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Essays in Law and Economics

by

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Committee in charge:

Professor Edward Miguel, Chair
Professor Patrick Kline
Professor Justin McCrary
Professor Steven Raphael

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Abstract

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This *Dissertation* consists of three empirical applications on the economics of crime and law enforcement.

Chapter 1 uses over 200,000 patrol stops conducted by the Oakland Police Department to estimate differences in policing behavior among black, white, Hispanic, and Asian officers. In contrast to previous studies which consider average differences at the city or state level, this study uses individual officers' patrol assignments and the exact date, time, and geographic coordinates of each stop to identify between-officer differences.

The data indicate little to no differences across officer race on average, but substantial differences within neighborhoods. In general, minority officers less intensely police all races in minority neighborhoods, but *more* intensely police all races in white neighborhoods relative to their white officer peers.

A model of police behavior with imperfect information offers one explanation for this result. In the model, an unbiased officer with relatively high ability to interpret suspect behavior polices with relatively low intensity. The observed outcomes are then consistent with officers possessing neighborhood-specific informational advantages in policing, particularly with respect to their own race. That is, minority officers better interpret suspect behavior in minority neighborhoods, while white officers better interpret suspect behavior in white neighborhoods. Simulation results suggest that small differences in interpretative ability (modeled as noise in signals observed by the officer) can generate the observed magnitudes.

Chapter 2, which is coauthored with Justin McCrary, presents evidence from six data sets on the participation of youth in crime near the age of criminal majority. The evidence suggests smooth behavior through the transition to adulthood, despite substantial changes in punitiveness, and is consistent with small deterrence effects of long prisons sentences for young offenders.

Chapter 3 reconsiders the empirical analysis of Knowles, Persico, and Todd (2001). Knowles, Persico, and Todd (2001) presents a model of police and motorist behavior in the

context of vehicle searches, and tests it using data from Maryland. The main implication of the Knowles et al. model is that in the absence of racial discrimination, the proportion of searches yielding drugs (or "hit rate") will be equated across races. A relatively low hit rate for any group suggests that police may improve their overall hit rate by shifting resources away from that group, and is thus evidence toward discrimination. Using data on vehicle searches by Maryland State Police, they find no bias against blacks relative to whites, but significant bias against white females and particularly Hispanics.

However, while their study focused on searches occurring along Interstate 95, this study considers all vehicle searches in Maryland, both for the time period studied in Knowles, Persico, and Todd (2001) (1995–1999) and in more recent years (1995–2006). The main results suggest substantially lower hit rates for blacks for searches occurring off Interstate 95, though almost no difference for searches on Interstate 95.

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To Luciana

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Chapter 1

Officer Race and Policing Intensity

1.1 Introduction

The potential for interracial contact in law enforcement has increased dramatically in recent years. Between 1960 and 2010, the share of minorities in the U.S. population more than doubled from 17 to 36 percent, with the fastest growth in shares of Hispanic/Latino (less than 5 to over 16 percent) and Asian (0.5 to nearly 5 percent).[1] Minority representation in local police forces has likely at least tripled over the same period. The most recent national survey in 2007 reports 1 in 4 sworn officers belonging to a minority: 12 percent black, 10 percent Hispanic, and 2 percent Asian.[2]

These demographic shifts motivate two questions asked in this paper: (1) Does an officer's race influence policing behavior? and (2) Does this influence depend on the type of neighborhood an officer patrols — rich or poor, black or white? In contrast to previous studies which consider averages at the city or state level, this paper uses individual officer patrol assignments from the Oakland Police Department (OPD) along with the exact date, time, and geographic coordinates of each officer-civilian contact to address these questions.

The data indicate three main results. First, there is little to no difference in the racial composition of stops across officer race on average. Relative to white officers, black officers are 0.6 percentage points more likely to stop a black suspect (standard error 0.5), Hispanic officers 0.8 (0.4) less likely, and Asian officers 1.2 (0.5) less likely. Second, there are moderate differences in average policing intensity across officer race as measured by number of stops per shift. Black and Asian officers stop 4–5 percent fewer people than white officers,

[1] U.S. Census Bureau. The earliest figures for Hispanic/Latino shares are from 1970.

[2] 2007 Law Enforcement Management and Administrative Statistics (LEMAS) survey. The first national LEMAS survey in 1987 reports a 15 percent minority share of total sworn officers. Earlier figures on police composition are more difficult to obtain. McCrary (2007) calculates for the largest 314 cities that the African American share of police tripled between 1970 and 1999 from 6 to 18 percent, while their demographic share in the same cities increased from 16 to 28 percent.

while Hispanic officers stop 1.5 percent more. Finally, there is substantial heterogeneity across neighborhoods. In general, minority officers less intensely police all races in minority neighborhoods, but *more* intensely police all races in white neighborhoods relative to their white officer peers. This is especially true for minorities policing minorities: Compared to white officers, Hispanic officers stop 3 percent fewer Hispanics when patrolling Hispanic neighborhoods, but 10 percent more when patrolling non-Hispanic neighborhoods. Black officers stop 14 percent fewer blacks in black neighborhoods, and 2.5 percent more in white neighborhoods, though the last figure is not statistically significant at conventional levels. There are similar trends in the racial composition of stops. Black officers are 3 percent less likely to stop blacks in black neighborhoods, but 2 percent more likely to stop blacks in white neighborhoods.

A model of police behavior with imperfect information offers one explanation for this result. In the model, an unbiased officer with relatively high ability to interpret suspect behavior polices with relatively low intensity. The observed outcomes are then consistent with officers possessing neighborhood-specific informational advantages in policing, particularly with respect to their own race. That is, minority officers better interpret suspect behavior in minority neighborhoods, while white officers better interpret suspect behavior in white neighborhoods. Simulation results presented in section 1.4.5 suggest that small differences in interpretative ability (modeled as noise in signals observed by the officer) can generate the observed magnitudes.

The estimates imply that minority neighborhoods would be less intensely policed if officers patrolled neighborhoods of their own race. Officer segregation would be consistent with policy elsewhere in the criminal justice system, for example in prisons (Goodman (2008)). In *Johnson v. California*, the Ninth Circuit upheld an unwritten policy of racial segregation for prison cell assignment in California, which the defense described as "Ground Zero for race-based…gangs."[3] But segregation would clearly conflict with broader policies in housing, education, and college admissions. Weitzer (2000) finds broad opposition toward officer segregation among households in Washington DC, who express a strong preference for teams of racially integrated officers over teams of segregated officers. The preference holds across all types of neighborhoods and even for individuals with beliefs that certain officer races are biased. Like Washington, Oakland has high crime, a diverse police force, and is highly segregated: Oakland is the most segregated U.S. city with respect to Hispanics, Washington is third-most with respect to blacks.[4] Integrated teams would have the potential benefit of promoting consistent, though not necessarily optimal, policing (Persico (2002), Eeckhout, Persico, and Todd (2010)).[5]

[3] *Johnson v. California*, 543 U.S. 499, 321 F.3d 791, reversed and remanded. Assistant Attorney General of California Frances T. Grunder in oral arguments.

[4] Segregation is measured by the dissimilarity index at the census block level for cities with populations above 100,000.

[5] Oakland has a relatively small police force and therefore conducts all regular patrols with solo officers.

Previous empirical studies of discrimination in law enforcement have encountered classic problems of missing data and omitted variables (Donohue and Levitt (2001)).[6] The missing data problem is that an officer's choice set — the vehicles or pedestrians that are observed but not stopped — is mostly unknown. Estimates of between-officer differences that do not control for the choice set are likely to be compromised if officers are assigned to neighborhoods with different characteristics. Simple benchmarking with population demographics to estimate differences at the city level is insufficient if criminal propensities differ across race or gender (Grogger and Ridgeway (2006)). Omitted variable problems arise when circumstances of the stop which might otherwise be used to control for the choice set are not recorded. These include factors characterizing the choice set itself such as time and location, or factors which limit an officer's discretion such as response to dispatch calls, special assignments, execution of warrants, or searches of probationers, parolees, and registered sex offenders.[7]

The leading empirical approaches have thus far relied on outcomes-based tests to overcome these problems, most notably in the context of vehicle search. In an influential contribution, Knowles, Persico, and Todd (2001) construct a model in which police are not racially biased if and only if they are on average equally successful in searches of white and minority suspects. In their model, a relatively low success rate for any one race reveals taste-based discrimination against that race. Subsequent models have extended the analysis to settings with multiple officer races (Anwar and Fang (2006), Antonovics and Knight (2009)). The advantage of these models is their ability to simultaneously disentangle types of discrimination and overcome the key empirical problems, but a disadvantage is their sensitivity to heterogeneity at the officer or neighborhood level (Ayres (2001), Anwar and Fang (2006), Sanga (2009)).

The richness of the data used in this study allows for identification of between-officer differences with relatively little reliance on modeling assumptions. It also allows for analysis of all police-civilian contacts, rather than restriction to stops in which a search occurs. In addition to the information story presented above, the data suggest a few other explanations for between-officer differences. For example, some of the minority-white officer differences attenuate as the level of geographic control is refined from the beat assignment (1.6 square miles on average) to the census tract (.5 square miles) to the limiting case of coordinate matching, suggesting that the gaps are partially explained by differences in patrol patterns conditional on a officer's assignment. Interactions between race, experience, and neighbor-

Partnered patrols are currently not feasible as a general practice. By comparison, Los Angeles and Chicago have approximately 30 and 150 percent more officers per capita, though their violent crime rates are 65 and 30 percent lower. (2009 Uniform Crime Reports, 2007 LEMAS, and 2010 OPD personnel records.)

[6]For examples in racial profiling litigation, see *United States v. Arizona*, 703 F.Supp.2d 980 [D. Ariz., 2010]; *Daniels v. City of New York*, 138 F.Supp.2d 562 (2001); *Rodriguez v. California Highway Patrol*, 89 F.Supp.2d 1131 (2000).

[7]See 18 U.S.C. §3522 and §3563(b) (2010).

hood demographics in the style of Altonji and Pierret (2001) also provide suggestive evidence of officers learning to police minorities less intensely.

1.2 Conceptual framework

1.2.1 Setup and equilibrium

Consider two officers, A and B (indexed by k), observing identical flows of suspects in separate locations. Each suspect's behavior is observed by the officer in the form of an aggregate suspicion index, x, which can be thought of as a weighted combination of factors that affect an officer's decision to stop a suspect. Such factors may include nervous behavior or speeding. Suspects come in two races, 1 and 2 (indexed by r), and their suspicion indices are summarized by c.d.f.s $F_1(x)$ and $F_2(x)$. For convenience, F_1 and F_2 share the same support, are strictly increasing, and have two bounded and continuous derivatives. Importantly, F_1 and F_2 — the choice set for each officer — are unobserved by the researcher.

Race 1 and 2 suspects arrive at rates η_1 and η_2. When a suspect arrives, an officer effects a stop if and only if the observed suspicion index exceeds an officer- and race-specific threshold, z_{kr}. The lower z_{kr}, the more intensely officer k polices race r.[8] Denoting equilibrium quantities with asterisks, the rate at which officer k stops type r is

$$m^*_{kr} = [1 - F_r(z_{kr})]\eta_r$$

and the equilibrium fraction of officer-k stops that are race 1 is

$$p^*_k = \frac{m^*_{kr}}{m^*_{k1} + m^*_{k2}}.$$

The empirical section will measure differences between officers' race-specific stop rates, total stop rates, and racial composition of stops. Denote these

$$\begin{aligned} \Delta m^*_r &:= m^*_{Ar} - m^*_{Br} \quad \forall r, \\ \Delta m^* &:= \Delta m^*_1 + \Delta m^*_2, \\ \Delta p^* &:= p^*_A - p^*_B. \end{aligned}$$

Three immediate results motivate the estimation strategy. First, a relatively high race-r stop rate indicates a higher policing intensity and a relatively low threshold for that race ($z_{Ar} < z_{Br} \iff \Delta m^*_r > 0$). Second, it is not possible to determine if one officer uses different

[8] Here the thresholds are taken as exogenous. However, the thresholds may be thought of as the solution to an officer's maximization problem in which expected utility from stopping suspects increases with the severity of the perceived crime (i.e., the suspicion index) and officers are subject to a time constraint.

thresholds for race 1 and 2 ($z_{A1} \neq z_{A2}$) without knowledge of race-specific distributions of criminal behavior and arrival rates (F_r and η_r). This is the missing data or "benchmarking" problem, and is discussed in detail in Grogger and Ridgeway (2006).

Third, the difference in the racial composition of stops across officers, Δp^*, does not by itself indicate even a partial ordering of the race-specific thresholds (z_{kr}) used by each officer. This is known as the infra-marginality problem. For intuition, suppose there are no differences in policing intensity across officers or suspect race ($z_{kr} = z$, $\forall k, r$). Then there will be no difference in the racial composition of stops across officers ($\Delta p^* = 0$). Now suppose officer A polices both races with slightly higher but still equal intensity ($z'_{Ar} = z_{Br} - \epsilon$, $\forall r$, $\epsilon > 0$). If there is a relatively high mass of race 1 at officer A's new threshold (f_1/f_2 is large around z'_{Ar}), then A will stop proportionally more of race 1, and Δp^* is greater than zero. On the other hand, if there is a relatively high mass of race 2 (f_1/f_2 is small around z'_{Ar}) then A stops proportionally more race 2 and Δp^* is less than zero. In both cases, officers police each race with the same intensity. Knowles, Persico, and Todd (2001) and Anwar and Fang (2006) discuss of the infra-marginality problem in the context of vehicle search, and give conditions under which average differences become informative.[9] In contrast, this paper has the benefit of richer data that allow for direct estimation of differences in stop rates.

1.2.2 Signal noise and policing intensity

An officer may observe the true suspicion index with noise because of a lack of experience or knowledge with respect to a certain race. In many cases, this friction in policing will generate the pattern observed in the data, that officers police neighborhoods of their own race less intensely, and neighborhoods of the other race more intensely. Intuitively, noise pushes out the suspicion distribution, increasing the likelihood of effecting a stop. If officers are relatively more knowledgeable about neighborhoods of their own race, then this noise will result in relatively less intense policing for own-race neighborhoods.

To focus on the effect of signal noise, suppose only one suspect race, and equal thresholds for both officers ($z_A = z_B = z$). Officer A observes s, with p.d.f. $f(s)$, but officer B observes $t = s + \epsilon$, where ϵ is independent of s and has p.d.f. $g(\epsilon)$. The p.d.f. of officer B's signal, t, is then the convolution of f and g:

$$h(t) := (f * g)(t) = \int_{-\infty}^{\infty} f(t - \epsilon) g(\epsilon) d\epsilon,$$

and denoting c.d.f.s with capital letters, officers A and B stop at rates

$$m_A^* = [1 - F(z)]\eta$$

[9] Knowles, Persico, and Todd (2001) use only suspect race-specific hit rates, while Anwar and Fang (2006) use officer- and suspect-race specific hit rates in a less restrictive setting.

and

$$m_B^* = [1 - H(z)]\eta.$$

Officer B polices more intensely if noise makes observing signals above the threshold more likely ($H(z) < F(z)$). This not guaranteed to occur for generic F, G, and z, but it is likely under conditions that are reasonable in the current context.

For example, suppose $s \sim N(\mu_s, \sigma_s^2)$ and $\epsilon \sim N(0, \sigma_\epsilon^2)$. Then $t = s + \epsilon$ will also be normal, with mean $\mu_t = \mu_s$ and variance $\sigma_t^2 = \sigma_s^2 + \sigma_\epsilon^2$. Officer B will stop $100 \times \beta$ percent more persons than A, with

$$\beta = \frac{H(z) - \Phi(z)}{1 - \Phi(z)}$$

β is greater than zero so long as $H(z) < F(z)$, which holds in this case when officers stop fewer than one half of the persons they observe. More generally, assuming $g(\epsilon)$ is symmetric around zero, $H(z) < F(z)$ holds if, for a neighborhood around z, the mass on the left side of $f(z)$ is relatively larger and/or bunched around z than the mass on the right, where the relevant neighborhood increases with the variance of ϵ. This is guaranteed if f is downward sloping in the right tail, and officers only stop the relatively extreme (in percentile terms) outliers. Section 1.4.5 extends this example by simulating β for various cutoffs, and finds that a relatively small amount of noise can generate the observed magnitudes in between-officer differences.

1.3 Data

The data come from stops conducted by the Oakland Police Department on regular patrol between 2005–2010. Stops made in response to dispatch calls, on special assignments or other cases of limited officer discretion discussed above have been excluded.

1.3.1 Summary statistics

Table 1.1 gives summary statistics. From column 1, the population of Oakland is 35 percent black, 24 percent white, 22 percent Hispanic and 15 percent Asian. Its police force is disproportionally white (44 percent), but otherwise representative.

Columns 2–5 give distributions of suspect and officer characteristics for regular patrol stops for various samples of the data. The first sample is all regular patrol stops. The second is those for which the officer's characteristics are known, which is limited to stops made by officers that are active as of 2010. The third is stops for which an officer's patrol assignment is known. The fourth and final sample is stops for which the officer's characteristics, patrol assignment, and exact geographic coordinates of the stop are known. It is also the sample that can be to linked to census data on neighborhood characteristics, and is therefore the main sample used in the estimation. The empirical section runs the main estimation on each of the samples with qualitatively similar results.

The distributions of stop characteristics are generally consistent across samples. Sixty-one percent of stops involve a black suspect — nearly twice their population share. Stops occur mostly in the late afternoon to late evening and are more likely to be conducted by less experienced officers. One quarter of officers have less than 4 years of experience, but they conduct over 40 percent of stops. This is partially because younger officers police more intensely, but mostly because older officers are less likely to be in the patrol division, which constitutes a little over one third of the force.

Table 1.2 lists stop characteristics by the race of the suspect stopped. Compared to stops involving Asian or white suspects, those involving black and Hispanic suspects stops last longer, are on more severe charges, and are more likely to result in a search and arrest. Twenty percent of black and Hispanic stops last more than 20 minutes compared to 10 percent of white and asian stops. Thirty percent of Hispanic and nearly 40 percent of black stops result in a search compared to 13 for whites and 12 for asians. The last row of table 1.2 reports the likelihood of finding contraband conditional on searching a suspect, or "hit rate." The Knowles, Persico, and Todd (2001) model would interpret a relatively low hit rate for any race as a negative preferential bias. The black, white, and asian hit rates are each about 17 percent, while the Hispanic rate is closer to 13. Knowles, Persico, and Todd (2001) also find lower hit rates for Hispanics in Maryland.

1.3.2 Oakland demographics

Figure 1.2 outlines Oaklands 338 block groups and gives points of reference. Figures 1.3–1.8 map characteristics of Oakland's neighborhoods and exclude the airport, the outer harbor, and the state park in the southeast. There is clear sorting across race and income. More affluent and suburban neighborhoods line the northeastern boundary known as the Oakland hills. Black neighborhoods are clustered in the west and south, Hispanics along the western harbor, Asians in the central downtown area, and whites in the northern hills and around Piedmont (a separate city).

1.3.3 Officer assignment

Shift assignments are given are at the beat level (35 total) and last for eight hours (either 0:00–8:00, 8:00–16:00, or 16:00–0:00). An officer's assignment is inferred from the data using the beat and time recorded for each stop.

There are three problems with inferring the beat assignment. The first is that officers sometimes conduct stops in neighboring beats. For this reason, the regressions below include fixed effects for each beat (or other geographic unit) in which the officer records a stop over the course of a shift.

The second is overtime. Stop rates are measured as number of stops per day, but the data do not indicate if an officer worked more than 8 hours. If, for example, black officers were more likely to work overtime than Hispanic officers, the black officer stop rates would be artificially increased. As an indirect measure of overtime, figure 1.1 shows the density of the time interval between the first and last stop conducted by an officer in one day, by officer race. These densities are nearly identical, suggesting that no one officer race is associated with greater overtime propensities. The sudden decline in the density at 8 hours highlights the low likelihood of working for over 8 hours, while the slight increase around 16 shows the possibility of working both the 12am–8am and 4pm–12am shifts.

The third is that since the data are at the stop level, there is no record of shifts in which the officer does not conduct a stop. The average number of stops per shift (excluding zeros) is 2.3, so it is possible that there are a significant number of zero-stop shifts. This will not affect the sign of between-officer differences if they are the same sign as differences in percent of shifts with zero stops, or are relatively small if of opposite sign. For example, if conditional on stopping at least one person, officer A stops 4 and B stops 5 persons on average, then B will unconditionally stop more if she is also less likely to have zero stops on a shift. Otherwise, for example, if 10 percent of A's shifts had zero stops, 28 percent of B's shifts would have to be zero-stop in order for both to have equal stop rates.

Figures 1.9–1.13 show the characteristics of officers conducting stops over different neighborhoods in Oakland, and highlight the importance of including controls for an officer's assignment. Black officers are much more likely to patrol white and more affluent neighborhoods, while white officers are more likely to patrol black and less affluent neighborhoods. More experienced officers also tend to patrol affluent neighborhoods in the Oakland Hills. Since officers choose their assignments in the order of seniority, this suggests a general preference for these neighborhoods.

1.4 Estimation and results

1.4.1 Policing intensity

Tables 1.4 and 1.5 report differences between minority and white officers' policing intensities. The baseline specification regresses an officer's number of stops per day on officer race indicators and controls for the officer's assignment:

$$m_{kt} = \mu_c + \beta_B B_k + \beta_H H_k + \beta_A A_i + u_{kt}, \tag{1.1}$$

where m_{kt} is the total number of stops by officer k on day t, B, H, and A are indicators for black, Hispanic, and Asian officers (white officer is omitted), and μ_c is the sum of indicators for year, day of the week, and interactions of the census block group and 8-hour time intervals (0:00–8:00, 8:00–16:00, 16:00–0:00) of each stop conducted on day t. The officer race-specific β's identify average differences in policing intensity (Δm^* from the model).

In table 1.4, the dependent variable is the total number of stops per shift. Column 1 lists the results with no controls for the officer's assignment. White officers stop 2.3 persons per day. Black officers stop 0.07 fewer, Hispanic officers .15 more, and Asian officers .03 fewer. When assignment controls are included in column 2, the estimates change to -0.11 (0.03) for black, 0.05 (0.02) for Hispanic, and -0.10 (0.02) for Asian officers. The addition of assignment controls raises the adjusted R-squared from 0.001 to nearly 0.8. Using the average number of stops by white officers from the first regression as the denominator, black officers stop 5 percent fewer, Hispanic officers 2 percent more, and Asian officers 4 percent more persons on average.

In table 1.5, the number of black stops in a shift is regressed on officer race indicators. With full controls, the estimates are -0.07 (0.02) for black, -0.01 (0.01) for Hispanic, and -0.08 (0.02) for Asian officers. In percentage terms, black and Asian officers stop 5 and 6 percent fewer blacks compared to white officers. Hispanic officers stop about the same.

1.4.2 Racial composition of stops

Table 1.6 uses the race of the suspect as the dependent variable to measure differences in the racial composition of stops. The baseline specification is

$$Y_{ik} = \alpha_c + \pi_B B_k + \pi_H H_i + \pi_A A_i + u_{ij}, \tag{1.2}$$

where Y_{ik} is equal to one if stop i by officer k involves a black suspect and zero otherwise, and α_c is the sum of indicators for the officer's assignment defined similar to μ_c above. The officer-specific π's identify differences in the racial composition of stops (Δp^* from the model). Column 2 of table 1.6 includes the full set of assignment controls. It suggests that as a fraction of total stops, black officers stop 0.004 (0.005) more black suspects, Hispanic officers 0.018 (0.005) fewer, and Asian officers 0.022 (0.006) fewer.

The matching estimates in table 1.7 are similar. Intuitively, the matching estimator matches each stop to the 'closest' M stops conducted by the opposite officer race to compute the unit difference in the racial composition of stops for each observation. The unit difference is then averaged over all stops. The point estimates and standard errors produced here directly follow the formulas given by Abadie and Imbens (2006).

For the black-white officer difference, the sample is first restricted to stops conducted by black and white officers. The unit difference for stop i is

$$\widehat{\Delta p^*}_M(i) = \begin{cases} \overline{Y}_j - Y_i & \text{if } B_i = 0 \\ Y_i - \overline{Y}_j & \text{if } B_i = 1, \end{cases}$$

where

$$\overline{Y}_j = \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} Y_j$$

and $\mathcal{J}_M(i)$ is the set of indices for the M closest matches for observation i. The matching estimator is the average of the unit differences:

$$\widehat{\Delta p^*}_M = \frac{1}{N} \sum_{i=1}^{N} \widehat{\Delta p^*}_M(i), \tag{1.3}$$

where N is the total number of stops by black and white officers. The estimator matches on date, time, and geographic coordinates. Matches are also restricted to be on the same part of the week (weekend or weekday).

From column 1 of table 1.7, the nearest-neighbor estimate of the black-white difference is 0.006 (0.005), nearly identical to the regression estimate. The nearest-neighbor estimates for the Hispanic-white and Asian-white differences are -0.008 (0.004) and -0.012 (0.005), about half the magnitude of the regression estimates. The matching estimates, however, have a slightly different interpretation. They are the differences after controlling for exact time and location, rather than 8-hour time interval and census block group. This suggests that the Asian and Hispanic differences may be partially explained by differences in the patrol location decision conditional on assignment. It is consistent with Hispanic and Asian officers spending more time patrolling areas where all officers stop proportionally more blacks and less time where all officers stop proportionally fewer blacks.

The estimates are also similar when the unit differences are averaged over only the stops conducted by minority officers, or those conducted by white officers. These estimates in columns 4 and 7 are analogous to the average treatment effect on the treated (ATT) and average treatment effect on the controls (ATC) of the treatment effect literature. Denoting N_0 the number of white officer stops and N_1 the number of black officer stops, the ATT estimator for the black-white officer difference is

$$\widehat{\Delta p^*}_{M,t} = \frac{1}{N_1} \sum_{i:B_i=1}^{N} \widehat{\Delta p^*}_M(i),$$

and the ATC estimator is

$$\widehat{\Delta p^*}_{M,c} = \frac{1}{N_0} \sum_{i:B_i=0}^{N} \widehat{\Delta p^*}_M(i).$$

The ATC has the advantage of producing estimates that are directly comparable across each officer race since the unit differences average over the same joint distribution of the matching variables, specifically, the distribution of date, time, and location conditional on a white officer conducting the stop. The ATT estimates, on the other hand, are in theory the most precise and least biased. The bias is relatively small because the ATT estimator only matches minority to white officer stops, and not vice versa. This results in higher quality matches because white officer stops are the most numerous. For the same reason, the variance is relatively small because the variance estimate includes a squared term on the number of times each eligible observation is used as a match. In practice, the differences between each estimator are small. Figure 1.15 shows how match use changes with each type of estimator.

Finally, increasing the number of matches does not significantly affect the estimates, though there is a clear bias-variance tradeoff. Figure 1.16 shows how the match quality decreases when the number of matches increases from 1 to 4 to 16. The median distance between matches for the ATT estimate of the black-white difference increases from 189 to 304 to 451 feet. The median difference in time increases from 27 to 37 to 55 minutes.

1.4.3 Heterogeneity across neighborhood

The next set of estimates show how differences between officer races vary with the characteristics of the neighborhood. These regressions interact the officer race indicators with characteristics of the neighborhood:

$$\begin{aligned} m_{kt} = \mu_c &+ \beta_B B_k + \beta_H H_k + \beta_A A_k \\ &+ \gamma_B (B_k \cdot X_{kt}) + \gamma_H (H_k \cdot X_{kt}) + \gamma_A (A_k \cdot X_{kt}) + \upsilon_{kr}, \end{aligned} \tag{1.4}$$

where X_{kt} is the fraction black of the neighborhood in which officer k conducted a stop on day t, measured at the block group level. If the officer conducted stops in more than one block group on day t, X_{kt} is the simple average over those block groups. X_{kt} has been rescaled to range from zero to one to ease interpretation.[10] The β_B coefficient measures the difference in policing intensity between black and white officers in the least black neighborhoods, while $\beta_B + \gamma_B$ measures the difference in the most black neighborhoods.

Column 3 of table 1.5 lists the results when the dependent variable is the number of black stops. The coefficient on black officer is 0.023 (0.028) and the interaction with neighborhood fraction black is -0.241 (0.054). Interpreted literally, black officers stop 0.023 more blacks in the least black neighborhoods, and 0.218 fewer in the most black neighborhoods compared to white officers. In percentage terms, this is roughly 2.5 percent more in white neighborhoods, and 14 percent fewer in black neighborhoods, though the former is not statistically significant.[11] There is a similar though less pronounced trend for Asian officers, and no sta-

[10]Specifically, if $\widetilde{X}$ is the unscaled share, with minimum and maximum values $\underline{X}$ and $\overline{X}$, then $X = (\widetilde{X} - \underline{X})/(\overline{X} - \underline{X})$.

[11]The denominators for these figures are the average number of black stops by white officers in white and black neighborhoods (1.51 and 0.91, respectively). Table 1.23 reports all neighborhood, officer, and suspect

tistically significant differential for Hispanic officers. However, when the dependent variable is the number of Hispanic stops and X_{kt} is the fraction of the neighborhood that is Hispanic (table 1.9), the Hispanic officer coefficient is 0.056 (0.014) and its interaction with fraction of neighborhood that is Hispanic is -0.070 (0.028). In percentage terms, Hispanic officers stop 10 percent more Hispanics in the least Hispanic neighborhoods, and 3 percent fewer in the most Hispanic neighborhoods.

There is a similar pattern in the racial composition of stops. Table 1.6 lists the regression results, but the pattern is easier to see in figure 1.17, which computes the matching estimator separately for each beat. In the top panel, the x-axis is the fraction of the beat that is black, while the y-axis is the black-white officer difference in the fraction of stops that are black. Each circle represents one of the 35 beats and is proportional to the square root of the sample size. The dotted line is the weighted linear fit. There is a clear downward trend for the black-white officer difference. Black officers stop proportionally more blacks in white neighborhoods, and proportionally fewer in black neighborhoods. Hispanic and Asian officers both stop proportionally fewer blacks in all neighborhoods. Their trends across neighborhood demographics are slight and driven by a few outliers. The regression estimates in table 1.6 are similar.

1.4.4 Heterogeneity across officer experience

Officer experience ranges from zero to 40 years, but younger officers are much more likely to be on regular patrol. Officers with less than 4 years of experience constitute 24 percent of OPD but conduct 43 percent of all stops (table 1.1).

The next set of regressions include indicators for experience levels as well as interactions of these indicators with each officer race:

$$\begin{aligned} m_{kt} = \mu_c &+ \beta_B B_k + \beta_H H_k + \beta_A A_k + \psi_{1W} E^1_{kt} + \psi_{2W} E^2_{kt} \\ &+ \phi_{1B}(B_k \cdot E^1_{kt}) + \phi_{1H}(H_k \cdot E^1_{kt}) + \phi_{1A}(A_k \cdot E^1_{kt}) \\ &+ \phi_{2B}(B_k \cdot E^2_{kt}) + \phi_{2H}(H_k \cdot E^2_{kt}) + \phi_{2A}(A_k \cdot E^2_{kt}) + v_{kr}, \end{aligned} \tag{1.5}$$

where E^1 is an indicator for 4–11 years of experience (middle), and E^2 is an indicator for over 11 years of experience (high). The coefficients ψ_1 and ψ_2 measure the additional number of stops conducted by middle and high experience officers compared to their low experience peers. The ϕ coefficients will be significant if the experience differentials depend on officer race.

Column 4 of table 1.4 lists the results for the experience effect. Middle experienced white officers stop 0.527 (0.081) more persons per shift, while high experienced stop 0.014 (0.025) fewer — nearly the same as low experienced white officers. The coefficient on the

race-specific means.

indicator for a black officer is 0.000 (0.018) suggesting that low experienced black and white officers police with the same average intensity. However, the estimates of ϕ_{1B} and ϕ_{2B} are negative and significant, suggesting that more experienced black officers stop fewer persons compared to their similarly experienced white officer peers. The coefficients on the black officer $\times$ experience interactions are -0.253 (0.064) for middle experience, and -0.079 (0.039) for high experience. The same pattern holds for policing black suspects. When the dependent variable is the number of black stops, the estimates are -0.156 (0.038) for middle experience black officers and -0.074 (0.026) for high experience black officers (table 1.5).

The triple interaction among black officers, experience, and fraction black of neighborhood is also negative and significant when the dependent variable is the number of black stops (results not shown). This is consistent with black officers in particular learning to less intensely police blacks in black neighborhoods (Altonji and Pierret (2001)). However, similar triple interactions with other officer races and dependent variables (total stops, total white stops, and total hispanic stops) are generally mixed in sign and imprecise.

Finally, the main results on experience and neighborhood effects are robust to estimating them jointly and to the addition of neighborhood median income (columns 4–7 of tables 1.4–1.6). They also hold over the different samples discussed in the data section and table 1.1, and for each geographic unit of control: beat, census tract, and census block group (tables 1.16–1.22).

1.4.5 Signal noise and policing intensity, simulation

Section 1.2.2 explored how noise in the suspicion signal observed by the officer can result in higher policing intensity. Intuitively, the additional noise pushes out the right tail of the signal, increasing the likelihood of observing signals above the threshold.

Figure 1.14 shows how this affects the estimates of β, the difference between officers' policing intensity, using the parametric example offered in section 1.2.2. In this example, officer A observes the suspicion signal, while B observes the signal with additive white noise. Both officers choose the same signal cutoff, above which they effect a stop. The suspicion signal's distribution is the standard normal, and the white noise is also normal with mean zero and variance σ_ϵ^2. The y-axis of figure 1.14 is the percent additional stops made by officer B. The x-axis is the variance in the white noise (σ_ϵ^2).[12]

Additional signal noise increases policing intensity, though the degree depends on the particular threshold (z) that the officers choose. The cutoff is indirectly expressed by α, where $\alpha = 1 - \Phi(z)$ (that is, the percent of all observed civilians stopped by officer A). Figure 1.14 graphs the σ_ϵ^2-β relation for $\alpha = 0.3$, 0.1, and 0.01. In this case, additional

[12] From section 1.2.2, $\beta = (1 - H(z) - \alpha)/\alpha$, where H is the normal c.d.f. with mean zero and variance $1 + \sigma_\epsilon^2$. $H(z)$ is simulated with 10 million draws for each $\alpha \in \{.01, .1, .3\}$ and 20 evenly spaced values of σ_ϵ^2 between 0 and 0.1. This process was repeated 100 times, and the median values of β are presented. The 5th and 95th percentile values exhibit negligible differences.

white noise has a relatively small effect when officers stop relatively fewer people. The exact relation between α and the degree to which noise increases policing intensity will depend on the distributions of signal and noise (see section 1.2.2). In any case, this example demonstrates how signal noise can account for the differences observed in the data. It is a particularly compelling explanation for the relatively low policing intensity observed for officers policing neighborhoods of their own race, and more generally for officers with more years of policing experience.

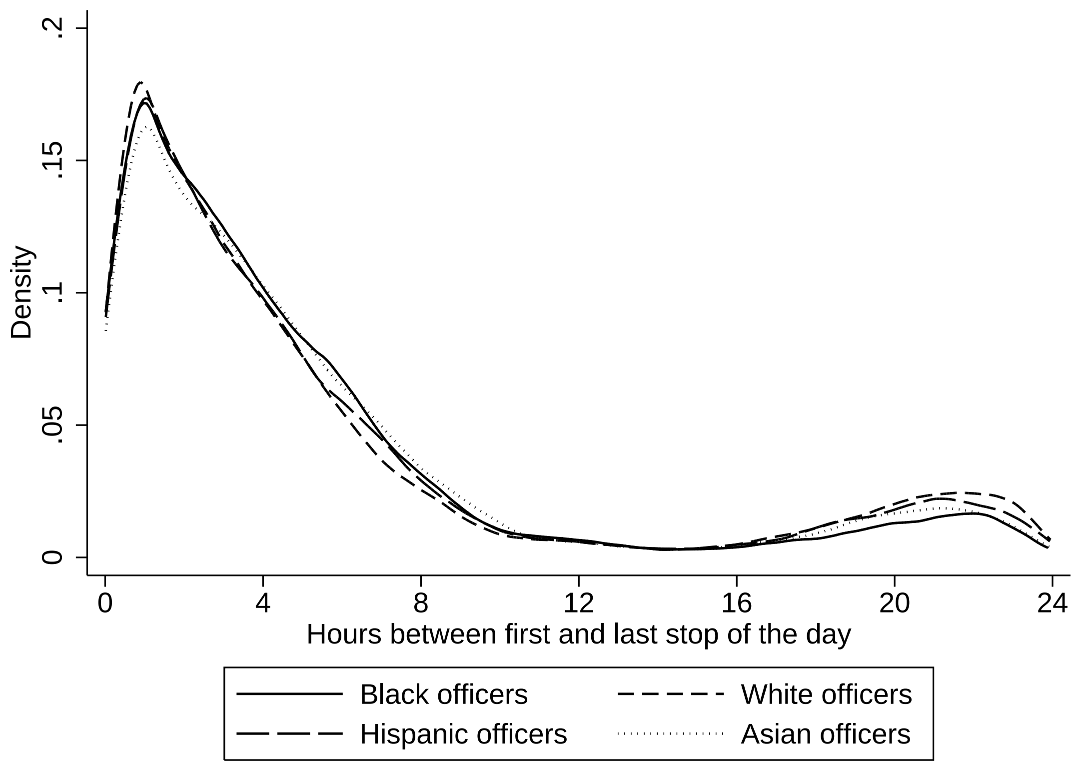

Figure 1.1: Distribution of the time difference between the first and last stop of the day (a rough measure of shift length) conditional on effecting more than 1 stop, by officer race.

