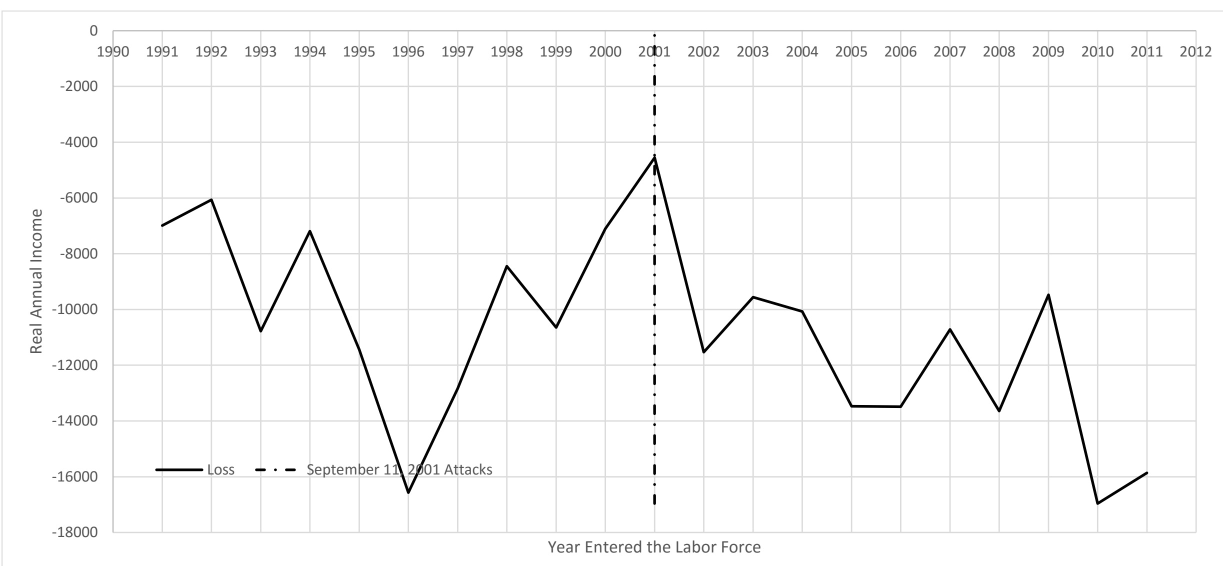


Figure 5 reframes Figure 4 into the loss in real annual income experienced by Middle Eastern, Afghan, and Pakistani Americans relative to other US-born workers of non-Indian descent. See Table A3 for point estimates and additional standard errors.

Figure 6: Graph of loss by cohort: Model Specifications

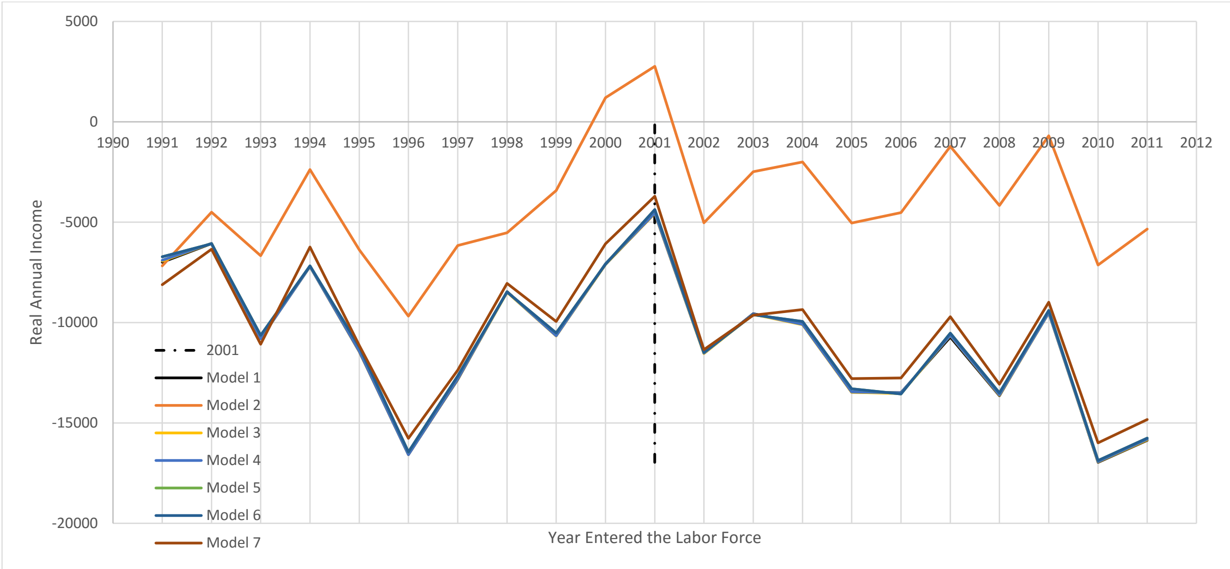
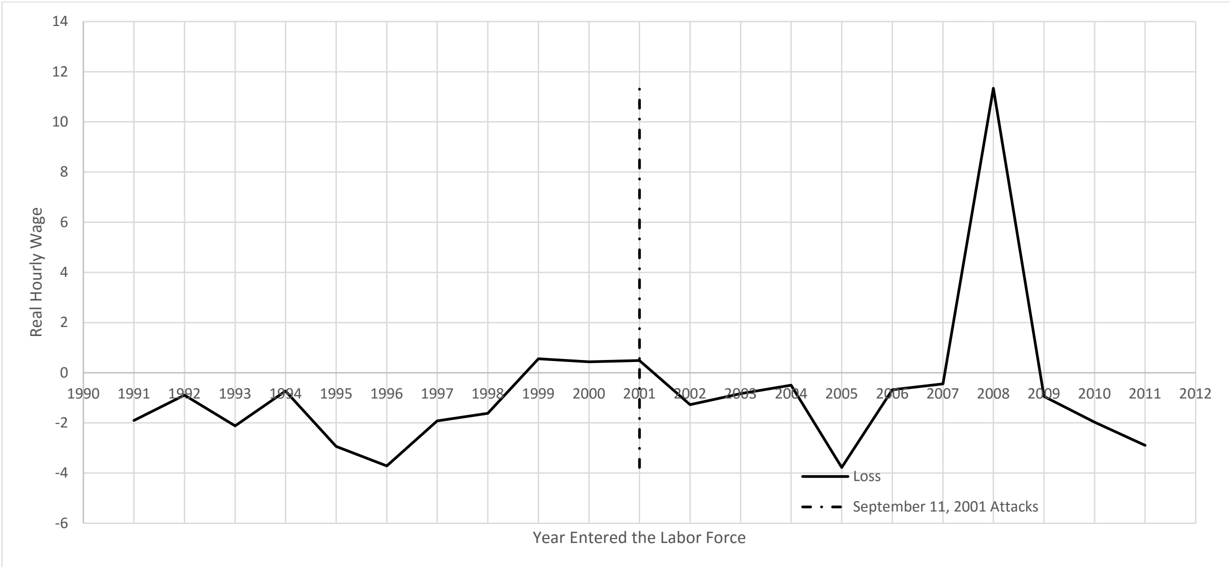


Figure 6 shows seven model specifications. Model 2 is the most different, and includes includes additional interactions of controls. Model 7 is also worth mentioning, and includes controls for years since entry into the labor force. See section 4.2 for more details on the different models depicted here, and Tables A7 and A8 for point estimates of each model.

