Essays in Labor Economics and Political Economy

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William Bruno

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Dissertation Advisors:
Professor Claudia Goldin
Professor Lawrence Katz

Author:
William Bruno

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Abstract

Each of the three chapters of this dissertation analyzes an empirical question bearing on social policy.

In the first chapter, I assess if there was differential selection between white and black men into World War II Army enlistment. Differential selection could represent (1) a potential artifact of well-documented historical discrimination in induction into service or (2) a potential confound in the attributing racial differences in relative postwar outcomes of veterans versus nonveterans to racial differences in the combined returns to military service plus veterans' benefits. My analysis demonstrates that white and African American World War II enlistees appear largely representative of their birth cohorts in terms of their family background in the 1930 Census and the racial and economic exclusivity of their 1930 neighborhood, proxied for with grades on Home Owners' Loan Corporation "redlining" maps.

In the second chapter, I investigate whether political polarization is shaped by social contact among peers across racial, ethnic, income, and social and political attitudinal lines. I estimate how the characteristics of randomly-assigned freshman roommates affect students' own attitudes. With scattered exceptions, I do not find robustly significant roommate effects. In contrast to the findings in previous studies, I find that white students assigned black roommates, as compared with those assigned white roommates, did not report more comfortable interracial interactions or higher support for affirmative action or campus diversity. But they did report lower closeness to their roommates. One possible explanation is that some white students' friendship and deliberation with black roommates, the presumptive channels for peer influence, are impaired by implicit racial bias formed before college.

In the third chapter, I examine how incarceration length affects recidivism. I estimate the joint effect of specific deterrence plus aging using quasi-random differences in average incarceration length for adults convicted of driving under the influence (DUI) and reckless driving who kill their victims as opposed to seriously injure them. Estimated impacts on the level of the recidivism rate are mainly precise zeros in Florida and on the small end of the previous literature in Georgia, while impacts expressed as a proportion of sample-mean recidivism rates are more in line with the moderately negative comparable estimates in a previous literature.

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Introduction

The three chapters of this dissertation are summarized below.

Racial Differences in Selection into World War II Army Enlistment: Evidence from the 1930 Census and 1935-1940 HOLC Maps

This chapter aims to better understand whether there were racial differences in how representative World War II veterans were of their birth cohorts. I study prewar proxies for the postwar economic and racial exclusivity of these cohorts' youth-age neighborhoods: 1930 Census characteristics and grades on Home Owners' Loan Corporation (HOLC) maps. Specifically, I analyze racial differences in selection on these neighborhood attributes into World War II Army enlistment. These attributes would be useful as baselines for these cohorts' chosen postwar locations that are exogenous to these choices' aggregate impacts on neighborhoods.

Between 1935 and 1940, HOLC created maps grading city neighborhoods on their supposed mortgage creditworthiness. These maps are frequently cited as a proxy for racially discriminatory housing policies such as redlining in low-graded areas and restrictive covenants with court enforcement in high-graded areas. Minority neighborhoods often received low grades, indicating a lack of racial exclusivity (albeit also limited mortgage financing due to redlining).

My analysis proceeds in three parts. First, consistent with Aaronson et al. (2017), I find that lower-graded HOLC neighborhoods had lower 1930 Census shares of white residents, homeowners, and more. Second, this relationship generally does not substantially differ between the heads of household of youths matched as future enlistees and youths not matched. And third, both white and African American World War II enlistees are largely representative of their birth cohorts. However, future volunteer enlistees specifically resided in more expensive housing with higher head-of-household radio ownership and literacy. White future volunteer enlistees also resided in higher-graded neighborhoods.

Racial differences in data matching and Census coverage represent potential sources of bias. But aside from these potential biases, this lack of substantial racial differences in selection into being enlisted in my results suggests that such differential selection is likely not a noteworthy driver of racial differences in veterans' postwar outcomes.

How Freshman Roommates Affect Political and Social Attitudes

This chapter studies how randomly-assigned freshman roommates affected students' political and social attitudes at a large public university in the US south. White students who were assigned black or other minority roommates compared with those assigned white roommates did not report significantly more support for affirmative action or campus diversity, and they did not have more comfortable interactions with people from other racial/ethnic groups. They did report more frequent interactions with black individuals and a higher proportion of friends who were in their own social class, though this finding is not robust. Similarly, support for immigration did not significantly differ by their roommates' race, ethnicity, or first-generation immigrant status. Finally, students' broader sociopolitical attitudes did not appear to significantly differ by their roommates' sociopolitical attitudes in a systematic way, despite scattered exceptions. My results suggest in contrast to previous studies that, at least during the college years, people's attitudes toward affirmative action and other issues are largely unchanged by those of new peers.

Incarceration and Recidivism: Evidence from Car-Crash Offenses

Using quasi-random outcomes of car crashes in Florida and Georgia, I provide estimates of the component of the total effect of incarceration length on recidivism arising from specific deterrence plus aging. In particular, I examine the recidivism rates of drivers sentenced for serious vehicular offenses (DUI or reckless driving), comparing those whose victims suffered death by vehicle (vehicular manslaughter or homicide) to those whose victims suffered serious injury. I focus on recidivism involving no new vehicular offenses, although I show that my results are largely robust to considering any recidivism, whether involving new vehicular offenses or not. The death of victims in a car crash leads to a discrete increase in sentence length for the perpetrator relative to similar crashes in which victims are seriously injured but survive.

Using variation in incarceration length among these drivers, my estimates are precise zeros for Florida and small but mostly significantly negative for Georgia, indicating from modest to no impact of incarceration length on the level of the three-year recidivism rate. I also calculate proportional impacts on recidivism rates since earlier studies use samples with higher recidivism rates. Then I test whether my data reject effects as negative as benchmark values from the literature. Left-tailed z-tests on my IV estimates for Florida reject all benchmark previous estimates of level impacts, but they fail to reject the small end of benchmark proportional-impact estimates. For Georgia, left-tailed z-tests fail to reject only the small end of benchmark level-impact estimates and also fail to reject all benchmark proportional-impact estimates. These findings for my basic results are robust to Oster (2017) bias correction on a modified IV specification using her suggested parametric assumptions for unobservables. And aside from less precise Georgia level impacts, these findings largely also carry over to specifications using a longer recidivism window, using only younger offenders or only older offenders, or using only car crashes early in the data window (to minimize differential attrition). Pooling the two states to control for fixed differences between comparison groups, my estimates are imprecise zeros.

The comparison of my findings to those in the literature suggests that the marginal effect of lengthening incarceration on recidivism by specific deterrence plus aging may be heterogeneous by incarceration length and recidivism risk. It is still unclear whether additional incarceration consistently reduces recidivism. But where it does reduce recidivism, proportional impacts on recidivism rates appear to be more comparable across different types of offenders than absolute impacts.

Chapter 1

Racial Differences in Selection into World War II Army Enlistment: Evidence from the 1930 Census and 1935-1940 HOLC Maps

1.1 Introduction

For much of the twentieth century, America's racial and ethnic minorities faced discrimination in buying and renting housing (Rothstein 2017). Their access to neighborhoods with amenities such as quality schools and strong community organizations was limited, and they were denied access to robust housing appreciation. Particularly in light of the growing neighborhood-effects literature, longstanding differential neighborhood access may have substantially contributed to racial and economic inequality and segregation. For example, Chetty and Hendren (2016) estimate that in more recent decades "roughly one-fifth of the gap in earnings between blacks and whites can be attributed to the counties in which they grow up."

In this study, I look at the World War II generation's "baseline" neighborhoods when they were youths. Intergenerational inequality was to be shaped profoundly by their neighborhood locations later as adults in the mid-twentieth century. I focus on racial differences in the proxied-for post-war economic and racial exclusivity of these "baseline" neighborhoods where future World War II Army veterans and non-veterans resided in 1930. Specifically, I analyze the 1930 Census characteristics and 1935-1940 HOLC grades of the 1930 Census neighborhoods of a limited Census sample: males native-born between April 2, 1915 and April 1, 1926 inclusive across 145 U.S. cities. Being aged 4 to 14 on April 1, 1930, these individuals were typically living with their parents.

Estimating any racial differences in World War II generation native-born males' selection into being enlisted serves two key purposes. First, particularly focusing on draftees, it provides suggestive evidence regarding any differential barriers to being called and then classified for service (though some barriers such as health and education standards that may have partially legitimate motivations). And second, it assesses whether racial differences in youth-age household and neighborhood were substantial enough to plausibly be a core driver of any racial differences in World War II veterans' postwar outcomes.

The 145 cities analyzed are among those covered by the 151 city maps created by the Home Owners' Loan Corporation (HOLC) between 1935 and 1940 and recently digitized by the Mapping Inequality project (Nelson et al. 2018). Various scholars have accused HOLC of having downgraded even minimally non-white neighborhoods based in part on race (Jackson 1985, Sugrue 1996, Gotham 2002). And papers such as Appel and Nickerson (2016) and Aaronson et al. (2017) have found that lower-rated neighborhoods experienced persistent, long-run decline likely due to credit rationing. There is some controversy as to how widely these HOLC maps were actually shared across government agencies and private lenders who might have redlined credit. Still, the maps and neighborhood-characteristic data underlying them were shared between HOLC and the FHA (Hillier 2003). The latter drew up its own, largely destroyed, maps that were used to determine neighborhood eligibility for FHA and VA mortgage guarantees, impacting millions of eligible families (Greer 2014). I study HOLC map zones as a geographically detailed proxy for post-war racial exclusivity, as indicated by discriminatory policies like post-war redlining by the FHA and private lenders in non-exclusive neighborhoods and court enforcement of racial covenants in exclusive neighborhoods. Note that redlining at least did not reach its full scale until after my 1930 Census snapshot of individuals' neighborhoods, though racial covenants had already begun proliferating nationwide by the 1920s (Hillier 2003, Correa-Jones 2000).

In my analysis, I first perform a partial replication of Aaronson et al. (2017) to investigate the relationship between 1930 Census household characteristics and later HOLC grades. I then test for differences in this relationship between the 1930 heads of household of future World War II Army enlistees versus those of future World War II Army non-enlistees. Finally, I evaluate whether future Army enlistees were differentially selected for African Americans compared to whites, distinguishing future drafted enlistees from future volunteer enlistees. I test for differential selection on the individual characteristics of sample males' heads of household as well as HOLC grades of their neighborhoods of residence when they were youths.

I find that future enlistees overall, and drafted enlistees specifically, were largely representative of their birth cohorts in terms of youth-age household characteristics and eventual neighborhood HOLC grades. On the other hand, for both whites and African Americans, future volunteer enlistees appear to have resided in more affluent households as youths than future non-enlistees. While white future volunteer enlistees also resided in neighborhoods with higher eventual HOLC grades, the same does not hold for African American future volunteer enlistees. In light of the Census's history of undercounting African Americans and my sample's lower match rates for African Americans, my results could be affected by racial differences in Census coverage or in enlistment-card matching with the Census. But it's not clear in what direction these might introduce bias.

For my data, I match 1930 Census males native-born between April 2, 1915 and April 1, 1926 to World War II Army enlistment cards for veterans native-born 1915-1926. I exclude the Enlisted Reserve Corps. These enlistment cards include the bulk of World War II veterans who served in this largest armed service branch. I distinguish future drafted enlistees from future volunteer enlistees based on the first digit in enlistment cards' Army Serial Numbers. I then geocode sample individuals' households and match them to HOLC city maps to obtain zone grades.

This paper proceeds as follows. Section 1.2 examines the historical background on government-supported racial discrimination in housing and military enlistment, Section 1.3 reviews the previous literature using FHA or HOLC maps to study possible racial discrimination in housing, Section 1.4 describes the data for this study, Section 1.5 presents the analysis and results, Section 1.6 discusses possible selection bias, and Section 1.7 draws conclusions. The Appendix re-runs relevant results under an alternative enlistment card matching procedure and then lays out the data matching and construction procedures.

1.2 Historical background on government-supported racial discrimination in housing and military enlistment

Beyond private discrimination by sellers and landlords, governments from the local to federal levels have been accused of enforcing or enabling racial discrimination in housing through the mid-twentieth century by enforcing racially restrictive covenants preventing sales to minorities (until 1948), possibly redlining minority neighborhoods from government mortgage insurance while restricting financial institutions' provision of low-down payment mortgages without this government insurance, backing segregated public housing and FHA-insured private developments, licensing real estate brokers under boards enforcing discrimination through codes of ethics, and more. And financial institutions may have frequently redlined minority neighborhoods as well, sometimes refusing aspiring homeowners and yet financing blockbusters (Rothstein 2017).

There is some empirical evidence to support historical arguments that systemic housing discrimination continued through the mid-twentieth century. For example, Cutler, Glaeser, and Vigdor (1999) find that in the mid-century, blacks paid more for housing in segregated than integrated cities even though migrants paid no more than longer-term residents, suggesting that whites used quasi-legal or other collective action to exclude blacks from white neighborhoods. By 1990, however, whites were paying more than blacks for similar housing, suggesting that the perpetuation of segregation had become primarily decentralized and driven by whites' preferences to live in white neighborhoods.

Below I further describe two key government policies supporting housing discrimination in the mid-twentieth century, possible redlining by the FHA and VA as well as court enforcement of racially restrictive covenants (until 1948). These policies generally functioned in complementary fashion in terms of neighborhood coverage: Home buying in white neighborhoods by minorities was frequently limited by racially restrictive covenants and other exclusionary policies. And affordable home buying in minority and mixed neighborhoods by minorities and non-minorities alike was frequently limited by redlining in conjunction with soundness regulations restricting lenders' provision of uninsured low-down payment mortgages (Gordon 2005). Thus minorities in many cases had to turn to installment contracts to finance home purchases, and they were foreclosed on sometimes without any built-up equity to recover if they failed to complete the course of payments, with foreclosure in turn depressing the neighborhood housing market (Rothstein 2017). I argue that redlining and racially restrictive covenants are among the likeliest such policies to have been widespread, impactful, and substantially correlated with HOLC map zone grades, which my analysis uses as a proxy for neighborhood racial exclusivity.

Redlining in the housing market is the practice by financial institutions or government agencies of withholding (or steeply pricing) mortgages or mortgage insurance in neighborhoods deemed to carry high impairment risk. In numerous cases, redlining as implemented has been criticized as having a sharply disparate impact by race or even partly reflecting direct racial discrimination beyond the pretext of a rational business purpose. Alongside a long history of redlining by private lenders until the 1968 Fair Housing Act and beyond, the FHA and VA allegedly withheld mortgage insurance (called a "guarantee" for the VA) either categorically or at least presumptively based on neighborhood risk grade (Hillier 2003, Greer 2014). In his guidance on FHA risk map creation, FHA economist Homer Hoyt suggested that the presence of even one nonwhite family should preclude a top "A" grade, and a higher than 10% nonwhite share should demand a "D" grade barring mortgage insurance in the neighborhood. FHA mortgage insurance applications would be rejected, among other reasons, for locating in low-rated neighborhoods on FHA risk maps or scoring below 50 points on any appraisal category including neighborhood location. Even the applicant's own "racial descent" was listed for the appraisers determining a mortgage's insurance eligibility (Light 2010, Light 2011). The

FHA and VA programs employing this redlining were particularly central to the post-World War II housing construction boom, together insuring mortgages on about 40% of all housing built from 1935 to 1951 (Grebler 1953).

However, the risk maps created by the Home Owners' Loan Corporation are the ones most associated with redlining today, likely due to their coverage, availability, and stark neighborhood categorization: the maps split 239 U.S. metro areas into zones graded A ["Best"], B ["Still Desirable"], C ["Definitely Declining"], or D ["Hazardous"] based on perceived mortgage risk. In addition to conventional economic factors, these risk assessments incorporated various racial stereotypes about how a neighborhood's growth path could supposedly be affected by the presence of a mere handful of nonwhite or foreign families. While housing stock quality and other correlated neighborhood factors have made it difficult to establish to what extent racial composition directly impacted zone grades, the association between race and grade is often stark: for example, all HOLC zones in Chicago, Cleveland, and Saint Louis with any black families were graded C or D, and the large majority were graded D (Greer 2013). Despite controversy as to whether the HOLC maps were widely shared or used directly for redlining, the recent literature has relied on these maps and found long-term effects of their zone grades on neighborhoods (see section 1.3).

Racially restrictive covenants, on the other hand, covered more heavily white and often affluent neighborhoods. They were written into property deeds and bound any owners not to sell or rent the property to minorities, immigrants, or other specified groups. While racially restrictive covenants existed before the invalidation of explicit racial zoning ordinances in 1917, they proliferated thereafter as an alternative means to effectively prevent minorities from moving into restricted neighborhoods (Correa-Jones 2000). Only in 1948 were property owners barred by Supreme Court decision Shelley v. Kraemer from having a court enforce a covenant against fellow property owners in their neighborhood and thereby void a noncompliant sale. While nationally representative statistics on racially restrictive covenants are lacking, in Detroit specifically over 80% of housing outside of the inner city had racially restrictive covenants by the 1940s, as did every subdivision developed between 1940 and 1947 (Sugrue 1996). Given their primary roles in the U.S. housing market, the FHA and VA (largely following FHA standards) likely helped catalyze neighborhoods' adoption of racially restrictive covenants by promoting them as highly favorable for mortgage insurance/guarantee approval (Jackson 1985, Woods II 2013, Rothstein 2017). The FHA Underwriting Manual stated that a racially restrictive covenant was favorable in the mortgage insurance risk appraisal, urged that insured homes be protected from "adverse influences" like the presence of nonwhite homeowners in the neighborhood, and, in later versions, provided a suggested deed with a racially restrictive covenant to bind the entire subdivision (Ibid).

Some data exist showing the exclusion of blacks from housing due to public and private policies more broadly. Less than 2% of FHA-insured housing was available to blacks as of 1959 (US Commission on Civil Rights 1959). And of the 200,000 housing units built in Philadelphia from 1946 and 1955, only 1,927 were available to blacks (Walton 1959). What housing stock was available to blacks likely skewed older and thus lower quality: despite the aforementioned racially restrictive covenants on the newer subdivisions, in the Detroit area 47,000 of 545,000 housing units overall were available to blacks in 1947 (Sugrue 1996).

Besides obtaining housing, military enlistment is the other key margin I study that was historically constrained by racial discrimination. The military remained segregated throughout World War II, and blacks serving in the military were disproportionately consigned to lower ranks and service roles. They constituted a relatively higher share of the Army and relatively lower shares in the Marines and Navy, but their representation in the armed services remained below their population share of 10.6% (DeBruyne and Leland 2015, National WWII Museum 2017, National Museum of the Pacific War 2018). For a time, blacks were called to active duty at well below their population share, reflecting a supposed lack of segregated facilities but also possibly a disinclination to accept black soldiers in some military branches. Black men also faced disproportionate 4-F classification as "mentally deficient." About one-third of black rejectees for service were classified as such, compared with 8% of white rejectees (Jenkins et al. 1944, Smith 2013). In some cases, vague criteria permitted classification to be used as a pretext for racial exclusion: newspapers ran stories of black examinees at local draft boards who were asked if they supported segregation and then labeled mentally deficient if they responded in the negative. Relatively lower levels of education among blacks may also have played a role: 14 of the 15 states with the highest rejection rates for black examinees were in the South with segregated educational systems (Ibid). Besides differentially selecting service-age men by race into actual enlistment, this disparate treatment of potential enlistees may have induced differential selection into even volunteering to be enlisted, including on education or on residence outside the Jim Crow South.

1.3 Previous literature using FHA and HOLC maps to study possible racial discrimination in housing

As I detail next, the literature overall depicts race and national origin as important determinants of HOLC grades (albeit not clearly as important as correlated housing value and quality, to the extent that determinants can be successfully disentangled) and depicts HOLC redlining as having persistent long-run, negative consequences for neighborhoods.

Although the HOLC maps have given rise to numerous studies, there remains uncertainty around who, if anyone, actually directly used their zone boundaries to determine mortgage eligibility. The FHA and VA weighed eligibility for individual (and for the FHA, multifamily) housing loan guarantees in part based on separate, largely destroyed risk grade maps (Hillier 2003, Greer 2014). And HOLC's own lending, in contrast to the FHA's lending guarantees, did not widely discriminate against neighborhoods based on demographic or economic characteristics, although HOLC did allow discrimination against individual buyers of foreclosed homes (Hillier 2003). There is controversy around how widely the HOLC maps were inputs into redlining maps made by other institutions accused of redlining like private lenders and the FHA, though it is known that HOLC did share its maps with associated zone demographic characteristics with the FHA (Ibid).

Despite this uncertainty about the HOLC maps' direct usage for redlining, empirical studies have frequently assessed gradients in credit rationing around these boundaries since the HOLC maps are detailed, publicly available, and contemporary with government and private-lender redlining policies during the post-Depression mortgage boom. My analysis employs neighborhoods' HOLC grades specifically as a proxy for neighborhoods' possible racially discriminatory policies, such as redlining or racially restrictive covenants discussed in section 1.2.

Determinants of redlining

The previous literature has taken divergent approaches to disentangling the possible determinants of redlining, especially residents' race and national origin. For example, Greer (2014) analyzes determinants of FHA zone grades in one of the few cities with known surviving FHA risk grade maps, Chicago. He estimates race to be a moderate predictor of risk grade alongside similarly or more important predictors like home value, age, amenities, and more. Race was, however, frequently closely related to housing quality, and Chicago's "Black Belt" and the surrounding areas were effectively redlined. Light (2010) similarly assesses nationality to significantly predict zone grade on the Chicago FHA map.

Taking a discontinuity approach by comparing adjacent census tracts across grades boundaries in 52 cities, Krimmel (2017) documents that blacks disproportionately lived in neighborhoods given low grades. Rather than just analyzing border tracts, Greer (2012) opts to look at all zones in 26 cities while employing housing market controls derived from the zone descriptions accompanying the maps. He finds that the market value, mortgage availability, age, and quality/repair of the housing stock are stronger predictors of HOLC grades than percentages of blacks and the foreign born. Finally, Hillier (2005) studies versions of HOLC maps from three different years for Philadelphia only, but she uses a spatial lag model with housing market controls so that her standard errors are robust to any spatial autocorrelation of the residuals. Race status significantly predicts HOLC grade, though its effect wanes in the later map versions. Foreign-born status is an inconsistently significant predictor across years.

Consequences of redlining

The literature attempting to evaluate the consequences of neighborhood grades on the HOLC maps has largely taken boundary-discontinuity and placebo approaches to identification.

Aaronson et al. (2017) find that neighborhoods with lower HOLC grades had reduced long-run homeownership, home values, and credit scores through to the modern day, presumably due to reduced credit access. Similarly, Appel and Nickerson (2016) assess that neighborhoods rated lower by HOLC had lower housing prices in 1990, despite a lack of discontinuities in housing characteristics across the zone boundaries of the HOLC maps at the time they were drawn. They point to possible credit rationing due to redlining, which may have had a persistent effect through feedback running between lower prices and fewer owner-occupied and non-vacant structures.

While lower-graded neighborhoods may have had housing markets that were persistently depressed, Hillier (2003) suggests that these neighborhoods were not categorically cut off from mortgage lending. Using Philadelphia address-level mortgage data from 1938 to 1950, she examines how numbers of mortgages, interest rates, loan-to-value ratios, and types of lenders all vary by HOLC grade and distance to the nearest redlined area. Her analyses show that red-graded areas had higher mortgage interest rates, but there was still conventional lending activity rather than a clear-cut denial of credit.

Unlike this literature studying HOLC grades' direct impacts on physical neighborhoods, this paper focuses on how HOLC grades relate to the demographics of residents. Therefore, rather than comparing HOLC zones specifically near grade discontinuities, I compare them zone-wide in aggregate.

1.4 Data

My initial full sample comprises all IPUMS-USA 100% 1930 Census persons in 145 U.S. cities, while an initial second, limited sample comprises all World War II generation native-born males (aged 4 to 14 on April 1, 1930) in these cities. My analysis will require three types of variables about sample persons: 1930 Census household characteristics, 1930 neighborhood's future HOLC grade, and a dummy variable on the limited sample for matching to a World War II Army enlistment card. Only observations in the initial full and limited samples that have been successfully geocoded and matched to HOLC grades enter the final full and limited samples.

To join these three types of variables together, I first match 1930 Census males in the initial limited sample to World War II Army enlistment cards from the National Archives for native-born veterans from the 1915-1926 birth cohorts. I exclude the Enlisted Reserve Corps, only a fraction of whom served in the war on active duty. A dummy for future enlistment allows me to study the households of matched future WWII Army enlistees (entering once for each enlistee in the household) as a subset of all sample males' households. I can further distinguish future drafted enlistees from future volunteer enlistees using the first digit in the enlistment cards' Army Serial Numbers. Of the 5,323,490 WWII Army enlistment cards for veterans native-born 1915-1926 with valid fields for matching, I match the percentages below of enlistees (by race and drafted versus volunteer) to 1930 Census males in the limited sample using a fuzzy bigram match that I use for my main results.

Table 1.1: *Match rates - bigram method used for main results*

Percent of enlistment cards matching to a sample 1930 Census male	Overall	White	African American
Overall	53.3%	55%	41.9%
Drafted	52.9%	54.9%	41.9%
Volunteer	54.4%	55.2%	40.7%

And I obtain the following match rates using an exact match with wildcards, on which I re-run relevant results in the Appendix to demonstrate robustness to a higher-confidence matching procedure. Both matching procedures are described in Appendix section A.2.

Table 1.2: *Match rates - wildcard exact method used for results in Appendix*

Percent of enlistment cards matching to a sample 1930 Census male	Overall	White	African American

Table 1.2 (Continued)

Overall	46.1%	47.5%	37.1%
Drafted	45.7%	47.3%	37.1%
Volunteer	47.5%	48.1%	36.7%

For both procedures, the match rates for African Americans are somewhat lower. Section 1.6 will discuss whether selection bias underlies this difference and, if so, how it could affect my results.

Next I geocode 1930 Census households and match them to the Mapping Inequality project's city HOLC maps by address. These HOLC maps contain zone polygons rated A ["Best"], B ["Still Desirable"], C ["Definitely Declining"], or D ["Hazardous"]. The 151 HOLC city maps I use partially cover at least my sample of 169 1930 IPUMS standardized cities (listed in Appendix sections A.4 and A.5), which include 22 of the 25 largest cities in 1930 and 37 of the largest 50. 141 of 7,463 HOLC shapes were detected as invalid polygons and were not used to match to Census households. Although I geocode 169 1930 IPUMS standardized cities, I analyze only 145 of them: the 149 cities analyzed by Aaronson et al. (2017) minus Buffalo, NY, Essex County, NJ, Hudson County, NJ, and Lexington, MA.

Of the 11,430,669 households residing in these 169 1930 IPUMS standardized cities with valid fields for matching, I geocoded 79.37% using ArcMap with ESRI Business Analyst after cleaning the data with the Urban Transition Historical GIS Project's "histcensusgis" Python package, as described in Appendix section A.3. Again, not all of the households in these cities necessarily resided in HOLC map zones. Note though that 369,318 or 5.24% of the 7,051,678 geocoded Census households in these cities that did match to HOLC zones were matched to multiple zones due to polygon overlaps; I drop these households from my analysis.

My geocoding and data matching procedures are outlined in Appendix sections A.2-A.5.

1.5 Analysis and results

My analysis proceeds in three parts. First, I partially replicate Aaronson et al. (2017) on my full sample of 1930 Census persons. Columns (1)-(4) of their Table 1 (1930 row only) and column (1) of their Table 2 explore the relationship between 1930 Census household characteristics and future HOLC grades. I replicate these tables in my Tables 1.3 and 1.4, respectively. This partial replication also serves to benchmark my geocoding of the 1930 Census against theirs.

My replication Table 1.3 presents average 1930 Census household characteristics by neighborhood HOLC zone grade. Characteristics include share African American, homeownership share, average home value, and share foreign born. And focusing on the reverse relationship of 1930 household characteristics as predictors of HOLC grades, my replication Table 1.4 presents an ordered logistic regression of neighborhood HOLC zone grade (D=4, C=3, B=2, A=1) on the household characteristics used above (logging home value) along with some others (log of rent, occupational score, employment, radio ownership, literacy, and school attendance). This regression uses log population weights, city fixed effects, and city standard error clustering.

The replicated summary statistics in Table 1.3 exhibit the same directional patterns as in Aaronson et al. (2017): lower-graded neighborhoods have higher African American and foreign-born shares and lower homeownership rates and home values. Compared to the original paper, the change in value moving from grade A to grade D in my data is generally somewhat steeper for share foreign born and homeownership share and flatter for home value.

The ordered logit regression estimates in Table 1.4 predicting a lower HOLC grade also have the same signs as in Aaronson et al. (2017). However, here African American share, share employed, share literate, and share attending school are statistically insignificant. A lower homeownership share, home value, occupation score, and share with radios continue to significantly predict a lower HOLC grade.

Overall, my partial replication is consistent with Aaronson et al. (2017) in finding evidence that lower-graded neighborhoods have lower shares of white residents, lower occupational income scores, less homeownership, lower housing costs, and worse housing amenities. This also suggests that our two geocoding outputs for the 1930 Census are broadly similar.

Table 1.3: *Aaronson et al. (2017) partial replication [Aaronson et al. (2017) Table 1 columns (1)-(4)]*

All Census persons/HOLC grade	A	B	C	D
Share African American	0.025	0.011	0.02	0.153
Share Foreign Born	0.106	0.155	0.198	0.222
Homeownership Share	0.736	0.576	0.461	0.337
Average Home Value	11773.34	8696.44	7169.55	5743.7

Table 1.4: *Aaronson et al. (2017) partial replication [Aaronson et al. (2017) Table 2 column (1) - Ordered logit on HOLC zones with log population weights, city fixed effects, and city standard error clustering]*

All HOLC zones	HOLC grade (A=1, B=2, C=3, D=4)
Share African American	-0.333 (0.62)
Share Foreign Born	1.37 (1.201)
Homeownership Share	-5.179*** (0.695)
Average Log Home Value	-1.897*** (0.265)
Average Log Rent	-0.432 (0.221)
Average Occupation Score	-0.147*** (0.026)
Share Employed	-2.153 (1.312)
Share with Radio	-5.261*** (0.711)
Share Literate	-0.926 (2.054)
Share Attending School	1.679 (1.335)
Cities	141
N	4710
Pseudo R^2	0.384

In the second part of my analysis, having benchmarked my data to the Aaronson et al. (2017) relationship between 1930 household characteristics and future HOLC grades, I seek to understand to what degree this relationship for future World War II Army enlistees specifically is representative of the relationship for my limited Census sample of all World War II generation males (native-born between April 2, 1915 and April 1, 1926 inclusive). I do so in Tables 1.5 and 1.6 repeating the analyses in replication Tables 1.3 and 1.4, but only on the 1930 heads of household of males in this limited sample (except school attendance is excluded and African American race is for the sample male himself). I also add in interactions with a dummy for the sample male's future World War II Army enlistment. Enlistment is identified by a "Fuzzy Bigram" match of Census persons to enlistment cards, as described further in Appendix section A.2.

However, the interaction terms are not statistically significant individually or jointly: when it comes to average household characteristics by HOLC grade as well as household characteristics predicting HOLC grade, the heads of the households of future enlistees do not significantly differ from those of the future non-enlistees in their native-born male birth cohorts. The same largely holds true in the Appendix, where enlistment is identified by an "Exact With Wildcards" match instead of a "Fuzzy Bigram" match. The main exception is that in Table A.1, the average shares by HOLC grade of the future enlistees' 1930 heads of household who are foreign born are now jointly significantly lower across HOLC grades than the average shares for non-future enlistees' 1930 heads of household. In Table A.2, although the coefficient estimate on African American share now significantly differs between enlistees and non-enlistees, the differences in estimates across all included household characteristics remain jointly insignificant.