Figure 1.2: Oakland block groups and reference points

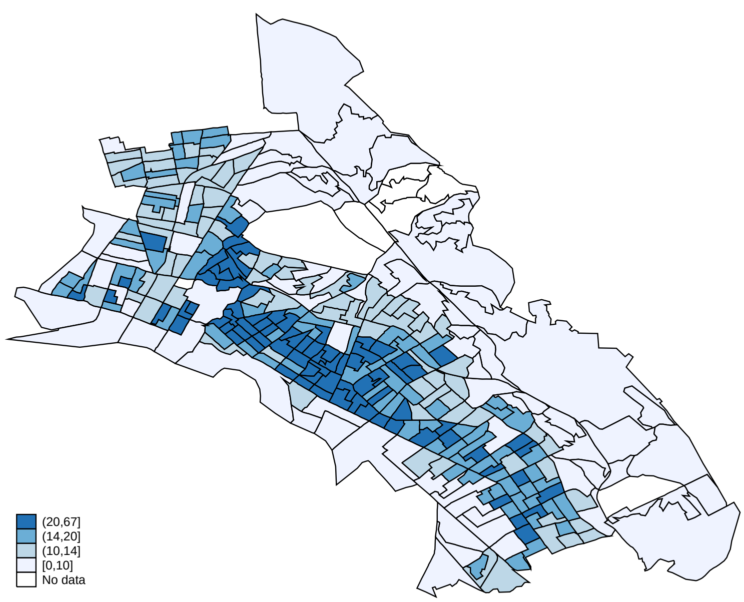

Figure 1.3: Population density (1000s per square mile), Oakland

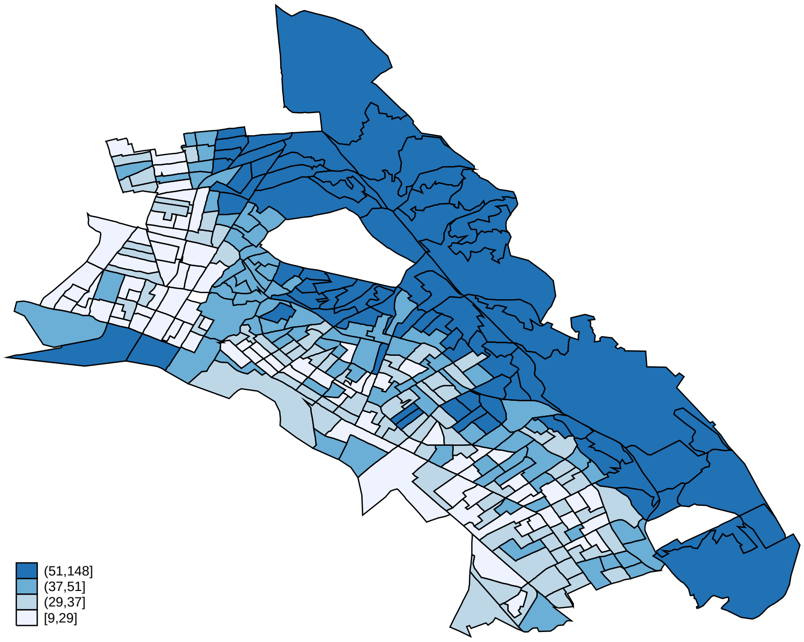

Figure 1.4: Median income (1000s of dollars), Oakland

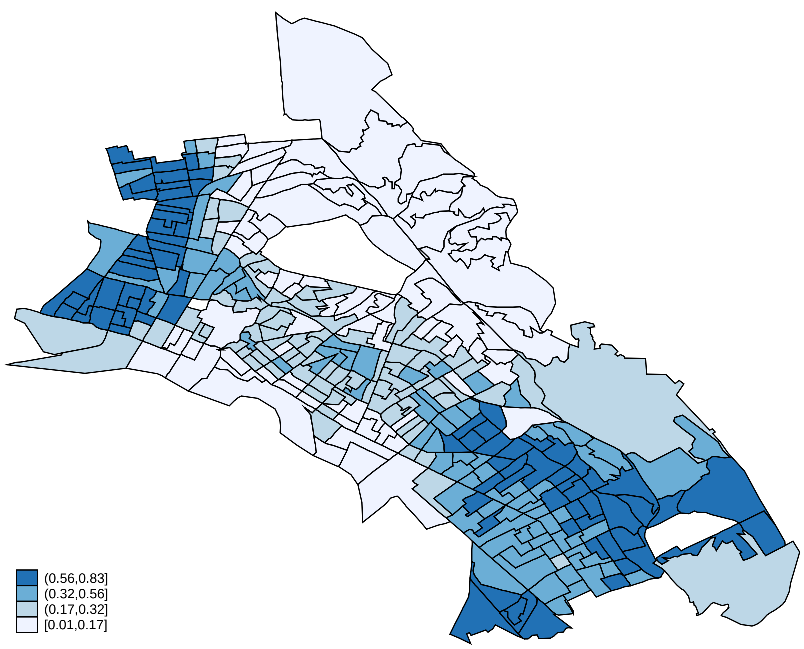

Figure 1.5: Fraction of population that is black, Oakland

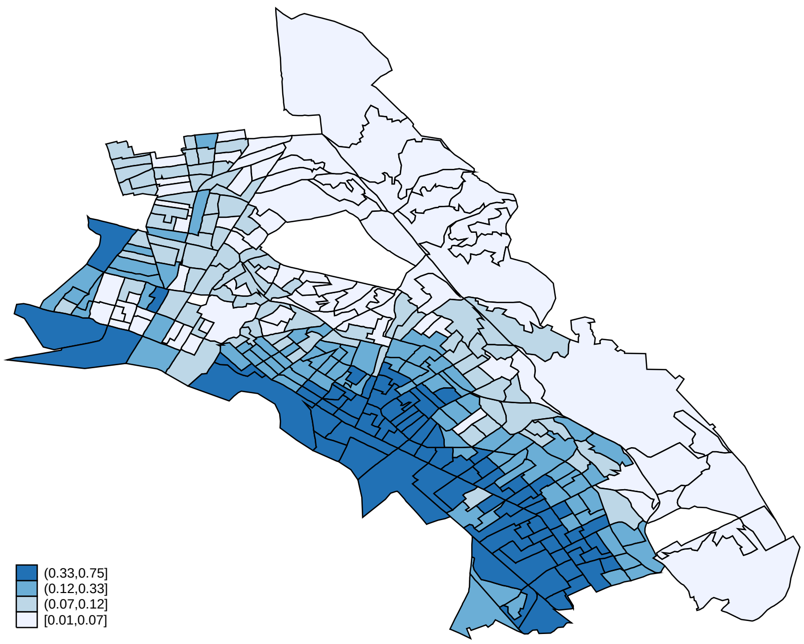

Figure 1.6: Fraction of population that is Hispanic, Oakland

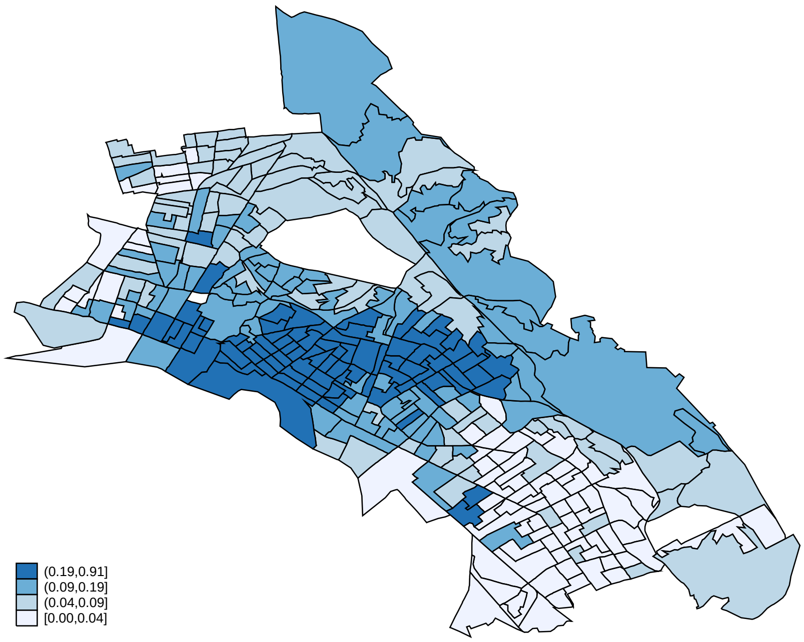

Figure 1.7: Fraction of population that is Asian, Oakland

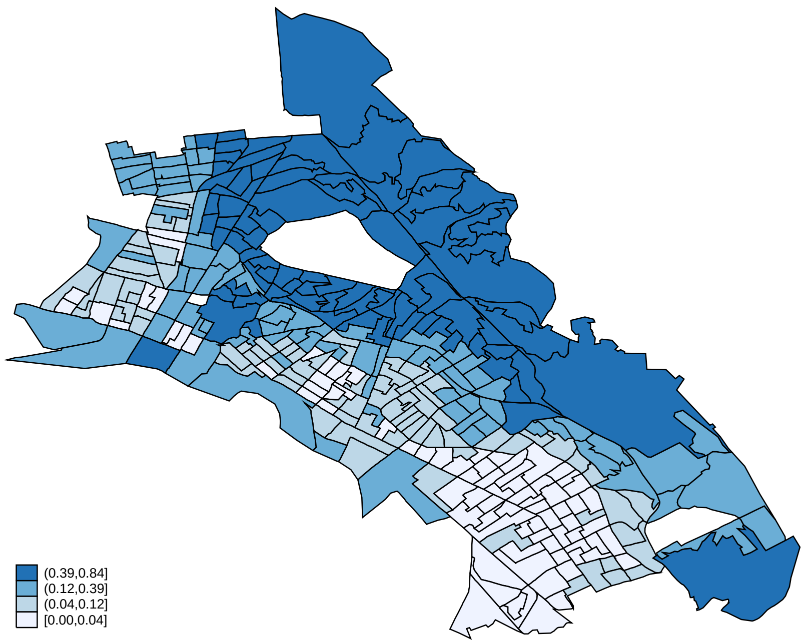

Figure 1.8: Fraction of population that is white, Oakland

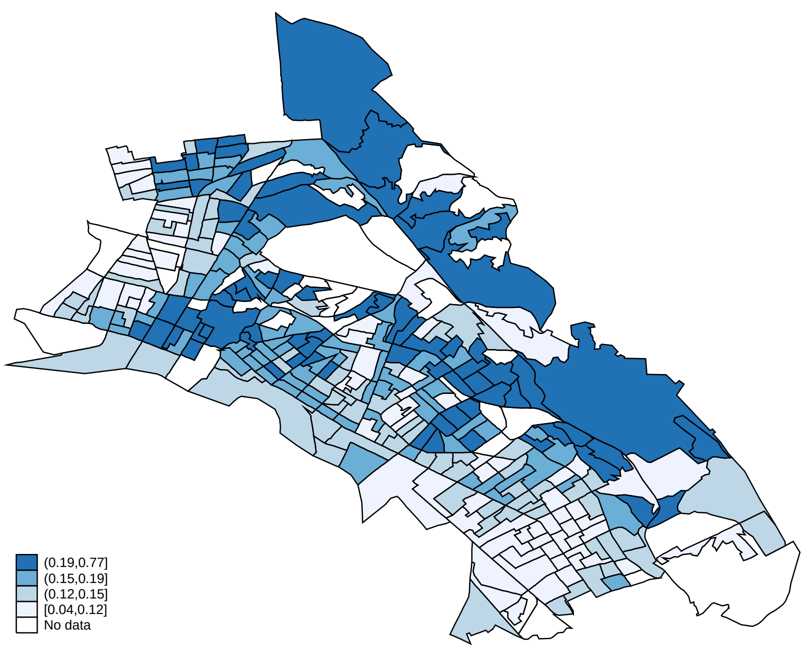

Figure 1.9: Fraction of stops conducted by Black officers, Oakland

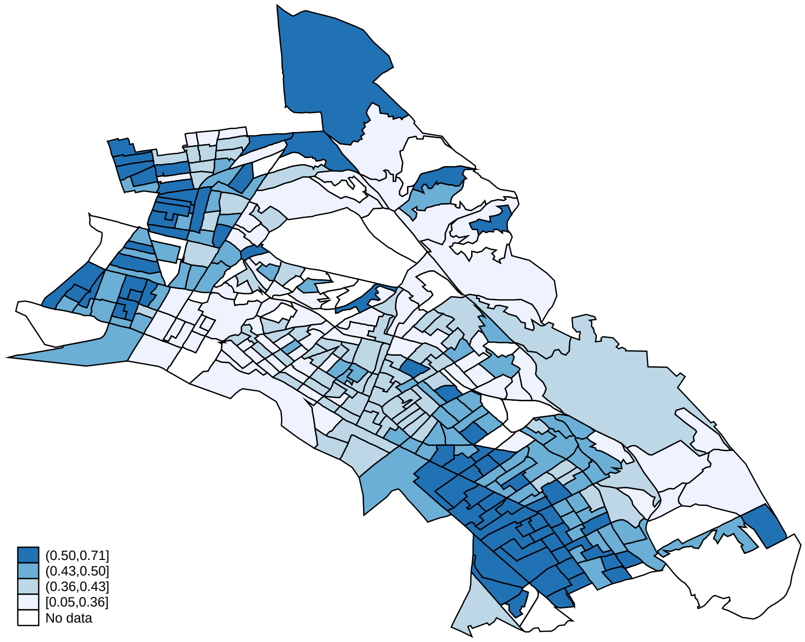

Figure 1.10: Fraction of stops conducted by White officers, Oakland

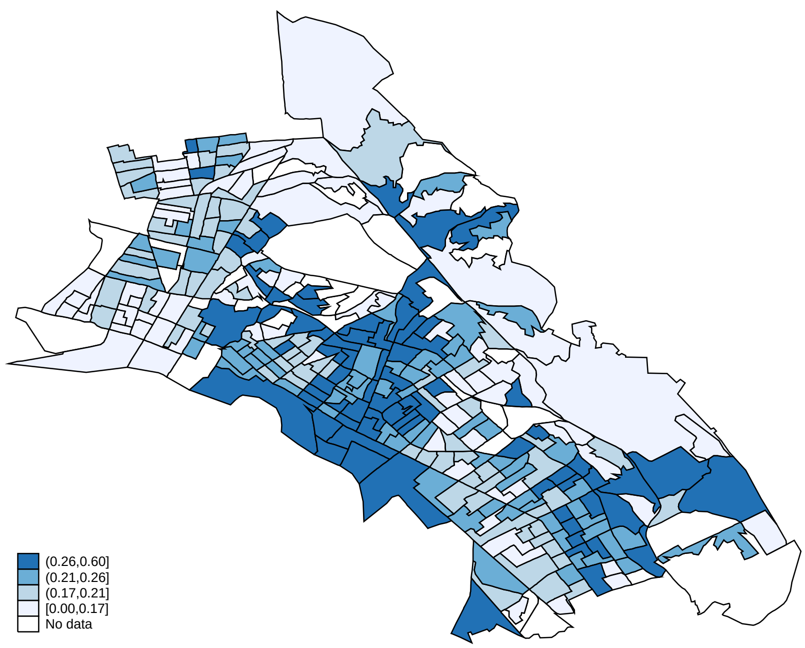

Figure 1.11: Fraction of stops conducted by Hispanic officers, Oakland

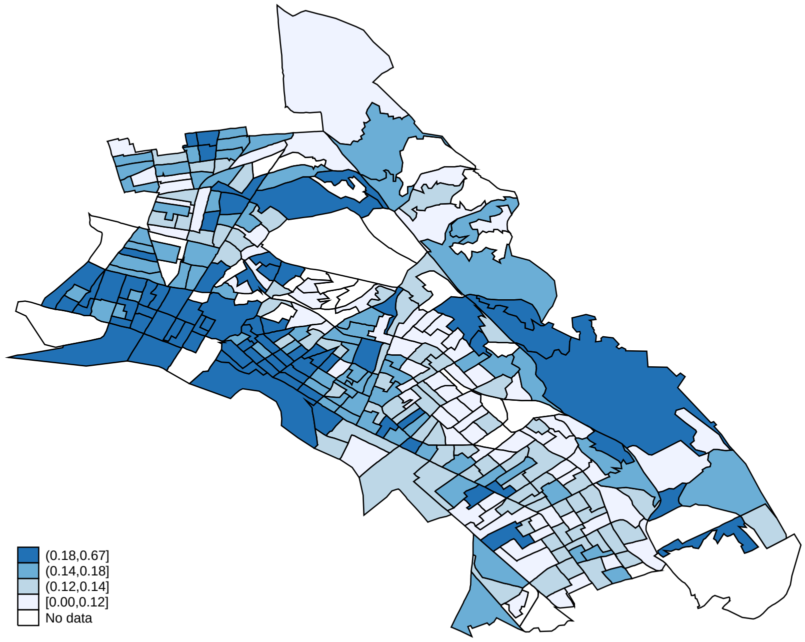

Figure 1.12: Fraction of stops conducted by Asian officers, Oakland

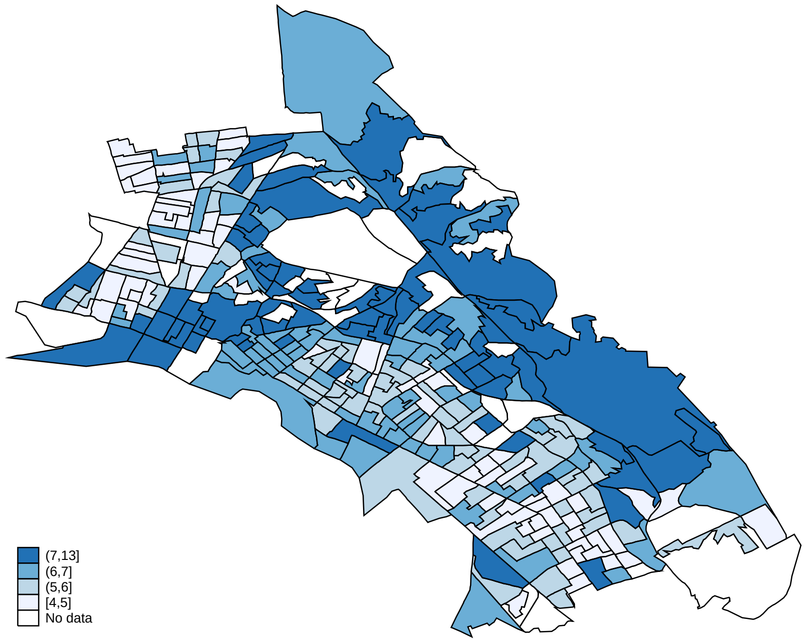

Figure 1.13: Average experience of officer conducting stop (years), Oakland

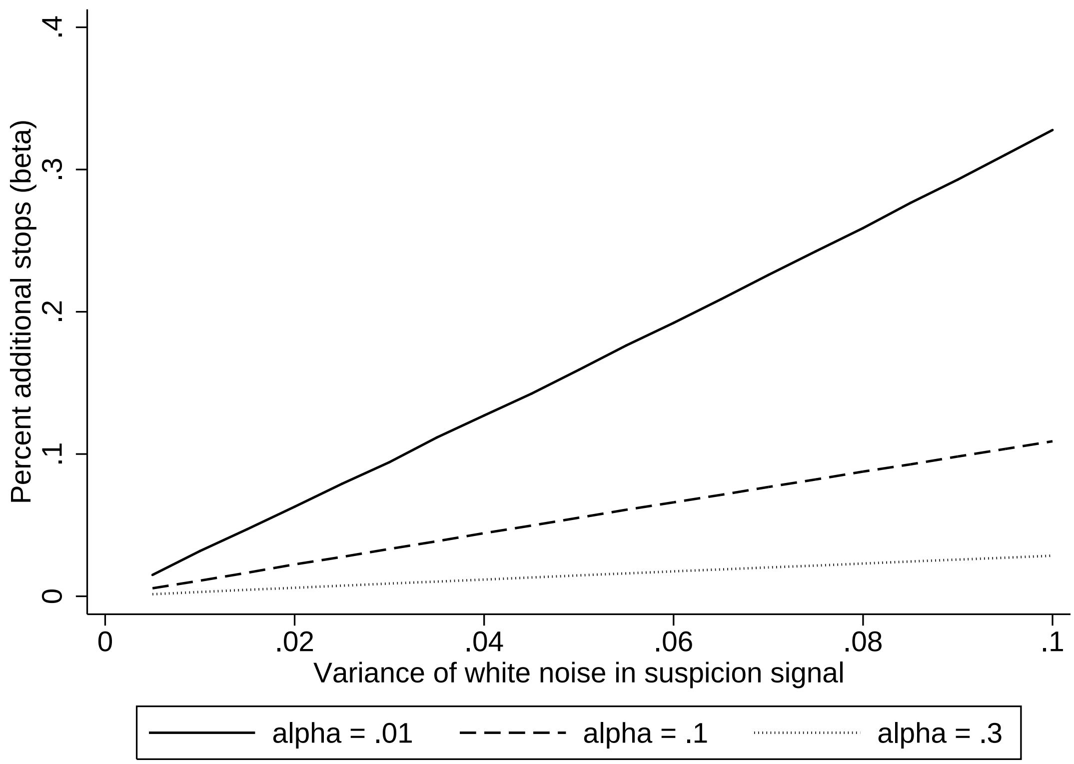

Figure 1.14: This figure shows how this effects the estimates of β, the difference between officers' policing intensity, using the parametric example offered in section 1.2.2. In this example, officer A observes the suspicion signal, while B observes the signal with additive white noise. Both officers choose the same signal cutoff, z, above which they effect a stop. The suspicion signal's distribution is the standard normal, and the white noise is also normal with mean zero and variance σ_ϵ^2. The y-axis is the percent additional stops made by officer B. The x-axis is the variance in the white noise (σ_ϵ^2). $\alpha = 1 - \Phi(z)$ is the percent of observed civilians stopped by officer A. From section 1.2.2, $\beta = (1 - H(z) - \alpha)/\alpha$, where H is the normal c.d.f. with mean zero and variance $1 + \sigma_\epsilon^2$. $H(z)$ is simulated with 10 million draws for each $\alpha \in \{.01, .1, .3\}$ and 20 evenly spaced values of σ_ϵ^2 between 0 and 0.1. This process was repeated 100 times, and the median values of β are presented. The 5th and 95th percentile values exhibit negligible differences.

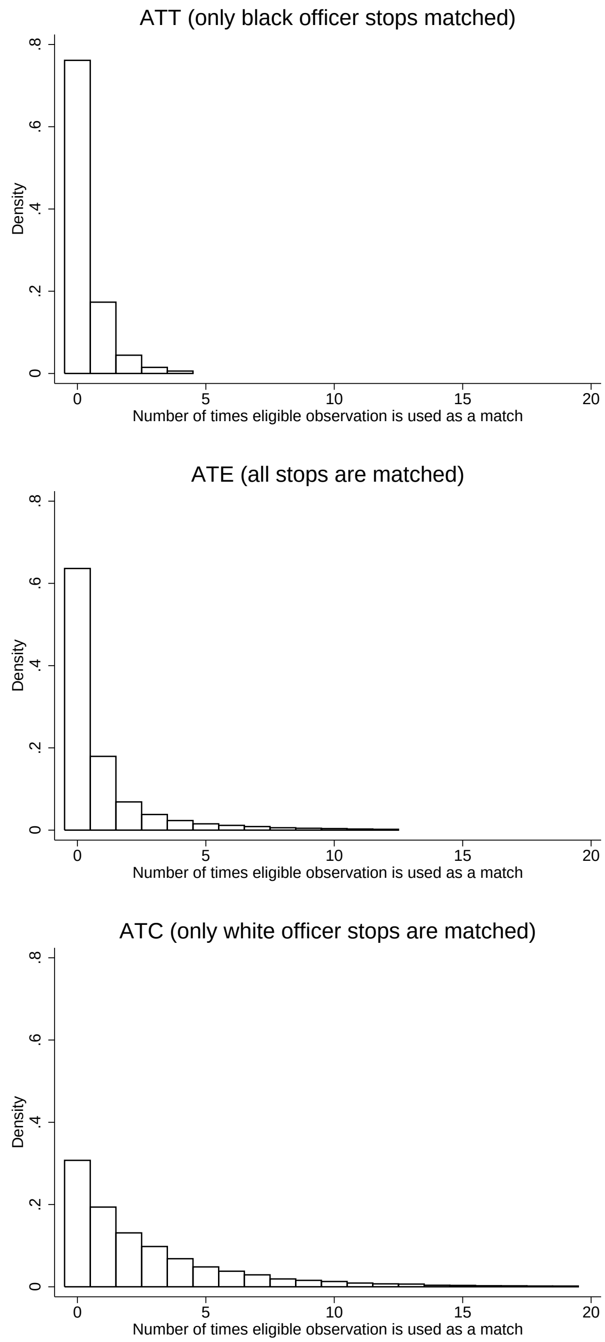

Figure 1.15: Match usage for black/white officer difference in percent of stops that are black when $M = 1$, by estimator type. For the estimates, see table 1.7, row 1, columns 1, 4, and 7.

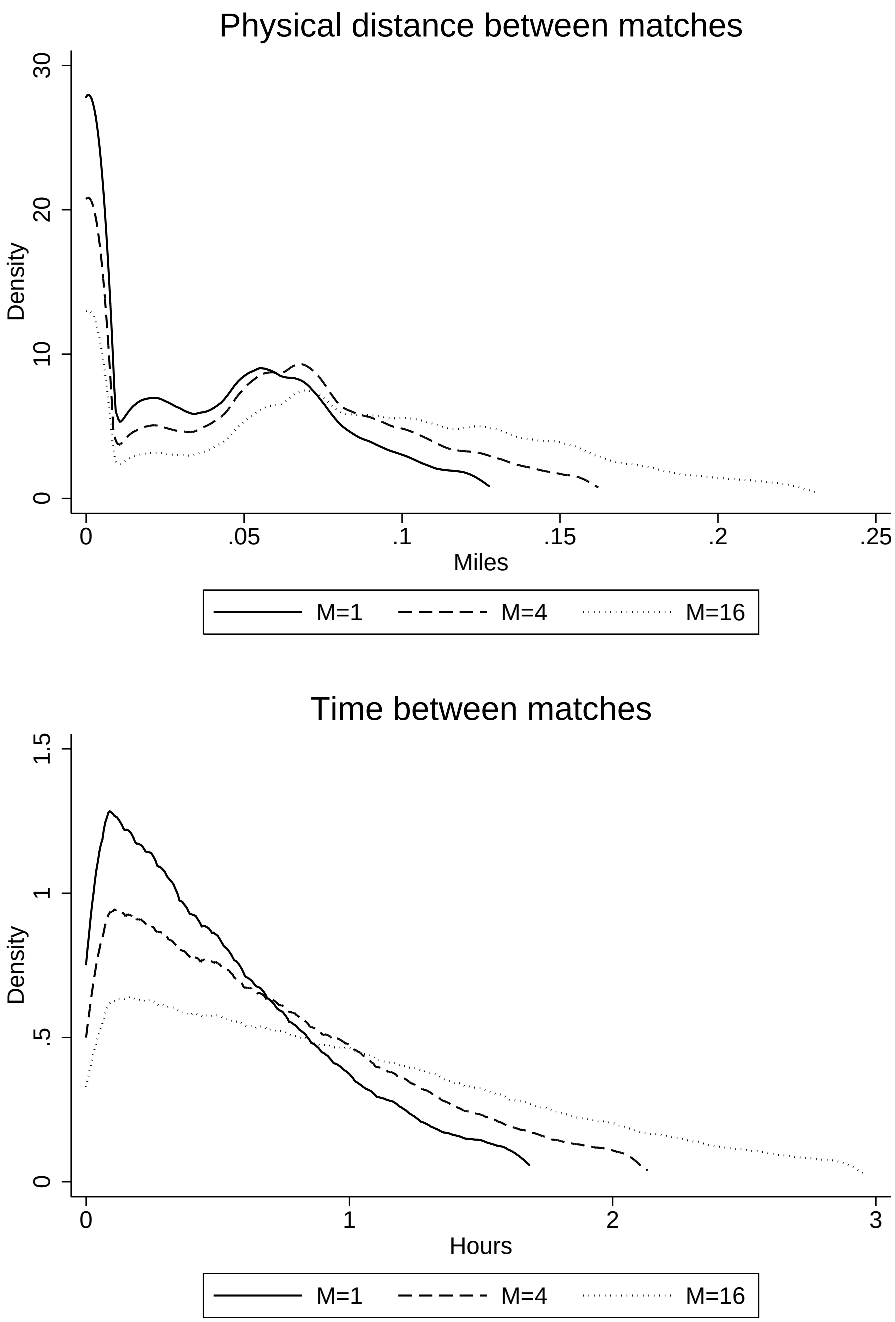

Figure 1.16: Quality of matches for ATT matching estimate of black/white officer difference in percent of stops that are black as M increase from 1 to 4 to 16. For the estimates, see table 1.7, row 1, columns 1–3)

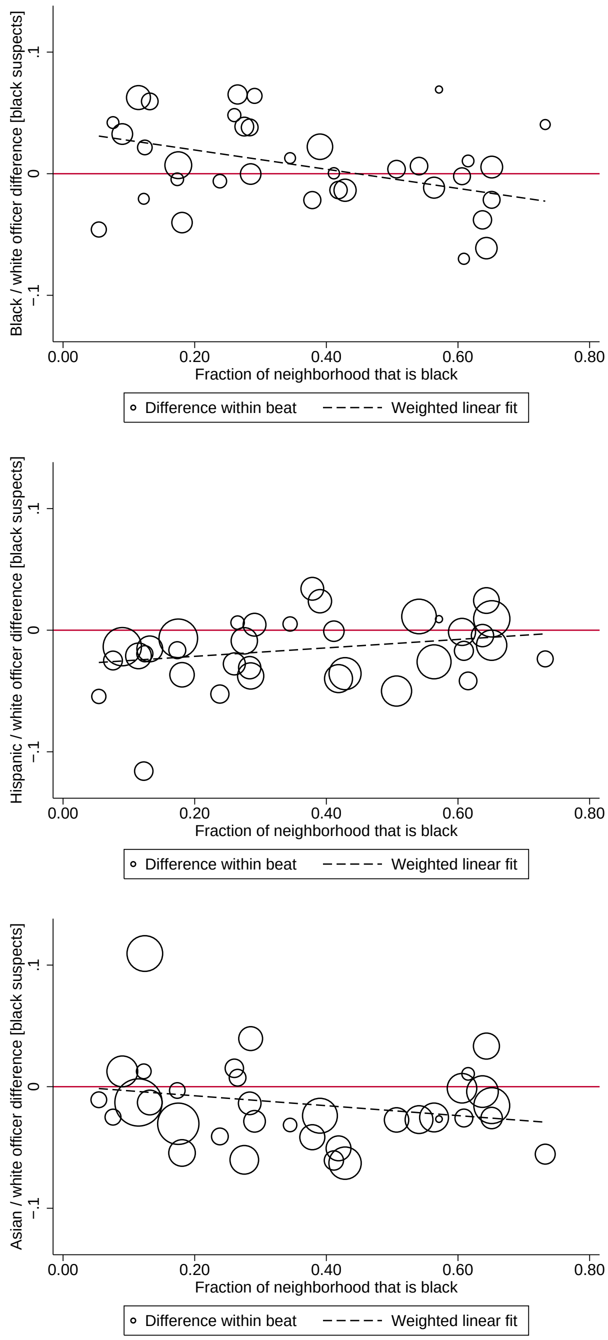

Figure 1.17: Decomposition of the ATT matching estimate of minority / white officer difference in percent of stops that are black over the demographic composition of the neighborhood in which the stop occurred. Each circle represents a beat (the geographic level of an officer's assignment) with circle size proportional to $\sqrt{N}$. For the estimates, see table 1.7, column 4, rows 1, 5, and 9 ($M = 16$).

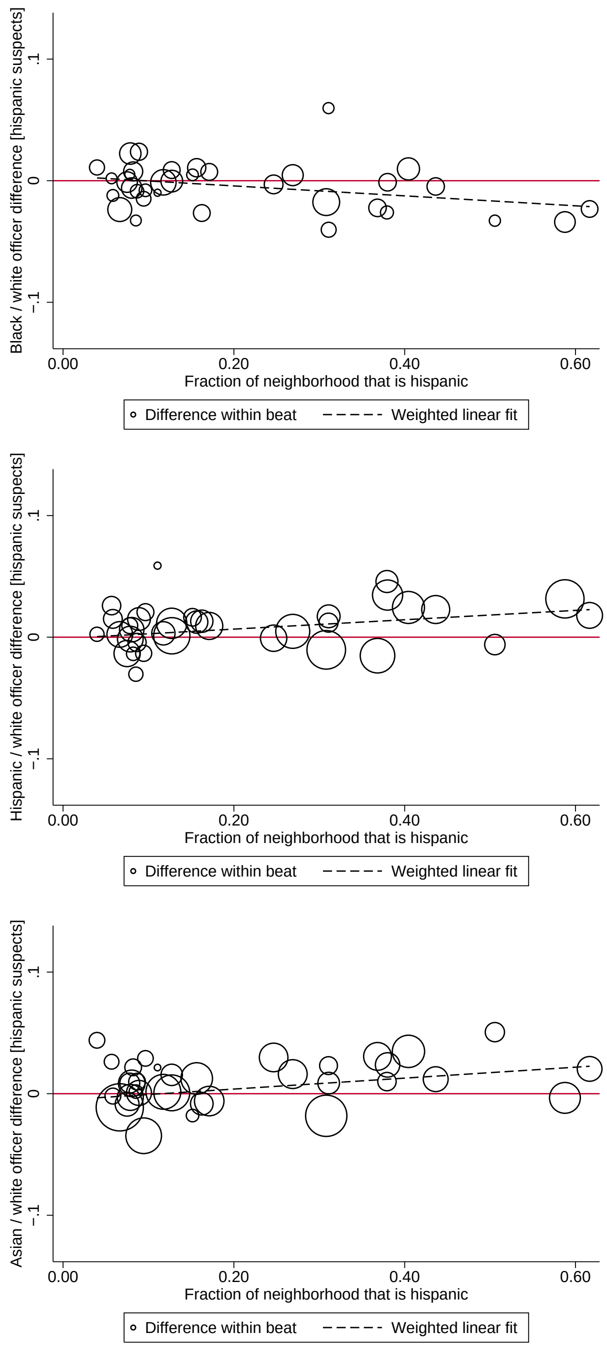

Figure 1.18: Decomposition of the ATT matching estimate of minority / white officer difference in percent of stops that are hispanic over the demographic composition of the neighborhood in which the stop occurred. Each circle represents a beat (the geographic level of an officer's assignment) with circle size proportional to $\sqrt{N}$. For the estimates, see table 1.15, column 4, rows 1, 5, and 9 ($M = 16$).

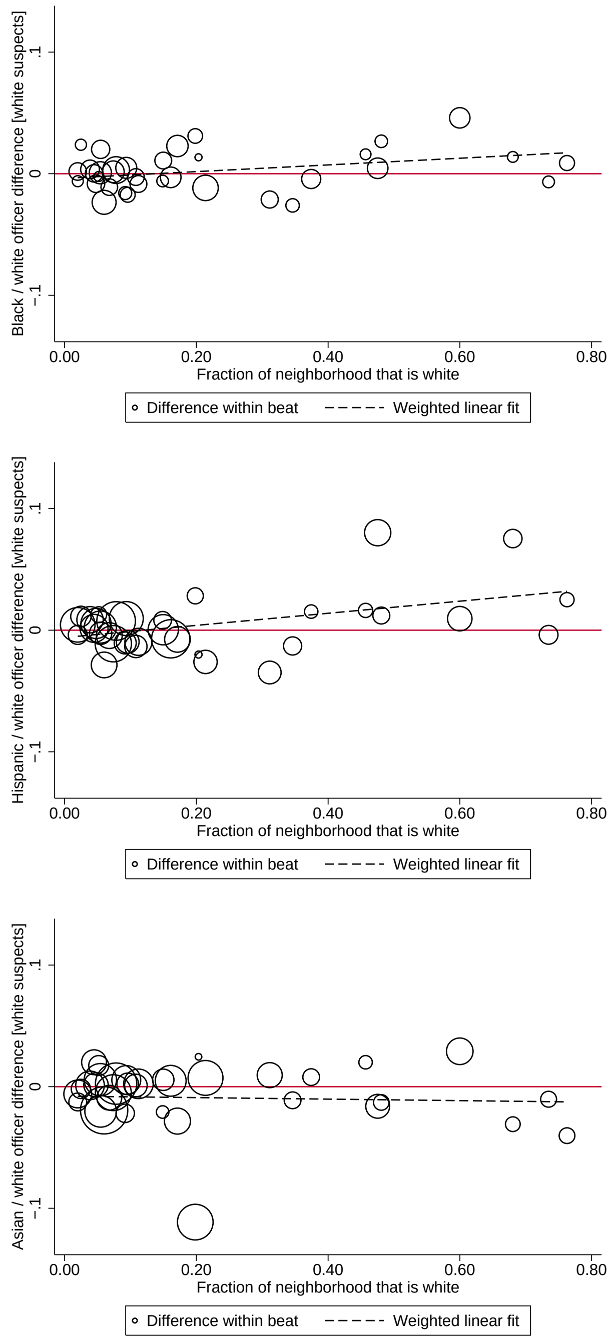

Figure 1.19: Decomposition of the ATT matching estimate of minority / white officer difference in percent of stops that are white over the demographic composition of the neighborhood in which the stop occurred. Each circle represents a beat (the geographic level of an officer's assignment) with circle size proportional to $\sqrt{N}$. For the estimates, see table 1.14, column 4, rows 1, 5, and 9 ($M = 16$).

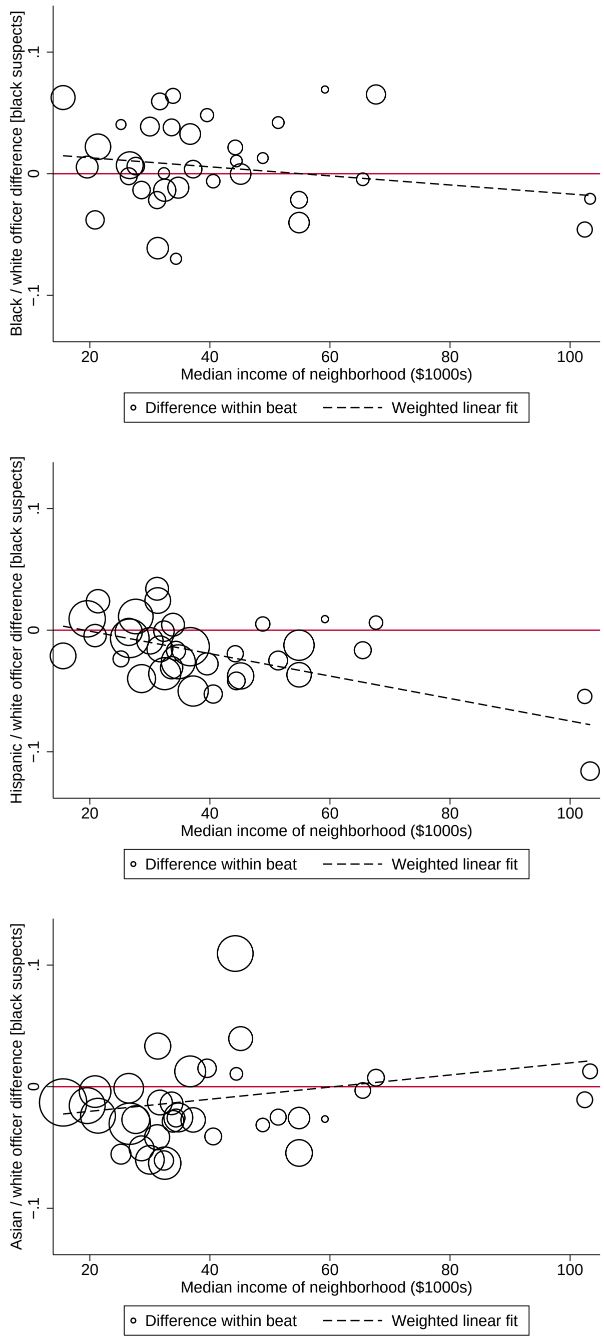

Figure 1.20: Decomposition of the ATT matching estimate of minority / white officer difference in percent of stops that are black over the median income of the neighborhood in which the stop occurred. Each circle represents a beat (the geographic level of an officer's assignment) with circle size proportional to $\sqrt{N}$. For the estimates, see table 1.7, column 4, rows 1, 5, and 9 $(M = 16)$.

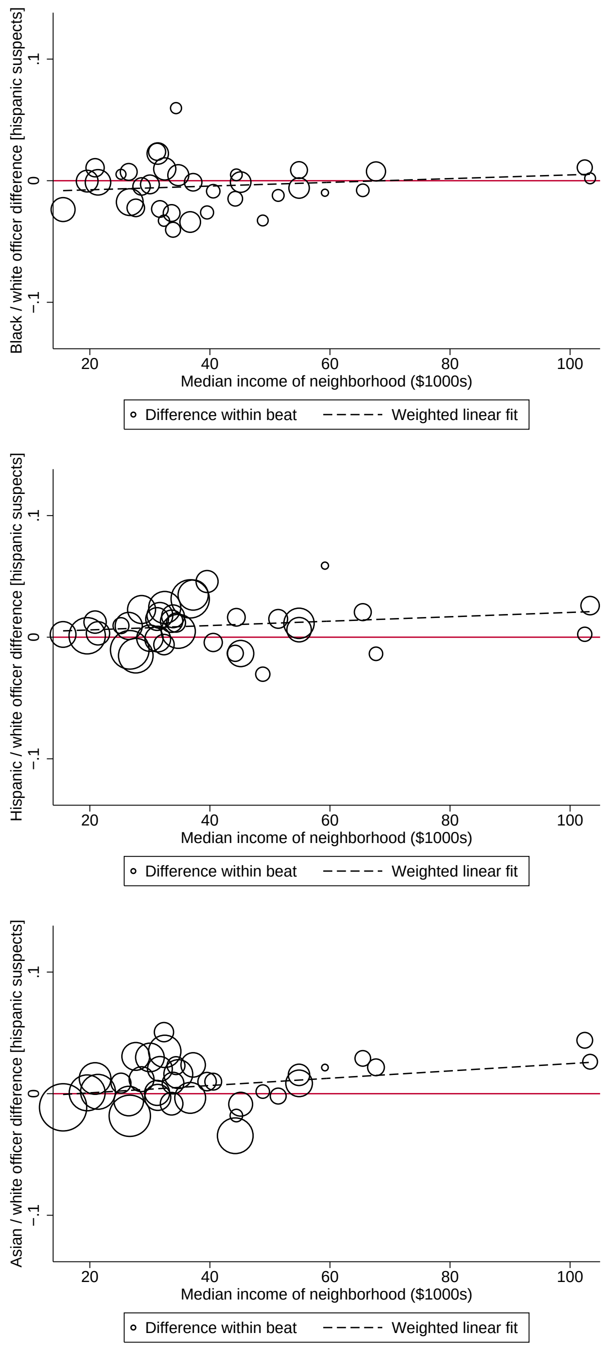

Figure 1.21: Decomposition of the ATT matching estimate of minority / white officer difference in percent of stops that are hispanic over the median income of the neighborhood in which the stop occurred. Each circle represents a beat (the geographic level of an officer's assignment) with circle size proportional to $\sqrt{N}$. For the estimates, see table 1.15, column 4, rows 1, 5, and 9 ($M = 16$).