Figure 7: Graph of loss by cohort: Real Hourly Wage

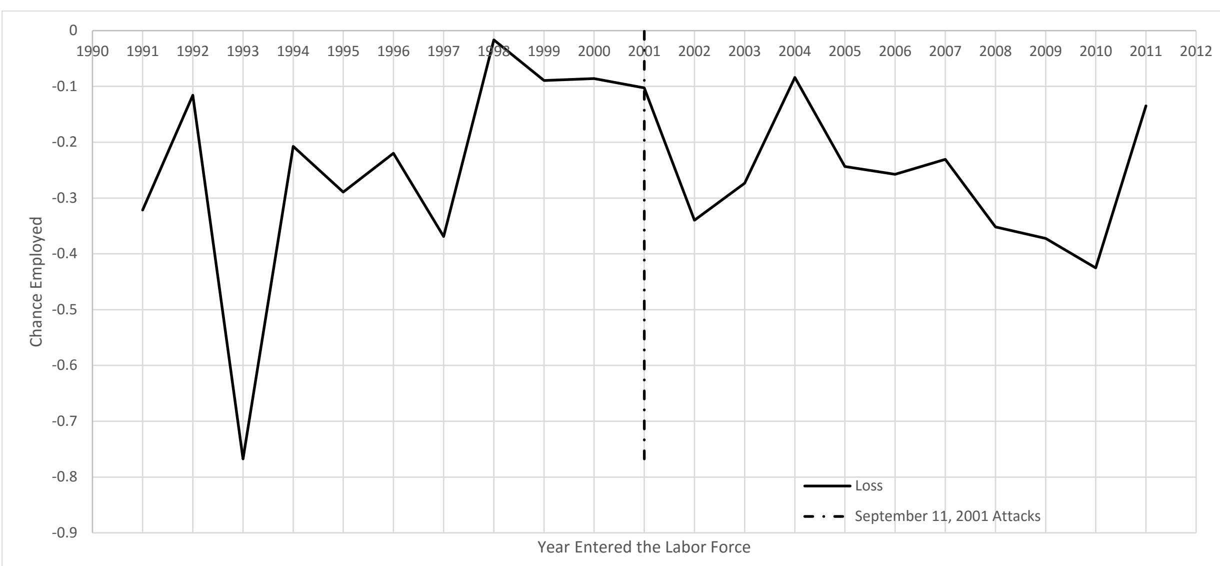


To this point I have focused on real annual income. Figures 7, 8, and 9 depict alternative labor market outcomes. Here in Figure 7, I show the loss in terms of real hourly wage. See Table A9 for estimates of Figures 7, 8, and 9.

Figure 8: Graph of loss by cohort: Real Weekly Earnings

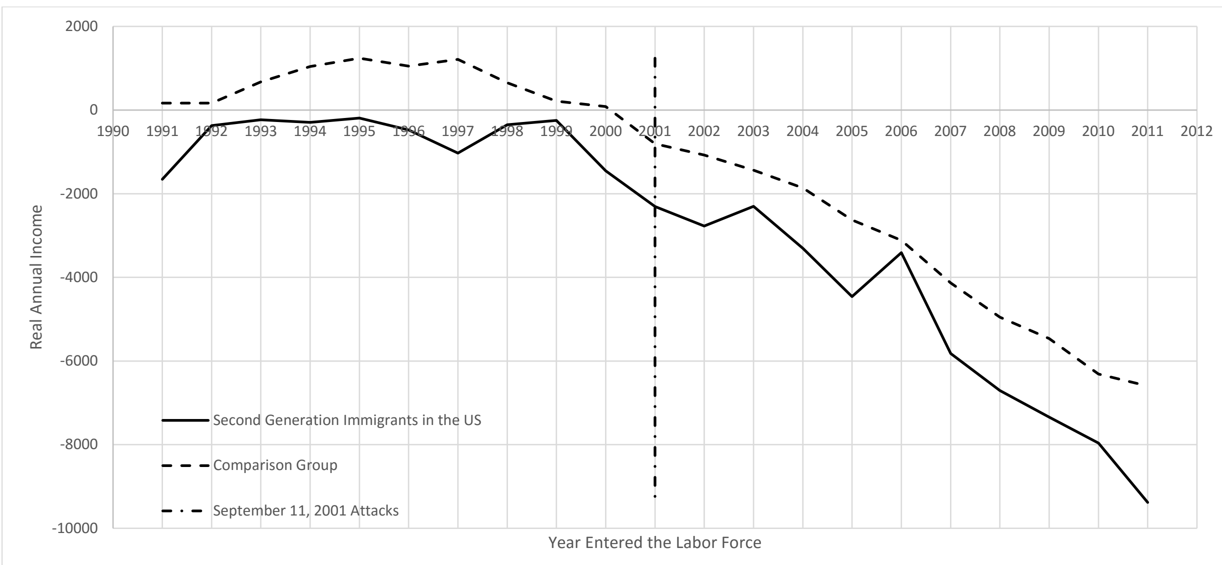
Figures 7, 8, and 9 depict alternative labor market outcomes. Here in Figure 8, I show the loss in terms of real weekly earnings. See Table A9 for estimates of Figures 7, 8, and 9.

Figure 9: Graph of loss by cohort: Probability Employed



Figures 7, 8, and 9 depict alternative labor market outcomes. Here in Figure 9, I show the loss in terms of probability of employment. This model is estimated with a probit model. See Table A9 for estimates of Figures 7, 8, and 9.

