Three Essays on Social Inequality and the U.S. Criminal Justice System

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ABSTRACT:

Michael Everett Roettger: Three Essays on Inequality and the U.S. Criminal Justice System

(Under the direction of Ted Mouw)

This dissertation uses three essays to examine issues related to inequality and the U.S. criminal justice system.

In the first essay, I examine links between arrest, residential segregation, and immigration within U.S. metropolitan statistical areas (MSAs). This research addresses two separate, but contemporary fields of research where a) increased crime is associated with highly segregated urban black ghettoes and b) decreased crime is observed among immigrant groups. Data for race and ethnic populations for MSAs are aggregated from 5% integrated public-use micro-samples [IPUMS] of the U.S. Census surveys from 1980-2000; data for arrest rates are taken from FBI Uniform Crime Reports from 1980-2000. Results from fixed effect models find statistically significant results indicating (i) African American social isolation positively correlates with arrest rates and (ii) immigrant groups are differentially correlated with arrest rates based on immigrant race and ethnic classification.

In the second essay, I examine the effects of race and history of incarceration on employment among less-skilled men. Recent findings of audit and employer surveys have found that African Americans and ex-offenders are groups who, respectively, are less likely to be hired than whites and non-offenders. Expanding on prior research, I use data from the 1979 National Longitudinal Survey

of Youth to test if labor force participation and unemployment are jointly impacted by race and history of incarceration. To control for unobserved invariant characteristics of individuals and interview periods, I utilize fixed effect error terms at the individual level. Results indicate that, relative to whites, African American and Hispanic ex-felons are more likely to experience persisting unemployment and time out of the labor force in years after incarceration.

In the third essay, I examine how genetic, individual, familial, and community-level variables possibly mediate a link between father's incarceration and adult son's deviance and arrest. Using twin and nationally-representative sub-samples from the National Longitudinal Survey of Adolescent Health, I test how molecular genetic, individual, familial, and community variables from adolescence may mediate this link. In analysis, father's incarceration is found to be robustly associated with increased delinquency and arrest among adult sons when these effects are estimated.

This dissertation is dedicated to my paternal grandmother and parents. They have always encouraged hard work, dedication, and pursuit of higher education. This dissertation would not have been possible without their love and support.

ACKNOWLEDGEMENTS

As a sociologist, I describe the process of writing a dissertation as “a bureaucratic certification of esoteric knowledge production, enabled by extensive social support.” In the many months of struggling with data to produce results I believe a committee of experts will certify as new and significant, I have often thought to the writings of Weber and Veblen. In the many times I have exclaimed “This can’t be done!” or “Why am I here [and not in construction]?” I have also become acutely aware of the social support network that has enabled me to complete my dissertation. It is this social support network I wish to thank for helping me complete this six-year journey.

Since beginning my graduate school career at UNC, Ted Mouw has helped me immeasurably as an advisor, teacher, M.A. and dissertation committee chair, and expert in all things quantitative. Prior to my dissertation, Ted frequently made time in his hectic schedule to help me successfully navigate the treacherous shoals of graduate school. Ted also taught me his rigorous and extensive knowledge of data analysis. In completing my dissertation, Ted’s counsel and questions greatly influenced both the form and quality of my dissertation. I absorbed much Ted’s enthusiasm for rigorous analysis of a wide range of datasets. In my time at Carolina, Ted has been of immeasurable importance in both my personal and professional development.

In addition to Ted Mouw, the other members of my dissertation committee were most helpful. Larry Griffin’s expertise in race & ethnic studies and understanding of research design provided crucial insights and critiques that incrementally improved the dissertation. Guang Guo’s expertise in twin’s studies and multilevel modeling were instrumental in design and statistical analysis. Kathie Harris' expertise in immigration and familiarity with Add Health has provided valuable empirical and substantive insights. Finally, Ken Land's suggestions and comments greatly helped with interpretation of criminological research and performing spatial modeling.

I also owe much to my friend and partner-in-crime research, Demetrius Semien. In writing this dissertation, Demetrius has always patiently listened to issues and concerns that have arisen, provided wise council and advice, and always asked great questions that have greatly aided my research and writing. I will consider myself lucky to return myself half of all he has given me, and look forward to our hooding ceremony in May 2009.

I would like to thank Bob Pleasants, also a UNC Ph.D., for his assistance in editing and preparing this dissertation for submission to the graduate school. His help with editing grammar and content are greatly appreciated, and have substantively improved my error-prone writing.

Though listed last, my parents have been of first importance in completing my dissertation. Their love, guidance and support have known no bounds, and they are the bedrock upon which my dissertation stands. Words cannot begin to describe the love and appreciation I have for them.

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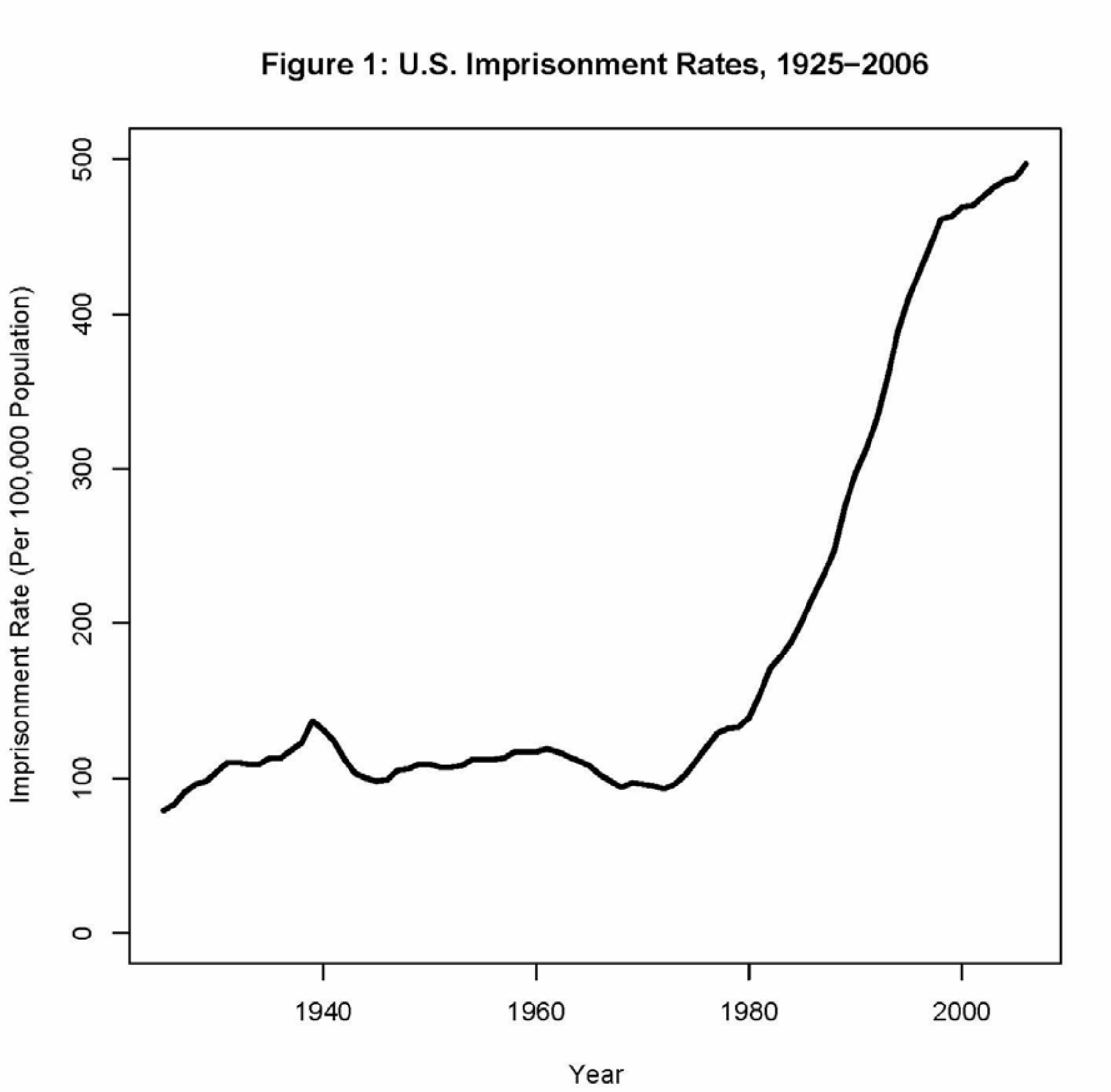
CHAPTER ONE:

Introduction

Raised in a rural county of Eastern Tennessee, some of my earliest memories involve pleasant trips to the state's largest natural area, Frozen Head State Park. The trips involved a winding road through a narrow valley owned by the state. The drive was notable for two things: the rugged contours and undisturbed wilderness of the Cumberland Mountains, and the presence of two of the state's oldest prisons. The beauty of the natural land stood in stark contrast to the razor-wire fences, guard towers, and gray concrete buildings.

Today, the freedom and beauty of the state park resting next to the grim, confining reality of the prison resonates with how I relate crime and inequality in U.S. society. In the post-Civil Rights Era, the total jailed and incarcerated population in the U.S. has quadrupled in the last four decades, growing from 250,000 in 1970 to 2.25 million individuals in 2006 (Harrison and Beck 2006). As Figure 1 illustrates, the incarceration rate between1980 and 2006 has grown well outside the range of earlier decades (Western 2006), increasing by nearly a factor of five from midcentury, even with the rapid increases in the U.S. population. Incarceration impacts large proportions of the U.S. population, especially racial minorities. Today, one-third of black males, 17% of Hispanic males, and 5% of white males will spend one year or more in prison during their lifespan (Bonczar 2003). Uggen, Manza and Thompson (2006) have recently estimated that 16 million men, including 5.5 million African American males, possessed a felony conviction in 2004. As Uggen and Manza (2006) and Western (2006) have recently argued, the rise of mass punishment and incarceration fundamentally contradicts the ideas



of equality and opportunity. In broadest terms, the question I address is how the construct of the prison relates to the "freedom" and "opportunities" in the areas of society where the prison does not exist. In these essays, by examining patterns of crime and incarceration, I make small attempts to relate the inequality resulting from these seemingly unrelated, but closely present social constructs.

Recent studies have failed to conclusively link general trends in income inequality with prison growth (Western 2006). Nevertheless, the impact of incarceration on offenders, families, and societies is substantial. Empirically, incarceration is linked to decreases in offender earnings and employment, (Western and Pettit 2005; Pager 2003; Edleman, Holzer, and Offner 2006), negative effects on children and families (Western, Loopo, and MacLanahan 2004; Johnson and Waldfogel 2002), a disproportionate impact on minorities (Pettit and Western 2004; Wacquaint 2001; Bonczar 2003), increased loss of voting rights (Uggen and Manza 2002), a 60% chance of recidivism within three years of release, and intergenerational patterns that link father's incarceration with adult son's delinquency and arrest (Roettger 2006). In combination, these sets of factors generate substantial penalties for individuals, families, and communities where ex-offenders work and live.

In criminological research, a well-established empirical association exists between crime and inequality. In urban metropolitan areas, increases in violent crime have been linked with unemployment, poverty, and residential segregation since the late 19th century (Du Bois 1996 [1898]; Land McCall and Cohen 1990; Massey and Denton 1994; Myrdal 1944/1945; Sampson, Morenoff, and Raudenbush 2005; Wilson 1987, 1996). The relationship between crime and inequality is magnified with large fines, legal costs, and other expenses associated with criminal justice system (Mauer 2003; Reimer 2003); welfare laws that require non-resident fathers to pay $40 to $200 weekly in child support while working at minimum wage (Holzer, Offner and Sorenson 2005; Edleman, Holzer, and Offner 2006); the difficulty of those with criminal records finding employment in the formal labor market (Pager 2003), 60% rates of recidivism for ex-inmates within three years of prison release; disproportional incarceration of less-educated men and minorities (Edleman, Holzer, and Offner 2006; Mincy, Lewis, and Han 2006); and lack of educational and work opportunities that provide meaningful alternatives for non-offending in adolescence (McLeod 1995; Hannon 2003). These issues create a durable inequality that increases propensities of individuals to engage in repeated criminal behaviors.

The racial disparities in incarceration also help to exacerbate existing racial inequality in the U.S. Since 1900, African American males have been incarcerated at four to six times the rate of white males in the U.S.; currently, 60% of black male high school dropouts will experience incarceration by age 45, while only 10% of white male high school dropouts will experience incarceration during this same period (Pettit and Western 2004; Western 2006). Some racial theorists such as Loic Wacquaint (2001) have argued that incarceration forms the basis of a new racial system that incarcerates black men while leaving black women to raise families at subsistence. In *Black Sexual Politics*, Patricia Hill Collins (2005) argues that disproportional incarceration of blacks is a result of a new paradigm for controlling the threat of black violence against white women after elimination of segregation. This racial disparity does not simply reflect current native population. For immigrants assimilating into American culture, research suggests that skin color greatly impacts likelihood of being incarcerated. For example, Portes and Raumbaut (2001) have found that Hispanic immigrant groups with darker skin tones such as West Indies Blacks and some Puerto Ricans have higher incarceration rates related to Hispanics with lighter skin coloration.

In completing this dissertation, I will extend these bodies of research in three essays. Using the 1979 National Longitudinal Survey of Youth, I test how incarceration changes employment patterns among a set of less-skilled men. Extending prior research from my master's thesis, I use sibling and nationally representative samples from the National Longitudinal Survey of Adolescent Health (Add Health) to examine how familial and community variables influence intergenerational crime and arrest among sons with fathers serving time in jail or prison. Finally, I utilize data from FBI Uniform Crime Reports and U.S. Census data to examine how residential segregation and immigration may explain crime and arrest patterns observed in major metropolitan areas. Thus, I contribute to understanding how incarceration generates long-term inequality, extend research in the intersection of racial theory and criminology, and test how intergenerational crime may be explained by genetic, individual, community and familial variables.

Dissertation Format & Overview

For this dissertation, I have chosen the university's three-essay format. The essays in this dissertation are on: (1) spatial analysis of the link between crime, residential segregation, and immigration within U.S. metropolitan statistical areas (MSAs); (2) effects of race and history of incarceration on employment among less-skilled men; and (3) effects of race and history of incarceration on employment among less-skilled men. Each essay is intended for journal publication. Although completion of this project involved usage of a number of data sets and theoretical frames, my goal in completing the work was to address the larger issues of inequality and its causes and the causes/consequences of incarceration. Through use of secondary data sources, I attempt to address these issues by applying quantitative methods to inequality resulting from crime and incarceration.

In the first dissertation essay, I conduct spatial analysis of the link among arrest, residential segregation, and immigration within U.S. metropolitan statistical areas (MSAs). This research addresses two separate but contemporary fields of research that indicate associations between a) increased crime associated with highly segregated urban black ghettoes (Massey and Denton 1994) and b) decreased crime rates observed among immigrant groups (Sampson, Morenoff and Raudenbush 2005). Data for race and ethnic populations for MSAs in the sample are taken from 5% integrated public-use micro-samples [IPUMS] of the U.S. Census for census periods 1980-2000; data for arrest rates are taken from FBI Uniform Crime Reports from 1980-2000. In the sample, results from fixed effect models find statistically significant results indicating (i) an increase in black social isolation is associated with increased arrest rates and (ii) immigrant groups are differentially correlated with arrest rates based on immigrant race and ethnic classification. A key methodological contribution of this paper is the usage of panel data with MSA-level fixed effects to model longitudinal *changes* in arrest rates within MSAs.

In the second essay, I examine the effects of race and history of incarceration on employment among less-skilled men. Recent findings of audit and employer studies by Pager and colleagues (Pager 2003; Pager and Quillian 2005; Pager and Western 2005) and Holzer, Rapheal, and Stolls (2004) have observed that African Americans and ex-offenders are groups that, respectively, are less likely to be hired than whites and non-offenders. Expanding on Bruce Western's (2002) work on wage trajectories and Raphael's (2006) analysis of employment after incarceration, I use data from the 1979 National Longitudinal Survey of Youth to test if labor force participation and unemployment are jointly impacted by race and history of incarceration. To control for unobserved invariant characteristics of individuals and periods, I utilize fixed effect error terms for individuals and interview sample-years. By interacting racial classification with employment changes of individuals with known criminal histories, I find that race and history of incarceration jointly impact time respondents spend out of the labor force.

In the final essay, I examine if genetic, individual, familial, and community-level variables mediate a link between father's incarceration and adult son's deviance and arrest. In prior research, I have found that adult sons of incarcerated biological fathers are 92% more likely than children without an incarcerated biological father to be arrested as an adult (Roettger 2006). In this essay, I use sibling and nationally-representative sub-samples data from the National Longitudinal Survey of Adolescent Health (Add Health) to test how molecular genetic, individual, familial, and community variables from adolescence may explain this link. In analysis, father's incarceration is found to be robustly associated with increased delinquency and arrest among adult sons. Extending statistical models developed by Guang Guo for longitudinal sibling analysis (Guo and Wang 2002; Guo and Tong 2006) and applied to molecular genetic predictors of delinquency (Guo, Roettger, and Shih 2007; Guo et. al 2008b; Guo, Roettger, and Cai 2008), this dissertation essay is among the first to examine intergenerational patterns of delinquency alongside molecular genetic variables. In using a nationally-representative sample of young men, the essay is also unique in providing a representative estimate for how father's history of incarceration correlates with increased delinquency and risk of arrest among young men in the U.S.

IRB Adherence

All previously-collected, publicly-available data are exempt from oversight by UNC IRB Guidelines found in 45 CFR part 46. Data for the 1979 National Longitudinal Survey of Youth (NLSY79), the FBI Uniform Crime Reports, and Integrated Public Use Microdata Series (IPUMS) are publicly-available samples, and are exempt from oversight by the Institutional Review Board at UNC. Access to the restricted sub-sample of National Longitudinal Survey of Adolescent Health has been previously granted to me conditioned on (1) my status as a doctoral student at the University and (2) my adherence to established guidelines for ensuring privacy of respondents under the direction of Ted Mouw.