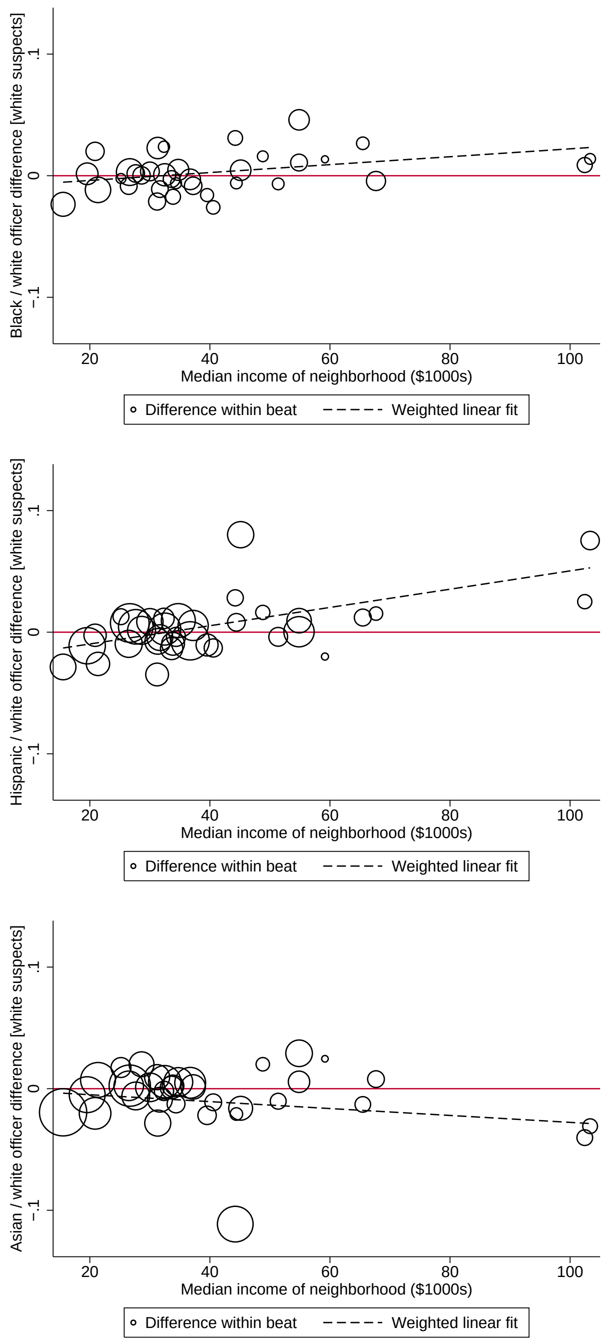

Figure 1.22: Decomposition of the ATT matching estimate of minority / white officer difference in percent of stops that are white over the median income of the neighborhood in which the stop occurred. Each circle represents a beat (the geographic level of an officer's assignment) with circle size proportional to $\sqrt{N}$. For the estimates, see table 1.14, column 4, rows 1, 5, and 9 ($M = 16$).

Table 1.1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)
Suspect Race					
black	0.351	0.611	0.610	0.626	0.632
white	0.235	0.132	0.132	0.123	0.121
hispanic	0.219	0.156	0.154	0.152	0.150
asian	0.151	0.055	0.056	0.054	0.054
Officer Race					
black	0.200	0.148	0.149	0.149	0.151
white	0.447	0.427	0.424	0.427	0.425
hispanic	0.189	0.236	0.236	0.232	0.228
asian	0.133	0.162	0.163	0.164	0.167
Experience					
0–3 years	0.240	0.434	0.435	0.444	0.458
4–10 years	0.258	0.398	0.399	0.386	0.372
11+ years	0.503	0.167	0.167	0.170	0.170
Time					
0:00–8:00		0.172	0.169	0.174	0.176
8:01–16:00		0.328	0.332	0.323	0.319
16:01–23:59		0.500	0.499	0.503	0.505
Data	Personnel | Census	Stop	Stop	Stop	Stop
Sample		Full	Officer	Beat	Geo
Years	2010 | 2000	2005–2010	2005–2010	2005–2010	2005–2010
Observations	776 | 399,477	281,248	244,333	210,474	191,177

Notes: In column 1, suspect race is the composition of each race in Oakland from the 2000 Census. Officer race and experience come from Oakland Police Department 2010 personnel records. Observations is the total number of officers | total population of Oakland. Columns 2–5 list the racial composition of stops (suspect race) and the composition of the police force conducting these stops across different samples. Full is the complete sample. Officer is the sample for which officer information is available (officers at OPD as of 2010). Beat includes both officer and shift information (the beat to which an officer is assigned). Geo is the intersection of Beat and the sample for which the address of the stop was successfully geocoded to a point within Oakland, and can therefore used in the exact matching estimator as well as linked to census information at a geographic level finer than the beat (tract, block group, or block).

Table 1.2: Summary Statistics: Type of Stop

	Race of suspect			
	black	white	hispanic	asian
Severity				
felony	0.039	0.012	0.024	0.012
misdimeanor	0.072	0.045	0.057	0.029
infraction	0.136	0.112	0.119	0.109
local ordinance	0.054	0.038	0.026	0.021
probation | parole	0.028	0.006	0.007	0.004
Duration				
zero–9 min	0.388	0.566	0.417	0.533
10–19 min	0.392	0.335	0.384	0.359
20–30 min	0.106	0.053	0.107	0.065
31+ min	0.113	0.045	0.092	0.043
traffic related	0.657	0.813	0.802	0.868
arrest	0.035	0.015	0.029	0.012
search	0.394	0.131	0.308	0.121
hit rate	0.170	0.174	0.134	0.169

Notes: The distribution of stop characteristics conditional on the race of the suspect stopped. hit rate is the likelihood of finding contraband conditional on effecting a search.

Table 1.3: Search and Hit Rates

		Neighborhood Type			
Suspect Race	Officer race	Minority	White	Minority	White
		Search Rate		Hit Rate	
black					
	asian	0.339	0.135	0.163	0.087
		(0.009)	(0.007)	(0.011)	(0.016)
	black	0.316	0.229	0.151	0.096
		(0.009)	(0.010)	(0.012)	(0.014)
	hispanic	0.320	0.171	0.170	0.147
		(0.007)	(0.008)	(0.010)	(0.018)
	white	0.338	0.254	0.203	0.144
		(0.005)	(0.007)	(0.007)	(0.011)
hispanic					
	asian	0.315	0.145	0.108	0.083
		(0.009)	(0.019)	(0.011)	(0.040)
	black	0.321	0.180	0.112	0.089
		(0.010)	(0.022)	(0.012)	(0.038)
	hispanic	0.303	0.141	0.126	0.069
		(0.007)	(0.017)	(0.009)	(0.034)
	white	0.353	0.203	0.156	0.149
		(0.006)	(0.017)	(0.007)	(0.034)
white					
	asian	0.131	0.067	0.133	0.045
		(0.002)	(0.007)	(0.003)	(0.022)
	black	0.083	0.049	0.062	0.072
		(0.002)	(0.005)	(0.003)	(0.029)
	hispanic	0.145	0.045	0.309	0.065
		(0.002)	(0.005)	(0.003)	(0.026)
	white	0.115	0.093	0.182	0.163
		(0.002)	(0.006)	(0.003)	(0.023)

Notes: Probability that a stop is searched and probability that the search is successful by suspect race, officer race, and neighborhood demographics.

Table 1.4: Effect of Officer Race on Stop Rate: All Suspects

	Dependent variable is an officer's number of stops in one day						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.074	-0.107	-0.143	0.000	0.030	-0.131	0.014
	(0.038)	(0.029)	(0.050)	(0.018)	(0.045)	(0.057)	(0.073)
×{FRACTION BLACK}			0.097		-0.103		-0.110
			(0.123)		(0.107)		(0.115)
×{EXPERIENCE 4–10 YRS}				-0.253	-0.225		-0.240
				(0.064)	(0.067)		(0.068)
×{EXPERIENCE 11+ YRS}				-0.079	-0.074		-0.071
				(0.039)	(0.039)		(0.040)
×{MEDIAN INCOME}						0.155	0.144
						(0.264)	(0.266)
HISPANICOFF	0.151	0.054	0.066	-0.013	-0.023	-0.108	-0.222
	(0.031)	(0.019)	(0.031)	(0.011)	(0.043)	(0.040)	(0.060)
×{FRACTION BLACK}			-0.032		0.015		0.151
			(0.073)		(0.102)		(0.096)
×{EXPERIENCE 4–10 YRS}				-0.055	-0.018		-0.006
				(0.057)	(0.054)		(0.047)
×{EXPERIENCE 11+ YRS}				0.662	0.671		0.654
				(0.064)	(0.065)		(0.067)
×{MEDIAN INCOME}						1.016	0.921
						(0.223)	(0.228)
ASIANOFF	-0.025	-0.097	-0.118	-0.036	0.001	-0.066	0.018
	(0.036)	(0.021)	(0.038)	(0.015)	(0.043)	(0.050)	(0.065)
×{FRACTION BLACK}			0.057		-0.098		-0.075
			(0.082)		(0.103)		(0.094)
×{EXPERIENCE 4–10 YRS}				-0.410	-0.375		-0.349
				(0.063)	(0.058)		(0.051)
×{EXPERIENCE 11+ YRS}				0.324	0.321		0.308
				(0.050)	(0.047)		(0.045)
×{MEDIAN INCOME}						-0.118	-0.145
						(0.293)	(0.264)
EXPERIENCE 4–10 YRS				0.527	0.232		0.259
				(0.081)	(0.071)		(0.070)
×{FRACTION BLACK}					0.721		0.551
					(0.218)		(0.171)
EXPERIENCE 11+ YRS				-0.014	-0.150		-0.132
				(0.025)	(0.036)		(0.035)
×{FRACTION BLACK}					0.348		0.279
					(0.091)		(0.088)
_CONS	2.323						
	(0.052)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.001	0.779	0.779	0.781	0.782	0.792	0.794
OBSERVATIONS	91,770	91,770	91,733	88,586	88,550	90,225	87,078

NOTES: Standard errors clustered at the block group level. CONTROLS includes day of week indicators, year indicators, and interactions of block group with time (measured in 8-hour blocks). MEDIAN INCOME and FRACTION BLACK measured at block group level and scaled to range from 0 (lowest) to 1 (highest).

Table 1.5: Effect of Officer Race on Stop Rate: Black Suspects

	Dependent variable is an officer's number of black stops in one day						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.160	-0.068	0.023	0.005	0.125	-0.058	0.192
	(0.024)	(0.018)	(0.028)	(0.015)	(0.027)	(0.032)	(0.044)
×{FRACTION BLACK}			-0.241		-0.322		-0.383
			(0.054)		(0.059)		(0.065)
×{EXPERIENCE 4–10 YRS}				-0.156	-0.145		-0.143
				(0.038)	(0.038)		(0.039)
×{EXPERIENCE 11+ YRS}				-0.074	-0.076		-0.084
				(0.026)	(0.027)		(0.027)
×{MEDIAN INCOME}						-0.046	-0.255
						(0.120)	(0.128)
HISPANICOFF	-0.031	-0.006	-0.017	-0.027	-0.037	-0.028	-0.064
	(0.020)	(0.012)	(0.020)	(0.010)	(0.025)	(0.021)	(0.034)
×{FRACTION BLACK}			0.032		0.021		0.044
			(0.046)		(0.055)		(0.056)
×{EXPERIENCE 4–10 YRS}				-0.025	-0.002		0.002
				(0.031)	(0.031)		(0.031)
×{EXPERIENCE 11+ YRS}				0.202	0.207		0.207
				(0.034)	(0.036)		(0.037)
×{MEDIAN INCOME}						0.148	0.124
						(0.102)	(0.108)
ASIANOFF	-0.148	-0.081	-0.037	-0.028	0.066	-0.138	-0.004
	(0.026)	(0.015)	(0.029)	(0.013)	(0.028)	(0.032)	(0.046)
×{FRACTION BLACK}			-0.115		-0.232		-0.184
			(0.058)		(0.061)		(0.062)
×{EXPERIENCE 4–10 YRS}				-0.221	-0.212		-0.203
				(0.033)	(0.032)		(0.032)
×{EXPERIENCE 11+ YRS}				0.099	0.077		0.056
				(0.051)	(0.047)		(0.038)
×{MEDIAN INCOME}						0.360	0.329
						(0.186)	(0.184)
EXPERIENCE 4–10 YRS				0.220	0.064		0.071
				(0.041)	(0.042)		(0.042)
×{FRACTION BLACK}					0.385		0.335
					(0.090)		(0.082)
EXPERIENCE 11+ YRS				-0.098	-0.137		-0.137
				(0.018)	(0.026)		(0.026)
×{FRACTION BLACK}					0.106		0.117
					(0.060)		(0.059)
_CONS	1.506						
	(0.032)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.002	0.734	0.734	0.735	0.736	0.740	0.742
OBSERVATIONS	91,770	91,770	91,733	88,586	88,550	90,225	87,078

NOTES: See table 1.4.

Table 1.6: Effect of Officer Race on Stop Composition: Black Suspects

	Dependent variable is an indicator for a black stop						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.044	0.004	0.023	-0.001	0.014	0.018	0.047
	(0.009)	(0.005)	(0.010)	(0.007)	(0.012)	(0.008)	(0.014)
×{FRACTION BLACK}			-0.054		-0.040		-0.066
			(0.024)		(0.025)		(0.024)
×{EXPERIENCE 4–10 YRS}				0.029	0.027		0.030
				(0.012)	(0.012)		(0.012)
×{EXPERIENCE 11+ YRS}				-0.023	-0.024		-0.029
				(0.013)	(0.013)		(0.013)
×{MEDIAN INCOME}						-0.077	-0.138
						(0.034)	(0.032)
HISPANICOFF	-0.051	-0.018	-0.038	-0.008	-0.023	0.005	-0.001
	(0.008)	(0.005)	(0.010)	(0.005)	(0.010)	(0.008)	(0.012)
×{FRACTION BLACK}			0.051		0.036		0.019
			(0.022)		(0.021)		(0.021)
×{EXPERIENCE 4–10 YRS}				0.001	0.002		0.001
				(0.009)	(0.009)		(0.009)
×{EXPERIENCE 11+ YRS}				-0.072	-0.070		-0.070
				(0.014)	(0.014)		(0.014)
×{MEDIAN INCOME}						-0.137	-0.095
						(0.039)	(0.036)
ASIANOFF	-0.064	-0.022	-0.030	-0.012	-0.016	-0.043	-0.051
	(0.009)	(0.006)	(0.012)	(0.005)	(0.011)	(0.009)	(0.017)
×{FRACTION BLACK}			0.022		0.011		0.035
			(0.023)		(0.022)		(0.022)
×{EXPERIENCE 4–10 YRS}				0.006	0.006		0.006
				(0.010)	(0.010)		(0.011)
×{EXPERIENCE 11+ YRS}				-0.037	-0.038		-0.044
				(0.026)	(0.024)		(0.018)
×{MEDIAN INCOME}						0.125	0.161
						(0.057)	(0.066)
EXPERIENCE 4–10 YRS				-0.046	-0.037		-0.036
				(0.007)	(0.008)		(0.008)
×{FRACTION BLACK}					-0.022		-0.020
					(0.020)		(0.020)
EXPERIENCE 11+ YRS				-0.042	-0.028		-0.029
				(0.008)	(0.012)		(0.012)
×{FRACTION BLACK}					-0.037		-0.024
					(0.028)		(0.025)
_CONS	0.662						
	(0.012)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.003	0.677	0.678	0.678	0.679	0.680	0.681
OBSERVATIONS	188,526	188,526	188,415	182,952	182,842	184,721	179,226

NOTES: See table 1.4.

Table 1.7: Effect of Officer Race on Stop Composition [Matching Estimator]: Black Suspects

	Dependent variable is an indicator for a black stop								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BLACKOFF	0.006	0.007	0.008	0.006	0.005	0.007	0.006	0.007	0.008
	(0.005)	(0.004)	(0.004)	(0.006)	(0.008)	(0.013)	(0.009)	(0.015)	(0.026)
×{BLACK NEIGHBORHOODS}	-0.004	-0.005	-0.007	-0.017	-0.016	-0.014	-0.001	-0.002	-0.006
	(0.008)	(0.007)	(0.007)	(0.009)	(0.012)	(0.017)	(0.018)	(0.029)	(0.051)
×{NON-BLACK NEIGHBORHOODS}	0.033	0.035	0.036	0.031	0.031	0.032	0.034	0.037	0.038
	(0.012)	(0.011)	(0.010)	(0.017)	(0.022)	(0.032)	(0.019)	(0.029)	(0.051)
OBSERVATIONS	111,934	111,934	111,934	29,075	29,075	29,075	82,859	82,859	82,859
HISPANICOFF	-0.008	-0.008	-0.008	-0.014	-0.013	-0.015	-0.006	-0.005	-0.005
	(0.004)	(0.004)	(0.003)	(0.005)	(0.007)	(0.011)	(0.006)	(0.009)	(0.016)
×{BLACK NEIGHBORHOODS}	-0.002	0.001	-0.001	-0.012	-0.007	-0.009	0.002	0.005	0.003
	(0.006)	(0.005)	(0.005)	(0.008)	(0.011)	(0.017)	(0.010)	(0.016)	(0.027)
×{NON-BLACK NEIGHBORHOODS}	-0.009	-0.014	-0.017	-0.012	-0.023	-0.027	-0.006	-0.008	-0.010
	(0.011)	(0.010)	(0.009)	(0.015)	(0.021)	(0.031)	(0.014)	(0.021)	(0.035)
OBSERVATIONS	127,089	127,089	127,089	44,230	44,230	44,230	82,859	82,859	82,859
ASIANOFF	-0.012	-0.012	-0.013	-0.010	-0.010	-0.014	-0.013	-0.013	-0.013
	(0.005)	(0.004)	(0.004)	(0.006)	(0.008)	(0.012)	(0.008)	(0.013)	(0.023)
×{BLACK NEIGHBORHOODS}	-0.008	-0.005	-0.005	-0.017	-0.013	-0.014	-0.006	-0.003	-0.003
	(0.007)	(0.007)	(0.006)	(0.007)	(0.009)	(0.014)	(0.015)	(0.024)	(0.043)
×{NON-BLACK NEIGHBORHOODS}	0.009	0.011	0.011	0.028	0.024	0.019	-0.006	0.002	0.004
	(0.011)	(0.010)	(0.009)	(0.016)	(0.023)	(0.037)	(0.015)	(0.023)	(0.039)
OBSERVATIONS	115,185	115,185	115,185	32,326	32,326	32,326	82,859	82,859	82,859
NUMBER OF MATCHES	1	4	16	1	4	16	1	4	16
ESTIMATOR	ATE	ATE	ATE	ATT	ATT	ATT	ATC	ATC	ATC

NOTES: Implements the matching estimator and standard error computation outlined in Abadie and Imbens (2006). Each estimate is the difference in the dependent variable between the officer race listed in the left-most column and white officers. ×{BLACK NEIGHBORHOODS} restricts the sample to the top quartile of beats ranked in ascending order of fraction black, while ×{NON-BLACK NEIGHBORHOODS} restricts to the bottom quartile. ATE averages the unit differences over the both officer races' stops and their respective matches, ATT over only the minority officers' stops and their white officer matches, and ATC over only the white officers' stops and their minority officer matches. OBSERVATIONS lists the total observations for which matches are found. Thus, for the black officer estimates, this is equal the total number of black officers' stops for ATT, white officers' stops for ATC, and the sum of black and white officers's stops for ATE.

Table 1.8: Effect of Officer Race on Stop Rate: White Suspects

	Dependent variable is an officer's number of white stops in one day						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	0.091	0.009	-0.019	-0.016	-0.031	-0.042	-0.050
	(0.013)	(0.011)	(0.012)	(0.008)	(0.012)	(0.025)	(0.025)
×{FRACTION WHITE}			0.150		0.093		0.033
			(0.064)		(0.061)		(0.058)
×{EXPERIENCE 4–10 YRS}				0.066	0.056		0.046
				(0.027)	(0.026)		(0.025)
×{EXPERIENCE 11+ YRS}				0.010	0.006		0.016
				(0.019)	(0.019)		(0.018)
×{MEDIAN INCOME}						0.288	0.176
						(0.148)	(0.153)
HISPANICOFF	0.055	0.018	-0.018	-0.007	-0.028	-0.081	-0.086
	(0.009)	(0.007)	(0.008)	(0.006)	(0.009)	(0.019)	(0.018)
×{FRACTION WHITE}			0.223		0.145		0.001
			(0.053)		(0.049)		(0.049)
×{EXPERIENCE 4–10 YRS}				-0.010	-0.011		-0.015
				(0.016)	(0.016)		(0.016)
×{EXPERIENCE 11+ YRS}				0.222	0.211		0.200
				(0.029)	(0.028)		(0.027)
×{MEDIAN INCOME}						0.590	0.504
						(0.122)	(0.124)
ASIANOFF	0.035	-0.039	-0.001	-0.025	0.016	0.015	0.033
	(0.008)	(0.007)	(0.009)	(0.007)	(0.009)	(0.017)	(0.015)
×{FRACTION WHITE}			-0.206		-0.277		-0.198
			(0.052)		(0.054)		(0.062)
×{EXPERIENCE 4–10 YRS}				-0.091	-0.081		-0.080
				(0.016)	(0.016)		(0.016)
×{EXPERIENCE 11+ YRS}				0.071	0.098		0.101
				(0.024)	(0.023)		(0.022)
×{MEDIAN INCOME}						-0.313	-0.181
						(0.097)	(0.102)
EXPERIENCE 4–10 YRS				0.093	0.067		0.064
				(0.015)	(0.015)		(0.014)
×{FRACTION WHITE}					0.156		0.152
					(0.052)		(0.051)
EXPERIENCE 11+ YRS				0.031	0.023		0.016
				(0.012)	(0.013)		(0.012)
×{FRACTION WHITE}					0.050		0.050
					(0.054)		(0.053)
_CONS	0.257						
	(0.009)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.002	0.391	0.392	0.396	0.398	0.402	0.407
OBSERVATIONS	91,770	91,770	91,733	88,586	88,550	90,225	87,078

NOTES: See table 1.4.

Table 1.9: Effect of Officer Race on Stop Rate: Hispanic Suspects

	Dependent variable is an officer's number of hispanic stops in one day						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
blackoff	-0.044	-0.027	0.022	-0.007	0.043	-0.018	0.059
	(0.009)	(0.007)	(0.011)	(0.008)	(0.013)	(0.013)	(0.017)
×{fraction hispanic}			-0.149		-0.141		-0.145
			(0.031)		(0.032)		(0.032)
×{experience 4–10 yrs}				-0.063	-0.071		-0.070
				(0.016)	(0.016)		(0.017)
×{experience 11+ yrs}				0.017	0.012		0.013
				(0.015)	(0.016)		(0.016)
×{median income}						-0.047	-0.079
						(0.062)	(0.059)
hispanicoff	0.074	0.030	0.056	0.014	0.038	0.017	0.041
	(0.010)	(0.008)	(0.014)	(0.007)	(0.012)	(0.011)	(0.014)
×{fraction hispanic}			-0.070		-0.064		-0.077
			(0.028)		(0.031)		(0.026)
×{experience 4–10 yrs}				-0.012	-0.007		0.003
				(0.020)	(0.019)		(0.015)
×{experience 11+ yrs}				0.158	0.160		0.167
				(0.023)	(0.023)		(0.022)
×{median income}						0.106	0.017
						(0.057)	(0.058)
asianoff	-0.004	0.012	0.010	0.005	-0.009	0.032	0.016
	(0.011)	(0.007)	(0.010)	(0.008)	(0.012)	(0.009)	(0.013)
×{fraction hispanic}			0.010		0.043		0.029
			(0.028)		(0.030)		(0.028)
×{experience 4–10 yrs}				-0.031	-0.031		-0.017
				(0.019)	(0.020)		(0.016)
×{experience 11+ yrs}				0.071	0.086		0.080
				(0.021)	(0.022)		(0.019)
×{median income}						-0.103	-0.124
						(0.042)	(0.043)
experience 4–10 yrs				0.113	0.135		0.107
				(0.020)	(0.030)		(0.018)
×{fraction hispanic}					-0.062		-0.031
					(0.037)		(0.028)
experience 11+ yrs				0.020	0.003		-0.002
				(0.011)	(0.014)		(0.014)
×{fraction hispanic}					0.043		0.046
					(0.032)		(0.032)
_cons	0.346						
	(0.013)						
Controls		Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.002	0.419	0.420	0.423	0.423	0.428	0.431
Observations	91,770	91,770	91,733	88,586	88,550	90,225	87,078

Notes: See table 1.4.

Table 1.10: Effect of Officer Race on Stop Rate: Asian Suspects

	Dependent variable is an officer's number of asian stops in one day						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	0.023	-0.017	0.021	-0.004	0.030	-0.015	0.046
	(0.004)	(0.004)	(0.006)	(0.005)	(0.007)	(0.009)	(0.010)
×{FRACTION ASIAN}			-0.203		-0.201		-0.208
			(0.038)		(0.036)		(0.035)
×{EXPERIENCE 4–10 YRS}				-0.019	-0.014		-0.014
				(0.009)	(0.009)		(0.009)
×{EXPERIENCE 11+ YRS}				-0.019	-0.011		-0.012
				(0.010)	(0.010)		(0.010)
×{MEDIAN INCOME}						-0.017	-0.088
						(0.050)	(0.048)
HISPANICOFF	0.045	0.016	0.024	0.006	0.013	-0.003	-0.003
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.008)	(0.009)
×{FRACTION ASIAN}			-0.053		-0.052		-0.048
			(0.031)		(0.032)		(0.032)
×{EXPERIENCE 4–10 YRS}				0.028	0.030		0.029
				(0.009)	(0.009)		(0.009)
×{EXPERIENCE 11+ YRS}				0.024	0.024		0.022
				(0.013)	(0.013)		(0.013)
×{MEDIAN INCOME}						0.116	0.095
						(0.048)	(0.046)
ASIANOFF	0.079	0.023	0.017	0.013	0.011	0.026	0.018
	(0.006)	(0.004)	(0.005)	(0.005)	(0.007)	(0.008)	(0.009)
×{FRACTION ASIAN}			0.015		0.010		0.001
			(0.031)		(0.033)		(0.033)
×{EXPERIENCE 4–10 YRS}				-0.023	-0.022		-0.018
				(0.009)	(0.009)		(0.009)
×{EXPERIENCE 11+ YRS}				0.091	0.082		0.082
				(0.015)	(0.014)		(0.014)
×{MEDIAN INCOME}						-0.017	-0.038
						(0.037)	(0.041)
EXPERIENCE 4–10 YRS				0.018	0.023		0.017
				(0.006)	(0.008)		(0.006)
×{FRACTION ASIAN}					-0.042		-0.030
					(0.025)		(0.022)
EXPERIENCE 11+ YRS				-0.011	-0.015		-0.014
				(0.006)	(0.007)		(0.007)
×{FRACTION ASIAN}					0.019		0.018
					(0.036)		(0.037)
_CONS	0.098						
	(0.004)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.004	0.251	0.254	0.254	0.257	0.257	0.262
OBSERVATIONS	91,770	91,770	91,733	88,586	88,550	90,225	87,078

NOTES: See table 1.4.

Table 1.11: Effect of Officer Race on Stop Composition: White Suspects

	Dependent variable is an indicator for a white stop						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	0.038	0.002	-0.005	-0.003	-0.008	-0.009	-0.014
	(0.008)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)
×{FRACTION WHITE}			0.036		0.037		0.014
			(0.019)		(0.018)		(0.021)
×{EXPERIENCE 4–10 YRS}				0.001	-0.002		-0.002
				(0.007)	(0.007)		(0.007)
×{EXPERIENCE 11+ YRS}				0.012	0.010		0.015
				(0.009)	(0.009)		(0.009)
×{MEDIAN INCOME}						0.065	0.056
						(0.032)	(0.038)
HISPANICOFF	0.013	0.002	-0.006	-0.003	-0.008	-0.015	-0.016
	(0.006)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
×{FRACTION WHITE}			0.048		0.035		0.009
			(0.024)		(0.020)		(0.023)
×{EXPERIENCE 4–10 YRS}				-0.001	-0.001		-0.004
				(0.006)	(0.006)		(0.005)
×{EXPERIENCE 11+ YRS}				0.028	0.024		0.025
				(0.012)	(0.010)		(0.010)
×{MEDIAN INCOME}						0.102	0.078
						(0.028)	(0.029)
ASIANOFF	0.019	-0.005	0.000	-0.005	0.001	0.012	0.015
	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.006)	(0.009)
×{FRACTION WHITE}			-0.029		-0.041		0.017
			(0.025)		(0.024)		(0.022)
×{EXPERIENCE 4–10 YRS}				-0.013	-0.011		-0.013
				(0.006)	(0.006)		(0.006)
×{EXPERIENCE 11+ YRS}				0.007	0.010		0.014
				(0.018)	(0.016)		(0.013)
×{MEDIAN INCOME}						-0.101	-0.138
						(0.042)	(0.057)
EXPERIENCE 4–10 YRS				0.015	0.013		0.014
				(0.004)	(0.004)		(0.004)
×{FRACTION WHITE}					0.015		0.014
					(0.016)		(0.016)
EXPERIENCE 11+ YRS				0.025	0.020		0.017
				(0.006)	(0.008)		(0.007)
×{FRACTION WHITE}					0.029		0.033
					(0.026)		(0.023)
_CONS	0.109						
	(0.006)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.002	0.229	0.229	0.232	0.232	0.231	0.234
OBSERVATIONS	188,526	188,526	188,415	182,952	182,842	184,721	179,226

NOTES: See table 1.4.

Table 1.12: Effect of Officer Race on Stop Composition: Hispanic Suspects

	Dependent variable is an indicator for a hispanic stop						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.012	-0.004	0.002	-0.004	-0.001	-0.007	-0.003
	(0.004)	(0.003)	(0.004)	(0.004)	(0.006)	(0.004)	(0.006)
×{FRACTION HISPANIC}			-0.019		-0.011		-0.010
			(0.012)		(0.012)		(0.012)
×{EXPERIENCE 4–10 YRS}				-0.001	-0.002		-0.003
				(0.007)	(0.007)		(0.007)
×{EXPERIENCE 11+ YRS}				0.009	0.010		0.010
				(0.008)	(0.008)		(0.008)
×{MEDIAN INCOME}						0.017	0.017
						(0.017)	(0.017)
HISPANICOFF	0.022	0.011	0.006	0.007	-0.001	0.008	-0.003
	(0.006)	(0.003)	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)
×{FRACTION HISPANIC}			0.014		0.021		0.021
			(0.011)		(0.012)		(0.012)
×{EXPERIENCE 4–10 YRS}				0.001	0.001		0.001
				(0.005)	(0.005)		(0.006)
×{EXPERIENCE 11+ YRS}				0.030	0.032		0.032
				(0.008)	(0.008)		(0.008)
×{MEDIAN INCOME}						0.023	0.016
						(0.019)	(0.019)
ASIANOFF	-0.000	0.010	-0.002	0.006	-0.008	0.010	-0.009
	(0.007)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
×{FRACTION HISPANIC}			0.034		0.040		0.040
			(0.014)		(0.013)		(0.013)
×{EXPERIENCE 4–10 YRS}				0.009	0.010		0.010
				(0.007)	(0.007)		(0.007)
×{EXPERIENCE 11+ YRS}				0.003	0.009		0.007
				(0.008)	(0.008)		(0.008)
×{MEDIAN INCOME}						-0.005	0.002
						(0.029)	(0.025)
EXPERIENCE 4–10 YRS				0.015	0.012		0.010
				(0.004)	(0.005)		(0.005)
×{FRACTION HISPANIC}					0.008		0.009
					(0.012)		(0.012)
EXPERIENCE 11+ YRS				0.010	-0.002		-0.002
				(0.005)	(0.006)		(0.006)
×{FRACTION HISPANIC}					0.032		0.031
					(0.012)		(0.012)
_CONS	0.148						
	(0.010)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.001	0.237	0.237	0.236	0.236	0.236	0.235
OBSERVATIONS	188,526	188,526	188,415	182,952	182,842	184,721	179,226

NOTES: See table 1.4.

Table 1.13: Effect of Officer Race on Stop Composition: Asian Suspects

	Dependent variable is an indicator for a asian stop						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	0.011	-0.004	0.007	-0.001	0.010	-0.004	0.014
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.004)
×{FRACTION ASIAN}			-0.051		-0.055		-0.056
			(0.020)		(0.020)		(0.020)
×{EXPERIENCE 4–10 YRS}				-0.004	-0.004		-0.003
				(0.004)	(0.004)		(0.004)
×{EXPERIENCE 11+ YRS}				-0.008	-0.005		-0.005
				(0.006)	(0.006)		(0.006)
×{MEDIAN INCOME}						-0.001	-0.023
						(0.021)	(0.015)
HISPANICOFF	0.015	0.006	0.003	0.002	-0.000	0.004	-0.003
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
×{FRACTION ASIAN}			0.018		0.017		0.018
			(0.011)		(0.012)		(0.011)
×{EXPERIENCE 4–10 YRS}				0.011	0.011		0.012
				(0.004)	(0.004)		(0.004)
×{EXPERIENCE 11+ YRS}				0.001	0.000		0.000
				(0.005)	(0.005)		(0.006)
×{MEDIAN INCOME}						0.013	0.014
						(0.014)	(0.014)
ASIANOFF	0.038	0.016	0.003	0.011	0.002	0.022	0.005
	(0.007)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)
×{FRACTION ASIAN}			0.061		0.055		0.053
			(0.014)		(0.013)		(0.013)
×{EXPERIENCE 4–10 YRS}				0.001	-0.003		-0.001
				(0.004)	(0.004)		(0.004)
×{EXPERIENCE 11+ YRS}				0.023	0.014		0.016
				(0.007)	(0.007)		(0.006)
×{MEDIAN INCOME}						-0.034	-0.020
						(0.014)	(0.017)
EXPERIENCE 4–10 YRS				-0.003	-0.003		-0.004
				(0.002)	(0.002)		(0.002)
×{FRACTION ASIAN}					-0.003		-0.002
					(0.009)		(0.009)
EXPERIENCE 11+ YRS				-0.002	-0.004		-0.004
				(0.003)	(0.003)		(0.003)
×{FRACTION ASIAN}					0.014		0.014
					(0.012)		(0.012)
_CONS	0.042						
	(0.003)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.004	0.118	0.118	0.119	0.119	0.118	0.120
OBSERVATIONS	188,518	188,518	188,407	182,944	182,834	184,713	179,218

NOTES: See table 1.4.

Table 1.14: Effect of Officer Race on Stop Composition [Matching Estimator]: White Suspects

	Dependent variable is an indicator for a white stop								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
blackoff	-0.000	-0.004	-0.003	0.002	-0.000	0.001	-0.001	-0.005	-0.004
	(0.005)	(0.004)	(0.004)	(0.006)	(0.008)	(0.012)	(0.009)	(0.014)	(0.026)
$\times$\{white neighborhoods\}	0.008	-0.000	-0.001	0.026	0.010	0.007	-0.002	-0.005	-0.006
	(0.012)	(0.010)	(0.009)	(0.017)	(0.023)	(0.034)	(0.019)	(0.028)	(0.048)
$\times$\{non-white neighborhoods\}	-0.001	-0.004	-0.003	-0.002	-0.002	-0.002	-0.001	-0.004	-0.003
	(0.008)	(0.007)	(0.007)	(0.008)	(0.011)	(0.019)	(0.016)	(0.028)	(0.051)
Observations	111,934	111,934	111,934	29,075	29,075	29,075	82,859	82,859	82,859
hispanicoff	-0.002	-0.002	-0.002	0.001	0.002	0.002	-0.003	-0.004	-0.004
	(0.004)	(0.003)	(0.003)	(0.005)	(0.007)	(0.011)	(0.006)	(0.009)	(0.016)
$\times$\{white neighborhoods\}	0.015	0.010	0.008	0.028	0.018	0.021	0.008	0.004	0.001
	(0.011)	(0.010)	(0.009)	(0.017)	(0.023)	(0.034)	(0.017)	(0.025)	(0.040)
$\times$\{non-white neighborhoods\}	-0.002	-0.000	-0.000	-0.002	0.002	0.001	-0.002	-0.001	-0.001
	(0.006)	(0.005)	(0.005)	(0.007)	(0.009)	(0.015)	(0.009)	(0.016)	(0.029)
Observations	127,089	127,089	127,089	44,230	44,230	44,230	82,859	82,859	82,859
asianoff	-0.006	-0.006	-0.005	-0.013	-0.011	-0.009	-0.003	-0.004	-0.004
	(0.004)	(0.004)	(0.004)	(0.005)	(0.008)	(0.012)	(0.008)	(0.013)	(0.023)
$\times$\{white neighborhoods\}	-0.001	0.004	0.004	0.001	-0.003	-0.001	-0.001	0.006	0.006
	(0.012)	(0.010)	(0.010)	(0.013)	(0.018)	(0.027)	(0.022)	(0.034)	(0.057)
$\times$\{non-white neighborhoods\}	-0.004	-0.003	-0.001	-0.006	-0.006	-0.006	-0.003	-0.002	0.001
	(0.007)	(0.006)	(0.006)	(0.009)	(0.013)	(0.021)	(0.013)	(0.022)	(0.039)
Observations	115,185	115,185	115,185	32,326	32,326	32,326	82,859	82,859	82,859
Number of matches	1	4	16	1	4	16	1	4	16
Estimator	ATE	ATE	ATE	ATT	ATT	ATT	ATC	ATC	ATC

Notes: See table **??**.