Figure 10: Real Annual Income by Cohort: A Placebo Group

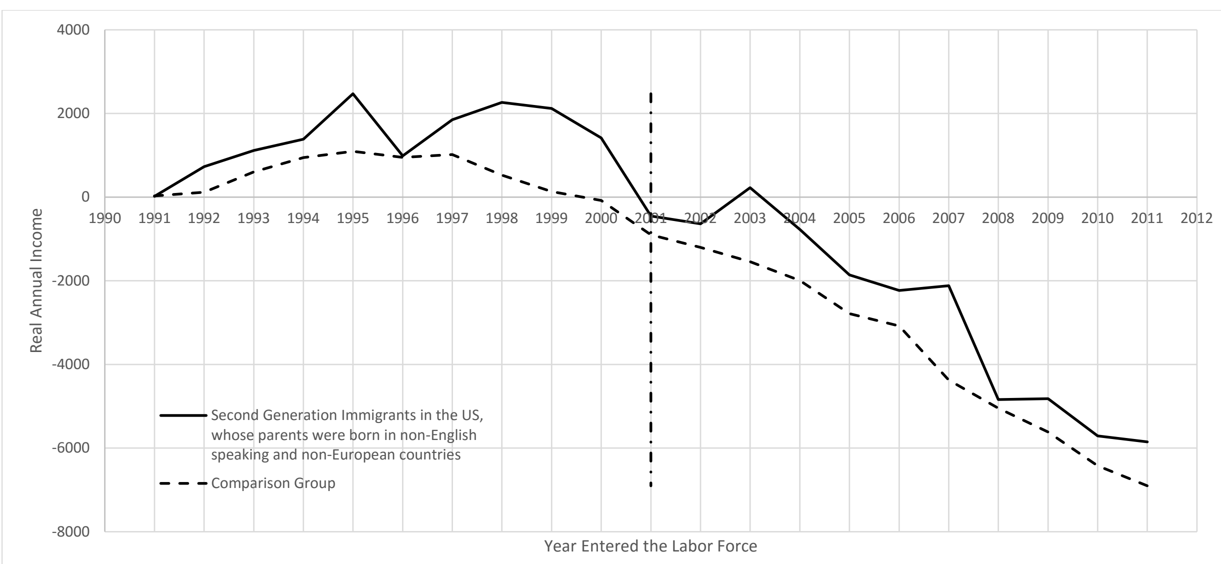


In Figures 10, 11, and 12 I test additional groups to determine whether the effect I measure is unique to Middle Eastern, Afghan, and Pakistani Americans, or if it is a broader effect. In Figure 10 above, I run my preferred model but replace Middle Eastern, Afghan, and Pakistani Americans with all second generation immigrants not in the target group. See Table A10 for the estimates used.

Figure 11: Real Annual Income by Cohort: A Second Placebo Group

In Figure 11 above, I run my preferred model but replace Middle Eastern, Afghan, and Pakistani Americans with all second generation immigrants not in the target group and whose parents were born in non-English speaking countries. See Table A11 for the estimates used.

Figure 12: Real Annual Income by Cohort: A Third Placebo Group



In Figure 11 above, I run my preferred model but replace Middle Eastern, Afghan, and Pakistani Americans with all second generation immigrants not in the target group and whose parents were born in non-English speaking, non-European countries. See Table A12 for the estimates used here.

Figure 13: Real Annual Income by Cohort: A Different Comparison Group

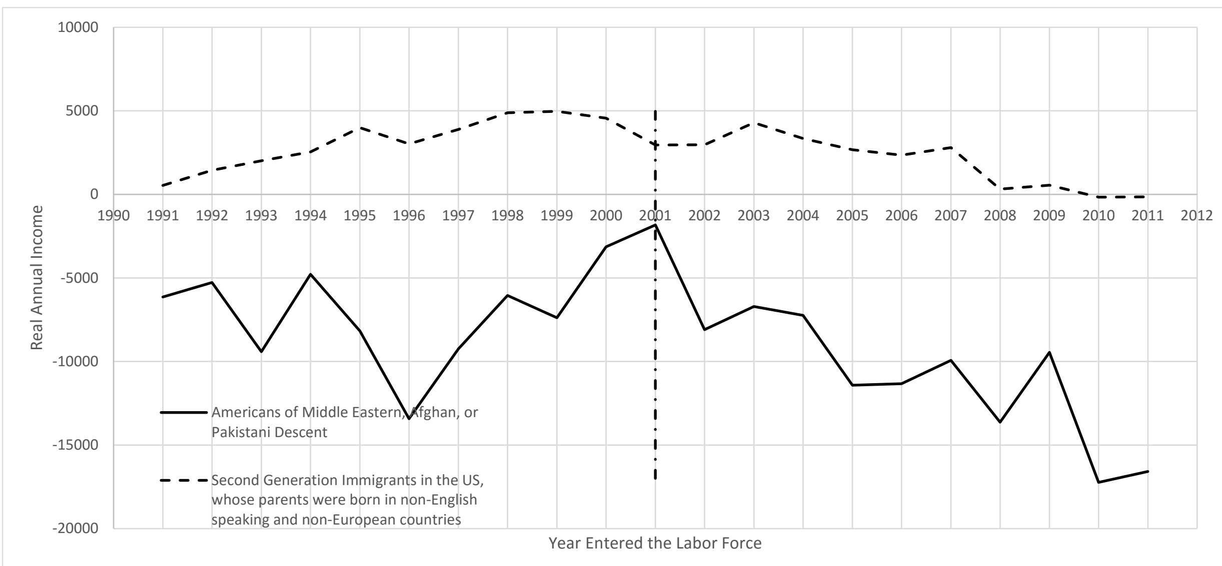


Figure 13 depicts the relative wages received by individuals of each cohort from 1991 to 2011, similar to Figure 4. However, I replace the comparison group with second generation immigrants in the US whose parents did not come from English-speaking or European countries. See Table A13 for the estimates used.

Figure 14: Real Annual Income by Cohort: Service Sector

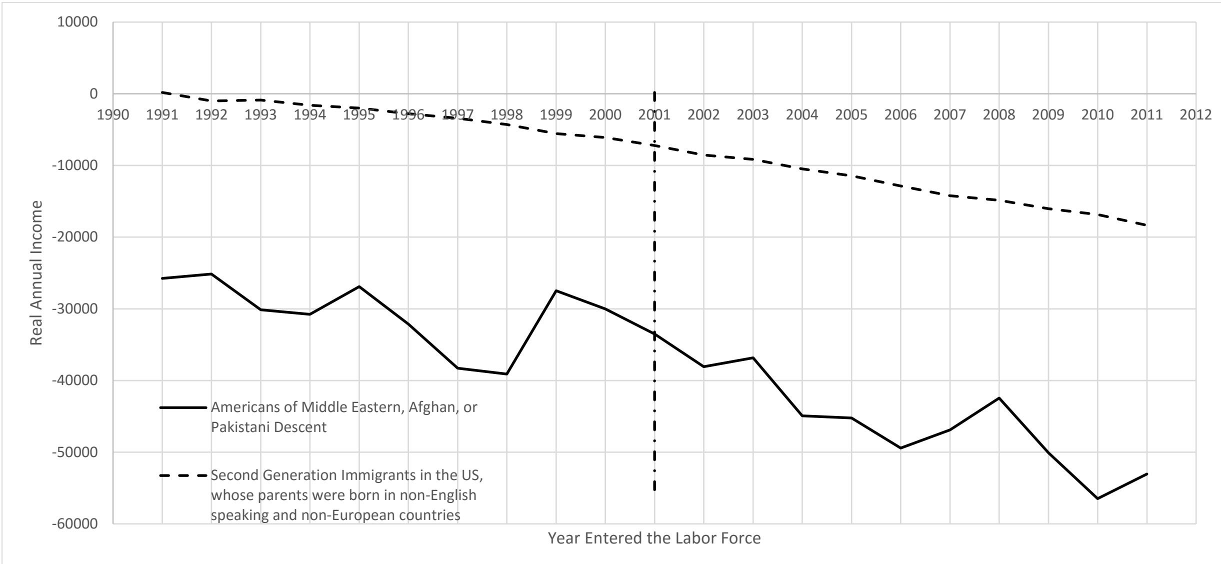


Figure 14 depicts the relative wages received by individuals of each cohort between 1991 and 2011, restricted to occupations classified as either service or sales. These would likely be some of the more customer-facing occupations, and therefore race and ethnicity may play a role in the production function if customers have a preference over who they interact with. See Table A14 for the estimates used.