CHAPTER TWO:

Immigration, African American Segregation, and Crime Changes in U.S. Metropolitan Statistical Areas, 1980-2000.

Introduction

A decade ago, William Julius Wilson (1996) analyzed crime in Chicago neighborhoods where urban ghettoes of high unemployment and poverty were associated with a host of negative outcomes, including drug use, violent crime, dropping out of high school, and chronic unemployment. Wilson's analysis demonstrated that African Americans suffered greatly from living within such urban ghettoes. Along with Wilson's contemporary research, much work has been directed towards understanding and documenting the consequences of segregation. A key thread in this research has been the spatial mismatch between jobs and African Americans (Dworak-Fisher 2004; Edelman, Holzer, and Offner 2006; Massey and Denton 1994; Mouw 2000; Wilson 1987, 1996). Recent research has noted the high disparities in incarceration observed between blacks and whites, with an association of crime to low wages and high unemployment. Though research has linked the spatial mismatch of jobs with residential and social segregation of blacks from jobs (Dworak-Fisher 2004; Mouw 2000, 2002), the link between residential segregation and crime has historically been explained as a consequence of discrimination, disparity, and inequality (Anderson 1990b, 1999; Drake and Cayton 1993 [1945]; Du Bois 1996 [1899]; MacLeod 1995b; Newman 2000). Leading economic theories help support this idea, with crime being treated as a rational alternative to legitimate labor market activities (Becker 1968).

A separate contemporary line of research has linked immigration with criminal behavior. This strand of research, however, focuses on the strong negative correlation of crime with immigrant status (Sampson, Morenoff, and Raudenbush 2005) or on crime as a measure for assimilation either into the mainstream economy or urban underclass based on existing racial classifications (Portes and Rumbaut 2001; Waters 1996). Reid et al. (2005) have used spatial data to show lack-of-correlation between crime and immigration, with effects for Asian immigrants showing a negative correlation with crime, to test popular Nativist arguments that immigrants commit more crime than non-immigrants. Using U.S. Census data, Butcher and Phiel (2006) have found that immigrants are less likely than non-immigrants to be incarcerated, explaining this difference by tough deportation laws for immigrants. This linkage of crime and immigration, however, has not been fully linked with general theories of race and ethnicity in the study of crime and deviance (Mears 2001; Reid et al. 2005; Sampson and Laub 2005; Sampson, Raudenbush, and Earls 1997).

The studies linking segregation of blacks with crime and the negative association of immigrants with criminal behavior raise an interesting question: to what extent may crime be explained by the simultaneous appearance of segregation of black males and the presence of immigrant populations? Historic sociological research has linked the struggle of immigrants and blacks for jobs and the spatial and social segregation of blacks and immigrant groups in urban centers (Cayton and Drake [1945] 1993; Du Bois 1996 [1899]; Ignatiev 1997; Massey and Denton 1994; Roediger 1999, 2005; Waldinger 1996). With the arrival of South, Central, and Eastern European immigrant groups into the U.S. in the late 19th and early 20th century, the contemporaneous development of heavily populated and segregated urban slums occurred in major U.S. metropolitan centers for both immigrants and African Americans (Lieberson 1980; Steinberg 1989). This trend continues today, with ethnic groups such as Cubans, Mexicans, and Vietnamese segregated into 'ethnic enclaves,' while the continued segregation of African Americans still occurs (Portes, Fernandez-Kelly, and Haller 2005; Portes and Rumbaut 2001; Waldinger 1996; Wilson 1996). The relationship between crime and populations of immigrants and blacks may be purely spurious, or may be a possible outcome of spatial mismatch of African Americans with job availability. However, if black segregation and emergence of immigrants both significantly correlate with changes in crime, this would suggest that patterns of immigration and segregation are related.

To test these hypotheses, I combine metropolitan data for arrest from the FBI Uniform Crime Reports with U.S. Census data for metropolitan statistical areas (MSAs) from the decennial census. FBI Crime arrest rates are readily available for analysis. The decennial census available for IPUMS provides a rich array of race and employment variables. Fixed effect models are used to control for unobserved, time-invariant variables that may influence arrest rates within counties. By using fixed-effects models to control for unobserved, time-invariant characteristics, I test if changes in immigration and segregation are significant in predicting crime and arrest within MSA's.

Explaining Crime, Race, and Immigration

In a recent study, Sampson et al. (2005) empirically demonstrated that first and second-generation immigrants commit less crime than non-immigrants and that racial differentials in crime are also largely explained by generic community-level variables (education, poverty, etc.). Using data from this Chicago survey and also macro-data on crime and immigration for the U.S., Sampson's (2006) recent *New York Times* editorial argued that crime rates in the U.S. have declined with influxes of international migrants beginning in the 1980's. While this theory of crime runs contrary to common Nativist theories arguing that immigrant groups contribute to overall increases in crime rates (Hagan and Palloni 1998; Mears 2001), immigrant groups who successfully assimilate into American society, in fact, exhibit lower levels of crime than U.S.-born natives (Reid et al. 2005; Sampson et al. 2005). Hispanics with darker skin colorations (which include large percentages of Puerto Ricans or West Indies blacks in the segmented assimilation literature) and African immigrants are exceptions, converging to crime rates exhibited by African Americans after the first generation (Portes and Rumbaut 2001; Waters 1994, 1996).

Why might crime be generally negatively associated with immigration, but vary among those commonly classified in American society as blacks, whites and of Hispanic origin?[1] As I will outline below, the general relationship between crime and immigration may be spurious, or suggest a more complicated relationship between crime and immigration patterns that vary by both race and ethnic status. Sampson, Morenuff, and Raudenbush (2005) have shown that this trend holds for violent crimes among first and second generation Mexican Immigrants in Chicago neighborhoods, but third-generation Mexican Americans behave similarly to native whites and Hispanics when immigration and SES variables are included in analysis. While groundbreaking, Sampson et al.'s (2005) study does not explain why Mexican immigrants are associated with lower levels of violence while Puerto Rican immigrants are associated with higher levels of violence. In fact, first and second-generation Puerto Rican and other Latino males are associated with relative higher crime rates ($p<0.01$) relative to all other immigrant groups, including Mexican Hispanics (Sampson et. al 2005, pg 228-229). Sampson et al.

[1] For this paper, racial classification is a single-category measure from self-reported identity that can be measured consistently across the 1980, 1990, and 2000 U.S. Censuses. This methodology, while consistent with racial classification present in government reports such as the Bureau of Justice Statistics (e.g., Bonczar 2003), has a number of limitations. Issues of multiple racial identity, treatment based on skin color, and/or differences in cultural values may substantively differentiate treatment/outcomes observed across racial groups. Lee and Bean (2004) discuss demographic trends for racial groups in the U.S., along with large, emerging subsets of individuals claiming multiple racial categories.

attribute causes of racial disparities in violent crime rates to general structural conditions such as disparities in education and poverty, but do not develop the issue of Hispanic ethnic origin. In doing so, their work does not fully address the differences of race and ethnic origin.

Similarly, Reid et al. (2005) have found that crime is negatively correlated with Asian-born populations and is statistically non-significant among other immigrant groups in a cross-sectional analysis of U.S. cities. Their research contradicts popular cultural arguments among non-immigrant majorities that immigrants are linked to violent and non-violent crimes. However, this study falls short of fully testing the process of immigration by not interpreting findings based on immigrant's race and ethnic status. Reid et al. also do not address the issue of racial segregation in explaining crime. Similarly, Reid et al. fail to control for unobserved, time-invariant heterogeneity across cities and incorporate panel data for longitudinal analysis. Usage of panel data for major U.S. cities that controls for the effects of international migration and black segregation on arrest can address many of the shortcomings of this study, while also providing a general test for associations between crime and immigration.

In addition to the adoption of panel data, segmented assimilation theory may also provide additional insights into why crime rates vary by an immigrant's ethnic origin and [U.S.] racial classification. Segmented assimilation theorists such as Mary Waters (1996, 1999) and Portes and colleagues (Portes and Rumbaut 2001; Portes and Zhou 1993) have emphasized the differences in assimilation processes among immigrant groups. Mary Water's (1999) work contrasts the movement of West Indians immigrants into native black classifications with her (1994) study of assimilation of Western Europeans into a white majority. Waters work finds that dark-skinned immigrants often must deal with perceptions that they are native-born blacks. Portes and Zhou (1993) argue that immigration is a process whereby immigrant groups either have continued economic success that converges to mainstream norms or experience confinement in inner cities and permanent social membership in the urban underclass. Building on this framework, Portes and Rambaut (2001) argue that residential and economic location of members of second-generation immigrants act to generally choose "plain American" or "panethnic' identities such as 'black' or 'Hispanic.' Comparative analysis of various Latino immigrant nationalities such as Cuban, Mexican, and West Indian have shown differential incarceration rates similar to categories of "white," "Hispanic," and "black" (Portes and Rumbaut 2001; Rumbaut et al. 2006). Thus, segmented assimilation processes may work to influence how immigrant groups become associated with crime and are processed differentially within the criminal justice system.

However, as Alba and Nee (2003) note, segmented assimilation theory may lead to potentially false bifurcation of immigrant assimilation into either mainstream American society or the American underclass. They argue that bifurcation of assimilation ignores variance occurring in families and individuals within immigrant groups. Bifurcation may also fail to fully capture improvements of immigrants, where only lateral movement into unskilled labor markets may denote greatly improved wellbeing relative to an immigrant's country of origin if living standards rise. The treatment of Puerto Ricans or black migrants as similar to immigrant groups also ignores unique historical factors and differences of these groups. In the mid twentieth century, Myrdal (1944/1945) recognized that black migrants moving out of a caste system within the South would face large class barriers to upward mobility. Duncan and Blau (1967) found that intergenerational mobility of black sharecroppers to the North only marked a transition into low-skilled blue-collar jobs. Analysis by Waldinger (1996) found that children of black manual laborers in New York City experienced discrimination in skilled jobs and barriers to penetrating immigrant-dominated niches of low-skilled jobs in private industry. New York foreign-born immigrants, in contrast, were able to maintain dominance within economic sectors by creating ethnic social networks and economic niches varying by ethnic origin. Left socially and physically isolated from jobs while concentrated in areas of high poverty and lack of employment opportunities, descendents of poor black migrants from the South may turn to crime and drugs as a means for escaping such conditions (Anderson 1990b, 1999; Wilson 1987, 1996a). Concurrently, segmented assimilation theory would predict other ethnic groups to experience general assimilation into the American "mainstream" based upon the ethnic group's racial classification by U.S. society. Hence, segmented assimilation due to racial labeling by U.S. society may lead to different crime and arrest rates across ethnic immigrant groups .

Black Historical Segregation and Crime

By differentiating urban black populations from immigrants, historical and structural issues become important factors linking race and crime. While assimilation allows for comparative treatment of individuals based on immigrant status, the spatial linkage between poverty and crime has long historical precedent, particularly among segregated blacks. Du Bois (1996 [1899]) found that disproportional arrest and incarceration among blacks occurred among Southern migrants living in impoverished areas of Philadelphia's seventh ward. Investigating the plight of blacks centralized in Chicago's "Bronzville" during the 1930's, Drake and Cayton (1945, pp. 200-210) observed that black Chicagoans resided in areas with the highest concentrations of male juvenile delinquents, illegitimate births, disease, and families living on public assistance in "ghetto conditions." More recent work by Wilson (1987, 1996) and

Anderson (1990, 1999) locates African Americans in similar ghetto neighborhoods characterized by a lack of jobs, high rates of crime, and continued segregation from other racial groups.

The historic segregation of blacks into ghettoes, beginning in the early twentieth century, continues with highly concentrated populations of blacks in central cities away from employment, education, and opportunities for assimilation (Dworak-Fisher 2004; Lieberson 1980; Massey and Denton 1994; Mouw 2000). Predatory lending practices and "redlining" by banks, movement of whites away from neighborhoods with increased populations of black residents, movement of jobs away from black neighborhoods, and lack of access to quality education are cited as key factors in generating impoverished ghettoes (Harris 1997; Massey and Denton 1994; Mouw 2000).

The location of blacks in historic ghettoes must also be emphasized along with the lasting effects of segregation and discrimination. In Myrdal's (1944/1945) treatise on the state of blacks in American society, Myrdal noted the prominence of criminal contacts as a means of black-white relationships. Myrdal wrote:

> *To the Northerners, this crime news is the most important source of information they get about Negroes. To white Southerners, the crime news reinforces the stereotypes and sometimes serves to unite the white community for collective violence [e.g., lynchings] against the individual Negro criminal or the local Negro community in general. (pg. 635)*

For Myrdal, race played a key role in society's justification of segregation of blacks from whites, while also generating stereotypes of African Americans. Contemporary racial theorists such as Roediger (1999, 2005), Collins (2005), Bonilla-Silva (2001, 2003) argue that continued perceptions of blacks as violent and as threats to non-minorities perpetuate stereotypes, even in a society where overt discrimination is highly stigmatized.

Recent empirical research finds persistent disparities that link adverse outcomes of African Americans that may be based on these stereotypes. Behrams, Uggens, and Manza (2003) have found that legal changes resulting in permanent political disenfranchisement of ex-felons and incarcerated populations are historically correlated with black population increases. The historical movement of other ethnic groups from neighborhoods that experience an increase in their percentages of African American residents continues today with "white flight," where crime, poverty, and drugs are often cited as causes for continued presence of segregated black neighborhoods and inner cities (Massey and Denton 1994; Harris 1999). In relation to the criminal justice system, racial disparities persist in sentencing (Steffensmeier and Demuth 2000), incarceration over the life-course (Bonczar 2003a; Pettit and Western 2004a), preferences among employers for whites even when blacks lack a criminal record (Holzer, Rapheal, and Stolls 2004; Pager 2003), and decreased earnings potential (Holzer et al. 2004; Pager 2003; Western 2002; Western and Pettit 2005). Rates of "idleness" are used by labor economists to describe black adult males who are not in school or working (Edelman, Holzer, and Offner 2006; Mincy, Lewis, and Han 2006). While these individual findings may be debated, such empirical research suggests that past and present stereotypes create adverse consequences for a racial group that remains physically and socially isolated from the rest of American society. Urban ethnographers (Anderson 1990b, 1999; Duneier 2000; Edin and Lein 1997) and researchers (Anderson 1999; Dworak-Fisher 2004; Mouw 2000, 2002; Sampson et al. 1997; Wilson 1987, 1996a) have documented the spatial and social isolation of blacks from other groups.

Segmented Assimilation, Segregation and Crime

If segregation of blacks and segmented assimilation for international migrants are simultaneous historical and structural processes in American societies, how do segregation and immigration relate within the structural context of American societies? The historical context provides a mechanism for testing and interpreting this relationship. As Du Bois (1996 [1899]), Roediger (1999, 2005) and Ignatiev (1997) have documented, Irish, German, and Eastern European immigrants competed with blacks in the labor force in the 19th and early 20th centuries. During the first half of the twentieth century, black migrants and various European immigrants settled into Northern cities. Over the next fifty years, blacks faced discrimination and segregation while European groups (e.g., Italian, Polish, Russian—nationalities Lieberson (1980) has defined as South-Central-Eastern [SCE] Europeans) eventually assimilated into mainstream American society (Lieberson 1980; Myrdal 1944/1945; Waldinger 1996). While these various European groups faced discrimination among first-generation immigrants and often lived in concentrated urban slums in "ethnic enclaves," access to educational resources, immigrant networks, and niches within the general economy provided means for assimilation into the mainstream American economy (Aldrich and Waldinger 1990; Waldinger 1996). These assimilation processes eventually allowed connections with "native" American society to include acculturation and inclusion (via intermarriage, high social prestige, and economic affluence) into traditional non-minority groups (Alba and Nee 1997; Alba and Nee 2003; Gans 1979; Gordon 1964).

The path of assimilation for racial groups of non-European origin historically has differed substantially from the path of European immigrants. While non-European racial minorities born on American soil were granted citizenship rights (though it is important to denote that treatment of such minority-group members created a "second class" citizenship with discrimination similar to those of blacks), non-European immigrants were not eligible for citizenship until the Immigration Act of 1965 (Alba and Nee 1997). Though the first significant numbers of Asians arrived in the U.S. during the 1860's, these groups remained segregated in "Chinatowns" and "Little Tokyo's" on the West Coast; assimilation into general American society began in the post-World War II era (Takaki 1989). The assimilation of many Asian nationalities into "Asian American" status continues, with Chinese, Japanese and Koreans residing in the U.S. for many generations alongside new influxes of Chinese, Vietnamese, and other Asian immigrant nationalities (Lee and Bean 2004; Takaki 1989; Waldinger 1996). Native Americans historically have been driven off native territories with the U.S. expansion, often driven onto reservations. Today, the economic success of Native Americans is contingent on movement away from tribal reservations (Nagel 1994).

The history of Hispanic assimilation demonstrates how racial treatment of immigrants has been based on historical context and skin coloration. Prior to the 19th century, Mexicans were heavily populated in the Southwest, but were driven out of American territories in the decades following the Mexican War of 1848. Not until after World War II did migratory workers begin entering the U.S. (Sowell 1982). Beginning in the 1960's, the movement of Mexicans, Puerto Ricans and other Latin Americans began to occur significantly in the Southwest and major urban areas such as New York, Miami, and Chicago (Waldinger 1996; Alba and Nee 1997). These "Hispanic" or Latino populations represent a combination of many nationalities and generations of immigrants; with pressure to claim Hispanic identity an ethnic label, many Hispanics claim secondary racial identities (e.g., Native American, Black, or white) (Harris and Sim 2001; Lee and Bean 2004). Depending on biological markers such as skin color, Hispanics more readily assimilate over generations into a majority population or become classified as native black citizens. The segmented assimilation for different Hispanic groups, such as West Indians and Cubans, illustrates how some immigrant groups will largely assimilate into mainstream American economic and social outcomes, while others will predominately move into highly segregated and impoverished conditions experienced by those classified as African Americans (Gans 1997; Portes and Rumbaut 2001; Waldinger 1996; Waters 1994). Thus, the case of Hispanic immigrants illustrates how race may lead to a delayed but divergent set of economic and social outcomes that correlate with discrete differences in criminal propensity.