Table 1.5: *Differences by enlistment in Aaronson et al. (2017) partial replication (using enlistment cards matched with Census by "Fuzzy Bigram" method; see Appendix section A.2 for results using "Exact With Wildcards" method). [Aaronson et al. (2017) Table 1 columns (1)-(4)] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons/ HOLC grade	A non-enlistee	A enlistee	p[t-test / H_0: non-enlistee =enlistee]	B non-enlistee	B enlistee	p[t-test / H_0: non-enlistee =enlistee]	C non-enlistee	C enlistee	p[t-test / H_0: non-enlistee =enlistee]	D non-enlistee	D enlistee	p[t-test / H_0: non-enlistee =enlistee]	p[F-test / H_0: non-enlistee =enlistee, for all grades]
Share African American	0.014	0.013	0.821	0.009	0.007	0.159	0.021	0.017	0.065	0.141	0.117	0.172	0.121
HoH Share Foreign Born	0.148	0.14	0.531	0.264	0.264	0.99	0.379	0.36	0.214	0.476	0.455	0.472	0.653
HoH Homeownership Share	0.754	0.745	0.581	0.611	0.595	0.42	0.488	0.476	0.317	0.344	0.334	0.624	0.7
HoH Average Home Value	11606.82	11553.21	0.918	8339.64	8458.52	0.677	6811.67	6705.03	0.53	5419.94	5474.33	0.786	0.957

Table 1.6: *Differences by enlistment in Aaronson et al. (2017) partial replication (using enlistment cards matched with Census by "Fuzzy Bigram" method; see Appendix section A.2 for results using "Exact With Wildcards" method). [Aaronson et al. (2017) Table 2 column (1) - Ordered logit on HOLC zones with log population weights, city fixed effects, and city standard error clustering] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

All HOLC zones	Non-enlistee	Enlistee Minus Non-enlistee
Share African American	1.172*	1.636
	(0.579)	(0.958)
HoH Share Foreign Born	1.5**	-0.234
	(0.481)	(0.662)
HoH Homeownership Share	-4.752***	0.004
	(0.598)	(0.729)
HoH Average Log Home Value	-2.029***	0.027
	(0.271)	(0.316)
HoH Average Log Rent	-0.354	-0.301
	(0.181)	(0.227)
HoH Average Occupation Score	-0.054**	-0.011
	(0.021)	(0.025)
HoH Share Employed	-2.366**	0.135
	(0.769)	(0.976)
HoH Share with Radio	-4.094***	0.975
	(0.565)	(0.695)
HoH Share Literate	-0.522	-2.007
	(1.017)	(1.639)
Cities	141	
N	4142	
Pseudo R^2	0.394	
p[chi2-test / H_0: Enlistee Minus Non-enlistee=0, for all regressors except intercept]	0.387	

Third and finally, I evaluate the extent to which future enlistees compared to future non-enlistees were selected on household characteristics and neighborhood HOLC grades within my limited Census sample of World War II generation males (native-born between April 2, 1915 and April 1, 1926 inclusive). I also test for any racial difference in this selection of future enlistees. Differential selection of enlistees by race could be potentially due to differing preferences, policy and administrative barriers, and so on.

In Table 1.7, I present summary statistics for white and African American future World War II Army enlistees versus future non-enlistees on neighborhood HOLC grades and the Census characteristics from the Aaronson et al. (2017) partial replication for the sample male's head of household (except school attendance is excluded and African American race is for the sample male himself). In Tables 1.8-1.9, I break down these summary statistics by race and drafted versus volunteer enlistees.

Then in Tables 1.10-1.11, I perform a logistic regression of a dummy for the sample male's future enlistment on neighborhood HOLC grades and Census characteristics for his head of household (except African American, log home value, log rent, and school attendance) as well as interactions with a dummy variable for the sample male's being African American. Standard errors are clustered by household. I test for differential selection of future enlistees compared to future non-enlistees by race based on the separate and joint statistical significance of the African American race dummy interaction terms. I also repeat this part performing a multinomial logistic regression predicting future status as a drafted or volunteer enlistee, with non-enlistment as the base outcome.

Differences in Table 1.7 summary statistics between the heads of household of future enlistees versus those of future non-enlistees are statistically significant but largely economically insignificant. However, future enlistees are somewhat less likely to be African American[1] or to have a foreign-born head of household and slightly more likely to have a radio.

In Tables 1.8-1.9, it is apparent that these modest overall differences reflect future non-enlistees' slight differences with future drafted enlistees together with their starker differences

[1]This is in line with the administrative data showing that African Americans' World War II representation in the armed services was below their proportion of the population (DeBruyne and Leland 2015, National WWII Museum 2017, National Museum of the Pacific War 2018).

with future volunteer enlistees. Future volunteer enlistees of both races reside in higher-value/rent housing (especially for African Americans) and have heads of households who are more likely to be literate and have a radio. White future volunteer enlistees specifically reside in higher-rated neighborhoods and are less likely to have a foreign-born head of household (whereas the latter is more likely, though still uncommon, for African American volunteer enlistees). Though African American volunteer enlistees appear on some characteristics to be more affluent than African American drafted enlistees and non-enlistees, they do not reside in substantially higher-rated neighborhoods: only about 2% of each of these subgroups of African American sample males resides in A- or B-rated neighborhoods compared to about 16-17% for sample white males overall. These patterns also hold in Tables A.4-A.5, where future enlistment is instead identified by an "Exact With Wildcards" match. Meanwhile, differences between future drafted enlistees and future non-enlistees are generally not economically significant: among whites, drafted enlistees' heads of household are slightly less likely to be foreign born in the main results only, and among African Americans, they are slightly more likely to be homeowners in both the main results and Appendix.

Tables 1.10-1.11 show that a head of household who is a homeowner significantly more positively predicts enlistment for African American sample males than white sample males,[2] but having a head of household with a radio or who is literate is a significantly more negative predictor for African American sample males than white sample males. Looking specifically at future volunteering to serve versus not enlisting, head-of-household radio ownership is now a significantly more positive predictor for African American samples males than white sample males, and head-of-household homeownership is an even more strongly positive predictor. These patterns again hold in the Appendix in Tables A.6-A.7, although radio ownership no longer significantly predicts overall enlistment differentially for African American versus white sample males.

Overall these results suggest that for both whites and African Americans, eventual drafted World War II Army enlistees are not substantially selected compared to non-enlistees in their

[2] Though significantly more positive as a predictor, having a foreign-born head of household is uncommon among African American sample males.

native-born male birth cohorts on their youth-age household characteristics or youth-age neighborhoods' eventual HOLC grades. Drafted enlistees comprised most of the enlistees overall, and the same holds broadly for enlistees overall. In contrast, in 1930 as youths, eventual volunteer enlistees of both races had heads of household likelier to have a radio, be literate, and keep more expensive owner-occupier or rental housing. However, this apparent relative affluence of future volunteer enlistees compared to future non-enlistees for both races did not translate into youth-age residence in higher-graded neighborhoods for African Americans, as it did for whites.

Table 1.7: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Fuzzy Bigram" method; see Appendix section A.2 for results using "Exact With Wildcards" method). [Summary statistics for future World War II Army enlistees versus non-enlistees] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	Enlistee	Non-enlistee	p[t-test / H_0: Enlistee Minus Non-enlistee=0]
Grade A	0.016	0.017	0
Grade B	0.139	0.131	0
Grade C	0.423	0.406	0
Grade D	0.421	0.445	0
African American	0.058	0.073	0
HoH Foreign Born	0.383	0.403	0
HoH Homeowner	0.437	0.444	0
HoH Log Home Value	8.586	8.572	0
HoH Log Rent	3.436	3.395	0
HoH Occupation Score	28.228	27.893	0
HoH Employed	0.854	0.845	0
HoH Radio	0.541	0.508	0
HoH Literate	0.952	0.941	0
N (less for some variables)	502070	1653943	

Table 1.8: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Fuzzy Bigram" method; see Appendix section A.2 for results using "Exact With Wildcards" method). [Summary statistics for future World War II Army draftees versus volunteers (enlistees) versus non-enlistees, separately for whites and African Americans] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White Enlistee	White Non-enlistee	p[t-test / H_0: White Enlistee Minus Non-enlistee=0]	African American Enlistee	African American Non-enlistee	p[t-test / H_0: African American Enlistee Minus Non-enlistee=0]
Grade A	0.017	0.018	0	0.003	0.003	0.561
Grade B	0.146	0.14	0	0.017	0.017	0.74
Grade C	0.443	0.43	0	0.124	0.117	0.003
Grade D	0.394	0.412	0	0.856	0.863	0.005
African American	0	0	.	1	1	.
HoH Foreign Born	0.404	0.431	0	0.038	0.036	0.069
HoH Home-owner	0.452	0.465	0	0.211	0.194	0
HoH Log Home Value	8.607	8.595	0	7.841	7.866	0.089
HoH Log Rent	3.474	3.443	0	3.002	2.986	0.01
HoH Occupation Score	28.759	28.562	0	20.115	19.911	0.002
HoH Employed	0.855	0.847	0	0.823	0.812	0
HoH Radio	0.563	0.534	0	0.184	0.176	0.002
HoH Literate	0.955	0.944	0	0.916	0.907	0
N (less for some variables)	472802	1530285		28944	120465	

Table 1.9: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Fuzzy Bigram" method; see Appendix section A.2 for results using "Exact With Wildcards" method). [Summary statistics for future World War II Army draftees versus volunteers (enlistees) versus non-enlistees, separately for whites and African Americans] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White Draftee	p[t-test / H_0: White Draftee Minus Non-enlistee=0]	White Volunteer	p[t-test / H_0: White Volunteer Minus Draftee=0]	White Non-enlistee	African American Draftee	p[t-test / H_0: African American Draftee Minus Non-enlistee=0]	African American Volunteer	p[t-test / H_0: African American Volunteer Minus Draftee=0]	African American Non-enlistee
Grade A	0.015	0	0.022	0	0.018	0.003	0.399	0.002	0.135	0.003
Grade B	0.138	0.003	0.171	0	0.14	0.017	0.729	0.017	0.919	0.017
Grade C	0.438	0	0.459	0	0.43	0.122	0.018	0.138	0.037	0.117
Grade D	0.41	0.011	0.35	0	0.412	0.857	0.026	0.842	0.067	0.863
African American	0	.	0	.	0	1	.	1	.	1
HoH Foreign Born	0.425	0	0.342	0	0.431	0.037	0.697	0.06	0	0.036
HoH Homeowner	0.455	0	0.44	0	0.465	0.21	0	0.221	0.209	0.194
HoH Log Home Value	8.593	0.39	8.651	0	8.595	7.823	0.005	8.031	0	7.866
HoH Log Rent	3.459	0	3.518	0	3.443	2.992	0.329	3.113	0	2.986
HoH Occupation Score	28.49	0	29.563	0	28.562	20.056	0.03	20.762	0.001	19.911

Table 1.9 (Continued)

HoH Employed	0.853	0	0.864	0	0.847	0.822	0	0.83	0.315	0.812
HoH Radio	0.548	0	0.605	0	0.534	0.178	0.366	0.25	0	0.176
HoH Literate	0.949	0	0.971	0	0.944	0.914	0.001	0.94	0	0.907
N (less for some variables)	353261		119541		1530285	26554		2390		120465

Table 1.10: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Fuzzy Bigram" method; see Appendix section A.2 for results using "Exact With Wildcards" method). [Logit on sample persons predicting future enlistees (and multinomial logit predicting draftees and volunteers, with non-enlistees as the base outcome), with African American race interaction terms and household standard error clustering] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White	African American Minus White
Grade A Minus Grade D	-0.084*** (0.016)	0.185 (0.136)
Grade B Minus Grade D	0.05*** (0.007)	-0.039 (0.06)
Grade C Minus Grade D	0.049*** (0.005)	0.023 (0.024)
Grade D	-1.358*** (0.013)	-0.236*** (0.04)
HoH Foreign Born	-0.07*** (0.004)	0.16*** (0.041)
HoH Homeowner	-0.064*** (0.004)	0.142*** (0.02)
HoH Occupation Score	0 (0)	0.002 (0.001)
HoH Employed	0.032*** (0.007)	0.016 (0.025)
HoH Radio	0.09*** (0.004)	-0.062** (0.021)
HoH Literate	0.147*** (0.01)	-0.072* (0.03)
N	1467458	116402
p[F-test / H_0: African American Minus White=0, for all regressors except intercept]	0	

Table 1.11: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Fuzzy Bigram" method; see Appendix section A.2 for results using "Exact With Wildcards" method). [Logit on sample persons predicting future enlistees (and multinomial logit predicting draftees and volunteers, with non-enlistees as the base outcome), with African American race interaction terms and household standard error clustering] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White (Y=DRAFT)	African American Minus White (Y=DRAFT)	White (Y=VOLUNTEER)	African American Minus White (Y=VOLUNTEER)
Grade A Minus Grade D	-0.188*** (0.019)	0.331* (0.139)	0.18*** (0.026)	-0.792 (0.577)
Grade B Minus Grade D	-0.009 (0.007)	0.019 (0.062)	0.22*** (0.011)	-0.203 (0.182)
Grade C Minus Grade D	0.02*** (0.005)	0.037 (0.025)	0.143*** (0.008)	0.089 (0.07)
Grade D	-1.532*** (0.014)	-0.108** (0.041)	-3.2*** (0.026)	-1.373*** (0.127)
HoH Foreign Born	-0.012* (0.005)	0.065 (0.043)	-0.25*** (0.008)	0.669*** (0.106)
HoH Homeowner	-0.037*** (0.005)	0.114*** (0.021)	-0.149*** (0.007)	0.23*** (0.06)
HoH Occupation Score	-0.001*** (0)	0.003** (0.001)	0.005*** (0)	0.003 (0.003)
HoH Employed	0.025** (0.008)	0.021 (0.026)	0.055*** (0.013)	0.01 (0.075)
HoH Radio	0.059*** (0.005)	-0.067** (0.022)	0.184*** (0.008)	0.188** (0.057)
HoH Literate	0.086*** (0.011)	-0.03 (0.031)	0.44*** (0.022)	-0.1 (0.102)
N	1467458		116402	

Table 1.11 (Continued)

p[F-test / H_0: African American Minus White=0, for all regressors except intercept]	0

1.6 Possible selection bias

While I regard the selection of World War II Army enlistees into my enlistment-card data set (see Appendix section A.2) as likely to be largely orthogonal to their characteristics, next I discuss two other potential sources of selection bias: Census coverage and enlistment-card matching with the Census.

To better understand how the biases might affect my results, consider the key comparison of whites versus African Americans in Tables 1.8-1.9 and A.4-A.5 on the differences between "enlistees" and "non-enlistees" in a given characteristic C. In particular, let C be a characteristic on which sample whites score higher on average than sample African Americans.

My analysis in these tables targets this desired comparison. But given my data, I am in fact calculating differences between enlistees matched with the Census and the unmatched males in my limited Census sample, who in turn consist of the unmatched enlistees and non-enlistees who are actually present in that limited sample.

Call "M" the subgroup of enlistees on sample enlistment cards that would match to my limited sample of eligible males drawn from a hypothetical Census with 100% coverage. Call "N" the subgroup of enlistees on sample enlistment cards that would not match but would still be in my limited sample of this hypothetical Census. Finally, call "O" the non-enlistee remainder of my limited sample of this hypothetical Census. A prime symbol marks the subset of a given subgroup that is actually present in my limited sample drawn from the actual Census, regardless of my success or failure to identify these individuals in the subset through my match process. Make the simplifying assumption that an enlistment card and a Census individual either match or do not match independently of the other enlistment cards and Census individuals involved in the match process, which is not strictly true in my match process (see Appendix section A.2). Then M′ are the enlistees in my limited sample who are matched to enlistment cards, N′ are the unmatched enlistees actually present in my limited sample, and O′ is the non-enlistee remainder of my limited sample. It should be clear that by their definitions I cannot use my match to distinguish subgroups N′ and O′.

The desired comparison I am targeting is then of whites versus African Americans on the differences between enlistees M U N versus non-enlistees O on the characteristic C, or C[M U N]-C[O].

First, consider this difference for each of African Americans and whites separately. Research back to the 1940 Census suggests that African Americans, especially men, are underincluded in the Census, and two explanations that have been offered are "deliberate omission" (often by those on public assistance) and atypical "residential arrangements" (Hainer et al. 1988). If these causes lead lower socioeconomic status to correlate with more 1930 Census undercounting, the subgroup (either enlistees or non-enlistees) with the higher average score for characteristic C might have higher Census coverage and so be less positively selected into the Census.[3] The shift from C[M U N]-C[O], the desired difference, to C[M′ U N′]-C[O′] is then directionally ambiguous, potentially including a change in sign.

Reaching the difference that I actually calculate then requires a second directionally ambiguous shift from C[M′ U N′]-C[O′] to C[M′]-C[N′ U O′], where unmatched enlistees actually contained in my Census limited sample are shifted to the second term: even if these unmatched enlistees N′ are differentially negatively selected relative to matched enlistees M′ so that C[M′]>C[N′], it's unclear how either would necessarily compare to C[O′]. And the sizes of subgroups M′, N′, and O′ impact the direction of this shift as well.

Thus the difference that I actually calculate C[M′]-C[N′ U O′] has an ambiguously signed bias relative to the desired difference C[M U N]-C[O].

Second, consider the desired comparison of whites W versus African Americans B on the differences between enlistees and non-enlistees: (C[M U N | W]-C[O | W])-(C[M U N | B]-C[O | B]) or (C[M U N | W]-C[M U N | B])-(C[O | W]-C[O | B]).

If lower socioeconomic status correlates with more 1930 Census undercounting as assumed above, the comparison group into which African Americans are relatively more negatively selected as compared to whites (either enlistees or non-enlistees) may exhibit the relatively more positive selection of African Americans as compared to whites into being included in the Census. This comparison group may thus exhibit the more understated racial gap in the

[3]It seems intuitively plausible that this could shrink the difference toward zero, but that need not be true: the bias depends on group differences in both the selection on C and the shape of the distribution of C (for example, less positive selection but a higher variance could actually translate into a more positive bias).

comparison that I actually calculate.[4]

That is, the sign of my desired comparison (C[M U N | W]-C[O | W])-(C[M U N | B]-C[O | B]) and equivalently (C[M U N | W]-C[M U N | B])-(C[O | W]-C[O | B]) may be the opposite of the bias relative to it of (C[M′ U N′ | W]-C[M′ U N′ | B])-(C[O′ | W]-C[O′ | B]), so the latter cannot reliably sign the former. And the latter is still two ambiguously signed shifts away from my actual comparison, (C[M′ | W]-C[N′ U O′ | W])-(C[M′ | B]-C[N′ U O′ | B]).[5]

In summary, potentially differential Census coverage and enlistment-card matching with the Census may lead to an ambiguously signed bias in my estimated differences between enlistees and non-enlistees and comparisons of whites versus African Americans on these differences.

1.7 Conclusion

After matching the 1930 Census microdata in 145 U.S. cities to HOLC map zone polygons and World War II Army enlistment cards, I study the youth-age neighborhoods of World War II Army enlistees and non-enlistees. I assess that the 1930 youth-age neighborhoods of future enlistees are largely representative of their native-born male birth cohorts on Census household characteristics and HOLC grades.

My results here show no substantial racial difference between enlistees and non-enlistees in youth-age household and neighborhood characteristics. Racial differences in Census coverage or in enlistment-card matching with the Census may introduce ambiguously signed bias into my results. But these potential biases aside, these results suggest that any racial difference uncovered by the literature in the exclusivity of post-war neighborhoods accessible to veterans would arguably be externally valid for the racial difference in post-war neighborhoods that hypothetically would have been accessible to all World War II generation males and their families if they had participated in military service and the GI Bill (although this extrapolation

[4] However, see the discussion in the previous footnote about how selection bias relates to both the selection on a characteristic and its distribution.

[5] After rearranging to (C[M′ U N′ | W]-C[O′ | W])-(C[M′ U N′ | B]-C[O′ | B]), this is clear based on the prior discussion about the bias in C[M′]-C[N′ U O′].

does not account for any general-equilibrium effects of an increased influx of returning veterans with GI Bill benefits). That is, my results suggest that any racial difference that existed in post-war neighborhood exclusivity among veterans and their families would not have been inflated by differential selection by race into military service.

Chapter 2

How Freshman Roommates Affect Political and Social Attitudes

2.1 Introduction

In the U.S. and elsewhere, polarization and segregation within the populace have been blamed for reducing indicia of government quality like social trust, service efficiency, political freedoms, timely policy adjustment, and partisan animosity (Dowd and Driessen, 2008; Alesina and Zhuravskaya, 2011; Helland and Sørensen, 2015; Lee, 2015; Webster and Abramowitz, 2017). Indeed, Americans are politically polarized by race and ideology: they are strongly sorted into the two major parties along these dimensions (Tesler and Sears, 2010; Lelkes, 2016). Americans are also politically segregated by race and ideology: they tend to discuss politics within social networks that are quite segregated on these dimensions, at least offline (Gentzkow and Shapiro, 2011). While recent polarization and segregation as presented in the studies above have not followed simple trend lines over time, their continuing salience in the U.S. and abroad has sparked considerable research on their relationship, especially how segregation versus contact shapes polarization (see Kim et al., 2018 for an overview).

Randomized roommate assignment remains one of the rare sources of exogenous long-term interpersonal contact across societal dividing lines such as race, ethnicity, parental income, and ideology. Roommate assignment across these lines could lead to deliberation or discord and to a convergence or divergence of various political attitudes. Roommate surveys can thus illuminate peer effects on political attitudes, which are of interest in and of themselves and which also constitute one of several channels through which contact may affect polarization.

In this paper, I assess roommate effects on surveyed political attitudes, building on previous studies estimating effects more narrowly on attitudes toward affirmative action and racial tolerance (Van Laar et al., 2005; Boisjoly et al., 2006; Shook and Fazio, 2008) as well as effects on political ideology indices (Zimmerman et al., 2004; Campos et al., 2016).

I analyze a sample of students who entered a large public university in the South in fall 2005 through fall 2007. These three classes of students were requested to complete the UCLA Higher Education Research Institute's Cooperative Institutional Research Program (CIRP) Freshman Survey before entering university, as with students entering many other universities nationwide.[1] Then these students were surveyed again in spring 2008 as part of an academic study (described further in section 2.3).

Using the student responses to the CIRP survey and follow-up survey, I first replicate the main analyses and robustness checks in Boisjoly et al. (2006). In particular, I estimate effects of roommate race, ethnicity, and roommate parental income on white students' support for affirmative action, support for racial diversity on campus, and comfortable interactions with people from other racial and ethnic groups. Compared with the sample in Boisjoly et al. (2006), my sample is drawn from a more racially and economically diverse university and one that is in the U.S. south as opposed to the Midwest.

I analyze how broader student outcomes (attitudes toward immigration as well as additional sociopolitical views and social behaviors) are affected by roommate characteristics (more refined race, ethnicity, first-generation immigrant status, and sociopolitical attitudes).

After including appropriate controls, I find no consistently statistically significant differences between white students assigned black roommates and those assigned white roommates in support for affirmative action, support for racial diversity on campus, or comfortable interactions with people from other racial and ethnic groups. Students' attitudes toward

[1] The CIRP Freshman Survey collects information about students' behaviors, attitudes, goals, academic background, and more.

immigration likewise are not significantly affected by roommate race/ethnicity, first-generation immigrant status, or political attitudes. And students' broader sociopolitical attitudes largely are not significantly affected by roommate race, ethnicity or broader sociopolitical attitudes. The lack of observed peer effects at least for the Boisjoly et al. (2006) replication could be partly attributable to white students' lower reported roommate closeness when assigned black roommates as compared with white roommates, a possibility I discuss in section 2.5.

This paper proceeds as follows. section 2.2 reviews the relevant literature, section 2.3 outlines the study background, data, and summary statistics, section 2.4 presents the main regressions and results, section 2.5 discusses the results, and section 2.6 concludes. The Appendix contains the full text of CIRP and follow-up survey questions that are summarized in my other tables along with additional regression results.

2.2 Literature review

The previous literature has examined peer effects on political ideology and political attitudes, including toward affirmative action.

In Zimmerman et al. (2004), randomly assigned roommates for freshman entering 1989 at two colleges had little effect on students' own self-rated liberal-conservative ideology index, except that students with far-left roommates identified as more conservative. Campos, Heap, and de Leon (2017) find that the self-identified political identification of Brazilian university freshmen was unaffected by the average political identification of their classmates, who are randomly assigned within majors. However, students with more politically-engaged classmates were more politically centrist.

Previous roommate studies have mostly concluded that being assigned minority roommates leads white students to more strongly favor racial and ethnic diversity. But the literature does not speak with a single voice and there are contrasting findings.

For instance, Van Laar et al. (2005) find that being assigned a black or Latino freshman roommate at UCLA increased several measures of whites' and Asian Americans' racial tolerance, such as having more ethnically diverse friends. Boisjoly, et al. (2006) find that being randomly assigned a black roommate increased whites' support for affirmative action and preference for racial diversity at university generally. Similarly, in Shook and Fazio (2008), white freshmen with randomly assigned black roommates, unlike those with white roommates, displayed a decline over their first semesters in negative racial attitudes and intergroup anxiety toward blacks. But whereas white freshmen assigned black roommates in Boisjoly et al. (2006) and those assigned white roommates reported comparably strong roommate relationships, white freshmen assigned black roommates in Shook and Fazio (2008) reported lower satisfaction, involvement, and time spent with roommates than those assigned white roommates.

The previous literature has also found peer effects on voter behavior, which may be driven at least in part by peer effects on political attitudes.

Klofstad (2010; 2015) studies the effects of political discussion by University of Wisconsin-Madison students on their voter turnout among other outcomes. Students who discussed "politics and current events" more with their randomly-assigned freshman roommates had higher civic participation lasting at least through senior year, including in charities and volunteering as well as student government, newspapers, and clubs. Among their various measures of political participation, including being active in partisan or politically vocal organizations and contacting elected officials about political issues, some experienced roommate effects persisting as many as eight years later. However, impacts on voter turnout only appeared in the 2006 election, before the typical graduation of the student cohort being studied. Several other papers studying peer effects on voting analyze other peer networks besides roommates. Using cross-sectional and panel variation, Barber and Imai (2014) assess that having larger proportions of a different race or party in one's nearby neighborhood decreases one's probability of voting. And in De Rooij and Foos (2017), randomized campaign contact mobilized voting more strongly for families consisting of members with heterogeneous versus homogeneous party identification.

2.3 Background, data, and summary statistics

I estimate effects on students of their randomly-assigned freshmen roommates using the sample from the College Roommate Study (ROOM). ROOM has already been used by many authors to study a host of topics (Guo et al., 2009; Wagner et al., 2013; Li and Guo, 2014; Guo et al., 2015a; Guo et al., 2015b; Li and Guo, 2016). In the survey portion of the study, students at a large public university in the South who entered in fall 2005 through fall 2007 were contacted to fill out an online survey in spring 2008. The survey also requested permission for the study investigators to access respondents' CIRP survey responses from when they were entering university. Table B.1 in the Appendix presents the full text of CIRP and follow-up survey questions that are summarized in my other tables.

Guo et al. (2009) provides an overview of the study and response rates. It also presents a student flow chart illustrating the university housing assignment process, with counts from earlier class years not in the ROOM sample. Freshmen who did not request a roommate or themed housing were randomly assigned a roommate within each gender-smoking-type of room cell for housing preferences. (I further subdivide cells by self-identification as male or female from the study survey.) Students entering in fall 2005 through fall 2007 were deemed eligible for inclusion in the ROOM study sample in spring 2008 if they lived on campus and were not studying abroad for the semester, were 18 or older, had email contact information listed with the university housing department, and had lived in a university dormitory their freshman year. Of the potentially eligible group, 2664 students participated in the survey, giving a response rate of 79.5% among contacted eligible students.

Limiting the analysis to students with just one freshman-year roommate who was assigned by the housing department and who responded to the survey leaves 993 roommate pairs for my sample. Among these students, 45% had CIRP survey responses available and granted permission for study investigators to access them.[2]

I begin by assessing that the CIRP survey responses of the sample students giving access permission to study investigators are representative of the CIRP survey responses of all students.

[2]The CIRP match rate appears to be driven more by low CIRP survey response rates for the university entering classes than by the share of study survey respondents who granted permission to access CIRP survey responses.

I only use students from one of the three sampled entering classes because CIRP variable aggregate tabulations for all students were only obtained from the university for this one entering class.

To assess representativeness, I test the individual statistical significance of the differences between sample students and the aggregate CIRP tabulations in the means of various student variables, including the regressors in the basic specification later on in section 2.4 (excluding ACT/SAT concordance score and the missing-variable dummies) but with alternatively coded race/ethnicity proportions.[3] Table 2.1.1 includes only variable differences since the levels might constitute sufficient information to identify the university being studied.

The aggregate CIRP tabulations feature only a gender cross-tab for the tabulated CIRP variables, not a cross-tab on race, ethnicity, or any other variable. In consequence, they do not provide sufficient covariance information to test the joint significance of the sample-versus-CIRP differences or to use the methods in Boisjoly et al. (2006) to correct my regression estimates in section 2.4 for response bias in the ROOM study relative to all students who responded to the CIRP survey.

However, the results of my individual tests for sample-CIRP differences are reassuring even on their own. In Table 2.1.1, all mean differences between sample students and aggregate CIRP tabulations are individually statistically insignificant. The point estimate is sizable and negative for the difference in proportions of students who are black since black and Hispanic students were oversampled in the ROOM study (Guo et al., 2009).

[3]Unlike in my regressions in section 2.4 where students checking multiple race/ethnicity boxes are coded as "Other" following Boisjoly et al. (2006), here students both from the sample and in the aggregate CIRP tabulation who check multiple race/ethnicity boxes are counted under all of these categories.

Table 2.1.1: *Representativeness check: differences for a single entering class in mean responses of sample students and aggregate CIRP tabulations*

	CIRP mean minus sample mean	Standard error	p-value[H_0: equal mean for this response between sample students and aggregate CIRP tabulations]
Racial discrimination a major problem in the US	-0.052	(0.224)	0.817
The wealthy should pay higher taxes	-0.061	(0.354)	0.863
Colleges should prohibit racist/sexist speech	0.033	(0.344)	0.924
Keep affirmative action in college admissions	0.003	(0.348)	0.992
Let undocumented immigrants access public education	0.005	(0.357)	0.988
Courts not overconcerned with rights of criminals	-0.046	(0.355)	0.898
Abolish the death penalty	-0.083	(0.356)	0.817
National health care plan needed	-0.018	(0.318)	0.956
Legally recognize same-sex marriage	-0.050	(0.339)	0.882
Father's education	0.054	(0.531)	0.919
Mother's education	0.098	(0.442)	0.824
White	0.024	(0.245)	0.921
Black	-0.053	(0.128)	0.677
Native American	-0.021	(0.026)	0.429
Asian/Asian American	-0.009	(0.117)	0.937
Hispanic	-0.003	(0.067)	0.960
Other minority	-0.012	(0.039)	0.752
Family income < \$50,000	-0.011	(0.084)	0.899
Family income \$50,000-\$74,999	-0.021	(0.108)	0.844

Table 2.1.1 (Continued)

Family income $150,000-$199,999	0.003	(0.108)	0.978
Family income >= $200,000	0.000	(0.123)	1.000

Next, I statistically corroborate the randomization of the roommate assignment process using the ROOM sample. In their own randomization checks using the ROOM sample, Guo et al. (2015a), Guo et al. (2015b), and Li and Guo (2016) do not reject null hypotheses that the Gamma or regression-model correlations of various CIRP and ROOM pre-college variables between roommates and within housing preference cells are equal to zero. The Gamma correlation coefficient is simply the proportion of roommate pairs with variable values that are equal to each other (Goodman and Kruskal, 1972).

Table 2.1.2 presents an additional randomization check and test the individual and joint statistical significance of within-housing preference cell differences in the non-race and ethnicity variables from Table 2.1.1 between white students assigned a black roommate as compared with a nonblack roommate. I do the same for within-cell differences between white students assigned a nonblack-minority roommate as compared to a white or black roommate.

Note that this randomization check does not compare student variables across any roommate variable unless the latter variable is constant among the former group of students, as with white students compared across roommate race and ethnicity status since race and ethnicity are constant across white students. The reason is because leave-oneself-out pairing mechanically generates negative correlation between paired units that can be especially evident within small cells (Stevenson, 2015). Again, my table includes only variable differences since levels could reveal the identity of the university.

In Table 2.1.2, within-cell differences are not individually statistically significant with a few exceptions. One of those exceptions is the within-cell difference between white students assigned black roommates and those assigned white roommates in the attitude that racial discrimination is no longer a major problem in America, but this attitude is included as a control in the regressions in section 2.4. Within-cell differences are also not jointly statistically significant within either of the two comparisons across roommate race and ethnicity.