Figure 15: Average Education by Cohort, by Group

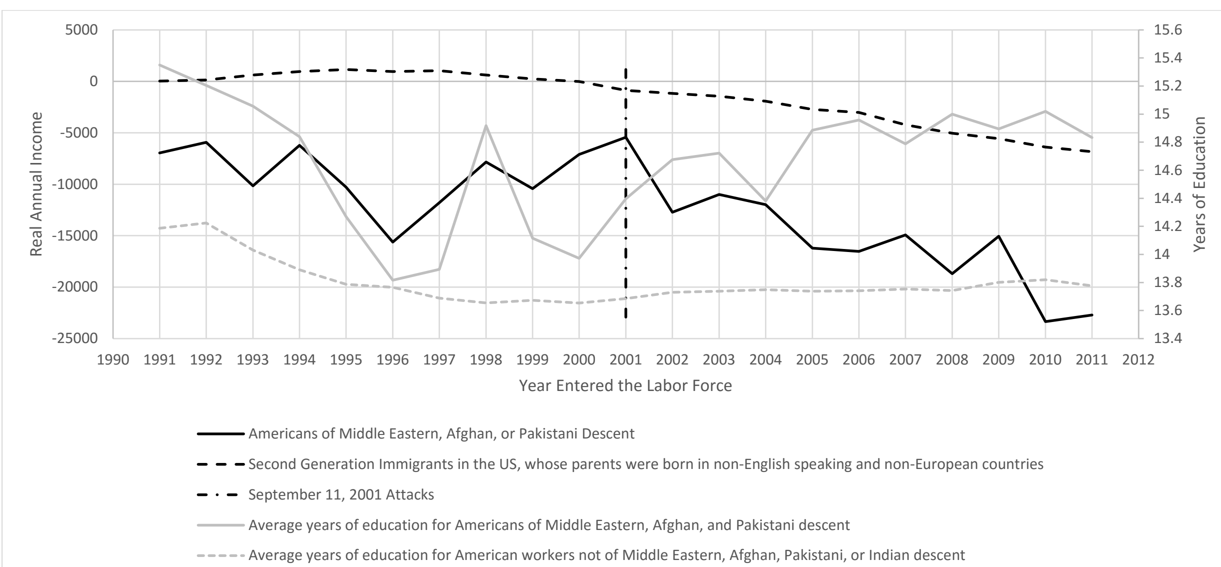


Figure 15 depicts average years of education obtained by Americans of Middle Eastern, Afghan, and Pakistani descent and compares this to the educational attainment of other American workers not of Indian descent. See Table A15 for the estimates used.

8 Appendix

Table A1: Real Annual Income by Cohort: Comparison Group

Year	Coefficient		Clustered SE	
		Robust SE	State x MEAP	Parent Birth Region
1991	29.90	216.64	303.14	126.00
1992	137.79	219.91	297.16	79.28
1993	620.75	222.46	263.49	87.15
1994	963.79	231.48	327.15	118.04
1995	1153.86	242.09	317.94	184.54
1996	952.39	257.21	394.38	201.71
1997	1045.72	274.34	408.98	187.14
1998	611.40	292.11	395.78	299.52
1999	233.95	308.68	453.42	367.73
2000	-4.04	328.66	512.51	324.84
2001	-879.58	348.44	521.13	352.41
2002	-1178.08	369.12	539.92	337.00
2003	-1441.18	392.07	541.19	474.06
2004	-1920.58	413.94	643.97	460.37
2005	-2735.29	433.82	592.67	442.45
2006	-3029.45	460.66	625.73	602.01
2007	-4216.78	481.28	627.53	556.66
2008	-5045.00	503.37	660.77	549.21
2009	-5569.96	529.56	707.34	578.00
2010	-6387.70	552.00	756.15	624.67
2011	-6834.67	583.26	825.18	598.96

Table A1 depicts the relative wages received by individuals of each cohort from 1991-1999, who are in the comparison group: Americans neither in the target group nor of Indian descent. The cohort for 1990 is included in the sample, but is the "left out" cohort. Estimates are calculated using Model 4. See Figure 4 to find the plot of these estimates in conjunction with those from Table A2.

I present three standard errors: White's robust standard errors and two clustered standard errors. The first cluster uses whether a worker is in the target group and each state. Thus there are 102 potential clusters. The second cluster considers parent birth region. I develop an indicator for the continent of birth for each parent: North America, South and Central America, Europe, Asia, Africa, and Oceana. I then cluster around each permutation of mother and father birth continent (36 clusters). The estimates from Table A1 are plotted with those from Table A2 in Figure 4.

Table A2: Real Annual Income by Cohort: Target Group

Year	Coefficient		Clustered SE	
		Robust SE	State x MEAP	Parent Birth Region
1991	-6960.14	6503.71	8115.73	8854.19
1992	-5931.17	6188.39	5809.51	8780.35
1993	-10159.52	6159.57	6002.09	6753.61
1994	-6228.66	5592.05	5394.37	5880.24
1995	-10285.70	5824.27	5302.29	6935.91
1996	-15617.39	5682.85	5915.17	7125.76
1997	-11788.05	5831.13	5503.68	8782.16
1998	-7840.72	6677.15	6752.68	12408.63
1999	-10416.31	5931.31	4868.35	6047.79
2000	-7107.97	5958.87	5111.16	7480.57
2001	-5434.46	5984.17	5858.58	8598.50
2002	-12708.36	5813.85	6046.84	7480.28
2003	-10997.65	6210.76	6656.19	7885.45
2004	-11990.37	5697.64	5832.67	6463.44
2005	-16207.95	5719.30	5691.20	11544.26
2006	-16519.02	6874.29	6954.14	8089.43
2007	-14934.11	5731.22	6366.84	9085.30
2008	-18687.98	5731.81	5425.95	8070.66
2009	-15042.41	6540.25	6443.00	7546.52
2010	-23349.41	6341.45	4933.41	9488.27
2011	-22701.37	5603.23	5630.53	7028.83

Table A2 depicts the relative wages received by individuals of each cohort from 1991-1999, who are in the target group: Americans of Middle Eastern, Afghan, and Pakistani descent. The cohort for 1990 is included in the sample, but is the "left out" cohort. Estimates are calculated using Model 4. See Figure 4 to find the plot of these estimates in conjunction with those from Table A2.

I present three standard errors: White's robust standard errors and two clustered standard errors. The first cluster uses whether a worker is in the target group and each state. Thus there are 102 potential clusters. The second cluster considers parent birth region. I develop an indicator for the continent of birth for each parent: North America, South and Central America, Europe, Asia, Africa, and Oceana. I then cluster around each permutation of mother and father birth continent (36 clusters). The estimates from Table A1 are plotted with those from Table A2 in Figure 4.