By placing crime in the context of segregation of blacks and the segmented assimilation of immigrants, it is possible to better contextualize crime and immigration in the U.S. with racial theory. Geographically, historical segregation and discrimination against black migrants from the South persisted throughout the twentieth century. For international black immigrants, acculturation has predominately implied movement into U.S.-born black populations, with resulting disparate economic and educational outcomes compared to non-black immigrants. Concentrated in economic disadvantage, urban centers have generated structural poverty and lack-of-opportunity associated with criminal behavior (Anderson 1990b, 1999; Blau and Blau 1982; Rosenfeld, Messner, and Baumer 2001; Sampson et al. 2005; Wilson 1987, 1996a). The barriers to opportunity contrast with ethnic economies and social networks that generate pathways among other immigrant nationalities towards economic prosperity and educational success in the mainstream economy (Aldrich and Waldinger 1990; Waldinger 1996).

By including a more nuanced test that includes both immigration and measures of segregation, I hope to more generally test how segregation and immigration may predict crime and arrest. By analyzing the effects of immigration on crime, along with structural variance in segregation, I will expand Sampson et al.'s (2005) and Butcher and Phiel's (2006) work by more fully examining how segmented assimilation and race may explain changes in arrest rates. Usage of panel data for major U.S. cities that controls for the effects of international migration and black segregation on arrest can provide a general test for associations between crime and immigration that is lacking in analysis by Reid et al. (2005).

Given the historical precedent of immigration and segregation discussed above in explaining crime, along with the limitations of existing research, the analysis of panel data that measures crime, race, and immigration helps to better inform research on contemporary relationships between race, immigration and crime. FBI Uniform Crime Reports provide measures of annual arrest in county areas that can be combined with U.S. Census data to examine how ten-year changes in immigration and black populations correlate with arrest and employment data. Given the growth of the Hispanic immigrant population throughout the U.S. since 1980 (Landale and Oropesa 2007), data from the last three decennial U.S. Censuses capture large changes in metropolitan-area immigrant populations. Resident black populations in most urban areas have remained both constant and segregated during this period. Thus, a test for concurrence of black segregation and immigration in explaining arrest should indicate positive and statistically significant interaction between native-resident and immigrant blacks when arrest is the dependent variable.

The proceeding argument suggested that racial segregation and immigration may be separate processes, but a test for significance of immigration in metropolitan areas on arrests may provide clarification of the relationship between crime and immigration, separate from crime and black segregation. In such a situation, existing spatial mismatch between black populations and employment may operate independently from employment of immigrant groups in metropolitan areas. In such a case, the historical legacy of discrimination and structural conditions of poverty and unemployment may explain racial differences in arrest, consistent with results from Sampson et al. (2005). The non-significance of immigration along with the significance of poverty and unemployment would provide further support for more traditional arguments for structural causes of crime argued by Wilson and empirically demonstrated by Reid et al. (2005). Evidence for relevance of segmented assimilation and crime would be associated with different arrest rates across racial and ethnic groups as Rambaut et al.'s (2006) research showing varying incarceration rates across Hispanic nationalities would suggest.

Empirical results with panel data combining arrests and the decennial census help to better understand the relationship between crime, changes in immigration, and segregation in U.S. metropolitan areas. Thus, most broadly I address whether the effects of immigration on crime and segregation may be generalized beyond urban ethnographies (e.g., Anderson and Wilson) and single-city studies (Wilson 1987, 1996; Sampson et al. 2005; Waldinger 1996) that presently comprise this body of research.

Data and Methods

Data

To test for links between crime, assimilation, and racial segregation, I utilize population data from the decennial U.S. Census (Ruggles et al. 2005) and metropolitan arrest reports from the FBI's annual Uniform Crime Reports (Chilton and Weber 2000; U.S. Dept. of Justice 2006). For population data, I will utilize 5% state samples from the 1980, 1990, and 2000 U.S. Censuses to create representative populations for metropolitan statistical areas (MSAs) in the United States. The combination of Uniform Crime Reports with the U.S. Census' Integrated Public–Use Microdata Series (IPUMS) data provides a unique source for longitudinal data pertaining to metropolitan area-specific information on crime, population counts of race and ethnic immigrant populations, and employment. This data allows for analysis with fixed-effects models at the MSA-level to test if changing patterns in immigration correlate with black-resident populations to predict crime. As discussed below in the methodology section, this provides controls of time-constant, unobserved characteristics and focuses on time-varying demographic and crime trends.

Population

Census data has been frequently utilized in analysis examining links between crime and population characteristics, particularly in the study of violent crime and inequality (Land, McCall, and Cohen 1990b). Most recently, Reid et al. (2005) and Parker, Stults and Rice (2005) use combinations of Census data with Uniform Crime Reports to examine issues of crime and social control in urban cities and counties. These methodologies rely on cross-sectional data, which may be used to examine differences across units of observation. Because of the long-term, intergenerational nature of immigrant assimilation (Alba and Nee 2003; Gordon 1964; Lieberson 1980; 2005; Sampson et al. 2005) and the development of concentrated and segregated populations of blacks in urban ghettoes (Cayton and Drake 1945; Lieberson 1980; Massey and Denton 1993; Wilson 1987, 1996), longitudinal data better captures resulting effects of these trends by allowing for statistical analysis of effects within units of observations (e.g., MSAs). Thus, representative panel data for urban counties provides another form of testing potential links in assimilation, black populations, and crime within counties. The 5%-state 1980, 1990, and 2000 IPUMS data samples provide representative populations measures that may be created and analyzed (Ruggles et al. 2005).

Segregation

Massey and Denton's (1994) *American Apartheid* represents a classic work on continued black/white segregation at the end of the twentieth century. In a sample of major U.S. metropolitan areas, the authors found African American segregation levels at between 70-80% in 1980. The authors also identified several underlying dimensions of segregation, including (1) *social isolation*, the extent to which minority members are exposed only to members of their own racial group; (2) *index of dissimilarity*, the percentage of a group's population that would have to change residence for each neighborhood to have the same percentage of that group as the metropolitan area overall; and (3) *spatial proximity*, the average intra-group proximities for the minority and majority populations, weighted by the proportions each group represents of the total population. These measures of segregation are used to capture the amount of 'exposure,' 'concentration,' and 'clustering' of African Americans relative to non-Hispanic whites within census tracts at the MSA level.

Recently, the U.S. Census Bureau released data on residential segregation for 220 MSAs (Iceland, Weinberg, and Steinmetz 2004) for the 1980, 1990, and 2000 censuses. The findings from the data indicate that African American dissimilarity decreased by 12%, African American isolation decreased by 10%, and African American spatial proximity decreased by 5% between 1980-2000. MSA-level scores for the African American isolation index, the African American dissimilarity index, and the African American spatial proximity index relative (each index with non-Hispanic whites as the reference group) are utilized as potential sources for African American segregation within cities.

Immigration

Sampson, et al. (2005) empirically associate convergence in crime rates with each subsequent generation of immigration. In analysis, distinguishing between resident populations of first-generation foreign-born and native-born populations is possible using IPUMS data. I include these measures in analysis. Due to a high degree of correlation between foreign and U.S.-born racial and ethnic groups, I estimate separate models for native and foreign-born populations.

Arrest

Analyses utilizing the FBI's Uniform Crime Reports have been a well-established tradition in empirical research (Baller et al. 2001; Blau and Blau 1982; Land et al. 1990b; Myrdal 1944/1945; Parker et al. 2005). Arrest counts represent official statistics of police agencies for known offenses cleared by arrest that are compiled annually by the FBI. The amount of crimes are likely downwardly biased relative to total crime actually committed (Thornberry and Krohn 2000). Using data from the National Crime Victimization Survey , Baumer (2002) has shown that neighborhood structure and composition do not alter reporting of violent crimes to police, suggesting that violent crime rates are consistent across racial groups and socioeconomic structures. The Uniform Crime Reports primarily report violent crimes and property theft, and hence represent a limited subset of total crime occurring within a given area. While arrest rates may remain correlated with forms of social control and acts by the state to deter crime (Levitt

2004; Parker et al. 2005), UCR total arrest counts are for all criminal activities and hence likely represent a broader array of offenses sanctioned by arrest by law enforcement agencies in a given MSA. Arrest data is widely known to provide an undercount of crime occurring with a given area, but it may still be used as a conservative estimate of total crime occurring within a given area.

MSA-level Analysis

The use of metropolitan-level data from the census captures a larger geographical region than neighborhood and census-tract areas. Recent work by Lynch and Sabol (2001) has found that ex-felons are often concentrated within particular urban communities where lack of jobs and poverty are thought to increase levels of criminal activity (Anderson 1990b; Johnson 2003; Wilson 1996a). Sampson et al. (2005) utilize data from the Project on Human Development in Chicago Neighborhoods (PHDCN) collected from 1995-2002 to analyze effects of immigration and social structure on violent crime rates. The PHDCN is a unique set of data in a long tradition of research examining race and assimilation within Chicago (dating to the early studies of Robert Park and W.I. Thomas) because it contains a rich set of community, individual, and familial-level variables with a wide variation in race and ethnic background. Their analysis of the PHDCN uses a multilevel random effects model to examine differences occurring between neighborhoods and focuses on individual-level propensities for an individual to commit crime in a given neighborhood. The data is excellent for observing variance between neighborhoods and testing for differentials in risk based on variance in social structure.

The focus on MSA-level data can be explained by some of the limitations of the PHDCN data. The analysis of PHDCN findings is limited to Chicago neighborhoods, while also representing a period of fairly large economic growth and general declines in crime rates. Immigrant and racial/ethnic populations are also measured on an annual basis for the PHDCN; given that immigrant groups and communities form and evolve over extended periods of years or decades, the use of decade-intervals provides an alternative angle for viewing consequences of long-term immigration. The analysis also measures individual propensity to commit crime based on reports from Chicago police departments and does not use individual based self-reports or criminal records commonly associated in analysis of criminal activity to calculate propensities to commit crime.

My analysis differs because it examines changes that occur over the course of three decades. By using metropolitan-level data, I also attempt to focus units of analysis where immigrant populations and native blacks may form communities within a given geopolitical region. The analysis of crime and violence (especially homicide) has focused on the city level to examine social and economically linked levels of analysis (Land et al. 1990b; Messner 1982; Reid et al. 2005). By examining changes in crime and arrest at the MSA-level over the course of three decades, I will be able to better document if immigration or economic variables, such as employment and poverty, influence crime and arrest.

MSA Dataset

To generate MSA data with population-specific counts, arrests totals, and segregation data, data were aggregated using a using a number of sources listed in Appendix 1. To obtain MSA population data, 5% samples of the U.S. population were drawn from the IPUMS state samples for each year of the decennial census in the sample. Due to large file size, these micro-samples were sorted using the University of North Carolina’s research computer Emerald. The data were then collapsed from 5% micro-samples into MSA areas using MSA-county definitions generated by the Missouri Census Data Center's MABLE software [available online at: http://mcdc2.missouri.edu/websas/geocorr2k.html]. MSA level data were then combined with measures of African American segregation from the U.S. Census and FBI arrest rates for MSAs. The aggregated dataset yielded a sample of 269 cities and 654 MSA-year observations. However, due to IPUMS data incompletely reporting MSA observations and incomplete reports from law enforcement agencies, a large number of observations were discarded.[2] A sample of 112 MSAs with a total of 276 MSA/year observations were found to yield consistent results with smaller sub-samples. To address incomplete data, MSA arrest and population data were adjusted to reporting and IPUMS estimated population rates, respectively, for a given MSA-year observation. A definition of these variables is provided in Table 1.

The number of missing MSA/year observations arise from incomplete or missing population data. One source of missing data arises from low population estimates in FBI Uniform Crime Reports, where jurisdiction populations are weighted as zero during periods when law enforcement agencies fail to voluntarily report arrest information. A second source of missing data arises from shifting MSA boundaries; portions of MSAs lying outside of census boundaries are excluded from population counts. Within the 5% IPUMS samples, a number of individual records are missing MSA identifiers [a complete listing is provided at: http://usa.ipums.org/usa/volii/incompmetareas.shtml], resulting in artificially low population counts for a number of MSA/year observations. Finally, sampling areas created by the U.S. Census may lie only within an MSA boundary.

[2] After experimentation, cases where FBI arrest populations, Census MSA populations, and IPUMS representative populations were within 10% were found to yield consistent results in regression models with a sub-sample containing with less than 1%. Biases in MSA population data occur due to incompatible county and census observations. FBI population differences arise from incomplete reporting by law enforcement agencies.

Means and standard deviations for relevant variables are provided by year and for the overall MSA sample in Table 1. A listing of the cities is provided in Appendix 2. Due to high collinearity, not all descriptive statistics presented in Table 2.1 are utilized in analysis.

Methods

In a large comparative study of variables used to predict macro-trends in homicide, Land et al. (1990) found that unit-of-analysis and regression methods substantively alter study findings for effects of poverty and community variables in predicting crime rates. Recent work by both Reid et al. (2005) and Parker et al. (2005) utilized U.S. Census data and MSA arrest/crime variables from FBI Uniform Crime Reports. Though I will utilize U.S. Census and FBI Uniform Crime Reports data, my work will differ in two ways: 1) I will aggregate decennial micro-level census data for metropolitan statistical areas (MSA's) with arrest data from 1980-2000 to create a longitudinal sample for crime and population characteristics, and (2) I will utilize fixed effects error models to observe how changes in immigration and U.S.-born black populations correlate with arrest patterns. The usage of longitudinal data for analysis and statistical methods may test for concurrent effects of immigration and residential segregation on changing arrest patterns over three decades. Similar methods have been employed in tests for spatial mismatch (Dworak-Fisher 2004), but remain largely untried in the conventional criminological literature.

To test if relationships exist between black segregation, immigration, and crime in metropolitan areas, I utilize fixed effects models. As Allison (2005a) and Halaby (2004) note, fixed effect regression models allow for consistent and unbiased measurement of longitudinal data while controlling for time-invariant characteristics. Because of the large variation in regional and local economic conditions, population characteristics, and immigration patterns at the MSA level, MSA-specific error would likely bias OLS and multilevel model estimates of regression coefficients. Because I also utilize all available counties for analysis, the use of an MSA-specific error term also allows for analysis of generalized trends of hypothesized links in the MSA sample.

By using a fixed effects model with error components for individual cities, it is possible to test if immigration and segregation distinctly occur within MSA's. While it is possible to draw a 'random' cross-section of counties in MSA's, as done by Reid et al (2005), cross-sectional OLS regression techniques the authors employ have several limitations: (1) cross-sectional regression does not employ a longitudinal design to determine whether immigration actually affects crime, (2) the estimation techniques do not take into account the intergenerational characteristics of immigrants that seem to be associated with criminal behavior, (3) the limited number of MSAs and missing data from IPUMS sub-sample create a limited and incomplete population from which to draw samples, and finally, (4) cross-sectional models do not determine how segmented assimilation may empirically lead to differential outcomes across different racial and ethnic populations.

The basic fixed-effects framework I will utilize may be more explicitly discussed in equation format as:

$$Y_{it}= \beta_{0t}+ \Sigma(\beta_{jt} * X_{jit})+ + \Sigma(\gamma_{kit} * z_{kit}) + \varepsilon_{it},$$

where i and t represent the ith Metropolitan Statistical Area (MSA) at time t, Y_{it} is the arrest rate recorded in MSA i at time t, β_{0t} is a constant, $\Sigma(\beta_{jt} * X_{jit})$ is the set of time-varying predictors and coefficients, $\Sigma(\gamma_{kit} * z_{kit})$ is the set of time-invariant predictors and coefficients, and ε_{it} is the error terms in the equation such that

$$\varepsilon_{it} = e_{it} + u_i + w_t,$$

where $e_{i\,t}$ represents a random disturbance term, u_i is an error term representing specific error for the MSA i, and w_t represents an error component for measuring arrest at time t.

As Allison (2005) demonstrates mathematically in the two-time period model, fixed effects models examine first differences in the dependent variable (e.g., $Y_{t=2,is}$ -$Y_{t=1,is)}$ for the ith MSA. As a result, the set of all time invariant variables $\Sigma(\gamma_{kit} * z_{kit})$, where $z_{ki,t=1} = z_{ki,t=2} = z_{ki,t=3} = \ldots z_{ki,t=n}$, cancels out of the regression equation. For studies such as Reid et al., (2005) cross-sectional analyses with OLS regression do not, in contrast, eliminate the set of time-invariant characteristics $\Sigma(\gamma_{kit} * z_{kit})$ from the sample. While Reid et al. (2005) attempt to draw on a "random and representative set of MSAs," their analysis rests on the premises that: 1) no correlation exists between predictors and errors that may bias estimation and 2) unobserved characteristics do not correlate with observed variables. By eliminating $\Sigma(\gamma_{kit} * z_{kit})$, the fixed-effects model eliminates these issues for all time-invariant variables.

The error structure of a fixed-effect model is also important to note. By incorporating error components for each specific MSA (u_i) and year of data (w_t), this error structure provides a mechanism to control for error that may be due to time or MSA-specific components. The fixed-effects model measures error within MSAs and not between MSAs. As a consequence, it is possible to determine if crime and arrest are correlated with changes in immigration and segregation in the specific MSA over three decades to more accurately test patterns of immigration, not simply observe if changes are a result of correlations observed across cities at a given time t. This error structure allows for random and MSA-specific errors to occur; consequently, the assumptions of equal weighting and independence of MSA units are needed for OLS regression.