Table 1.15: Effect of Officer Race on Stop Composition [Matching Estimator]: Hispanic Suspects

	Dependent variable is an indicator for a Hispanic stop								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BLACKOFF	-0.004	-0.001	-0.003	-0.003	-0.002	-0.005	-0.005	-0.001	-0.003
	(0.005)	(0.004)	(0.004)	(0.006)	(0.008)	(0.012)	(0.009)	(0.014)	(0.026)
×{HISPANIC NEIGHBORHOODS}	-0.009	-0.007	-0.008	-0.010	-0.010	-0.011	-0.009	-0.007	-0.007
	(0.007)	(0.006)	(0.006)	(0.007)	(0.009)	(0.014)	(0.014)	(0.023)	(0.041)
×{NON-HISPANIC NEIGHBORHOODS}	-0.008	-0.000	-0.002	-0.008	-0.001	-0.002	-0.007	0.000	-0.002
	(0.010)	(0.008)	(0.008)	(0.014)	(0.020)	(0.030)	(0.017)	(0.026)	(0.044)
OBSERVATIONS	111,934	111,934	111,934	29,075	29,075	29,075	82,859	82,859	82,859
HISPANICOFF	0.010	0.008	0.008	0.009	0.008	0.009	0.010	0.008	0.008
	(0.004)	(0.003)	(0.003)	(0.005)	(0.007)	(0.011)	(0.006)	(0.009)	(0.016)
×{HISPANIC NEIGHBORHOODS}	0.010	0.006	0.010	0.008	0.008	0.012	0.011	0.005	0.008
	(0.005)	(0.005)	(0.005)	(0.007)	(0.009)	(0.014)	(0.007)	(0.012)	(0.021)
×{NON-HISPANIC NEIGHBORHOODS}	0.006	0.006	0.002	0.010	0.005	0.000	0.004	0.007	0.003
	(0.009)	(0.008)	(0.007)	(0.013)	(0.019)	(0.029)	(0.014)	(0.022)	(0.038)
OBSERVATIONS	127,089	127,089	127,089	44,230	44,230	44,230	82,859	82,859	82,859
ASIANOFF	0.006	0.007	0.008	0.004	0.005	0.005	0.007	0.008	0.009
	(0.004)	(0.004)	(0.004)	(0.005)	(0.008)	(0.012)	(0.008)	(0.013)	(0.023)
×{HISPANIC NEIGHBORHOODS}	0.014	0.013	0.014	0.014	0.012	0.014	0.014	0.013	0.014
	(0.006)	(0.006)	(0.006)	(0.007)	(0.009)	(0.014)	(0.013)	(0.021)	(0.037)
×{NON-HISPANIC NEIGHBORHOODS}	0.005	0.006	0.004	0.000	0.003	0.000	0.007	0.008	0.006
	(0.010)	(0.008)	(0.008)	(0.012)	(0.017)	(0.028)	(0.016)	(0.026)	(0.044)
OBSERVATIONS	115,185	115,185	115,185	32,326	32,326	32,326	82,859	82,859	82,859
NUMBER OF MATCHES	1	4	16	1	4	16	1	4	16
ESTIMATOR	ATE	ATE	ATE	ATT	ATT	ATT	ATC	ATC	ATC

NOTES: See table ??.

Table 1.16: Effect of Officer Race on Stop Rate [By Sample]: All Suspects

	Dependent variable is an officer's number of stops in one day									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BLACKOFF	-0.082	-0.087	-0.074	-0.170	-0.133	-0.126	-0.104	-0.170	-0.129	-0.107
	(0.021)	(0.022)	(0.022)	(0.018)	(0.018)	(0.017)	(0.016)	(0.018)	(0.017)	(0.016)
HISPANICOFF	0.211	0.212	0.151	0.162	0.103	0.062	0.051	0.164	0.065	0.054
	(0.022)	(0.023)	(0.023)	(0.017)	(0.017)	(0.016)	(0.015)	(0.017)	(0.016)	(0.015)
ASIANOFF	-0.015	-0.014	-0.025	-0.047	-0.049	-0.122	-0.095	-0.050	-0.124	-0.097
	(0.020)	(0.021)	(0.021)	(0.016)	(0.016)	(0.015)	(0.014)	(0.016)	(0.015)	(0.014)
_CONS	2.276	2.317	2.323							
	(0.011)	(0.011)	(0.011)							
SAMPLE	OFFICER	BEAT	GEO	BEAT	GEO	GEO	GEO	BEAT	GEO	GEO
BEAT×TIME FE				YES	YES			YES		
TRACT×TIME FE						YES			YES	
BLOCK×TIME FE							YES			YES
WEEKDAY FE								YES	YES	YES
YEAR FE								YES	YES	YES
ADJ. R^2	0.002	0.002	0.001	0.697	0.722	0.743	0.777	0.699	0.744	0.779
OBSERVATIONS	105,024	97,175	91,770	97,175	91,770	91,770	91,770	97,175	91,770	91,770

NOTES: OLS regression with robust standard errors in parentheses. WEEKDAY FE is separate indicators for day of week, YEAR FE is separate indicators for year, BEAT×TIME FE is interactions of the officer's beat with time measured in 8-hour blocks, and similarly for TRACT×TIME FE (Census 2000 tract) and BLOCK×TIME FE (Census 2000 block group). See table 1.1 for an explanation of the samples.

Table 1.17: Effect of Officer Race on Stop Rate [By Sample]: Black Suspects

	Dependent variable is an officer's number of black stops in one day									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BLACKOFF	-0.173	-0.167	-0.160	-0.102	-0.086	-0.083	-0.066	-0.103	-0.085	-0.068
	(0.012)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)
HISPANICOFF	-0.008	-0.011	-0.031	0.030	0.011	-0.003	-0.007	0.031	-0.002	-0.006
	(0.013)	(0.014)	(0.014)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)
ASIANOFF	-0.140	-0.139	-0.148	-0.073	-0.076	-0.104	-0.080	-0.074	-0.105	-0.081
	(0.013)	(0.013)	(0.014)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)
_CONS	1.452	1.492	1.506							
	(0.007)	(0.008)	(0.008)							
Sample	Officer	Beat	Geo	Beat	Geo	Geo	Geo	Beat	Geo	Geo
Beat×Time FE				Yes	Yes			Yes		
Tract×Time FE						Yes			Yes	
Block×Time FE							Yes			Yes
Weekday FE								Yes	Yes	Yes
Year FE								Yes	Yes	Yes
Adj. R^2	0.002	0.002	0.002	0.669	0.681	0.703	0.732	0.670	0.704	0.734
Observations	105,024	97,175	91,770	97,175	91,770	91,770	91,770	97,175	91,770	91,770

Notes: See table 1.16

Table 1.18: Effect of Officer Race on Stop Rate [By Sample]: White Suspects

	Dependent variable is an officer's number of white stops in one day									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BLACKOFF	0.106	0.093	0.091	0.006	0.009	0.012	0.010	0.005	0.010	0.009
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
HISPANICOFF	0.072	0.073	0.055	0.047	0.033	0.020	0.019	0.047	0.019	0.018
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)
ASIANOFF	0.038	0.036	0.035	-0.023	-0.021	-0.043	-0.040	-0.023	-0.043	-0.039
	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
_CONS	0.261	0.260	0.257							
	(0.003)	(0.003)	(0.003)							
Sample	Officer	Beat	Geo	Beat	Geo	Geo	Geo	Beat	Geo	Geo
Beat×Time FE				Yes	Yes			Yes		
Tract×Time FE						Yes			Yes	
Block×Time FE							Yes			Yes
Weekday FE								Yes	Yes	Yes
Year FE								Yes	Yes	Yes
Adj. R^2	0.002	0.002	0.002	0.303	0.315	0.362	0.390	0.305	0.363	0.391
Observations	105,024	97,175	91,770	97,175	91,770	91,770	91,770	97,175	91,770	91,770

Notes: See table 1.16

Table 1.19: Effect of Officer Race on Stop Rate [By Sample]: Hispanic Suspects

	Dependent variable is an officer's number of Hispanic stops in one day									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BLACKOFF	-0.050	-0.048	-0.044	-0.039	-0.031	-0.030	-0.028	-0.039	-0.029	-0.027
	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
HISPANICOFF	0.082	0.084	0.074	0.050	0.038	0.033	0.028	0.051	0.034	0.030
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
ASIANOFF	0.001	-0.001	-0.004	0.017	0.016	0.014	0.013	0.015	0.012	0.012
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
_CONS	0.344	0.347	0.346							
	(0.004)	(0.004)	(0.004)							
SAMPLE	OFFICER	BEAT	GEO	BEAT	GEO	GEO	GEO	BEAT	GEO	GEO
BEAT×TIME FE				YES	YES			YES		
TRACT×TIME FE						YES			YES	
BLOCK×TIME FE							YES			YES
WEEKDAY FE								YES	YES	YES
YEAR FE								YES	YES	YES
ADJ. R^2	0.003	0.003	0.002	0.361	0.374	0.382	0.418	0.363	0.384	0.419
OBSERVATIONS	105,024	97,175	91,770	97,175	91,770	91,770	91,770	97,175	91,770	91,770

NOTES: See table 1.16

Table 1.20: Effect of Officer Race on Stop Composition [By Sample]: Black Suspects

	Dependent variable is an indicator for a black stop									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BLACKOFF	-0.055	-0.044	-0.044	0.001	-0.000	0.004	0.004	0.001	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
HISPANICOFF	-0.059	-0.058	-0.051	-0.027	-0.022	-0.018	-0.017	-0.028	-0.019	-0.018
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
ASIANOFF	-0.058	-0.062	-0.064	-0.026	-0.027	-0.023	-0.022	-0.026	-0.023	-0.022
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
_CONS	0.644	0.658	0.662							
	(0.001)	(0.002)	(0.002)							
SAMPLE	OFFICER	BEAT	GEO	BEAT	GEO	GEO	GEO	BEAT	GEO	GEO
BEAT×TIME FE				YES	YES			YES		
TRACT×TIME FE						YES			YES	
BLOCK×TIME FE							YES			YES
WEEKDAY FE								YES	YES	YES
YEAR FE								YES	YES	YES
ADJ. R^2	0.003	0.003	0.003	0.660	0.666	0.673	0.677	0.660	0.673	0.677
OBSERVATIONS	240,322	207,331	188,526	207,331	188,526	188,526	188,526	207,331	188,526	188,526

NOTES: See table 1.16

Table 1.21: Effect of Officer Race on Stop Composition [By Sample]: White Suspects

	Dependent variable is an indicator for a white stop									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BLACKOFF	0.054	0.040	0.038	0.006	0.007	0.004	0.003	0.006	0.003	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
HISPANICOFF	0.019	0.018	0.013	0.007	0.005	0.002	0.002	0.007	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ASIANOFF	0.018	0.018	0.019	-0.005	-0.004	-0.005	-0.006	-0.004	-0.005	-0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
_CONS	0.116	0.109	0.109							
	(0.001)	(0.001)	(0.001)							
SAMPLE	OFFICER	BEAT	GEO	BEAT	GEO	GEO	GEO	BEAT	GEO	GEO
BEAT×TIME FE				YES	YES			YES		
TRACT×TIME FE						YES			YES	
BLOCK×TIME FE							YES			YES
WEEKDAY FE								YES	YES	YES
YEAR FE								YES	YES	YES
ADJ. R^2	0.003	0.002	0.002	0.205	0.204	0.221	0.229	0.205	0.221	0.229
OBSERVATIONS	240,322	207,331	188,526	207,331	188,526	188,526	188,526	207,331	188,526	188,526

NOTES: See table 1.16

Table 1.22: Effect of Officer Race on Stop Composition [By Sample]: Hispanic Suspects

	Dependent variable is an indicator for a Hispanic stop									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BLACKOFF	-0.017	-0.014	-0.012	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
HISPANICOFF	0.020	0.021	0.022	0.011	0.010	0.011	0.010	0.012	0.012	0.011
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ASIANOFF	0.001	0.001	-0.000	0.012	0.011	0.010	0.010	0.012	0.010	0.010
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
_CONS	0.153	0.149	0.148							
	(0.001)	(0.001)	(0.001)							
Sample	Officer	Beat	Geo	Beat	Geo	Geo	Geo	Beat	Geo	Geo
Beat×Time FE				Yes	Yes			Yes		
Tract×Time FE						Yes			Yes	
Block×Time FE							Yes			Yes
Weekday FE								Yes	Yes	Yes
Year FE								Yes	Yes	Yes
Adj. R^2	0.001	0.001	0.001	0.223	0.223	0.228	0.236	0.224	0.229	0.237
Observations	240,322	207,331	188,526	207,331	188,526	188,526	188,526	207,331	188,526	188,526

Notes: See table 1.16

Table 1.23: Policing Intensity (Stops per Shift)

Officer race	Type of Neighborhood	
# of black stops/shift		
	black	non-black
asian	1.34	1.02
black	1.35	1.02
hispanic	1.53	0.98
white	1.51	0.91
# of white stops/shift		
	white	non-white
asian	0.61	0.12
black	0.92	0.10
hispanic	1.04	0.10
white	0.72	0.10
# of hispanic stops/shift		
	hispanic	non-hispanic
asian	0.59	0.16
black	0.50	0.17
hispanic	0.60	0.16
white	0.51	0.15
# of asian stops/shift		
	asian	non-asian
asian	0.35	0.04
black	0.20	0.04
hispanic	0.28	0.04
white	0.23	0.03

Notes: Average stop rates by officer race and type of neighborhood. black neighborhood is defined as within the top quartile of percent census block group black, while non-black is the lowest quartile, and similarly for other races.

Table 1.24: Effect of Officer Race on Search Rate: Black Suspects

	Dependent variable is an indicator for a search						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.042	-0.012	-0.015	0.012	0.012	-0.008	0.019
	(0.009)	(0.007)	(0.015)	(0.010)	(0.017)	(0.012)	(0.023)
×{FRACTION BLACK}			0.007		-0.003		-0.005
			(0.031)		(0.031)		(0.034)
×{EXPERIENCE 4–10 YRS}				0.023	0.025		0.026
				(0.016)	(0.015)		(0.015)
×{EXPERIENCE 11+ YRS}				-0.174	-0.173		-0.177
				(0.016)	(0.016)		(0.016)
×{MEDIAN INCOME}						-0.016	-0.034
						(0.047)	(0.052)
HISPANICOFF	-0.029	-0.011	-0.009	0.016	0.033	0.014	0.061
	(0.008)	(0.007)	(0.012)	(0.006)	(0.013)	(0.013)	(0.016)
×{FRACTION BLACK}			-0.006		-0.040		-0.058
			(0.029)		(0.027)		(0.027)
×{EXPERIENCE 4–10 YRS}				-0.060	-0.060		-0.059
				(0.015)	(0.015)		(0.015)
×{EXPERIENCE 11+ YRS}				-0.116	-0.117		-0.119
				(0.020)	(0.020)		(0.020)
×{MEDIAN INCOME}						-0.157	-0.133
						(0.068)	(0.063)
ASIANOFF	-0.069	-0.039	-0.068	-0.000	-0.007	-0.029	-0.008
	(0.014)	(0.007)	(0.014)	(0.010)	(0.015)	(0.015)	(0.018)
×{FRACTION BLACK}			0.070		0.013		0.011
			(0.028)		(0.027)		(0.027)
×{EXPERIENCE 4–10 YRS}				-0.004	-0.001		-0.002
				(0.017)	(0.017)		(0.017)
×{EXPERIENCE 11+ YRS}				-0.206	-0.200		-0.204
				(0.016)	(0.016)		(0.017)
×{MEDIAN INCOME}						-0.064	0.015
						(0.082)	(0.047)
EXPERIENCE 4–10 YRS				-0.061	-0.081		-0.084
				(0.011)	(0.017)		(0.017)
×{FRACTION BLACK}					0.043		0.057
					(0.039)		(0.038)
EXPERIENCE 11+ YRS				0.038	0.001		-0.001
				(0.012)	(0.018)		(0.018)
×{FRACTION BLACK}					0.088		0.101
					(0.033)		(0.033)
_CONS	0.353						
	(0.008)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.003	0.371	0.371	0.376	0.377	0.373	0.378
OBSERVATIONS	103,891	103,891	103,872	100,862	100,843	102,129	99,144

NOTES: See table 1.4.

Table 1.25: Effect of Officer Race on Search Rate: Hispanic Suspects

	Dependent variable is an indicator for a search						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.050	-0.029	-0.046	-0.029	-0.050	-0.049	-0.078
	(0.011)	(0.010)	(0.017)	(0.014)	(0.022)	(0.020)	(0.030)
×{FRACTION HISPANIC}			0.035		0.040		0.054
			(0.032)		(0.035)		(0.037)
×{EXPERIENCE 4–10 YRS}				0.068	0.069		0.068
				(0.023)	(0.024)		(0.025)
×{EXPERIENCE 11+ YRS}				-0.114	-0.113		-0.127
				(0.029)	(0.030)		(0.027)
×{MEDIAN INCOME}						0.120	0.136
						(0.097)	(0.100)
HISPANICOFF	-0.040	-0.038	-0.035	-0.036	-0.039	-0.049	-0.048
	(0.009)	(0.009)	(0.017)	(0.012)	(0.018)	(0.017)	(0.024)
×{FRACTION HISPANIC}			-0.007		0.005		0.014
			(0.030)		(0.031)		(0.032)
×{EXPERIENCE 4–10 YRS}				-0.001	0.000		-0.005
				(0.020)	(0.020)		(0.021)
×{EXPERIENCE 11+ YRS}				-0.065	-0.067		-0.077
				(0.032)	(0.031)		(0.030)
×{MEDIAN INCOME}						0.057	0.044
						(0.078)	(0.082)
ASIANOFF	-0.043	-0.030	-0.041	-0.013	-0.026	-0.057	-0.078
	(0.011)	(0.010)	(0.020)	(0.015)	(0.023)	(0.016)	(0.026)
×{FRACTION HISPANIC}			0.020		0.025		0.058
			(0.032)		(0.037)		(0.031)
×{EXPERIENCE 4–10 YRS}				0.044	0.044		0.041
				(0.023)	(0.023)		(0.023)
×{EXPERIENCE 11+ YRS}				-0.151	-0.151		-0.163
				(0.025)	(0.025)		(0.025)
×{MEDIAN INCOME}						0.136	0.193
						(0.075)	(0.075)
EXPERIENCE 4–10 YRS				-0.095	-0.076		-0.046
				(0.015)	(0.033)		(0.020)
×{FRACTION HISPANIC}					-0.037		-0.073
					(0.046)		(0.032)
EXPERIENCE 11+ YRS				0.042	0.052		0.086
				(0.021)	(0.043)		(0.029)
×{FRACTION HISPANIC}					-0.017		-0.051
					(0.060)		(0.050)
_CONS	0.325						
	(0.009)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.002	0.326	0.326	0.331	0.331	0.329	0.334
OBSERVATIONS	27,369	27,369	27,348	26,354	26,334	26,429	25,438

NOTES: See table 1.4.

Table 1.26: Effect of Officer Race on Search Rate: White Suspects

	Dependent variable is an indicator for a search						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.057	-0.025	-0.031	-0.004	-0.007	-0.030	-0.012
	(0.009)	(0.007)	(0.011)	(0.013)	(0.017)	(0.011)	(0.023)
×{FRACTION BLACK}			0.016		0.005		0.008
			(0.030)		(0.032)		(0.039)
×{EXPERIENCE 4–10 YRS}				-0.014	-0.014		-0.016
				(0.017)	(0.017)		(0.018)
×{EXPERIENCE 11+ YRS}				-0.060	-0.060		-0.066
				(0.020)	(0.020)		(0.021)
×{MEDIAN INCOME}						0.011	0.020
						(0.029)	(0.038)
HISPANICOFF	-0.029	-0.012	-0.030	-0.008	-0.021	0.001	-0.009
	(0.010)	(0.008)	(0.012)	(0.011)	(0.017)	(0.013)	(0.022)
×{FRACTION BLACK}			0.061		0.038		0.022
			(0.036)		(0.039)		(0.043)
×{EXPERIENCE 4–10 YRS}				0.007	0.009		0.009
				(0.019)	(0.019)		(0.020)
×{EXPERIENCE 11+ YRS}				-0.045	-0.040		-0.043
				(0.024)	(0.025)		(0.027)
×{MEDIAN INCOME}						-0.058	-0.038
						(0.033)	(0.042)
ASIANOFF	-0.036	-0.018	-0.026	-0.015	-0.019	-0.026	-0.029
	(0.009)	(0.007)	(0.011)	(0.012)	(0.017)	(0.011)	(0.019)
×{FRACTION BLACK}			0.029		0.011		0.017
			(0.035)		(0.038)		(0.036)
×{EXPERIENCE 4–10 YRS}				0.024	0.025		0.020
				(0.017)	(0.017)		(0.017)
×{EXPERIENCE 11+ YRS}				-0.045	-0.043		-0.051
				(0.021)	(0.021)		(0.021)
×{MEDIAN INCOME}						0.030	0.044
						(0.033)	(0.033)
EXPERIENCE 4–10 YRS				-0.027	-0.029		-0.030
				(0.011)	(0.014)		(0.014)
×{FRACTION BLACK}					0.004		0.016
					(0.034)		(0.035)
EXPERIENCE 11+ YRS				0.012	-0.002		-0.001
				(0.015)	(0.019)		(0.019)
×{FRACTION BLACK}					0.045		0.062
					(0.039)		(0.042)
_CONS	0.150						
	(0.009)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.004	0.221	0.221	0.223	0.223	0.223	0.225
OBSERVATIONS	22,093	22,093	22,078	21,617	21,602	21,691	21,217

NOTES: See table 1.4.

Table 1.27: Effect of Officer Race on Hit Rate: Black Searches

	Dependent variable is an indicator for a successful search						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.056	-0.052	-0.036	-0.027	-0.005	-0.050	0.005
	(0.006)	(0.007)	(0.015)	(0.010)	(0.019)	(0.013)	(0.026)
×{FRACTION BLACK}			-0.036		-0.049		-0.058
			(0.032)		(0.033)		(0.036)
×{EXPERIENCE 4–10 YRS}				-0.018	-0.017		-0.019
				(0.016)	(0.015)		(0.015)
×{EXPERIENCE 11+ YRS}				-0.147	-0.146		-0.150
				(0.020)	(0.020)		(0.020)
×{MEDIAN INCOME}						-0.017	-0.040
						(0.065)	(0.073)
HISPANICOFF	-0.031	-0.029	0.004	-0.025	0.017	-0.038	0.014
	(0.006)	(0.007)	(0.012)	(0.008)	(0.013)	(0.014)	(0.018)
×{FRACTION BLACK}			-0.074		-0.091		-0.095
			(0.028)		(0.028)		(0.028)
×{EXPERIENCE 4–10 YRS}				-0.006	-0.010		-0.014
				(0.016)	(0.016)		(0.016)
×{EXPERIENCE 11+ YRS}				-0.044	-0.044		-0.053
				(0.029)	(0.029)		(0.027)
×{MEDIAN INCOME}						0.054	0.042
						(0.071)	(0.073)
ASIANOFF	-0.063	-0.057	-0.060	-0.032	-0.036	-0.049	-0.029
	(0.006)	(0.006)	(0.013)	(0.009)	(0.015)	(0.011)	(0.020)
×{FRACTION BLACK}			0.008		0.008		0.006
			(0.027)		(0.029)		(0.030)
×{EXPERIENCE 4–10 YRS}				-0.033	-0.031		-0.034
				(0.013)	(0.013)		(0.012)
×{EXPERIENCE 11+ YRS}				-0.118	-0.114		-0.117
				(0.022)	(0.022)		(0.022)
×{MEDIAN INCOME}						-0.049	-0.027
						(0.053)	(0.057)
EXPERIENCE 4–10 YRS				0.037	0.039		0.039
				(0.009)	(0.014)		(0.014)
×{FRACTION BLACK}					-0.004		-0.003
					(0.024)		(0.024)
EXPERIENCE 11+ YRS				0.107	0.087		0.092
				(0.016)	(0.021)		(0.021)
×{FRACTION BLACK}					0.046		0.039
					(0.037)		(0.038)
_CONS	0.197						
	(0.004)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.005	0.184	0.184	0.188	0.189	0.184	0.189
OBSERVATIONS	34,157	34,157	34,154	32,982	32,979	33,759	32,597

NOTES: See table 1.4.

Table 1.28: Effect of Officer Race on Hit Rate: Hispanic Searches

	Dependent variable is an indicator for a successful search						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.046	-0.041	-0.029	-0.024	-0.007	-0.047	-0.018
	(0.012)	(0.013)	(0.029)	(0.016)	(0.034)	(0.032)	(0.045)
×{FRACTION HISPANIC}			-0.022		-0.030		-0.026
			(0.044)		(0.045)		(0.047)
×{EXPERIENCE 4–10 YRS}				-0.017	-0.018		-0.022
				(0.029)	(0.029)		(0.030)
×{EXPERIENCE 11+ YRS}				-0.104	-0.107		-0.108
				(0.038)	(0.039)		(0.040)
×{MEDIAN INCOME}						0.036	0.063
						(0.184)	(0.188)
HISPANICOFF	-0.034	-0.033	-0.045	-0.033	-0.037	-0.026	-0.040
	(0.010)	(0.012)	(0.029)	(0.016)	(0.029)	(0.028)	(0.045)
×{FRACTION HISPANIC}			0.019		0.007		0.021
			(0.044)		(0.043)		(0.044)
×{EXPERIENCE 4–10 YRS}				0.002	-0.000		0.001
				(0.024)	(0.025)		(0.025)
×{EXPERIENCE 11+ YRS}				-0.015	-0.015		-0.021
				(0.035)	(0.035)		(0.035)
×{MEDIAN INCOME}						-0.062	-0.043
						(0.153)	(0.162)
ASIANOFF	-0.049	-0.050	-0.031	-0.041	-0.008	-0.030	0.023
	(0.012)	(0.013)	(0.029)	(0.016)	(0.033)	(0.029)	(0.046)
×{FRACTION HISPANIC}			-0.034		-0.057		-0.065
			(0.048)		(0.047)		(0.050)
×{EXPERIENCE 4–10 YRS}				-0.019	-0.020		-0.022
				(0.022)	(0.022)		(0.023)
×{EXPERIENCE 11+ YRS}				-0.057	-0.060		-0.059
				(0.041)	(0.041)		(0.043)
×{MEDIAN INCOME}						-0.121	-0.156
						(0.163)	(0.158)
EXPERIENCE 4–10 YRS				0.028	0.016		0.018
				(0.013)	(0.024)		(0.026)
×{FRACTION HISPANIC}					0.023		0.021
					(0.033)		(0.035)
EXPERIENCE 11+ YRS				0.073	0.079		0.072
				(0.021)	(0.033)		(0.035)
×{FRACTION HISPANIC}					-0.010		-0.000
					(0.045)		(0.046)
_CONS	0.152						
	(0.007)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.004	0.147	0.147	0.149	0.148	0.147	0.148
OBSERVATIONS	8,219	8,219	8,214	7,852	7,847	8,031	7,672

NOTES: See table 1.4.

Table 1.29: Effect of Officer Race on Hit Rate: White Searches

	Dependent variable is an indicator for a successful search						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BLACKOFF	-0.090	-0.098	-0.081	-0.081	-0.051	-0.118	-0.060
	(0.019)	(0.026)	(0.045)	(0.040)	(0.059)	(0.043)	(0.080)
×{FRACTION BLACK}			-0.057		-0.100		-0.107
			(0.131)		(0.144)		(0.139)
×{EXPERIENCE 4–10 YRS}				0.015	0.014		0.010
				(0.061)	(0.061)		(0.062)
×{EXPERIENCE 11+ YRS}				-0.082	-0.069		-0.062
				(0.074)	(0.071)		(0.072)
×{MEDIAN INCOME}						0.123	0.075
						(0.164)	(0.189)
HISPANICOFF	-0.017	-0.023	-0.067	-0.030	-0.080	0.002	-0.064
	(0.018)	(0.025)	(0.048)	(0.034)	(0.050)	(0.041)	(0.074)
×{FRACTION BLACK}			0.140		0.143		0.159
			(0.114)		(0.112)		(0.125)
×{EXPERIENCE 4–10 YRS}				0.035	0.051		0.052
				(0.062)	(0.059)		(0.062)
×{EXPERIENCE 11+ YRS}				-0.003	0.003		0.006
				(0.062)	(0.064)		(0.066)
×{MEDIAN INCOME}						-0.148	-0.102
						(0.163)	(0.202)
ASIANOFF	-0.083	-0.075	-0.080	-0.009	-0.007	-0.076	-0.016
	(0.019)	(0.025)	(0.043)	(0.048)	(0.058)	(0.039)	(0.078)
×{FRACTION BLACK}			0.014		-0.006		0.026
			(0.114)		(0.121)		(0.133)
×{EXPERIENCE 4–10 YRS}				-0.100	-0.100		-0.106
				(0.057)	(0.056)		(0.057)
×{EXPERIENCE 11+ YRS}				-0.125	-0.122		-0.114
				(0.085)	(0.085)		(0.089)
×{MEDIAN INCOME}						0.020	0.025
						(0.131)	(0.151)
EXPERIENCE 4–10 YRS				0.041	0.036		0.033
				(0.033)	(0.041)		(0.042)
×{FRACTION BLACK}					0.011		0.038
					(0.079)		(0.083)
EXPERIENCE 11+ YRS				0.086	0.107		0.119
				(0.045)	(0.058)		(0.058)
×{FRACTION BLACK}					-0.077		-0.143
					(0.157)		(0.171)
_CONS	0.195						
	(0.012)						
CONTROLS		YES	YES	YES	YES	YES	YES
ADJ. R^2	0.009	0.216	0.217	0.219	0.219	0.214	0.217
OBSERVATIONS	2,784	2,784	2,782	2,676	2,674	2,723	2,615

NOTES: See table 1.4.

Chapter 2

Youth Offenders and the Deterrence Effect of Prison

2.1 Introduction

A core prediction of deterrence theory is that potential offenders reduce involvement in crime when confronted with *ceteris paribus* increases in punishment. There can be caveats to this prediction in specific circumstances, such as those involving strategic considerations Silverman (2004), but these are as the Giffen good to the law of demand: unusual circumstances may reign, but must be clarified to be present, else the rule of deterrence is presumed to hold.[1]

Empirical tests of the punishment prong of the deterrence hypothesis face difficult identification problems. Individuals facing elevated punitive sanctions are typically demonstrated recidivists. This is a classical selection problem: punishment depends on criminality, which in turn depends on punishment.

Lee and McCrary (2009) argue that the most compelling test of the punishment prong is the criminal participation of youth around the transition to adulthood. In most states, youth are treated as adults upon reaching the 18th birthday, whereupon the youth face the punitive sanctions of the adult subsystem, rather than the lenient sanctions of the juvenile subsystem. A few states have youth transitioning to adulthood for the purposes of criminal law at 17 or even 16.

For one interested in testing deterrence theory, the transition to adulthood is a captivating episode. First, youth are presumably similarly inclined to crime, *ceteris paribus*, just before and just after the transition to adulthood, perhaps even particularly for states where the age

[1]Punishment may, of course, be interpreted broadly, but we give it a narrow reading as being distinctly about the effect of an increase in punishment conditional on apprehension. Apprehension may well have its own deterrence effect, but we will have little to say about that in this article.

of majority is 17 or 16. However, the evidence cited in Lee and McCrary (2009) pertains to a particular sample, a particular time period, and a particular age of transition to adulthood.

In this paper, we present results from six different data sets pertaining to the effect of adult punishments, relative to juvenile punishments, on the behavior of youthful criminals. We give a comprehensive analysis of these results, choosing to present all of the evidence, even when it is subject to a large degree of sampling variability. Our overall aim is to transparently present the evidence for or against the proposition that discontinuous desistance is an important feature of crime data.[2] Our hope is that although each of the data sets we use is flawed, a sense of the overall empirical tendency emerges.

The remainder of the paper is structured as follows. Section 2.2 gives an overview of the data sets we use. Section 2.3 discusses estimation. Section 2.4 presents results from the Uniform Crime Reports program of the Federal Bureau of Investigation, with a particular focus on the age-sex-race files of the arrest return. Section 2.5 considers results from longitudinal data sets we have obtained on arrest records. Section 2.6 presents results pertaining to robustness checks. Section 2.7 discusses the extent to which our data speak to the existence of a discontinuous sanctions regime, with qualitatively more severe punishments on the adult side, as compared to the juvenile side. Section 2.8 concludes.

2.2 Data

The six data sets analyzed in this paper are of two main types: administrative arrest records and longitudinal surveys on criminal behavior. The administrative data contain arrest records compiled at the agency, state, and national level, while the longitudinal surveys follow specific populations over time.

Table 2.1 summarizes the main features of each data set. The first four data sets are administrative records. ASR refers to the national database of age, sex, and race characteristics of arrests in the United States tabulated by the FBI in the Uniform Crime Reports. The ASR strives to contain the universe of all arrests in the United States for the years 1960–2005, though there are inconsistencies in individual agencies' reports (explored in more detail below). The appeal of these data is the national coverage, but its weakness is that arrestees' ages are reported at the year level. The administrative records from Florida, New Orleans, and Oakland prove more useful for the discontinuity analysis because the age of arrestee is reported at the day level. The New Orleans and Oakland data sets come from individual agencies, while the Florida data is compiled by authorities at the state level. The Florida results reported here come from Lee and McCrary (2009).

The last two data sets are longitudinal surveys. The 1997 National Longitudinal Survey of Youth (NLSY) is nationally representative of youths aged 12 to 16 as of 1997 and contains several questions on individual and neighborhood crime. This paper focuses on the section

[2]See Lee and McCrary (2009) for interpretation.

that contains self-reported arrest records which includes data on the specific charge. The NLSY also contains data on self-reported criminal acts, though these are less useful because the age at the time of the act is not clear. The Philadelphia data follows a cohort born in Philadelphia in 1958. The data include socio-economic indicators and details of each offense until age 30, including victimization indicators. Both the NLSY and Philadelphia data report birthdate and age of arrest at the monthly level.

The strength of the NLSY and Philadelphia data sets is that the individual likelihood of arrest for a given month can be directly computed for a representative sample (national, or city-level). On the other hand, the administrative records, with the exception of ASR, offer greater precision in the age at arrest variable and so are more suited for discontinuity analysis. Furthermore, they contain individual identifiers, so arrest hazards for multiple offenders can be computed. The main weakness of all but one of these data sets is the absence of measures of expected or actual punishment for each crime, which would be necessary to determine empirical magnitudes of deterrence. The exception is New Orleans, which includes data on sentence conferred as well as prison time actually served, though the latter are low quality. The sentencing data convey a clear increase in sentence lengths at the age of majority, and are analyzed in section 2.7.

2.3 Estimation

An individual's hazard of arrest is modeled with the logit

$$P(Y_{it} = 1 \mid t) = F(g(t) + \theta D_t)$$

where Y_{it} is one if individual i is arrested in period t for the first time since one year prior to the age of majority, $F(z) = \exp(z)/(1 + \exp(z))$, $g(t)$ is a continuous function of t, D is an indicator for being a major, and t is time expressed in units of weeks or months (depending on the data set) since the age of majority. The time window is limited to one year before and after the age of majority The parameter θ measures the discontinuity in the log odds of being arrested at the age of majority. In the main results, $g(t)$ is approximated with a third degree polynomial, $\beta_0 + \beta_1 t + \beta_2 t^2 + \beta_3 t^3$, and the estimated change in the hazard at the age of majority,

$$F(\widehat{\theta} + \widehat{\beta}_0) - F(\widehat{\beta}_0),$$

is reported.

The discontinuity in the likelihood of arrest (as opposed to hazard) is modeled similarly, except Y_{it} is equal to one if i is arrested in period t, regardless of whether or not it is for the first time since one year prior to the age of majority. In practice, the estimates of the hazard and likelihood discontinuities are very similar.

It is not possible to estimate either of these discontinuities in the national ASR data because it does not include individual identifiers. Furthermore, the age of arrestee is measured in years—too coarse to infer the discontinuity at the age of majority as an instantaneous change. Instead, the discontinuity estimate will conflate the instantaneous change (which we interpret as the deterrence effect) with the secular change in criminal propensity over the year before the age of majority.

Nevertheless, the presentation of results begins with using the national ASR data to estimate the discontinuity of per capita arrests at the age of majority. The baseline specification uses state-level arrest numbers for three years before and after the age of majority to estimate the equation

$$Y_{jast} = \alpha_{jt} + \gamma_{jt} a + \phi_{jt} D_t + \pi_{jt} a \times D_t + u_{jast}$$

where Y_{jast} is arrests per 100,000 persons for offense j, of age a, in state s, and in year t. The equation is estimated with weighted least squares, where the weight is the normalized product of state-year population and the triangle kernel centered at the age of majority. The triangle kernel is chosen because it is efficient for estimating boundaries of a distribution. Formally, denoting the right hand side variables X, the weighting matrix W, and the vector of coefficients $\boldsymbol{\beta}$, we have

$$\widehat{\boldsymbol{\beta}} = (X'WX)^{-1}X'WY$$

where

$$\mathbb{V}(\widehat{\boldsymbol{\beta}}) = (X'WX)^{-1}X'W\Sigma W'X(X'WX)^{-1},$$
$$\Sigma = \text{diag}\left\{\mathbb{V}(Y_{jast})\right\},$$

and

$$\mathbb{V}(Y_{jast}) = Y_{jast}(1 - Y_{jast})/n_{jast}.$$

The parameter π_{jt} measures the discontinuity in arrests per 100,000 at the age of majority for offense j in year t.

2.4 Results from ASR

The ASR data contains, in principle, all arrests for the years 1960–2005, and are therefore potentially the most informative. However, it is also the coarsest of all data sets in that age of arrestee is recorded at the year level. This section reports regression discontinuity results, first for all states pooled, and second for individual states around years in which the age of majority changed. These states (Alabama, New Hampshire, and Wisconsin) offer a natural experiment with which to measure the effect of increasing the severity punishment.

2.4.1 Full sample

This section focuses on murder arrests around the age of majority using the age, race, and sex tabulations from the Uniform Crime Reports. Other offenses show similar trends and are available upon request.

Figures 2.1 and 2.2 present age profiles of murder arrests from 1960 to 2005 for states with age of majority at 17 and 18, respectively.[3] Each plot within the figures is a histogram of age of murder arrestee for a specific year. The most striking trend in these distributions is the growing spike at 18 to 20 years of age. There is a similar trend for the same plot of histograms for all arrests (not shown).

Table 2.2 lists discontinuity estimates of murder arrests at the age of majority. Before computing the estimator, the data were first aggregated to the age-state-year level (e.g., number of arrests of 16-year-olds in California in 1995), and each observation was normalized by the age-state-year population. Table 2.2 lists the estimates separately for states with age of majority 17 and 18. The sample is limited to three years before and after the age of majority.[4] The estimates range from about -6 to 21, do not reveal any clear positive or negative trends, and are only occasionally significant. For reference, there were approximately 17 murder arrests per 100,000 18-year-olds in 2005.

2.4.2 Case studies

This section presents estimates of changes in arrest rates in states experiencing changes in the age of majority. The comparison is between age cohorts which experienced different punishment regimes in the same state (e.g., 17-year-olds in time period 1 versus 17-year-olds in time period 2, given a change in age of majority from 18 to 17 in year 2).[5] Of course, it is not possible to also include controls for years in a linear regression setting because of colinearity with cohort and age. The results are generally inconclusive because of low power from small samples and because the ASR data are unfortunately often missing around years in which the age of majority changed.

2.4.2.1 New Hampshire and Wisconsin

New Hampshire and Wisconsin lowered their age of criminal majority from 18 to 17 in 1996. In this case, the cohort that turned 17 in 1996 was the first to experience criminal majority

[3] The year 1973 is also excluded due to data cleaning issues.

[4] For states with age of majority at 17, it is two years before and three years after. This is because the data for arrests at age 14 is reported in the ASR as the total for ages 13 and 14. The three states with age of majority at 16 are not included.

[5] These are not cohorts in typical sense in that they do not constitute mutually exclusive populations. A person may be arrested in Wisconsin in 1996 twice, once when at 17 and again 18. This person would be noted in the data as belonging to two cohorts.

at 17, as opposed to the cohort that turned 17 in 1995 which were treated as minors.

Figures 2.3 and 2.4 present distributions of age of arrestee for Wisconsin and New Hampshire. The vertical lines mark ages 17 and 18. The histogram in the center of each figure (1996) is the first year in which the age of majority decreased from 18 to 17. Between 1995 and 1997, there appears to be a slight decline in the share of arrests of 17 relative to 18-year-olds, though this may be simply a continuation of previous trends (data from 1998 and 1999 are missing). Unfortunately, data from New Hampshire are missing for 1995, the year before the change. New Hampshire is therefore excluded from further analysis. Figures 2.5 and 2.6 give time series of number of arrests by age for each state, and expose additional data reporting issues after 1998. Data for Wisconsin is therefore limited to pre-1998 in the following analyses.