Table A3: Loss Estimates in terms of Real Annual Income

Year	Loss Point Estimate	Robust SE	Clustered Standard Errors	
			State x MEAP	Parent Birth Region
1991	-6990.04	6507.48	8119.53	8835.84
1992	-6068.96	6191.93	5815.27	8749.03
1993	-10780.28	6162.85	6001.85	6738.87
1994	-7192.46	5595.70	5399.48	5837.85
1995	-11439.56	5827.66	5303.47	6909.26
1996	-16569.77	5684.69	5909.63	7072.69
1997	-12833.77	5832.43	5490.22	8689.55
1998	-8452.13	6678.34	6743.62	12263.48
1999	-10650.26	5931.01	4851.59	5940.18
2000	-7103.94	5959.29	5096.01	7334.87
2001	-4554.88	5981.34	5840.12	8445.94
2002	-11530.29	5809.91	6032.23	7374.43
2003	-9556.47	6205.29	6628.20	7695.51
2004	-10069.80	5691.38	5804.21	6416.85
2005	-13472.66	5712.11	5663.81	11319.05
2006	-13489.57	6869.01	6917.54	7870.51
2007	-10717.33	5721.26	6324.46	8827.79
2008	-13642.98	5718.34	5369.75	7852.91
2009	-9472.46	6528.65	6398.51	7362.52
2010	-16961.71	6325.47	4863.49	9215.27
2011	-15866.70	5584.43	5570.71	6773.84

Table A3 reframes the estimates in Tables A1 and A2 into the loss in real annual income experienced by each cohort of Middle Eastern, Afghan, and Pakistani Americans relative to other US-born workers of non-Indian descent. We can see the plot of these estimates in Figure 5.

I present three standard errors: White's robust standard errors and two clustered standard errors. The first cluster uses whether a worker is in the target group and each state. Thus there are 102 potential clusters. The second cluster considers parent birth region. I develop an indicator for the continent of birth for each parent: North America, South and Central America, Europe, Asia, Africa, and Oceana. I then cluster around each permutation of mother and father birth continent (36 clusters).

Table A4: Yearly Observed Wage Effects: Log Real Annual Wage

	US-Born only		Foreign-Born and US-Born	
Year	Coefficient	Standard Error	Coefficient	Standard Error
1994	-0.291***	0.093	-0.0596	0.046
1995	-0.019	0.104	-0.004	0.047
1996	-0.126	0.102	-0.0413	0.042
1997	-0.210**	0.093	-0.0605	0.037
1998	-0.129	0.156	-0.0518	0.059
1999	-0.131	0.205	0.036	0.053
2000	-0.032	0.153	-0.0295	0.054
2001	-0.187	0.13	-0.039	0.057
2002	0.049	0.119	-0.0312	0.05
2003	-0.034	0.165	-0.0287	0.05
2004	-0.067	0.1	-0.0385	0.053
2005	-0.13	0.169	-0.0081	0.054
2006	-0.039	0.117	-0.0082	0.051
2007	-0.111	0.112	0.0082	0.042
2008	0.054	0.135	-0.0293	0.043
2009	-0.081	0.075	-0.0293	0.055
2010	0.025	0.078	-0.0269	0.053
2011	0.157	0.123	0.0089	0.028

Table A4 shows the estimated loss each year over the period 1994-2011 for Middle Eastern, Afghan, and Pakistani workers in the US compared to US-born workers not of Indian descent in terms of log(real annual wages). Two estimates are included: the first includes only US-born workers (the focus of this paper) and the second includes both foreign-born and US-born workers (like in most of the related discrimination literature). The period starts in 1994 because this is when the Census Bureau began collecting the information I use to define the target and comparison groups. See Figure 1 to see the plot of these estimates. Standard errors presented here are White's robust standard errors.

Table A5: Yearly Observed Wage Effects: Log Real Weekly Earnings

	US-Born only		Foreign-Born and US-Born	
Year	Coefficient	Standard Error	Coefficient	Standard Error
1994	-0.288***	0.091	-0.0596	0.044
1995	-0.014	0.103	-0.003	0.044
1996	-0.124	0.101	-0.0459	0.037
1997	-0.208**	0.092	-0.0626	0.04
1998	-0.127	0.153	-0.0587	0.06
1999	-0.127	0.203	0.0305	0.05
2000	-0.03	0.152	-0.0355	0.053
2001	-0.171	0.124	-0.0392	0.055
2002	0.051	0.118	-0.039	0.05
2003	-0.033	0.163	-0.0358	0.051
2004	-0.067	0.099	-0.0476	0.049
2005	-0.128	0.168	-0.0166	0.049
2006	-0.036	0.115	-0.0149	0.05
2007	-0.112	0.111	-0.0014	0.041
2008	0.061	0.131	-0.0337	0.04
2009	-0.084	0.077	-0.0384	0.053
2010	0.022	0.077	-0.0334	0.049
2011	0.155	0.121	0.0139	0.025

Table A5 shows the estimated loss each year over the period 1994-2011 for Middle Eastern, Afghan, and Pakistani workers in the US compared to US-born workers not of Indian descent in terms of log(real weekly earnings). See Figure 2 to see the plot of these estimates. Standard errors presented here are White's robust standard errors.

Table A6: Yearly Observed Wage Effects: Log Real Hourly Wage

	US-Born only		Foreign-Born and US-Born	
Year	Coefficient	Standard Error	Coefficient	Standard Error
1994	-0.077	0.079	-0.0316	0.04
1995	-0.061	0.051	-0.0305	0.038
1996	-0.091*	0.054	-0.05	0.032
1997	-0.119**	0.052	-0.0492*	0.029
1998	-0.286***	0.105	-0.0887**	0.042
1999	-0.12	0.133	0.016	0.046
2000	-0.033	0.115	-0.0425	0.051
2001	-0.123	0.076	-0.0613	0.045
2002	0.044	0.11	-0.0219	0.04
2003	-0.02	0.106	-0.0469	0.051
2004	-0.095	0.068	-0.0221	0.044
2005	-0.129	0.085	0.0218	0.041
2006	-0.105	0.079	-0.0126	0.049
2007	-0.001	0.062	0.0088	0.035
2008	0.025	0.076	-0.0094	0.038
2009	-0.069	0.044	-0.044	0.033
2010	0.076	0.056	-0.0157	0.042
2011	0.114	0.083	0.0186	0.029

Table A6 shows the estimated loss each year over the period 1994-2011 for Middle Eastern, Afghan, and Pakistani workers in the US compared to US-born workers not of Indian descent in terms of log(real hourly wage). See Figure 3 to see the plot of these estimates. Standard errors presented here are White's robust standard errors.