It should be noted that one- and two-way fixed effect models are not without limitations. Fixed effect modeling provides consistent and unbiased standard errors, but it is an inefficient estimator. Hence, there remains significant potential for Type II (false negatives) errors relative to more efficient estimators like OLS regression and random effects models. Work by Sampson et al. (2005) and Baller et al. (2001) utilizes random effects models when controlling for, respectively, community and county level errors, in addition to a random disturbance term. When these models approximate random and identically distributed populations through such error structures, they are preferred. However, as in the case of cities in this sample, unobserved characteristics generate results which fail the Hausman test across all estimated models, implying the need for fixed effect errors for reliable inference (Allison 2005a; Halaby 2004).

Table 2.1 lists the major variables I propose to test in analysis. As previously discussed above, use of arrest and crime data for metropolitan areas allows for measurement of reported crimes and arrests as a function of the sampled population. An identifier for a given year and metropolitan area provides mechanisms for generating error components in the fixed effects model for time and geography. Measurements for population growth, single-parent households, and poverty rates provide tests for structural conditions that may influence poverty rates in a given metropolitan area.

Land et al.'s (1990) analysis established a robust set of common variables predicting homicide in geographic data at the state, county, and MSA level from the years 1950-1980.

Using principle component analysis, the authors construct a set of variables that have low collinearity and explain a high proportion of variance. These common variables include: a measure of relative deprivation/affluence within a geographic unit, a measure of the geographical population structure, the unemployment rate of the geographical unit, and the divorce rate of a geographic unit. These components were widely adopted and effectively used as base models in research on homicide, violence, and crime across geographical areas (Baller et al. 2001; Parker et al. 2005; Reid et al. 2005).

The wide adoption of Land et al.'s (1990) work is a testament to the study's robustness and scope. However, two limitations of this framework apply to this analysis. The first is that observations for this established work relate to modeling *between* geographical units, and not *within* geographical units. In unreported analysis, principle components for population and relative deprivation failed significance tests when year and MSA level fixed effects were applied. However, models measuring differences between MSAs were found to remain significant in OLS and models with MSA-level random effects. From a theoretical standpoint, this would imply that the effects of factorial variables for population structure and relative deprivation had time invariant influence on arrest rates within MSAs.

A second limitation is that predictor variables analyzed directly relate to population structure and relative deprivation. High correlations were observed between African American segregation and relative deprivation. Empirical analysis has observed that African American segregation correlates extensively with structural components such as poverty, low educational attainment, and single-parent families (Haynie, Silver, and Teasdale 2006; Land et al. 1990b; Massey and Denton 1994; Western and Pettit 2005; Wilson 1987, 1996a).

As a solution to these issues, I attempt to use variables centered around Land et al.'s (1990) criteria for variable selection by 1) selecting variables for usage where collinearity is minimized (e.g., correlations between variables remain below 30% in estimated models while generating spurious results) and 2) minimizing variable usage while selecting variables that roughly meet the dimensions established by Land et al. Experimentation yielded four variables which were generally found to meet these criteria: the sex ratio, the divorce rate, unemployment rate, and the percentage of individuals residing below the poverty line. The sex ratio is used as a population structure variable that has been found to be empirically correlated with arrest rates (Messner and Sampson 2005).

The concept of relative deprivation may be linked with (lack of) economic opportunity. Becker's (1968) hypothesis that crime is an outcome related to labor market opportunities is widely accepted in existing economic research, with empirical studies indicating a negative correlation between macroeconomic growth and crime rates (Edelman et al. 2006; Freeman 1996, 2000b). Economic expansions are also empirically linked to crime (Edelman et al. 2006; Freeman 2001; Holzer and Offner 2006). Poor outcomes in the labor market may explain differences in crime rates observed across groups, making segregation, immigration and crime as spurious. Recent work by Butcher and Phiel (2006) has used U.S. Census data to argue that increased penalties for immigrants encoded into U.S. law creates a rational deterrence effect for behaviors leading to detention/arrest.

Results

Bivariate Regression

Tables 2.2 to 2.5 present results from MSA-level fixed effect models of arrest and population change. These models provide basic null hypothesis tests if, controlling for unobserved MSA-level time-invariant effects, changes in a race/ethnic population are associated with differing arrest rates. In the MSA sample, high correlation between immigrant and U.S. native populations was observed.[3] Consequently, estimation of population changes of foreign and native-born groups are presented for each race and ethnic group in the census data.

Tables 2.2 and 2.3 list bivariate regression results for changes in arrest rates and racial populations within MSAs. Native-born populations are presented in Table 2.2. For native-born Hispanics, a one percentage increase in proportion of MSA composition is associated with a net decline of 208 total arrests ($p<0.01$) and 59 property arrests ($p<0.001$) per 100,000 population. Significant declines in property arrest rates were also observed for Asian groups ($p<0.001$), while increases in property crimes were observed with increases among native whites ($p<0.001$). Excluding Hispanics, changes in race and ethnic populations were not found to be significant for violent and overall arrest rates.

Table 2.3 presents results for changes in arrest associated with changes in foreign-born immigrant groups. Overall, a one percent increase of immigrants living within an MSA is associated with no significant change in total arrest rates, an increase of 375 violent crimes ($p<0.05$), and a decline of 2400 property crimes ($p<0.001$) per 100,000 population. These values not only suggest that the effects of immigration may vary by types of crime, but high

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[3] Correlation between U.S. born and foreign born Hispanics, for example was 0.95. This makes simultaneous estimation almost impossible within regression models.

standard deviations may also result from significant variation among immigrant populations. Breakdown of immigrants demonstrates this result. Increases in percentage of Hispanic immigrants within an MSA are significantly associated with increased violent arrest rates ($p<0.05$), but decreases in property arrest rates ($p<0.05$). Similarly, the proportion of Asian immigrants residing in an MSA is associated with an increase in violent crime rates ($p<0.05$), but is also associated with highly significant ($p<0.001$) property arrest rates. Black immigrants are associated with a significant increase in total arrests ($p<0.01$), but a decline in property rates.

Across immigrant populations, arrest rates are most consistently associated with decreased property arrest rates. Only black immigrants are associated with increased rates of total arrest, which prior research links to a mechanism of social control (Parks et al. 2005). Overall, some variance among immigrant groups is observed based on immigrant's racial classification.

Both native-born and immigrant Hispanics are associated with decreased property crime rates. But increases in native-born Hispanic populations are associated with decreased total arrest rates ($p<0.01$), while Hispanic immigrants are not associated with changes in total arrest rates. Given the 0.95 correlation between Hispanic native-born and immigrant populations across MSAs, this difference is somewhat surprising. Tables 2.4 and 2.5 present bivariate regressions for four Hispanic racial sub-classifications available by the U.S. Census: Puerto Ricans, Cubans, Mexicans, and other [primarily Central and South American]. As I will discuss later, ethnic distinctions of Cubans, Mexicans, and Hispanics are associated with segmented assimilation theory in existing research (Portes and Raumbaut 2001; Waters 1994, 1999).

Table 2.4 presents changes in MSAs' proportion of these four Hispanic groups, without considering immigration status. While the 'other Hispanic' category is associated with decreased total arrest rates ($p<0.05$) and property crime rates ($p<0.001$), no significant coefficients are observed with changes in an MSA's proportion of residents identified as Mexicans, Cubans, and Puerto Ricans.

Table 2.5, however, yields highly significant results when Hispanic groups are differentiated by immigration status. Increases in U.S.-born Mexican populations are associated with a highly significant increase in violent crime rates ($p<0.001$), while foreign-born Mexicans are associated with decreased total arrest rates ($p<0.10$) and property arrest rates ($p<0.05$). Changes in the proportion of native and foreign-born Cubans are not associated with changes in arrest rates within MSAs. Changes in the proportion of foreign-born Puerto Ricans are associated with highly significant increase in violent arrest ($p<0.001$) and an increase in total arrests ($p<0.05$). In contrast, native-born Puerto Ricans are associated with decreases in total arrest rates ($p<0.05$) and violent crime rates ($p<0.01$). For those in the 'other Hispanic' category, immigrants are uniformly associated with decreases in total arrest rates ($p<0.001$), violent crime rates ($p<0.01$), and property crime rates ($p<0.001$); in contrast, changes native-born 'other Hispanic' categories are significantly only with a decrease in property crime rates ($p<0.01$).

The results from bivariate regressions using MSA-level fixed effects indicate two general empirical trends: (1) Overall, changes in arrest patterns within MSAs are associated with influxes of immigrants that vary by racial groups group. (2) Among ethnic subgroups, changes in native and foreign-born Hispanic populations are associated with different arrest outcomes. These results indicate that immigration is a significant predictor of arrest, but immigration results vary by race and ethnic classification. In the next section, I introduce measures of segregation and controls to rigorously test and contextualize these results.

Results from Fixed Effect Modeling

The bivariate regression models given above suggest that changes in immigrant populations have significant effects on arrest patterns within MSAs, but vary by race and Hispanic ethnicity. These bivariate results lack controls that may also explain results, but also result from a simplified error structure. Finally, these results do not take historic African American segregation into account as a predictor of arrest.

In this section, controls for MSA population and relative deprivation are incorporated into the regression framework, fixed effects for both year and MSA are incorporated into regression models, and measures of African American segregation are added. These results are presented first for native-born and immigrant population variables. Finally, arrest rates that examine Hispanic ethnic origin and immigration status are presented.

It is also important to note that both one- and two-way fixed effects models are used in analysis. In one-way fixed effect models, MSA fixed effects are used to remove time-invariant MSA-level effects that impact regression results. In two-way fixed effect models, MSA and year fixed effects are used to remove both MSA and period-specific effects which impact regression results. Hence, when presenting results from one and two-way fixed models, I essentially am presenting results with and without period effects. I do this because immigration during the period from 1980-2000 led to almost uniform increases in MSA immigrant populations during the period. Removing invariant, period-specific effects in regression models tests changes in race/ethnic groups within MSAs affected arrest rates during time of study, but this removal has costs of increasing standard errors and removes traits of immigrants that may decrease crime which lie outside the period of study. In contrast, using only one-way fixed effects for MSAs decreases standard errors and allows for invariant, non-period effects of immigration to be measured, but does fully test how period-specific changes in race/ethnic groups within MSAs affect arrest rates. I include both one and two-way effects models to more closely examine how changes in race/ethnic populations predict changes in arrest within MSAs.

Models including Immigration, Race, African American Segregation

Tables 2.6-2.8 present results using fixed effect error components that control for year and MSA level. At the MSA level, a Hausman specification test rejected a random effects model in favor of a fixed effect model. Year fixed effects terms were also found to be significant. The two-way fixed effects presented in these tables show relatively large standard errors, but substantially diminish the possibility of bias resulting from unobserved, time-invariant effects for year and MSA.

As discussed in the methods section above, measures of African American segregation at the MSA-level are on the dimensions of social isolation, dissimilarity, and spatial proximity. The results below present these measures of segregation from the U.S. Census when native and foreign-born racial groups are estimated as co-predictors of arrest.

Table 2.6 presents regression models predicting changes in arrest rates for total arrest rates within MSAs. Among Asians and Hispanic immigrants, there was found to be no correlation between changes in population and changes in arrest. This finding is consistent with Reid et al.'s (2005) findings using cross-sectional data, and it indicates that increases in percentages of immigrants do not correlate with increased arrest rates in MSAs. Black immigrants, in contrast, were found to be associated with increased arrest rates. Interestingly, among measures of African American segregation, social isolation was found to be a significant predictor of crime. When changes in Hispanic, Latino, and black immigrant populations were taken into account, a one point increase in the social isolation index was found to be associated with an aggregate increase of 75 arrests per 100,000 population ($p<0.001$). The co-significance of black immigrants and social isolation with increases in total arrest rates is consistent with analysis by Parks et al. (2005), suggesting that arrest functions as a mechanism of social control.

For native-born populations, increases in native Hispanic populations are associated with a significant decline ($p<0.05$ for baseline and social isolation, $p<0.01$ for social dissimilarity and spatial proximity) in total arrest rates. Social isolation is also a significant predictor of increased arrest rates ($p<0.05$). It is interesting to note that, despite the ~95% correlation between native and immigrant Hispanic groups, changes in the proportion of native-born Hispanics are associated with significant declines in arrest, while no significant correlations is found among Hispanics.

Table 2.7 presents regression models predicting changes in arrest rates for violent crimes within MSAs. A positive but marginally significant correlation exists between changes in Hispanic immigrants and violent arrest rates. However, among both native and immigrant racial groups, changes in population do not correlate with changes in violent crime. While these results are not consistent with the findings of Sampson et al. (2005), it should be noted that large standard errors and the low frequency of violent crime may lead to type II errors in analysis.

Table 2.8 presents regression models predicting changes in arrest rates for property crimes within MSAs. Increases in native-born Hispanic populations are marginally associated with decreases in property arrest rates. However, property arrest rates are not associated with changes among both native and foreign-born populations. A correlation in social isolation is found to be a significant, positive association ($p<0.05$) when examining foreign-born racial groups. Lack of correlation between changes in foreign born population and property arrest rates differs from bivariate regression results presented in Tables 2A and 2B. It is possible that large standard errors may lead to type II errors; however, it should also be noted that no evidence is found to suggest that changes in immigrant racial groups within an MSA increase property crime.

In presenting the models above, shifts in immigrant populations remain largely uncorrelated with changes in total, violent, and property arrest rates within MSAs. Among black immigrants, increases in black immigrant populations are associated with an increase in total arrest rates. This significance is particularly strong when measures of African American social isolation within MSAs are also introduced and are consistent with measures of using arrest as a measure of social control of African American communities. The results occur despite the general lower rates of incarceration of immigrants relative to non-immigrants observed in U.S. census data (Raumbaut et al. 2006). Given that social isolation is a measure of interracial contact of African Americans with other racial groups, these results suggest that a lack of integration for native-born blacks and assimilation of black immigrants is different relative to other racial groups.

For total arrest rates and property crimes, increases in native-born Hispanic groups are associated with significant declines in total arrest rates ($p<0.05$) and property arrest rates ($p<0.10$). These correlations provide some evidence that increases in Hispanic populations may be associated with declines in arrest rates within MSAs. However, as prior research has suggested, arrest rates (as a proxy of crime) should negatively correlate with immigrant groups. Findings by Sampson et al. (2005) and Reid et al. (2005) suggest that differences in crime may vary by ethnic origin. To further examine if variances exist across ethnic groups within Hispanic origin, I use the U.S. Census categories of Mexican, Cuban, Puerto Rican, and Other Hispanic to test if arrest varies by ethnic subgroups.

Hispanic Ethnic Origin

As discussed above, segmented assimilation theory has argued that immigrant groups will differentially assimilate into mainstream society based on racial classification. Empirical analysis by Lieberson (1980) empirically demonstrated that South, Central, and Eastern (SCE) European immigrants experienced intergenerational declines in residential segregation, gains in educational attainment, and upward socially mobility in a sample of major U.S. cities between 1880-1960; in contrast, African Americans experienced little decline in segregation, lack of educational attainment, and upward mobility. Work by Mary Waters (1996, 1999) has found that West Indies immigrants in the U.S. experience lack of opportunity and discrimination that leads intergenerational assimilation into African Americans. Waters has suggested that ethnic identities are primarily optional for white immigrants who have experienced assimilation into mainstream U.S. culture.

Work by Portes and colleagues (Portes et al. 2005; Portes and Hao 2004; Portes and Rumbaut 2001; Portes and Zhou 1993, 1996) has utilized data from Hispanic immigrants to determine assimilation patterns among immigrants. Among ethnic groups, Portes et al (2005) find that Cubans have higher relative incomes and educational attainment, while West Indies and Haitian immigrants have lower education, income, and relatively higher incarceration rates. Portes and Hau (2004) examine Asian and Hispanic immigrants, finding that Mexican immigrants often wind up in inner city areas and experience relatively low educational achievement outcomes in the second generation. Portes and Raumbaut (2001) have found that ethnic origin significantly alters outcomes of immigrant groups, with differential outcomes varying by an immigrant's race and ethnic status. Hispanic immigrants are found to experience segmented assimilation based on racial classification systems that individuals fall in.

Empirical research into differential incarceration rates has found that Hispanic incarceration rates vary significantly by incarceration status. Sampson et al. (2005) report that first-generation Mexican immigrants in Chicago are associated with significantly lower violent crime rates. Raumbaut et al. (2006) find that Hispanic immigrants have uniformly lower incarceration rates relative to non-immigrants, with lower rates for Mexican Hispanics and higher rates among Latinos from Puerto Rico and Caribbean locales. Similar findings are found for incarceration rates in U.S. Census data by economists Butcher and Phiel (2006), though these authors argue that laws increasing criminal sanctions and deportations create a "deterrence effect" that uniformly reduces crime among foreign-born populations relative to native-born populations.

Using IPUMS data, it is possible to examine how changes in Hispanic populations of Mexican, Puerto Rican, Cuban and 'other Hispanic' [largely Central and South American] ethnic origin correlate with changes in arrest rates. Tables 2.9-2.13 present one and two way fixed effect models with separate estimations for native and foreign born immigrant ethnic groups. These separate models allow for comparison of ethnic origin as a function of immigration status.

In the Tables presented in this section, I provide results for MSA [one-way] and MSA and Year [two-way] fixed effect models. The results are presented because of issues specifically related to the influx of Hispanics in the U.S. between 1980-2000. While immigration of Puerto Ricans and Cubans has had historical associations before 1980, a rapid increase of Mexican and Central/South American Hispanics has occurred from 1980-2000. At present, Mexican (58% of

Hispanics), Puerto Rican (10% of Hispanics), and Cuban (4% of Hispanics) ethnic origins comprise the largest Hispanic subgroups in the U.S. (Landale and Oropresa 2007). Year fixed effects control for large, positive increases in Mexican and other Hispanic populations occurring from 1980-2000. This may lead to better controls for period effects that are unobserved and invariant across observations, but it also eliminates potential historical issues associated with crime. If the immigration effects hypothesized by Sampson (2006), for example, have an invariant period effect in reducing crime rates between 1980-2000, the year fixed effects may cancel out immigration effects.