Figure 2.7 gives arrests per person in Wisconsin by cohort. The x-axis is years since turning 17 and the y-axis is total number of arrests. Cohorts that turned 17 before or after 1996 are subject to criminal majority at 17, while those turning 17 before 1996 are still minors at 17. The discontinuity estimate listed on the x-axis is computed with a simple ordinary least squares regression using the points on the graph, and indicator for being both 17-years-old and belonging to a cohort that experienced the change in punishment regime. Formally, it is the estimate of ϕ from the equation

$$Y_{ca} = \alpha_c + \beta_1 a + \beta_2 a^2 + \phi I(c \leq 17 \wedge a = 17) + v_c$$

where Y_{ca} is arrests per person of cohort c at age a, α_c is a cohort-specific intercept, and v_c an error term. The cohort, c, is expressed in terms of age at 1996.

Figures 2.8, 2.9, and 2.10 present the same graphs for Milwaukee, Madison, and the rest of Wisconsin separately. The y-axis is total number of arrests in thousands. The Madison estimate is noisy, while the rest of Wisconsin estimate is a fairly precise zero (-0.21 with standard error 0.95). The rest of Wisconsin estimate only includes the 70 percent of agencies that report numbers for each year represented on the graph (1990-1997).

Milwaukee, the largest city in Wisconsin, is the only one of the three groups for which there is a significant change in the total number of arrests. The discontinuity estimate is -2.4 with a standard error of 0.5, a decrease of roughly 50 percent from the total arrests of previous cohorts at age 17. However, this is not a pure deterrence effect. Since age is measured coarsely, it is at least a conflation of deterrence and incapacitation. It may also include responses to the law change by law enforcement or criminal justice authorities. In addition, since data are missing in Wisconsin for years 1998–2001, there are only two post-majority years available for analysis from the first cohort to experience the new regime (1996 and 1997), and only one year available for the second cohort (1997). Finally, the pervasive issues in reporting suggest any estimate should be interpreted with caution.

2.4.2.2 Alabama

Alabama raised its age of majority from 16 to 17 beginning 1976, and then again to 18 in 1977. This change gave the cohort that was 16 in 1976 two additional years of minor status, as opposed to the cohort that was 17 in 1976, which experienced no change in punishment regime. Figure 2.11 presents age profiles for all arrests in Alabama around the years of the change in law. The vertical lines indicate ages of 16, 17, and 18. As mentioned above, 1973 is omitted because the ASR data is likely compromised at the national level for that year.

The discontinuity is estimated similarly to Wisconsin, except that the regression includes an indicator for belonging to a cohort that experienced the change and was either 17 or 18. Formally, it is the estimate of ϕ from the equation

$$Y_{ca} = \alpha_c + \beta_1 a + \beta_2 a^2 + \phi I(c \leq 17 \wedge (a = 17 \vee a = 18)) + v_c$$

This equation is estimated using the points on figure 2.12, and uses only agencies that report for all years represented on the graph (1972–1981). This is only 7 percent of all Alabama agencies, but together they account for about one quarter of total arrests reported in Alabama. The discontinuity estimate is, counterintuitively, negative and significant: -0.37 with standard error 0.12, suggesting a decrease in arrest numbers in response to a decrease in punishment levels. Like the estimate for Wisconsin, and unlike the analysis below which uses arrest data at the month and day level, this estimate conflates deterrence and incapacitation effects, along with possibly other responses by law enforcement officers.

2.5 Main arrest hazard results

This section presents the main arrest hazard results from each data set. The administrative data sets (Florida, New Orleans, and Oakland) all have exact day of birth and day of arrest. The nonparametric arrest hazards, indicated by circles in figures 2.13, 2.14, and 2.15, are constructed by first limiting the sample to individuals with at least one recorded arrest prior to the year before age of majority. The weekly hazard of first arrest since one year prior to age of majority is computed for each individual and then aggregated to the population average. The individual weekly hazard data are used in the parametric logit model detailed in section 2.3 to compute the discontinuity estimate. The logit function includes an indicator for being a major and a third degree polynomial trend in weeks since becoming a major. In the figures below, the logit fit is overlaid on the nonparametric weekly hazard population averages to assess goodness of fit and the underlying variance.

Figures 2.13, 2.14, and 2.15 include only arrests for felonies because officers may use more discretion in the decision to arrest for lesser offenses, and possibly discontinuously so at the age of majority.[6] This discretion is less likely to be exercised for major offenses. The

[6]For New Orleans, only arrest data for burglary and robbery are available.

discontinuity estimates of the weekly arrest hazard (and standard errors) for Florida, New Orleans, and Oakland are -0.0000 (0.0000), 0.0003 (0.0004), and -0.0004 (0.0006). Relative to the nonparametric arrest hazard around the age of majority (around 0.001 to 0.003 for each of the data sets), these are either economically or statistically insignificant.

Figures 2.16 and 2.17 show the results for NLSY and Philadelphia. These data sets record date of birth and date of arrest at the month level, so observations on month zero (relative to age of majority) may be before or after becoming a major. Figures 2.16 and 2.17 count month zero as post-major, and the estimates for NLSY and Philadelphia are both noisy: 0.0005 (0.0009) and 0.0008 (0.0005). In figures 2.18 and 2.19, month zero is excluded from the logit fit, and there is little change in either estimate.[7]

2.6 Robustness checks and extensions

2.6.1 Likelihood versus hazard of arrest

Figures 2.20–2.23 present the same results for NLSY and Philadelphia except using the monthly likelihood of arrest. The dependent variable in the logit model is now equal to one if the individual was arrested in that month (with multiple arrests counting as one). In the hazard model, the dependent variable is one only for the first arrest since one year prior to the age of majority. The results exhibit little change.

2.6.2 Formal charges and severity of charges

The NLSY includes data on the whether authorities brought formal charges against the arrestee. Figure 2.24 shows the hazard of having formal charges brought against the arrestee (after an arrest under any charge). This is a situation in which the authorities are likely to use significant discretion. The two standard error confidence interval is (0.0001, 0.0033), and the nonparametric hazard estimate at the age of majority is about 0.003. This marginally significant effect may come from authorities treating minors with more leniency in the decision to bring formal charges. The results are positive and slightly less precise for the likelihood (figure 2.25) and when month zero is excluded (figures 2.26 and 2.27).

The Philadelphia data includes a severity index based on the arrest charge code, victimization, and types of weapons used. Unfortunately, the documentation does not include the details of the index construction. However, a glance through the data suggests it is highly

[7]The level of detail and specific categories of crime codes differ for each data set. The crimes included from the NLSY are assault, robbery, burglary, theft, and destruction of property For Philadelphia, homicide rape robbery aggravated assault, burglary, theft (including auto theft), arson, forgery/counterfeiting, fraud, embezzlement, stolen property (buying, receiving, possession), vandalism, weapons, prostitution, and sex offenses. Section 2.6 estimates the discontinuity for individual crimes and other groups of crimes (e.g., the most severe only), with little qualitative change in the results.

correlated with the severity of the charge (the most severe are all murder or rape) and extent of victimization. The baseline graph above uses the index level of drunk driving (2.7 and above) as the cutoff for the definition of an offense/arrest. Figure 2.28 gives a histogram of the severity index. The spike after 10 compromises mostly drug-related offenses. Figure 2.29 plots the discontinuity estimate and two standard error band as the threshold for offense becomes increasingly severe. The estimates are reported in terms of fraction increase from the offense hazard at age of majority and are generally insignificant. Figure 2.30 plots the same results for the estimates of the discontinuity in the likelihood of arrest, while figures 2.31 and 2.32 report the results when month zero is excluded. These results are all similar.

Finally, figures 2.33 and 2.34 show the hazard results from Oakland and the NLSY when all arrests are included, for which the officer may excerise the most discretion. The Oakland estimate is approximately zero with two standard error confidence interval (-0.0012, 0.0017). The NLSY is positive though statistically insignificant (-0.0004, 0.003). Similar results are obtained for the likelihood and when month zero is excluded (not shown).

2.6.3 Censoring

Throughout the estimation, individuals that are censored are treated as missing beginning the day (or month) that the individual was censored. In the administrative data (Florida, New Orleans, and Oakland) an individual is censored if she became a major within one year of the day the data was collected. Such individuals are treated as censored at random, and contribute to hazard calculations until the day before being censored. Since the (potential) censoring date is the same for everyone, there seems little reason to suspect that it is discontinuously selective at the age of majority.

The NLSY suffers from additional kinds of censoring. An individual is also censored if he is lost to followup of if he migrates to a state with a different age of majority within one year of becoming a major. Figure 2.38 plots the c.d.f. of individuals' censor dates. The censoring due to migration is approximately twice that of loss to followup. At the age of majority, about 5 percent of observations are censored for one of these reasons. Though there is no indication of discontinuity in censor dates around the age of majority, censoring may bias the estimates if it there is a specific selection mechanism that changes at the age of majority. For example, estimates would be downward biased if youths at greater risk of being arrested disproportionately migrate after becoming a major, while lower-risk youths discontinuously decrease migrating habits at the age of majority. Such a mechanism, however, seems unlikely.

2.6.4 Self-reported crime

The NLSY contains responses for self-reported crimes. At face value, these would seem more useful than arrests since they represent actual rather than alleged criminal acts. Unfortunately, it is not possible to determine the age of the respondent at the time of the reported acts, since responses are in terms of number of criminal acts since the date of the last interview. Nevertheless, this section uses these data to calculate a rough estimate of the discontinuity in self-reported criminal acts. The data come from responses to the question "How many times did you steal something worth ≥ 50 dollars since last interview?"

The data are first pro-rated over the individual's age in whole years according the percent overlap with the period since the last interview. Then a 'naive' regression is run with the resulting collapsed data. For example, if an individual is interviewed at 16.8 and 17.8 years of age and reports 2 criminal acts during that time, then 0.4 of the acts are assigned to 16 and 1.6 to 17. Figures 2.39 and 2.40 do this separately for age of majority at 17 and 18, while figures 2.41 and 2.42 change the dependent variable to an indicator for any reported acts. As expected, the results are inconsistent and mostly insignificant. Figure 2.43 shows how little the interview periods overlap with individuals' ages in whole years. It shows the distribution of the fraction of the interview span (typically about one year) that overlaps with a year of an individual's age. A value of one indicates perfect overlap. The distribution is roughly uniform between zero and one.

2.7 Sentence length

The discontinuity results on sentence length are presented last because there are only few data available on sentences conferred and time actually served. The New Orleans (administrative) and NLSY both include sentencing data, though only parts of the former prove informative.

2.7.1 Probability of any jail

The New Orleans data include the length of the sentence conferred, while the NLSY directly asks questions on sentencing, though the latter's small numbers yield little power. Figure 2.44 shows the New Orleans results of a simple linear regression of an indicator for receiving any jail time on date of arrest in days since becoming a major. The regression includes separate left and right linear trends in days since becoming a major and an indicator for being a major. The sample is burglary and robbery arrests (the only data available from New Orleans) and excludes individuals with sentences exceeding five years. Figure 2.45 excludes only those receiving sentences greater than 99 years, though this is only a few cases. Both figures display similar results, with the probability of jail jumping from about 5 to 35 percent at the age of majority. Both discontinuity estimates are significant at conventional levels.

The NLSY results on sentencing are too noisy to draw any conclusions. Figure 2.46 shows the probability of jail for those that were convicted or plead guilty to any charge, figure 2.47 shows the probability of being sent to an adult or juvenile corrections facility, and figures 2.48 and 2.49 exclude month zero in the logit fit. Figures 2.50–2.53 include only those convicted or pleading guilty to relatively severe charges.

2.7.2 Sentence length

Figure 2.54 runs the same regression with the sentence length conferred as the dependent variable, including those sentenced to 5 or fewer years. The expected sentence length conferred on these individuals jumps from about zero to one year at the age of majority, and is highly significant. Figure 2.54 includes those with sentences less than 99 years and is similar.

Figures 2.56 and 2.57 are similar to the previous two, except the dependent variable is now time actually served. This was computed by linking separate flow data from New Orleans prisons to the New Orleans arrest database. Court and booking dates are included in the prison flow data, so each arrest should in theory have a corresponding entry in the flow data. Unfortunately, the juvenile and adult prison flow data were in separate places and had significantly different success in linking with the arrest data. Only about one third of juvenile and one half of adult arrests were linked. The results are smaller in magnitude though otherwise similar to the discontinuity in sentence length conferred.

2.8 Conclusion

This paper presented evidence from six data sets on patterns in criminal behavior at the transition to adulthood. As emphasized in Lee and McCrary (2009), this is a unique episode in an individual's life, where most other factors in the decision regarding criminal participation are similar, but sharp differences in punitiveness are present. Thus, the high frequency patterns in lifecycle participation are tightly linked to the deterrence hypothesis. As our data show, youths experience a large and discontinuous increase in punishment on the day of criminal majority (usually the 18th birthday).

However, the results from each data set either point toward little to no deterrence, or yield estimates that are too noisy to draw meaningful inference. The administrative data with individual identifiers and exact day of birth and arrest—arrest databases from New Orleans, Oakland, and Florida—often give the most precise results, while the longitudinal surveys from Philadelphia and the NLSY, which include date of birth and arrest at the month level, are qualitatively similar in conclusion, but often quite imprecise.

Overall, the results point towards a need for more robust administrative data with information on date of birth, date of offense, and crime type. Such information is available, but not yet available to researchers. However, even now, we can hazard a guess as to the overall pattern. Very large behavioral effects would be detectable, even with the noisy data that we already possess. Consequently, it seems likely that with better data, we would continue to conclude that the lifecycle elasticity is small.

A major question for the economics of crime literature is why this is so. Lee and McCrary (2009) discusses interpretation in more detail, but more work needs to be done to understand this pivotal issue. For now, the only firm conclusions pertain to policy evaluation. Youth do not change their behavior in qualitatively important ways upon transitioning to the more punitive adult criminal justice subsystem. Two important recent policy reforms—changes to the age of criminal majority and transferring greater numbers of juveniles to the adult criminal court—will both have scant deterrence effects.

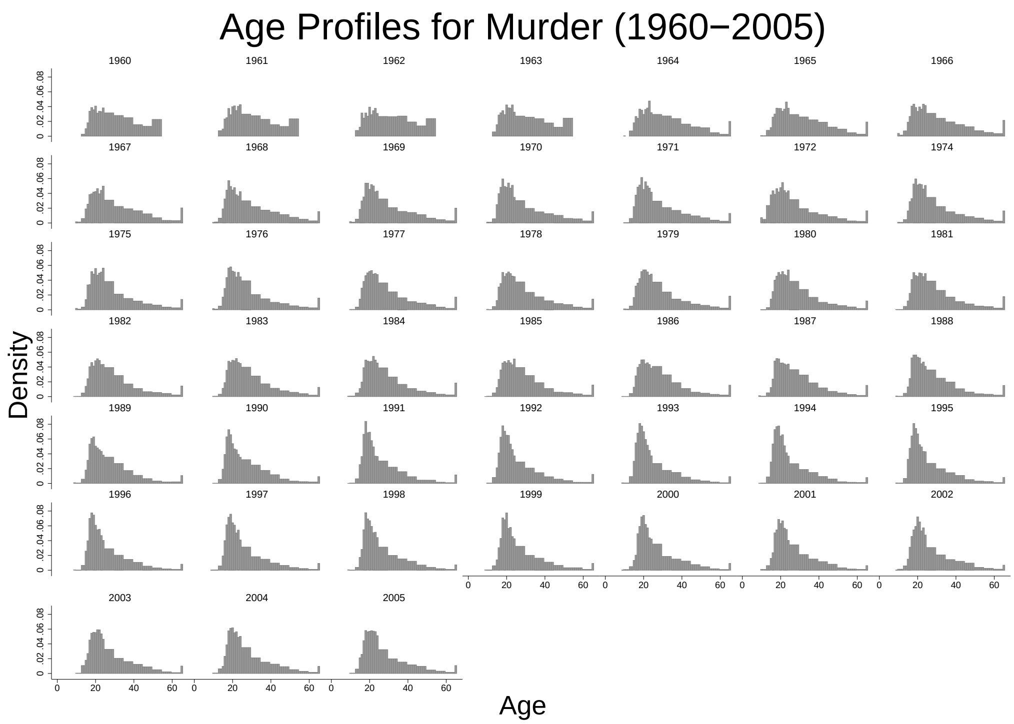

Figure 2.1: Age profiles of murder arrests for states where age of majority is 17

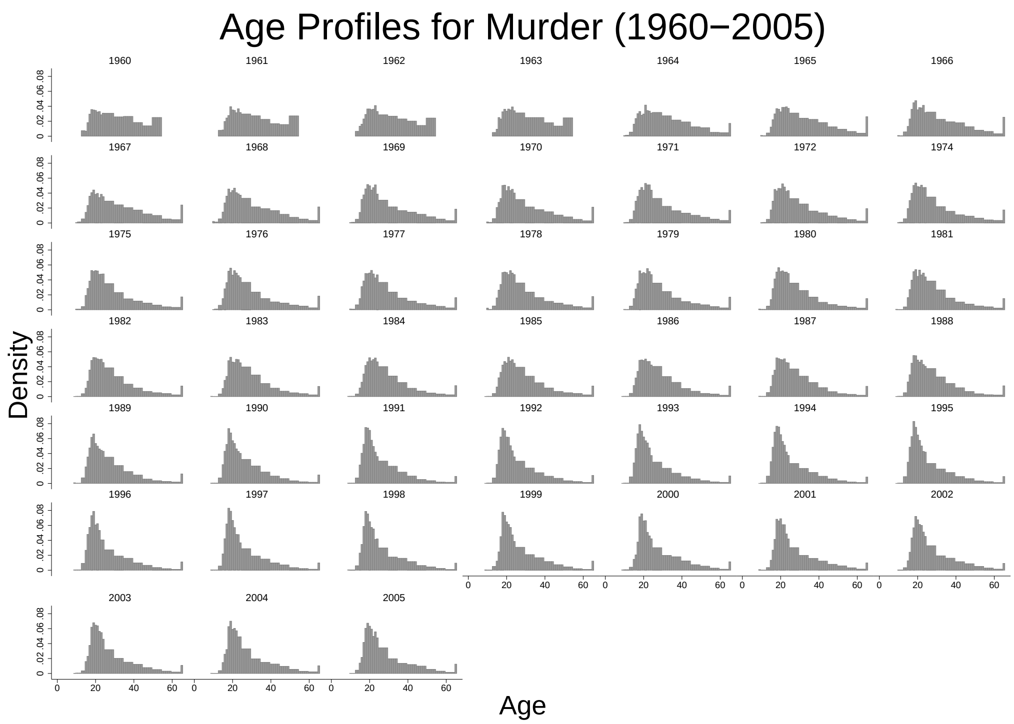

Figure 2.2: Age profiles of murder arrests for states where age of majority is 18

Figure 2.3: Age profiles of all arrests in Wisconsin. Beginning in 1996, the age of majority decreased from 18 to 17. The vertical lines mark the histogram bars of 17 and 18.

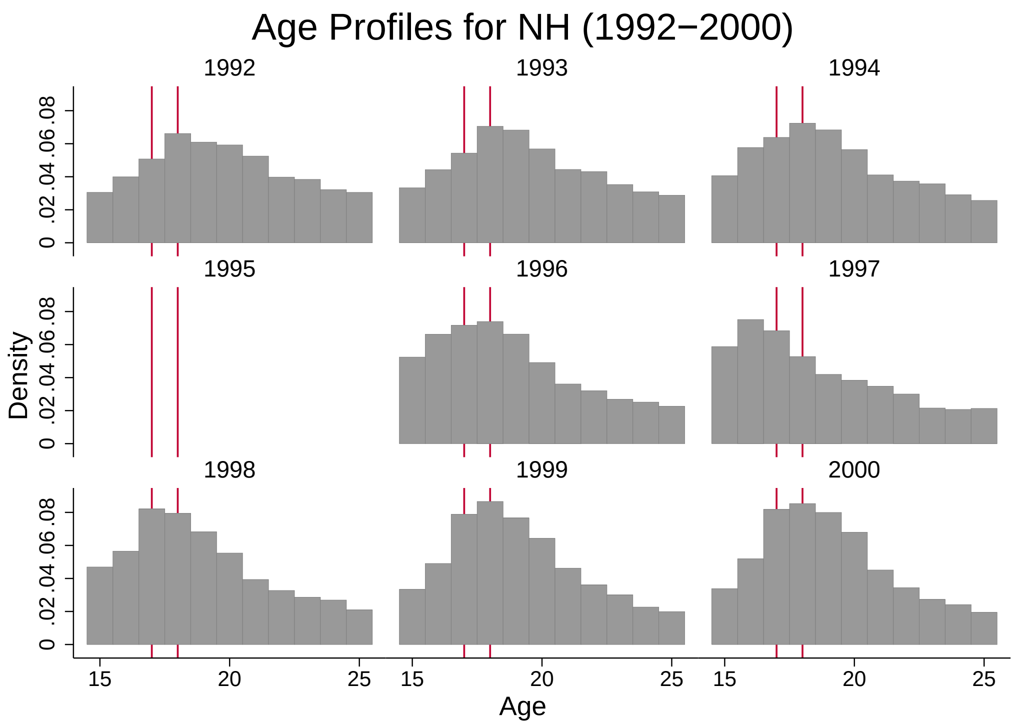

Figure 2.4: Age profiles of all arrests in New Hampshire. Beginning in 1996, the age of majority decreased from 18 to 17. The vertical lines mark the histogram bars of 17 and 18.

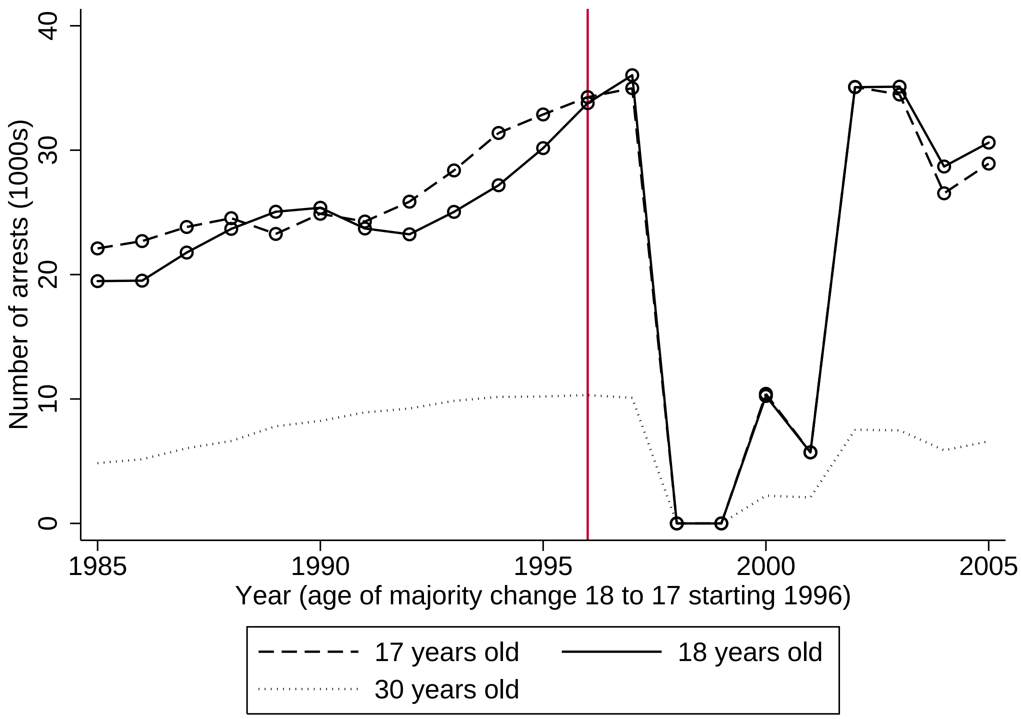

Figure 2.5: Number of arrests by age in Wisconsin. Beginning in 1996, the age of majority decreased from 18 to 17.

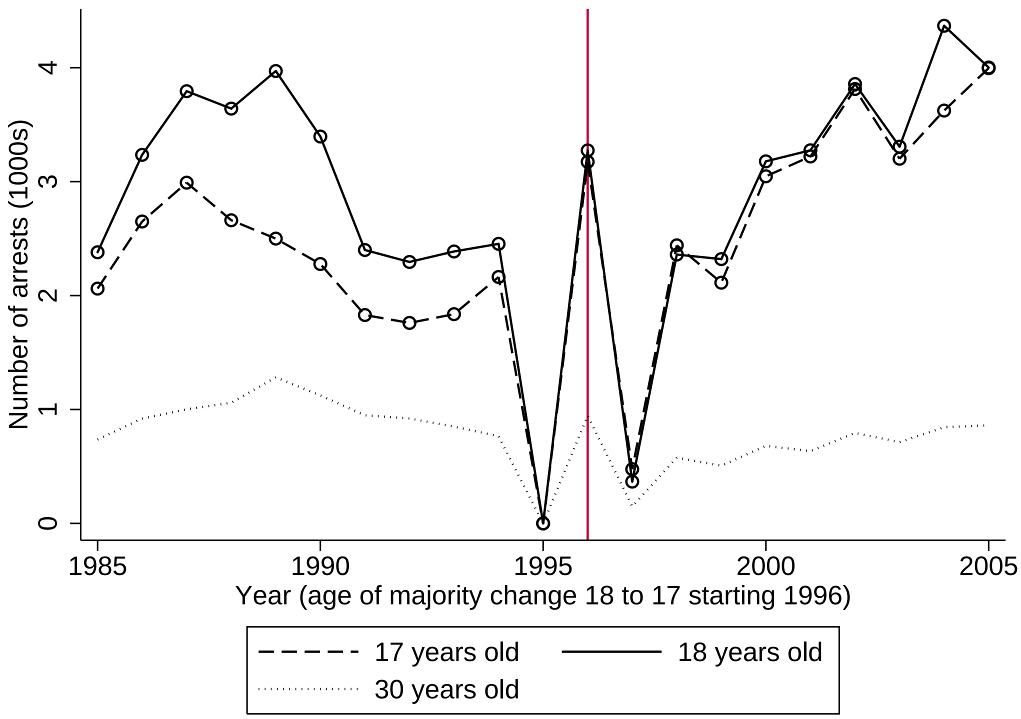

Figure 2.6: Number of arrests by age in New Hampshire. Beginning in 1996, the age of majority decreased from 18 to 17.

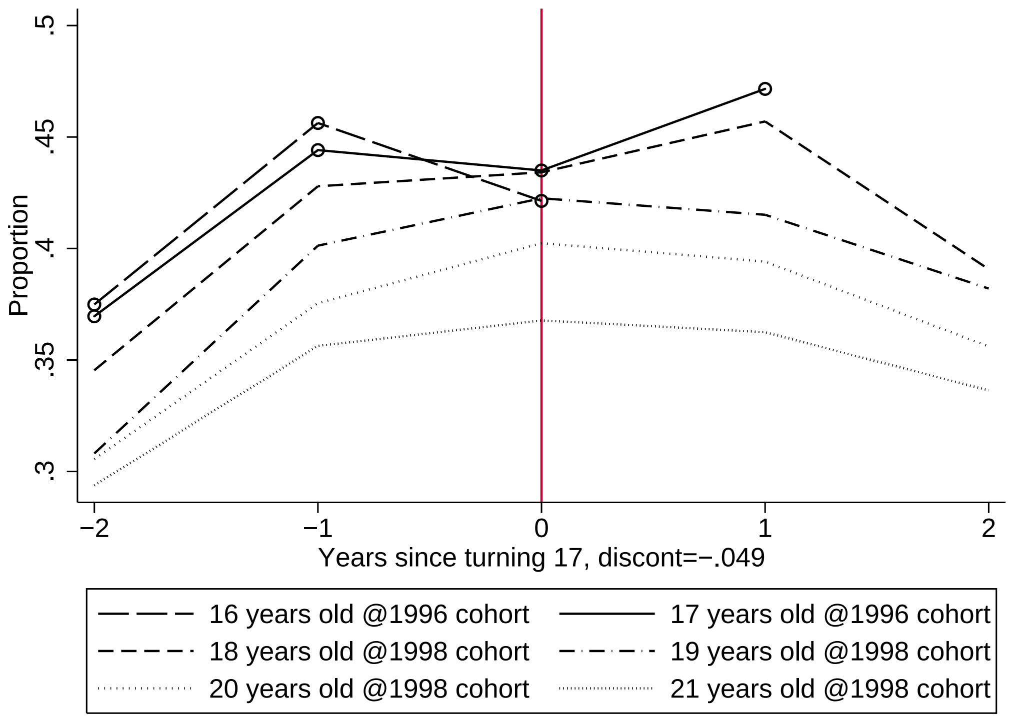

Figure 2.7: Arrests per person in Wisconsin, by cohort

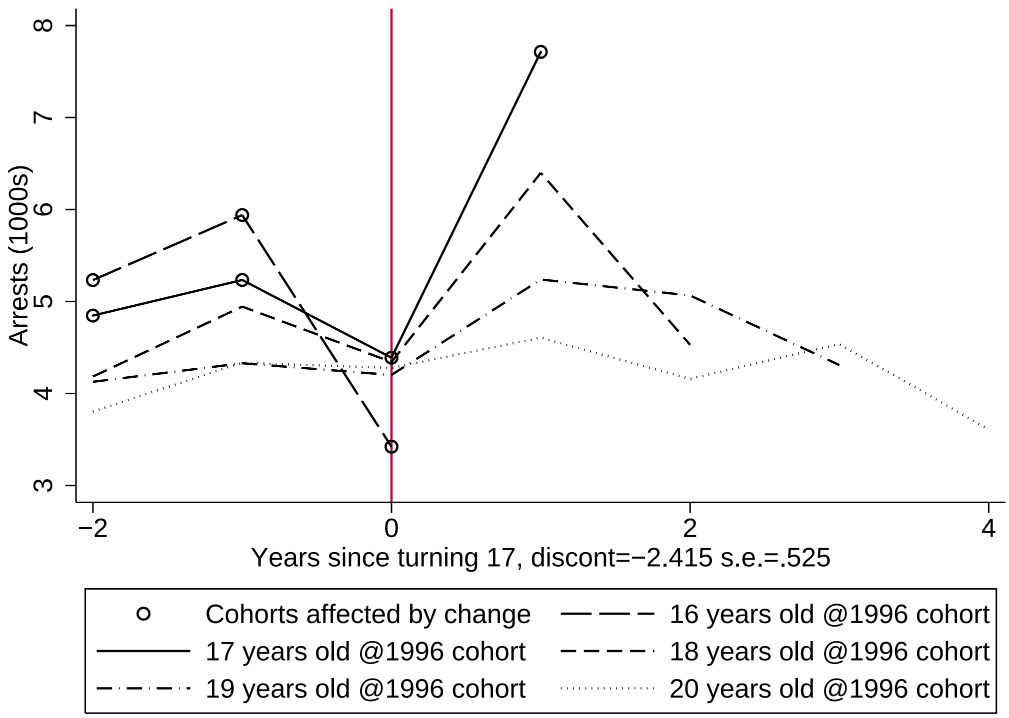

Figure 2.8: Number of arrests by cohort in Milwaukee

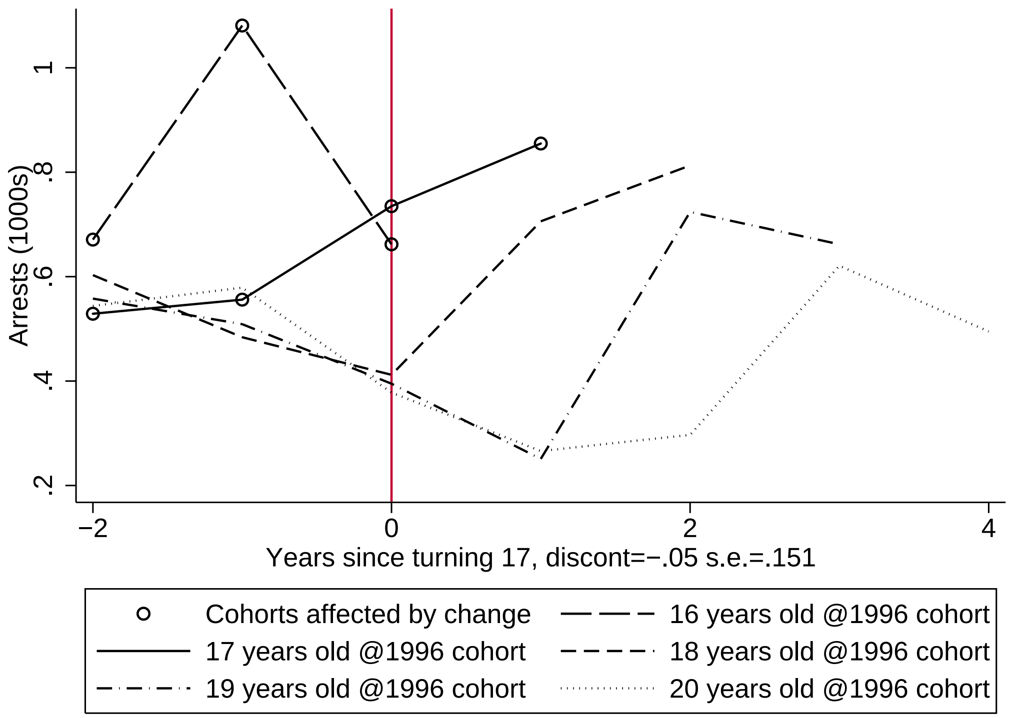

Figure 2.9: Number of arrests by cohort in Madison

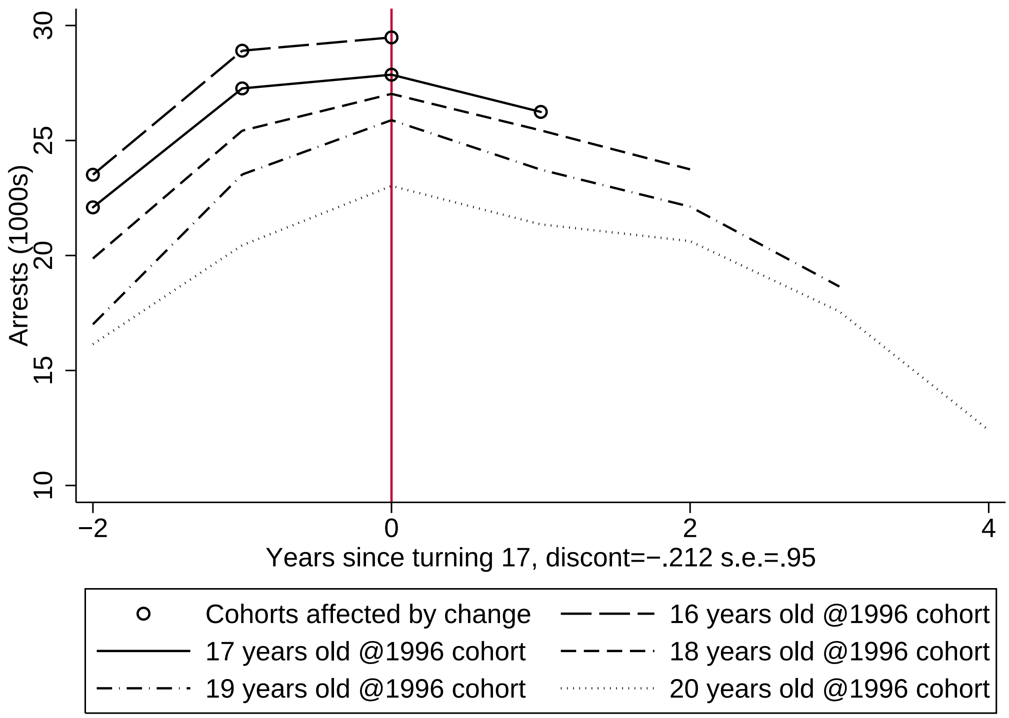

Figure 2.10: Number of arrests by cohort in Wisconsin, excluding Milwaukee and Madison. The sample is further limited to the 70 percent of agencies reporting in all years represented on the graph (1990–1997).

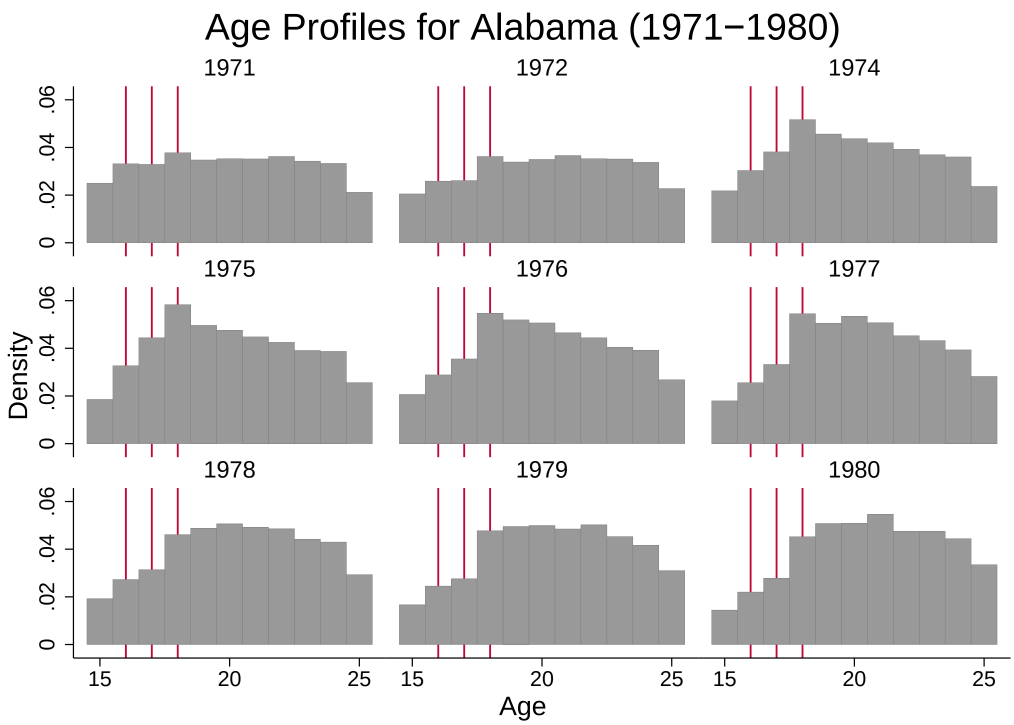

Figure 2.11: Age profiles of all arrests in Alabama. The age of majority increased from 16 to 17 in 1976, and then 18 in 1977. The vertical lines mark the histogram bars of 16, 17, and 18.

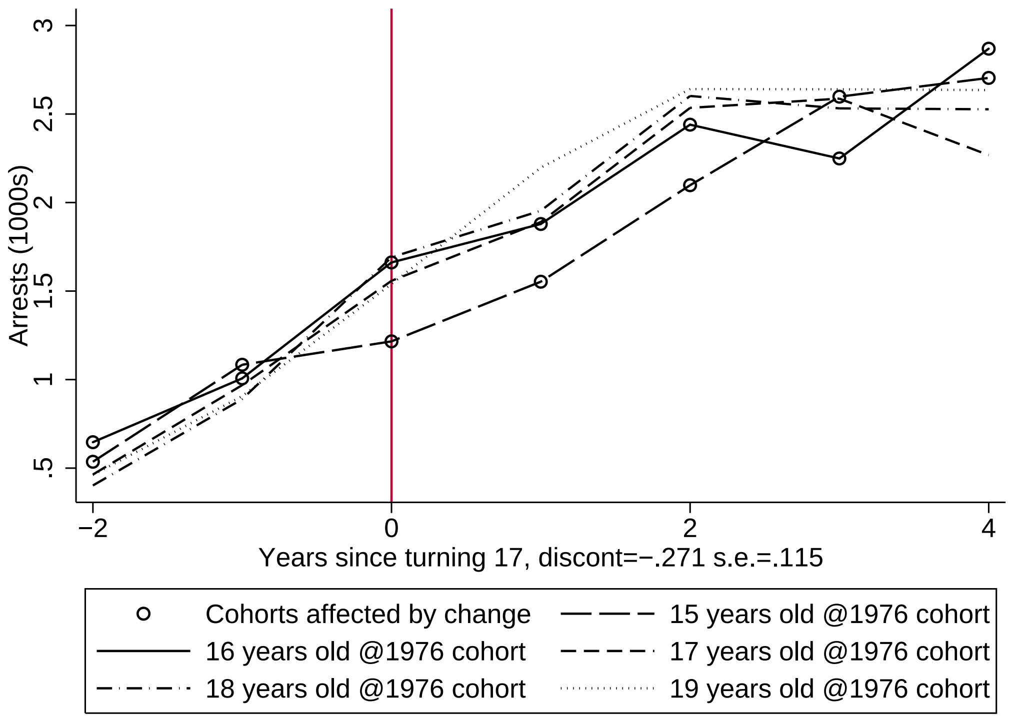

Figure 2.12: Number of arrests by cohort in Alabama. The sample is further limited to the 7 percent of agencies reporting in all years represented on the graph (1972–1981). These agencies account for about one quarter of all reported arrests in Alabama.