Table A7: Graph of Loss by Cohort: Model Specifications

Year	Model 1		Model 2		Model 3		Model 4	
1991	-6990.04	(8044.37)	-7170.19	(6604.19)	-6942.78	(8066.42)	-6892.16	(8100.32)
1992	-6068.96	(5884.43)	-4497.91	(4702.89)	-6054.05	(5914.47)	-6056.25	(5932.28)
1993	-10780.28*	(6008.25)	-6664.42	(4879.43)	-10809.83*	(6044.29)	-10809.71*	(6061.07)
1994	-7192.46	(5386.83)	-2380.66	(4798.81)	-7212.33	(5411.73)	-7203.57	(5419.84)
1995	-11439.56**	(5270.47)	-6381.92	(5114.21)	-11432.95**	(5287.44)	-11427.25**	(5301.92)
1996	-16569.77***	(5939.71)	-9677.49*	(5306.35)	-16567.91***	(5948.45)	-16573.10***	(5958.18)
1997	-12833.77**	(5586.52)	-6160.33	(4789.39)	-12813.97**	(5631.75)	-12800.98**	(5654.31)
1998	-8452.13	(6785.04)	-5521.77	(6301.33)	-8490.79	(6808.67)	-8466.51	(6818.94)
1999	-10650.26**	(4967.98)	-3423.39	(4821.72)	-10671.57**	(4987.70)	-10660.35**	(4986.61)
2000	-7103.94	(5187.95)	1204.4	(3936.91)	-7089.26	(5214.61)	-7066.15	(5233.11)
2001	-4554.88	(5928.53)	2764.35	(4674.91)	-4554.66	(5958.61)	-4535.11	(5980.23)
2002	-11530.29*	(6174.54)	-5031.49	(4737.94)	-11541.31*	(6184.64)	-11515.83*	(6201.56)
2003	-9556.47	(6744.58)	-2486.02	(5199.67)	-9587.59	(6781.01)	-9569.84	(6794.84)
2004	-10069.80*	(5880.62)	-1994.76	(5273.44)	-10102.71*	(5902.34)	-10084.33*	(5910.40)
2005	-13472.66**	(5847.74)	-5035.32	(4507.05)	-13465.33**	(5855.87)	-13445.88**	(5878.25)
2006	-13489.57*	(7123.91)	-4525.39	(5670.18)	-13532.18*	(7133.70)	-13505.94*	(7154.55)
2007	-10717.3	(6570.58)	-1219.29	(4896.94)	-10647.8	(6599.89)	-10634.5	(6617.32)
2008	-13642.98**	(5463.43)	-4167.09	(4967.16)	-13618.04**	(5473.95)	-13602.13**	(5484.89)
2009	-9472.46	(6406.08)	-696.24	(5218.50)	-9526.1	(6427.73)	-9515.52	(6431.67)
2010	-16961.71***	(4908.08)	-7134.59*	(3941.48)	-16942.20***	(4924.63)	-16928.21***	(4944.15)
2011	-15866.70**	(5979.47)	-5340.34	(4859.05)	-15836.78**	(5999.02)	-15806.50**	(6018.28)

Table A7 shows estimates for the first four model specificiations seen in Figure 6. Model 1 is my baseline, which I used previously and will continue to use in my other analysis. It estimates Equation 4, but does not include the interaction of the control variables, a time trend control, or a control for years since entering the labor market. Model 2 includes interactions of the control variables, and we can see that while these estimates do not share the same magnitude as the other models, they do share a similar shape and display a similar drop after the attacks (as well as a similar increase prior). Model 3 adds a linear time trend to Model 1. Model 4 changes this linear time trend to a cubic time trend. Standard errors presented here are White's robust standard errors.

Table A8: Graph of Loss by Cohort: Additional Model Specifications

Year	Model 5		Model 6		Model 7	
1991	-6718.74	(8112.63)	-6718.74	(8112.63)	-8110.19	(6816.25)
1992	-6070.42	(5932.92)	-6070.42	(5932.92)	-6340.01	(6564.05)
1993	-10636.14*	(6075.45)	-10636.14*	(6075.45)	-11078.65*	(6432.25)
1994	-7174.07	(5422.28)	-7174.07	(5422.28)	-6232.01	(5913.85)
1995	-11206.25**	(5333.01)	-11206.25**	(5333.01)	-11146.90*	(6171.26)
1996	-16447.61***	(5972.61)	-16447.61***	(5972.61)	-15763.11***	(6015.22)
1997	-12666.76**	(5684.67)	-12666.76**	(5684.67)	-12365.92**	(6160.42)
1998	-8476.79	(6812.87)	-8476.79	(6812.87)	-8048.79	(6933.98)
1999	-10518.22**	(4970.75)	-10518.22**	(4970.75)	-9945.55	(6232.99)
2000	-7088.13	(5228.96)	-7088.13	(5228.96)	-6076.9	(6284.41)
2001	-4370.77	(5966.28)	-4370.77	(5966.28)	-3707.58	(6302.97)
2002	-11489.57*	(6215.66)	-11489.57*	(6215.66)	-11343.70*	(6167.75)
2003	-9608.36	(6837.64)	-9608.36	(6837.64)	-9634.9	(6506.73)
2004	-9945.40*	(5916.99)	-9945.40*	(5916.99)	-9348.15	(6026.87)
2005	-13297.45**	(5872.22)	-13297.45**	(5872.22)	-12792.92**	(6046.66)
2006	-13559.16*	(7191.12)	-13559.16*	(7191.12)	-12760.98*	(7069.14)
2007	-10529.3	(6628.53)	-10529.3	(6628.53)	-9703.19	(6024.40)
2008	-13501.11**	(5484.24)	-13501.11**	(5484.24)	-13069.57**	(6102.00)
2009	-9399.82	(6396.58)	-9399.82	(6396.58)	-8983.11	(6813.68)
2010	-16870.90***	(5004.20)	-16870.90***	(5004.20)	-15990.90**	(6734.14)
2011	-15759.10**	(6085.88)	-15759.10**	(6085.88)	-14831.58**	(5981.95)

Table A8 shows estimates for the last three model specificiations seen in Figure 6. Model 5 uses month fixed effects for each month in the sample. Model 6 replaces the fixed effects of model 5 with state by month fixed effects. Model 7 is identical to Model 1, with the exception that it adds a control for years since entering the labor market. Standard errors presented here are White's robust standard errors.

Table A9: Graph of Loss by Cohort: Additional Outcome Variables

Year	Real Hourly Wage		Real Weekly Earnings		Probability Employed	
	Coefficient	Robust SE	Coefficient	Robust SE	Coefficient	Robust SE
1991	-1.90	2.60	-66.98	135.73	-0.32	0.26
1992	-0.89	2.83	-98.16	91.68	-0.12	0.22
1993	-2.12	2.18	-165.02*	96.62	-0.77***	0.20
1994	-0.73	2.13	-83.29	84.34	-0.21	0.19
1995	-2.93	2.29	-187.95**	85.22	-0.29	0.21
1996	-3.71	2.49	-260.41***	95.12	-0.22	0.22
1997	-1.92	2.30	-201.16**	93.54	-0.37*	0.21
1998	-1.61	2.52	-128.99	107.42	-0.02	0.19
1999	0.56	2.05	-161.64*	81.23	-0.09	0.19
2000	0.44	2.20	-88.74	84.48	-0.09	0.20
2001	0.49	2.11	-68.84	92.93	-0.10	0.17
2002	-1.27	2.69	-172.84*	101.06	-0.34**	0.17
2003	-0.84	2.45	-157.93	105.64	-0.27	0.20
2004	-0.49	2.48	-135.63	99.79	-0.08	0.16
2005	-3.77	2.32	-218.00**	89.86	-0.24	0.16
2006	-0.68	2.64	-217.05*	111.09	-0.26	0.17
2007	-0.44	2.23	-147.51	96.11	-0.23	0.19
2008	11.34	13.75	-199.79**	86.04	-0.35*	0.19
2009	-0.94	2.29	-130.19	101.61	-0.37*	0.21
2010	-1.97	2.92	-279.20***	79.05	-0.43**	0.18
2011	-2.89	2.25	-240.00***	88.70	-0.13	0.20

Table A9 depicts the loss estimates by cohort over additional outcome variables: Real Hourly Wage, Real Weekly Earnings, and Probability Employed; these are the estimates used for Figures 7, 8, and 9, respectively. Probability Employed is estimated using a probit model.