Table 2.1.2: *Randomization check: differences by roommate race/ethnicity in mean responses of white students*

	Roommate - Black	Standard error	p-value[H_0: equal means between White students with versus without Black roommate]	Roommate - Other minority	Standard error	p-value[H_0: equal means between White students with versus without other minority roommate]
Racial discrimination a major problem in the US	-0.253*	(0.120)	0.036	0.134	(0.099)	0.175
The wealthy should pay higher taxes	-0.086	(0.135)	0.528	0.200	(0.129)	0.123
Colleges should prohibit racist/sexist speech	-0.035	(0.141)	0.802	-0.073	(0.117)	0.532
Keep affirmative action in college admissions	-0.026	(0.124)	0.836	0.185	(0.118)	0.119
Let undocumented immigrants access public education	-0.027	(0.130)	0.835	0.042	(0.132)	0.751
Courts not overconcerned with rights of criminals	-0.281*	(0.118)	0.018	-0.014	(0.110)	0.896
Abolish the death penalty	-0.048	(0.160)	0.764	0.025	(0.154)	0.869

Table 2.1.2 (Continued)

National health care plan needed	0.009	(0.115)	0.941	0.235*	(0.119)	0.049
Legally recognize same-sex marriage	-0.180	(0.165)	0.276	0.046	(0.147)	0.755
Father's education	-0.125	(0.116)	0.281	0.091	(0.098)	0.352
Mother's education	-0.019	(0.113)	0.868	0.031	(0.102)	0.761
Family income < \$50,000	0.024	(0.031)	0.432	0.008	(0.025)	0.760
Family income \$50,000-\$74,999	0.018	(0.036)	0.612	-0.013	(0.030)	0.650
Family income \$150,000-\$199,999	-0.061*	(0.029)	0.036	-0.042	(0.027)	0.119
Family income >= \$200,000	-0.012	(0.036)	0.752	0.082*	(0.036)	0.022
	p-value[H_0: equal vector of means between White students with versus without Black roommate]		0.420	p-value[H_0: equal vector of means between White students with versus without other minority roommate]		0.061

Finally, as presented in Table 2.2 below, the students in my sample are relatively diverse on select descriptive variables from among those summarized in previous publications using the ROOM sample, including race and ethnicity, parental income, and family educational background. The regression output tables, which are presented later in section 2.4, will offer some additional descriptive insight into my sample: they include the shares of students in the regression subsamples with the very low and very high values of the dependent variable.

Mother's and father's education are valued "1–7 for response categories of middle school or less, some high school, high school graduate, some college, college degree, postsecondary school other than college, and graduate or professional coursework or degree" (Guo et al., 2015a). In the regressions, those two variables will be valued differently to match the coding in the CIRP survey, except that graduate coursework and graduate degrees are grouped.

Table 2.2: *Descriptive statistics*

	. Mean	Standard Deviation
Asian/Asian American	0.070	(0.255)
Black	0.127	(0.333)
Hispanic	0.068	(0.253)
Other minority	0.111	(0.314)
White	0.685	(0.465)
Male	0.374	(0.484)
Math plus verbal SAT score	1331.298	(132.622)
Fall 2007 semester GPA	3.231	(0.549)
Family income < \$75,000	0.296	(0.457)
Family income \$75,000-\$149,999	0.380	(0.486)
Family income >= \$150,000	0.280	(0.449)
Mother's education	5.326	(1.386)
Father's education	5.502	(1.485)

2.4 Regression results

My analysis proceeds in three parts. All estimates are "intent to treat" since students may not have ended up living with their initially-assigned roommates.

2.4.1 Roommate effects on race-related attitudes and behaviors

In the first part of my analysis, I replicate Boisjoly et al. (2006) by running the regression specifications in their replication files on my sample, with some minor alterations.[4]

In addition, because the non-attitudinal regressors are likely to be exogenous, I replace the CIRP variables for these regressors with equivalently coded variables from the study survey. These include both student and roommate race/ethnicity, gender, parental education, and parental income in the twelve months prior to entering college. I make these replacements because the regression samples would suffer substantial attrition if inclusion were conditioned on non-missing non-attitudinal regressors and thus on non-missing roommate CIRP responses.

The basic regression specifications for the replication are described in Tables 2.3.1 and 2.4.1. For the replication portion of my analysis only, I also assess the robustness of my results from

[4]My TIME variable is zero to two years out from freshman year; my Table 2.4.1 dependent variables are slightly different; for students entering in fall 2006 and fall 2007, a CIRP response regarding colleges' right to ban extreme speakers from campus is used as a control variable in place of a response to the CIRP question regarding colleges' prohibiting racist/sexist speech that was dropped after 2005; and my entering class year, parental education, and SAT/ACT concordance score variables are coded slightly differently, as I describe next. The entering class year variable codes juniors and seniors in the spring of 2008 as having entered in fall 2005, sophomores as having entered in fall 2006, and freshmen as having entered in fall 2007. I can cross-check this coding against the known entrance years of students in my sample with matched CIRP survey responses, and the coding turns out to be correct for all of these students except one. The parental education variables are coded as in the CIRP survey except that graduate coursework and graduate degree are lumped together and valued as 7. The SAT/ACT concordance score variable is coded to approximately follow Boisjoly et al. (2006). It's not clear from their article how an SAT/ACT concordance score is calculated for students who took both the SAT and ACT. However, their concordance scores are whole numbers if they're not missing, which is unlikely to consistently arise as an average of an ACT score and the ACT equivalent of an SAT score. I therefore calculate the SAT/ACT concordance score as the ACT score if non-missing or else the ACT equivalent of the SAT score if non-missing. To obtain ACT equivalents, I use the concordance table in Dorans (1999) for SAT scores assigned to a date January 2005 or before and the concordance table in ACT Research & Policy (2009) for SAT scores assigned to a date after January 2005. Among high school graduates in 2006 and 2007 who ever took the SATs, ballpark 2/3 to 3/4 (both nationwide and in the state where the university is located) took the test for the last time their senior year, and most of the rest took it for the last time their junior year (College Board, 2019). Assuming more specifically that the bulk of students took the SATs for the last time either their junior spring semester or senior fall semester, the bulk of the classes entering university in 2006 and 2007 would have taken the new SATs and the bulk of the class entering in 2005 would have taken the old SATs. I therefore use those concordance tables for those respective entering classes.

these basic specifications to a series of modifications[5] presented in Tables B.3.1.1-B.4.1.8 in the Appendix. For the rest of my analysis, regression specifications will build upon the basic specifications described in Tables 2.3.1 and 2.4.1 by adding or substituting variables.

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[5]Modifications 1, 2, 5, and 6 are also performed in Boisjoly et al. (2006). The modifications are (1) adding linear interactions of years since freshman year (i.e., cohort) with roommate race/ethnicity and parental income; (2) adding roommate's pre-college CIRP attitude toward whether affirmative action should be abolished as a control variable; (3) estimating only on the subsample of white students who shared their pre-college CIRP responses, including attitude toward whether affirmative action should be abolished; (4) estimating only on the subsample of white students who shared their pre-college CIRP responses and who reported a "moderate" CIRP attitude (Agree or Disagree) toward whether affirmative action should be abolished, on the premise that outcomes for these students might be more influenceable by roommates and less often censored at either bound, Strongly Agree or Strongly Disagree; (5) estimating only on white women; (6) estimating only on white men; (7) estimating only on black students; and finally (8) estimating only on nonblack minority students.

Table 2.3.1: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [white students]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.098	-0.190	-0.129	-0.005	0.007	-0.000	-0.176	-0.167	-0.106
	(0.096)	(0.108)	(0.085)	(0.095)	(0.108)	(0.085)	(0.102)	(0.119)	(0.088)
Roommate - Other minority	0.089	0.029	0.022	-0.064	-0.112	-0.081	-0.027	-0.047	-0.042
	(0.087)	(0.089)	(0.071)	(0.088)	(0.092)	(0.072)	(0.093)	(0.097)	(0.070)
Roommate - Family income $<$ \$50,000		-0.027	-0.017		-0.039	-0.020		-0.023	-0.019
		(0.108)	(0.086)		(0.106)	(0.083)		(0.112)	(0.079)
Roommate - Family income \$50,000-\$74,999		-0.043	-0.031		-0.012	-0.011		0.112	0.058
		(0.099)	(0.079)		(0.105)	(0.082)		(0.109)	(0.075)

Table 2.3.1 (Continued)

Roommate - Family income \$150,000-\$199,999		-0.101	-0.078		-0.126	-0.092		-0.048	-0.036
		(0.113)	(0.089)		(0.106)	(0.083)		(0.118)	(0.085)
Roommate - Family income >= \$200,000		-0.067	-0.055		-0.077	-0.054		-0.075	-0.049
		(0.101)	(0.079)		(0.100)	(0.077)		(0.102)	(0.074)
Years since freshman year		0.151	0.136		-0.274	-0.199		0.055	0.039
		(0.332)	(0.272)		(0.566)	(0.444)		(0.559)	(0.399)
Share with dependent variable equaling 1	0.254	0.254	0.254	0.185	0.185	0.185	0.033	0.033	0.033
Share with dependent variable equaling 4	0.079	0.079	0.079	0.095	0.095	0.095	0.462	0.462	0.462
R-squared/pseudo-R-squared	0.061	0.108	0.236	0.056	0.103	0.224	0.056	0.087	0.173
Number of observations	1337	1337	1337	1337	1337	1337	1346	1346	1346

Table 2.3.1 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table 2.4.1: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [white students]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	-0.200	-0.129	-0.054	0.008	2.635*	0.042*
	(0.112)	(0.128)	(0.067)	(0.017)	(1.242)	(0.020)
Roommate - Other minority	0.094	0.100	0.057	-0.003	0.192	-0.019
	(0.101)	(0.102)	(0.048)	(0.015)	(1.022)	(0.018)
Roommate - Family income < $50,000		-0.131	-0.052	-0.005	-1.067	-0.003
		(0.127)	(0.063)	(0.018)	(1.259)	(0.022)
Roommate - Family income $50,000-$74,999		0.037	0.023	0.007	-0.354	-0.012
		(0.119)	(0.058)	(0.018)	(1.097)	(0.019)
Roommate - Family income $150,000-$199,999		-0.242*	-0.112	0.018	-1.437	0.032
		(0.122)	(0.065)	(0.017)	(1.171)	(0.021)
Roommate - Family income >= $200,000		0.049	0.020	-0.015	0.116	0.013
		(0.116)	(0.057)	(0.017)	(1.046)	(0.018)
Years since freshman year		-0.024	-0.003	-0.078	-0.404	0.069

Table 2.4.1 (Continued)

		(0.495)	(0.248)	(0.094)	(4.849)	(0.100)
Share with dependent variable equaling 1	0.017	0.017	0.017			
Share with dependent variable equaling 4	0.633	0.633	0.633			
R-squared/pseudo-R-squared	0.085	0.104	0.136	0.174	0.168	0.233
Number of observations	1350	1350	1350	1351	1338	1341

Table 2.4.1 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

In Boisjoly et al. (2006), white students assigned black roommates relative to those assigned white roommates reported significantly more support for affirmative action and racial diversity on campus, and they had more comfortable interactions with people from other racial and ethnic groups.

In contrast, none of the results in Table 2.3.1 is robustly significantly affected by being assigned a black or other minority roommate or by roommate parental income. More specifically, the estimated effects on white students of being assigned a black or other minority roommate and of roommate parental income are not consistently statistically significant across all three (two ordered probit and one OLS) estimated models for any outcome for white students in Table 2.3.1 or Tables B.3.1.1-B.3.1.6 in the Appendix. The only exception is the significant linear trend across cohorts in the effect of a black roommate on "Campus diversity improves higher education" in Table B.3.1.1. Roommate race/ethnicity is also not consistently statistically significant for any outcome for nonblack-minority students in Table B.3.1.8. In Table B.3.1.7, a single roommate parental income bucket ($150,000-$199,999) significantly affects black students across all three estimated models for a single outcome ("Ensuring campus diversity justifies affirmative action").

In Table 2.4.1, white students assigned a black roommate as compared with those assigned a white roommate report socializing more frequently with black individuals and also report having a higher share of friends who are in their own social class. However, these effects are not robust across the modifications presented in Tables B.4.1.1-B.4.1.6. In Table B.4.1.8, nonblack-minority students assigned a black roommate compared with those assigned a white roommate reported socializing more frequently with black individuals. This also holds in some of the specifications estimated on white students in Tables B.4.1.1-B.4.1.6.

Overall, none of the race-related attitudes and behaviors of white students in Tables 2.3.1 and 2.4.1 above are consistently affected by being assigned a black roommate.[6]

[6]One potential concern is that these differing results from Boisjoly et al. (2006) may be attributable to differing sample compositions amidst roommate effect heterogeneity as opposed to differing average roommate effects. While differences in sample composition could be explored through a comparison of the distributions of pre-freshman CIRP responses between my sample and the Boisjoly et al. (2006) sample, this information might be sufficient to identify the university being studied. At a minimum, my differing results do not appear to be mainly attributable to mechanical attenuation of the estimated effects due to outcome censoring at Strongly Agree or Strongly Disagree. While a substantial share of student responses lies at the upper bound of "Strongly Agree" for the attitude outcomes

2.4.2 Roommate effects on attitude toward immigration

Analogously to how interpersonal contact with black roommates might affect students' attitudes toward affirmative action, interpersonal contact with first-generation immigrant roommates could affect students' attitudes toward immigration. Compared with the basic specification used in Tables 2.3.1 and 2.4.1, I add additional regressors from the study survey: refined race and ethnicity dummy variables (black, Hispanic, Asian/Asian American, and other minority), a first-generation immigrant status dummy variable for whether both parents were born in the U.S., and interactions between each of those refined race/ethnicity dummy variables and the first-generation immigrant status dummy variable.

In Table 2.4.2, roommate refined race and ethnicity, roommate first-generation immigrant status, and their interactions all do not significantly affect attitudes toward immigration. Given the low shares of student responses lying at the outcome bounds Strongly Agree and Strongly Disagree, these results do not appear to be mainly attributable to mechanical attenuation of the estimates due to outcome censoring.

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in the third question in Table 2.3.1 and the first question in Table 2.4.1, relatively small shares of responses lie at the bounds of the attitude outcomes in the first and second questions in Table 2.3.1.

Table 2.4.2: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [white students]*

	Immigration is a good thing for the US		
	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	0.207	0.229	0.118
	(0.574)	(0.545)	(0.349)
Roommate - Asian/Asian American	0.115	-0.029	0.019
	(0.487)	(0.472)	(0.313)
Roommate - Hispanic	0.132	0.343	0.204
	(0.527)	(0.515)	(0.350)
Roommate - Other minority	-0.187	-0.264	-0.165
	(0.566)	(0.529)	(0.362)
Roommate - Both parents were born in U.S.	0.378	0.396	0.244
	(0.454)	(0.455)	(0.304)
Roommate - Black × Roommate - Both parents were born in U.S.	-0.321	-0.288	-0.147
	(0.616)	(0.578)	(0.381)
Roommate - Asian/Asian American × Roommate - Both parents were born in U.S.	0.332	-0.171	-0.089
	(0.521)	(0.624)	(0.426)
Roommate - Hispanic × Roommate - Both parents were born in U.S.	0.155	0.161	0.092
	(0.691)	(0.794)	(0.533)
Roommate - Other minority × Roommate - Both parents were born in U.S.	-0.576	-0.778	-0.415

Table 2.4.2 (Continued)

	(0.668)	(0.665)	(0.457)
Roommate - Family income < $50,000		0.133	0.062
		(0.262)	(0.179)
Roommate - Family income $50,000-$74,999		-0.038	-0.027
		(0.211)	(0.139)
Roommate - Family income $150,000-$199,999		0.030	0.015
		(0.266)	(0.187)
Roommate - Family income >= $200,000		0.213	0.132
		(0.248)	(0.171)
Share with dependent variable equaling 1	0.046	0.046	0.046
Share with dependent variable equaling 4	0.229	0.229	0.229
R-squared/pseudo-R-squared	0.126	0.205	0.366
Number of observations	327	327	327

2.4.3 Roommate effects on broader sociopolitical attitudes

In the final part of my analysis, I estimate roommate effects on various socio-political issues like crime, taxes, health care, inequality, immigration, religion, and same-sex marriage. The same triplet of models from Table 2.3.1 are estimated for each attitude outcome, except that a total of nine roommate pre-freshman CIRP attitudes are included in each model in the triplet. The regression output is presented in Tables B.2.1-B.2.6 in the Appendix. Overall, students' attitudes do not appear to be significantly affected by their roommates' attitudes in any systematic way.

Although some scattered roommate attitudes exhibit statistically significant estimated effects, they do not form a coherent pattern consistent with either issue-by-issue or overarching ideological peer effects. For instance, of the seven attitudes that are included both as study outcomes and pre-freshman CIRP regressors, only one significantly affects a roommate's attitude in all three estimated models for that outcome: viewing a national health care plan as needed. It does so with a negative sign, representing attitudinal divergence. Two other attitudes significantly affect roommates' same or related attitudes in only two out of three estimated models. Attitude toward courts' concern for rights of criminals negatively affects roommates' same attitude in two out of three estimated models, while attitude toward whether to keep affirmative action positively affects roommates' closely related attitude toward whether campus diversity justifies affirmative action.

Technological limitations make it difficult to conduct comprehensive cross-model hypothesis testing including fixed-effects ordered probit models. Testing within estimated models, only one out of twelve sociopolitical attitude outcomes are jointly significantly affected by the set of nine roommate pre-freshman CIRP sociopolitical attitude regressors in either the second or third estimated model for that outcome. This outcome, attitude toward whether economic differences in the US are justified, is significantly affected by roommates' attitude toward whether racial discrimination is a major problem in the US. But when taken together, these significant coefficients on roommate attitudes resist a coherent interpretation as either issue-by-issue or overarching ideological peer effects.

2.5 Discussion of results

College students have been observed by others to exhibit relatively flexible attitudes, including those concerning race (Visser and Krosnick, 1998; Danigelis et al., 2007). Yet my replication of the main analyses and robustness checks in Boisjoly et al. (2006) yields differing results.

As presented in section 2.4, across specifications controlling for student and roommate characteristics including pre-freshman CIRP political attitudes, white students who were assigned black or other minority roommates as compared with those assigned white roommates did not report significantly more support for affirmative action, support for racial diversity on campus, or comfortable interactions with people from other racial/ethnic groups. Compared with Boisjoly et al. (2006) where only around 2% of sample students were African American and 11% had parental income under \$50,000, around 16% of students in my sample are African American and 16% have parental income under \$50,000. My differing results cannot be attributable to limited power. My Table 2.4.1 dependent variables are not directly comparable to those in Boisjoly et al. (2006), but separate univariate hypothesis tests for each of the nine estimated models in Table 2.3.1 all reject an effect of being assigned a black roommate that is as large in magnitude as that same effect from the counterpart estimated model in Boisjoly et al. (2006).

Unlike in Boisjoly et al. (2006), white students who were assigned black roommates reported significantly lower roommate closeness than those who were assigned white roommates. In Table 2.5 below, the estimated effect on white students of being assigned a black roommate versus being assigned a white roommate is significantly negative across all three estimated models for three out of four closeness-related outcomes. These three outcomes concern whether a student refers to their roommate as comfortable to talk to, the type of person they would be friends even absent being roommates, and one of their best friends. The estimated effect is insignificant across all three estimated models for the fourth outcome, often having conflicts or arguments. But responses for this fourth outcome are concentrated at Strongly Disagree.

These findings do not hold consistently across the modifications to the basic specification in Tables B.5.1-B.5.6 in the Appendix, although this could be due to lower power in several subsamples. They do hold for white women but not white men. And white women but not white men who were assigned a nonblack-minority roommate compared with those who were assigned a white roommate also reported significantly lower roommate closeness on two of four outcomes. However, these differing findings by gender may be attributable to lower power for white men given their lower representation in my sample than white women. My subgroup analysis below finds a significant gender difference (controlling for all other roommate race and ethnicity-covariate interactions) in the effect of being assigned a black roommate versus a white roommate across all three estimated models for zero of the four roommate-closeness outcomes. For one outcome, white students' considering their roommate to be one of their best friends, there is a significant gender difference in the effect of being assigned a black roommate versus a white roommate in two out of three estimated models for that outcome.

There is another roommate characteristic in Table 2.5 that significantly predicts roommate closeness. White roommates assigned roommates with parental income in the highest bracket compared to the omitted middle bracket reported higher roommate closeness across estimated models for two of the four outcomes. However, this is not very robust across modifications.

Looking instead at black students in Table B.5.7 and nonblack-minority students in Table B.5.8 in the Appendix, my subsamples are much smaller than my subsample of white students. Nonetheless, black students assigned a black roommate compared to those assigned a white roommate reported significantly higher roommate closeness across two of three estimated models on one outcome, considering their roommate to be the type of person they would be friends with absent being roommates. Nonblack-minority students assigned a nonblack-minority roommate compared to those assigned a white roommate reported significantly higher roommate closeness across all three estimated models on a different outcome, considering their roommate to be one of their best friends.

Table 2.5: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [white students]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.408***	-0.336**	-0.257*	-0.446***	-0.396***	-0.340**
	(0.107)	(0.119)	(0.103)	(0.103)	(0.115)	(0.105)
Roommate - Other minority	-0.173	-0.179	-0.152	-0.213*	-0.186	-0.159
	(0.098)	(0.103)	(0.087)	(0.095)	(0.100)	(0.093)
Roommate - Family income < $50,000		0.050	0.035		-0.053	-0.044
		(0.118)	(0.100)		(0.119)	(0.108)
Roommate - Family income $50,000-$74,999		-0.017	-0.010		-0.043	-0.040
		(0.100)	(0.086)		(0.099)	(0.092)
Roommate - Family income $150,000-$199,999		0.003	0.004		0.051	0.041
		(0.121)	(0.103)		(0.124)	(0.115)
Roommate - Family income >= $200,000		0.213*	0.166*		0.210*	0.183*
		(0.100)	(0.081)		(0.097)	(0.092)
Years since freshman year		0.255	0.186		-0.368	-0.307
		(0.369)	(0.299)		(0.471)	(0.392)

Table 2.5 (Continued)

Share with dependent variable equaling 1	0.098	0.098	0.098	0.254	0.254	0.254
Share with dependent variable equaling 4	0.374	0.374	0.374	0.185	0.185	0.185
R-squared/pseudo-R-squared	0.074	0.086	0.187	0.074	0.088	0.202
Number of observations	1351	1351	1351	1349	1349	1349

	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.145	-0.092	-0.088	-0.555***	-0.440***	-0.375**
	(0.121)	(0.136)	(0.098)	(0.116)	(0.126)	(0.115)
Roommate - Other minority	-0.015	0.031	0.028	-0.171	-0.139	-0.136
	(0.097)	(0.105)	(0.072)	(0.100)	(0.104)	(0.102)
Roommate - Family income < \$50,000		0.045	0.032		-0.018	-0.018
		(0.135)	(0.096)		(0.124)	(0.117)
Roommate - Family income \$50,000-\$74,999		0.086	0.076		-0.080	-0.074
		(0.110)	(0.078)		(0.107)	(0.101)

Table 2.5 (Continued)

Roommate - Family income \$150,000-\$199,999		0.167	0.093		0.103	0.087
		(0.121)	(0.083)		(0.124)	(0.122)
Roommate - Family income >= \$200,000		0.192	0.108		0.148	0.140
		(0.115)	(0.077)		(0.099)	(0.100)
Years since freshman year		0.272	0.305		0.046	-0.026
		(0.419)	(0.289)		(0.418)	(0.371)
Share with dependent variable equaling 1	0.046	0.046	0.046	0.436	0.436	0.436
Share with dependent variable equaling 4	0.603	0.603	0.603	0.193	0.193	0.193
R-squared/pseudo-R-squared	0.094	0.106	0.192	0.093	0.107	0.225
Number of observations	1347	1347	1347	1349	1349	1349

Given that the Table 2.5 modifications hinted at gender heterogeneity in white students' differentially lower reported roommate closeness when assigned a black roommate, it seems warranted to undertake a fuller exploration of whether differences in reported roommate closeness by assigned roommate race/ethnicity are more or less pronounced within certain covariate subgroups of white students.

For my subgroup analysis, I opt for assessing interactions between roommate race/ethnicity and covariates holding constant the others within one regression rather than each independently in their own separate regression. I modify the basic regression specifications for white students from Table 2.5 by adding some additional regressors: a gender dummy variable as well as separate interactions of the black roommate dummy variable and of the nonblack-minority roommate variable with each student-specific (as opposed to roommate-specific) non-race/ethnicity regressor. Excluding the interactions with the missing-value dummies, the interactions with being assigned a black roommate as compared to a white roommate are not jointly significant in any of the three estimated models for any of the four closeness-related outcomes. The same holds for the interactions with being assigned a nonblack-minority roommate as compared to a white roommate. Again excluding the interactions with missing-value dummies, only two out of the 112 interactions of either the black-roommate dummy variable or nonblack minority-roommate dummy variable with a covariate across the four closeness-related outcomes are individually significant for all three estimated models for a given outcome. Overall, the data do not clearly indicate dimensions of heterogeneity in white students' roommate racial/ethnic differences in reported roommate closeness.

2.6 Conclusions

This paper has analyzed how the societal and political attitudes of students are affected by randomly-assigned freshman dormitory roommates at a large public university in the U.S. south. Students entering university in fall 2005 through 2007 filled out the pre-freshman CIRP survey that fall and then a follow-up survey in spring 2008, so the effects were estimated zero to two years later.

I find that being assigned a black or nonblack-minority roommate does not significantly affect white university students' attitudes toward affirmative action or campus diversity, nor does it significantly affect their comfortable interactions with people from other racial/ethnic groups. White students' attitudes toward immigration are also not significantly affected by the race/ethnicity and first-generation immigrant status of their freshman roommate. Finally, students' broader sociopolitical attitudes do not appear to be significantly affected by their roommates' broader sociopolitical attitudes in a systematic way.

White students' lower closeness to black roommates is one possible explanation for why the results of my replication differ from those in Boisjoly et al. (2006). Lower closeness could hinder deliberation as a channel for one kind of peer effect, or it could spark discord as a channel for a potentially altogether different kind of peer effect. Note that multiple other studies have similarly reported that white students expressed relatively lower self-reported involvement and satisfaction with roommates who were nonwhite (Phelps et al., 1998; Towles-Schwen and Fazio, 2006; Shook and Fazio, 2008).

Another possible explanation for my differing results would be implicit (subconscious) racial bias among some white students. The extent of explicit (conscious and thus reportable) racial bias among white students may not substantially differ from in Boisjoly et al. (2006): For example, as a plausible correlate, the average pre-freshman CIRP attitude on the extent of racial discrimination in the U.S. is comparable between my sample and theirs. Implicit bias I have no direct measure of in the study data. However, it has been linked to higher stability in explicit attitudes, and implicit racial bias among white students specifically has been found to predict negative interracial interactions (McConnell and Leibold, 2001; Charlesworth and Banaji, 2019).

Chapter 3

Incarceration and Recidivism: Evidence from Car-Crash Offenses

3.1 Introduction

How long a sentence of incarceration should the criminal justice system impose for committing a crime? For many considerations like expressive condemnation, retribution, and loss of liberty and relationships, the debate has largely progressed past the factual to the moral. But incarceration's effectiveness in reducing crime is still subject to substantial empirical disagreement. Using what to my knowledge is a novel instrument for incarceration length,[1] I aim to identify the specific deterrence-plus-aging component of the effect of incarceration length on crime.

Using criminal records from Florida and Georgia, I compare the recidivism rates of DUI or reckless-driving offenders whose car-crash victims suffered death (vehicular manslaughter/homicide) by vehicle to those whose victims suffered serious bodily injury. Compared to victim survival with a serious injury, death of the victim leads to a longer sentence for the perpetrator. Because victim death may influence the perpetrator's psychological disposition to

[1] The instrument here is reminiscent of the one used by Jones and Olken (2009), which assesses how national government leadership can affect institutions and war by using whether a national leader dies or not given an assassination attempt as an instrument for leadership change orthogonal to pre-attempt country characteristics.

commit future vehicular offenses in particular, in the main results I estimate effects of longer incarceration only on rates of non-vehicle related recidivism, defined as a return to prison involving no new vehicular offenses. Then in Appendix section C.2, I estimate effects on rates of any recidivism, whether involving new vehicular offenses or not. Unsurprisingly since vehicle-related recidivism rates are very low, my findings from the main results are largely robust to lumping in vehicle-related recidivism.

The motivation for my identification strategy is that whether victims experiencing severe physical impact putting them on the knife-edge of death do or don't die is largely independent of the offender and yet determines the severity of the offender's sentence. Offenders convicted of the two studied offenses may be differentially selected by the justice system, including in arresting, charging, conviction, or sentencing. Indeed, consistent with more leniency toward sample offenders causing victim serious injury than those causing victim death, I find that offenders convicted of serious bodily injury by vehicle are somewhat older and have a higher rate of prior incarceration(s). To address the concern that this imbalance on observables could indicate an imbalance on unobservables, I will show that the qualitative conclusions for my basic three-year recidivism results in Florida and Georgia are robust to Oster (2017) bias correction on a modified IV specification under her suggested parametric assumptions for the unobservables.

Aside from any such differential selection in the justice-system process, the survival of the perpetrator's victim(s) and therefore which of the two offenses the perpetrator is convicted of should be roughly orthogonal to his or her pre-offense characteristics. This exclusion restriction permits identification along the margin of the difference in actual incarceration length between the two offenses studied. For both states I study, only the current incarceration lengths differ between the two offenses, with resulting future sentence enhancements the same between the offenses.[2]

The various channels through which incarceration length affects crime are well explored in the literature.[3] General deterrence captures how the threat of punishment increases the costs of

[2]§§ 775.084(1), 775.082(9)(a), Fla. Stat.; O.C.G.A. § 17-10-7

[3]See Nagin (2013) for a summary.

crime facing would-be offenders, tilting their decision making. Specific deterrence captures how the actual experience of incarceration affects future recidivism. Offenders might update their beliefs about or find more salient the deprivations of incarceration and other sanctions through the legal system. They may also gain access to rehabilitation programs. But the experience of incarceration could also increase recidivism by increasing resentment or other psychological hardening, enabling networking with and learning from other criminals, or damaging the offender's relationships and employment prospects. As discussed in Sampson and Laub (2005), incarceration not only provides the experience of incarceration (specific deterrence) but also inseparably ages, leaving the offender older and therefore potentially more or less prone to crime upon release.[4] Finally, for the time that an offender is incarcerated, incapacitation of course also prevents the offender from recidivating on the outside.

It is important, then, to recognize which effects my identification strategy does and does not capture. It estimates how actual incarceration length affects non-vehicle related recidivism through the joint effect of specific deterrence plus aging. General deterrence is excluded by design since there are no differential sentence enhancements in my states, except for a key caveat: there may be differential parole or probation lengths within the post-release recidivism window between the two relevant crash offenses, and offenders may know that parole or probation violations reactivate any differential suspended jail time. Then my estimates would partially lump in differential general deterrence or recidivism risk over time due to differential duration at risk of parole/probation violation. I discuss this issue further in section 3.4.

I consider several alternative specifications. In particular, I vary the recidivism window as well as look only at original offenses from earlier years to minimize differential attrition between offenses due to a lack of a fully observed recidivism window. Another variation includes the incapacitation effect: recidivism here is defined as return to prison within a certain number of years after the start of incarceration, as opposed to after release. Finally, to address concerns about differential selection, I also control for any fixed differences in recidivism rates

[4]While specific deterrence and aging are inseparable for a given offender, they must be distinguished because they admit a highly age-dependent and nonlinear effect of incarceration. For example, in a world where aging primarily drives a negative incarceration effect on recidivism, incarceration beyond the length necessary to age the offender to a certain age (such as long incarcerations for older offenders) might not have substantial effects on recidivism.

(invariant to incarceration length) between the treatment and control groups using state fixed effects after pooling the two states, which differ in their gap in average incarceration length between treatment and control groups.

My results are generally consistent with percent (of sample-mean recidivism rate) effects of incarceration length on recidivism rate via specific deterrence plus aging in line with the literature but level effects that lie between the small end of the comparable previous estimates (in Georgia) and zero (in Florida).

The paper proceeds as follows. Section 3.2 highlights relevant literature, section 3.3 summarizes the empirical strategy, section 3.4 discusses potential threats to identification, section 3.5 describes the regression analysis, section 3.6 examines the results for non-vehicle related recidivism, and section 3.7 outlines the conclusions. The Appendix presents the variable and sample construction as well as the results for any recidivism, whether vehicle related or not.

3.2 Previous literature

Studies focusing on how incarceration affects crime often distinguish between the channels of general deterrence[5] and specific deterrence plus aging. Some also estimate incapacitation effects.[6] The following studies on specific deterrence plus aging have mostly estimated a negative effect on recidivism, with some exceptions.

Studies using sentencing guidelines and early release. Berecochea and Jaman (1981) analyze prisoners randomly released six months early in California and find that their two-year recidivism rate is 26.8% compared to 22.6%. And Maurin and Ouss (2009) find that offenders whose sentences were reduced under a collective pardon exhibited an elasticity of recidivism rate with respect to sentence-reduction rate of about 0.77 to 0.98.

Using a Washington-state guidelines discontinuity, Hjalmarsson (2009) estimates that juveniles have a 35% lower daily hazard rate of recidivism (or 11% lower one-year recidivism

[5]See, e.g., Levitt (1998), Kessler and Levitt (1999), Raphael (2006), Webster et al. (2006), Helland and Tabarrok (2007), Iyengar (2008), Lee and McCrary (2009), Vollaard (2013).

[6]See, e.g., Owens (2009), Drago et al. (2009), Buonanno and Raphael (2013), Hansen (2015), de Figueiredo (2015).

rate) if incarcerated for at least 15 weeks versus not at all.