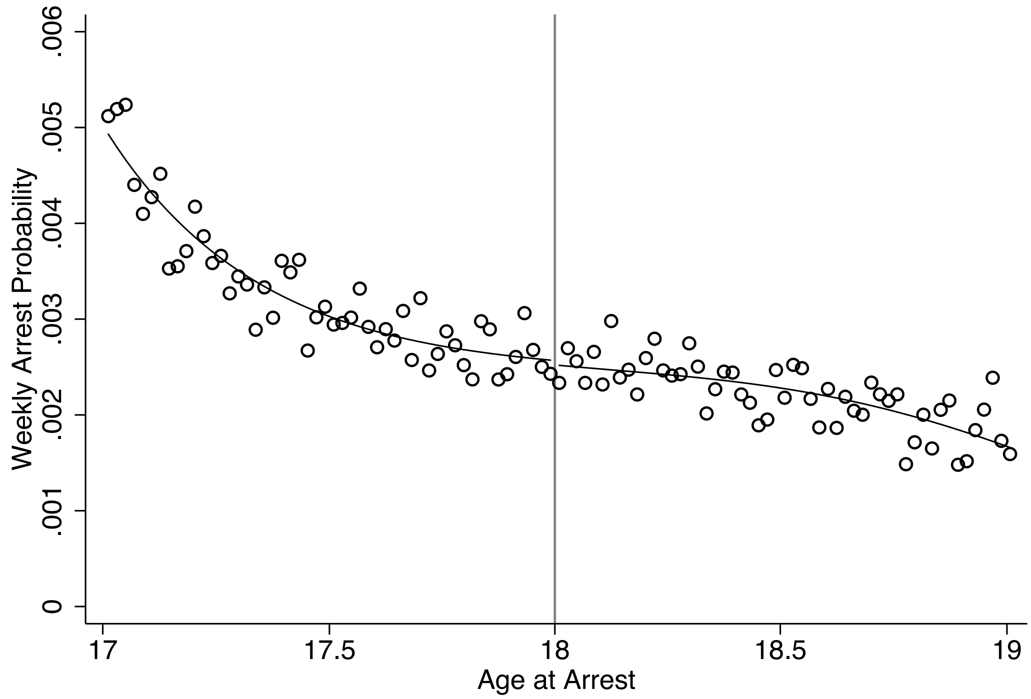

Figure 2.13: Weekly hazard of first felony arrest since age of majority, Florida

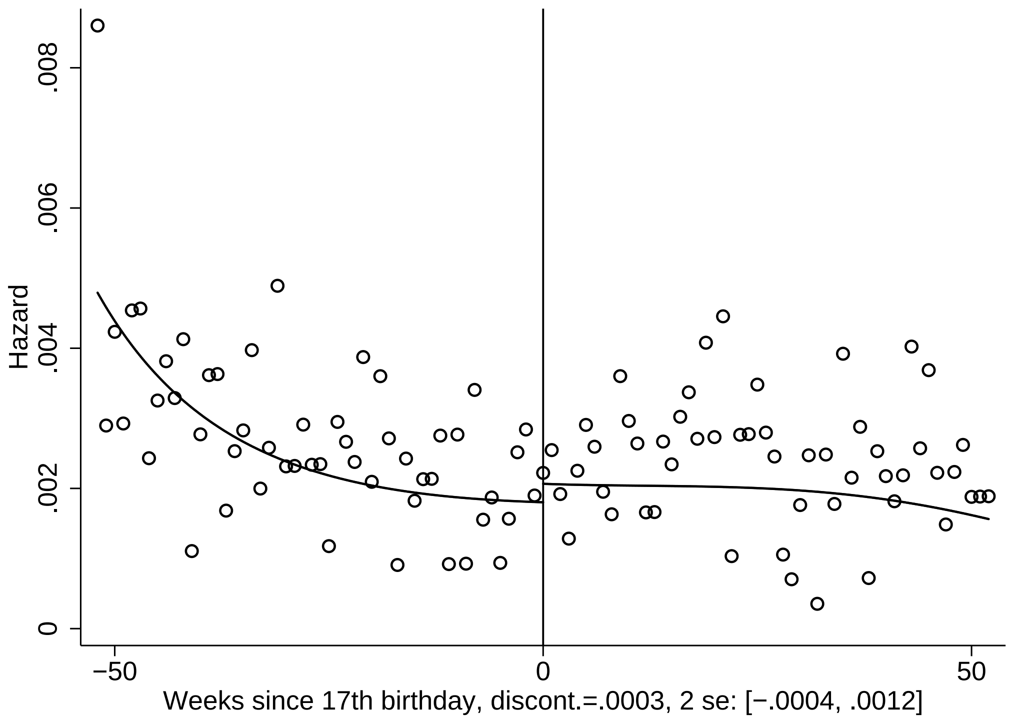

Figure 2.14: Weekly hazard of first felony arrest since age of majority, New Orleans

Figure 2.15: Weekly hazard of first felony arrest since age of majority, Oakland

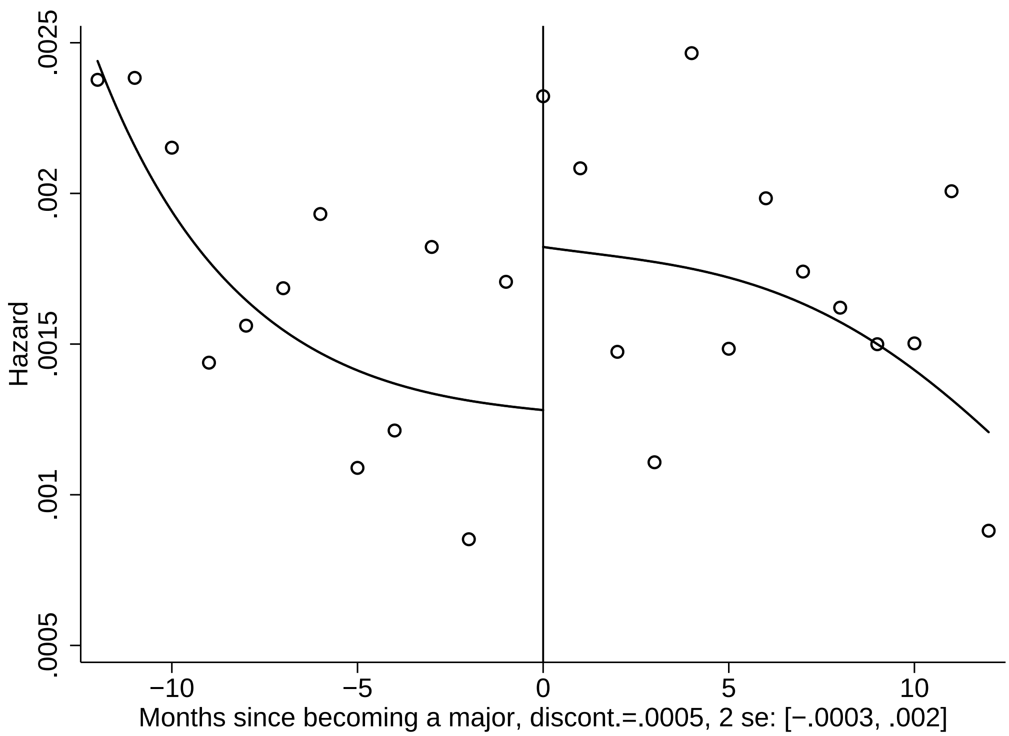

Figure 2.16: Monthly hazard of first arrest with bad charges since year before age of majority, NLSY

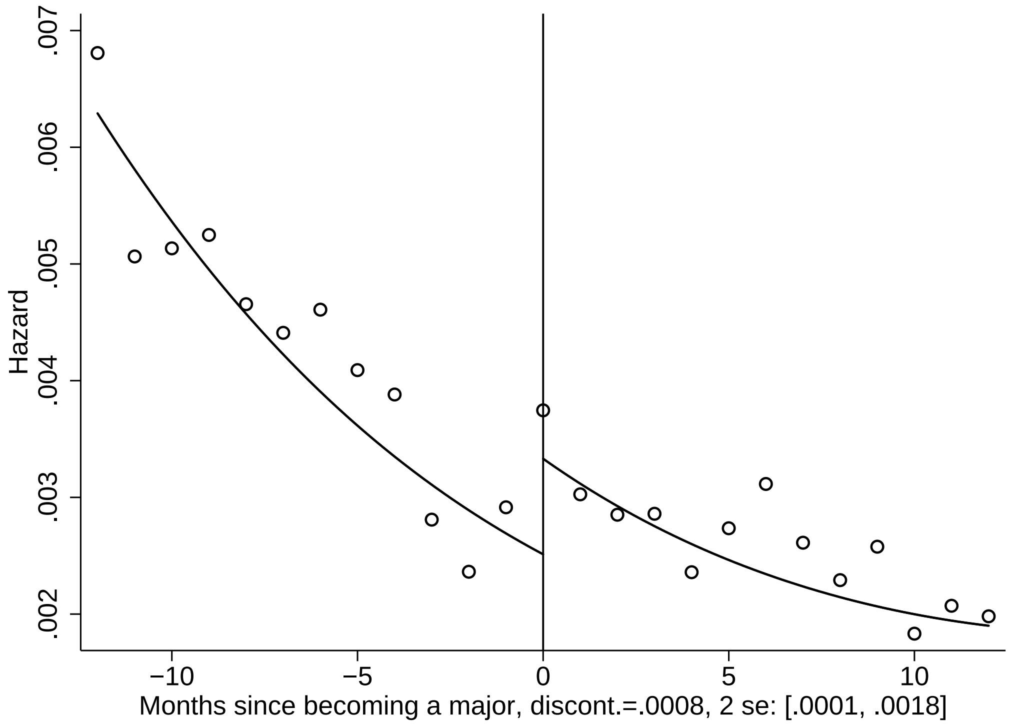

Figure 2.17: Monthly hazard of first arrest since year before age of majority, Philadelphia

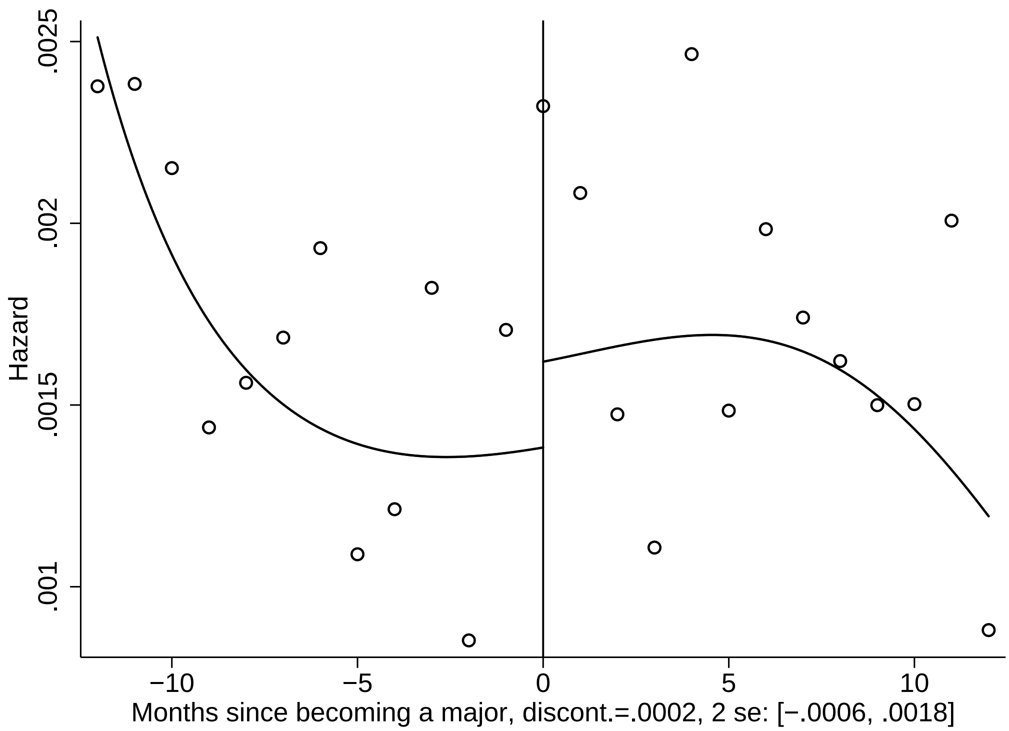

Figure 2.18: Monthly hazard of first arrest with bad charges since year before age of majority, excluding month $= 0$, NLSY

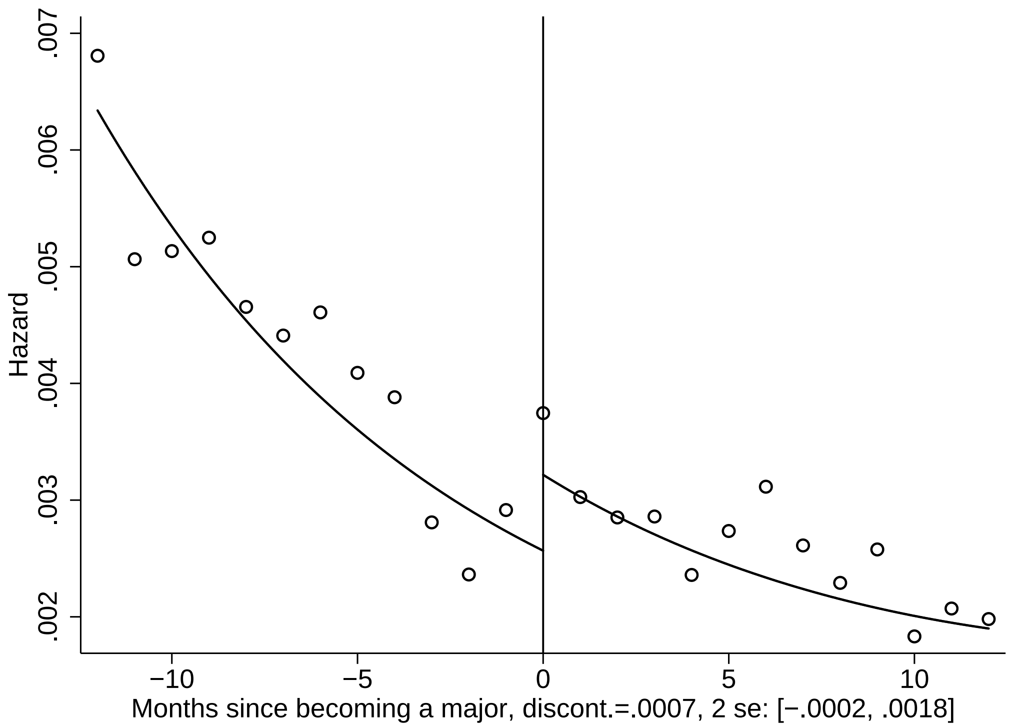

Figure 2.19: Monthly hazard of first arrest with bad charges since year before age of majority, excluding month $= 0$, Philadelphia

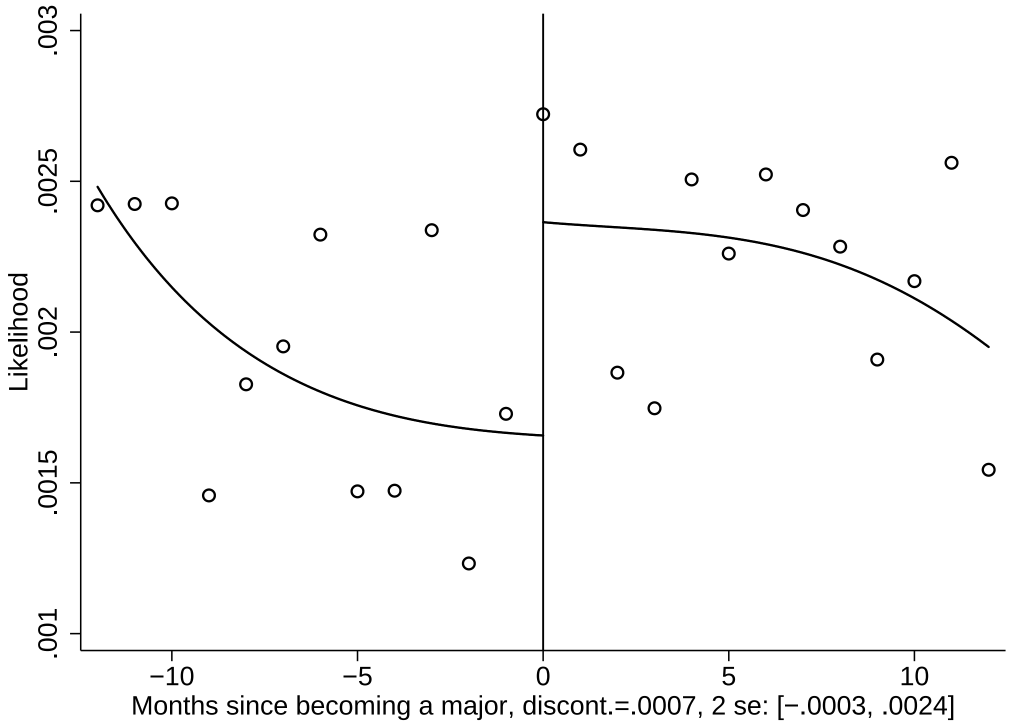

Figure 2.20: Monthly likelihood of arrest with bad charges, NLSY

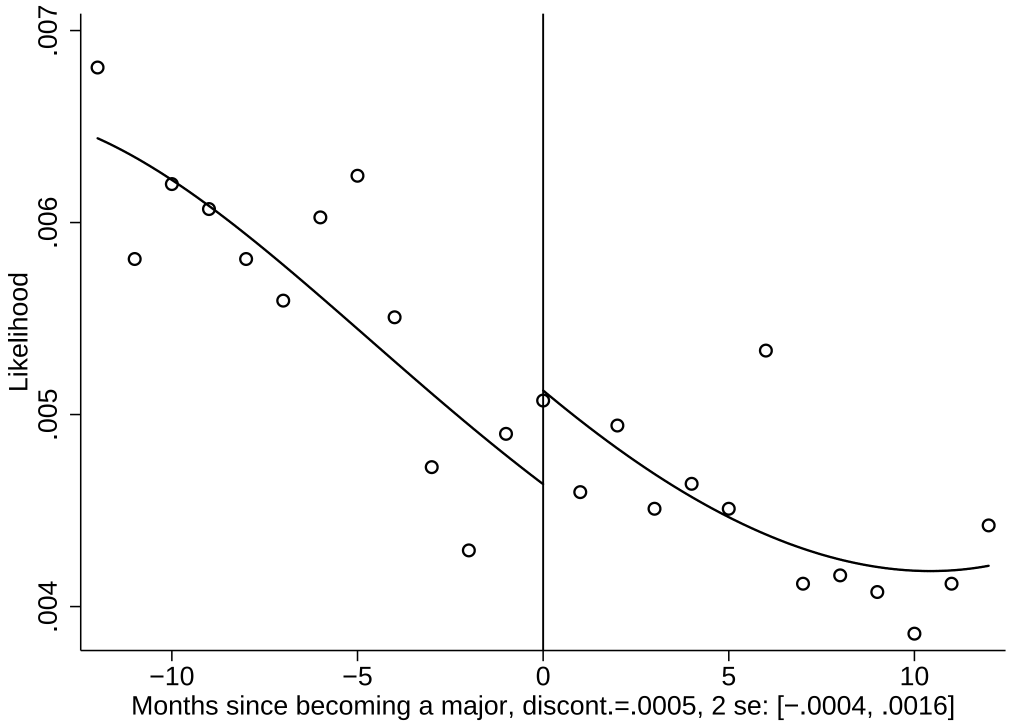

Figure 2.21: Monthly hazard of first arrest since year before age of majority, Philadelphia

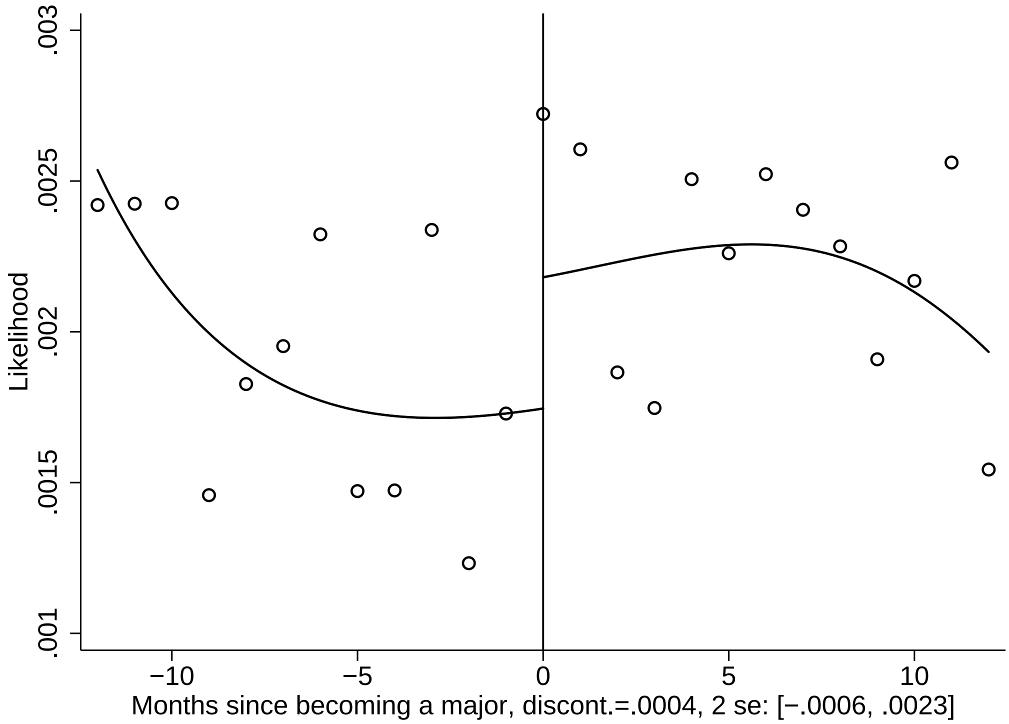

Figure 2.22: Monthly likelihood of arrest with bad charges, including month $= 0$ (top) and excluding month $= 0$ (bottom), NLSY

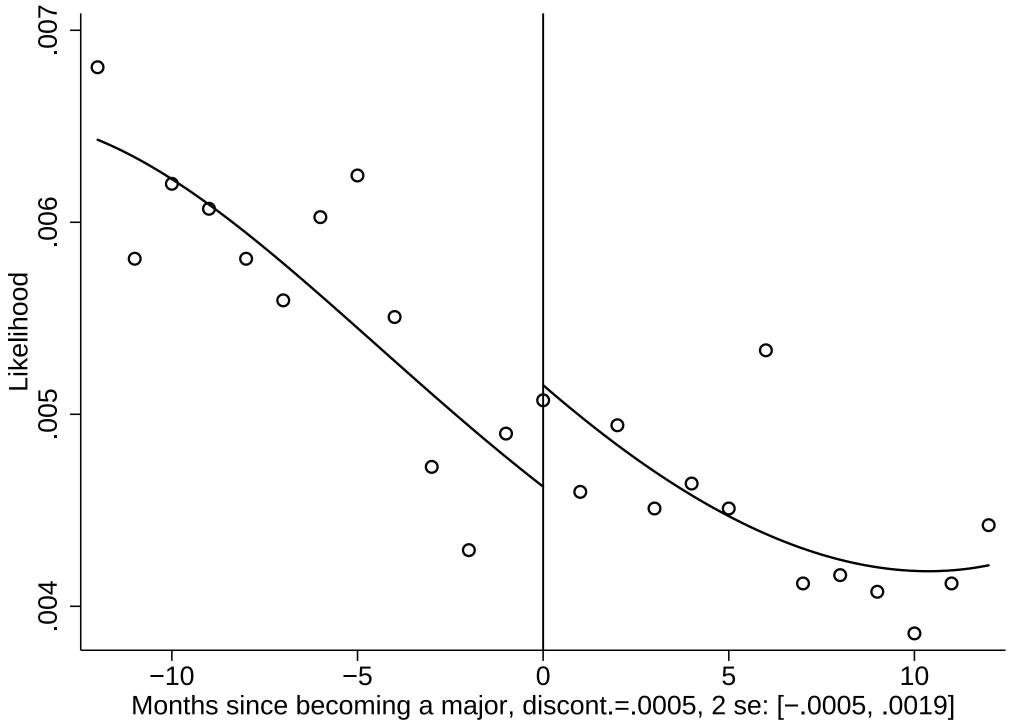

Figure 2.23: Monthly hazard of first arrest since year before age of majority, excluding month $= 0$, Philadelphia

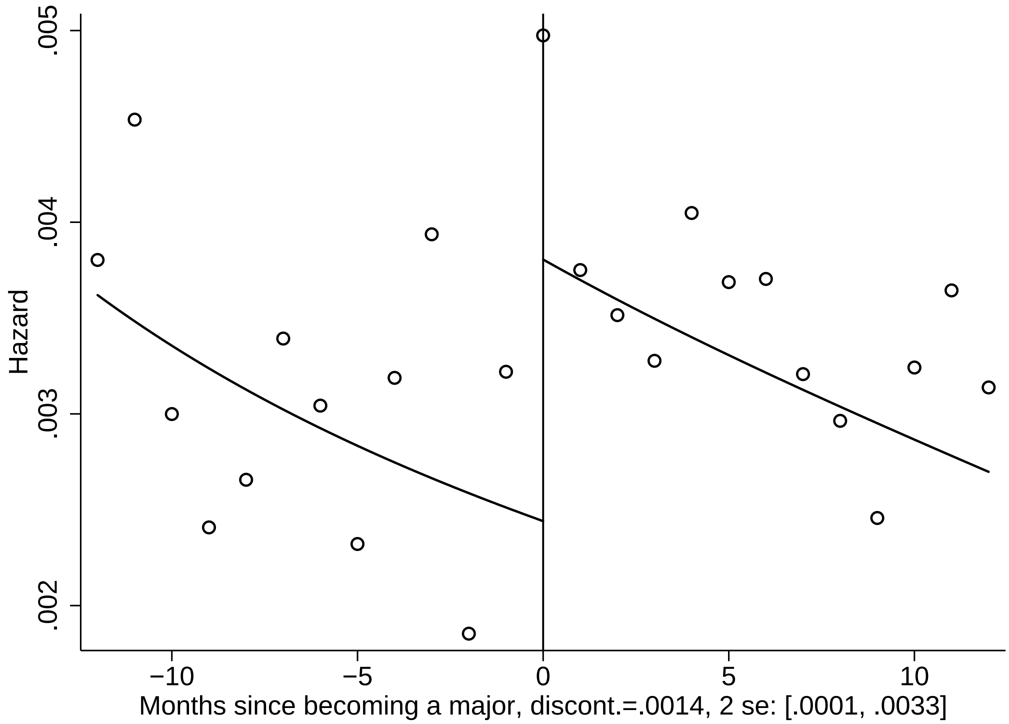

Figure 2.24: Monthly hazard of first arrest with any charges since age of majority, NLSY

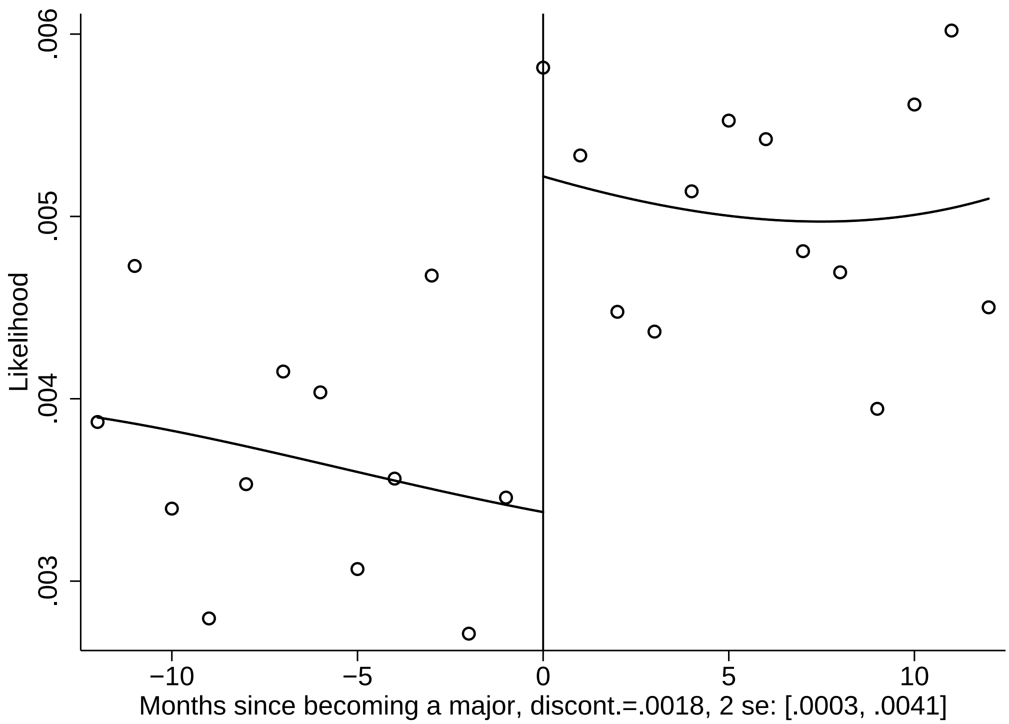

Figure 2.25: Monthly likelihood of arrest with any charges since age of majority, NLSY

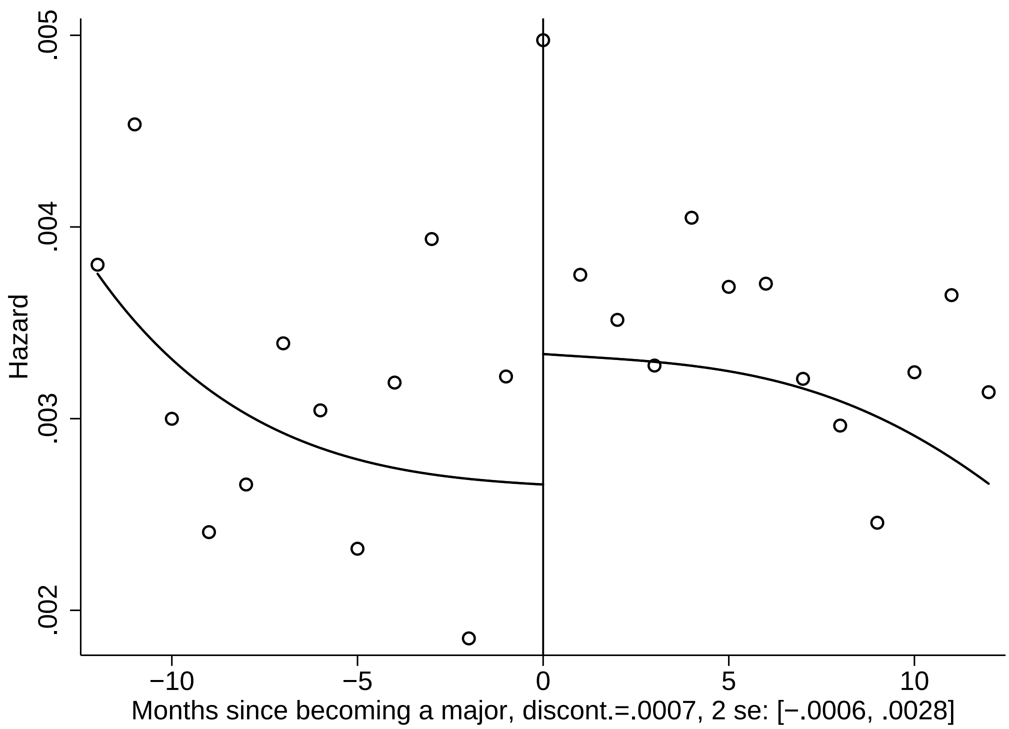

Figure 2.26: Monthly hazard of first arrest with any charges since age of majority, excluding month $= 0$, NLSY

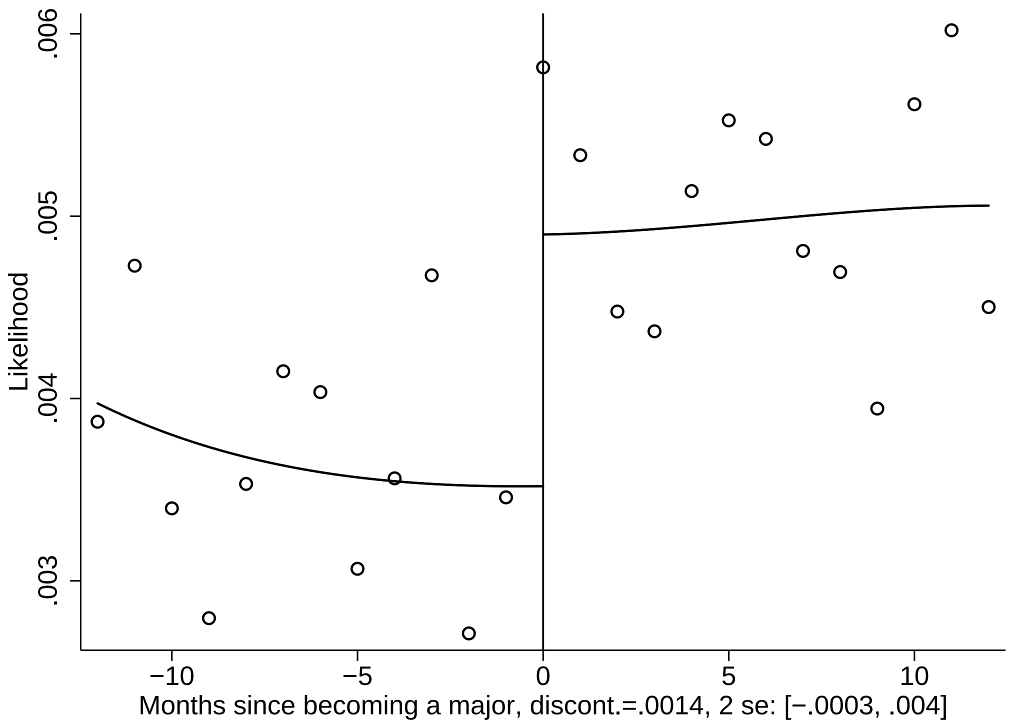

Figure 2.27: Monthly likelihood of arrest with any charges since age of majority, excludes month $= 0$, NLSY

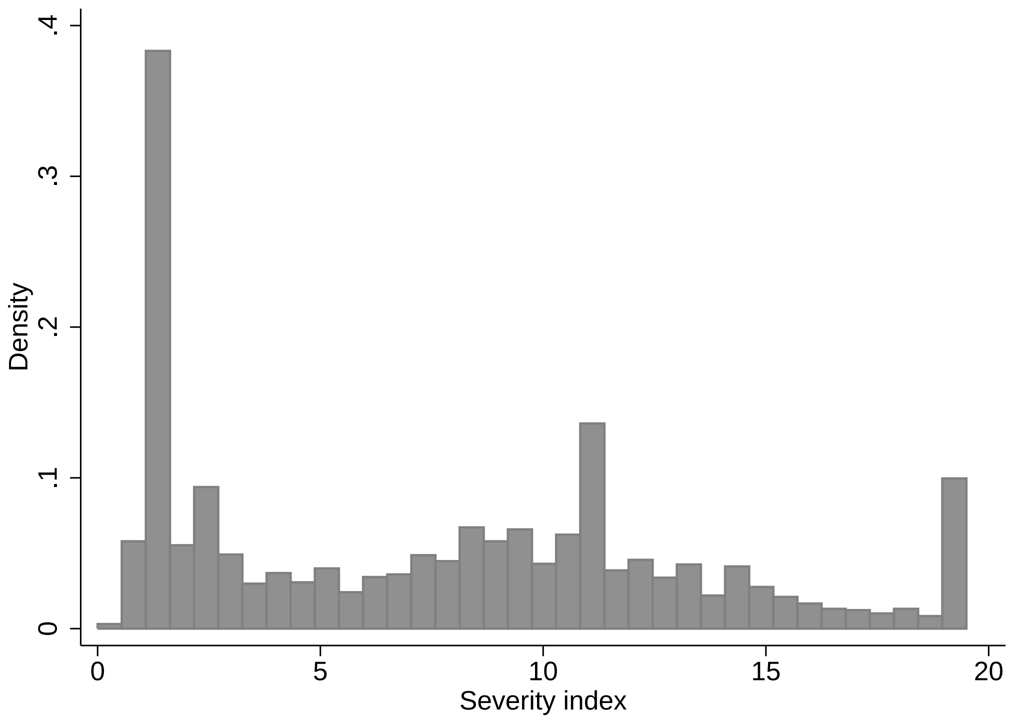

Figure 2.28: Histogram of crime severity index for Philadelphia data

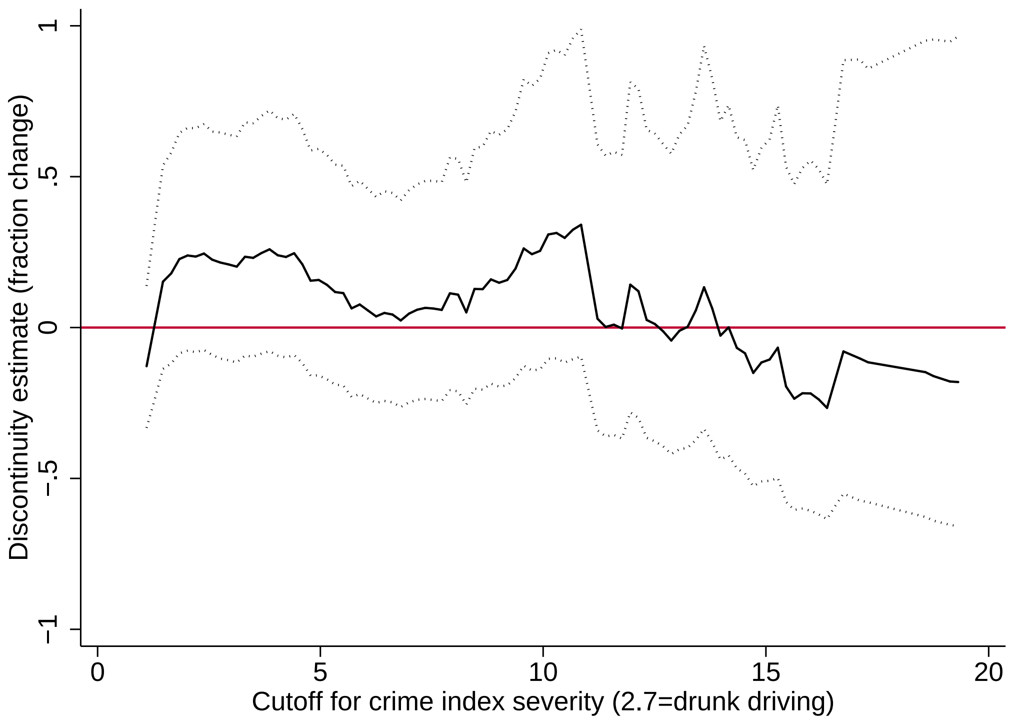

Figure 2.29: Philadelphia discontinuity estimate of hazard of first arrest since 17, by cutoff of crime severity (dotted line is point-wise 2 standard error band)