Table A10: Real Annual Income by Cohort: A Placebo Group

Cohort Year	Comparison Group		Target Group	
	Coefficient	Robust SE	Coefficient	Robust SE
1991	165.02	307.71	-1652.97**	692.43
1992	167.65	314.66	-369.47	759.88
1993	675.96**	287.39	-232.82	1332.01
1994	1042.86***	332.63	-293.99	723.66
1995	1240.11***	346.98	-191.47	1150.07
1996	1052.06***	393.88	-474.12	802.67
1997	1211.39***	433.68	-1026.92	1234.32
1998	654.03	407.91	-348.45	775.46
1999	214.01	460.07	-249.74	930.84
2000	83.05	526.58	-1454.43	1044.32
2001	-806.24	541.89	-2305.94**	926.56
2002	-1077.89*	539.69	-2772.24***	1035.18
2003	-1437.83**	561.50	-2299.34***	853.88
2004	-1862.43***	656.03	-3307.08***	947.80
2005	-2629.79***	616.79	-4457.30***	1033.47
2006	-3112.87***	655.40	-3406.59***	736.39
2007	-4135.70***	680.70	-5822.14***	1083.89
2008	-4949.61***	688.26	-6705.91***	1005.52
2009	-5461.34***	742.78	-7344.25***	1101.99
2010	-6313.10***	779.41	-7963.34***	1016.98
2011	-6598.11***	844.58	-9381.20***	1064.34

Table A10 shows the estimates used in Figure 10, in which I run my preferred model, but replace my target group with second generation immigrants not in the target group and not of Indian descent.

Table A11: Real Annual Income by Cohort: A Second Placebo Group

Cohort Year	Comparison Group		Target Group	
	Coefficient	Robust SE	Coefficient	Robust SE
1991	21.89	310.18	158.75	788.72
1992	107.62	310.43	742.93	1248.57
1993	609.62**	270.69	718.29	1609.82
1994	957.70***	326.78	717.59	921.14
1995	1101.30***	339.19	1935.52	1632.83
1996	950.78**	402.53	846.05	1059.19
1997	1017.14**	432.57	1457.48	1263.07
1998	567.31	415.42	1085.86	1263.55
1999	153.30	464.98	1245.87	982.62
2000	-38.54	525.04	288.90	1166.06
2001	-860.47	543.81	-1503.02	1181.28
2002	-1191.77**	550.72	-1226.98	1247.70
2003	-1510.65**	568.20	-763.63	1234.04
2004	-1948.95***	662.77	-1929.24*	1109.91
2005	-2755.62***	617.68	-2778.82**	1244.25
2006	-3065.32***	656.04	-2922.89***	1004.78
2007	-4298.89***	665.91	-3602.99***	1160.94
2008	-5032.86***	686.11	-5475.99***	1233.41
2009	-5554.16***	751.19	-6007.71***	1256.68
2010	-6380.27***	784.01	-6702.53***	1045.52
2011	-6831.01***	872.99	-7150.13***	1266.19

Table A11 shows the estimates used in Figure 11, in which I run my preferred model, but replace my target group with second generation immigrants not in the target group and whose parents were born in non-English speaking countries.

Table A12: Real Annual Income by Cohort: A Third Placebo Group

Cohort Year	Comparison Group		Target Group	
	Coefficient	Robust SE	Coefficient	Robust SE
1991	27.43	313.10	19.66	903.34
1992	117.69	312.86	725.54	948.97
1993	601.86**	274.47	1115.10	1553.96
1994	943.47***	335.92	1383.94*	791.11
1995	1094.81***	342.59	2472.01*	1312.38
1996	950.48**	407.29	989.17	755.76
1997	1015.45**	423.99	1848.33**	905.80
1998	527.26	410.16	2263.69*	1294.23
1999	135.27	458.12	2120.26**	920.92
2000	-78.70	528.54	1414.36*	770.56
2001	-903.25	547.94	-451.94	882.43
2002	-1203.76**	561.82	-638.22	1055.90
2003	-1542.86***	567.35	227.05	881.71
2004	-1996.40***	663.88	-770.60	616.15
2005	-2787.99***	620.84	-1858.12*	957.34
2006	-3082.19***	655.44	-2229.52**	899.24
2007	-4372.86***	659.06	-2116.85**	979.06
2008	-5047.01***	684.84	-4842.03***	1047.53
2009	-5614.96***	750.29	-4821.15***	1148.55
2010	-6423.27***	781.91	-5711.74***	762.13
2011	-6902.56***	872.76	-5851.91***	995.14

Table A12 shows the estimates used in Figure 12, in which I run my preferred model, but replace my target group with second generation immigrants not in the target group and whose parents were born in non-English speaking, non-European countries.

Table A13: Real Annual Income By Cohort: A Different Comparison Group

Cohort Year	Comparison Group		Target Group	
	Coefficient	Robust SE	Coefficient	Robust SE
1991	530.71	834.50	-6143.58	7760.26
1992	1447.57	1053.72	-5268.20	5942.70
1993	2015.74	1724.34	-9412.23	5850.86
1994	2546.86**	1133.83	-4779.37	5421.04
1995	3987.51***	1308.09	-8168.94	5417.98
1996	3019.37**	1284.61	-13424.68**	5678.64
1997	3889.27***	1299.17	-9244.52*	5273.10
1998	4890.32***	1563.06	-6052.99	6642.75
1999	4971.81***	1413.13	-7383.77	4750.53
2000	4563.87***	1315.95	-3141.49	5061.02
2001	2957.99**	1234.26	-1828.01	5665.36
2002	2971.92**	1439.22	-8097.53	5771.28
2003	4292.37***	1524.06	-6703.59	6205.98
2004	3347.26**	1610.94	-7236.29	5708.22
2005	2676.94	1647.20	-11424.44**	5533.40
2006	2344.66	1453.43	-11324.40	7136.17
2007	2803.19*	1444.81	-9929.75	6326.98
2008	313.44	1874.13	-13627.23**	5341.61
2009	549.42	1842.90	-9456.86	6976.69
2010	-165.22	2109.90	-17233.04***	5370.28
2011	-149.93	1918.22	-16579.15***	5937.38

Table A13 shows the point estimates used in the creation of Figure 13. The model is estimated with my base model, in a similar fashion to Figure 4. However, I replace the comparison group with second generation immigrants in the US whose parents did not come from English-speaking or European countries. This tests whether there is a measurable effect, even compared to the group most likely affected by spillover discrimination.