Table 2.9 and Table 2.10 present models where changes in the proportion of Hispanic ethnic subgroups predict total arrest rates in MSAs. For the models presented in Table 2.9 using year and MSA-level fixed effect terms, no significant effects for race are found at the $p<0.05$ level. However, a highly significant ($p<0.01$) correlation is found between social isolation and arrest; across the estimated models, a one point increase in social isolation is associated with 65 to 75 arrests per 100,000 population. For the models estimated in Table 2.10 using MSA fixed effects only, social isolation becomes much less significant (significant at the $p<0.05$ level), while Hispanic ethnic subgroups are associated with varying rates of significance. Among native-born Hispanics, statistically significant declines in total arrest rates are associated with increases in the proportion of Puerto Ricans ($p<0.01$) and 'other' Hispanics ($p<0.05$) living within an MSA. Among foreign-born ethnic groups, an increase in the proportion of Mexican immigrants is associated with a significant decline in total arrest rates ($p<0.05$).

Table 2.11 and Table 2.12 present regression output for how changes in Hispanic ethnic subgroups predict changes in violent arrest rates. Table 2.11 presents output with fixed effect error terms for both year and MSA level. With two-way fixed effects, no significant effects were found for the social isolation index. Among Hispanic ethnic subgroups, an increase in the proportion of Puerto Ricans is associated with an increase in violent crime rates (*$p<0.05$*). Table 2.12 contains one-way fixed effect models at the MSA level only, with similar results. Social isolation is not found to be a significant predictor of violent crime arrest, while increases in the proportion of Puerto Rican immigrants are associated with a highly significant increase in violent arrest rates (*$p<0.001$*). Interestingly, increases in the proportion of native-born Puerto Rican Hispanics are associated with a marginally significant decline ($p<0.10$) in violent arrest rates.

Table 2.13 and Table 2.14 present regression output for how changes in Hispanic ethnic subgroups predict changes in property arrest rates. For the models with year and MSA-level fixed effect rates presented in Table 2.13, no significant correlations were found for changes in the proportion of Hispanic ethnic subgroups. For all models estimated, marginally significant associations (*$p<0.10$*) were observed for increases in the segregation index and property arrest rates. This suggests a weak association between changes in black social isolation and property arrest rates. For the models presented in Table 2.14 that contain MSA-level fixed effects only, no significant associations between black social isolation and property are observed. However, the one-way fixed-effect models yield significant results for racial groups. One-way fixed effect models, however, yield positive associations between Hispanic ethnic subgroups and property arrest rates. Among native-born subgroups, an increase in the proportion of ‘other’ Hispanics is associated with a significant (*$p<0.01$*) decline in property arrest rates. Among immigrant groups, a highly significant association (*$p<0.001$*) was found predicting that a one point percentage increase in Mexican immigrants was associated with a decline of 71 property arrests per 100,000; no significant effect was found among native-born Mexican Hispanics, which is consistent with Sampson et al.’s (2005) findings. Interestingly, a marginally significant association ($p<0.10$) between increases in the proportion of Puerto Rican immigrants and increases in property arrest rates was observed; though not significant, native-born Puerto Rican Hispanics are associated with decreases in property arrest rates.

The results of one-way and two-way fixed effects models presented above provide interesting insights into the general issues associated with immigration and segregation. In the period from 1980-2000, measures of black social isolation declined by an average of 10% in MSAs (Iceland et al. 2004), while the proportion of Hispanics in the U.S. population grew from 4% in 1980 to 13% in 2000 (Landale and Oropesa 2007). Evidence for an effect of social isolation on total arrest and property rates within MSAs occurs when both year and MSA fixed effect terms are added, but are not significant when one-way fixed effects are calculated. This suggests that, when controlling for effects of social isolation, unobserved, invariant period effects lead to type I (false-negative) errors in hypothesis testing of social isolation. In contrast, changes in the proportion of Hispanic subgroups are more frequently significant when fixed effect error components at the MSA-level only are utilized relative to both year and MSA level. The historic increases in Hispanic populations from 1980-2000 are time-dependent and associated with the non-random characteristics and issues these populations face. Such issues include a response to increased threat of deportation for immigrants relative to native-born populations (Butcher and Phiel 2007), the formation of ethnic enclaves and niches that socially impact individual behaviors (Waldinger 1996), and social response [through discrimination or social control] possessed by specific ethnic subgroups due to their classification by native-born populations. As a result, including fixed-effect terms for year may cancel out period-specific effects that lead to correlations between changes in subgroup populations and arrest rates.

The above analysis also suggests usage of year and MSA fixed effect error components may substantively alter findings. However, the models presented above estimate the separate effects of Hispanic subgroups. To examine how immigrant subgroups may separately impact arrest rates, I estimate the effects of Mexican, Cuban, and Puerto Ricans subgroups on arrest rates. As Landale and Oropesa (2007) note, these groups represent approximately 75% of Hispanics immigrants residing in the U.S. In the sample, the proportion of Mexican, Cuban, and Puerto Rican Hispanics within an MSA were not found to have collinearity sufficient to substantively alter results in analysis.

Table 2.15 and Table 2.16 present results on crime rates using, respectively, two- and one-way fixed effect models. Table 2.15 reports results for Asian and Hispanic ethnic subgroups using year and MSA fixed effects. In models with U.S.-born populations, African American social isolation positively correlates with increases in total arrest rates ($p<0.05$). In models with immigrant variables, social isolation is found to be a significant positive predictor for both total arrest rates ($p<0.001$) and property arrest rates ($p<0.05$). Among U.S.-born and foreign-born groups, an MSA's proportion of foreign-born Puerto Ricans is associated with an increase in violent arrest rates ($p<0.001$). An increase in the proportion of Mexican Hispanics within an MSA is also associated with a decline in property arrest rates ($p<0.05$). These results suggest, generally, that including both year and MSA fixed effects shows statistical significance for measures of black isolation in predicting arrest, but relatively little correlation is observed between Hispanic ethnic subgroups and arrest rates.

Table 2.16 presents results where fixed effects at the MSA-level only [e.g., 'one-way'] were used. Among all models tested, no significant relationship was observed for MSA black isolation and arrest rates at the MSA level. For total arrest rates, U.S.-born Puerto Rican

($p<0.01$) and foreign-born Mexican Hispanics ($p<0.05$) were associated with declines in arrest; in contrast, Puerto Rican-born Hispanics were associated with a significant increase in total arrest ($p<0.001$). For violent arrest rates, Puerto Rican-born Hispanics were associated with increase in total arrest ($p<0.001$). For property arrest rates, U.S. born Asians and foreign-born Mexican immigrants were associated with decreases in arrest rates ($p<0.001$); Puerto Rican-born Hispanics were associated with increases in arrest ($p<0.01$). In all models, no correlation was found between changes in the proportion of both immigrant and U.S.-born Cuban Hispanics. In all models, a highly significant correlation ($p<0.001$) was also observed between increases in the proportion of individuals living below the poverty line and increases in arrest rates.

Conclusion

This paper has examined Sampson and colleagues' (Sampson et al. 2005; Sampson 2006) assertion that immigration has influenced arrest in the U.S., but it is contextualized in the framework of segmented assimilation theory and spatial segregation of African Americans. Using panel data for 112 U.S. MSAs, fixed effect models suggest that changes in the proportion of a particular race and ethnic population of an MSA predict changes in arrest rates. Bivariate regression models find that immigrants generally and particular immigrant race & ethnic groups correlate with decreases in total arrest and property arrest rates. Results from estimates of one-way fixed effect models with additional controls also suggest that increases in Mexican-born Hispanic, foreign-born 'other' Hispanics and foreign-born Asians are associated with declines in property arrest rates. These results contrast with changes in U.S.-born populations, where little or no effects for comparative populations are observed.

While results suggest that immigration changes have correlated with changes in arrest within U.S. MSAs, it is equally import to note that these correlations vary across race and ethnic groups. Results from the two-way fixed effect models in Tables 2.9 2.11, and 2.13 suggest that increases in foreign born black immigrants are associated with increases in total arrest rates ($p<0.05$), while increases in foreign-born Hispanics are associated with marginally significant increases in violent arrest rates. Results from Table 2.16 suggest that increases in Puerto-Rican born immigrants are associated with increases in total arrest rates ($p<0.001$), violent arrest rates ($p<0.01$), and property arrest rates ($p<0.001$); in contrast, increases in U.S.-born Puerto Rican-Hispanics are associated with declines in total arrest rates ($p<0.001$) and non-significant decreases in violent and property arrest rates. These findings are consistent with prior research on using arrest as a mechanism for the social control of blacks (Parker et al. 2005) and research on segmented assimilation of immigrant groups (Portes and Rambaut 2001; Rambaut et al. 2006). While some economists such as Butcher and Phiel (2006) have argued that threat of deportation deters immigrants from delinquency relative to U.S.-born populations, the results presented above suggest that arrest varies by immigrant race and ethnic status. These findings are consistent with differential associations observed in studies by Sampson et al. (2005) and Reid et al. (2005) where different effects by race and ethnic status of immigrants are observed.

The usage of MSA and year fixed effect models also presented differential findings. Models with both MSA and year fixed effects found that increase in social isolation significantly predicted increases in total arrest ($p<0.001$) and property arrest rates ($p<0.05$) when variables for foreign-born race and ethnic Hispanic groups were used. However, social isolation was not found to be significant in the one-way fixed effect at the MSA-level were used. Given that a nearly universal decline in African American segregation occurred between 1980-2000 (Iceland et al 2004), the significance of social isolation in the two-way fixed effect models suggests that eliminating invariant period effects is needed to find the effects of segregation on arrest. A decline in social isolation of African Americans in MSAs may also be a factor in explaining general decreases in crime and arrest observed between 1980-2000.

In contrast to segregation, race and ethnic variables seem to be generally more significant when MSA-level fixed effects are only used. The one-way fixed effect models presented in Tables 2.10, 2.12, 2.14, and 2.16 show significance for ethnic Hispanics and foreign-born Asians that are not present in the two-way fixed effect models presented in Tables 2.09, 2.11, 2.13, and 2.15. These results generally suggest the significance of invariant period effects for race and ethnic Hispanic groups. Given the rapid increase in Hispanics from 1980-2000 in the U.S. population, issues such as laws mandating deportation of immigrants convicted of crimes, characteristics/culture unique to foreign-born groups, and economic niches filled by ethnic groups may be examples of invariant period effects specific to these groups.

Hence, this work finds that both segregation and immigration impact overall MSA-level arrest rates. For segregation, the degree to which African Americans were exposed to other racial groups was found to be the significant predictor of changes in arrest rates. For immigrant groups, changes in arrest rates were found to vary significantly by racial classification and ethnic subgroup. An increase in the proportion of Asian and Mexican immigrants decreased arrest rates; in contrast, increases in Puerto Rican and black immigrants were associated with increases in arrest rates. If arrest is a measure of assimilation into U.S. norms, these differences across race and ethnic groups may point towards segmented assimilation that is consistent with general findings by Portes and Colleagues (Portes and Hao 2004; Portes and Rumbaut 2001; Portes and Zhou 1993, 1996) and Mary Waters (1994, 1996, 1999).

It should also be noted that, while this work provides some of the first longitudinal tests for the effects of segregation and immigration on arrest in existing research, much is lacking in empirically validating the issues proposed above. Incomplete representation of individuals residing within MSAs and incomplete arrest data greatly reduce MSA sample size. Likewise, missing data also may lead to wide variances in arrest rates. The IPUMS data also only tracks first-generation immigrants into the U.S. and lacks data to estimate individual propensities across immigrant groups. Further research using state data, increased number of time periods, and better measurement of race and ethnic origin would allow for more precise and accurate statistical analysis. These critiques provide a framework for future research.

Table 2.1: Means and standard deviations for Metropolitan Statistical Areas (MSAs) in sample

Variable	1980	1990	2000	Composite
Criminal Justice Variables				
Total Arrest Rate	4968.3	5524.5	4854.9	5215.7
	(2739.1)	(2096.3)	(2419.5)	(2331.5)
Violent Crime Rate	208.2	265.1	214.3	230.6
	(125.7)	(178.8)	(150.5)	(152.6)
Property Crime Rate	1218.7	1403.0	1088.5	1242.4
	(437.1)	(567.3)	(532.3)	(533.3)
Racial Classification				
Proportion Black	9.46	9.29	9.57	9.43
	(8.76)	(8.63)	(9.02)	(8.8)
Proportion White	80.4	76.3	69.6	75.3
	(14.5)	(16.9)	(19.6)	(17.7)
Proportion Hispanic	7.5	10.3	15.1	11.1
	(1.22)	(14.9)	(17.4)	(15.3)
Proportion Asian	1.98	3.34	4.73	3.42
	(6.89)	(6.77)	(8.57)	(7.50)
Proportion Native American	0.59	0.71	0.91	0.73
	(0.72)	(0.97)	(1.05)	(0.95)
Hispanic Ethnic Subgroup				
Percent Mexican Foreign Born	4.23	5.34	6.61	5.45
	(8.23)	(9.82)	(9.71)	(9.37)
Percent Puerto Rican Foreign Born	0.73	0.47	0.59	0.59
	(1.81)	(0.89)	(1.10)	(1.28)
Percent Cuban Foreign Born	0.061	0.081	0.061	0.069
	(0.157)	(0.18)	(0.12)	(0.15)
Percent Other Hispanic Foreign Born	0.95 (1.74)	0.80 (1.15)	2.04 (2.89)	1.25 (2.10)
Immigation Variables				
Percentage Foreign Born	5.56	9.05	12.2	9.08
	(4.89)	(7.82)	(9.76)	(8.26)
Immigrant Race Variables				
Percentage Foreign-Born Asian Immigrant	0.89 (1.51)	2.00 (2.58)	2.93 (3.72)	2.03 (2.95)
Percent Foreign Born White Immigrant	2.68 (1.92)	2.93 (1.82)	2.83 (1.81)	2.83 (1.84)
Percent Black Foreign Born	0.19	0.42	0.54	0.83
	(3.4)	(0.89)	(1.06)	(0.78)
Percentage Foreign-Born Hispanic Origin	1.74 (3.12)	3.61 (5.30)	5.81 (6.63)	3.80 (5.56)
Hispanic Ethnic Subgroup				
Percent Mexican Foreign Born	1.22	2.39	4.084	2.62
	(2.96)	(4.68)	(5.65)	(4.75)

Percent Puerto Rican Foreign Born	0.017 (.042)	0.41 (0.870)	.040 (0.82)	0.29 (0.73)
Percent Cuban Foreign Born	0.19 (0.81)	0.157 (0.61)	0.097 (0.45)	0.147 (0.635)
Percent Other Hispanic Foreign Born	0.363 (0.77)	0.69 (1.37)	1.25 (2.03)	0.78 (1.54)
African American Segregation Measures				
Dissimilarity	0.619 (0.148)	0.549 (0.152)	0.491 (0.15)	0.550 (0.160)
Isolation Index	0.403 (0.254)	0.33 (0.241)	0.313 (0.24)	0.347 (0.245)
Concentration Index	1.21 (0.208)	1.17 (0.193)	1.16 (0.19)	1.18 (0.186)
Population Variables				
Log Population	12.82 (1.10)	12.94 (1.12)	13.07 (1.13)	12.95 (1.11)
Percentage of Population Aged 15-29	28.49 (4.59)	24.2 (4.90)	22.6 (4.64)	24.9 (5.31)
Percentage Of population Above Age 65	11.2 (4.22)	12.34 (4.2)	11.67 (2.52)	11.76 (3.72)
Measures of Relative Deprivation/Affluence				
Percentage of Individuals with Incomes above 500% of poverty level	17.8 (5.24)	21.3 (7.85)	24.33 (8.19)	21.3 (7.68)
Percentage of Residing Below Poverty Line	15.06 (4.96)	16.41 (6.57)	15.89 (5.86)	15.85 (5.92)
Unemployment Rate	7.1 (2.45)	6.9 (2.24)	6.9 (2.57)	6.95 (2.33)_
Proportion of Population over 25 and with college Degree	9.11 (2.59)	12.4 (4.08)	14.9 (17.9)	12.27 (4.51)
Percentage of Population over 25 and lacking No High School Degree	17.67 (4.64)	12.1 (4.39)	9.60 (3.89)	12.97 (5.32)
Percentage of Single Mothers in Population	4.00 (0.81)	4.63 (1.04)	2.08 (0.56)	3.60 (1.38)
Sex Ratio (males per 100 females in population)	95.97 (5.95)	98.95 (9.21)	100.02 (6.42)	65.4 (16.59)
Divorce Rate (per 1,000 individuals)	50.48 (11.4)	67.2 (13.2)	75.98 (14.5)	98.5 (7.65)
N	79	106	91	276

Table 2.2: Bivariate fixed-effect relationship between changes in MSA arrest rates and native born populations, by race

	Native Born Hispanic	Native Born White	Native Born Asian	Native Born Black	Native Born Native American	Native Born Other Race
Total crime rate	-207.9**	36.7	183.1	22.5	493.6	-3612.0+
	(70.1)	(26.2)	(181.0)	(132.4)	(922.4)	(2158.5)
Violent Crime Rate	-0.879	1.58	11.8	0.3 1	-16.3	-100.9
	(4.53)	(1.35)	(10.4)	(10.1)	(59.2)	(142.6)
Property Crime crate	-58.3***	18.3***	-140.4***	11.1	121.4	-1179.5+
	(17.1)	(5.14)	(40.3)	(36.3)	(221.2)	(644.4)

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.3: Bivariate fixed-effect relationship between changes in MSA arrest rates and immigrant populations, by race

	Percent Immigrant	Immigrant Hispanic	Immigrant White	Immigrant Asian	Immigrant Black
Total crime rate	-2623.6	-54.36	29.6	-52.1	314.7*
	(3655.8)	(60.04)	(252.0)	(81.92)	(166.4)
Violent Crime Rate	375.0*	5.89*	-9.80	9.01*	10.3
	(192.2)	(2.98)	(17.01)	(4.41)	(17.01)
Property Crime crate	-2393.9***	-32.5*	9.47	-56.5***	-89.3*
	(717.8)	(13.36)	(63.02)	(14.3)	(44.75)

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.4: Bivariate fixed-effect relationship between changes in MSA arrest rates and Hispanic ethnic subgroup

	Percent Mexican	Percent Cuban	Percent Puerto Rican	Other, Hispanic
Total crime rate	-38.98	-361.4	-65.3	-183.3**
	(41.72)	(680.4)	(290.3)	(60.37)
Violent Crime Rate	5.98*	23.94	19.03	-7.26+
	(2.56)	(42.37)	(18.02)	(3.82)
Property Crime Rate	-18.33+	165.18	-15.7	-65.40***
	(10.33)	(169.3)	(72.4)	(14.60)

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.5: Bivariate fixed-effect relationship between changes in MSA arrest rates and Hispanic origin, by immigrant status

	Percent Mexican Native	Percent Mexican Immigrant	Percent Cuban Native	Percent Cuban Immigrant	Percent Puerto Rican Native	Percent Puerto Rican Immigrant	Percent Other Hispanic Native	Percent Other Hispanic Immigrant
Total crime rate	34.72	-128.6+	1684.8	-592.9	-534.6*	412.1*	-334.2 ***	-9.2
	(82.8)	(73.8)	(2932.7)	(744.8)	(226.7)	(207.8)	(85.2)	(148.8)
Violent Crime Rate	16.52***	5.68	347.7+	-3.96	- 36.70**	42.5***	-15.6**	2.32
	(4.99)	(4.61)	(180.8)	(46.4)	(14.07)	(12.6)	(5.01)	(9.28)
Property Crime Rate	-6.65	-45.31*	581.7	176.94	-34.7	41.87	- 92.6***	-105.6**
	(20.64)	(18.2)	(730.6)	185.6	(57.4)	(52.4)	(22.0)	36.2

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.6: Two-way fixed-effect coefficients and standard errors predicting total arrest rates within MSAs.