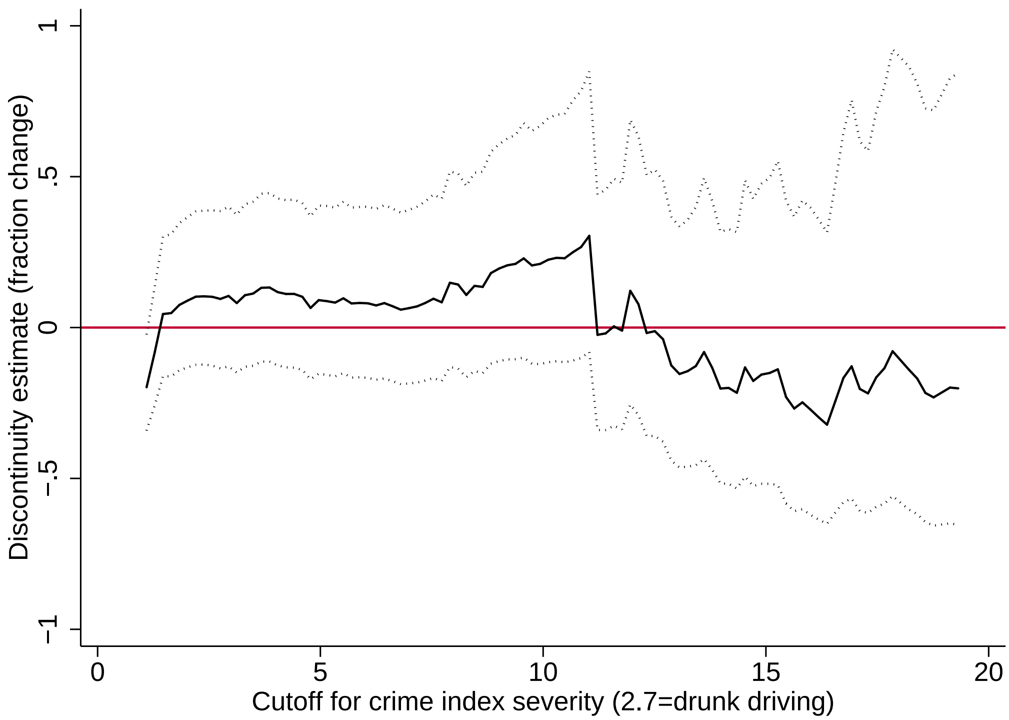

Figure 2.30: Philadelphia discontinuity estimate of likelihood of arrest, by cutoff of crime severity (dotted line is point-wise 2 standard error band)

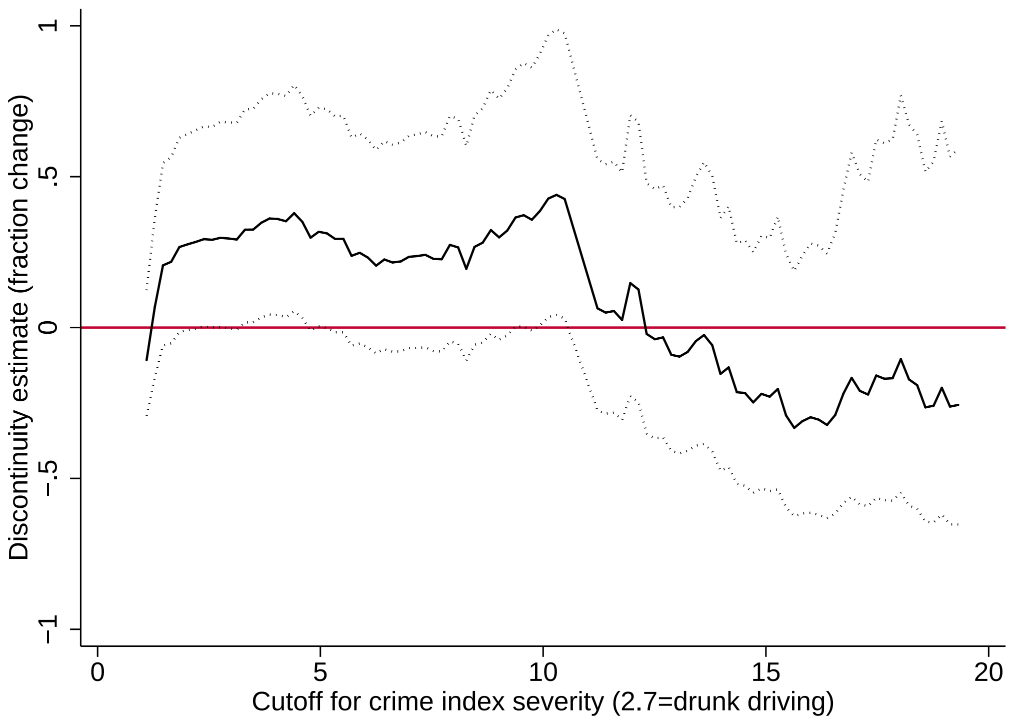

Figure 2.31: Philadelphia discontinuity estimate of hazard of first arrest since 17 excluding month zero, by cutoff of crime severity (dotted line is point-wise 2 standard error band)

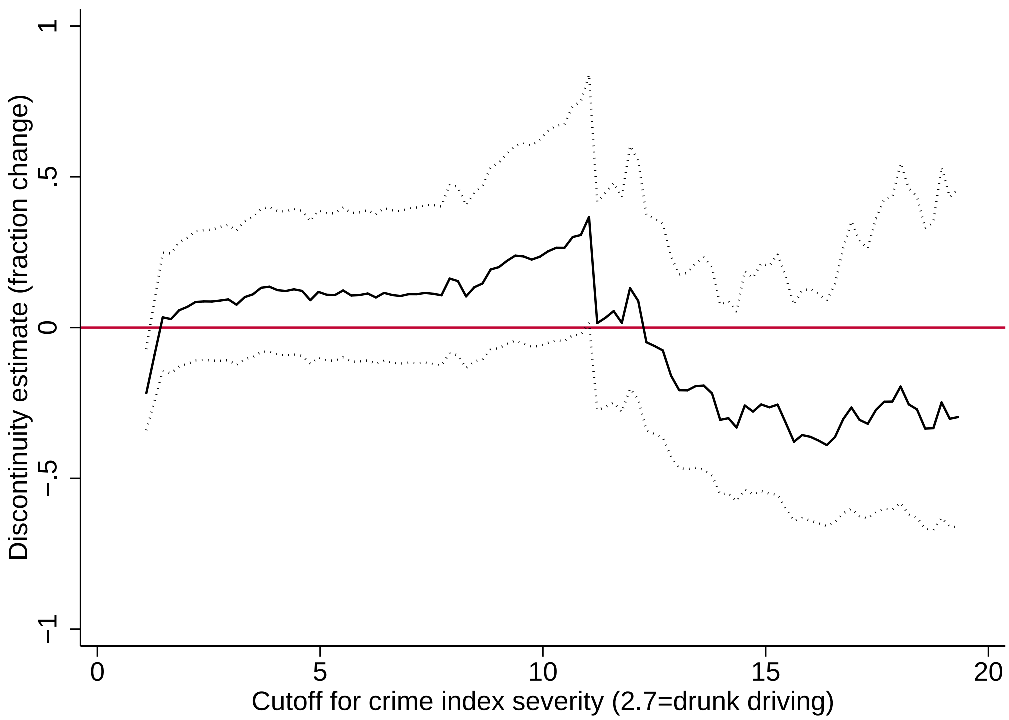

Figure 2.32: Philadelphia discontinuity estimate of likelihood of arrest excluding month zero, by cutoff of crime severity (dotted line is point-wise 2 standard error band)

Figure 2.33: Weekly hazard of first arrest since year before age of majority, Oakland

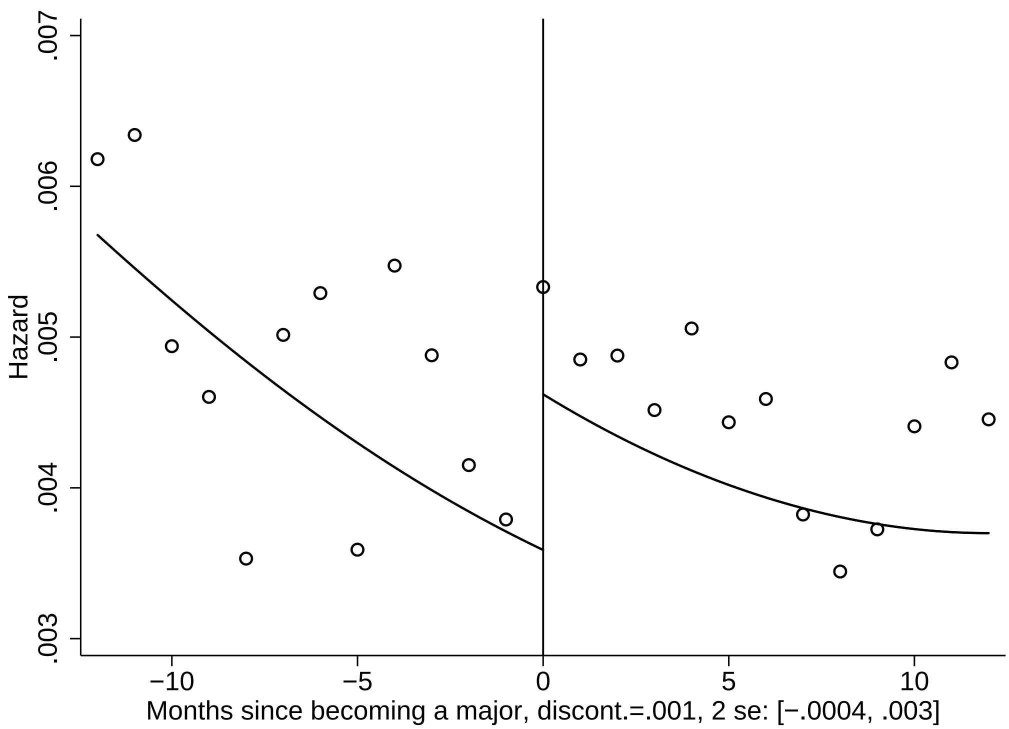

Figure 2.34: Monthly hazard of first arrest since year before age of majority, NLSY

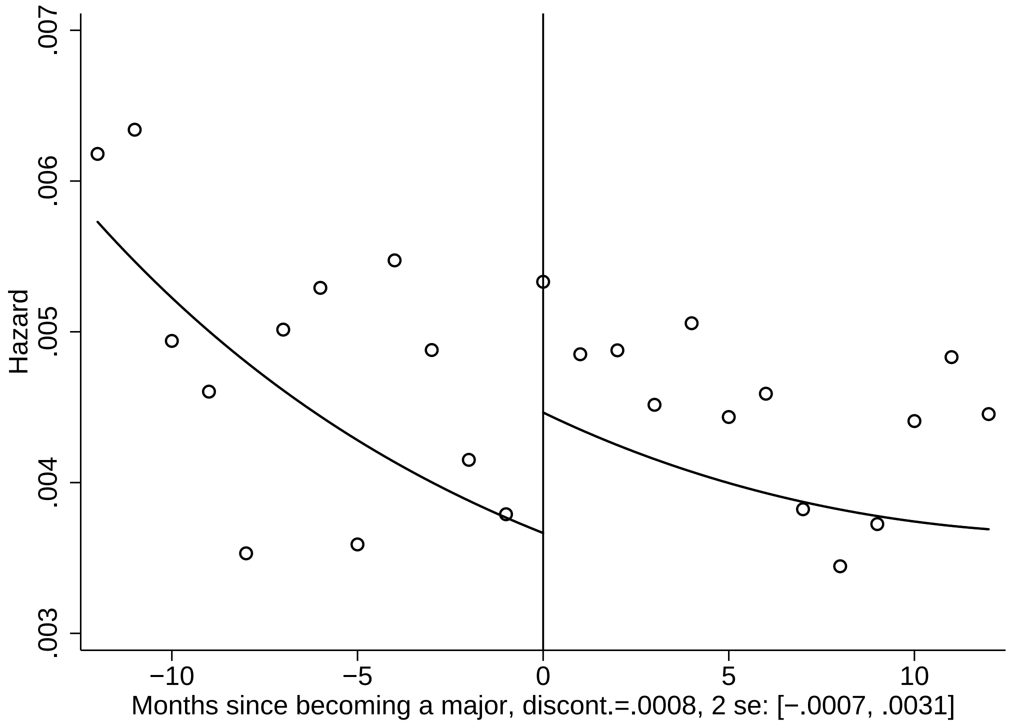

Figure 2.35: Monthly hazard of first arrest since year before age of majority, NLSY excluding month $= 0$

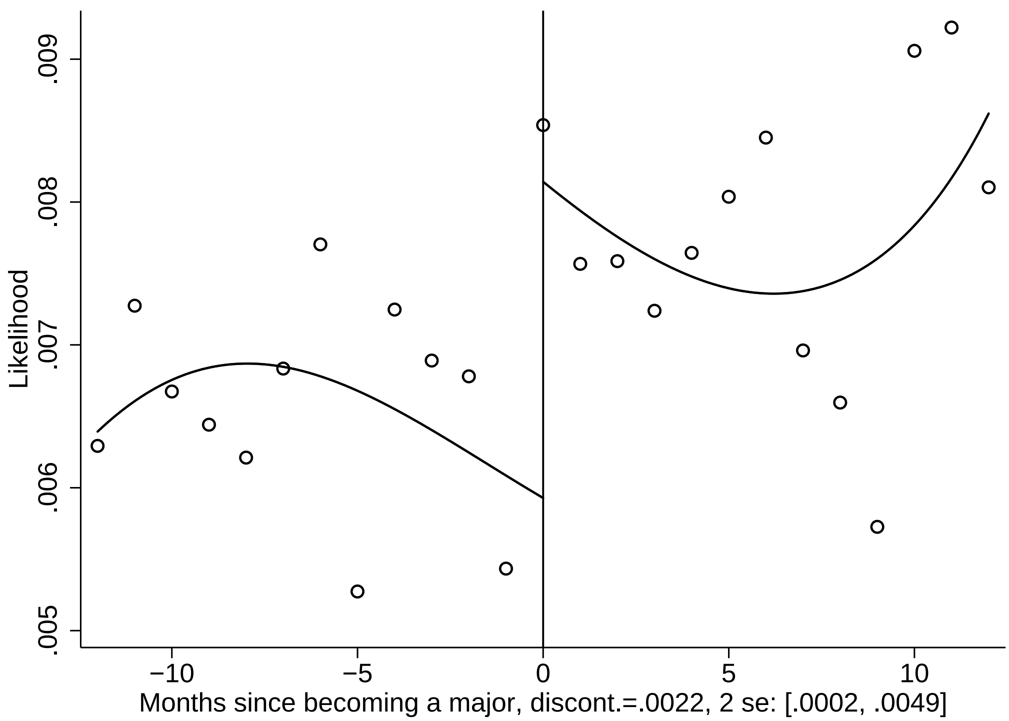

Figure 2.36: Monthly likelihoood of arrest, NLSY

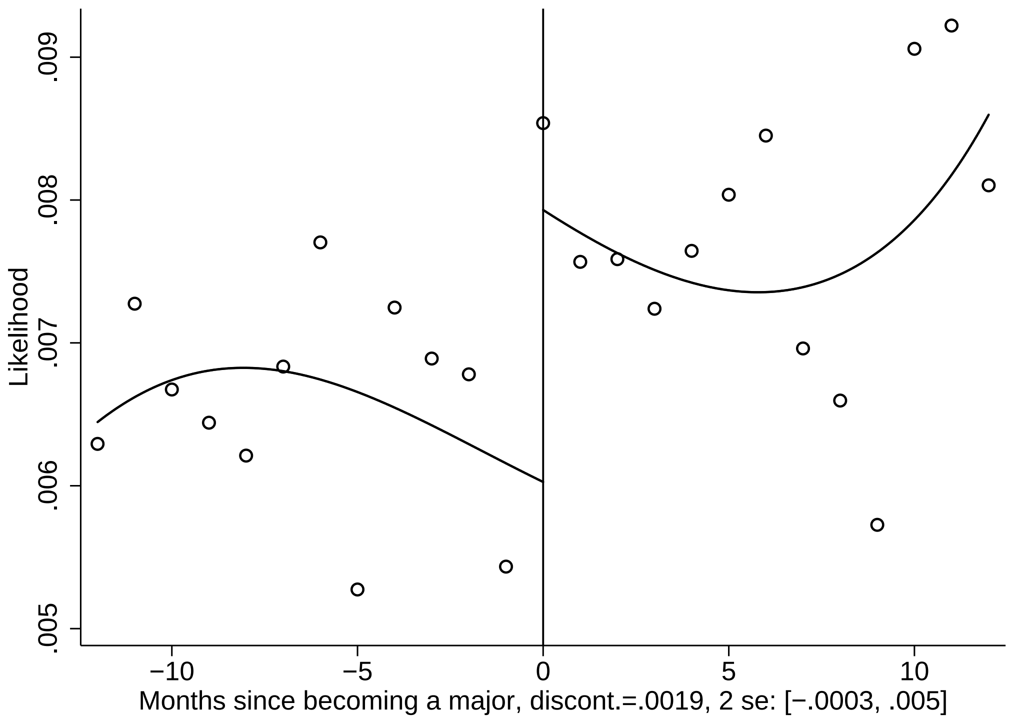

Figure 2.37: NLSY likelihoood of arrest since year before age of majority, excluding month $= 0$

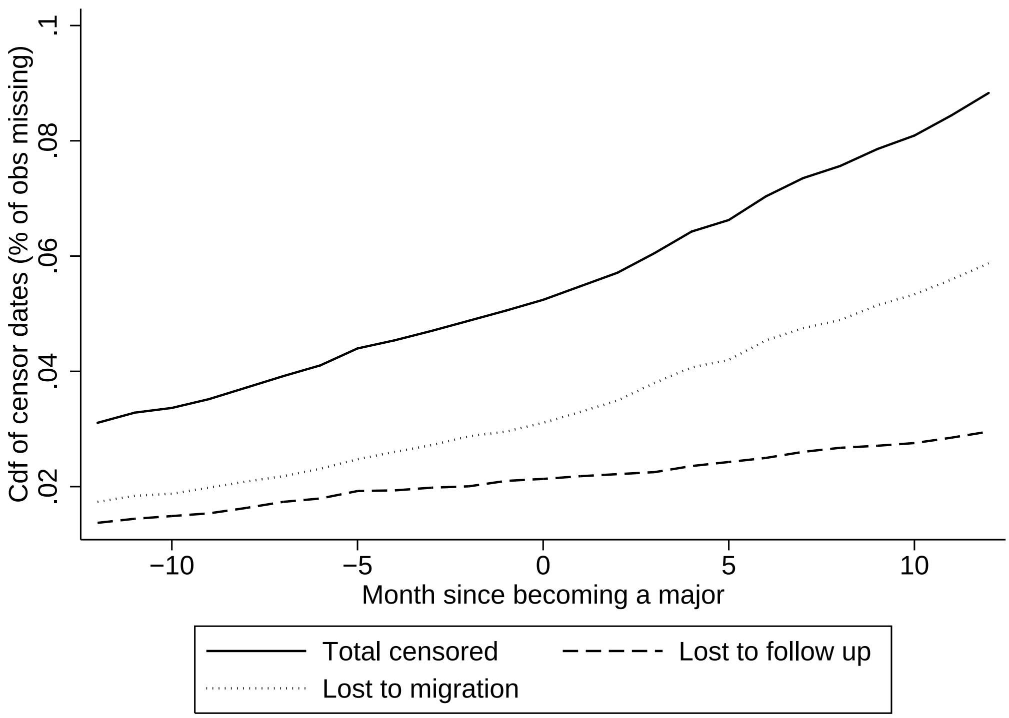

Figure 2.38: Probability of being censored, NLSY

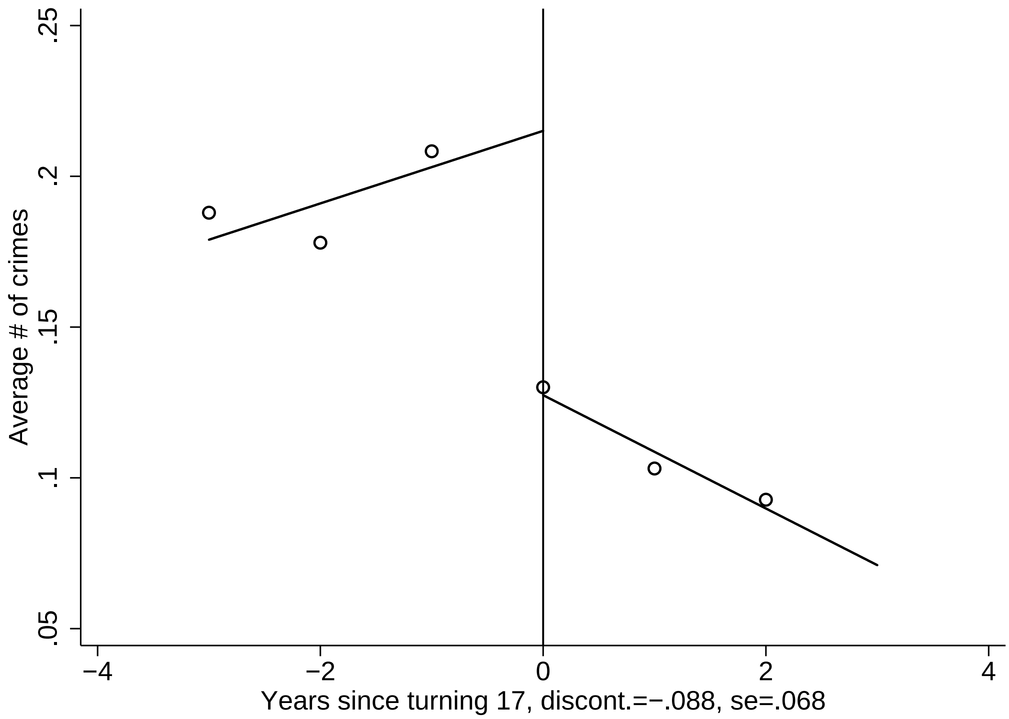

Figure 2.39: Annual number of thefts for age of majority$= 17$, NLSY

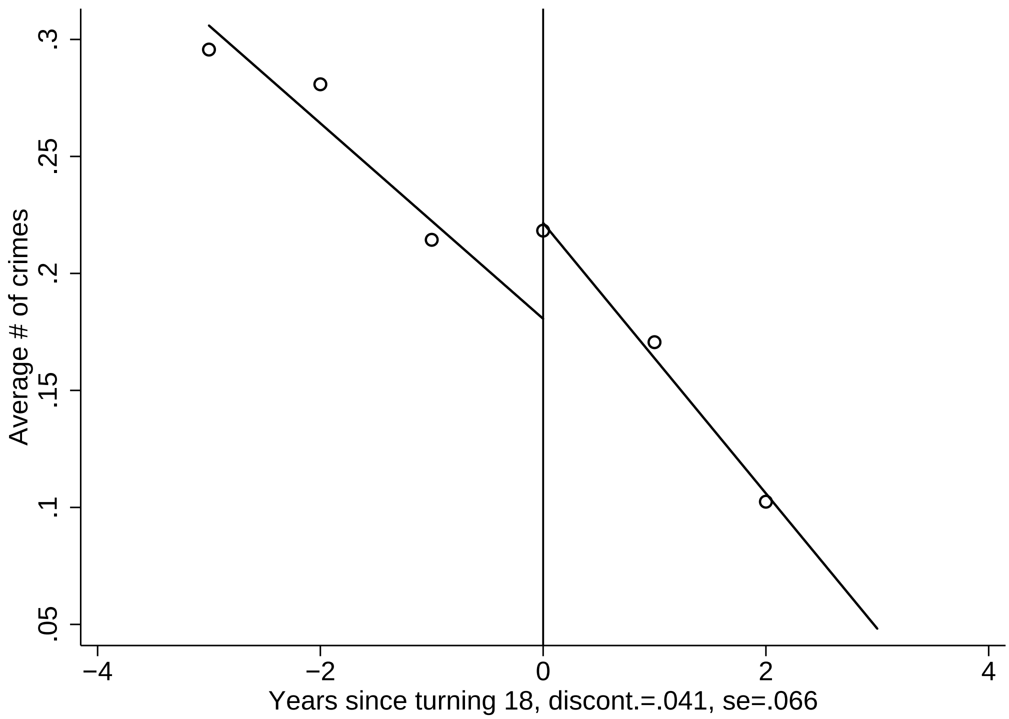

Figure 2.40: Annual number of thefts for age of majority$= 18$, NLSY

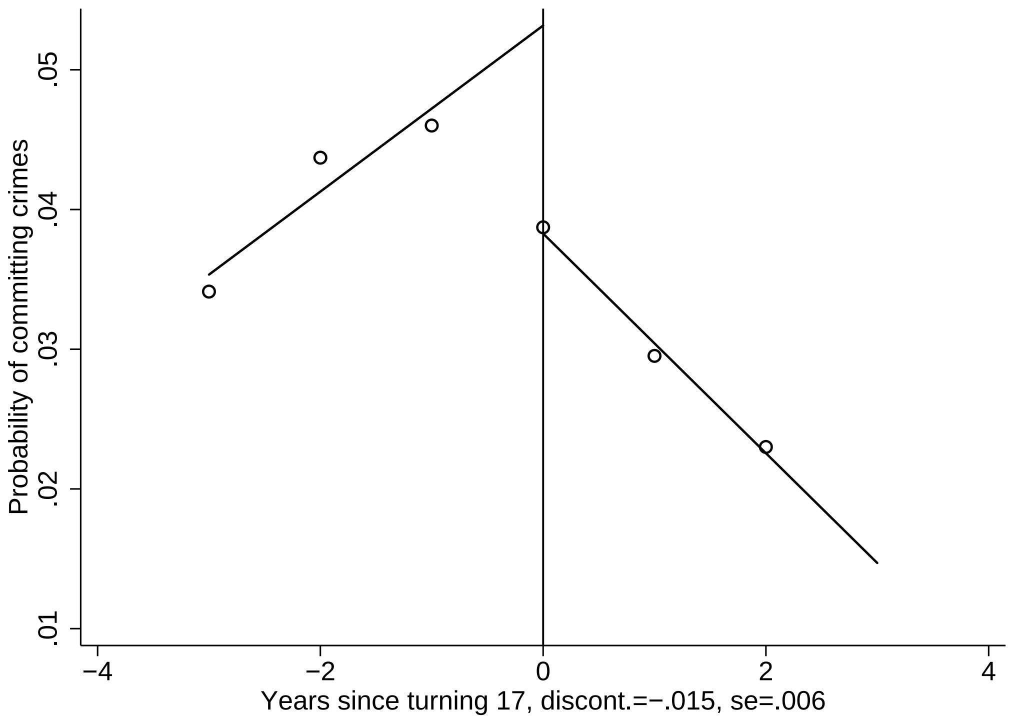

Figure 2.41: Annual likelihood of theft for age of majority$= 17$, NLSY

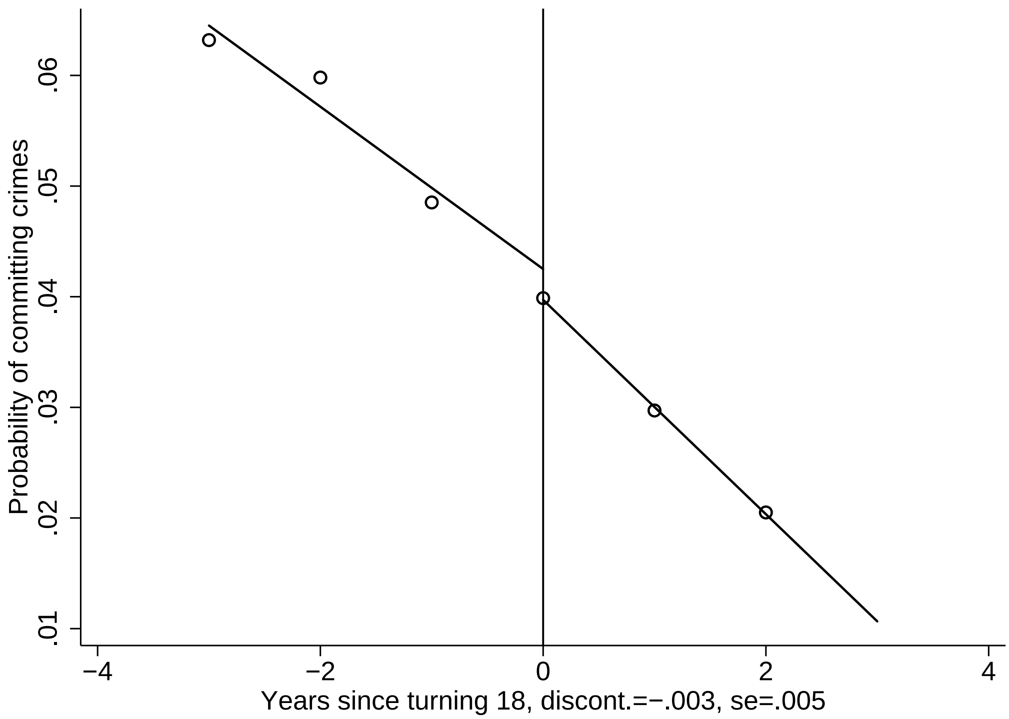

Figure 2.42: Annual likelihood of theft for age of majority$= 18$, NLSY

Figure 2.43: Quality of overlap interview length and age intervals (in years) for self-reported crimes, NLSY

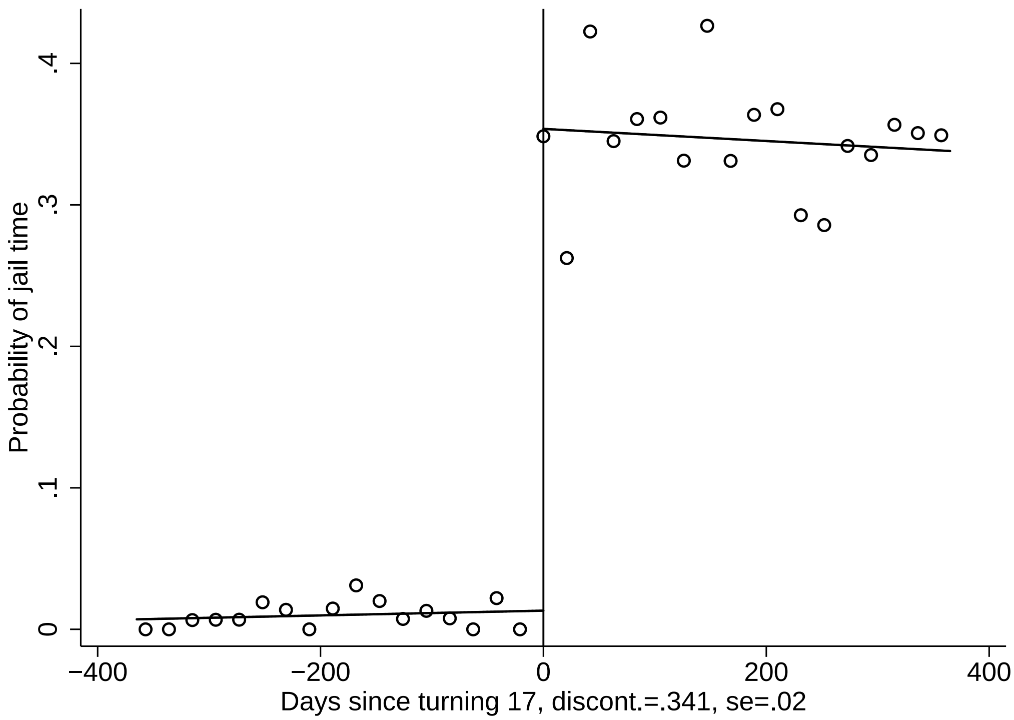

Figure 2.44: Probability of jail time given age at arrest conditional on sentence length ≤ 5 years, New Orleans

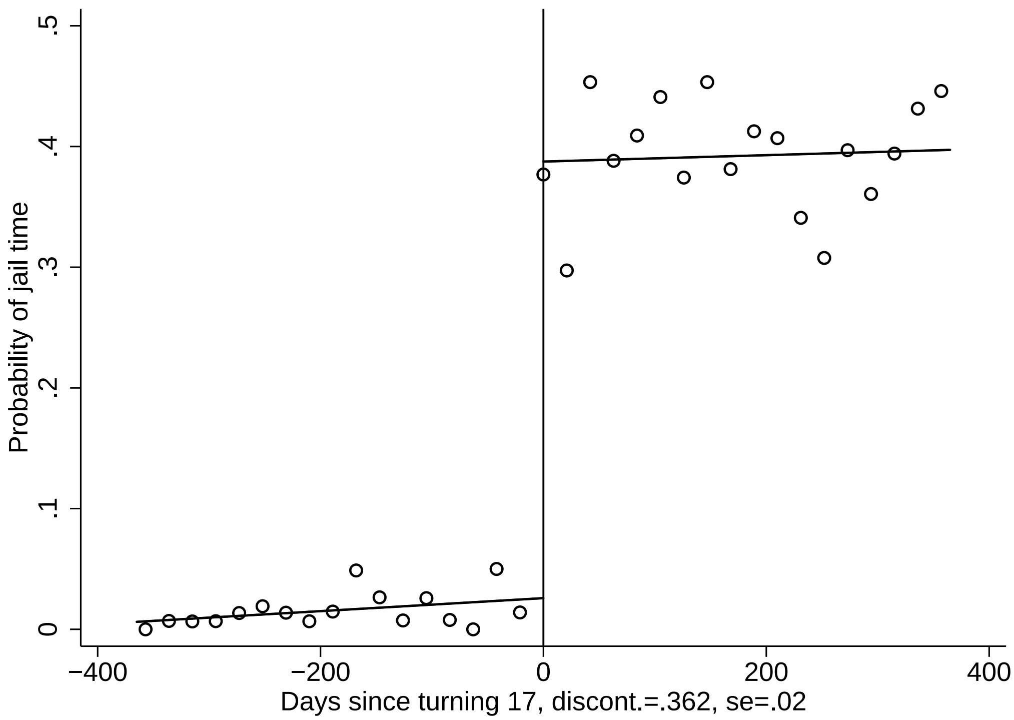

Figure 2.45: Probability of jail time given age at arrest conditional on sentence length ≤ 99 years, New Orleans

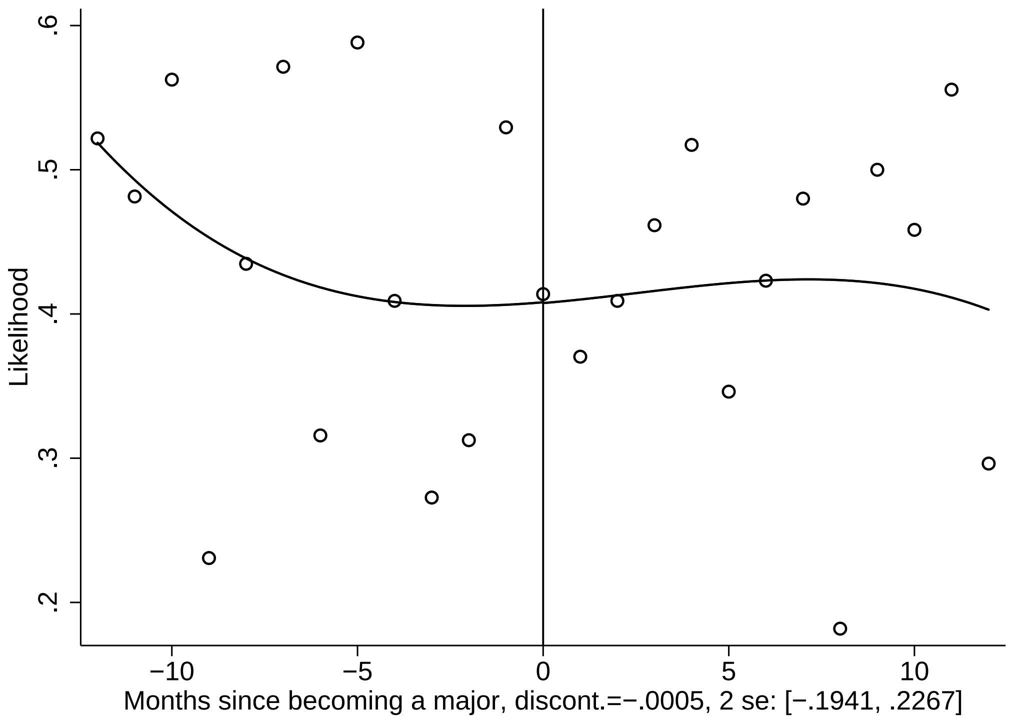

Figure 2.46: Probability of jail time given age at arrest and formally charged, NLSY

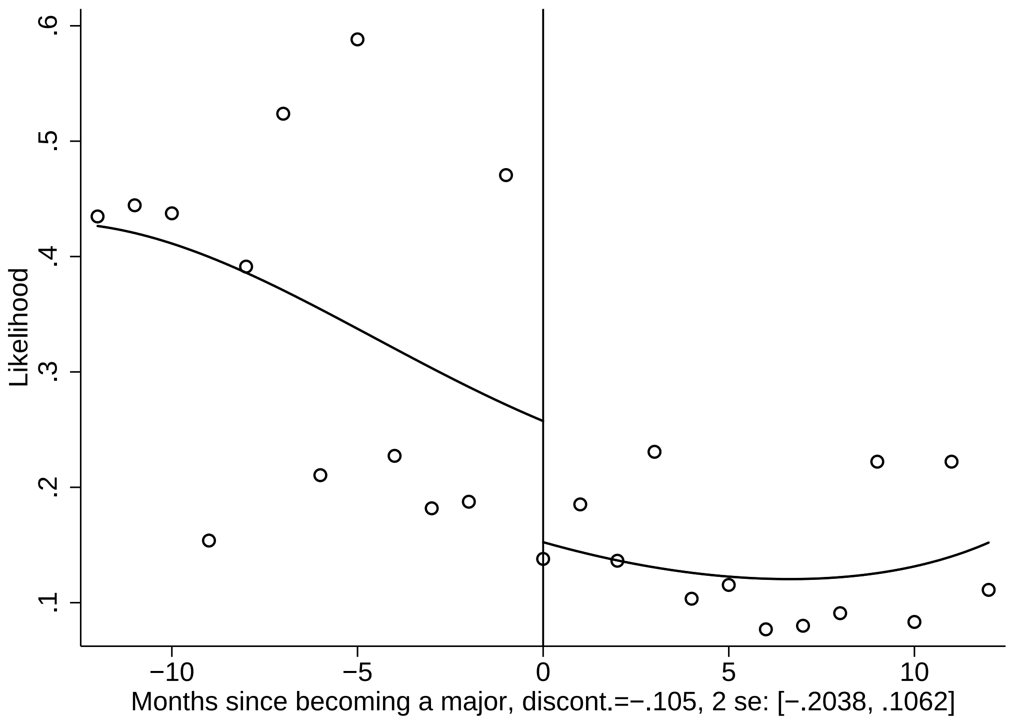

Figure 2.47: Probability of adult or juvenile corrections facility given age at arrest and formally charged, NLSY