Several studies have specifically analyzed guidelines and offenders in Georgia. Ganong (2012) exploits a change in sentencing guidelines and estimates that an additional year of time served decreases the three-year recidivism rate by 5.9 percentage points (14%). Kuziemko (2013) exploits discontinuities in Georgia's parole guidelines and assesses that an extra month of incarceration reduces the three-year recidivism rate by 1.3 percentage points. Yet a reform limiting parole to at most the last 10% of an original sentence decreased participation in rehabilitation programs and increased the three-year recidivism rate by 29.7%. The author contrasts the specific deterrence from discontinuity-induced increases in incarceration with the crime increases from parole elimination-induced increases, which she characterizes as reduced rehabilitation following the removal of a key incentive (improved odds of receiving parole) to participate in relevant programming. Also in Georgia, Zapryanova (2017) instruments for incarceration length with random judge assignment and parole length with guideline discontinuities. Parole time has no significant estimated effect, but an extra month of incarceration reduces the three-year recidivism rate by 1.12 percentage points (lower than Kuziemko (2013), who omitted parole time negatively correlated with the guidelines instrument).

Studies using quasi-random assignment to judges. Green and Winik (2010) also use random judge assignment and find that jail and probation time do not statistically significantly affect recidivism rates of DC drug offenders. Nagin and Snodgrass (2013) use quasi-random assignment of offenders to judges in Pennsylvania in an attempt to estimate specific deterrence, but their test's power is too low. Finally, Aizer and Doyle (2015) use quasi-random assignment to initial judges to estimate that juvenile incarceration (as opposed to none) increases the probability of adult incarceration by age 25 by 22-26 percentage points.

Specific deterrence-plus-aging benchmark magnitudes from the literature

Below I summarize previous specific-deterrence-plus-aging estimates with comparable functional forms to mine. I will study the level and percent (of sample-mean rate) effect on the recidivism rate from an additional month actually incarcerated, using these estimates from Berecochea and Jaman (1981), Ganong (2012), Kuziemko (2013), and Zapryanova (2017) as benchmarks for my results in section 3.6.[7] They range from -0.005 to -0.013 for the level effect and from -1.3% to -3.86% for the percent effect. Note that recidivism rates are substantially larger than mine in each paper.

Table 3.1: *Specific deterrence-plus-aging benchmark magnitudes from the literature*

Paper	Relation of recidivism rate (R) to months actually incarcerated (P)	Recidivism rate (R)	Period after release during which recidivism is assessed
Berecochea and Jaman (1981)	dR/dP=(0.225-0.268)/6.6= -0.00652, or -2.65% of sample-mean R (using the quasi-random early release of California prisoners)	0.268 and 0.225 (treatment and control), 0.246 (sample mean)	Two years

[7]Maurin and Ouss (2009) estimates a similar relationship but in a functional form that is not directly comparable. Green and Winik (2010) find no statistically significant effect, but their recidivism window is measured in years from case disposition rather than release from incarceration.

Table 3.1 (Continued)

Ganong (2012)	dR/dP=-0.005 (scaled down linearly to a one-month denominator), or -1.3% of sample-mean R (comparing the before versus after pool of criminals in a given crime severity-by-criminal history cell, before versus after a change in Georgia sentencing guidelines)	0.34 and 0.42 (treatment and control), 0.377 (calculated sample mean)	Three years
Kuziemko (2013)	dR/dP=-0.013, or -3.78% of sample-mean R (using discontinuity in Georgia parole guidelines)	0.344 (sample mean)	Three years
Zapryanova (2017)	dR/dP=-0.0112, or -3.86% of sample-mean R (using discontinuity in parole guidelines and quasi-random assignment to judges in Georgia)	0.29 (sample mean)	Three years

3.3 Empirical strategy

My analysis uses data from two states: Florida and Georgia.[8] Each state has a relevant pair of vehicular offenses in the criminal code. One in the pair involves DUI or reckless driving plus victim death, and the other involves the same plus victim serious bodily injury (but see a qualification for Georgia in the paragraph after next).

Ideally for my identification strategy, serious bodily injury would be defined such that a knife-edge divides the classes of physical impacts resulting in death versus serious bodily injury. Then which one materializes would be largely uncorrelated with offender pre-offense characteristics. In the two states, the definitions of serious bodily injury arguably hew fairly closely to this ideal.

In Florida, DUI manslaughter or vehicular homicide with reckless driving versus serious bodily injury by vehicle while DUI or felony reckless driving differ only on the element of victim death versus serious bodily injury. The sentence ranges are four years to life versus zero to ten years, respectively. I begin my sample in 1999 since license-suspension lengths for both offenses were set at a minimum of three years (or more at the court's discretion) in 1998.[9]

In Georgia, felony homicide by vehicle and serious injury by vehicle while DUI or reckless driving differ only on the element of victim death versus serious bodily injury, with a qualification: besides DUI or reckless driving, felony homicide by vehicle can also be predicated on driving near a school bus, hit and run, fleeing a police officer, or driving as a habitual violator while in license revocation.[10] The sentence ranges are three to 20 years versus one to 15 years, respectively. I begin my sample in 2000 because there are too few serious-injury offenses in prior years. In addition, a significant increase in sentence length for both offenses occurred in

[8]The Georgia data includes only the ten most serious offenses per prison episode.

[9]Serious bodily injury is defined here as "injury... which consists of a physical condition that creates a substantial risk of death, serious personal disfigurement, or protracted loss or impairment of the function of any bodily member or organ" (§ 316.1933, Fla. Stat.). The offenses are detailed in §§ 316.193(3)(c)(3), 782.071, Fla. Stat. & §§ 316.193(3)(c)(2), 316.192(3)(c)(2), Fla. Stat.; sentence ranges in §§ 775.082, 775.084, Fla. Stat.; and license suspension changes in Ch. 98-223, § 10, Laws of Fla. (1998) & § 322.28, Fla. Stat.

[10]Since these non-death/injury offenses are "lesser included offenses," offenders are not convicted of them separately. The two samples could therefore be balanced more precisely in terms of included other traffic offense by obtaining and searching the felony-homicide by vehicle indictments offender by offender and including only those offenders with DUI or reckless-driving charges.

1999.[11]

My sample tracks offender recidivism after the first incarceration episode following an offender's first conviction that includes either but not both of serious injury or vehicular manslaughter/homicide by vehicle. In Georgia, first-time offenders who had completed their sentences pursuant to the First Offender Act and any offenders who were deceased as of the data snapshot in March 2017 are not included in my data.[12] See section 3.5 and Appendix section C.1 for further details on variable and sample construction.

3.4 Potential threats to the empirical strategy

My empirical strategy could be scrutinized for several potential violations of the exclusion restriction that victim death should only relate to non-vehicle related recidivism through incarceration length, but I argue below that these are unlikely to create first-order biases.

First, victim death might relate to recidivism through future employment as a substitute for a life of crime rather than just through incarceration length directly. Employers might assess candidates with a past vehicular manslaughter/homicide by vehicle conviction more harshly than a past serious injury by vehicle conviction. That said, the margin of victim death is relative to the baseline offense of seriously injuring a victim in a crash, which is already quite grave. And to the extent that the longer incarceration length alone increases the social disapprobation and thus stigma of an offense in employment background checks, this effect might to some degree properly includable in the estimate. Nonetheless, I help minimize this concern by pooling states with state fixed effects to eliminate any fixed difference (invariant to incarceration length) in recidivism between the two offender groups driven by, for example,

[11] Serious bodily injury is defined here as "bodily harm to another by depriving him of a member of his body, by rendering a member of his body useless, by seriously disfiguring his body or a member thereof, or by causing organic brain damage which renders the body or any member thereof useless" (O.C.G.A. § 40-6-394). The offenses are detailed in O.C.G.A. § 40-6-393 except (c) & O.C.G.A. § 40-6-394; sentence ranges in O.C.G.A. §§ 40-6-393, 40-6-394; and sentence length changes in 1999 Ga. Laws 391.

[12] First-time offender status should be orthogonal to victim death. As previously discussed, differential leniency by the justice system may drive the observed imbalance in shares aged 41+ as of the offense date. But these shares are low and not that far apart for the treatment and control groups (18.6% and 26.1%, respectively, from Table 3.3), and the earliest possible offense date was only 18 years before 2017. So offenders should not be substantially differentially excluded as first-time offenders or as deceased.

differential employability.

Second, the offender groups might differ in recidivism due to differential psychological responses to victim death versus serious injury by the perpetrator or by family, friends, and others. Any differential psychological response driven solely by resulting differences in incarceration length is an includable intermediate outcome. And to the extent that the effect on the recidivism rate of any other psychological response varies by neither state nor incarceration length, pooling states with fixed effects will eliminate this confound as a fixed difference in recidivism rates. A differential psychological response might also be expected to differentially deter vehicle-related recidivism in particular. I define recidivism as a return to prison not involving new vehicular offenses in the main results, and then in Appendix section C.2 I re-run the results defining recidivism as any return to prison.

Third, the offender groups might differ in recidivism due to differing pre-offense characteristics. One might suggest that offenders who, for example, drive more on highways where driving speeds are faster are differentially more likely to cause victim death in a crash and that driving more on highways is correlated with pre-offense characteristics affecting recidivism. Again, however, the motivation of the empirical strategy is that the classes of crashes with physical impact inducing death versus serious bodily injury should be very similar, with the outcome as good as random. A subtler channel is that victim death could relate to pre-offense offender characteristics not through the nature of the crash but rather through the nature of the victim. If offenders with pre-offense characteristics correlated with recidivism differentially crash into the elderly, obese, or others who are likelier to die as a result because they live or travel near such types of potential victims, selection on those characteristics could occur.

Fourth, incarceration length might correlate with the extent of plea bargaining, parole, probation, license suspension, and other ancillary sanctions. In considering only incarceration length, their omission generally makes the estimate more negative to the extent that those ancillary sanctions reduce recidivism. For parole or probation specifically, if their length within the recidivism window is longer for one relevant crash offense, the estimated specific deterrence-plus-aging effect may lump in differential duration at risk of parole/probation violation (making the estimate more positive). And if offenders further know that parole or probation violations reactivate any differential suspended incarceration time, the effect may lump in differential general deterrence (making it more negative). Depending on what type of criminal justice reform is being contemplated, variation in these ancillary sanctions might be considered a properly includable intermediate outcome. Zapryanova (2017) controls for parole length, and Ganong (2012) has a control for whether a released offender is on parole. But typically the literature leaves the effect of variation in ancillary sanctions to be lumped in with that of incarceration length.

Finally, there remains the confound of differing post-offense treatment by the justice system, including differential arresting and charging decisions, conviction outcomes, civil liability, or fear of differential future sanctions due to judge or parole-board discretion beyond the guidelines. This is a possible source of the modest imbalance between treatment and control groups on age and prior incarceration. But the qualitative conclusions for my basic three-year recidivism results in Florida and Georgia are robust to Oster (2017) bias correction for imbalance on unobservables on a modified IV specification using her suggested parametric assumptions.

Two other potential confounds are largely inapplicable in these states. First, using the three-year window, licenses are suspended over the full recidivism window for both key offenses in Florida and Georgia.[13] Second, while conceivably our two key offenses could have differential sentence enhancements for future crimes in state sentencing guidelines, lumping in a general-deterrence effect, in fact these enhancements are the same between the two offenses in our particular states.

3.5 Regression specifications

I run baseline IV regressions for recidivism[14] involving no new vehicular offenses within three years of release. OLS regressions are included for comparison. Only offenders released at least that many years before the start of 2017 are included. I compare these results to those

[13]§ 322.28, Fla. Stat.; O.C.G.A. 40-5-63

[14]I define recidivism as returning to prison (even for mere parole or probation violation) in line with the literature, including all four papers providing my benchmark estimates.

in Appendix section C.2 for any recidivism, whether involving new vehicular offenses or not. Samples consist of first incarceration episodes following an offender's first conviction that includes either but not both of serious injury by vehicle or vehicular manslaughter/homicide by vehicle. Each offender thus enters a specification at most once. The included variables are discussed below.

Specific Deterrence+Aging (plus Incapacitation for Setup IV. "Recidivism window from incarceration rather than release")

Individual states: Regress recidivism indicator on Actual incarceration length in months S, instrumenting with an indicator for offense (Vehicular Manslaughter/Homicide versus Serious Injury by Vehicle) separately by year y, separately with and without controls.

$$\text{II. } R_i = \sum_y \alpha_y + \beta \hat{S}_i + \zeta Z_i + \mu_i$$

$$\text{I. } S_i = \sum_y \theta_y + \sum_y \mathrm{I}_i^{Death} \cdot \gamma_y + \phi Z_i + \epsilon_i$$

States pooled: Suppose that, due to omitted differences between treatment and control groups, their recidivism rates differ by a fixed effect. Pooling U.S. states s while controlling for offense to isolate interstate variation can separate out this fixed difference.

$$\text{II. } R_i = \sum_{s,y} \alpha_{sy} + \sum_y \mathrm{I}_i^{Death} \cdot \lambda_y + \beta \hat{S}_i + \zeta_s Z_i + \mu_i$$

$$\text{I. } S_i = \sum_{s,y} \theta_{sy} + \sum_{s,y} \mathrm{I}_i^{Death} \cdot \gamma_{sy} + \phi_s Z_i + \epsilon_i$$

The instrumental variables are year-of-incarceration dummy variables interacted with a dummy variable for whether the offender's relevant offense is vehicular manslaughter/homicide versus serious injury by vehicle. Letting the first stage vary by year of incarceration mitigates concern about differential time effects or heterogeneous sentencing differences across time between the two offenses.

The dependent variable is a dummy variable for whether the offender returns to prison within the time window applicable to the particular specification. In these main results, recidivism is coded as one rather than zero only if there is a return to prison involving no new vehicular offense listed in Appendix section C.1. In the results in Appendix section C.2, recidivism is coded as one rather than zero if there is a return to prison, whether or not there is a new vehicular offense.

The independent variable is the actual total time served (in months, censored above at 840) during the first prison episode following conviction for the relevant offense. So it reflects the counts of that offense as well as all other offenses from the convictions the offender is serving for. An assumption is made here that the variation induced by victim death in incarceration length in total, not just for the relevant offense, is orthogonal to omitted determinants of recidivism.

As control variables, I use whether the offender at the time of offense is male, black, Hispanic, other non-white, aged 21-25 or 26-30 or 31-40 or 41+ at commission of the relevant offense, holding at least one prior incarceration, and holding multiple prior incarcerations (though again, for certain offenses in Georgia, first-time offenders may be spared a recorded conviction so that those incarcerations are not included[15]). Note that both the setups with and without controls include year-of-incarceration dummy variables or, in the setups pooling the two states, all interactions of year-of-incarceration dummy variables with state dummy variables.

Alternative specifications: I then run four variations on this baseline analysis. First, I repeat the analysis with a five-year instead of three-year recidivism window. Second, I stratify the analysis based on whether the offender was older or younger than 35 on the date of the

[15] O.C.G.A. § 42-8-60

relevant offense. Third, for a three-year window only, I analyze the subsample of offenders beginning incarceration up until the latest year by state such that less than 10% of offenders for either offense are dropped due to an insufficient recidivism window (release 2014 or later). This variation therefore mitigates differential selection between offenses on early release, e.g., for good behavior. And finally, I analyze recidivism within, separately, five and seven years of incarceration, as opposed three and five years of release. This last variation captures the policy-relevant effect of incarceration length on offenders' future crime rate over a fixed time period, operating via incapacitation during incarceration as well as specific deterrence plus aging upon release (though general deterrence is excluded).

3.6 Results

Pre-treatment balance on observables: As preliminary checks on my identification assumption, Tables 3.2 and 3.3 below suggest pre-treatment balance between treatment and control groups, except that serious injury-by-vehicle offenders in both states are somewhat older and have a higher rate of prior incarceration(s). This could reflect more discretion to sentence offenders with less criminal history on a lesser charge in cases without the pressure created by victim death to seek maximum penalties. Serious injury-by-vehicle offenders would then be selected on having prior criminal history and as a consequence tend to be older as well. Overall, F-tests reject pre-treatment balance on observables between the two offenses for Florida, while Georgia with a smaller sample size does not have balance on observables consistently rejected.

Note that even when including both non-vehicle related recidivism and vehicle-related recidivism, recidivism rates in my sample are lower than in the previous literature, which looks at a broader set of original offenses: my three-year recidivism rates at 11.6% for Florida and 19.4% for Georgia are somewhat lower than the two-year rate 24.6% from Berecochea and Jaman (1981) and substantially lower than the three-year rates 29%, 34.4%, and 37.7% from the other benchmark papers in Table 3.1.

Table 3.2: *Pre-Treatment Offender Characteristics - Balance on Observables (State=FL)*

	Basic - recidivism over 3 years			Basic - recidivism over 5 years			"Early in data window" setup - recidivism over 3 years		
	Serious Injury by Vehicle	Vehicular Homicide/ Manslaughter	p[t-test / H_0: group proportions are equal]	Serious Injury by Vehicle	Vehicular Homicide/ Manslaughter	p[t-test / H_0: group proportions are equal]	Serious Injury by Vehicle	Vehicular Homicide/ Manslaughter	p[t-test / H_0: group proportions are equal]
Male	0.836	0.817	0.285	0.842	0.817	0.212	0.869	0.835	0.222
Black	0.112	0.133	0.17	0.107	0.139	0.075	0.11	0.125	0.564
Hispanic	0.053	0.052	0.951	0.051	0.046	0.655	0.03	0.057	0.087
Other non-white	0	0.005	0.013	0	0.004	0.066	0	0.007	0.132
Age 26-30	0.15	0.146	0.837	0.149	0.139	0.589	0.148	0.131	0.538
Age 31-40	0.27	0.227	0.037	0.276	0.231	0.059	0.338	0.269	0.06
Age 41+	0.279	0.183	0	0.281	0.177	0	0.264	0.172	0.005
Prior incarcerations	0.124	0.059	0	0.117	0.057	0	0.128	0.071	0.018
Multiple prior incarcerations	0.047	0.015	0	0.045	0.013	0.001	0.039	0.02	0.176

Table 3.2 (Continued)

p[F-test / H_0: vectors of group proportions are equal]			0			0			0
Average actual term (months)	28.366	54.165		27.343	49.388		33.09	71.45	
Non-vehicle related recidivism rate	0.095	0.088		0.131	0.144		0.08	0.091	
Overall recidivism rate	0.116	0.093		0.159	0.153		0.113	0.098	
N	1121	731		913	541		337	297	

Table 3.3: *Pre-Treatment Offender Characteristics - Balance on Observables (State=GA)*

	Basic - recidivism over 3 years			Basic - recidivism over 5 years			"Early in data window" setup - recidivism over 3 years		
	Serious Injury by Vehicle	Vehicular Homicide/ Manslaughter	p[t-test / H_0: group proportions are equal]	Serious Injury by Vehicle	Vehicular Homicide/ Manslaughter	p[t-test / H_0: group proportions are equal]	Serious Injury by Vehicle	Vehicular Homicide/ Manslaughter	p[t-test / H_0: group proportions are equal]
Male	0.889	0.875	0.592	0.89	0.866	0.468	0.876	0.936	0.104
Black	0.249	0.289	0.284	0.254	0.287	0.473	0.281	0.357	0.219
Hispanic	0.103	0.119	0.544	0.092	0.111	0.549	0.045	0.064	0.527
Other non-white	0.016	0.01	0.512	0.017	0.005	0.218	0.011	0.006	0.639
Age 26-30	0.229	0.209	0.563	0.202	0.194	0.847	0.135	0.187	0.288
Age 31-40	0.225	0.244	0.596	0.254	0.227	0.529	0.258	0.257	0.985
Age 41+	0.261	0.186	0.034	0.254	0.185	0.1	0.303	0.205	0.077
Prior incarcerations	0.206	0.141	0.044	0.225	0.116	0.004	0.281	0.135	0.004
Multiple prior incarcerations	0.091	0.051	0.066	0.098	0.028	0.003	0.157	0.047	0.002

Table 3.3 (Continued)

p[F-test / H_0: vectors of group proportions are equal]			0.221			0.039			0.047
Average actual term (months)	26.747	48.301		27.322	45.188		36.297	60.136	
Non-vehicle related recidivism rate	0.174	0.109		0.254	0.153		0.169	0.105	
Overall recidivism rate	0.194	0.116		0.277	0.157		0.191	0.105	
N	253	311		173	216		89	171	

Regression estimates: For benchmark magnitudes for my estimated effects of incarceration length in months on the recidivism rate, I look to four papers (see Table 3.1). These estimates range from -0.005 to -0.013 (level impacts), which equal from -1.3% to

-3.86% of the sample-mean recidivism rates (proportional impacts).

My IV effects in Tables 3.4-3.6 on the level of the non-vehicle related recidivism rate are precise zeros for Florida. Left-tailed z-tests reject effects as negative as the comparable magnitudes from the four benchmark papers. For my Georgia IV estimates, left-tailed z-tests fail to reject the smaller benchmark estimates (-0.005 and sometimes -0.00652), while the larger ones (-0.112 and -0.013) are rejected for the three-year recidivism window or sometimes near the cusp of rejection for the five-year recidivism window. As seen in Tables C.2-C.4 in Appendix section C.2, these findings largely also hold for level impacts on the rate of any recidivism, whether vehicle related or not (except that Florida IV effects for older offenders in Table C.3 are now sometimes significantly negative but small). This is unsurprising since vehicle-related recidivism rates are very low.

I also pool states with state fixed effects to net out any fixed difference (invariant to incarceration length) in recidivism rates between the treatment and control groups. The pooled IV estimates are imprecise zeros. Standard errors may be higher because, despite differences between Florida and Georgia in the statutory ranges for the two offenses, the second difference that provides identification here (between the two states in the difference in time incarcerated between the two offenses) is empirically small.

Table 3.4: *Effect in level of non-vehicle related recidivism rate [Basic]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2416	-0.0009***	-0.00079**	-0.004	-0.0019	1843	-0.0011**	-0.0011*	0.0014	-0.000047
		(0.00026)	(0.00027)	(0.0061)	(0.0062)		(0.00042)	(0.00044)	(0.0082)	(0.0076)
S_{FL}	1852	-0.00046	-0.00043	-0.000055	-0.000088	1454	-0.00059	-0.00052	0.0003	0.00056
		(0.00025)	(0.00025)	(0.00047)	(0.00049)		(0.0004)	(0.0004)	(0.00075)	(0.0008)
S_{GA}	564	-0.0024***	-0.0023***	-0.0037*	-0.0048*	389	-0.0026*	-0.0028**	-0.0049	-0.0062*
		(0.00063)	(0.00066)	(0.0018)	(0.002)		(0.001)	(0.0011)	(0.0027)	(0.0029)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table 3.5: *Effect in level of non-vehicle related recidivism rate [II. YOUNG (34 or younger only) versus OLD (35 or older only)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
$S_{Pooled\_}$ $YOUNG$	1508	-0.0009*	-0.00051	-0.0073	-0.0084	1157	-0.0013*	-0.00091	-0.00031	-0.0022
		(0.00037)	(0.00038)	(0.0059)	(0.0065)		(0.00058)	(0.0006)	(0.007)	(0.0072)
S_{Pooled}_OLD	908	-0.00086*	-0.00083*	0.0072	0.0082	686	-0.00085	-0.00096	0.0047	0.0065
		(0.00036)	(0.00037)	(0.0062)	(0.0066)		(0.00061)	(0.00059)	(0.0063)	(0.008)
$S_{FL\_}$ $YOUNG$	1134	-0.00036	-0.00023	0.00012	-0.0002	888	-0.0006	-0.00045	0.00056	0.00029
		(0.00037)	(0.00037)	(0.00074)	(0.00078)		(0.00057)	(0.00058)	(0.0012)	(0.0012)
S_{FL}_OLD	718	-0.00071*	-0.0007*	-0.00048	-0.00059	566	-0.00074	-0.00065	-0.000059	-0.00011
		(0.00029)	(0.00029)	(0.00052)	(0.00052)		(0.00052)	(0.0005)	(0.00085)	(0.00082)
$S_{GA\_}$ $YOUNG$	374	-0.0028**	-0.0022*	-0.0033	-0.0051*	269	-0.003*	-0.0026	-0.0028	-0.0047
		(0.00092)	(0.00097)	(0.0023)	(0.0025)		(0.0014)	(0.0014)	(0.0032)	(0.0032)
S_{GA}_OLD	190	-0.0016*	-0.0017*	-0.0034	-0.003	120	-0.00057	-0.0025*	-0.0051	-0.0075
		(0.00073)	(0.00066)	(0.002)	.		(0.0011)	(0.0011)	(0.0043)	(0.0045)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table 3.6 limits the sample to early in the data window in order to minimize differential attrition due to an insufficient recidivism window.[16] And Table 3.7 lumps the incapacitation effect in with the specific deterrence-plus-aging effect, looking at recidivism in the years after the date of incarceration rather than after the date of release. This yields statistically significant negative effects as expected, at least for the three-year window.

[16] Again, here I limit the 3 year-window sample to offenders who begin incarceration up until the latest year such that less than 10% of offenders for either offense are released 2014 or later.

Table 3.6: *Effect in level of non-vehicle related recidivism rate [For 3-year recidivism window setup only, offenders beginning incarceration "early in the data window": up until the latest year by state such that less than 10% of offenders for either offense are dropped due to lack of a fully observed recidivism window (release 2014 or later)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$	
S_{Pooled}	894	-0.0006	-0.00066	-0.0037	-0.009
		(0.00034)	(0.00034)	(0.0077)	(0.011)
S_{FL}	634	-0.00029	-0.0003	0.0002	0.00032
		(0.00031)	(0.00031)	(0.00051)	(0.00054)
S_{GA}	260	-0.0014*	-0.0015*	-0.0041*	-0.0048*
		(0.00068)	(0.00069)	.	(0.0022)
Controls?		Yes	No	Yes	No

Table 3.7: *Effect in level of non-vehicle related recidivism rate [Recidivism window from date of incarceration rather than release]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2343	-0.0021***	-0.002***	-0.011	-0.009	1775	-0.0029***	-0.0029***	-0.0011	-0.001
		(0.00022)	(0.00022)	(0.0057)	(0.0048)		(0.00034)	(0.00035)	(0.0079)	(0.0074)
S_{FL}	1807	-0.0018***	-0.0017***	-0.0016***	-0.0015***	1412	-0.0023***	-0.0022***	-0.00099	-0.00069
		(0.00019)	(0.00019)	(0.00039)	(0.0004)		(0.0003)	(0.0003)	(0.00064)	(0.00066)
S_{GA}	536	-0.0035***	-0.0033***	-0.0057***	-0.0066***	363	-0.0046***	-0.0047***	-0.008**	-0.0096***
		(0.00056)	(0.00058)	(0.0016)	(0.0018)		(0.00085)	(0.00086)	(0.0026)	(0.0028)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

My IV proportional effects on the non-vehicle related recidivism rate in Tables 3.8-3.10 are again zeros for Florida, but left-tailed z-tests fail to consistently reject only the small end of benchmark proportional-impact magnitudes (-1.3% and sometimes -2.65% but not -3.78% and -3.86%). For Georgia, the IV proportional effects are statistically significant, and left-tailed z-tests fail to consistently reject all of these benchmark proportional-impact magnitudes. These findings also hold for the estimated effects on the rate of any recidivism in Tables C.6-C.8. Again, this is unsurprising since vehicle-related recidivism rates are very low.

Note that these proportional effects are just the estimated level effects in Tables 3.4-3.7 and Tables C.2-C.5 divided by the mean of the dependent variable, which is the estimation sample's observed recidivism rate. Their standard errors are thus those for the level effects divided by the mean of the dependent variable, and they are not adjusted for sampling error in the denominator. Defining the reference population as all offenders in the states and years analyzed who meet the inclusion criteria in section 3.5, the denominator is a population-average recidivism rate. But adjusting the standard error for sampling error in the denominator could be appropriate if the reference population were instead defined as some underlying theoretical distribution of offenders.

Table 3.8: *Effect in percent of non-vehicle related recidivism rate (level effect divided by sample-mean recidivism rate) [Basic]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2416	-0.0088***	-0.0077**	-0.039	-0.018	1843	-0.0074**	-0.0073*	0.0095	-0.00031
		(0.0026)	(0.0026)	(0.059)	(0.06)		(0.0028)	(0.0029)	(0.055)	(0.051)
S_{FL}	1852	-0.005	-0.0046	-0.00059	-0.00095	1454	-0.0043	-0.0039	0.0022	0.0041
		(0.0027)	(0.0027)	(0.0051)	(0.0053)		(0.0029)	(0.0029)	(0.0055)	(0.0058)
S_{GA}	564	-0.018***	-0.017***	-0.026*	-0.035*	389	-0.013*	-0.014**	-0.025	-0.031*
		(0.0045)	(0.0047)	(0.013)	(0.014)		(0.0052)	(0.0055)	(0.014)	(0.015)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table 3.9: *Effect in percent of non-vehicle related recidivism rate (level effect divided by sample-mean recidivism rate) [II. YOUNG (34 or younger only) versus OLD (35 or older only)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
$S_{Pooled\_}$ $YOUNG$	1508	-0.0075*	-0.0042	-0.06	-0.07	1157	-0.007*	-0.005	-0.0017	-0.012
		(0.0031)	(0.0032)	(0.049)	(0.054)		(0.0032)	(0.0033)	(0.038)	(0.039)
S_{Pooled}_OLD	908	-0.012*	-0.011*	0.098	0.11	686	-0.0093	-0.01	0.051	0.07
		(0.0049)	(0.0051)	(0.084)	(0.09)		(0.0066)	(0.0065)	(0.068)	(0.087)
$S_{FL\_}$ $YOUNG$	1134	-0.0033	-0.0021	0.0011	-0.0019	888	-0.0036	-0.0027	0.0033	0.0017
		(0.0034)	(0.0034)	(0.0068)	(0.0072)		(0.0034)	(0.0035)	(0.0069)	(0.0073)
S_{FL}_OLD	718	-0.011*	-0.01*	-0.0072	-0.0088	566	-0.0086	-0.0075	-0.00068	-0.0013
		(0.0043)	(0.0043)	(0.0077)	(0.0078)		(0.006)	(0.0057)	(0.0098)	(0.0095)
$S_{GA\_}$ $YOUNG$	374	-0.017**	-0.014*	-0.021	-0.032*	269	-0.013*	-0.011	-0.012	-0.02
		(0.0058)	(0.0061)	(0.015)	(0.016)		(0.0058)	(0.006)	(0.014)	(0.014)
S_{GA}_OLD	190	-0.016*	-0.017*	-0.034	-0.03	120	-0.0049	-0.021*	-0.044	-0.064
		(0.0073)	(0.0066)	(0.02)	.		(0.0095)	(0.0091)	(0.037)	(0.038)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table 3.10: *Effect in percent of non-vehicle related recidivism rate (level effect divided by sample-mean recidivism rate) [For 3-year recidivism window setup only, offenders beginning incarceration "early in the data window": up until the latest year by state such that less than 10% of offenders for either offense are dropped due to lack of a fully observed recidivism window (release 2014 or later)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$	
S_{Pooled}	894	-0.0061	-0.0067	-0.038	-0.092
		(0.0035)	(0.0035)	(0.079)	(0.11)
S_{FL}	634	-0.0034	-0.0035	0.0023	0.0038
		(0.0036)	(0.0036)	(0.006)	(0.0064)
S_{GA}	260	-0.011*	-0.012*	-0.032*	-0.038*
		(0.0053)	(0.0055)	(0.016)	(0.017)
Controls?		Yes	No	Yes	No

Table 3.11: *Effect in percent of non-vehicle related recidivism rate (level effect divided by sample-mean recidivism rate) [Recidivism window from date of incarceration rather than release]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2343	-0.027***	-0.026***	-0.15	-0.12	1775	-0.023***	-0.023***	-0.0084	-0.0078
		(0.0029)	(0.0028)	(0.075)	(0.063)		(0.0027)	(0.0027)	(0.062)	(0.058)
S_{FL}	1807	-0.026***	-0.025***	-0.023***	-0.022***	1412	-0.02***	-0.019***	-0.0084	-0.0058
		(0.0028)	(0.0028)	(0.0057)	(0.0058)		(0.0026)	(0.0025)	(0.0055)	(0.0056)
S_{GA}	536	-0.034***	-0.033***	-0.056***	-0.065***	363	-0.027***	-0.028***	-0.048**	-0.057***
		(0.0056)	(0.0057)	(0.016)	(0.018)		(0.0051)	(0.0051)	(0.015)	(0.016)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

As an extension, I explored the robustness of my basic results regarding the three-year recidivism rates to any bias arising from possible imbalance on unobservables between my two comparison groups. I calculated Oster (2017) bias-corrected estimates for my basic IV regressions with controls, introducing the modification that, unlike the regressions for my results in the Tables, my controls were included only in the second stage as opposed to both stages.[17] I used parametric assumptions suggested in Oster (2017), and I bootstrapped 90% confidence intervals for my left-tailed z-tests.[18]

For my basic results, my findings still hold when using the bias-corrected modified estimates and bootstrap confidence intervals. In particular, consider the Oster (2017) suggested bound for her bias correction parameterized by [Rmax=1.3*R-tilde, δ=1] and apply this correction to the second stage of the IV regressions for the three-year recidivism rate in Tables 3.4-3.6 and 3.8-3.10 (along with their counterparts in Appendix section C.2), but modified by including the controls only in the second stage as mentioned above. Bias-corrected level-impact estimates remain precise zeros for Florida. Left-tailed z-tests for Florida's bias-corrected IV level-impact estimates still reject all of the benchmark level-impact estimates, while those for Georgia still fail to consistently reject only the small end of benchmark estimates. And left-tailed z-tests for

[17]I apply the STATA PSACALC command to the second stage of these modified two-stage least squares IV regressions, including the assumption that unobservable variables correlate with all control variables. Oster (2017) does not directly address bias correction for IV estimation. But assuming that victim death is a valid instrument even without conditioning on my controls, this modified second-stage setup provides a consistent estimator for the effects of incarceration length on recidivism in both the short regression excluding controls from the second stage and the long regression including them. Contrast this with a second stage where the first stage included controls, which provides a consistent, and indeed more efficient, estimator in the long regression but exhibits mechanical bias in the short regression since control variable-driven variation in predicted incarceration length from the first stage is correlated with the omitted controls in the second stage. The Oster (2017) bias from unobservables is modeled as a rescaling of the shift in the estimated coefficient of interest between the short and long second-stage regressions (via including the observable control variables). This shift should reflect the correlation of the controls with the variation in incarceration length predicted by victim death versus serious injury as an instrument, not by the controls as instruments themselves. I therefore apply the Oster (2017) bias correction to the second stage of this modified setup where the first stage excludes my controls.