	Baseline Model	Dissimilarity Index	Isolation Index	Spatial Proximity	Baseline Model	Dissimilarity Index	Isolation Index	Spatial Proximity
Racial Classification								
Native-Born Racial Groups								
Proportion Native-Born Black	-	-	-	-	128.8	106.6	42.9	111.7
					(148.4)	(151.1)	(160.1)	(157.5)
Proportion Native-Born White	-	-	-	-	-	-	-	-
Proportion Native-Born Hispanic	-	-	-	-	-272.0**	-250.7*	-259.5*	-265.2**
					(102.0)	(105.0)	(102.8)	(97.8)
Proportion Native-Born Asian	-	-	-	-	-79.3	-72.8	-128.3	-72.0
					(289.9)	(281.5)	(285.3)	(299.0)
Racial Immigrant Groups								
Percentage Foreign Born								
Percentage Foreign-Born Asian Immigrant	-36.8	-48.2	-80.5	-31.1	-	-	-	-
	(122.9)	(138.9)	(123.8)	(129.6)				
Percent Foreign Born White Immigrant	-	-	-	-	-	-	-	-
Percentage Foreign-Born Hispanic Origin	-65.3	-46.3	-82.6	-54.9	-	-	-	-
	(103.8)	(102.0)	(102.6)	(108.7)				
-Percent Black Foreign Born	605.4*	540.1+	642.7*	592.7+	-	-	-	-
	(306.1)	(297.7)	(286.9)	(330.6)				
African American Segregation Measures								
Dissimilarity	-	49.6	-	-	-	30.9	-	-
		(31.5)				(32.0)		
Isolation Index	-	-	75.0***	-	-	-	58.7*	-
			(23.1)				(23.9)	
Concentration Index	-	-	-	30.1	-	-	-	12.9
				(20.8)				(23.4)
Adult Sex Ratio	-63.5+	-57.7	-52.4	-67.8+	-82.5*	-76.5+	-71.3+	-83.1*
	(36.6)	(38.4)	(39.0)	(38.5)	(37.8)	(39.2)	(38.6)	(37.5)
Divorce Rate	50.0	41.8	-35.1	42.8	-73.7	-63.6	-68.6	-71.9
	(252.4)	(269.5)	(246.2)	(257.0)	(187.8)	(194.8)	(190.0)	(196.9)
Percentage of Population Below	64.2	64.0	38.4	61.5	103.3	101.3	104.4	101.8

Poverty Line	(75.1)	(75.3)	(146.5)	(80.7)	(85.9)	(81.1)	(83.5)	(83.2)
Unemployment Rate	12.4	13.6	60.2	19.9	106.3	101.3	111.6	107.2
	(145.0)	(138.2)	(73.6)	(143.9)	(136.0)	(134.3)	(142.4)	(136.9)
Constant	9,572.0**	6,522.5	6,744.5+	6,469.1	12541.2**	10,372.7+	10,168.0*	11,182.3*
	(3,635.2)	(4,319.9)	(4,047.9)	(4,635.0)	(4670.3)	(5,383.3)	(4,904.5)	(4,900.3)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.192	0.204	0.226	0.199	0.221	0.226	0.240	0.222

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.7: Two-way fixed-effect coefficients and standard errors predicting violent arrest rates within MSAs.

	Baseline Model	Dissimilarity Index	Isolation Index	Spatial Proximity	Baseline Model	Dissimilarity Index	Isolation Index	Spatial Proximity
Racial Classification								
Native-Born Racial Groups								
Proportion Native-Born Black	-	-	-	-	1.6 (9.0)	1.9 (8.5)	-0.9 (9.0)	1.0 (9.7)
Proportion Native-Born White	-	-	-	-				
Proportion Native-Born Hispanic	-	-	-	-	-2.0 (6.8)	-2.3 (7.0)	-1.6 (7.2)	-1.8 (7.1)
Proportion Native-Born Asian	-	-	-	-	13.3 (15.9)	13.2 (16.4)	11.8 (16.0)	13.5 (16.2)
Racial Immigrant Groups								
Percentage Foreign Born								
Percentage Foreign-Born Asian Immigrant	8.2 (5.7)	8.1 (5.7)	7.4 (5.7)	8.3 (6.2)	-	-	-	-
Percent Foreign Born White Immigrant	-	-	-	-	-	-	-	-
Percentage Foreign-Born Hispanic Origin	10.5+ (5.5)	10.5* (5.4)	10.1+ (5.7)	10.8+ (5.7)	-	-	-	-
Percent Black Foreign Born	-8.4 (17.6)	-8.6 (19.8)	-7.7 (20.6)	-8.8 (20.8)	-	-	-	-
African American Segregation Measures								
Dissimilarity	-	0.2 (1.9)	-	-	-	-0.5 (1.9)	-	-
Isolation Index	-	-	1.4 (1.4)	-	-	-	1.7 (1.3)	-
Concentration Index	-	-	-	0.9 (1.5)	-	-	-	0.4 \(1.6)
Adult Sex Ratio	-2.6 (2.5)	-2.6 (2.6)	-2.4 (2.6)	-2.7 (2.6)	-2.1 (2.5)	-2.2 (2.6)	-1.8 (2.5)	-2.1 (2.6)
Divorce Rate	13.5 (15.7)	13.5 (15.9)	11.9 (16.1)	13.3 (15.9)	-4.5 (12.2)	-4.6 (12.2)	-4.3 (12.1)	-4.4 (12.0)
Percentage of Population Below Poverty Line	10.1+ (5.7)	10.1+ (5.6)	10.0+ (5.7)	10.0+ (5.8)	10.4 (7.0)	10.5 (7.0)	10.5 (7.1)	10.4 (7.0)
Unemployment Rate	-10.1	-10.1	-9.6	-9.9	-6.5	-6.4	-6..3	-6.4

	(8.1)	(8.0)	(8.3)	(8.0)	(7.2)	(7.5)	(7..3)	(7.7)
Constant	183.1	173.7	130.9	93.2	305.2	336.9	235.8	260.6
	(206.8)	(241.5)	(225.9)	(272.9)	(271.2)	(303.1)	(288.4)	(312.6)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.220	0.220	0.223	0.221	0.202	0.202	0.207	0.203

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.8: Two-way fixed-effect coefficients and standard errors predicting property arrest rates within MSAs.

	Baseline Model	Dissimilarity Index	Isolation Index	Spatial Proximity	Baseline Model	Dissimilarity Index	Isolation Index	Spatial Proximity
Racial Classification								
Native-Born Racial Groups								
Proportion Native-Born Black	-	-	-	-	61.1+	58.4+	51.1	57.2+
					(32.4)	(32.5)	(33.0)	(33.9)
Proportion Native-Born White	-	-	-	-				
Proportion Native-Born Hispanic	-	-	-	-	-43.9*	-41.2+	-42.4+	-42.3+
					(21.3)	(23.6)	(21.8)	(21.9)
Proportion Native-Born Asian	-	-	-	-	-69.3	-68.5	-75.0	-67.6
					(57.8)	(59.5)	(59.1)	(55.7)
Racial Immigrant Groups								
Percentage Foreign Born								
Percentage Foreign-Born Asian	-34.4	-37.0	-42.2	-32.9	-	-	-	-
Immigrant	(27.8)	(28.2)	(27.3)	(28.1)				
Percent Foreign Born White Immigrant	-	-	-	-	-	-	-	-
Percentage Foreign-Born Hispanic	-13.4	-9.1	-16.5	-10.7	-	-	-	-
Origin	(20.3)	(20.4)	(20.6)	(20.5)				
Percent Black Foreign Born	-17.9 (57.7)	-32.7	-11.3	-21.2	-	-	-	-
		(64.4)	(64.1)	(64.8)				
African American Segregation Measures								
Dissimilarity	-	11.2	-	-	-	3.8	-	-
		(6.9)				(7.3)		
Isolation Index	-	-	13.3*	-	-	-	6.8	-
			(6.7)				(7.3)	
Concentration Index	-	-	-	7.9	-	-	-	2.9
				(5.8)				(5.9)
Adult Sex Ratio	-15.4+	-14.1	-13.4	-16.6*	-22.4*	-21.7*	-21.1*	-22.5*
	(8.1)	(8.9)	(9.3)	(8.3)	(9.2)	(9.5)	(9.3)	(9.0)
Divorce Rate	23.9	22.1	8.8	22.0	5.5	6.8	6.1	34.0
	(59.4)	(59.8)	(58.4)	(59.5)	(47.5)	(48.4)	(47.5)	(33.1)
Percentage of Population Below Poverty	8.3	8.2	7.6	7.6	18.5	18.2	18.6	5.9

Line	(13.5)	(14.1)	(13.3)	(14.1)	(13.2)	(13.7)	(13.9)	(46.4)
Unemployment Rate	22.9	23.2	27.5	24.9	33.8	33.1	34.4	18.1
	(34.3)	(36.1)	(36.8)	(34.2)	(34.2)	(33.8)	(34.4)	(13.7)
Constant	2,358.9*	1,669.6	1,857.6	1,539.7	2,728.6*	2,461.8+	2,452.2+	2,420.2+
	(1,024.1)	(1,261.0)	(1,182.0)	(1,191.5)	(1,304.1)	(1,491.5)	(1,361.4)	(1,377.8)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.330	0.340	0.348	0.338	0.363	0.364	0.367	0.364

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.9: Two-way fixed-effect results of immigration status, Hispanic ethnic origin, and African American isolation on changes in total arrest rates within MSAs.

	Percent Mexican Native-Born U.S.	Percent Cuban Native-Born U.S.	Percent Puerto Rican Native-Born U.S.	Percent Other Hispanic Native-Born U.S.	Percent Mexican U.S. Immigrant	Percent Cuban U.S. Immigrant	Percent Puerto Rican U.S. Immigrant	Percent Other Hispanic U.S. Immigrant
Racial Classification								
Native-Born Racial Groups								
Percent Mexican Native	67.2 (188.4)	-	-	-	-	-	-	-
Percent Cuban Native	-	-668.34 (2776.0)	-	-	-	-	-	-
Percent Puerto Rican Native	-	-	-382.3 (365.1)	-	-	-	-	-
Percent Other Hispanic Native	-	-	-	-250.1 (191.6)	-	-	-	-
Racial Immigrant Groups								
Percent Mexican Immigrant	-	-	-	-	-134.7 (120.8)	-	-	-
Percent Puerto Rican Native	-	-	-	-	-	-852.8 (1813.9)	-	-
Percent Other Hispanic Native	-	-	-	-	-	-	334.0+ (197.9)	-
Percent Other Hispanic Immigrant	-	-	-	-	-	-	-	80.4 (251.5)
African American Segregation Measures								
Isolation Index	74.5** (23.6)	71.6*** (22.54)	64.2** (23.41)	76.9** (25.0)	72.9** (23.1)	68.9** (23.4)	66.3** (23.5)	71.4** (21.4)
Population and Deprivation Control Variables								
Adult Sex Ratio	-41.0 (29.6)	-46.08 (34.9)	-57.6 (37.31)	-41.8 (34.1)	-45.4 (34.9)	-50.2 (33.9)	-61.2 (40.3)	-48.4 (35.1)
Divorce Rate	146.1 (224.3)	118.8 (196.2)	165.8 (186.9)	83.6 (193.3)	-19.6 (211.9)	139.7727 (200.1)	117.6 (201.5)	145.4 (211.2)

Percentage of Population Below Poverty Line	12.7	31.8	56.8	16.2	48.1	40.6	55.9	36.8
	(62.7)	(67.59)	(70.01)	(67.6)	(65.1)	(68.1)	(74.1)	(68.2)
Unemployment Rate	46.1	55.9	31.7	78.5	77.8	46.9	33.2	47.3
	(100.4)	(102.5)	(103.9)	(101.3)	(112.0)	(100.7)	(105.7)	(102.6)
Constant	4,223.1	5133.4	6079.6	5373.5	6162.4	5439.6	6386.068	4990.3
	(3,718.5)	(3752.4)	(4065.5)	(3928.5)	(3792.4)	(3544.9)	(4399.4)	(3706.1)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.205	0.203	0.217	0.238	0.210	0.213	0.214	0.204

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.10: MSA-level fixed-effect results of immigration status, Hispanic ethnic origin, and African American isolation on changes in total arrest rates within MSAs.

	Percent Mexican Native-Born U.S.	Percent Cuban Native-Born U.S.	Percent Puerto Rican Native-Born U.S.	Percent Other Hispanic Native-Born U.S.	Percent Mexican U.S. Immigrant	Percent Cuban U.S. Immigrant	Percent Puerto Rican U.S. Immigrant	Percent Other Hispanic U.S. Immigrant
Racial Classification								
Native-Born Racial Groups								
Percent Mexican Native	-68.3 (168.3)	-	-	-	-	-	-	-
Percent Cuban Native	-	2102.0	-	-	-	-	-	-
		(3024.7)						
Percent Puerto Rican Native	-	-	-629.4**	-	-	-	-	-
			(219.2)					
Percent Other Hispanic Native	-	-	-	-375.7*	-	-	-	-
				(164.7)				
Racial Immigrant Groups								
Percent Mexican Immigrant	-	-	-	-	-212.5*	-	-	-
					(91.5)			
Percent Puerto Rican Native	-	-	-	-	-	-684.3	-	-
						(1581.5)		
Percent Other Hispanic Native	-	-	-	-	-	-	570.3	-
							(169.4)	
Percent Other Hispanic Immigrant	-	-	-	-	-	-	-	-108.3
								(304.6)
African American Segregation Measures								
Isolation Index	44.0 (27.3)	49.61+	39.73	56.3*	43.1+	44.5+	41.41	46.86+
		(26.9)	(26.8)	(26.9)	(25.6)	(26.2)	(27.8)	(26.8)
Population and Deprivation Control Variables								
Adult Sex Ratio	-54.2	-50.5 (36.4)	-72.8+	-37.3	-38.4 (37.8)	-55.0	-78.7+	-45.35
	(35.1)		(38.8)	(36.3)		(37.6)	(44.2)	(38.5)
Divorce Rate	151.7	122.0	106.2	246.7**	225.5*	128.0	59.9	154.2+

	(94.8)	(92.5)	(90.8)	(95.2)	(96.0)	(90.6)	(96.4)	(93.0)
Percentage of Population Below Poverty Line	146.9*	130.5+	148.8	79.5	145.2*	132.3	151.8*	122.5+
	(61.5)	(71.4)	(71.3)	(69.9)	(65.2)	(72.0)	(76.8)	(74.0)
Unemployment Rate	55.0	41.2	5.86	87.4	90.2	38.1	7.56	56.42
	(106.5)	(106.5)	(106.3)	(101.7)	(122.3)	(108.8)	(102.1)	(107.9)
Constant	5549.0	5028.7	8095.2+	3832.8	3560.2	5837.0	8326.2+	4666.8
	(3805.3)	(3869.7)	(4075.6)	(3915.9)	(3967.0)	(3883.3)	(4739.6)	(3998.1)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	No	No	No	No	No
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.058	0.058	0.97	0.151	0.093	0.060	0.090	0.058

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.11: Two-way fixed-effect results of immigration status, Hispanic ethnic origin, and African American isolation on changes in violent arrest rates within MSAs.