Table A14: Real Annual Income by Cohort: Service Sector

Cohort Year	Comparison Group		Target Group	
	Coefficient	Robust SE	Coefficient	Robust SE
1991	184.57	427.81	-25748.74**	11802.36
1992	-997.81**	478.23	-25147.33	15437.99
1993	-885.22*	450.82	-30124.38**	12098.21
1994	-1613.18***	516.28	-30760.13**	12890.26
1995	-2009.00***	576.18	-26910.06*	14126.49
1996	-2783.64***	722.72	-32132.00**	12183.00
1997	-3403.29***	470.10	-38297.28***	12268.69
1998	-4281.23***	534.62	-39095.87***	14054.82
1999	-5558.34***	649.94	-27479.52***	7698.53
2000	-6088.90***	764.37	-30021.26**	13024.07
2001	-7213.86***	751.33	-33520.02**	12795.53
2002	-8540.13***	720.57	-38069.76***	12735.03
2003	-9153.00***	782.27	-36827.86***	13612.82
2004	-10494.10***	913.04	-44926.91***	12417.95
2005	-11453.68***	917.91	-45231.70***	13596.48
2006	-12869.66***	891.93	-49430.73***	12181.63
2007	-14233.13***	940.57	-46894.87***	12595.62
2008	-14865.25***	927.77	-42456.30***	13091.14
2009	-16034.00***	1092.44	-50083.33***	12411.49
2010	-16850.63***	1096.22	-56480.95***	12642.65
2011	-18348.01***	1040.38	-53054.79***	11773.85

Table A14 shows the point estimates used in Figure 14, which restricts our analysis to occupations classified as service or sales. These would likely be some of the more customer-facing occupations, and therefore race and ethnicity may play a role in the production function if customers have a preference over the kind of person they interact with. Standard errors are White's Robust Standard Errors. Estimates are created using Specification 1 from Figure 6.

Table A15: Average Education by Cohort

	Target Group			Comparison Group		
Cohort	Mean Years of Education	Standard Deviation	Observations	Mean Years of Education	Standard Deviation	Observations
1991	15.35	2.46	60	14.19	2.25	37,392
1992	15.21	2.13	78	14.22	2.30	37,032
1993	15.06	2.39	71	14.03	2.36	37,805
1994	14.84	2.74	112	13.89	2.47	37,519
1995	14.27	2.30	89	13.79	2.44	36,603
1996	13.82	3.37	98	13.77	2.45	34,789
1997	13.89	2.40	85	13.69	2.40	33,725
1998	14.92	3.04	73	13.65	2.40	32,574
1999	14.12	2.63	95	13.67	2.39	31,694
2000	13.97	2.39	107	13.65	2.36	29,743
2001	14.40	2.78	96	13.68	2.35	28,316
2002	14.68	2.68	74	13.73	2.35	27,196
2003	14.72	2.29	79	13.74	2.34	25,396
2004	14.38	2.53	105	13.75	2.36	23,657
2005	14.89	2.67	87	13.74	2.34	21,867
2006	14.96	2.31	71	13.74	2.33	19,879
2007	14.79	3.07	80	13.75	2.31	18,239
2008	15.00	2.39	80	13.74	2.30	16,293
2009	14.89	2.37	66	13.80	2.26	14,533
2010	15.02	1.93	51	13.82	2.21	12,765
2011	14.83	2.40	66	13.78	2.22	11,224
All	14.61	2.60	1,723	13.82	2.36	568,241

This table shows the education point estimates used in the creation of Figure 15, which depicts average years of education obtained by Americans of Middle Eastern, Afghan, and Pakistani descent and compares this to the educational attainment of my comparison group. These points are plotted in conjunction with estimates from Table A3, in order to visualize whether there is a correlation between variations in education by cohort and income differences by cohort.

Figure A1: For convenience, I include here Figure 2 from Charles and Guryan (2008), which provides the most intuitive depiction I have found of Becker's (1957) discrimination model.

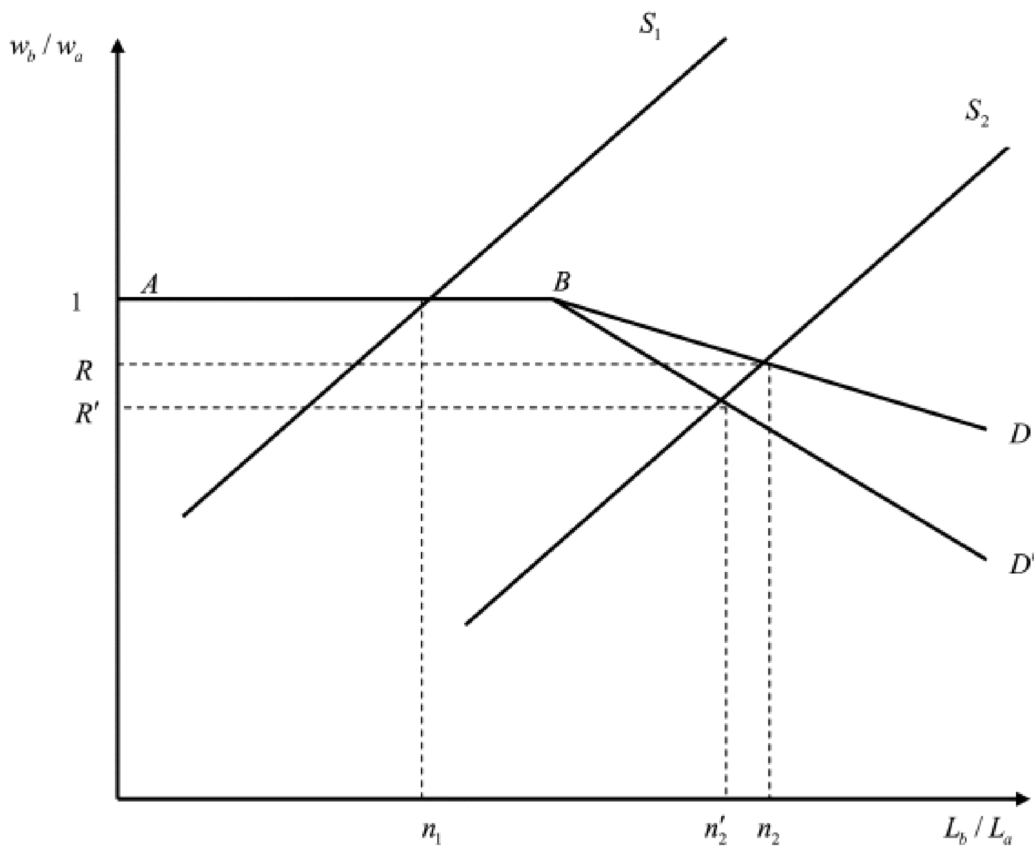


FIG. 2.—Relationship between racial tastes and the relative wages and relative supply of blacks and whites. The figure shows how the equilibrium ratio of black to white wages responds to three sets of market conditions. When the relative supply of black workers is small relative to the number of unprejudiced employers, as is the case when supply is as depicted by S_1, the marginal discriminator is unprejudiced and there is no racial wage gap in equilibrium. When the distribution of racial preferences among employers is held constant, a shift out in the relative supply of black workers (from S_1 to S_2) requires that more prejudiced employers hire blacks, and the ratio of black to white wages falls from one to *R*. When the relative supply of black workers is held constant, an increase in prejudice among employers likely to be the marginal discriminator (which causes the relative demand curve to rotate from *ABD* to *ABD'*), further reduces the equilibrium ratio of black to white wages to *R'*.

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