[18]I bootstrap confidence intervals using five hundred replications with random-number seed 1234. Note that in some bootstrap replicates not all modified regressions could be estimated, and these replicates were excluded from estimation of the bootstrap confidence intervals. Building on a line of papers including Altonji, Elder, and Taber (2005), the Oster (2017) bias correction depends on two parameters. The first is the "proportionality" value δ, a ratio of simple linear regression coefficients: that from the variable of interest on unobservables divided by that from the variable of interest on observables. And the second is Rmax, the R-squared of the hypothetical regression on the variable of interest including unobservables as well as observables. In part based on past studies and simulations, Oster (2017) suggests bounding her bias correction using an Rmax of 1.3 times R-tilde (the R-squared of the regression with observable controls) and a δ of 1. I adopt these as my parameters.

Florida's bias-corrected IV proportional-impact estimates still fail to consistently reject only the small end of benchmark proportional-impact estimates, while those for Georgia still fail to consistently reject all benchmark estimates. These findings are robust to this Oster (2017) correction whether the type of bootstrap confidence interval used is normal, percentile, or bias corrected. However, with the Oster (2017) correction, estimates pooling the two states are now inconsistently zeros and in particular are sometimes significantly negative.

3.7 Conclusions

I study quasi-random variation in the incarceration length of DUI and reckless-driving offenders in car crashes by comparing those whose victims die versus those whose victims are seriously injured. Estimated specific deterrence-plus-aging effects of incarceration length on the level of the three-year rate of non-vehicle related recidivism are precise zeros for Florida and small but mostly significantly negative for Georgia. Left-tailed z-tests for Florida IV estimates reject all of the benchmark level-impact estimates, while those for Georgia fail to consistently reject only the small end of benchmark estimates. These findings largely carry over to several alternative specifications and to the estimated impacts on any recidivism, whether vehicle related or not.

However, serious injury by vehicle and homicide or manslaughter by vehicle, the original offenses that I study, are serious and reckless but non-malicious offenses. Unsurprisingly then, my sample's recidivism rates are lower than in the benchmark papers using broader sets of original offenses. Given this difference, it might be preferable to consider proportional effects on the recidivism rate (level effects divided by sample-mean recidivism rate). Left-tailed z-tests on my proportional-impact IV estimates for Florida fail to consistently reject only the small end of the benchmark proportional-impact estimates, while those for Georgia fail to consistently reject all of the benchmark estimates. Thus, I cannot exclude the negative proportional impacts on the recidivism rate in the previous literature.

A modest imbalance between comparison groups on age and prior incarceration raises the possibility of bias from imbalance on correlated unobservables, but I find that my basic three-year recidivism results in Florida and Georgia are robust to Oster (2017) bias correction on a modified IV specification under her suggested parametric assumptions for unobservables. Aside from less precise Georgia level impacts, these results also largely hold in specifications lengthening the recidivism window or using only car crashes early in the data window (to minimize differential attrition).

Overall, my results are consistent with specific deterrence-plus-aging effects of incarceration length on the recidivism rate that may or may not be as negative as in the literature but level effects that are smaller or zero. The comparison of my findings to estimates from previous studies suggests an empirical possibility that the literature so far has not adequately addressed: the marginal effect of lengthening incarceration on recidivism via specific deterrence plus aging may be heterogeneous by incarceration length and recidivism risk. If impacts on recidivism rates are in fact nonzero, proportional impacts appear to be more comparable across different types of offenders than absolute impacts.

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Appendix A

Appendix to Chapter 1

A.1 Results Tables A.1-A.2 and A.3-A.7, Using "Exact With Wildcards" Matches (see Appendix section A.2) between Census and Enlistment Cards

Table A.1: *Differences by enlistment in Aaronson et al. (2017) partial replication (using enlistment cards matched with Census by "Exact With Wildcards" method; see main paper for results using "Fuzzy Bigram" method). [Aaronson et al. (2017) Table 1 columns (1)-(4)] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons/ HOLC grade	A non-enlistee	A enlistee	p[t-test / H_0: non-enlistee =enlistee]	B non-enlistee	B enlistee	p[t-test / H_0: non-enlistee =enlistee]	C non-enlistee	C enlistee	p[t-test / H_0: non-enlistee =enlistee]	D non-enlistee	D enlistee	p[t-test / H_0: non-enlistee =enlistee]	p[F-test / H_0: non-enlistee =enlistee, for all grades]
Share African American	0.014	0.013	0.906	0.009	0.007	0.27	0.021	0.018	0.232	0.138	0.125	0.477	0.531
HoH Share Foreign Born	0.149	0.135	0.275	0.267	0.253	0.587	0.382	0.345	0.017	0.48	0.43	0.089	0.039
HoH Homeownership Share	0.753	0.745	0.603	0.61	0.596	0.484	0.488	0.472	0.19	0.345	0.33	0.449	0.549
HoH Average Home Value	11589.13	11618.62	0.955	8348.69	8441.1	0.745	6807.5	6703.55	0.539	5428.74	5445.76	0.932	0.974

Table A.2: *Differences by enlistment in Aaronson et al. (2017) partial replication (using enlistment cards matched with Census by "Exact With Wildcards" method; see main paper for results using "Fuzzy Bigram" method). [Aaronson et al. (2017) Table 2 column (1) - Ordered logit on HOLC zones with log population weights, city fixed effects, and city standard error clustering] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

All HOLC zones	Non-enlistee	Enlistee Minus Non-enlistee
Share African American	1.124	2.078*
	(0.575)	(0.982)
HoH Share Foreign Born	1.432**	0.084
	(0.494)	(0.677)
HoH Homeownership Share	-4.768***	0.169
	(0.592)	(0.746)
HoH Average Log Home Value	-2.036***	0
	(0.261)	(0.315)
HoH Average Log Rent	-0.346*	-0.311
	(0.168)	(0.224)
HoH Average Occupation Score	-0.053*	-0.011
	(0.021)	(0.026)
HoH Share Employed	-2.613***	0.592
	(0.734)	(0.954)
HoH Share with Radio	-4.081***	1.223
	(0.573)	(0.699)
	-0.676	-1.673

Table A.2 (Continued)

HoH Share Literate	(1.053)	(1.577)
Cities	141	
N	4120	
Pseudo R^2	0.392	
p[chi2-test / H_0: Enlistee Minus Non-enlistee=0, for all regressors except intercept]	0.16	

Table A.3: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Exact With Wildcards" method; see main paper for results using "Fuzzy Bigram" method). [Summary statistics for future World War II Army enlistees versus non-enlistees] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	Enlistee	Non-enlistee	p[t-test / H_0: Enlistee Minus Non-enlistee=0]
Grade A	0.017	0.017	0.256
Grade B	0.143	0.131	0
Grade C	0.427	0.406	0
Grade D	0.412	0.446	0
African American	0.062	0.071	0
HoH Foreign Born	0.36	0.407	0
HoH Homeowner	0.435	0.444	0
HoH Log Home Value	8.586	8.573	0
HoH Log Rent	3.437	3.396	0
HoH Occupation Score	28.37	27.876	0
HoH Employed	0.856	0.845	0
HoH Radio	0.548	0.507	0
HoH Literate	0.957	0.941	0
N (less for some variables)	429119	1726894	

Table A.4: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Exact With Wildcards" method; see main paper for results using "Fuzzy Bigram" method). [Summary statistics for future World War II Army draftees versus volunteers (enlistees) versus non-enlistees, separately for whites and African Americans] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White Enlistee	White Non-enlistee	p[t-test / H_0: White Enlistee Minus Non-enlistee=0]	African American Enlistee	African American Non-enlistee	p[t-test / H_0: African American Enlistee Minus Non-enlistee=0]
Grade A	0.018	0.018	0.052	0.003	0.003	0.453
Grade B	0.151	0.14	0	0.017	0.017	0.852
Grade C	0.448	0.429	0	0.126	0.117	0
Grade D	0.383	0.413	0	0.854	0.863	0
African American	0	0	.	1	1	.
HoH Foreign Born	0.381	0.434	0	0.038	0.036	0.255
HoH Home-owner	0.451	0.464	0	0.214	0.194	0
HoH Log Home Value	8.609	8.595	0	7.848	7.864	0.242
HoH Log Rent	3.477	3.443	0	3.004	2.986	0.007
HoH Occupation Score	28.953	28.53	0	20.145	19.91	0.001
HoH Employed	0.857	0.847	0	0.823	0.813	0
HoH Radio	0.571	0.534	0	0.188	0.175	0

Table A.4 (Continued)

HoH Literate	0.96	0.943	0	0.916	0.907	0
N (less for some variables)	402802	1600285		26049	123360	

Table A.5: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Exact With Wildcards" method; see main paper for results using "Fuzzy Bigram" method). [Summary statistics for future World War II Army draftees versus volunteers (enlistees) versus non-enlistees, separately for whites and African Americans] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White Draftee	p[t-test / H_0: White Draftee Minus Non-enlistee=0]	White Volunteer	p[t-test / H_0: White Volunteer Minus Draftee=0]	White Non-enlistee	African American Draftee	p[t-test / H_0: African American Draftee Minus Non-enlistee=0]	African American Volunteer	p[t-test / H_0: African American Volunteer Minus Draftee=0]	African American Non-enlistee
Grade A	0.016	0	0.022	0	0.018	0.003	0.3	0.001	0.073	0.003
Grade B	0.142	0.001	0.172	0	0.14	0.017	0.944	0.018	0.677	0.017
Grade C	0.444	0	0.457	0	0.429	0.124	0.003	0.139	0.036	0.117
Grade D	0.398	0	0.351	0	0.413	0.855	0.003	0.841	0.054	0.863
African American	0	.	0	.	0	1	.	1	.	1
HoH Foreign Born	0.402	0	0.346	0	0.434	0.036	0.92	0.057	0	0.036
HoH Home-owner	0.455	0	0.439	0	0.464	0.214	0	0.215	0.581	0.194
HoH Log Home Value	8.594	0.792	8.653	0	8.595	7.832	0.032	8.011	0	7.864

Table A.5 (Continued)

HoH Log Rent	3.461	0	3.521	0	3.443	2.994	0.222	3.108	0	2.986
HoH Occupation Score	28.69	0	29.528	0	28.53	20.084	0.013	20.786	0.001	19.91
HoH Employed	0.855	0	0.864	0	0.847	0.822	0.001	0.834	0.152	0.813
HoH Radio	0.557	0	0.604	0	0.534	0.183	0.016	0.243	0	0.175
HoH Literate	0.955	0	0.97	0	0.943	0.914	0.001	0.939	0	0.907
N (less for some variables)	299509		103293		1600285	23882		2167		123360

Table A.6: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Exact With Wildcards" method; see main paper for results using "Fuzzy Bigram" method). [Logit on sample persons predicting future enlistees (and multinomial logit predicting draftees and volunteers, with non-enlistees as the base outcome), with African American race interaction terms and household standard error clustering] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White	African American Minus White
Grade A Minus Grade D	-0.047**	0.164
	(0.017)	(0.141)
Grade B Minus Grade D	0.083***	-0.04
	(0.007)	(0.061)
Grade C Minus Grade D	0.075***	0.01
	(0.005)	(0.025)
Grade D	-1.641***	-0.092*
	(0.014)	(0.042)
HoH Foreign Born	-0.15***	0.21***
	(0.005)	(0.043)
HoH Homeowner	-0.069***	0.167***
	(0.005)	(0.021)
HoH Occupation Score	0.001***	0.001
	(0)	(0.001)
HoH Employed	0.036***	0.011
	(0.008)	(0.026)
	0.1***	-0.04

Table A.6 (Continued)

HoH Radio		
	(0.005)	(0.022)
HoH Literate	0.209***	-0.136***
	(0.011)	(0.032)
N	1467458	116402
p[F-test / H_0: African American Minus White=0, for all regressors except intercept]	0	

Table A.7: *Differential selection into future enlistment between African Americans and whites (using enlistment cards matched with Census by "Exact With Wildcards" method; see main paper for results using "Fuzzy Bigram" method). [Logit on sample persons predicting future enlistees (and multinomial logit predicting draftees and volunteers, with non-enlistees as the base outcome), with African American race interaction terms and household standard error clustering] In limited sample of only males native-born between April 2, 1915 and April 1, 1926 inclusive. Variables are for sample male head of household except enlistment and African American dummies.*

Sample persons	White (Y=DRAFT)	African American Minus White (Y=DRAFT)	White (Y=VOLUNTEER)	African American Minus White (Y=VOLUNTEER)
Grade A Minus Grade D	-0.147*** (0.02)	0.316* (0.144)	0.206*** (0.028)	-1.121 (0.707)
Grade B Minus Grade D	0.021** (0.008)	0.005 (0.063)	0.257*** (0.012)	-0.039 (0.175)
Grade C Minus Grade D	0.046*** (0.006)	0.021 (0.026)	0.166*** (0.009)	0.102 (0.072)
Grade D	-1.809*** (0.016)	0.034 (0.044)	-3.512*** (0.03)	-1.248*** (0.134)
HoH Foreign Born	-0.094*** (0.005)	0.115* (0.045)	-0.32*** (0.009)	0.722*** (0.112)
HoH Homeowner	-0.04*** (0.005)	0.14*** (0.022)	-0.158*** (0.008)	0.224*** (0.062)
HoH Occupation Score	0 (0)	0.002 (0.001)	0.005*** (0)	0.003 (0.003)
HoH Employed	0.025** (0.008)	0.016 (0.027)	0.071*** (0.014)	0.031 (0.079)

Table A.7 (Continued)

HoH Radio	0.069*** (0.005)	-0.043 (0.023)	0.192*** (0.008)	0.186** (0.06)
HoH Literate	0.14*** (0.012)	-0.088** (0.033)	0.534*** (0.025)	-0.181 (0.108)
N	1467458		116402	
p[F-test / H_0: African American Minus White=0, for all regressors except intercept]	0			

A.2 Matching the 1930 Census to World War II Army enlistment cards

The 1930 Census microdata are a restricted-use version from IPUMS-USA containing individuals' names. The enlistment cards are from the National Archives and include about 87% of enlistees with available microfilmed punch card records and exclude officers (Documentation relating to World War II Army Enlistment Records, 6/1/2002 - 9/30/2002 [Electronic Record]). Note that unread characters are rendered in enlistment-card names as spaces and in Census names as "?". Below I refer to these characters in their respective data sets as "wildcards."

I begin with the 1930 Census observations and WWII Army enlistment cards for native-born males[1] who are not missing any of the matching variables I use below. I exclude the Enlisted Reserve Corps. I subset the enlistment cards to those born in a year ending in digits 15-26 (almost surely born 1915-1926) since these are core cohorts who served in World War II (Bound and Turner 2002). Then I further subset them to those with Army Serial Numbers starting with either 0, 1, or 2 for volunteer enlistees (in which I include National Guardsmen inducted into federal service) or 3 or 4 for drafted enlistees (Documentation relating to World War II Army Enlistment Records, 6/1/2002 - 9/30/2002 [Electronic Record]). To generate "candidate matches" that I can winnow down based on full name similarity,[2] I first form all pairs between the data sets that have the same race,[3] birthplace,[4] first two non-space characters of the last name (Census)/full name (enlistment card), and possible age on April 1, 1930. I consider a veteran born in year 1930-A and a Census individual aged A or A-1 on April 1, 1930 to have

[1]An enlistee is coded as native born if the Nativity field value is in the United States. And an enlistee is coded as male if the Component field value does not equal "9", the Branch: code field value does not equal "18," the Branch: alpha designation field value does not equal "WAC," and the Army serial number does not begin with "A."

[2]Because the enlistment cards do not distinguish different parts of the full name, for Census individuals I construct a single string that is last name followed by a space and then first name. Census first name may include middle initials and suffixes. I strip off Census first-name endings JR, SR, JUNIOR, and SENIOR since they should usually be distinguishable by age.

[3]White including Puerto Rican, black, Native American, Chinese American, Japanese American, Hawaiian, Filipino American, or other.

[4]The U.S. state or territory, "Native American", "United States, n.s.", or the "US OUTLYING AREAS/TERRITORIES" category.

the same possible age on April 1, 1930.

My main analysis uses Army enlistees' de-duplicated (see below) Census matches with a highest "fuzzy bigram" match score. It first subsets candidate matches: (a) to those where the Census first name of string length L has a Jaccard bigram matchit score of at least 0.65 (a threshold established based on some manual inspection) with the first L characters of the enlistment-card full name; and then further (b) to those where the Census last name of string length M has a Jaccard bigram matchit score of at least 0.65 with the substring of the enlistment-card full name starting at position L+1. The match score given to the remaining matches equals the Jaccard bigram matchit score of the full names from each data set.

In Appendix section A.1, I repeat my analyses using Army enlistees' de-duplicated Census matches with a highest "exact with wildcards" score, equal to the number of consecutive matching characters in the full names starting with the first character if one full name is an exact substring of the other, or else zero (except that one data set's wildcard is allowed to match with any character in the same position in the other data set).

Because my matching algorithms may duplicate Census individuals across multiple veterans' highest-score matches, I run the following de-duplication procedure: I first assign each veteran a "provisional match" among his highest-score Census matches by applying a sequence of tiebreakers: Census individuals who aren't among another veteran's high-score matches, then the lower of the two matching Census ages on April 1, 1930 since it gives the higher conditional probability of having been born in the veteran's birth year, and finally and arbitrarily the lower household serial number and then lower person number.

If there are duplicate Census individuals across veterans' provisional matches, I then apply a second step and loop the two steps until there aren't any duplicates left. This second step assigns each duplicate Census individual to be a single veteran's provisional match by applying a sequence of tiebreakers: highest match score, then the lower of the two matching birth years since it gives the higher conditional probability of having been the same age on April 1, 1930, and finally and arbitrarily the lower enlistment card data set order number. Now drop duplicate individuals' other provisional matches and return to the first step to assign new provisional matches among the remaining flexible matches to veterans who had theirs dropped.

A.3 Geocoding the 1930 Census and matching to HOLC map zones

I begin with 151 HOLC-map areas pulled from the Mapping Inequality website https://dsl.richmond.edu/panorama/redlining/.

- Four (Bergen, Essex, and Hudson in NJ; Lower Westchester in NY) are named after a county. I unpack the county into the 1930 IPUMS standardized cities (listed on the IPUMS-USA website as available in the 1930-full count sample) that are listed under the county name at https://stevemorse.org/census/unified.html?year=1930. I include the unpacked cities in the stevemorse.org fetch list.
- Three (Greater Kansas City in MO, Lake County Gary in IN, and Milwaukee County in WI) have names that include a 1930 IPUMS standardized city and a county (for Greater Kansas City, I used Johnson and Wyandotte in KS and Clay, Jackson, and Platte in MO). I unpack the county (or both Lake County in two states) in the aforementioned way and include the mentioned city (or both Kansas City in two states) as well in the stevemorse.org fetch list.
- Several have names that are a list of 1930 IPUMS standardized cities. I include all listed cities in the stevemorse.org fetch list.
- Finally, the rest are named after a 1930 IPUMS standardized city. I include the city in the stevemorse.org fetch list.

I format this fetch list appropriately and run the Urban Transition Historical GIS Project's Census histcensusgis stevemorse subcommand to pull a dictionary of street names from stevemorse.org for each city.[5] I then save the subset of the 1930 Census observations residing in the cities on the fetch list and run the histcensusgis clean_microdata subcommand on these

[5] https://github.com/graziul/hist-census-gis/blob/master/histcensusgis/text/stevemorse.py (modifying file paths appropriately)

observations to clean the street names and house numbers, interpolate house numbers, and fill in missing streets.[6]

Finally, I run the cleaned address information (house number, street, city, state) through the geocoder in ArcMap with ESRI Business Analyst. I treat any addresses with a negative or missing street number or with no letters in the street address (generally corresponding to a street name) as automatically failing to be geocoded. I drop a Bronx HOLC zone rated "E", close any unclosed HOLC polygon by connecting its final sequential vertex back to its first sequential vertex, and then use the GEOINPOLY STATA command to match Census households to HOLC zones. 141 of 7,463 HOLC shapes were detected as invalid polygons by GEOINPOLY and were not used to match to Census households. 369,318 or 5.24% of the 7,051,678 Census households matched to HOLC zones were matched to multiple zones due to polygon overlaps; I resolve this ambiguity by dropping these households from my analysis. Although I geocode 169 1930 IPUMS standardized cities, I analyze only 145 of them: the 149 cities analyzed by Aaronson et al. (2017) minus Buffalo, NY, Essex County, NJ, Hudson County, NJ, and Lexington, MA.

Note that under my method to try geocoding HOLC map-residing 1930 households, households incorrectly included or excluded for my geocoding are those: (1) in one but not both of a HOLC-map area named after a 1930 IPUMS standardized city and that standardized city; and (2) in a HOLC-map area without a 1930 IPUMS standardized city of the same name who are not then added by the supplementary steps described above (namely those in Darien and New Canaan in CT and Lexington in MA as well as in some towns with less than a 25,000 population in the HOLC-map areas named after counties).

A.4 Included HOLC-map areas

[6] See histcensusgis at pypi.org for more information. I run only households, so the remove_duplicates subscript exhibits the expected behavior of exiting with a fail status.

Table A.8: *Included HOLC-map areas*

Akron, OH	Duluth, MN	Madison, WI	Saginaw, MI
Albany, NY	Durham, NC	Malden, MA	San Diego, CA
Altoona, PA	East Hartford, CT	Manchester, NH	San Francisco, CA
Arlington, MA	East St. Louis, IL	Manhattan, NY	San Jose, CA
Asheville, NC	Elmira, NY	Medford, MA	Saugus, MA
Atlanta, GA	Erie, PA	Melrose, MA	Schenectady, NY
Atlantic City, NJ	Essex County, NJ	Miami, FL	Seattle, WA
Augusta, GA	Evansville, IN	Milton, MA	Somerville, MA
Aurora, IL	Everett, MA	Milwaukee Co., WI	South Bend, IN
Baltimore, MD	Flint, MI	Minneapolis, MN	Spokane, WA
Battle Creek, MI	Fort Wayne, IN	Mobile, AL	Springfield, IL
Bay City, MI	Fresno, CA	Montgomery, AL	Springfield, MO
Belmont, MA	Grand Rapids, MI	Muncie, IN	Springfield, OH
Bergen Co., NJ	Greater Kansas City,	Muskegon, MI	St. Joseph, MO
Binghamton/Johnson	MO	Needham, MA	St. Louis, MO
City, NY	Greensboro, NC	New Britain, CT	St. Petersburg, FL
Birmingham, AL	Hamilton, OH	New Castle, PA	Stamford, Darien,
Boston, MA	Haverhill, MA	New Haven, CT	and New Canaan,
Braintree, MA	Holyoke Chicopee,	New Orleans, LA	CT
Brockton, MA	MA	Newport News, VA	Staten Island, NY
Bronx, NY	Hudson County, NJ	Newton, MA	Stockton, CA
Brookline, MA	Indianapolis, IN	Niagara Falls, NY	Syracuse, NY
Brooklyn, NY	Jacksonville, FL	Norfolk, VA	Tacoma, WA
Cambridge, MA	Johnstown, PA	Oakland, CA	Tampa, FL
Camden, NJ	Joliet, IL	Oshkosh, WI	Terre Haute, IN
Canton, OH	Kalamazoo, MI	Philadelphia, PA	Toledo, OH
Charleston, WV	Kenosha, WI	Pittsburgh, PA	Trenton, NJ
Charlotte, NC	Knoxville, TN	Pontiac, MI	Troy, NY
Chattanooga, TN	Lake County Gary,	Portland, OR	Utica, NY
Chelsea, MA	IN	Portsmouth, OH	Waltham, MA
Chicago, IL	Lexington, KY	Poughkeepsie, NY	Warren, OH
Cleveland, OH	Lexington, MA	Queens, NY	Watertown, MA
Columbus, GA	Lima, OH	Quincy, MA	Wheeling, WV
Columbus, OH	Lorain, OH	Racine, WI	Wichita, KS
Dallas, TX	Los Angeles, CA	Revere, MA	Winchester, MA
Dayton, OH	Louisville, KY	Richmond, VA	Winston Salem, NC
Decatur, IL	Lower Westchester	Roanoke, VA	Winthrop, MA
Dedham, MA	Co., NY	Rochester, NY	Youngstown, OH
Denver, CO	Lynchburg, VA	Rockford, IL	
Detroit, MI	Macon, GA	Sacramento, CA	

A.5 Included 1930 IPUMS standardized cities

Table A.9: *Included 1930 IPUMS standardized cities*

Akron, OH	East Hartford, CT	Melrose, MA	Saint Louis, MO
Albany, NY	East Orange, NJ	Miami, FL	Saint Petersburg, FL
Altoona, PA	East St. Louis, IL	Milton, MA	San Diego, CA
Arlington, MA	Elmira, NY	Milwaukee, WI	San Francisco, CA
Asheville, NC	Erie, PA	Minneapolis, MN	San Jose, CA
Atlanta, GA	Evansville, IN	Mobile, AL	Saugus, MA
Atlantic City, NJ	Everett, MA	Montclair, NJ	Schenectady, NY
Augusta, GA	Flint, MI	Montgomery, AL	Seattle, WA
Aurora, IL	Fort Wayne, IN	Mount Vernon, NY	Somerville, MA
Baltimore, MD	Fresno, CA	Muncie, IN	South Bend, IN
Battle Creek, MI	Garfield, NJ	Muskegon, MI	Spokane, WA
Bay City, MI	Gary, IN	Needham, MA	Springfield, IL
Bayonne, NJ	Grand Rapids, MI	New Britain, CT	Springfield, MO
Belleville, NJ	Greensboro, NC	New Castle, PA	Springfield, OH
Belmont, MA	Hamilton, OH	New Haven, CT	Stamford, CT
Binghamton, NY	Hammond, IN	New Orleans, LA	Stockton, CA
Birmingham, AL	Haverhill, MA	New Rochelle, NY	Syracuse, NY
Bloomfield, NJ	Hoboken, NJ	New York, NY	Tacoma, WA
Boston, MA	Holyoke, MA	Newark, NJ	Tampa, FL
Braintree, MA	Indianapolis, IN	Newport News, VA	Terre Haute, IN
Brockton, MA	Irvington, NJ	Newton, MA	Toledo, OH
Brookline, MA	Jacksonville, FL	Niagara Falls, NY	Trenton, NJ
Cambridge, MA	Jersey City, NJ	Norfolk, VA	Troy, NY
Camden, NJ	Johnson City, NY	North Bergen, NJ	Union City, NJ
Canton, OH	Johnstown, PA	Oakland, CA	Utica, NY
Charleston, WV	Joliet, IL	Orange, NJ	Waltham, MA
Charlotte, NC	Kalamazoo, MI	Oshkosh, WI	Warren, OH
Chattanooga, TN	Kansas City, KS	Philadelphia, PA	Watertown, MA
Chelsea, MA	Kansas City, MO	Pittsburgh, PA	Waukegan, IL
Chicago, IL	Kearney, NJ	Pontiac, MI	Wauwatosa, WI
Chicopee, MA	Kenosha, WI	Portland, OR	West Allis, WI
Cleveland, OH	Knoxville, TN	Portsmouth, OH	West New York, NJ
Columbus, GA	Lexington, KY	Poughkeepsie, NY	Wheeling, WV
Columbus, OH	Lima, OH	Quincy, MA	White Plains, NY
Dallas, TX	Lorain, OH	Racine, WI	Wichita, KS
Dayton, OH	Los Angeles, CA	Revere, MA	Winchester, MA
Decatur, IL	Louisville, KY	Richmond, VA	Winston-Salem, NC
Dedham, MA	Lynchburg, VA	Roanoke, VA	Winthrop, MA
Denver, CO	Macon, GA	Rochester, NY	Yonkers, NY
Detroit, MI	Madison, WI	Rockford, IL	Youngstown, OH
Duluth, MN	Malden, MA	Sacramento, CA	
Durham, NC	Manchester, NH	Saginaw, MI	
East Chicago, IN	Medford, MA	Saint Joseph, MO	

Appendix B

Appendix to Chapter 2

B.1 Appendix

Table B.1: *Full text of CIRP and follow-up survey questions*

Summary label	**Full text**
Abolish the death penalty (dependent variable)	The death penalty should be abolished. (4=Strongly Agree)
Abolish the death penalty (control variable)	The death penalty should be abolished (4=Agree strongly)
Student diversity essential for quality higher education	Having a diverse student body is essential for high quality higher education. (4=Strongly Agree)
Colleges should prohibit racist/sexist speech	Colleges should prohibit racist/sexist speech on campus (4=Agree strongly)
Comfort in interacting with other racial/ethnic groups	When you interact with people from other racial/ethnic groups, how comfortable is it for you? (4=Very Comfortable)
Comfortable talking with roommate	I was comfortable talking with Roommate#1 (4=Strongly Agree)
Conflicts or arguments with roommate not frequent	Roommate#1 and I often had conflicts or arguments. (4=Strongly Disagree)
Courts not overconcerned with rights of criminals (dependent variable)	There is too much concern in the courts for the rights of criminals. (4=Strongly Disagree)
Courts not overconcerned with rights of criminals (control variable)	There is too much concern in the courts for rights of criminals (4=Disagree strongly)
Economic differences in the US are not justified	The economic differences in this country are justified. (4=Strongly Disagree)
Ensuring campus diversity justifies affirmative action	Affirmative action is justified if it ensures a diverse student body on college campuses. (4=Strongly Agree)
Father's education (Table 2 only)	What is the highest level of formal education obtained by your Father? (1 - Middle school or less; to 7 - Graduate or Professional coursework or degree)

Table B.1 (Continued)

Father's education (all other tables)	What is the highest level of formal education obtained by your parents? Father (1 - Grammar school or less; to 7 - Some graduate school or Graduate degree)
Frequency of interactions with someone black during past semester	This question refers to the past fall semester (Fall 2007). Please indicate how often you engaged in the activities listed below during that time. Socialized with someone from an African-American background (valued at midpoint of monthly frequency bucket)
Immigration is a good thing for the US	On the whole, immigration is a good thing for this country today (4=Strongly Agree)
Keep affirmative action in college admissions (dependent variable)	Affirmative action in college admissions should be abolished. (4=Strongly Disagree)
Keep affirmative action in college admissions (control variable)	Affirmative action in college admissions should be abolished (4=Disagree strongly)
Legally recognize same-sex marriage (dependent variable)	Same-sex couples should have the right to legal marital status. (4=Strongly Agree)
Legally recognize same-sex marriage (control variable)	Same-sex couples should have the right to legal marital status (4=Agree strongly)
Let undocumented immigrants access public education	Undocumented immigrants should be denied access to public education (4=Disagree strongly)
Mother's education (Table 2 only)	What is the highest level of formal education obtained by your Mother? (1 - Middle school or less; to 7 - Graduate or Professional coursework or degree)

Table B.1 (Continued)