	Percent Mexican Native-Born U.S.	Percent Cuban Native-Born U.S.	Percent Puerto Rican Native-Born U.S.	Percent Other Hispanic Native-Born U.S.	Percent Mexican U.S. Immigrant	Percent Cuban U.S. Immigrant	Percent Puerto Rican U.S. Immigrant	Percent Other Hispanic U.S. Immigrant
Racial Classification								
Native-Born Racial Groups								
Percent Mexican Native	15.4 (13.1)	-	-	-	-	-	-	-
Percent Cuban Native	-	190.8 (194.7)	-	-	-	-	-	-
Percent Puerto Rican Native	-	-	-26.76 (27.08)	-	-	-	-	-
Percent Other Hispanic Native	-	-	-	-12.8 (15.35)	-	-	-	-
Racial Immigrant Groups								
Percent Mexican Immigrant	-	-	-	-	5.40 (5.55)	-	-	-
Percent Puerto Rican Native	-	-	-	-	-	-7.83 (112.4)	-	-
Percent Other Hispanic Native	-	-	-	-	-	-	36.65* (15.03)	-
Percent Other Hispanic Immigrant	-	-	-	-	-	-	-	1.75 (10.84)
African American Segregation Measures								
Isolation Index	2.29 (1.46)	2.00 (1.34)	1.23 (1.53)	2.035 (1.44)	1.78 (1.25)	1.76 (1.33)	1.14 (1.60)	1.78 (1.31)
Population and Deprivation Control Variables								
Adult Sex Ratio	-0.43 (1.77)	-1.58 (2.38)	-2.40 (2.69)	-1.38 (2.18)	-1.62 (2.39)	-1.63 (2.44)	-3.26 (3.11)	-1.65 (2.37)
Divorce Rate	-1.09	-8.87	-4.52	-9.66	-2.58	-7.79	-7.82	-7.37

	(14.3)	(10.30)	(10.64)	(10.0)	(12.7)	(11.07)	(11.12)	(12.15)
Percentage of Population Below	4.47	9.81+	10.89*	8.38	8.70	9.34	11.71*	9.34+
Poverty Line	(4.68)	(5.43)	(5.26)	(5.30)	(5.51)	(5.70)	(5.48)	(5.40)
Unemployment Rate	-5.56	-3.96	-5.22	-2.38	-4.54	-3.68	-5.97	-3.77
	(4.81)	(5.07)	(5.52)	(5.79)	(5.43)	(5.07)	(5.37)	(5.10)
Constant	111.7	258.9	353.6073	282.3	251.2	286.4	426.7	282.5
	(217.8)	(235.8)	(270.7)	(244.2)	(234.5)	(239.4)	(322.7)	(236.2)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.232	0.206	0.217	0.224	0.203	0.213	0.235	0.200

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.12: MSA-level fixed-effect results of immigration status, Hispanic ethnic origin, and African American isolation on changes in violent arrest rates within MSAs.

	Percent Mexican Native-Born U.S.	Percent Cuban Native-Born U.S.	Percent Puerto Rican Native-Born U.S.	Percent Other Hispanic Native-Born U.S.	Percent Mexican U.S. Immigrant	Percent Cuban U.S. Immigrant	Percent Puerto Rican U.S. Immigrant	Percent Other Hispanic U.S. Immigrant
Racial Classification								
Native-Born Racial Groups								
Percent Mexican Native	9.10	-	-	-	-	-	-	-
	(10.35)							
Percent Cuban Native	-	326.2	-	-	-	-	-	-
		(200.4)						
Percent Puerto Rican Native	-	-	-41.65+	-	-	-	-	-
			(23.98)					
Percent Other Hispanic Native	-	-	-	-17.31	-	-	-	-
				(13.9)				
Racial Immigrant Groups								
Percent Mexican Immigrant	-	-	-	-	-0.42	-	-	-
					(3.99)			
Percent Cuban Immigrant	-	-	-	-	-	-9.07	-	-
						(108.6)		
Percent Puerto Rican Immigrant	-	-	-	-	-	-	49.40***	-
							(12.0)	
Percent Other Hispanic Immigrant	-	-	-	-	-	-	-	-1.50
								(9.73)
African American Segregation Measures								
Isolation Index	0.47	0.58	-0.37	0.56	0.11	0.090	-0.33	0.12
	(1.43)	(1.43)	(1.56)	(1.57)	(1.44)	(1.53)	1.71	(1.84)
Population and Deprivation Control Variables								
Adult Sex Ratio	-0.86	-1.30	-2.78	-0.70	-1.29	-1.37	-3.74	-1.24
	(2.22)	(2.69)	(2.85)	(2.27)	(2.56)	(2.74)	(3.42)	(2.16)
Divorce Rate	5.74	5.71	5.89	12.96*	8.03	7.75+	1.27	8.11*
	(5.15)	(5.02)	(4.86)	(6.62)	(5.05)	(4.45)	(5.28)	(5.73)

Percentage of Population Below Poverty Line	11.73**	14.9**	15.81**	12.2**	14.40**	14.44**	16.52**	14.31***
	(4.27)	(5.23)	(5.32)	(4.60)	(5.34)	(5.61)	(5.67)	(3.93)
Unemployment Rate	-5.09	-4.31	-6.33	-1.78	-3.65	-3.83	-6.978	-3.58
	(5.69)	(5.70)	(5.95)	(5.96)	(5.97)	(5.51)	(5.63)	(6.21)
Constant	51.85	63.54	284.1	30.1	91.2	102.5	362.8	86.64
	(234.6)	(264.4)	(312.3)	(249.4)	(254.0)	(272.8)	(367.6)	(237.5)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	No	No	No	No	No	No	No	No
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.116	0.058	0.149	0.155	0.102	0.102	0.170	0.102

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.13: Two-way fixed-effect results of immigration status, Hispanic ethnic origin, and African American isolation on changes in property arrest rates within MSAs.

	Percent Mexican Native-Born U.S.	Percent Cuban Native-Born U.S.	Percent Puerto Rican Native-Born U.S.	Percent Other Hispanic Native-Born U.S.	Percent Mexican U.S. Immigrant	Percent Cuban U.S. Immigrant	Percent Puerto Rican U.S. Immigrant	Percent Other Hispanic U.S. Immigrant
Racial Classification								
Native-Born Racial Groups								
Percent Mexican Native	-15.29 (24.93)	-	-	-	-	-	-	-
Percent Cuban Native	-	-205.3 (752.5)	-	-	-	-	-	-
Percent Puerto Rican Native	-	-	21.12 (111.4)	-	-	-	-	-
Percent Other Hispanic Native	-	-	-	-45.33 (33.69)	-	-	-	-
Racial Immigrant Groups								
Percent Mexican Immigrant	-	-	-	-	-15.29 (25.5)	-	-	-
Percent Puerto Rican Immigrant	-	-	-	-	-	56.8 (326.2)	-	-
Percent Other Hispanic Immigrant	-	-	-	-	-	-	8.37 (61.5)	-
Percent Other Hispanic Immigrant	-	-	-	-	-	-	-	-53.5 (50.4)
African American Segregation Measures								
Isolation Index	12.43+ (6.76)	12.15+ (6.64)	12.82+ (6.93)	13.20+ (6.79)	12.43+ (6.41)	12.59+ (6.79)	12.22+ (7.32)	12.98* (6.51)
Population and Deprivation Control Variables								
Adult Sex Ratio	-14.65+ (7.51)	-14.81* (7.54)	-14.16 (8.74)	-14.03+ (7.38)	-14.72+ (7.75)	-14.51+ (7.83)	-15.17 (9.36)	-13.22 (8.11)
Divorce Rate	77.97161 (45.9)	78.04 (46.4)	74.36 (45.3)	71.30 (47.13)	61.75 (53.3)	75.52 (48.97)	77.2 (45.3)	57.42 (50.18)
Percentage of Population Below Poverty	9.11	9.12	8.42	6.53	11.3	9.24	10.26	7.66

Line	(16.6)	(13.9)	(13.2)	(15.0)	(12.9)	(13.62)	(13.7)	(13.6)
Unemployment Rate	25.4	26.01	26.90	29.96	28.3	26.14	25.09	30.51
	(23.96)	(23.7)	(25.38)	(22.92)	(24.9)	(24.09)	(25.07)	(24.5)
Constant	1609.6+	1656.1	1574.5	1630.2	1719.1	1602.1	1663.6	1629.0
	(980.2)	(1013.4)	(1141.2)	(1010.3)	(1058.6)	(1089.1)	(1198.1)	(1119.9)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.337	0.337	0.336	0.355	0.338	0.337	0.336	0.344

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.14: MSA-level fixed-effect results of immigration status, Hispanic ethnic origin, and African American isolation on changes in property arrest rates within MSAs.

	Percent Mexican Native-Born U.S.	Percent Cuban Native-Born U.S.	Percent Puerto Rican Native-Born U.S.	Percent Other Hispanic Native-Born U.S.	Percent Mexican U.S. Immigrant	Percent Cuban U.S. Immigrant	Percent Puerto Rican U.S. Immigrant	Percent Other Hispanic U.S. Immigrant
Racial Classification								
Native-Born Racial Groups								
Percent Mexican Native	-48.7 (33.9)	-	-	-	-	-	-	-
Percent Cuban Native	-	678.7	-	-	-	-	-	-
		(928.1)						
Percent Puerto Rican Native	-	-	-60.5	-	-	-	-	-
			(83.6)					
Percent Other Hispanic Native	-	-	-	-98.9**	-	-	-	-
				(34.16)				
Racial Immigrant Groups								
Percent Mexican Immigrant	-	-	-	-	-70.98***	-	-	-
					(19.5)			
Percent Cuban Immigrant	-	-	-	-	-	137.3	-	-
						(406.1)		
Percent Puerto Rican Immigrant	-	-	-	-	-	-	82.60+	-
							(48.9)	
Percent Other Hispanic Immigrant	-	-	-	-	-	-	-	-117.1
								(75.3)
African American Segregation Measures								
Isolation Index	3.73	6.59	4.96	8.16	4.43	6.04	4.87	5.89
	(8.12)	(7.74)	(8.18)	(7.89)	(7.79)	(7.96)	(8.28)	(7.71)
Population and Deprivation Control Variables								
Adult Sex Ratio	-20.7**	-18.2**	-20.41*	-14.77+	-14.2+	-17.4*	-22.3*	-12.54
	(7.88)	(8.55)	(10.04)	(8.04)	(8.38)	(8.81)	(10.6)	(9.08)
Divorce Rate	37.0	21.3	22.9	54.9**	55.7*	27.31	14.7	45.5+
	24.50	(23.3)	(22.02)	(21.3)	(24.0)	(22.1)	(25.33)	(23.8)

Percentage of Population Below Poverty Line	52.3**	39.3**	40.23**	25.6	44.2***	37.1*	41.7*	33.2*
	(16.59)	(15.9)	(14.79)	(16.34)	(14.5)	(15.4)	(15.20)	(15.09)
Unemployment Rate	28.333	19.9	17.36	32.3	36.25	22.49	15.70	33.55
	(25.11)	(27.19)	(27.9)	(24.5)	(28.3)	(27.5)	(28.01)	(25.9)
Constant	2161.3*	1868.3	2208.7+	1565.5	1375.6	1811.1	2381.6+	1324.7
	(1062.3)	(1102.7)	(1292.2)	(1035.2)	(1092.5)	(1105.7)	(1345.2)	(1126.5)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	No	No	No	No	No	No	No	No
Number of MSA sample years	276	276	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112	112	112
R-square	0.096	0.076	0.078	0.179	0.138	0.074	0.083	0.123

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 2.15: Two-way fixed effect results predicting changes in arrest rates (per 100,000 population) within MSAs for Asians and Hispanic ethnic subgroup, by immigration status

	Total Arrest Native	Total Arrest Immigrant	Violent Arrest Native	Violent Arrest Immigrant	Property Arrest Native	Property Arrest Immigrant
Racial Classification						
Native-Born Racial Groups						
Percent Mexican Native	63.7	-	15.36	-	1.10	-
	(190.5)		(13.36)		(34.1)	
Percent Cuban Native	-1588.7	-	145.7	-	-275.0	-
	(2765.4)		(192.9)		(698.04)	
Percent Puerto Rican Native	-406.9	-	-23.15	-	8.48	-
	(348.0)		(27.9)		(94.08)	
Percent Asian Native	25.20	-	13.02	-	-45.3	-
	(282.3)		(16.13)		(57.4)	
Racial Immigrant Groups						
Percent Mexican Immigrant	-	-108.0	-	8.55	-	-19.2
		(123.1)		(6.19)		(25.0)
Percent Puerto Rican Immigrant	-	253.587	-	40.9***	-	6.90
		(200.03)		(15.64)		(59.0)
Percent Cuban Immigrant	-	-658.5	-	27.93	-	14.3
		(1687.3)		(118.8)		(276.4)
Percent Asian Immigrant	-	-65.0	-	8.44	-	-45.9*
		(124.6)		(6.37)		(27.8)
African American Segregation Measures						
Isolation Index	56.04*	67.71***	1.83	0.87	8.32	13.9*
	(25.9)	(25.7)	(1.79)	(1.70)	(7.61)	(7.06)
Population and Deprivation Control Variables						
Adult Sex Ratio	-57.2*	-59.90	-1.05	-3.41	-16.86*	-14.6
	(31.58)	(40.7)	(2.04)	(3.02)	(9.06)	(9.55)
Divorce Rate	174.9	-38.70	6.56	8.56	36.3	11.0
	(257.8)	(247.5)	(17.2)	(17.09)	(49.85)	(60.4)
Percentage of Population Below Poverty Line	25.1	64.4	6.24	11.3*	3.19	9.92
	(65.3)	(67.5)	(4.67)	(5.71)	(17.8)	(13.3)

Unemployment Rate	36.01	51.2	-7.34	-7.48	33.9	28.8
	(112.4)	(115.6)	(5.74)	(5.80)	(26.3)	(26.5)
Constant	5430.8	7869.8*	42.73	249.5	1457.8	1855.5
	(4371.1)	(4355.9)	(281.4)	(299.4)	(1250.6)	(1248.5)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of MSA sample years	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112
R-square	0.223	0.223	0.176	0.249	0.358	0.350

*p<.05 **p<.01 ***p<.001 [one-tailed test]

Table 2.16 MSA-level fixed effects predicting changes in arrest rates (per 100,000 population) within MSAs for Asians and Hispanic ethnic subgroup, by immigration status

	Total Arrest Native	Total Arrest Immigrant	Violent Arrest Native	Violent Arrest Immigrant	Property Arrest Native	Property Arrest Immigrant
Racial Classification						
Native-Born Racial Groups						
Percent Mexican, U.S. Born	-48.4		9.44		-35.9	
	(184.6)		(11.1)		(36.5)	
Percent Cuban, U.S. Born	340.9		246.6		387.7	
	(3082.9)		(202.1)		(816.6)	
Percent Puerto Rican, U.S. Born	-653.9***		-36.0		-77.6	
	(247.1)		(24.65)		(83.38)	
Percent Asian, U.S. Born	-283.1		-3.37		-145.2***	
	(224.1)		(15.4)		(51.60)	
Racial Immigrant Groups						
Percent Mexican, U.S. Immigrant	-	-194.4*	-	-0.94	-	-56.6***
		(108.1)		(4.30)		(22.0)
Percent Puerto Rican, U.S. Immigrant	-	505.1***	-	51.1***	-	89.07**
		168.88		(12.1)		(45.50)
Percent Cuban, U.S. Immigrant	-	-336.66	-	44.65	-	124.9
		(1580.1)		(134.4)		(352.7)
Percent Asian, U.S. Immigrant	-	-13.49	-	6.22	-	-35.7
		(95.9)		(6.13)		(26.93)
African American Segregation Measures						
Isolation Index	42.5	6.59	1.13	-0.23	3.44	4.28
	(29.25)	(7.74)	(1.61)	(1.86)	(8.49)	(8.64)
Population and Deprivation Control Variables						
Adult Sex Ratio	-70.7*	-65.7	-1.77	-3.80	-21.2***	-16.82
	(35.3)	(44.8)	(2.24)	(3.32)	(10.90)	(11.48)
Divorce Rate	153.6	150.2	5.03	0.37	38.5	48.2*
	(111.07)	(105.9)	(5.97)	(5.95)	(26.5)	(27.08)
Percentage of Population Below Poverty Line	167.14***	167.9***	13.68***	16.44***	51.8***	45.2***
	(57.2)	(69.5)	(3.80)	(5.66)	(17.10)	(15.22)

Unemployment Rate	12.0	50.4	-8.59	-6.65	25.01	29.81
	(112.2)	(123.4)	(6.37)	(6.56)	(29.5)	(28.80)
Constant	8221.3*	6684.3	193.6	352.9	2303.5	1701.8
	(4211.1)	(4827.3)	(276.1)	(371.5)	(1464.3)	(1453.1)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	No	No	No	No	No	No
Number of MSA sample years	276	276	276	276	276	276
Number of MSAs	112	112	112	112	112	112
R-square	0.111	0.124	0.176	0.178	0.161	0.163

*p<.05 **p<.01 ***p<.001 [one-tailed test]

CHAPTER THREE:

The Effects of Race and Incarceration on Labor Market Outcomes of Less-Skilled Men

Background

Why do less-skilled[4] African American and Hispanic men continue to experience disparities in employment outcomes relative to whites? In answering this question, neoclassical economists generally suggest that disparities stem from skill differences, while many racial theorists commonly assert discriminatory practices. The rise of incarceration as a lifecourse event among less-skilled minorities, particularly African American men, has made involvement in the U.S. criminal justice system a critical factor in evaluating racial employment disparities. By examining how race and incarceration history jointly predict employment disparities within individuals while controlling for racial differences in skills, this paper empirically examines employment theories of neoclassical economists and racial theorists.