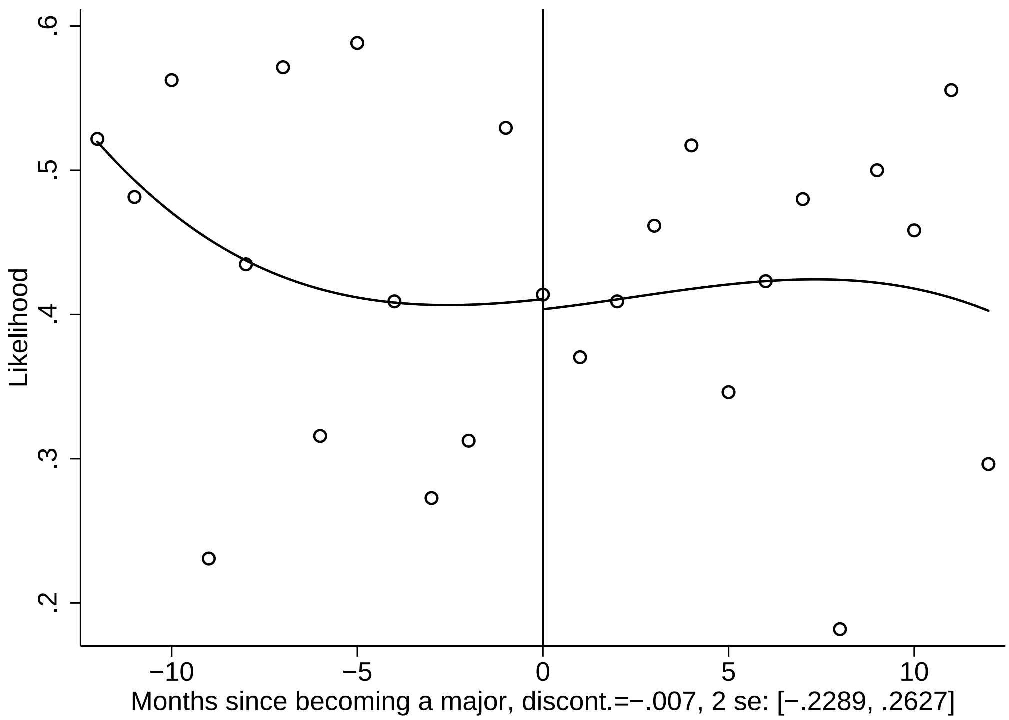

Figure 2.48: Probability of jail time given age at arrest and formally charged excluding month zero, NLSY

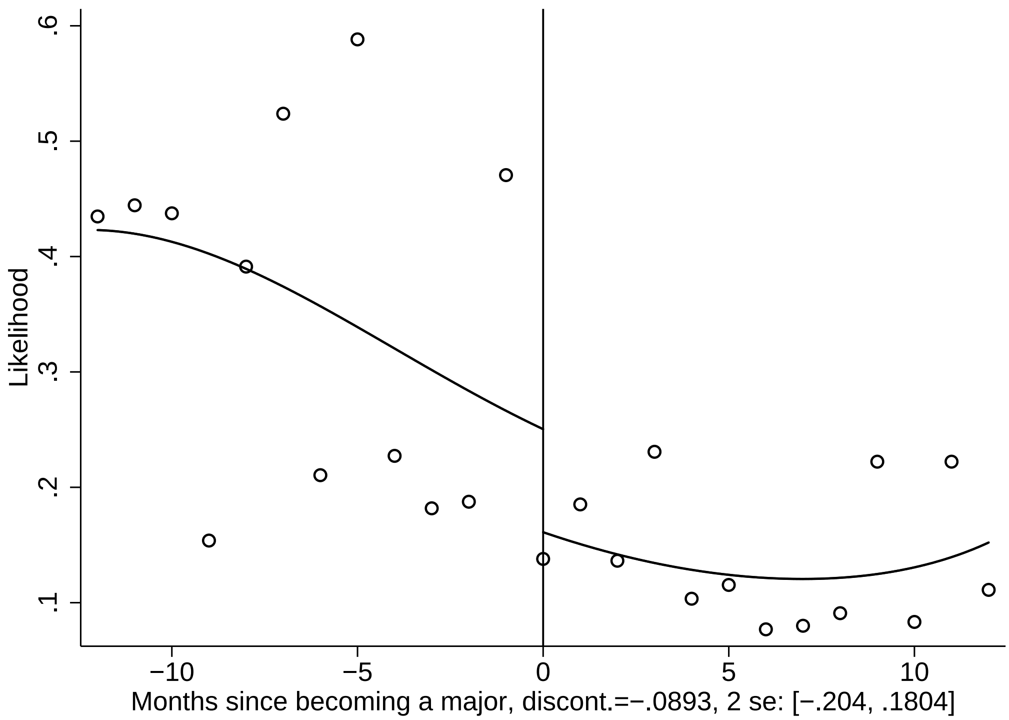

Figure 2.49: Probability of adult or juvenile corrections facility given age at arrest formally charged excluding month zero, NLSY

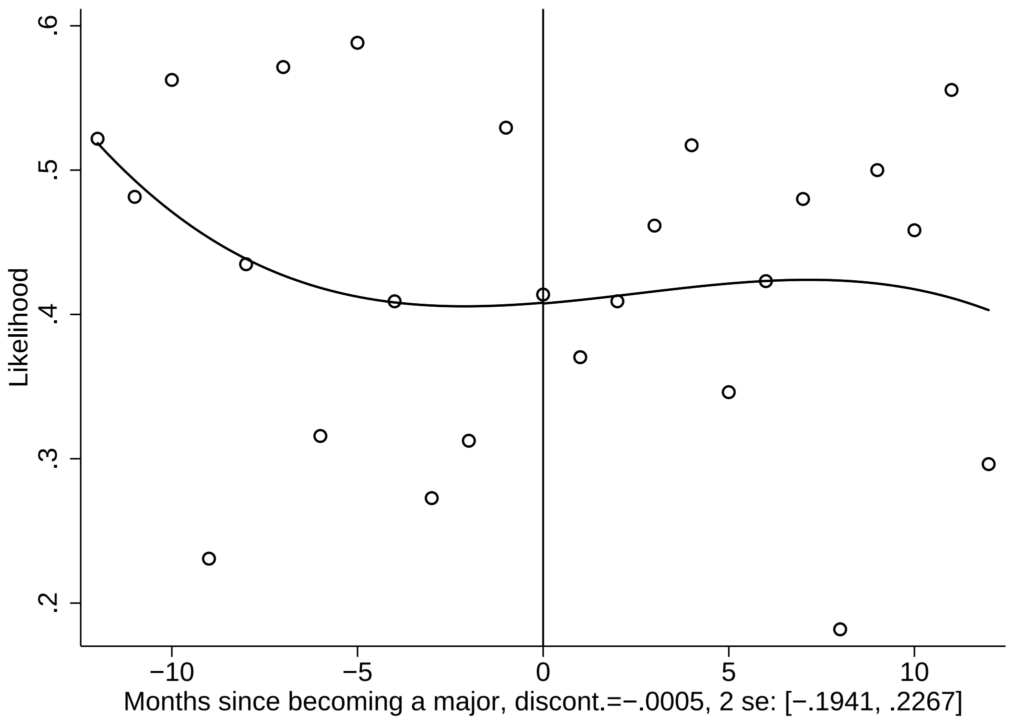

Figure 2.50: Probability of jail time given age at arrest and formally charged with something bad, NLSY

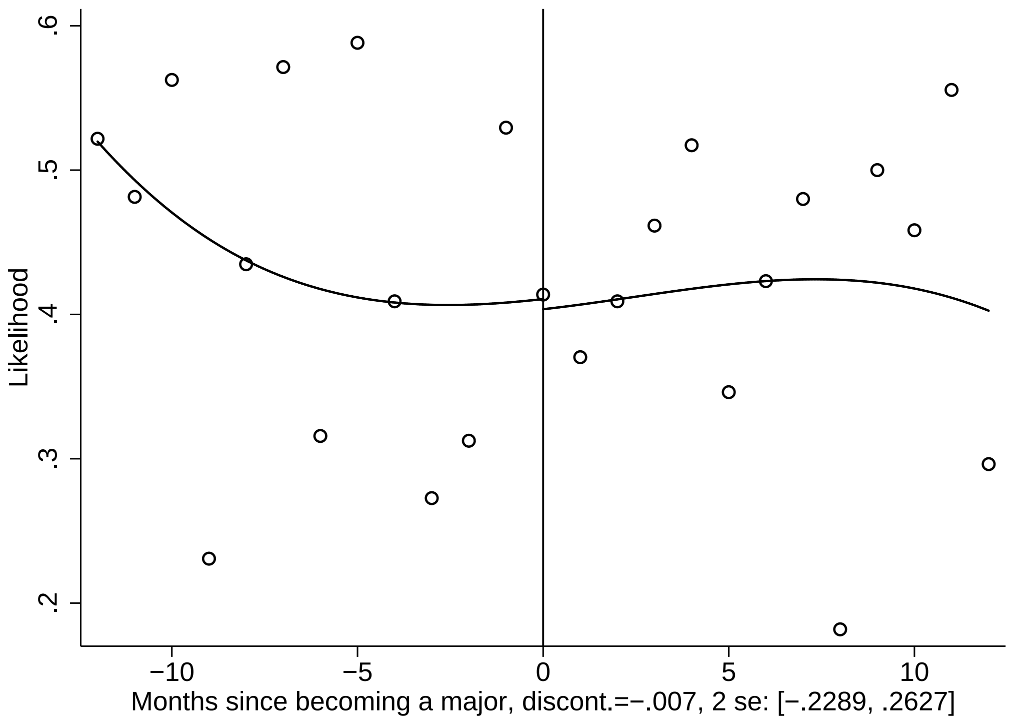

Figure 2.51: Probability of jail time given age at arrest and formally charged with something bad excluding month zero, NLSY

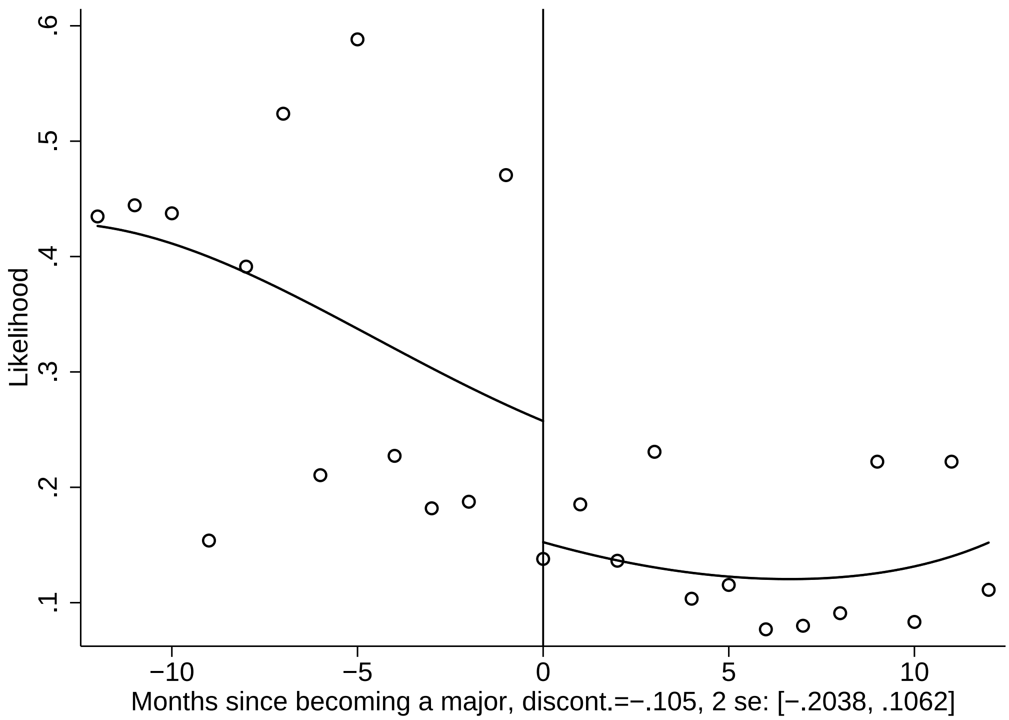

Figure 2.52: Probability of adult or juvenile corrections facility given age at arrest and formally charged with something bad, NLSY

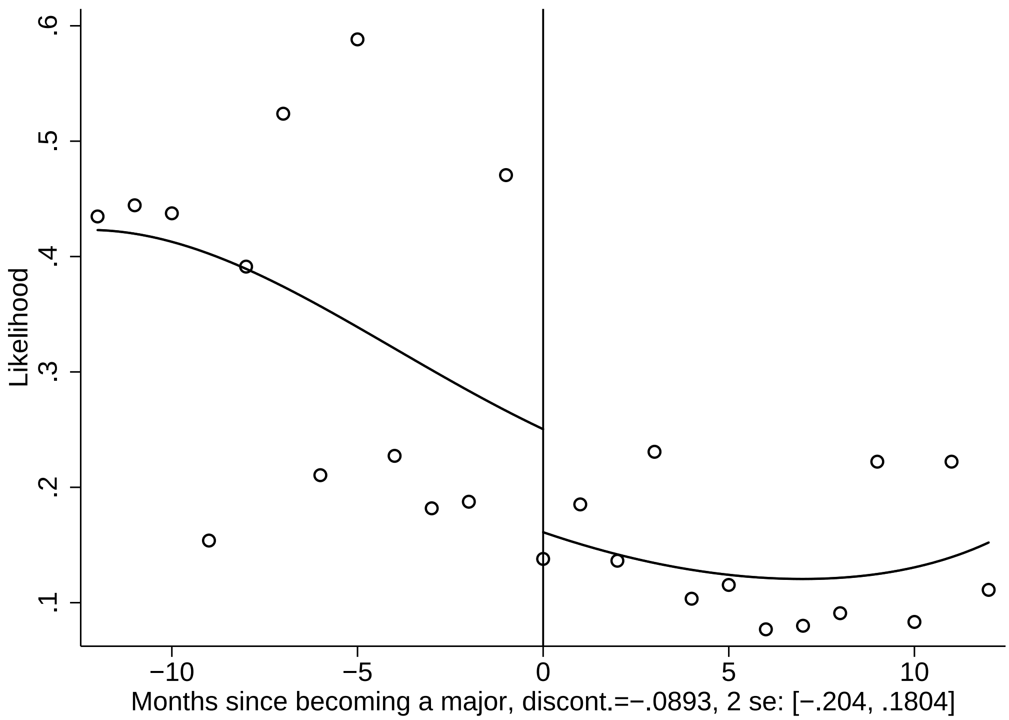

Figure 2.53: Probability of adult or juvenile corrections facility given age at arrest formally charged with something bad excluding month zero, NLSY

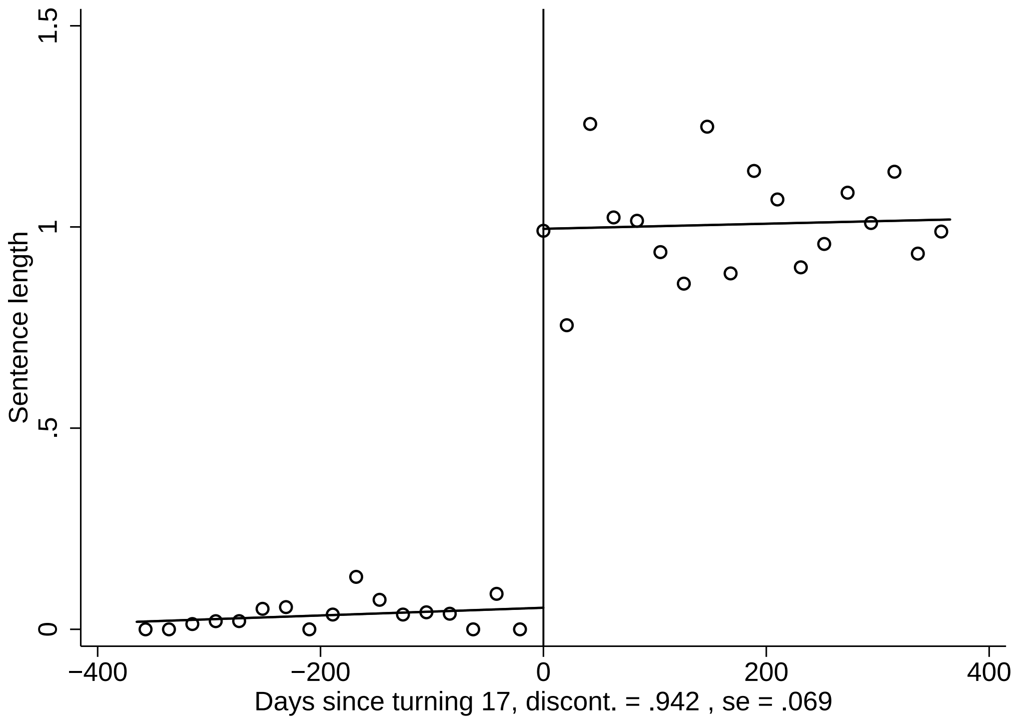

Figure 2.54: Sentence length given age at arrest conditional on sentence length ≤ 5 years, New Orleans

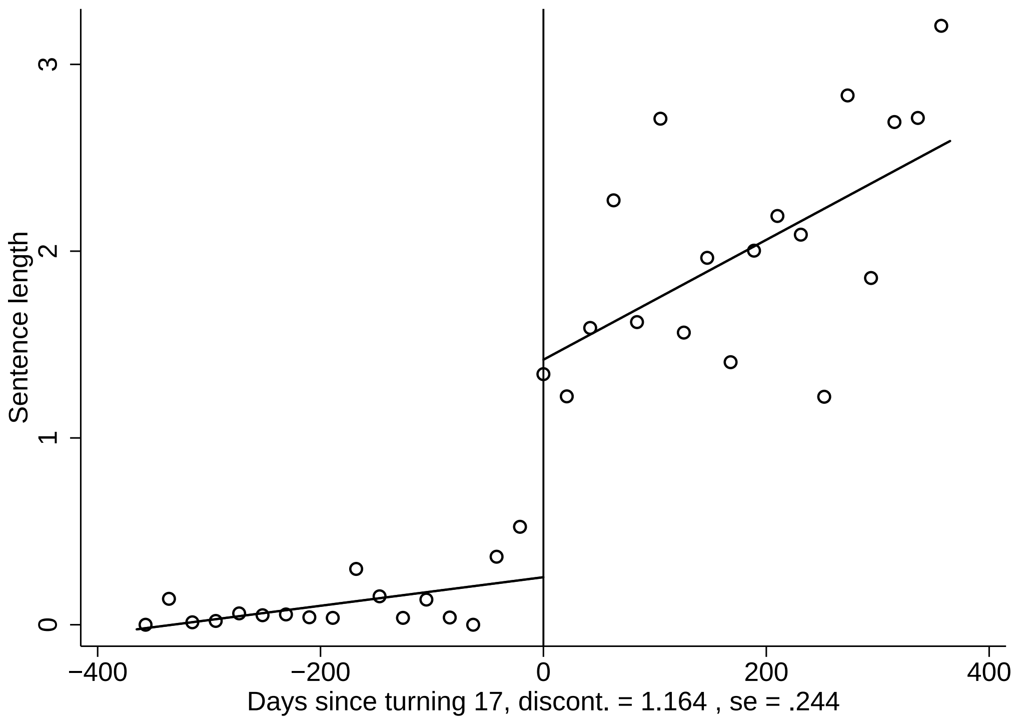

Figure 2.55: Sentence length given age at arrest conditional on sentence length ≤ 99 years, New Orleans

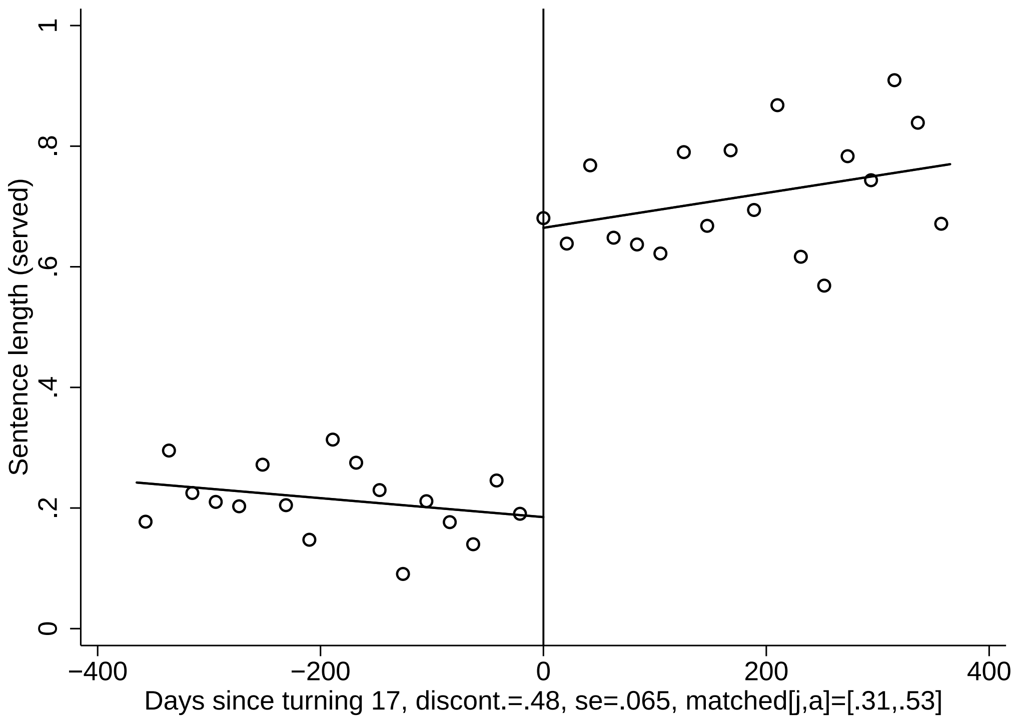

Figure 2.56: Jail time served given age at arrest conditional on serving ≤ 5 years, New Orleans

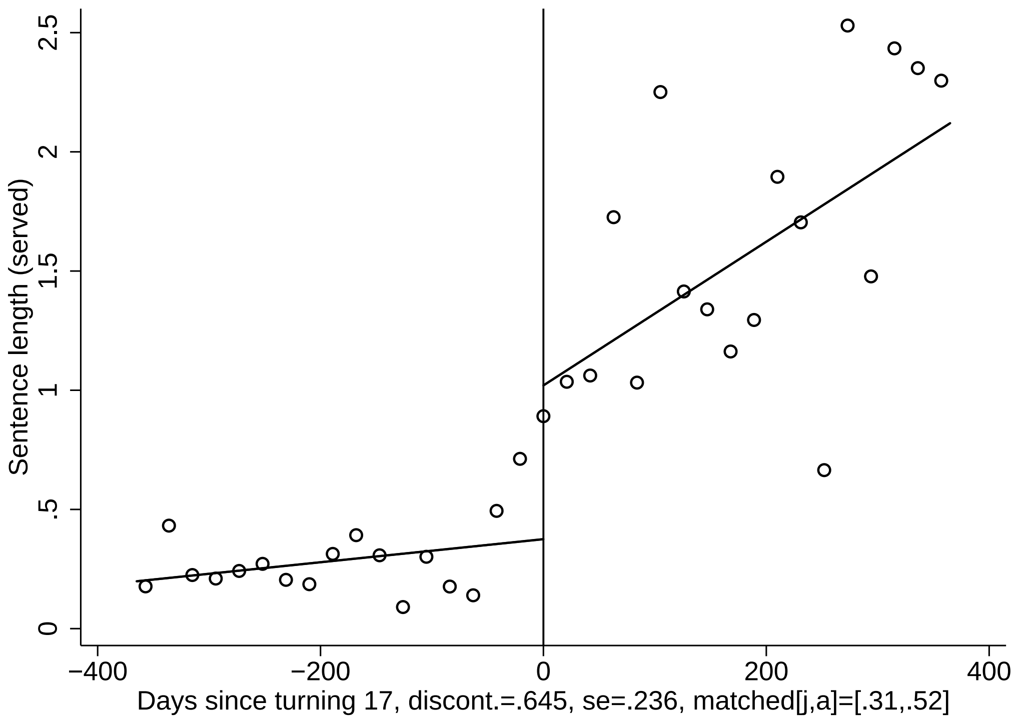

Figure 2.57: Jail time served given age at arrest conditional on serving ≤ 99 years, New Orleans

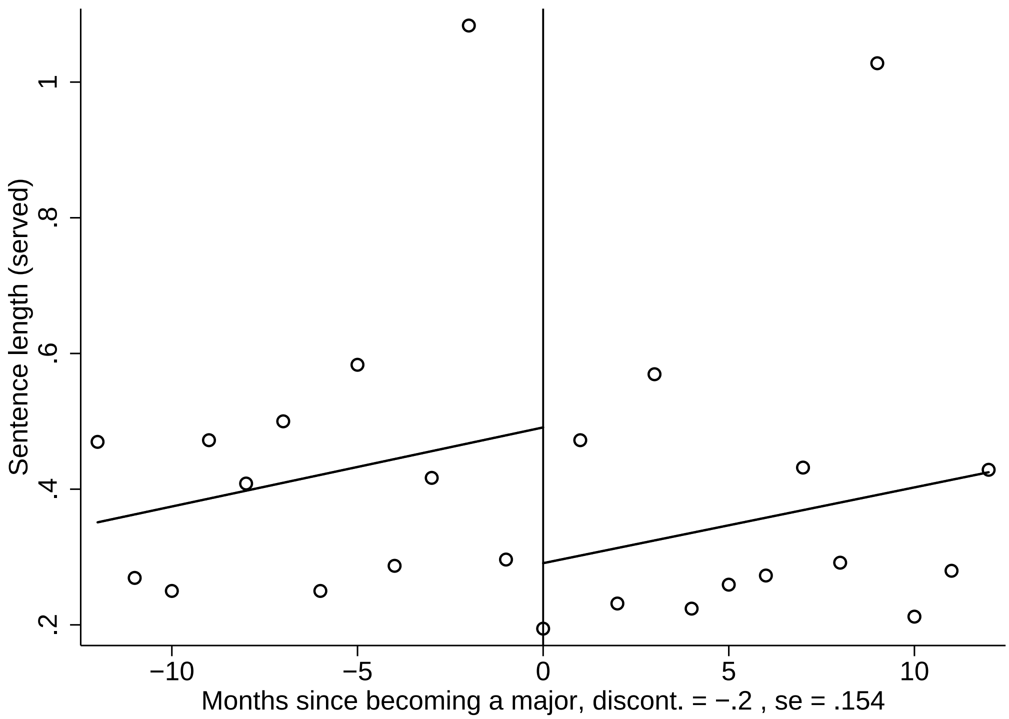

Figure 2.58: Jail time served given age at arrest and formally charged, NLSY

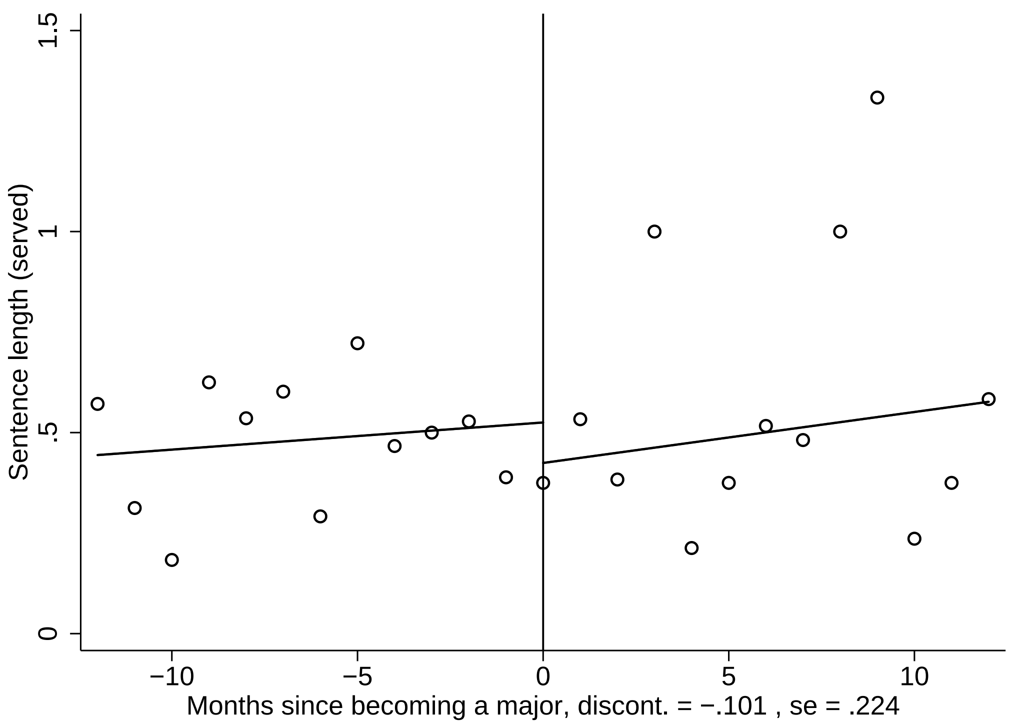

Figure 2.59: Jail time served given age at arrest and formally charged with something bad, NLSY

Table 2.1: Summary of Data Sets

	ASR	Florida	Oakland	New Orleans	NLSY	Philadelphia
Type	Admin	Admin	Admin	Admin	Longitundinal	Longitudinal
Individual identifier?	No	Yes	Yes	Yes	Yes	Yes
Precision of date	Year	Day	Day	Day	Month	Month
Years	1960–2005	1989–2002	2005–2010	1973–1986	1997–2008	1958–1988
Unit of observation	Agency-year	Arrest	Arrest	Arrest	Person	Person
Observations	8,423,595	4,928,226	231,204	35,270	8,984	27,160

Notes: Summary of each data set used in the analysis. ASR refers to the age-sex-race arrest records tabulated by the FBI in the Uniform Crime Reports. NLSY refers to the National Longitudinal Survey of Youth, 1997 cohort. Florida, Oakland, and New Orleans include all arrests from those areas, Philadelphia includes only a sampling. See section 2.2 for more details.

Table 2.2: Discontintuity in Arrests per 100,000

	Age of majority			
	17-years-old (10 states)		18-years-old (37 states)	
Year	Discontinuity	Standard error	Discontinuity	Standard error
1980	2	2.5	-1.7	1.5
1981	4.5	2.6	0.4	1.6
1982	3.9	2.8	0	1.5
1983	5.8	2.5	5.6	1.4
1984	5.8	2.6	1.2	1.5
1985	1.3	2.5	0.1	1.5
1986	-1.6	2.8	3.3	1.6
1987	8.3	2.8	3	1.5
1988	8.7	3.1	0.5	1.7
1989	4.3	3.7	0.6	1.9
1990	-0.7	4	2.2	2.2
1991	20.7	4.5	3.8	2.3
1992	2	4.5	-2.3	2.3
1993	-6.3	4.9	-5	2.4
1994	-2	4.3	-4.1	2.3
1995	2.2	4	1.4	2.2
1996	10	3.3	1.9	2
1997	0	3.1	2.3	1.9
1998	8.5	2.8	1.3	1.8
1999	-0.3	2.6	5.6	1.5
2000	8.1	2.2	6.4	1.3
2001	6.2	2.1	3	1.3
2002	-0.4	2.3	0.6	1.3
2003	3	2.1	3.9	1.2
2004	1	1.7	6.8	1.3
2005	4.2	1.9	2.3	1.4

Notes: The unit of observation is at the state-year-offense level. Each estimate is the discontinuity in the number of arrests per 100,000 persons. Regressions are weighted by the normalized product of the state-year population and the triangle kernel. The sample is restricted to three years before and after the age of majority.

Chapter 3

Reconsidering Racial Bias in Motor Vehicle Searches

3.1 Introduction

In an influential paper in the February 2001 *Journal of Political Economy*, John Knowles, Nicola Persico, and Petra Todd present a model of police and motorist behavior in the context of vehicle searches, and test it using data from Maryland.[1] Their work marked a resurgence in interest on how to interpret purported evidence of statistical and racial discrimination. For recent studies, see Levitt (2004), Hernández-Murillo and Knowles (2004), Persico and Todd (2006), Dominitz and Knowles (2006), and Anwar and Fang (2006).

The main implication of the Knowles et al. model is that in the absence of racial discrimination, the proportion of searches yielding drugs (or "hit rate") will be equated across races. A relatively low hit rate for any group suggests that police may improve their overall hit rate by shifting resources away from that group, and is thus evidence toward discrimination. Using data on vehicle searches by Maryland State Police (MSP), they find no bias against blacks relative to whites, but significant bias against white females and particularly Hispanics (though both groups had limited observations: 41 white females and 97 Hispanics).

An important feature of the data used in Knowles et al. is that it is limited to searches occurring on Interstate 95, which was also the focus of the racial profiling lawsuit filed against MSP in 1993. Since MSP started collecting data on vehicle searches in 1995 (as part of the settlement of the case), there is no way of empirically verifying the grounds for the suit (i.e. if there was racial bias before 1993).

However, while the suit focused on I-95 searches, the settlement required MSP to record all vehicle searches, of which I-95 searches constitute about one third.[2] This paper reconsiders

[1] Knowles, Persico, and Todd (2001).

[2] More specifically, the settlement required MSP to record all searches conducted by one of the twenty-four

the Knowles et al. analysis using all MSP searches, both for the time period studied in Knowles et al. (1995–1999) and in more recent years (1995–2006).

3.2 Results

Table 3.1 lists the results. For each sample, columns 1–3 list the hit rates for whites, blacks, and Hispanics, respectively, with standard errors in parentheses. Column 4 lists the difference between blacks and whites, and similarly for Hispanics and whites in column 5. By the Knowles et al. model, statistically significant negative values in these two columns are evidence toward racial discrimination against blacks and Hispanics. The last column reports the total number of vehicle searches involving black, white, and Hispanic drivers.

The first row lists the original results from Knowles et al. (from tables 2 and 3 of Knowles et al.). In the second row, the Knowles et al. results are successfully replicated, though the estimates differ slightly. This is mostly likely due to minor differences in the precise definition of a successful search.[3] The third row excludes double entries which were not removed in the original study. Again, there are significant differences.

The next two rows include all MSP searches. Row 4 includes only searches occurring during the time period considered in Knowles et al. (on or before January 29, 1999). The sample size is now about 3.5 times the original, and the estimates are naturally more precise estimates. Black and Hispanic hit rates are approximately 6 and 25 percentage points lower than whites, suggesting racial bias against these two groups, particularly Hispanics.[4] In the full sample (all Maryland, 1995–2006), the disparity increases by about 4 percentage points for both groups, suggesting that the bias has increased in recent years.

Rows 6 and 7 separate by on and off I-95. Row 6 confirms the Knowles et al. result: searches along I-95 exihibit no significant difference between black and white hit rates. However, the disparity is large and significant for non-I-95 searches. Also, the Hispanic-white difference, while large and significant both on and off I-95, is substantially larger off I-95. Both statistics suggest that police may have responded to the lawsuit, which primarily concerned racial profiling on I-95. Finally, the relatively low hit rates for all searches on I-95 suggest that police are over-searching I-95 motorists. It may be that police are over-searching I-95 because of its reputation for drug trafficking and/or that drug traffickers have responded

barracks that constitute MSP.

[3] A small number of drugs, such as valium, were excluded from the definition of a positive search. However, there may be differences in the coding schemes that arise from borderline cases of excluded drugs, such as discovery in the driver's shoe by a police canine.

[4] When the data is restricted to the relatively small sample of females, there is bias against whites relative to blacks in the Knowles et al. sample (searches on I-95, 1995–1999). When all MSP searches are used, there is no significant female black-white difference for the Knowles et al. years. Finally, the black female hit rate is statistically significantly lower than the white female hit rate when the full sample is used (about 7 percentage points).

Table 3.1: Hit Rates and t-tests of Significant Difference

Sample	White (1)	Black (2)	Hispanic (3)	Black− White (4)	Hispanic− White (5)	N (6)
1995–1999						
1. Original	.32	.34	.11	.02	-.21	1570
2. Best replication	.326	.349	.124	.022	-.202	1570
	(.022)	(.015)	(.034)	(.027)	(.040)	
3. Best replication	.329	.351	.124	.022	-.205	1554
(no doubles)	(.022)	(.015)	(.034)	(.027)	(.040)	
4. All Maryland	.352	.294	.102	-.058	-.249	5306
	(.009)	(.010)	(.019)	(.013)	(.021)	
1995–2006						
5. All Maryland	.380	.279	.083	-.102	-.297	18927
	(.005)	(.005)	(.007)	(.007)	(.009)	
6. I-95 only	.277	.261	.077	-.016	-.200	6577
	(.010)	(.007)	(.008)	(.012)	(.013)	
7. Non-I-95 only	.408	.293	.095	-.114	-.312	12346
	(.006)	(.007)	(.014)	(.009)	(.015)	
84% subsample with matched location 1995–2006						
8. All Maryland						
a. without location fixed effects	.373	.275	.080	-.098	-.293	15907
	(.005)	(.006)	(.013)	(.008)	(.013)	
b. with location fixed effects				-.077	-.255	15907
				(.008)	(.014)	
c. row b − row a				.021	.037	
				(.011)	(.019)	

Notes: Standard errors listed in parentheses.

to police behavior.[5] Alternatively, officers may place a higher value on I-95 finds because, for example, they yield larger quantities on average.[6]

Lastly, consider the effect of heterogenous search costs. In the original Knowles et al. model, an important assumption is that the cost of a search is constant. However, if police allocated their resources strategically across time and space, this may not hold. For example, suppose high crime areas (for reasons other than drugs) are areas of both high police and minority presence. Then we might expect higher search intensity and therefore lower hit rates across all races in such areas. Since, in this example, these areas are also areas of high minority presence, the unconditional hit rate gap between whites and minorities may be different from zero even in the absence of racial discrimination. I thank an anonymous referee for this example.

This issue is addressed by the inclusion of location fixed effects in a linear regression of an indicator for a positive drug search on indicators for each race. Location was observed for each search, but was unfortunately not recorded in a systematic way. An algorithm was created to search for keywords of highways, road names and mile markers (and misspelled versions of these). The algorithm then assigned each observation to a neighborhood based on the results of the search. The neighborhoods roughly correspond to either a major road or an actual neighborhood (usually nearby a major road). Interstate 95 searches were split into seven neighborhoods based on mile marker. The algorithm was able to assign a location to about 84 percent of searches. Neighborhood size varied considerably as searches were usually conducted along a few major roads. The typical neighborhood contained about 50–500 or 1000–2000 searches. The details of the algorithm are available upon request.

The last three rows of table 1 list the results. In row 8a, an indicator for a positive drug search is regressed on indicators for each race, including only the 84 percent subsample that was matched to a location. The unconditional hit rates are similar to the full sample (row 5). The next row includes a full set of location fixed effects, while excluding the indicator for white to prevent collinearity between the race and location indicators. The qualitative results remain, in that there are significant black-white and Hispanic-white hit rate gaps. However, the magnitude of these gaps shrink slightly for both, and in the last row estimate the difference between these coefficients, which are significant at 90 percent confidence.

The addition of location fixed effects explains about 20 percent of the hit rate gap for blacks and 10 percent for Hispanics. This suggests that heterogeneity in search cost could be an important factor in the hit rate gap. However, the results should be interpreted with caution, as they concern only a non-randomly selected subsample (the 84 percent of searches to which the algorithm successfully assigned a location). Furthermore, this strategy would underestimate the importance of heterogenous search costs inasmuch as location is a noisy indicator of search cost, and the location variable itself is subject to measurement error from

[5]The I-95 and non-I-95 hit rates were similar until around 2002, when I-95 hit rates dropped considerably, reaching a low of 8.7 percent in 2004.

[6]I thank an anonymous referee for this insight.

the assignment algorithm. Alternatively, the analysis may overestimate the importance of search cost heterogeneity if, for example, racial discrimination existed in higher levels of police management, and managers discriminated against minorities by instructing officers to search minority neighborhoods at a greater-than-optimal level of intensity.[7]

3.3 Conclusion

Knowles et al. test for racial bias in Maryland State Police using vehicle search data along Interstate 95—a very interesting strip of road considering its connection to the racial profiling lawsuit filed against MSP. This reconsidered their analysis using all MSP searches. The results largely confirm theirs: searches along I-95 suggest significant and large bias against Hispanics, but no black-white disparity. When considering all MSP searches, though, there is evidence toward racial discrimination against blacks and especially Hispanics, and that these disparities have increased in recent years.

[7] An alternative specification including location×time fixed effects yields results similar to the specification that includes only location fixed effects.

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