Mother's education (all other tables)	What is the highest level of formal education obtained by your parents? Mother (1 - Grammar school or less; to 7 - Some graduate school or Graduate degree)
National health care plan needed (dependent variable)	A national health care plan is needed to cover everybody's medical costs. (4=Strongly Agree)
National health care plan needed (control variable)	A national health care plan is needed to cover everybody's medical costs (4=Agree strongly)
Percentage of close friends from own racial group	What percentage of your close friends are from your own racial group? (valued at midpoint of percentage bucket)
Percentage of close friends from own socioeconomic class	What percentage of your close friends are from your social class? (By , we mean categories like working class, middle class, upper-middle class, and upper class.) (valued at midpoint of percentage bucket)
Racial discrimination a major problem in the US	Racial discrimination is no longer a major problem in America (4=Disagree strongly)
Religion should have less national influence	The US would be a better country if religion had less influence. (4=Strongly Agree)
Roommate is one of best college friends	I considered Roommate#1 to be one of my best college friends. (4=Strongly Agree)
Success mainly depends on family background	What one can achieve in life depends mainly upon one's family background. (4=Strongly Agree)
The wealthy should pay higher taxes (dependent variable)	Wealthy people should pay a larger share of taxes than they do now. (4=Strongly Agree)

Table B.1 (Continued)

The wealthy should pay higher taxes (control variable)	Wealthy people should pay a larger share of taxes than they do now (4=Agree strongly)
Would be friends even if not roommates	Roommate#1 was the type of person I would be friends with even if we were not roommates. (4=Strongly Agree)

Table B.2.1: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college*

	Courts not over-concerned with rights of criminals			Abolish the death penalty		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.133	-0.070	-0.049	0.052	0.055	0.039
	(0.086)	(0.098)	(0.068)	(0.086)	(0.095)	(0.087)
Roommate - Other minority	-0.038	-0.053	-0.039	-0.015	-0.022	-0.021
	(0.069)	(0.073)	(0.050)	(0.069)	(0.073)	(0.068)
Roommate - Racial discrimination a major problem in the US	-0.093*	-0.094	-0.063	-0.091	-0.086	-0.066
	(0.047)	(0.060)	(0.041)	(0.048)	(0.060)	(0.055)
Roommate - The wealthy should pay higher taxes	0.068	0.056	0.034	0.018	0.024	0.018
	(0.046)	(0.053)	(0.037)	(0.047)	(0.051)	(0.047)
Roommate - Colleges should prohibit racist/sexist speech	0.034	0.005	0.000	0.011	0.003	0.006

Table B.2.1 (Continued)

	(0.040)	(0.048)	(0.033)	(0.036)	(0.044)	(0.040)
Roommate - Keep affirmative action in college admissions	0.049	0.051	0.036	0.023	-0.004	0.002
	(0.048)	(0.055)	(0.038)	(0.046)	(0.053)	(0.048)
Roommate - Let undocumented immigrants access public education	0.018	0.033	0.024	0.009	0.021	0.018
	(0.043)	(0.047)	(0.032)	(0.047)	(0.051)	(0.047)
Roommate - Courts not overconcerned with rights of criminals	-0.053	-0.141*	-0.093*	0.039	-0.024	-0.021
	(0.049)	(0.058)	(0.040)	(0.049)	(0.060)	(0.055)
Roommate - Abolish the death penalty	0.034	0.009	0.006	-0.001	-0.012	-0.008
	(0.043)	(0.048)	(0.033)	(0.044)	(0.049)	(0.046)
Roommate - National health care plan needed	-0.043	-0.013	-0.008	0.023	0.024	0.025
	(0.047)	(0.056)	(0.039)	(0.045)	(0.054)	(0.051)

Table B.2.1 (Continued)

Roommate - Legally recognize same-sex marriage	0.003	0.011	0.006	-0.029	-0.026	-0.024
	(0.040)	(0.043)	(0.030)	(0.040)	(0.043)	(0.040)
Roommate - Family income < $50,000		-0.049	-0.034		0.035	0.028
		(0.091)	(0.063)		(0.089)	(0.082)
Roommate - Family income $50,000-$74,999		-0.023	-0.016		-0.044	-0.044
		(0.085)	(0.058)		(0.085)	(0.079)
Roommate - Family income $150,000-$199,999		0.043	0.027		0.010	0.010
		(0.092)	(0.063)		(0.092)	(0.084)
Roommate - Family income >= $200,000		-0.004	-0.003		-0.071	-0.063
		(0.082)	(0.056)		(0.080)	(0.075)
Years since freshman year		0.440	0.294		0.657	0.568
		(0.263)	(0.173)		(0.451)	(0.365)

Table B.2.1 (Continued)

Racial discrimination a major problem in the US		0.192**	0.120**		0.093	0.086
		(0.059)	(0.041)		(0.058)	(0.053)
The wealthy should pay higher taxes		0.152**	0.098**		0.201***	0.174***
		(0.049)	(0.034)		(0.048)	(0.043)
Colleges should prohibit racist/sexist speech		-0.149**	-0.096**		-0.131**	-0.117**
		(0.050)	(0.034)		(0.047)	(0.042)
Keep affirmative action in college admissions		0.105*	0.070*		0.075	0.067
		(0.051)	(0.035)		(0.046)	(0.043)
Share with dependent variable equaling 1		0.049	0.049		0.152	0.152
Share with dependent variable equaling 4		0.171	0.171		0.275	0.275

Table B.2.1 (Continued)

R-squared/pseudo-R-squared	0.051	0.080	0.164	0.044	0.067	0.163
Number of observations	1951	1951	1951	1952	1952	1952
p-value[H_0: coefficients on all roommate attitude regressors equal zero]	.	0.389	0.452	.	0.924	0.966

Table B.2.2: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college*

	The wealthy should pay higher taxes			National health care plan needed		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.049	-0.041	-0.027	-0.168*	-0.117	-0.081
	(0.078)	(0.090)	(0.069)	(0.083)	(0.096)	(0.076)
Roommate - Other minority	-0.115	-0.135	-0.102	0.038	0.026	0.007
	(0.070)	(0.072)	(0.055)	(0.070)	(0.074)	(0.057)
Roommate - Racial discrimination a major problem in the US	-0.030	-0.045	-0.034	-0.066	-0.048	-0.036
	(0.046)	(0.055)	(0.043)	(0.048)	(0.061)	(0.048)
Roommate - The wealthy should pay higher taxes	0.058	0.049	0.041	0.111*	0.079	0.058
	(0.046)	(0.051)	(0.038)	(0.045)	(0.053)	(0.041)
Roommate - Colleges should prohibit racist/sexist speech	0.014	0.011	0.009	0.058	0.056	0.037

Table B.2.2 (Continued)

	(0.036)	(0.044)	(0.033)	(0.037)	(0.045)	(0.035)
Roommate - Keep affirmative action in college admissions	0.041	0.002	0.005	0.070	0.058	0.039
	(0.044)	(0.051)	(0.039)	(0.046)	(0.054)	(0.042)
Roommate - Let undocumented immigrants access public education	-0.012	0.049	0.034	0.018	0.042	0.039
	(0.045)	(0.048)	(0.037)	(0.048)	(0.053)	(0.042)
Roommate - Courts not overconcerned with rights of criminals	0.090	0.023	0.022	0.041	-0.007	-0.010
	(0.047)	(0.057)	(0.043)	(0.049)	(0.059)	(0.046)
Roommate - Abolish the death penalty	-0.031	-0.066	-0.046	-0.039	-0.067	-0.041
	(0.041)	(0.045)	(0.035)	(0.044)	(0.048)	(0.039)
Roommate - National health care plan needed	-0.115*	-0.060	-0.043	-0.173***	-0.146*	-0.108*
	(0.046)	(0.056)	(0.042)	(0.046)	(0.057)	(0.044)

Table B.2.2 (Continued)

Roommate - Legally recognize same-sex marriage	-0.005	0.046	0.033	0.006	0.024	0.017
	(0.040)	(0.044)	(0.034)	(0.041)	(0.045)	(0.035)
Roommate - Family income < \$50,000		-0.158	-0.106		-0.148	-0.100
		(0.086)	(0.066)		(0.088)	(0.069)
Roommate - Family income \$50,000-\$74,999		-0.043	-0.029		-0.111	-0.078
		(0.083)	(0.063)		(0.085)	(0.066)
Roommate - Family income \$150,000-\$199,999		-0.157	-0.108		-0.040	-0.025
		(0.093)	(0.072)		(0.092)	(0.072)
Roommate - Family income >= \$200,000		-0.123	-0.086		-0.031	-0.015
		(0.080)	(0.061)		(0.080)	(0.063)
Years since freshman year		0.478	0.350		0.668	0.460
		(0.405)	(0.303)		(0.382)	(0.235)

Table B.2.2 (Continued)

Racial discrimination a major problem in the US		0.079	0.055		0.170**	0.116**
		(0.050)	(0.038)		(0.057)	(0.043)
The wealthy should pay higher taxes		0.666***	0.472***		0.439***	0.315***
		(0.047)	(0.032)		(0.048)	(0.035)
Colleges should prohibit racist/sexist speech		-0.074	-0.053		-0.010	-0.007
		(0.042)	(0.031)		(0.046)	(0.034)
Keep affirmative action in college admissions		0.051	0.037		0.142**	0.120***
		(0.044)	(0.033)		(0.050)	(0.036)
Share with dependent variable equaling 1		0.085	0.085		0.083	0.083
Share with dependent variable equaling 4		0.284	0.284		0.349	0.349

Table B.2.2 (Continued)

R-squared/pseudo-R-squared	0.044	0.113	0.244	0.052	0.098	0.209
Number of observations	1947	1947	1947	1955	1955	1955
p-value[H_0: coefficients on all roommate attitude regressors equal zero]	.	0.748	0.783	.	0.143	0.241

Table B.2.3: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college*

	Success mainly depends on family background			Economic differences in the US are not justified		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	0.034	0.079	0.052	-0.124	-0.097	-0.063
	(0.083)	(0.092)	(0.069)	(0.084)	(0.095)	(0.064)
Roommate - Other minority	-0.090	-0.095	-0.069	0.013	-0.002	-0.003
	(0.069)	(0.071)	(0.052)	(0.070)	(0.072)	(0.049)
Roommate - Racial discrimination a major problem in the US	-0.066	-0.118*	-0.086	-0.144**	-0.240***	-0.147***
	(0.051)	(0.059)	(0.044)	(0.048)	(0.061)	(0.040)
Roommate - The wealthy should pay higher taxes	-0.040	-0.093	-0.065	0.097	0.072	0.045
	(0.044)	(0.051)	(0.038)	(0.050)	(0.054)	(0.036)
Roommate - Colleges should prohibit racist/sexist speech	0.014	-0.002	0.001	0.011	-0.092*	-0.056

Table B.2.3 (Continued)

	(0.037)	(0.046)	(0.034)	(0.037)	(0.045)	(0.030)
Roommate - Keep affirmative action in college admissions	0.018	0.054	0.040	0.033	-0.006	-0.002
	(0.047)	(0.053)	(0.040)	(0.049)	(0.054)	(0.036)
Roommate - Let undocumented immigrants access public education	0.018	0.039	0.027	0.052	0.041	0.023
	(0.047)	(0.050)	(0.037)	(0.047)	(0.050)	(0.033)
Roommate - Courts not over-concerned with rights of criminals	-0.034	-0.091	-0.065	0.064	-0.012	-0.007
	(0.049)	(0.058)	(0.043)	(0.051)	(0.058)	(0.039)
Roommate - Abolish the death penalty	-0.002	0.001	0.000	-0.014	-0.041	-0.027
	(0.042)	(0.044)	(0.032)	(0.042)	(0.043)	(0.029)
Roommate - National health care plan needed	0.068	0.085	0.059	-0.042	-0.054	-0.032
	(0.047)	(0.056)	(0.042)	(0.050)	(0.059)	(0.039)

Table B.2.3 (Continued)

Roommate - Legally recognize same-sex marriage	0.062	0.055	0.039	-0.016	-0.024	-0.014
	(0.039)	(0.043)	(0.032)	(0.042)	(0.044)	(0.029)
Roommate - Family income < \$50,000		0.003	0.001		0.049	0.032
		(0.090)	(0.067)		(0.091)	(0.061)
Roommate - Family income \$50,000-\$74,999		0.066	0.050		0.013	0.004
		(0.083)	(0.062)		(0.086)	(0.058)
Roommate - Family income \$150,000-\$199,999		0.046	0.030		-0.068	-0.042
		(0.091)	(0.069)		(0.090)	(0.061)
Roommate - Family income >= \$200,000		0.022	0.021		0.050	0.031
		(0.082)	(0.061)		(0.081)	(0.054)
Years since freshman year		-0.060	-0.060		0.351	0.223
		(0.287)	(0.216)		(0.281)	(0.184)

Table B.2.3 (Continued)

Racial discrimination a major problem in the US		0.049	0.036		0.121*	0.075*
		(0.057)	(0.042)		(0.056)	(0.037)
The wealthy should pay higher taxes		-0.012	-0.008		0.259***	0.164***
		(0.048)	(0.036)		(0.046)	(0.031)
Colleges should prohibit racist/sexist speech		0.051	0.036		-0.103*	-0.064*
		(0.047)	(0.035)		(0.045)	(0.029)
Keep affirmative action in college admissions		-0.034	-0.024		0.225***	0.140***
		(0.052)	(0.039)		(0.051)	(0.034)
Share with dependent variable equaling 1		0.301	0.301		0.032	0.032
Share with dependent variable equaling 4		0.028	0.028		0.240	0.240

Table B.2.3 (Continued)

R-squared/pseudo-R-squared	0.045	0.056	0.123	0.067	0.107	0.216
Number of observations	1965	1965	1965	1947	1947	1947
p-value[H_0: coefficients on all roommate attitude regressors equal zero]	.	0.246	0.307	.	0.002	0.005

Table B.2.4: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.129	-0.116	-0.080	-0.105	-0.054	-0.037	-0.141	-0.089	-0.048
	(0.087)	(0.098)	(0.080)	(0.081)	(0.091)	(0.072)	(0.090)	(0.104)	(0.070)
Roommate - Other minority	0.039	-0.007	-0.000	-0.018	-0.055	-0.042	-0.065	-0.078	-0.054
	(0.071)	(0.072)	(0.059)	(0.072)	(0.072)	(0.057)	(0.072)	(0.075)	(0.050)
Roommate - Racial discrimination a major problem in the US	-0.077	-0.103	-0.083	-0.048	-0.066	-0.048	-0.013	-0.024	-0.021
	(0.047)	(0.056)	(0.046)	(0.047)	(0.058)	(0.045)	(0.048)	(0.058)	(0.037)

Table B.2.4 (Continued)

Roommate - The wealthy should pay higher taxes	0.054	0.027	0.020	0.009	-0.026	-0.020	0.076	0.027	0.013
	(0.043)	(0.048)	(0.040)	(0.045)	(0.049)	(0.039)	(0.047)	(0.053)	(0.034)
Roommate - Colleges should prohibit racist/sexist speech	0.025	0.010	0.009	0.024	-0.016	-0.012	0.055	0.005	-0.002
	(0.035)	(0.045)	(0.037)	(0.039)	(0.048)	(0.038)	(0.038)	(0.046)	(0.030)
Roommate - Keep affirmative action in college admissions	0.003	0.045	0.030	0.052	0.104*	0.078*	-0.008	-0.005	0.001
	(0.046)	(0.051)	(0.041)	(0.045)	(0.049)	(0.038)	(0.048)	(0.052)	(0.034)
Roommate - Let undocumented immigrants access public education	-0.010	-0.020	-0.014	0.012	-0.013	-0.004	0.054	0.048	0.030
	(0.043)	(0.048)	(0.039)	(0.044)	(0.049)	(0.039)	(0.048)	(0.052)	(0.033)

Table B.2.4 (Continued)

Roommate - Courts not overconcerned with rights of criminals	0.036	0.051	0.045	0.021	0.027	0.020	0.021	0.014	0.008
	(0.047)	(0.058)	(0.048)	(0.048)	(0.054)	(0.043)	(0.049)	(0.060)	(0.038)
Roommate - Abolish the death penalty	0.006	-0.001	-0.001	-0.014	-0.043	-0.035	-0.043	-0.085	-0.055
	(0.040)	(0.045)	(0.037)	(0.042)	(0.047)	(0.037)	(0.044)	(0.047)	(0.031)
Roommate - National health care plan needed	0.013	-0.014	-0.007	-0.014	-0.045	-0.029	-0.114*	-0.111	-0.064
	(0.043)	(0.053)	(0.044)	(0.048)	(0.057)	(0.045)	(0.051)	(0.061)	(0.039)
Roommate - Legally recognize same-sex marriage	-0.021	-0.013	-0.011	-0.005	-0.005	-0.007	-0.002	0.000	0.000
	(0.039)	(0.043)	(0.035)	(0.038)	(0.041)	(0.032)	(0.042)	(0.046)	(0.030)

Table B.2.4 (Continued)

Roommate - Family income < $50,000	0.065	0.056	-0.008	-0.001	0.006	-0.005
	(0.086)	(0.071)	(0.086)	(0.069)	(0.094)	(0.061)
Roommate - Family income $50,000-$74,999	0.002	0.007	0.037	0.021	0.050	0.012
	(0.080)	(0.066)	(0.084)	(0.067)	(0.092)	(0.059)
Roommate - Family income $150,000-$199,999	-0.143	-0.109	-0.090	-0.069	-0.104	-0.070
	(0.096)	(0.079)	(0.091)	(0.073)	(0.099)	(0.067)
Roommate - Family income >= $200,000	-0.059	-0.047	-0.091	-0.068	-0.164	-0.100
	(0.079)	(0.065)	(0.082)	(0.065)	(0.084)	(0.058)
Years since freshman year	0.026	0.020	0.017	0.012	-0.187	-0.112
	(0.266)	(0.232)	(0.373)	(0.305)	(0.404)	(0.296)

Table B.2.4 (Continued)

Racial discrimination a major problem in the US	0.109*	0.079*	0.130*	0.088*	0.189**	0.108**
	(0.050)	(0.040)	(0.052)	(0.040)	(0.058)	(0.039)
The wealthy should pay higher taxes	0.158***	0.115**	0.097*	0.072	0.028	0.024
	(0.045)	(0.035)	(0.048)	(0.037)	(0.050)	(0.033)
Colleges should prohibit racist/sexist speech	0.034	0.021	0.019	0.006	-0.056	-0.032
	(0.043)	(0.034)	(0.047)	(0.035)	(0.048)	(0.031)
Keep affirmative action in college admissions	0.580***	0.439***	0.542***	0.405***	0.315***	0.188***
	(0.051)	(0.038)	(0.049)	(0.036)	(0.050)	(0.032)
Share with dependent variable equaling 1	0.203	0.203	0.148	0.148	0.027	0.027

Table B.2.4 (Continued)

Share with dependent variable equaling 4		0.151	0.151		0.173	0.173		0.520	0.520
R-squared/pseudo-R-squared	0.051	0.113	0.257	0.048	0.110	0.248	0.056	0.088	0.164
Number of observations	1953	1953	1953	1950	1950	1950	1964	1964	1964
p-value[H_0: coefficients on all roommate attitude regressors equal zero]	.	0.796	0.807	.	0.575	0.645	.	0.470	0.508

Table B.2.5: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college*

	Immigration is a good thing for the US		
	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.009	-0.003	-0.005
	(0.084)	(0.093)	(0.063)
Roommate - Other minority	-0.070	-0.092	-0.053
	(0.069)	(0.072)	(0.049)
Roommate - Racial discrimination a major problem in the US	-0.099*	-0.098	-0.065
	(0.046)	(0.057)	(0.039)
Roommate - The wealthy should pay higher taxes	0.113*	0.111*	0.071*
	(0.046)	(0.050)	(0.034)
Roommate - Colleges should prohibit racist/sexist speech	-0.014	-0.025	-0.016
	(0.037)	(0.043)	(0.030)
Roommate - Keep affirmative action in college admissions	0.009	-0.011	-0.005
	(0.046)	(0.052)	(0.036)
Roommate - Let undocumented immigrants access public education	0.045	0.074	0.048

Table B.2.5 (Continued)

	(0.047)	(0.049)	(0.034)
Roommate - Courts not overconcerned with rights of criminals	0.087	0.035	0.022
	(0.050)	(0.062)	(0.043)
Roommate - Abolish the death penalty	-0.052	-0.064	-0.044
	(0.044)	(0.048)	(0.033)
Roommate - National health care plan needed	-0.093*	-0.139*	-0.086*
	(0.046)	(0.054)	(0.036)
Roommate - Legally recognize same-sex marriage	-0.000	-0.006	-0.003
	(0.038)	(0.041)	(0.028)
Roommate - Family income < \$50,000		-0.163	-0.104
		(0.091)	(0.062)
Roommate - Family income \$50,000-\$74,999		-0.128	-0.080
		(0.084)	(0.057)
Roommate - Family income \$150,000-\$199,999		-0.157	-0.098
		(0.091)	(0.062)

Table B.2.5 (Continued)

Roommate - Family income >= $200,000		-0.047	-0.033
		(0.081)	(0.055)
Years since freshman year		0.108	0.075
		(0.380)	(0.252)
Racial discrimination a major problem in the US		0.153*	0.098*
		(0.061)	(0.043)
The wealthy should pay higher taxes		0.060	0.035
		(0.046)	(0.032)
Colleges should prohibit racist/sexist speech		-0.078	-0.051
		(0.044)	(0.030)
Keep affirmative action in college admissions		0.194***	0.127***
		(0.051)	(0.035)
Share with dependent variable equaling 1		0.038	0.038
Share with dependent variable equaling 4		0.262	0.262
R-squared/pseudo-R-squared	0.052	0.082	0.168
Number of observations	1945	1945	1945

Table B.2.5 (Continued)

p-value[H_0: coefficients on all roommate attitude regressors equal zero]	.	0.077	0.114

Table B.2.6: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college*

	Religion should have less national influence			Legally recognize same-sex marriage		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.032	-0.020	-0.024	-0.099	-0.081	-0.071
	(0.081)	(0.088)	(0.082)	(0.091)	(0.099)	(0.090)
Roommate - Other minority	-0.047	-0.077	-0.069	0.025	-0.019	-0.010
	(0.067)	(0.070)	(0.065)	(0.072)	(0.075)	(0.068)
Roommate - Racial discrimination a major problem in the US	-0.025	0.018	0.021	-0.074	-0.153*	-0.145**
	(0.046)	(0.057)	(0.052)	(0.050)	(0.063)	(0.055)
Roommate - The wealthy should pay higher taxes	0.149***	0.136**	0.116*	0.059	0.007	0.008
	(0.044)	(0.052)	(0.047)	(0.048)	(0.058)	(0.050)
Roommate - Colleges should prohibit racist/sexist speech	-0.007	-0.030	-0.028	0.018	-0.061	-0.056

Table B.2.6 (Continued)

	(0.035)	(0.043)	(0.039)	(0.039)	(0.048)	(0.042)
Roommate - Keep affirmative action in college admissions	-0.029	-0.048	-0.040	0.053	0.033	0.042
	(0.044)	(0.049)	(0.045)	(0.047)	(0.053)	(0.047)
Roommate - Let undocumented immigrants access public education	0.013	0.050	0.043	0.040	0.058	0.058
	(0.043)	(0.047)	(0.043)	(0.048)	(0.053)	(0.047)
Roommate - Courts not overconcerned with rights of criminals	0.049	0.003	0.005	0.043	-0.040	-0.037
	(0.052)	(0.062)	(0.056)	(0.052)	(0.064)	(0.055)
Roommate - Abolish the death penalty	-0.065	-0.087	-0.075	-0.048	-0.074	-0.061
	(0.043)	(0.046)	(0.042)	(0.044)	(0.048)	(0.043)
Roommate - National health care plan needed	-0.046	0.013	0.002	-0.016	0.020	0.016
	(0.046)	(0.057)	(0.052)	(0.049)	(0.062)	(0.054)

Table B.2.6 (Continued)

Roommate - Legally recognize same-sex marriage	-0.050	-0.058	-0.047	-0.060	-0.077	-0.060
	(0.039)	(0.042)	(0.038)	(0.042)	(0.047)	(0.041)
Roommate - Family income < \$50,000		-0.147	-0.127		-0.039	-0.049
		(0.088)	(0.081)		(0.096)	(0.086)
Roommate - Family income \$50,000-\$74,999		-0.077	-0.061		-0.090	-0.077
		(0.084)	(0.078)		(0.089)	(0.080)
Roommate - Family income \$150,000-\$199,999		-0.032	-0.023		-0.100	-0.060
		(0.091)	(0.084)		(0.100)	(0.089)
Roommate - Family income >= \$200,000		-0.024	-0.027		0.074	0.071
		(0.079)	(0.072)		(0.081)	(0.072)
Years since freshman year		0.288	0.226		0.743	0.407
		(0.462)	(0.418)		(0.406)	(0.217)

Table B.2.6 (Continued)

Racial discrimination a major problem in the US		0.263***	0.231***		0.352***	0.301***
		(0.058)	(0.051)		(0.060)	(0.054)
The wealthy should pay higher taxes		0.226***	0.196***		0.261***	0.206***
		(0.048)	(0.042)		(0.052)	(0.044)
Colleges should prohibit racist/sexist speech		-0.154***	-0.130**		-0.180***	-0.140***
		(0.046)	(0.041)		(0.047)	(0.040)
Keep affirmative action in college admissions		-0.174***	-0.148***		-0.112*	-0.093*
		(0.045)	(0.041)		(0.052)	(0.044)
Share with dependent variable equaling 1		0.187	0.187		0.136	0.136
Share with dependent variable equaling 4		0.236	0.236		0.490	0.490

Table B.2.6 (Continued)

R-squared/pseudo-R-squared	0.041	0.078	0.190	0.046	0.089	0.188
Number of observations	1954	1954	1954	1962	1962	1962
p-value[H_0: coefficients on all roommate attitude regressors equal zero]	.	0.111	0.182	.	0.066	0.061

Table B.3.1.1: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [white students, with linear interactions with cohort]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Years since freshman year	0.141	0.210	0.172	-0.283	-0.274	-0.196	-0.103	0.074	0.043
	(0.380)	(0.348)	(0.285)	(0.567)	(0.580)	(0.456)	(0.541)	(0.540)	(0.381)
Roommate - Black	-0.116	-0.242	-0.167	-0.143	-0.174	-0.131	-0.425**	-0.458*	-0.319*
	(0.163)	(0.174)	(0.138)	(0.159)	(0.180)	(0.142)	(0.163)	(0.183)	(0.141)
Years since freshman year × Roommate - Black	0.017	0.034	0.025	0.130	0.159	0.115	0.239	0.285*	0.207*
	(0.136)	(0.141)	(0.111)	(0.129)	(0.137)	(0.108)	(0.134)	(0.141)	(0.104)

Table B.3.1.1 (Continued)

Roommate - Other minority	0.074	0.042	0.023	-0.073	-0.104	-0.078	0.003	-0.050	-0.034
	(0.137)	(0.143)	(0.117)	(0.143)	(0.150)	(0.117)	(0.140)	(0.148)	(0.109)
Years since freshman year × Roommate - Other minority	0.017	-0.018	-0.003	0.007	-0.009	-0.002	-0.038	-0.005	-0.014
	(0.114)	(0.116)	(0.093)	(0.124)	(0.125)	(0.098)	(0.118)	(0.125)	(0.090)
Roommate - Family income < \$50,000		-0.094	-0.061		-0.087	-0.037		0.122	0.087
		(0.168)	(0.135)		(0.163)	(0.129)		(0.168)	(0.119)
Years since freshman year × Roommate - Family income < \$50,000		0.068	0.045		0.040	0.012		-0.152	-0.114
		(0.133)	(0.106)		(0.136)	(0.106)		(0.138)	(0.097)

Table B.3.1.1 (Continued)

Roommate - Family income \$50,000-\$74,999	0.003	-0.002	-0.096	-0.065	0.162	0.075
	(0.161)	(0.128)	(0.169)	(0.131)	(0.171)	(0.123)
Years since freshman year × Roommate - Family income \$50,000-\$74,999	-0.053	-0.033	0.087	0.057	-0.048	-0.016
	(0.127)	(0.100)	(0.133)	(0.104)	(0.140)	(0.099)
Roommate - Family income \$150,000-\$199,999	-0.189	-0.139	-0.193	-0.135	-0.039	-0.029
	(0.175)	(0.136)	(0.171)	(0.134)	(0.199)	(0.147)

Table B.3.1.1 (Continued)

Years since freshman year × Roommate - Family income \$150,000-\$199,999		0.082	0.057		0.061	0.038		-0.012	-0.010
		(0.158)	(0.123)		(0.135)	(0.106)		(0.157)	(0.115)
Roommate - Family income >= \$200,000		0.110	0.063		0.079	0.057		-0.055	-0.054
		(0.164)	(0.129)		(0.169)	(0.131)		(0.167)	(0.128)
Years since freshman year × Roommate - Family income >= \$200,000		-0.192	-0.129		-0.169	-0.120		-0.015	0.010
		(0.144)	(0.113)		(0.139)	(0.108)		(0.141)	(0.106)
Share with dependent variable equaling 1	0.254	0.254	0.254	0.185	0.185	0.185	0.033	0.033	0.033

Table B.3.1.1 (Continued)

Share with dependent variable equaling 4	0.079	0.079	0.079	0.095	0.095	0.095	0.462	0.462	0.462
R-squared/pseudo-R-squared	0.061	0.110	0.237	0.057	0.105	0.227	0.058	0.089	0.176
Number of observations	1337	1337	1337	1337	1337	1337	1346	1346	1346

Table B.3.1.1 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.3.1.2: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [white students, with control for roommate CIRP attitude toward affirmative action]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.098	-0.200	-0.135	-0.005	-0.021	-0.021	-0.176	-0.152	-0.096
	(0.096)	(0.108)	(0.086)	(0.095)	(0.111)	(0.087)	(0.102)	(0.122)	(0.090)
Roommate - Other minority	0.089	0.024	0.019	-0.064	-0.128	-0.092	-0.027	-0.041	-0.038
	(0.087)	(0.089)	(0.071)	(0.088)	(0.093)	(0.072)	(0.093)	(0.098)	(0.071)
Roommate - Family income $<$ \$50,000		-0.028	-0.018		-0.042	-0.022		-0.021	-0.018
		(0.108)	(0.086)		(0.106)	(0.083)		(0.112)	(0.079)

Table B.3.1.2 (Continued)

Roommate - Family income \$50,000-\$74,999		-0.044	-0.031		-0.015	-0.013		0.107	0.055
		(0.100)	(0.080)		(0.106)	(0.082)		(0.109)	(0.075)
Roommate - Family income \$150,000-\$199,999		-0.102	-0.078		-0.130	-0.096		-0.052	-0.038
		(0.113)	(0.089)		(0.107)	(0.084)		(0.118)	(0.085)
Roommate - Family income >= \$200,000		-0.066	-0.054		-0.071	-0.050		-0.077	-0.050
		(0.101)	(0.080)		(0.100)	(0.077)		(0.102)	(0.074)
Years since freshman year		0.147	0.134		-0.277	-0.201		0.059	0.041
		(0.334)	(0.274)		(0.575)	(0.452)		(0.559)	(0.399)
Share with dependent variable equaling 1	0.254	0.254	0.254	0.185	0.185	0.185	0.033	0.033	0.033

Table B.3.1.2 (Continued)

Share with dependent variable equaling 4	0.079	0.079	0.079	0.095	0.095	0.095	0.462	0.462	0.462
R-squared/pseudo-R-squared	0.061	0.109	0.236	0.056	0.105	0.227	0.056	0.087	0.173
Number of observations	1337	1337	1337	1337	1337	1337	1346	1346	1346

Table B.3.1.2 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.3.1.3: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [subsample of white students with CIRP attitude toward affirmative action non-missing]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.142	-0.264*	-0.185	-0.016	-0.065	-0.047	-0.251*	-0.270	-0.161
	(0.114)	(0.133)	(0.105)	(0.118)	(0.137)	(0.105)	(0.120)	(0.142)	(0.098)
Roommate - Other minority	0.051	-0.025	-0.018	-0.074	-0.136	-0.096	-0.027	-0.054	-0.051
	(0.111)	(0.114)	(0.091)	(0.118)	(0.128)	(0.099)	(0.127)	(0.131)	(0.083)
Roommate - Family income < \$50,000		-0.045	-0.031		0.006	0.007		-0.122	-0.081
		(0.134)	(0.106)		(0.129)	(0.100)		(0.139)	(0.089)

Table B.3.1.3 (Continued)

Roommate - Family income \$50,000-\$74,999		-0.006	-0.001		-0.019	-0.016		0.138	0.057
		(0.126)	(0.100)		(0.130)	(0.101)		(0.142)	(0.089)
Roommate - Family income \$150,000-\$199,999		-0.108	-0.077		-0.082	-0.053		-0.029	-0.025
		(0.142)	(0.112)		(0.134)	(0.104)		(0.150)	(0.097)
Roommate - Family income >= \$200,000		-0.249	-0.177		-0.203	-0.134		-0.200	-0.131
		(0.138)	(0.108)		(0.128)	(0.099)		(0.130)	(0.088)
Years since freshman year		-0.060	-0.032		-0.578	-0.419		-0.368	-0.220
		(0.413)	(0.329)		(0.693)	(0.527)		(0.632)	(0.463)
Share with dependent variable equaling 1	0.219	0.219	0.219	0.149	0.149	0.149	0.022	0.022	0.022

Table B.3.1.3 (Continued)

Share with dependent variable equaling 4	0.076	0.076	0.076	0.096	0.096	0.096	0.500	0.500	0.500
R-squared/pseudo-R-squared	0.051	0.097	0.211	0.039	0.087	0.188	0.056	0.103	0.186
Number of observations	811	811	811	812	812	812	816	816	816

Table B.3.1.3 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.3.1.4: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [subsample of white students with non-missing CIRP attitude toward affirmative action]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	0.006	-0.029	-0.009	0.028	0.168	0.095	0.026	0.047	0.024
	(0.187)	(0.203)	(0.159)	(0.153)	(0.197)	(0.158)	(0.202)	(0.247)	(0.196)
Roommate - Other minority	0.136	0.127	0.083	-0.049	-0.043	-0.037	-0.024	-0.048	-0.029
	(0.139)	(0.150)	(0.120)	(0.132)	(0.141)	(0.111)	(0.136)	(0.152)	(0.123)
Roommate - Family income < \$50,000		-0.034	0.001		-0.179	-0.091		0.114	0.081
		(0.190)	(0.159)		(0.188)	(0.149)		(0.198)	(0.158)

Table B.3.1.4 (Continued)

Roommate - Family income \$50,000-\$74,999		-0.179	-0.115		-0.105	-0.057		0.035	0.025
		(0.172)	(0.136)		(0.184)	(0.144)		(0.176)	(0.140)
Roommate - Family income \$150,000-\$199,999		-0.025	-0.030		-0.169	-0.132		0.005	0.013
		(0.188)	(0.147)		(0.181)	(0.142)		(0.192)	(0.156)
Roommate - Family income >= \$200,000		0.215	0.139		0.082	0.050		0.208	0.146
		(0.155)	(0.122)		(0.164)	(0.129)		(0.170)	(0.134)
Years since freshman year		1.010***	0.807***		0.572*	0.508*		4.995***	0.815***
		(0.265)	(0.213)		(0.246)	(0.198)		(0.359)	(0.231)
Share with dependent variable equaling 1	0.308	0.308	0.308	0.242	0.242	0.242	0.051	0.051	0.051

Table B.3.1.4 (Continued)

Share with dependent variable equaling 4	0.084	0.084	0.084	0.093	0.093	0.093	0.404	0.404	0.404
R-squared/pseudo-R-squared	0.069	0.153	0.318	0.070	0.145	0.302	0.041	0.099	0.208
Number of observations	526	526	526	525	525	525	530	530	530

Table B.3.1.4 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.3.1.5: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only white women]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.230	-0.316	-0.186	-0.079	-0.099	-0.069	-0.155	-0.152	-0.066
	(0.164)	(0.189)	(0.139)	(0.169)	(0.196)	(0.139)	(0.168)	(0.205)	(0.138)
Roommate - Other minority	0.103	-0.109	-0.057	-0.115	-0.276	-0.165	0.048	-0.056	-0.050
	(0.148)	(0.150)	(0.105)	(0.143)	(0.154)	(0.107)	(0.157)	(0.174)	(0.114)
Roommate - Family income < \$50,000		0.003	0.006		0.043	0.027		-0.026	-0.020
		(0.175)	(0.126)		(0.185)	(0.128)		(0.182)	(0.121)

Table B.3.1.5 (Continued)

Roommate - Family income $50,000-$74,999		-0.487**	-0.269*		-0.240	-0.142		-0.133	-0.083
		(0.166)	(0.118)		(0.169)	(0.120)		(0.172)	(0.117)
Roommate - Family income $150,000-$199,999		-0.181	-0.118		-0.154	-0.096		0.059	0.015
		(0.203)	(0.148)		(0.185)	(0.131)		(0.201)	(0.130)
Roommate - Family income >= $200,000		-0.297	-0.181		-0.109	-0.061		-0.137	-0.081
		(0.173)	(0.124)		(0.176)	(0.123)		(0.188)	(0.129)
Years since freshman year		-5.782***	-0.945**		-5.407***	-0.709*		0.339	0.200
		(0.355)	(0.303)		(0.463)	(0.307)		(.)	(0.325)
Share with dependent variable equaling 1	0.250	0.250	0.250	0.188	0.188	0.188	0.030	0.030	0.030

Table B.3.1.5 (Continued)

Share with dependent variable equaling 4	0.073	0.073	0.073	0.083	0.083	0.083	0.471	0.471	0.471
R-squared/pseudo-R-squared	0.104	0.230	0.433	0.112	0.232	0.436	0.125	0.205	0.350
Number of observations	593	593	593	591	591	591	596	596	596

Table B.3.1.5 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.3.1.6: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only white men]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.394	-0.515*	-0.273	-0.213	-0.238	-0.119	-0.261	-0.399	-0.207
	(0.208)	(0.233)	(0.160)	(0.213)	(0.242)	(0.159)	(0.204)	(0.262)	(0.166)
Roommate - Other minority	-0.117	-0.297	-0.154	-0.307	-0.435*	-0.217	-0.017	-0.171	-0.089
	(0.185)	(0.193)	(0.130)	(0.193)	(0.207)	(0.134)	(0.206)	(0.223)	(0.134)
Roommate - Family income < $50,000		0.173	0.095		0.369	0.196		0.179	0.065
		(0.229)	(0.156)		(0.230)	(0.149)		(0.231)	(0.137)

Table B.3.1.6 (Continued)

Roommate - Family income $50,000-$74,999		-0.425	-0.226		-0.123	-0.068		-0.118	-0.042
		(0.225)	(0.153)		(0.235)	(0.156)		(0.237)	(0.163)
Roommate - Family income $150,000-$199,999		0.321	0.157		0.436	0.227		0.375	0.151
		(0.268)	(0.187)		(0.237)	(0.156)		(0.275)	(0.163)
Roommate - Family income >= $200,000		-0.303	-0.168		-0.049	-0.021		-0.008	0.009
		(0.232)	(0.158)		(0.222)	(0.146)		(0.240)	(0.149)
Years since freshman year		-6.059***	-0.021		-5.334***	-0.434		-1.351	-0.527**
		(0.662)	(0.162)		(0.667)	(0.238)		(0.791)	(0.189)
Share with dependent variable equaling 1	0.121	0.121	0.121	0.096	0.096	0.096	0.020	0.020	0.020

Table B.3.1.6 (Continued)

Share with dependent variable equaling 4	0.070	0.070	0.070	0.083	0.083	0.083	0.521	0.521	0.521
R-squared/pseudo-R-squared	0.146	0.223	0.396	0.145	0.238	0.419	0.173	0.247	0.377
Number of observations	398	398	398	397	397	397	401	401	401

Table B.3.1.6 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.3.1.7: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only black students]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	0.197	0.594	0.264	0.252	0.594	0.248	-0.196	-0.827	-0.134
	(0.409)	(0.418)	(0.244)	(0.317)	(0.426)	(0.218)	(0.428)	(0.544)	(0.168)
Roommate - Other minority	-0.144	-0.120	-0.061	0.322	0.641*	0.237	-0.290	-0.490	-0.176
	(0.233)	(0.280)	(0.179)	(0.233)	(0.301)	(0.152)	(0.269)	(0.369)	(0.149)
Roommate - Family income < \$50,000		-0.175	-0.075		-0.221	-0.102		0.184	0.004
		(0.353)	(0.217)		(0.314)	(0.173)		(0.490)	(0.148)

Table B.3.1.7 (Continued)

Roommate - Family income \$50,000-\$74,999		0.091	0.041		0.206	0.090		-0.315	-0.098
		(0.350)	(0.227)		(0.392)	(0.224)		(0.427)	(0.165)
Roommate - Family income \$150,000-\$199,999		0.407	0.194		1.583***	0.481*		0.538	0.092
		(0.493)	(0.314)		(0.435)	(0.187)		(0.756)	(0.205)
Roommate - Family income >= \$200,000		0.128	0.058		0.771*	0.292		0.201	0.079
		(0.298)	(0.183)		(0.339)	(0.178)		(0.417)	(0.153)
Years since freshman year		-0.855	0.084		-0.415	0.015		-0.653	-0.308
		(1.023)	(0.465)		(1.405)	(0.251)		(.)	(0.198)
Share with dependent variable equaling 1	0.020	0.020	0.020	0.004	0.004	0.004	0.012	0.012	0.012

Table B.3.1.7 (Continued)

Share with dependent variable equaling 4	0.484	0.484	0.484	0.538	0.538	0.538	0.717	0.717	0.717
R-squared/pseudo-R-squared	0.220	0.293	0.452	0.231	0.347	0.504	0.374	0.478	0.581
Number of observations	252	252	252	249	249	249	251	251	251

Table B.3.1.7 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.3.1.8: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only nonblack-minority students]*

	Keep affirmative action in college admissions			Ensuring campus diversity justifies affirmative action			Campus diversity improves higher education		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	0.307	0.488	0.319	0.182	0.106	0.062	0.254	0.125	0.099
	(0.197)	(0.253)	(0.201)	(0.173)	(0.250)	(0.182)	(0.210)	(0.294)	(0.154)
Roommate - Other minority	-0.220	0.089	0.059	-0.118	0.084	0.056	-0.206	-0.003	0.008
	(0.222)	(0.239)	(0.199)	(0.219)	(0.241)	(0.185)	(0.212)	(0.221)	(0.123)
Roommate - Family income < \$50,000		0.207	0.146		0.466	0.288		0.424	0.175
		(0.281)	(0.230)		(0.284)	(0.207)		(0.329)	(0.182)

Table B.3.1.8 (Continued)

Roommate - Family income \$50,000-\$74,999		-0.222	-0.143		0.401	0.256		0.185	0.064
		(0.275)	(0.223)		(0.285)	(0.208)		(0.289)	(0.154)
Roommate - Family income \$150,000-\$199,999		-0.296	-0.219		-0.058	-0.031		0.148	0.030
		(0.292)	(0.247)		(0.287)	(0.212)		(0.303)	(0.155)
Roommate - Family income >= \$200,000		0.070	0.055		-0.325	-0.168		-0.597*	-0.245
		(0.211)	(0.171)		(0.221)	(0.161)		(0.263)	(0.148)
Years since freshman year		-0.193	-0.140		1.485**	0.960*		-0.283	-0.065
		(0.583)	(0.483)		(0.509)	(0.371)		(0.510)	(0.403)
Share with dependent variable equaling 1	0.141	0.141	0.141	0.110	0.110	0.110	0.017	0.017	0.017

Table B.3.1.8 (Continued)

Share with dependent variable equaling 4	0.183	0.183	0.183	0.206	0.206	0.206	0.597	0.597	0.597
R-squared/pseudo-R-squared	0.133	0.217	0.426	0.163	0.277	0.501	0.191	0.294	0.390
Number of observations	355	355	355	355	355	355	357	357	357

Table B.3.1.8 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondent's: father's education, mother's education, family income, high-school grade point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich and prohibition of racist/sexist speech. For roommates': average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.1: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [white students, with linear interactions with cohort]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Years since freshman year	-0.015	0.087	0.057	-0.085	0.401	0.080
	(0.445)	(0.496)	(0.242)	(0.095)	(5.347)	(0.099)
Roommate - Black	-0.081	-0.006	0.028	0.014	7.874***	0.053
	(0.202)	(0.215)	(0.107)	(0.031)	(2.033)	(0.033)
Years since freshman year × Roommate - Black	-0.115	-0.114	-0.075	-0.006	-4.762**	-0.010
	(0.149)	(0.157)	(0.080)	(0.022)	(1.501)	(0.024)
Roommate - Other minority	0.288	0.307	0.153*	0.006	0.135	-0.025
	(0.170)	(0.177)	(0.077)	(0.023)	(1.660)	(0.027)
Years since freshman year × Roommate - Other minority	-0.206	-0.217	-0.108	-0.009	0.183	0.006

Table B.4.1.1 (Continued)

	(0.141)	(0.144)	(0.069)	(0.019)	(1.372)	(0.023)
Roommate - Family income < $50,000		-0.165	-0.032	-0.025	0.207	0.014
		(0.212)	(0.105)	(0.028)	(1.915)	(0.033)
Years since freshman year × Roommate - Family income < $50,000		0.038	-0.016	0.021	-1.168	-0.017
		(0.169)	(0.088)	(0.020)	(1.534)	(0.026)
Roommate - Family income $50,000-$74,999		0.248	0.136	0.012	-0.278	-0.016
		(0.199)	(0.089)	(0.029)	(1.754)	(0.032)
Years since freshman year × Roommate - Family income $50,000-$74,999		-0.222	-0.124	-0.006	-0.000	0.005
		(0.155)	(0.073)	(0.022)	(1.387)	(0.025)
Roommate - Family income $150,000-$199,999		0.044	0.021	0.044	0.643	0.030
		(0.225)	(0.121)	(0.030)	(1.835)	(0.033)

Table B.4.1.1 (Continued)

Years since freshman year × Roommate - Family income \$150,000-\$199,999		-0.282	-0.133	-0.026	-1.970	0.002
		(0.184)	(0.102)	(0.024)	(1.545)	(0.025)
Roommate - Family income >= \$200,000		0.143	0.065	-0.020	-0.920	0.030
		(0.202)	(0.097)	(0.029)	(1.745)	(0.027)
Years since freshman year × Roommate - Family income >= \$200,000		-0.095	-0.047	0.006	1.089	-0.019
		(0.162)	(0.079)	(0.024)	(1.428)	(0.023)
Share with dependent variable equaling 1	0.017	0.017	0.017			
Share with dependent variable equaling 4	0.633	0.633	0.633			
R-squared/pseudo-R-squared	0.086	0.107	0.142	0.176	0.179	0.234

Table B.4.1.1 (Continued)

Number of observations	1350	1350	1350	1351	1338	1341

Table B.4.1.1 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.2: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [white students, with control for roommate CIRP attitude toward affirmative action]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	-0.200	-0.143	-0.062	0.004	2.448	0.041*
	(0.112)	(0.129)	(0.067)	(0.017)	(1.263)	(0.020)
Roommate - Other minority	0.094	0.094	0.053	-0.005	0.119	-0.020
	(0.101)	(0.103)	(0.048)	(0.015)	(1.026)	(0.018)
Roommate - Family income < $50,000		-0.132	-0.053	-0.005	-1.092	-0.003
		(0.127)	(0.063)	(0.018)	(1.262)	(0.022)
Roommate - Family income $50,000-$74,999		0.037	0.024	0.008	-0.280	-0.014
		(0.120)	(0.058)	(0.018)	(1.106)	(0.019)
Roommate - Family income $150,000-$199,999		-0.242*	-0.111	0.018	-1.390	0.030
		(0.122)	(0.065)	(0.017)	(1.173)	(0.021)

Table B.4.1.2 (Continued)

Roommate - Family income >= $200,000		0.053	0.021	-0.014	0.147	0.013
		(0.115)	(0.056)	(0.017)	(1.049)	(0.018)
Years since freshman year		-0.027	-0.005	-0.079	-0.452	0.069
		(0.497)	(0.250)	(0.093)	(4.858)	(0.103)
Share with dependent variable equaling 1	0.017	0.017	0.017			
Share with dependent variable equaling 4	0.633	0.633	0.633			
R-squared/pseudo-R-squared	0.085	0.104	0.137	0.176	0.169	0.236
Number of observations	1350	1350	1350	1351	1338	1341

Table B.4.1.2 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.3: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [subsample of white students with CIRP attitude toward affirmative action non-missing]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	-0.234	-0.157	-0.071	0.005	0.608	0.056*
	(0.132)	(0.158)	(0.083)	(0.022)	(1.503)	(0.024)
Roommate - Other minority	0.051	0.034	0.035	-0.004	-0.358	-0.009
	(0.131)	(0.139)	(0.069)	(0.021)	(1.415)	(0.024)
Roommate - Family income < $50,000		-0.102	-0.040	-0.005	-0.502	-0.002
		(0.152)	(0.074)	(0.022)	(1.567)	(0.027)
Roommate - Family income $50,000-$74,999		-0.013	-0.010	0.001	-1.249	-0.004
		(0.150)	(0.075)	(0.023)	(1.347)	(0.025)
Roommate - Family income $150,000-$199,999		-0.152	-0.060	-0.000	-1.134	0.048
		(0.162)	(0.086)	(0.022)	(1.519)	(0.027)

Table B.4.1.3 (Continued)

Roommate - Family income >= $200,000		0.152	0.062	-0.022	-0.238	0.012
		(0.156)	(0.075)	(0.024)	(1.406)	(0.025)
Years since freshman year		-0.401	-0.144	-0.019	-4.844	0.078
		(0.564)	(0.295)	(0.102)	(4.727)	(0.113)
Share with dependent variable equaling 1	0.016	0.016	0.016			
Share with dependent variable equaling 4	0.626	0.626	0.626			
R-squared/pseudo-R-squared	0.080	0.110	0.138	0.157	0.178	0.199
Number of observations	818	818	818	819	810	813

Table B.4.1.3 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.4: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [subsample of white students with non-missing CIRP attitude toward affirmative action]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	-0.112	-0.068	0.002	0.016	5.423*	0.014
	(0.206)	(0.234)	(0.119)	(0.033)	(2.373)	(0.039)
Roommate - Other minority	0.155	0.176	0.083	-0.003	0.372	-0.033
	(0.158)	(0.166)	(0.073)	(0.023)	(1.491)	(0.028)
Roommate - Family income < $50,000		-0.178	-0.059	-0.000	-2.278	-0.001
		(0.252)	(0.130)	(0.033)	(2.120)	(0.037)
Roommate - Family income $50,000-$74,999		0.172	0.102	0.030	1.340	-0.029
		(0.209)	(0.094)	(0.030)	(1.883)	(0.032)
Roommate - Family income $150,000-$199,999		-0.342	-0.156	0.047	-1.939	-0.002
		(0.202)	(0.107)	(0.027)	(1.870)	(0.033)

Table B.4.1.4 (Continued)

Roommate - Family income >= $200,000		-0.074	-0.024	-0.010	1.401	0.009
		(0.189)	(0.095)	(0.024)	(1.607)	(0.029)
Years since freshman year		5.111***	0.469**	-0.371***	12.162***	0.044
		(0.389)	(0.147)	(0.046)	(3.276)	(0.061)
Share with dependent variable equaling 1	0.019	0.019	0.019			
Share with dependent variable equaling 4	0.643	0.643	0.643			
R-squared/pseudo-R-squared	0.092	0.145	0.188	0.255	0.216	0.318
Number of observations	532	532	532	532	528	528

Table B.4.1.4 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.5: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [only white women]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	-0.220	-0.128	-0.055	0.022	1.661	0.034
	(0.186)	(0.221)	(0.113)	(0.030)	(2.104)	(0.035)
Roommate - Other minority	0.152	0.137	0.039	0.017	2.435	0.001
	(0.157)	(0.170)	(0.073)	(0.027)	(1.722)	(0.033)
Roommate - Family income $< \$50,000$		-0.340	-0.119	0.009	-0.686	0.016
		(0.202)	(0.098)	(0.026)	(2.022)	(0.036)
Roommate - Family income \$50,000-\$74,999		-0.110	-0.031	0.011	1.075	-0.028
		(0.199)	(0.088)	(0.027)	(1.755)	(0.032)
Roommate - Family income \$150,000-\$199,999		-0.206	-0.078	-0.012	0.152	0.037
		(0.211)	(0.098)	(0.033)	(2.077)	(0.038)

Table B.4.1.5 (Continued)

Roommate - Family income >= $200,000		-0.249	-0.088	0.001	-0.180	0.001
		(0.201)	(0.101)	(0.029)	(1.821)	(0.033)
Years since freshman year		0.739	0.263	-0.590***	-14.166**	-0.634***
		(2.811)	(0.221)	(0.073)	(4.545)	(0.088)
Share with dependent variable equaling 1	0.013	0.013	0.013			
Share with dependent variable equaling 4	0.637	0.637	0.637			
R-squared/pseudo-R-squared	0.186	0.221	0.255	0.245	0.255	0.306
Number of observations	597	597	597	597	592	595

Table B.4.1.5 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.6: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [only white men]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	0.010	0.169	0.048	0.019	2.498	0.058
	(0.232)	(0.276)	(0.126)	(0.037)	(2.514)	(0.044)
Roommate - Other minority	0.248	0.151	0.059	0.005	4.089	-0.013
	(0.208)	(0.225)	(0.090)	(0.034)	(2.198)	(0.044)
Roommate - Family income < $50,000		-0.392	-0.164	-0.003	-0.475	0.020
		(0.266)	(0.124)	(0.035)	(2.623)	(0.051)
Roommate - Family income $50,000-$74,999		-0.410	-0.133	0.009	0.642	-0.026
		(0.273)	(0.131)	(0.035)	(2.312)	(0.047)
Roommate - Family income $150,000-$199,999		-0.070	-0.012	-0.005	2.895	0.073
		(0.300)	(0.128)	(0.042)	(2.655)	(0.051)

Table B.4.1.6 (Continued)

Roommate - Family income >= $200,000		-0.481	-0.152	-0.010	4.070	0.004
		(0.266)	(0.130)	(0.034)	(2.472)	(0.046)
Years since freshman year		0.523	-0.599**			
		(0.606)	(0.199)			
Share with dependent variable equaling 1	0.012	0.012	0.012			
Share with dependent variable equaling 4	0.647	0.647	0.647			
R-squared/pseudo-R-squared	0.250	0.300	0.341	0.296	0.362	0.362
Number of observations	402	402	402	402	399	400

Table B.4.1.6 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.7: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [only black students]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	0.094	0.055	0.059	0.021	0.215	-0.097
	(0.349)	(0.462)	(0.213)	(0.095)	(3.559)	(0.099)
Roommate - Other minority	-0.060	0.029	0.047	-0.051	-0.833	0.040
	(0.247)	(0.302)	(0.151)	(0.064)	(1.801)	(0.050)
Roommate - Family income < $50,000		0.667	0.134	-0.065	0.245	0.072
		(0.418)	(0.187)	(0.075)	(2.475)	(0.064)
Roommate - Family income $50,000-$74,999		-0.175	0.032	-0.054	0.611	0.095
		(0.371)	(0.163)	(0.089)	(2.463)	(0.064)
Roommate - Family income $150,000-$199,999		1.005	0.274	0.001	2.156	-0.165
		(0.581)	(0.229)	(0.097)	(2.968)	(0.088)

Table B.4.1.7 (Continued)

Roommate - Family income >= $200,000		-0.080	-0.039	0.034	-0.714	-0.035
		(0.381)	(0.203)	(0.071)	(1.816)	(0.076)
Years since freshman year		7.582***	-0.929**			
		(0.411)	(0.337)			
Share with dependent variable equaling 1	0.012	0.012	0.012			
Share with dependent variable equaling 4	0.625	0.625	0.625			
R-squared/pseudo-R-squared	0.266	0.391	0.439	0.575	0.581	0.465
Number of observations	251	251	251	252	251	250

Table B.4.1.7 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.4.1.8: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes and behaviors of students one to three years after entering college [only nonblack-minority students]*

	Comfort in interacting with other racial/ethnic groups			Percentage of close friends from own racial group	Frequency of interactions with someone black during past semester	Percentage of close friends from own socioeconomic class
	Ordered probit without controls	Ordered probit with controls	OLS			
Roommate - Black	-0.035	-0.012	0.005	-0.114	5.296*	0.078
	(0.248)	(0.339)	(0.137)	(0.068)	(2.562)	(0.061)
Roommate - Other minority	-0.305	-0.214	-0.105	0.028	-0.421	0.039
	(0.238)	(0.293)	(0.137)	(0.061)	(2.662)	(0.053)
Roommate - Family income < $50,000		0.031	0.045	-0.072	-2.823	-0.039
		(0.410)	(0.182)	(0.082)	(2.621)	(0.063)
Roommate - Family income $50,000-$74,999		-0.401	-0.170	-0.063	-0.284	0.022
		(0.300)	(0.134)	(0.067)	(2.727)	(0.058)
Roommate - Family income $150,000-$199,999		-0.068	-0.071	0.018	0.564	0.037
		(0.324)	(0.131)	(0.076)	(2.864)	(0.061)

Table B.4.1.8 (Continued)

Roommate - Family income >= $200,000		-0.508	-0.188	0.021	2.354	-0.049
		(0.265)	(0.129)	(0.068)	(2.379)	(0.053)
Years since freshman year		-6.543***	-1.012***	0.490***	10.501	0.328
		(0.727)	(0.217)	(0.129)	(7.842)	(0.204)
Share with dependent variable equaling 1	0.014	0.014	0.014			
Share with dependent variable equaling 4	0.717	0.717	0.717			
R-squared/pseudo-R-squared	0.213	0.300	0.385	0.431	0.490	0.344
Number of observations	357	357	357	356	353	357

Table B.4.1.8 (Continued)

"Standard errors are adjusted for room clustering using Huber-White robust estimations. All regressions include controls for respondents: father's education, mother's education, family income, high-school grade-point average, ACT/SAT score, CIRP-based attitudes about race discrimination, taxation of the rich, and prohibition of racist/sexist speech. For roommates: average father's education, average mother's education, average high school grade-point average, average ACT/SAT score. All regressions also control for respondent housing preferences, gender, cohort, test taken; values not shown." (Boisjoly et al., 2006). In the basic specification, years since freshman year is that cohort control. Following Boisjoly et al. (2006), missing-variable dummies for regressors except roommate race/ethnicity are also included, reducing regression-sample attrition from missing values.

Table B.5.1: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [white students, with linear interactions with cohort]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Years since freshman year	0.262	0.193	0.145	-0.394	-0.451	-0.388
	(0.402)	(0.358)	(0.285)	(0.493)	(0.466)	(0.386)
Roommate - Black	-0.338	-0.247	-0.152	-0.361*	-0.270	-0.234
	(0.178)	(0.198)	(0.167)	(0.179)	(0.196)	(0.184)
Years since freshman year × Roommate - Black	-0.065	-0.095	-0.104	-0.080	-0.132	-0.111
	(0.144)	(0.150)	(0.125)	(0.143)	(0.150)	(0.139)
Roommate - Other minority	-0.251	-0.183	-0.149	-0.194	-0.108	-0.096
	(0.152)	(0.159)	(0.129)	(0.133)	(0.144)	(0.137)
Years since freshman year × Roommate - Other minority	0.088	0.007	-0.001	-0.022	-0.083	-0.064
	(0.129)	(0.133)	(0.109)	(0.129)	(0.135)	(0.122)

Table B.5.1 (Continued)

Roommate - Family income $<$ \$50,000		-0.180	-0.117		-0.294	-0.271
		(0.180)	(0.150)		(0.180)	(0.168)
Years since freshman year $\times$ Roommate - Family income $<$ \$50,000		0.248	0.166		0.253	0.231
		(0.142)	(0.114)		(0.153)	(0.140)
Roommate - Family income \$50,000-\$74,999		0.081	0.085		-0.121	-0.132
		(0.156)	(0.126)		(0.151)	(0.144)
Years since freshman year $\times$ Roommate - Family income \$50,000-\$74,999		-0.107	-0.104		0.072	0.087
		(0.121)	(0.100)		(0.128)	(0.118)
Roommate - Family income \$150,000-\$199,999		0.151	0.123		0.041	0.007
		(0.187)	(0.149)		(0.184)	(0.173)

Table B.5.1 (Continued)

Years since freshman year × Roommate - Family income \$150,000-\$199,999		-0.150	-0.121		0.010	0.033
		(0.152)	(0.127)		(0.155)	(0.142)
Roommate - Family income >= \$200,000		0.179	0.123		0.123	0.113
		(0.163)	(0.125)		(0.145)	(0.140)
Years since freshman year × Roommate - Family income >= \$200,000		0.038	0.050		0.095	0.074
		(0.135)	(0.105)		(0.128)	(0.119)
Share with dependent variable equaling 1	0.098	0.098	0.098	0.254	0.254	0.254
Share with dependent variable equaling 4	0.374	0.374	0.374	0.185	0.185	0.185
R-squared/pseudo-R-squared	0.074	0.088	0.191	0.074	0.089	0.205

Table B.5.1 (Continued)

Number of observations	1351	1351	1351	1349	1349	1349

	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Years since freshman year	0.218	0.166	0.234	-0.046	-0.173	-0.195
	(0.432)	(0.398)	(0.273)	(0.430)	(0.408)	(0.356)
Roommate - Black	-0.206	-0.086	-0.070	-0.619**	-0.448*	-0.450*
	(0.213)	(0.231)	(0.148)	(0.190)	(0.206)	(0.199)
Years since freshman year × Roommate - Black	0.055	-0.017	-0.023	0.068	0.002	0.063
	(0.166)	(0.173)	(0.116)	(0.165)	(0.174)	(0.156)
Roommate - Other minority	-0.050	0.031	0.047	-0.393**	-0.273	-0.239
	(0.146)	(0.158)	(0.093)	(0.149)	(0.158)	(0.161)

Table B.5.1 (Continued)

Years since freshman year × Roommate - Other minority	0.037	0.006	-0.017	0.257	0.159	0.121
	(0.131)	(0.138)	(0.090)	(0.140)	(0.146)	(0.141)
Roommate - Family income < $50,000		-0.209	-0.111		-0.276	-0.255
		(0.206)	(0.131)		(0.191)	(0.184)
Years since freshman year × Roommate - Family income < $50,000		0.249	0.145		0.274	0.243
		(0.157)	(0.101)		(0.170)	(0.156)
Roommate - Family income $50,000-$74,999		-0.032	0.002		-0.190	-0.171
		(0.176)	(0.111)		(0.159)	(0.159)
Years since freshman year × Roommate - Family income $50,000-$74,999		0.112	0.073		0.120	0.102
		(0.143)	(0.099)		(0.137)	(0.128)

Table B.5.1 (Continued)

Roommate - Family income \$150,000-\$199,999		0.121	0.066		0.156	0.142
		(0.213)	(0.124)		(0.179)	(0.182)
Years since freshman year × Roommate - Family income \$150,000-\$199,999		0.044	0.025		-0.059	-0.060
		(0.174)	(0.113)		(0.159)	(0.149)
Roommate - Family income >= \$200,000		0.117	0.041		-0.019	0.012
		(0.181)	(0.110)		(0.150)	(0.155)
Years since freshman year × Roommate - Family income >= \$200,000		0.078	0.072		0.190	0.140
		(0.147)	(0.095)		(0.139)	(0.133)
Share with dependent variable equaling 1	0.046	0.046	0.046	0.436	0.436	0.436
Share with dependent variable equaling 4	0.603	0.603	0.603	0.193	0.193	0.193

Table B.5.1 (Continued)

R-squared/pseudo-R-squared	0.094	0.107	0.193	0.095	0.109	0.230
Number of observations	1347	1347	1347	1349	1349	1349

Table B.5.2: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [white students, with control for roommate CIRP attitude toward affirmative action]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.408***	-0.336**	-0.256*	-0.446***	-0.406***	-0.350**
	(0.107)	(0.121)	(0.105)	(0.103)	(0.119)	(0.109)
Roommate - Other minority	-0.173	-0.179	-0.151	-0.213*	-0.191	-0.164
	(0.098)	(0.103)	(0.087)	(0.095)	(0.101)	(0.093)
Roommate - Family income < $50,000		0.050	0.035		-0.053	-0.045
		(0.118)	(0.100)		(0.119)	(0.107)
Roommate - Family income $50,000-$74,999		-0.017	-0.009		-0.044	-0.041
		(0.100)	(0.086)		(0.099)	(0.092)
Roommate - Family income $150,000-$199,999		0.003	0.004		0.048	0.039
		(0.121)	(0.104)		(0.124)	(0.115)

Table B.5.2 (Continued)

Roommate - Family income >= $200,000		0.212*	0.166*		0.213*	0.185*
		(0.100)	(0.082)		(0.097)	(0.092)
Years since freshman year		0.255	0.186		-0.370	-0.308
		(0.370)	(0.301)		(0.466)	(0.387)
Share with dependent variable equaling 1	0.098	0.098	0.098	0.254	0.254	0.254
Share with dependent variable equaling 4	0.374	0.374	0.374	0.185	0.185	0.185
R-squared/pseudo-R-squared	0.074	0.086	0.187	0.074	0.088	0.203
Number of observations	1351	1351	1351	1349	1349	1349
	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS

Table B.5.2 (Continued)

Roommate - Black	-0.145	-0.142	-0.119	-0.555***	-0.449***	-0.384**
	(0.121)	(0.140)	(0.101)	(0.116)	(0.131)	(0.119)
Roommate - Other minority	-0.015	0.012	0.014	-0.171	-0.145	-0.141
	(0.097)	(0.106)	(0.072)	(0.100)	(0.105)	(0.103)
Roommate - Family income < \$50,000		0.043	0.028		-0.018	-0.019
		(0.134)	(0.096)		(0.124)	(0.116)
Roommate - Family income \$50,000-\$74,999		0.099	0.085		-0.086	-0.077
		(0.110)	(0.078)		(0.107)	(0.101)
Roommate - Family income \$150,000-\$199,999		0.174	0.099		0.098	0.083
		(0.122)	(0.084)		(0.124)	(0.122)
Roommate - Family income >= \$200,000		0.202	0.114		0.149	0.143
		(0.115)	(0.077)		(0.100)	(0.100)
Years since freshman year		0.265	0.301		0.045	-0.026

Table B.5.2 (Continued)

		(0.423)	(0.293)		(0.411)	(0.363)
Share with dependent variable equaling 1	0.046	0.046	0.046	0.436	0.436	0.436
Share with dependent variable equaling 4	0.603	0.603	0.603	0.193	0.193	0.193
R-squared/pseudo-R-squared	0.094	0.107	0.195	0.093	0.107	0.226
Number of observations	1347	1347	1347	1349	1349	1349

Table B.5.3: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [subsample of white students with CIRP attitude toward affirmative action non-missing]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.514***	-0.460**	-0.363**	-0.532***	-0.486***	-0.419**
	(0.129)	(0.152)	(0.135)	(0.124)	(0.145)	(0.131)
Roommate - Other minority	-0.321*	-0.322*	-0.254*	-0.378**	-0.363**	-0.302*
	(0.126)	(0.133)	(0.116)	(0.129)	(0.136)	(0.123)
Roommate - Family income < $50,000		0.167	0.123		0.028	0.025
		(0.144)	(0.122)		(0.145)	(0.129)
Roommate - Family income $50,000-$74,999		-0.035	-0.008		0.023	0.009
		(0.121)	(0.106)		(0.121)	(0.112)
Roommate - Family income $150,000-$199,999		-0.003	-0.001		-0.023	-0.029
		(0.161)	(0.140)		(0.159)	(0.150)

Table B.5.3 (Continued)

Roommate - Family income >= $200,000		0.250	0.201		0.148	0.122
		(0.129)	(0.107)		(0.128)	(0.120)
Years since freshman year		0.351	0.235		-0.373	-0.283
		(0.419)	(0.332)		(0.534)	(0.415)
Share with dependent variable equaling 1	0.107	0.107	0.107	0.269	0.269	0.269
Share with dependent variable equaling 4	0.369	0.369	0.369	0.186	0.186	0.186
R-squared/pseudo-R-squared	0.076	0.094	0.206	0.075	0.096	0.223
Number of observations	819	819	819	819	819	819
	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS

Table B.5.3 (Continued)

Roommate - Black	-0.206	-0.175	-0.135	-0.604***	-0.501**	-0.433**
	(0.139)	(0.165)	(0.126)	(0.134)	(0.153)	(0.146)
Roommate - Other minority	-0.087	-0.049	-0.019	-0.318*	-0.299*	-0.253
	(0.132)	(0.143)	(0.105)	(0.140)	(0.145)	(0.140)
Roommate - Family income < $50,000		0.227	0.152		0.068	0.069
		(0.158)	(0.116)		(0.151)	(0.144)
Roommate - Family income $50,000-$74,999		0.143	0.136		-0.068	-0.046
		(0.133)	(0.101)		(0.136)	(0.131)
Roommate - Family income $150,000-$199,999		0.278	0.174		0.038	0.043
		(0.150)	(0.108)		(0.165)	(0.164)
Roommate - Family income >= $200,000		0.252	0.145		0.230	0.211
		(0.159)	(0.113)		(0.131)	(0.136)
Years since freshman year		0.541	0.462		0.270	0.113

Table B.5.3 (Continued)

		(0.495)	(0.311)		(0.418)	(0.374)
Share with dependent variable equaling 1	0.055	0.055	0.055	0.441	0.441	0.441
Share with dependent variable equaling 4	0.583	0.583	0.583	0.227	0.227	0.227
R-squared/pseudo-R-squared	0.086	0.106	0.192	0.091	0.116	0.239
Number of observations	816	816	816	818	818	818

Table B.5.4: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [subsample of white students with non-missing CIRP attitude toward affirmative action]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.133	0.019	0.068	-0.239	-0.080	-0.039
	(0.180)	(0.195)	(0.157)	(0.180)	(0.195)	(0.177)
Roommate - Other minority	0.039	0.108	0.062	0.009	0.126	0.103
	(0.158)	(0.172)	(0.137)	(0.143)	(0.156)	(0.142)
Roommate - Family income < $50,000		-0.254	-0.189		-0.280	-0.231
		(0.216)	(0.180)		(0.214)	(0.193)
Roommate - Family income $50,000-$74,999		-0.067	-0.069		-0.205	-0.155
		(0.190)	(0.157)		(0.179)	(0.164)
Roommate - Family income $150,000-$199,999		0.064	0.042		0.223	0.184
		(0.178)	(0.139)		(0.190)	(0.169)

Table B.5.4 (Continued)

Roommate - Family income >= $200,000		0.146	0.094		0.298	0.255
		(0.163)	(0.128)		(0.154)	(0.142)
Years since freshman year		-0.614*	-0.374		-0.622*	-0.606*
		(0.298)	(0.232)		(0.288)	(0.267)
Share with dependent variable equaling 1	0.083	0.083	0.083	0.230	0.230	0.230
Share with dependent variable equaling 4	0.382	0.382	0.382	0.183	0.183	0.183
R-squared/pseudo-R-squared	0.074	0.112	0.229	0.074	0.112	0.252
Number of observations	532	532	532	530	530	530
	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS

Table B.5.4 (Continued)

Roommate - Black	0.045	0.238	0.069	-0.431*	-0.200	-0.140
	(0.251)	(0.268)	(0.142)	(0.217)	(0.231)	(0.193)
Roommate - Other minority	0.095	0.225	0.115	0.025	0.167	0.108
	(0.142)	(0.161)	(0.094)	(0.147)	(0.160)	(0.150)
Roommate - Family income < \$50,000		-0.314	-0.187		-0.310	-0.214
		(0.252)	(0.170)		(0.224)	(0.194)
Roommate - Family income \$50,000-\$74,999		-0.101	-0.074		-0.184	-0.148
		(0.198)	(0.122)		(0.183)	(0.164)
Roommate - Family income \$150,000-\$199,999		-0.020	-0.032		0.193	0.151
		(0.211)	(0.134)		(0.185)	(0.172)
Roommate - Family income >= \$200,000		-0.048	-0.016		0.008	0.009
		(0.173)	(0.100)		(0.156)	(0.146)
Years since freshman year		-0.996**	-0.530**		-5.674	-1.080***

Table B.5.4 (Continued)

		(0.349)	(0.198)		(.)	(0.277)
Share with dependent variable equaling 1	0.032	0.032	0.032	0.427	0.427	0.427
Share with dependent variable equaling 4	0.633	0.633	0.633	0.141	0.141	0.141
R-squared/pseudo-R-squared	0.105	0.139	0.247	0.102	0.136	0.279
Number of observations	531	531	531	531	531	531

Table B.5.5: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only white women]*

	Comfortable talking with roommate			Would be friends even if not room-mates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.435*	-0.296	-0.212	-0.453**	-0.452*	-0.366*
	(0.171)	(0.195)	(0.162)	(0.162)	(0.181)	(0.166)
Roommate - Other minority	-0.197	-0.154	-0.120	-0.401**	-0.333*	-0.250
	(0.166)	(0.177)	(0.143)	(0.155)	(0.164)	(0.149)
Roommate - Family income < \$50,000		0.031	0.003		-0.174	-0.138
		(0.203)	(0.159)		(0.190)	(0.172)
Roommate - Family income \$50,000-\$74,999		-0.038	-0.006		-0.189	-0.151
		(0.158)	(0.129)		(0.162)	(0.148)
Roommate - Family income \$150,000-\$199,999		-0.322	-0.215		-0.385*	-0.298
		(0.200)	(0.166)		(0.193)	(0.171)

Table B.5.5 (Continued)

Roommate - Family income >= $200,000		0.154	0.101		0.080	0.066
		(0.163)	(0.126)		(0.160)	(0.151)
Years since freshman year		-0.675	-0.452		5.397	0.635
		(0.621)	(0.335)		(.)	(0.358)
Share with dependent variable equaling 1	0.089	0.089	0.089	0.255	0.255	0.255
Share with dependent variable equaling 4	0.360	0.360	0.360	0.173	0.173	0.173
R-squared/pseudo-R-squared	0.131	0.166	0.329	0.120	0.153	0.319
Number of observations	597	597	597	596	596	596
	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS

Table B.5.5 (Continued)

Roommate - Black	-0.143	-0.022	-0.064	-0.503**	-0.326	-0.287
	(0.200)	(0.224)	(0.159)	(0.172)	(0.193)	(0.189)
Roommate - Other minority	-0.206	-0.074	-0.041	-0.199	-0.103	-0.120
	(0.156)	(0.173)	(0.118)	(0.169)	(0.180)	(0.179)
Roommate - Family income < $50,000		-0.084	-0.014		-0.135	-0.095
		(0.203)	(0.144)		(0.207)	(0.196)
Roommate - Family income $50,000-$74,999		0.257	0.171		-0.225	-0.187
		(0.170)	(0.115)		(0.166)	(0.161)
Roommate - Family income $150,000-$199,999		0.025	0.019		-0.285	-0.253
		(0.211)	(0.155)		(0.195)	(0.187)
Roommate - Family income >= $200,000		0.296	0.169		0.010	-0.001
		(0.193)	(0.129)		(0.164)	(0.168)
Years since freshman year		6.476***	1.317***		5.675***	0.979*

Table B.5.5 (Continued)

		(0.696)	(0.292)		(0.820)	(0.393)
Share with dependent variable equaling 1	0.052	0.052	0.052	0.456	0.456	0.456
Share with dependent variable equaling 4	0.593	0.593	0.593	0.203	0.203	0.203
R-squared/pseudo-R-squared	0.157	0.196	0.312	0.135	0.174	0.329
Number of observations	594	594	594	597	597	597

Table B.5.6: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only white men]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.253	-0.128	-0.055	-0.362	-0.342	-0.255
	(0.208)	(0.241)	(0.190)	(0.207)	(0.233)	(0.214)
Roommate - Other minority	-0.219	-0.269	-0.153	-0.397*	-0.423*	-0.305
	(0.200)	(0.213)	(0.167)	(0.192)	(0.212)	(0.193)
Roommate - Family income < \$50,000		0.143	0.065		-0.009	-0.017
		(0.261)	(0.195)		(0.237)	(0.215)
Roommate - Family income \$50,000-\$74,999		-0.084	-0.041		-0.119	-0.080
		(0.216)	(0.168)		(0.229)	(0.208)
Roommate - Family income \$150,000-\$199,999		-0.150	-0.080		-0.354	-0.285
		(0.277)	(0.222)		(0.266)	(0.244)

Table B.5.6 (Continued)

Roommate - Family income >= $200,000		0.268	0.165		-0.001	0.007
		(0.219)	(0.164)		(0.221)	(0.209)
Years since freshman year		-1.406*	0.261		-1.725*	0.129
		(0.675)	(0.282)		(0.851)	(0.387)
Share with dependent variable equaling 1	0.067	0.067	0.067	0.229	0.229	0.229
Share with dependent variable equaling 4	0.378	0.378	0.378	0.184	0.184	0.184
R-squared/pseudo-R-squared	0.150	0.201	0.370	0.148	0.193	0.379
Number of observations	402	402	402	402	402	402
	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS

Table B.5.6 (Continued)

Roommate - Black	-0.181	-0.101	-0.083	-0.510*	-0.376	-0.328
	(0.230)	(0.259)	(0.177)	(0.213)	(0.249)	(0.256)
Roommate - Other minority	0.000	0.028	0.033	-0.279	-0.323	-0.238
	(0.194)	(0.221)	(0.138)	(0.212)	(0.236)	(0.239)
Roommate - Family income < $50,000		0.184	0.155		0.049	0.062
		(0.257)	(0.167)		(0.250)	(0.254)
Roommate - Family income $50,000-$74,999		0.236	0.152		-0.170	-0.118
		(0.219)	(0.142)		(0.230)	(0.226)
Roommate - Family income $150,000-$199,999		0.291	0.164		-0.108	-0.128
		(0.295)	(0.205)		(0.236)	(0.247)
Roommate - Family income >= $200,000		0.159	0.077		-0.092	-0.064
		(0.256)	(0.177)		(0.226)	(0.234)
Years since freshman year		4.952***	0.421		-7.007***	0.753

Table B.5.6 (Continued)

		(0.961)	(0.313)		(0.837)	(0.519)
Share with dependent variable equaling 1	0.043	0.043	0.043	0.418	0.418	0.418
Share with dependent variable equaling 4	0.598	0.598	0.598	0.229	0.229	0.229
R-squared/pseudo-R-squared	0.182	0.228	0.360	0.155	0.201	0.376
Number of observations	400	400	400	402	402	402

Table B.5.7: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only black students]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	0.137	0.147	0.107	0.665*	0.927**	0.647
	(0.328)	(0.375)	(0.316)	(0.339)	(0.350)	(0.342)
Roommate - Other minority	-0.291	-0.423	-0.240	0.118	0.061	0.078
	(0.227)	(0.286)	(0.245)	(0.213)	(0.252)	(0.253)
Roommate - Family income $<$ \$50,000		0.158	0.085		0.211	0.119
		(0.327)	(0.269)		(0.328)	(0.310)
Roommate - Family income \$50,000-\$74,999		-0.299	-0.229		-0.564	-0.447
		(0.293)	(0.253)		(0.293)	(0.284)
Roommate - Family income \$150,000-\$199,999		0.553	0.379		0.413	0.297
		(0.473)	(0.397)		(0.477)	(0.469)

Table B.5.7 (Continued)

Roommate - Family income >= $200,000		-0.495	-0.294		-0.460	-0.344
		(0.294)	(0.248)		(0.301)	(0.295)
Years since freshman year		0.589	-1.197*		1.248*	-0.073
		(0.614)	(0.591)		(0.555)	(0.714)
Share with dependent variable equaling 1	0.111	0.111	0.111	0.291	0.291	0.291
Share with dependent variable equaling 4	0.306	0.306	0.306	0.187	0.187	0.187
R-squared/pseudo-R-squared	0.206	0.272	0.490	0.201	0.261	0.482
Number of observations	252	252	252	251	251	251
	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS

Table B.5.7 (Continued)

Roommate - Black	-0.003	0.471	0.183	0.310	0.388	0.127
	(0.321)	(0.420)	(0.334)	(0.321)	(0.388)	(0.371)
Roommate - Other minority	-0.126	-0.068	0.002	0.271	0.171	0.113
	(0.235)	(0.315)	(0.271)	(0.229)	(0.308)	(0.272)
Roommate - Family income < $50,000		-0.489	-0.373		0.272	0.244
		(0.317)	(0.277)		(0.381)	(0.342)
Roommate - Family income $50,000-$74,999		-0.575	-0.334		-0.715*	-0.387
		(0.361)	(0.319)		(0.314)	(0.273)
Roommate - Family income $150,000-$199,999		-0.142	-0.085		0.104	0.130
		(0.447)	(0.381)		(0.434)	(0.445)
Roommate - Family income >= $200,000		-0.125	-0.132		-0.918**	-0.402
		(0.363)	(0.289)		(0.346)	(0.263)
Years since freshman year		0.558	-0.570		8.029***	1.649**

Table B.5.7 (Continued)

		(0.503)	(0.310)		(0.570)	(0.534)
Share with dependent variable equaling 1	0.072	0.072	0.072	0.526	0.526	0.526
Share with dependent variable equaling 4	0.584	0.584	0.584	0.131	0.131	0.131
R-squared/pseudo-R-squared	0.175	0.229	0.364	0.185	0.274	0.463
Number of observations	250	250	250	251	251	251

Table B.5.8: *Ordered probit and OLS regressions for freshman roommate predictors of attitudes of students one to three years after entering college [only nonblack-minority students]*

	Comfortable talking with roommate			Would be friends even if not roommates		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS
Roommate - Black	-0.488*	-0.582	-0.341	-0.342	-0.310	-0.231
	(0.236)	(0.311)	(0.251)	(0.225)	(0.280)	(0.266)
Roommate - Other minority	0.185	0.197	0.119	0.342	0.357	0.270
	(0.214)	(0.257)	(0.204)	(0.255)	(0.270)	(0.255)
Roommate - Family income < \$50,000		-0.235	-0.164		-0.211	-0.111
		(0.299)	(0.247)		(0.276)	(0.268)
Roommate - Family income \$50,000-\$74,999		0.057	0.027		-0.111	-0.064
		(0.273)	(0.206)		(0.245)	(0.235)
Roommate - Family income \$150,000-\$199,999		0.274	0.186		0.319	0.265
		(0.264)	(0.194)		(0.282)	(0.275)

Table B.5.8 (Continued)

Roommate - Family income >= $200,000		0.484*	0.264		0.281	0.234
		(0.227)	(0.180)		(0.232)	(0.226)
Years since freshman year		1.437	1.185		1.141	0.903
		(0.942)	(0.711)		(1.031)	(0.848)
Share with dependent variable equaling 1	0.118	0.118	0.118	0.263	0.263	0.263
Share with dependent variable equaling 4	0.429	0.429	0.429	0.221	0.221	0.221
R-squared/pseudo-R-squared	0.212	0.285	0.501	0.177	0.232	0.444
Number of observations	357	357	357	357	357	357
	Conflicts or arguments with roommate not frequent			Roommate is one of best college friends		
	Ordered probit without controls	Ordered probit with controls	OLS	Ordered probit without controls	Ordered probit with controls	OLS

Table B.5.8 (Continued)

Roommate - Black	-0.071	0.285	0.207	-0.267	0.030	-0.011
	(0.212)	(0.278)	(0.176)	(0.230)	(0.291)	(0.292)
Roommate - Other minority	0.248	0.408	0.209	0.540*	0.723**	0.570*
	(0.245)	(0.271)	(0.169)	(0.234)	(0.252)	(0.253)
Roommate - Family income < $50,000		-0.200	-0.117		0.040	0.067
		(0.323)	(0.225)		(0.285)	(0.256)
Roommate - Family income $50,000-$74,999		-0.528	-0.359		0.020	0.044
		(0.284)	(0.226)		(0.267)	(0.257)
Roommate - Family income $150,000-$199,999		-0.041	0.022		0.320	0.162
		(0.337)	(0.211)		(0.300)	(0.289)
Roommate - Family income >= $200,000		0.470	0.223		0.492*	0.296
		(0.250)	(0.148)		(0.240)	(0.248)
Years since freshman year		6.091***	1.189		1.131	1.003

Table B.5.8 (Continued)

		(0.739)	(0.642)		(0.933)	(0.853)
Share with dependent variable equaling 1	0.059	0.059	0.059	0.437	0.437	0.437
Share with dependent variable equaling 4	0.619	0.619	0.619	0.202	0.202	0.202
R-squared/pseudo-R-squared	0.181	0.285	0.475	0.194	0.245	0.444
Number of observations	357	357	357	357	357	357

Appendix C

Appendix to Chapter 3

C.1 Construction of variables and sample

Table C.1: *Construction of variables and sample*

	BOTH STATES	FLORIDA	GEORGIA[1]
Serious bodily injury by vehicle (Z)		Offense is "DUI,CAUSE SERIOUS BODILY INJRY" or "RECKLESS DRIVE-BODILY INJ."	Offense is "INJURY BY VEHICLE"

Table C.1 (Continued)

Vehicular homicide (Z)		Offense is "DUI MANSLAUGHTER", "DUI, MANSLAUGHTER", "DUI. MANS.-NO AID OR INFO", "HOMICIDE NEGLIG MANSL VEH", "HOMICIDE-NEGLIG MANSL-VEH", or "VEHICLE HOMCID-NO AID/INFO"	Offense is (("VEHICULAR HOMICIDE" or "VEH HOMICIDE TIT 40 CH 6") and (offense-specific sentence length is at least 3 years))
Actual months of sentence served in the first incarceration involving crash offense (X)	Difference between actual release and sentence begin dates (drop if negative), censored above at 840 months	Match offense to earliest incarceration episode for which offense's sentence date is before actual release date. Use this episode's sentence begin and actual release dates.	

Table C.1 (Continued)

Recidivism (Y) if there is a next incarceration episode that has a sentence begin date within ...[varies by specification]... following release from the previous episode. In the main result, the next incarceration episode must have no "new" vehicular offense. In the Appendix, it must have at least one. "New" is defined as having a non-missing offense date after release from the previous incarceration episode.		Offense code (http://www.dc.state.fl.us/appcommon/offctgy.asp) is 5400, 2694, 8640, 2617, 2427, 2419, 2415, 2820, 6135, 8641, 2404, 2409, 2304, 2408, 2406, 2401, 2402, 2403, 2405, 2411, 2400, 5405, 9812, 5406, 1216, 1217, 8808, 9813, 5403, 9811, 5408, 5401, 9810, 8813, 8812, 8810, 5404, 5409 (RECKLESS DRIVE-BODILY INJ.), 8822, 5407, 8821, 0909, 0922, 0917	VIOL MOTOR VEHICLE LAWS VIOL MOTOR VEH LAW DUI INJURY BY VEHICLE VIOL MOTOR VEH LICENSE/PL HABIT TRAF VIOL/OTHER LEAVE SCENE HAD INJURIES RECKLESS DRIVING VEHICULAR HOMICIDE HIT-RUN W/INJURY/FATALITY HABIT TRAF VIOL/IMPAIRED DRVNG HABTL VIOLATOR THEFT MOTORVEH OR PART HIJACKING MOTOR VEHICLE FETICIDE BY VEHICLE ENTERING VEHICLE CARJACKING ATTEMPTED

Table C.1 (Continued)

Criterion for sample/possible recidivism episodes		Sentence begin date at least 1999. Relevant crash offense at least 1999.	Sentence begin date at least 2000. Relevant crash offense at least 2000. Offenders spared recorded convictions under the First Offender Act or deceased are not included.
Further criteria for sample	No missing variables, including controls First incarceration including any key offense (serious injury or manslaughter/homicide by vehicle) Only one of the two key offense types in present incarceration	Age at earliest offense in incarceration at least 18 Crash offense is PRINCIPAL, ADJUDICATION NOT WITHHELD	Age at sentencing (only month of birth given; use first day of next month to be conservative) at least 18

C.2 Overall recidivism estimates

[1]The original data set did not include offense date to distinguish if an offense was newly committed since the last incarceration, so offense dates were downloaded manually for offenders eligible for the analysis if available on the Georgia Department of Corrections website, finishing around November 11, 2017. Some offenders were missing. Manually checking their GDC IDs, some returned a blank page and so were excluded from the data set due to missing offense date. One was found to have an error, so the data was pulled from the html page: 1199451. The rest were manually accessible, so their data was pulled from the html page as well: 1001332130, 1110165, 1135819, 1150934, 1152411, 1184657, 1197200, 685187, 1001278720, and 617061.

Table C.2: *Effect in level of overall recidivism rate [Basic]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2416	-0.00091***	-0.00074**	-0.0075	-0.0056	1843	-0.0013**	-0.0012**	-0.0025	-0.0028
		(0.00027)	(0.00029)	(0.0065)	(0.0065)		(0.00043)	(0.00045)	(0.0081)	(0.0076)
S_{FL}	1852	-0.00067*	-0.00061*	-0.00065	-0.00085	1454	-0.001*	-0.00097*	-0.0007	-0.00059
		(0.00026)	(0.00026)	(0.00051)	(0.00055)		(0.00041)	(0.00042)	(0.0008)	(0.00086)
S_{GA}	564	-0.0024***	-0.0023***	-0.005**	-0.0061**	389	-0.0027**	-0.003**	-0.0071*	-0.0086**
		(0.00064)	(0.00067)	(0.0019)	(0.0021)		(0.001)	(0.0011)	(0.0029)	(0.0031)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table C.3: *Effect in level of overall recidivism rate [II. YOUNG (34 or younger only) versus OLD (35 or older only)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
$S_{Pooled\_}$ $YOUNG$	1508	-0.00091*	-0.00036	-0.011	-0.012	1157	-0.0014*	-0.0009	-0.0032	-0.0047
		(0.00038)	(0.00041)	(0.0065)	(0.0074)		(0.00058)	(0.00062)	(0.0071)	(0.0074)
S_{Pooled}_OLD	908	-0.00096*	-0.00094*	0.0066	0.0074	686	-0.0012	-0.0013*	0.0016	0.0042
		(0.00037)	(0.00039)	(0.0062)	(0.0065)		(0.00064)	(0.00062)	(0.0068)	(0.0079)
$S_{FL\_}$ $YOUNG$	1134	-0.00057	-0.00034	-0.0005	-0.0012	888	-0.001	-0.0008	-0.00037	-0.0011
		(0.00038)	(0.00039)	(0.00079)	(0.00086)		(0.00058)	(0.0006)	(0.0012)	(0.0013)
S_{FL}_OLD	718	-0.0011**	-0.0011***	-0.0013*	-0.0015*	566	-0.0015*	-0.0014*	-0.0016	-0.0017
		(0.00033)	(0.00033)	(0.00063)	(0.00063)		(0.00058)	(0.00056)	(0.001)	(0.00099)
$S_{GA\_}$ $YOUNG$	374	-0.0027**	-0.0021*	-0.005*	-0.0069**	269	-0.003*	-0.0026	-0.0053	-0.0073*
		(0.00094)	(0.00098)	(0.0025)	(0.0026)		(0.0013)	(0.0014)	(0.0033)	(0.0033)
S_{GA}_OLD	190	-0.0016*	-0.0017*	-0.0034	-0.003	120	-0.0011	-0.0029*	-0.0065	-0.0092
		(0.00073)	(0.00066)	(0.002)	.		(0.0012)	(0.0011)	(0.0045)	(0.0048)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table C.4: *Effect in level of overall recidivism rate [For 3-year recidivism window setup only, offenders beginning incarceration "early in the data window": up until the latest year by state such that less than 10% of offenders for either offense are dropped due to lack of a fully observed recidivism window (release 2014 or later)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$	
S_{Pooled}	894	-0.00068	-0.0007*	-0.0077	-0.013
		(0.00035)	(0.00035)	(0.0085)	(0.012)
S_{FL}	634	-0.00061	-0.00061	-0.00039	-0.00044
		(0.00032)	(0.00033)	(0.00056)	(0.00061)
S_{GA}	260	-0.0014*	-0.0016*	-0.0058**	-0.0065**
		(0.00069)	(0.0007)	.	(0.0024)
Controls?		Yes	No	Yes	No

Table C.5: *Effect in level of overall recidivism rate [Recidivism window from date of incarceration rather than release]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2343	-0.0023***	-0.0022***	-0.014*	-0.012*	1775	-0.0033***	-0.0032***	-0.0042	-0.003
		(0.00023)	(0.00022)	(0.0065)	(0.0056)		(0.00036)	(0.00036)	(0.008)	(0.0075)
S_{FL}	1807	-0.0021***	-0.0021***	-0.0021***	-0.0022***	1412	-0.0029***	-0.0028***	-0.002**	-0.0019*
		(0.00021)	(0.00021)	(0.00044)	(0.00045)		(0.00032)	(0.00032)	(0.0007)	(0.00073)
S_{GA}	536	-0.0036***	-0.0035***	-0.0067***	-0.0075***	363	-0.0048***	-0.005***	-0.0096***	-0.011***
		(0.00057)	(0.00058)	(0.0017)	(0.0019)		(0.00085)	(0.00086)	(0.0027)	(0.0029)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table C.6: *Effect in percent of overall recidivism rate (level effect divided by sample-mean recidivism rate) [Basic]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2416	-0.0078	-0.0064	-0.064	-0.048	1843	-0.0077	-0.0073	-0.015	-0.017
		(0.0023)	(0.0024)	(0.056)	(0.055)		(0.0026)	(0.0027)	(0.048)	(0.045)
S_{FL}	1852	-0.0062	-0.0057	-0.0061	-0.008	1454	-0.0066	-0.0062	-0.0045	-0.0038
		(0.0024)	(0.0025)	(0.0048)	(0.0051)		(0.0026)	(0.0027)	(0.0051)	(0.0055)
S_{GA}	564	-0.016	-0.015	-0.033	-0.041	389	-0.013	-0.014	-0.034	-0.041
		(0.0042)	(0.0044)	(0.013)	(0.014)		(0.0049)	(0.0052)	(0.014)	(0.015)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table C.7: *Effect in percent of overall recidivism rate (level effect divided by sample-mean recidivism rate) [II. YOUNG (34 or younger only) versus OLD (35 or older only)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
$S_{Pooled\_}$ *YOUNG*	1508	-0.0066	-0.0026	-0.077	-0.089	1157	-0.007	-0.0044	-0.016	-0.023
		(0.0028)	(0.0029)	(0.047)	(0.053)		(0.0028)	(0.003)	(0.034)	(0.036)
S_{Pooled}_*OLD*	908	-0.012	-0.011	0.08	0.089	686	-0.012	-0.013	0.016	0.04
		(0.0045)	(0.0047)	(0.075)	(0.079)		(0.0061)	(0.0059)	(0.064)	(0.075)
$S_{FL\_}$ *YOUNG*	1134	-0.0046	-0.0027	-0.004	-0.0094	888	-0.0054	-0.0041	-0.0019	-0.0055
		(0.003)	(0.0031)	(0.0063)	(0.0069)		(0.003)	(0.0031)	(0.0062)	(0.0069)
S_{FL}_*OLD*	718	-0.014	-0.014	-0.017	-0.019	566	-0.015	-0.014	-0.016	-0.017
		(0.0042)	(0.0043)	(0.0081)	(0.0081)		(0.0057)	(0.0055)	(0.01)	(0.0098)
$S_{GA\_}$ *YOUNG*	374	-0.015	-0.012	-0.028	-0.039	269	-0.012	-0.01	-0.021	-0.029
		(0.0053)	(0.0056)	(0.014)	(0.015)		(0.0054)	(0.0056)	(0.013)	(0.013)
S_{GA}_*OLD*	190	-0.016	-0.017	-0.034	-0.03	120	-0.009	-0.023	-0.052	-0.073
		(0.0073)	(0.0066)	(0.02)	.		(0.0097)	(0.0091)	(0.036)	(0.038)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No

Table C.8: *Effect in percent of overall recidivism rate (level effect divided by sample-mean recidivism rate) [For 3-year recidivism window setup only, offenders beginning incarceration "early in the data window": up until the latest year by state such that less than 10% of offenders for either offense are dropped due to lack of a fully observed recidivism window (release 2014 or later)]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$	
S_{Pooled}	894	-0.006	-0.0061	-0.067	-0.12
		(0.0031)	(0.0031)	(0.074)	(0.11)
S_{FL}	634	-0.0058	-0.0058	-0.0037	-0.0042
		(0.0031)	(0.0031)	(0.0053)	(0.0058)
S_{GA}	260	-0.011	-0.012	-0.043	-0.048
		(0.0051)	(0.0052)	(0.016)	(0.017)
Controls?		Yes	No	Yes	No

Table C.9: *Effect in percent of overall recidivism rate (level effect divided by sample-mean recidivism rate) [Recidivism window from date of incarceration rather than release]*

Actual incarceration (months)	N	$\beta^{OLS,3-year}$		$\beta^{IV,3-year}$		N	$\beta^{OLS,5-year}$		$\beta^{IV,5-year}$	
S_{Pooled}	2343	-0.027	-0.025	-0.16	-0.14	1775	-0.023	-0.022	-0.029	-0.021
		(0.0026)	(0.0026)	(0.075)	(0.064)		(0.0025)	(0.0025)	(0.056)	(0.052)
S_{FL}	1807	-0.027	-0.026	-0.027	-0.027	1412	-0.021	-0.021	-0.015	-0.014
		(0.0026)	(0.0026)	(0.0055)	(0.0056)		(0.0024)	(0.0023)	(0.0051)	(0.0053)
S_{GA}	536	-0.032	-0.032	-0.061	-0.068	363	-0.027	-0.028	-0.055	-0.064
		(0.0052)	(0.0053)	(0.016)	(0.017)		(0.0048)	(0.0049)	(0.016)	(0.016)
Controls?		Yes	No	Yes	No		Yes	No	Yes	No