Audit studies and employer surveys have found penalties for both a criminal record and race in the labor market (Holzer et al. 2004; Holzer 2007; Pager 2003; Pager and Quillian 2005; Pager and Western 2005), with disparities present even when employers express no preference against hiring ex-offenders or African Americans. Pager's (2003) audit study dramatically shows these results. In matched pairs interviewing for jobs, white ex-felons were as likely to receive callbacks as black men without a criminal record. Rate of callbacks received by black ex-felons was close to zero, with study size possibly suppressing full differences between black men with and without a criminal record.

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[4] Here, I define 'less-skilled' as those who have at most a high school education and no extended college or educational training. Morris and Western (1999) document the decline in real wages for the bottom 80% of income earners in the 1970s, 1980s, and 1990s.

Neoclassical economists such as the Nobel Laureate James Heckman (1998) have discounted the findings of audit studies as evidence of discrimination, viewing statistical racial differences in wages as approximate “markers” for average racial differences in AFQT or SAT scores (Becker 1957; Neal 2005; Neal and Johnson 1996). Relative to employers who do not perform criminal background checks, research of employers by Holzer, Offner, and Stolls (2004) has found that employers who perform criminal background checks are statistically more likely to hire African Americans. Such research suggests that race may be used as a proxy for not hiring those with criminal histories.

The extension of incarceration to racial disparities in employment results, in part, from incarceration becoming increasingly common among less-skilled minorities. Approximately16 million men, including 5.5 million black men, are estimated to have possessed a felony conviction in 2004 (Uggen, Manza, and Thompson 2006). At current incarceration rates, Bonczar (2003a) has estimated that one-third of black males, 17% of Hispanic males, and 5% of white males will spend one year or more in state or federal prison. Pettit and Western (2004b) have found that incarceration disproportionately impacts less-skilled men and minorities; black high school dropouts remain at greatest risk, with 60% spending one year or more in prison by age 44. Work by urban ethnographers (Anderson 1990b, 1999; Duneier 1999) and quantitative researchers (Holzer 2007; Western 2002; Wilson 1996b) have documented how issues of poverty, homelessness, wage penalties, and unemployment occur for ex-offenders. Western and Pettit (2005) have argued that as much as 60% of the wage gap between blacks and whites may be explained by wage penalties leading black ex-offenders to choose unemployment..

Among neoclassical economists, these wage and employment disparities are interpreted as derived from skill differentials or perceived discrimination. James Heckman (1998, 2000) argues that an individual's internalization of perceived discrimination creates behaviors and sorting that drives down mean earnings and wages. In studies of wages and labor-market outcomes (Donohue and Heckman 1991; Neal and Johnson 1996), empirical evidence supports the view that perceived discrimination alters both performance and decisions to enter labor markets. Extending the statistical discrimination models of Gary Becker, Holzer, et al (2004) interpret reluctance of small firms to hire black men as a proxy for risks associated with hiring ex-felons.

Countering neoclassical interpretations, urban ethnographies (Anderson 1990b; Newman 2000; Wilson 1996b) and audit studies (Pager 2003; Pager and Western 2005) provide evidence that effects of race and a criminal record hinder less-skilled workers in the labor market. However, empirical research on how race and incarceration may jointly impact incarceration is currently lacking. Sociological research by Western (2002) and Johnson (2003) document wage differentials by race and criminal background, but sample size and quantitative methods limit applicability to findings from audit studies and ethnographic research. In researching the effects of incarceration in the transition to adulthood, Raphael (2006) utilizes advanced modeling techniques to determine how incarceration impacts employment, but fails to investigate how race impacts labor market outcomes. Traditional fixed effect modeling also assumes normally-distributed outcomes, predisposing analysis to continuous variables such as wages.

This paper augments existing audit studies and prior empirical research. Using panel data from the 1979 National Longitudinal Survey of Youth (NLSY79), I focus on how labor force participation and unemployment may differ for minorities and ex-felons. By starting with a sample of less-skilled men (e.g., no more than a high school degree or GED at age 22), I attempt to empirically test if unemployment and labor force participation vary for populations more closely resembling audit studies of less-skilled men. By estimating fixed effects models using negative binomial regression, I am able to (a) address critiques suggesting that disparities in employment reflect natural differences in productivity, (b) model non-normal distributions for weeks unemployed and not in the labor force during a calendar year, and (c) observe whether blacks and Hispanics differ from whites in employment outcomes given prior incarceration.

Theory/Hypotheses

In this section, I examine prior theoretical and statistical research using the NLSY79 to formulate hypotheses for examining how race and history of incarceration impact employment of less skilled men. Hence, this section contains three subsections in which I (a) examine existing theoretical debates relevant to wages and incarceration, (b) discuss limitations of prior statistical research on analysis, and (c) discuss hypotheses tested in analysis. In the first subsection, I discuss audit studies and critiques. A review of this literature suggests that a more nuanced interpretation of employment outcomes varying by race and incarceration is needed to empirically verify if race and incarceration jointly impact incarceration.

In the next subsection, I discuss prior analysis of NLSY79 data to estimate consequences of incarceration on employment. Prior statistical methods and modeling techniques are discussed, with the goal of constructing methods used in data analysis.

By doing so, I attempt to more fully take advantage of NLSY79 data to test the effects of race and history of incarceration on employment outcomes.

Finally, I discuss hypotheses I will test in this study. This subsection is generated from the reviews in the prior two sections, with the intent of framing the theoretical and statistical methods I later use in analysis.

Current Theoretical Issues

Audit Studies and the Neoclassical Critique

Recent audit studies by Pager (2003) and Pager and Western (2005) have found that potential employers initiate callbacks to whites and non-offenders more frequently than for blacks and those with a criminal record. By careful matching and inclusion of secondary variables, these new audit studies attempt to address validity claims made by neoclassical econometricians (Heckman 1998, 2000; Heckman et al. 1998; O'Neill, Sweetman, and Van de gaer 2006). These critiques argue that unobserved heterogeneity and lack of extended testing severely draw into question claims that the audit methods reveal any form of discrimination, while racial differences are largely explained by skill differences observed between racial groups in empirical analysis. Heckman's (1998) argument, in particular, strongly asserts that perceived returns for skill are causal for observed disparities in wage differences and unemployment in the labor market. Research in this vein suggests that differences in hiring and wages, based on "average" group differences, explain relative disparities observed for blacks and ex-offenders (Edelman et al. 2006; Neal and Johnson 1996).

Neoclassical theory underlying this critique and associated empirical research can be traced to Becker's (1957) notion of "statistical discrimination" and Becker's (1975) theory of human capital. Regarding the former, Becker's theory of discrimination has been extensively utilized by economists to argue that no employer actively discriminating against African Americans can stay in business. Modern reinterpretations of this thesis (e.g., Heckman 1998) interpret "actively discriminate" as "above accepted norms," yet still maintain that discrimination is nearly non-existent in the Post-Civil-Rights Era. Countering this argument, contemporary racial theorists such as Bonilla-Silva (2001, 2003) and Bobo (1999) argue that discrimination may reside in group-differences which lie beyond conscious or individual-based prejudice. For these racial theorists, discrimination by employers lies in group-based norms and practices, such as adopting an industry-wide standard of background checks or using skin tone as a proxy for observed productivity of African Americans. As Loury (1998) also notes, the use of racial stereotypes may also create practices and policies which influence employee-applicant pools; for example, more productive ex-felons may enter the informal labor market and not apply to jobs due to an understanding that wages will not be based on skills, but a set of socially-defined norms. This would downwardly bias productivity among applicants with criminal records, generating outcomes that are not reflective of all ex-felons.

Such theories are also used to explain differential treatment of ex-offenders; thus, it is important to consider how patterns may form based on both the hiring patterns of employers and the behavior of individuals. Regarding employer practices, some empirical research has begun to address the issue of employer behavior and treatment of ex-offenders. In a national survey of employers, Holzer et al. (2004) find that: (a) smaller firms who do not use background checks are less likely to hire blacks; (b) employers in service and finance industries are less likely to hire offenders; and (c) employers are more likely to hire ex-felons through networks or referrals than through help wanted ads or agencies. In surveying firm behavior, Pager and Quillian (2005) have found that employers, even while expressing no objection to hiring either non-convicted African Americans or felony drug offenders, make fewer callbacks to job candidates who are black and ex-felons. In a review of the literature on employer preferences for hiring whites and non-offenders, Edleman et al. (2006) argue that these employer preferences typically result from variation in the skill gaps of African Americans and other groups, with fears of increased liability due to hiring ex-felons also listed as a reason for hiring decisions. Similar to Holzer et al. (2004), African Americans are viewed to suffer in the labor market relative to whites through a combination of low skills and disproportionate rates of incarceration.

At the other side of the employer-employee relationship, the behavior of potential employees who are black and possess a criminal record must also be considered. The work of urban researchers has provided evidence that chronic unemployment leads to criminal activity and work in the informal labor market. Anderson’s (1990b, 1999) research into black urban ghettoes in Philadelphia finds that black males engage in criminal activities to generate income. Wilson (1987; 1996b) argues that crime, extended social networks, and subsistence on government support are survival strategies employed by those in urban ghettoes. Duneier’s (1999) study of “men without accounts” (i.e., men unable to find employment in the formal economy) found these individuals engage in selling and collecting magazines, books and other re-sellable items, thus creating work and labor markets outside the formal economy. Given the structural and cultural mechanisms that have led to the physical and social segregation of minorities (in particular, blacks) from non-minority regions of economic growth and job networks (Massey and Denton 1994; Mouw 2000, 2002; Wilson 1996b), such urban research suggests that strategies of crime, welfare receipt and self-employment in the formal or informal labor market become employment sources for less-skilled blacks.

Race, Incarceration Status & Labor Market Outcomes

These racial disparities in both employment and wages are well established in the empirical literature. Independent of incarceration status, both African American males and females are disproportionately regulated into the secondary labor market (Bonacich 1972; Edwards 1976; Handel 1999; Kalleberg, Reskin, and Hudson 2000; Morris and Western 1999; Waldinger 1996). Empirically, non-incarcerated blacks males have been adversely impacted relative to low-skilled whites and Hispanics for both earnings and employment levels in the 1980s and 1990s (Darity and Meyers 1998; Holzer and Offner 2006; Johnson 2003; Tomaskovic-Devey, Thomas, and Johnson 2005). The relegation of blacks to secondary labor markets regardless of criminal record has been well documented among sociologists (Handel 1999; Kalleberg et al. 2000; Morris and Western 1999). Western and Pettit (2005), in an analysis of the black-white wage gap with NLSY79 data, use a two-stage Heckman model to impute that up to 60% of black-white earnings may be explained by employment issues related to criminal histories.

Given Holzer et al.'s (2004) finding that employers often express preference for hiring non-offenders to those with criminal records regardless of racial classification, ex-felons are also likely delegated to the secondary labor markets.[5] For black ex-felons who might traditionally have been employed within secondary labor markets, observed employer preference against hiring blacks and ex-felons may imply this group is especially disadvantaged over white ex-felons and black non-felons. As a result, black ex-felons may turn to informal or illegal economic activities if limitations in labor market opportunities are too severe. Johnson's (2003) analysis of cumulative unemployment and labor force experience among minorities and ex-offenders finds that incarceration status is associated with increased unemployment and decreased work experience across the lives of low-skilled men. Western's (2002) analysis of NLSY79 data finds that incarceration substantially penalizes wage trajectories for all males, reporting similar findings for other racial groups for unemployment.

By Becker's (1975) standard theory of skills (i.e., human capital), individuals accumulate both general and job-specific skills through education and work that determine an individual's labor market wage. Neoclassical economists argue that race and gender differences in wages are a proxy for average differences in productivity (Neal 2005; Neal and Johnson 1996; Sowell 1982), while sociologists argue that standard human capital theory justifies discrimination in work and employment against women and minorities (Becker 1975; Budig 2001; Johnson 2003; Reskin 1998; Tomaskovic-Devey et al. 2005). Standard human capital models predict that human capital may stagnate or depreciate while individuals undergo incarceration (Western 2002). While a

[5] It should be noted that "secondary labor market" here refers to the category of low-wage, part-time, and low-skilled work defined by Edwards (1979) as a peripheral labor market. Edwards classifies secondary labor markets as part of the "industrial reserve army" of workers for labor not requiring training that may vary by day. The secondary labor market is also part of the formal economy, whereas the term "informal economies" discussed elsewhere in this essay is reserved for jobs outside the formal labor market (e.g., work falling outside state/federal law and formal GDP estimates).

criminal record has been shown to correlate with negative earnings (Edelman et al. 2006; Freeman 1996; Western 2002), recent research has suggested that length of incarceration does not impact wage penalties (Kling 2006).

Experiences and research of former felons-turned-academics suggests that barriers to rehabilitation, such as memory of societal norms, accumulation of legal debt and child support, untreated drug habits, and legal barriers to being employed within industry sectors, impact the ability to work (Richard and Jones 2004). Yet, as a recent report by the Urban Institute indicates, incarceration may provide opportunities for further educational or vocational attainment. Soloman et al (2004) have found that a majority of state and federal prisoners receive formal education or vocational training while in prison; in 2000, almost 1.2 million of the 1.4 million individuals serving time in state and federal prison participated in some type of inmate labor (e.g., farm labor, general work, or contract work with private firms). Paired with inmate labor, the educational and vocational training suggests that general human capital and work experience likely does not decline or stagnate during incarceration, contrary to normal assumptions. Rather, structural issues, such as lack of drug treatment, debt and child support, and legal barriers to work, act against ex-offender's reintegration into society. These issues suggest that the general benefits of work in desistence from crime reported for long-term, high paying jobs (Sampson and Laub 1993; Uggen 2000) may be generally counteracted by social stigma and labeling due to incarceration (Johnson 2003). Edleman et al. (2006) conclude that ex-offenders obtain inferior GED credentials and have criminal records that eliminate possible employment in security, health care, education, and financial industries; the lower-skilled credentials and legal barriers to various industries combine to produce these results, not social stigma. Some research suggests that the wide array of disadvantages and obstacles faced by ex-offenders may "saturate" offenders in a manner that prevents social reintegration[6] (Hannon 2003). If the incarceration effect prevents changes in educational attainment and substance abuse from influencing employment outcomes, empirical results would lend support to this thesis.

One of the most important identified characteristics of current and former ex-felons is formal educational attainment. Among African American males, for example, 60% of high school dropouts versus 10% of college graduates will serve one year or more in state or federal prison (Pettit and Western 2004b). Lack of skill is generally hypothesized to play a major role in wage trajectories and labor market decisions to engage in illegal activities (Becker 1968; Neal and Johnson 1996); during the 1980s and 1990s, changes in returns-to-education were generally found to increasingly divide low-skilled and college-educated males (Darity and Meyers 1998; Holzer, Offner, and Sorensen 2005; Morris and Western 1999). Western and colleagues (Western and Beckett 1999; Western and Pettit 1999, 2005) have argued that the convergence of black-white earnings observed during the economic boom of the 1990s occurs artificially due to exclusion of incarcerated populations and ex-felons from the formal labor market. While other factors identified above, such as substance abuse, social stigma, lack of family stability, and impoverished neighborhoods may be underlying causes, educational attainment represents a key factor in distinguishing "at risk" populations.

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[6] Hannon's (2003) usage of "disadvantage saturation" extends McLeod's (1995b) argument that a lack of cultural capital may create impermeable barriers to assimilation for impoverished, inner city black men. Here, I similarly use "saturate" to imply that environmental factors may prevent reintegration of ex-offenders into formal labor markets and society in general.

Prior Empirical Research Using the NLSY79

As with most publicly-available national datasets, prior research on employment outcomes and incarceration informs the scope of this paper. In this section, I discuss prior statistical research using the NLSY79 dataset to examine links between employment and incarceration.

Freeman (1993) analyzes NSLY79 data to determine effects of incarceration on employment outcomes in interview years of 1980 and 1983. This analysis relies on a single-period incarceration measure as a pseudo-experiment. Freeman uses two other datasets (The Boston Youth Survey and Inner City Survey) to replicate findings that incarceration reduces number of weeks of employment for 15-20 weeks during a calendar year. Use of multiple datasets helps to validate findings. However, the NLSY sample of those incarcerated in 1980 and 1983 represents less than 20% of reported total jail interviews, substantially limiting range of sample data, and does not include effects of multiple incarceration spells or the large economic expansion of the 1990s. Nevertheless, Freeman's analysis with multiple data sets is convincing evidence that incarceration negatively impacts employment outcomes of ex-offenders. In analyzing predictors of incarceration from early deviance and arrest patterns, Freeman (2000a) tabulates differences among current and former ex-felons but does not conduct further regression analysis extending his 1992 work on employment or wages.

For statistical analysis, Freeman (1993) also adopts usage of an instrumental variables (IV) estimation approach to control for estimation bias which may result from unobserved respondent's personal characteristics prior to incarceration. Instrumental variable techniques are used to obtain estimators or predictors thought to correlate with individual error terms. As Greene (2003) notes, IV methods do not allow for estimation of potential correlation between predictors and unobserved error terms; additionally, choice of instrument may also be highly normative. Freeman's IV approach assumes a single random error component (e_i) and does not include panel analysis where repeat incarceration spells may occur.

Work by Western and colleagues (Western 2002, 2006; Western and Beckett 1999; Western, Kling, and Weinman 2001; Western and Pettit 1999, 2005) has attempted to estimate labor force participation, wage trajectories and unemployment among incarcerated men. Early work by Western and Beckett (1999) uses random effects models to show that incarcerated males have lower employment rates relative to non-incarcerated males after a seven-year period. These models create "lagged incarceration effects" through pooled time series analysis. This sample focuses on labor supply effects for juvenile offenders in analysis (Western, Kling, and Weisman 2001), while treating later incarceration spells as a control variable. Given the substantive difference in treatment of juvenile and adult offenders (Mauer 2003; Uggen and Massoglia 2003) and the differences observed among offenders that commit crimes only in adolescence versus also in later adulthood (Moffitt 1993b; Nagin and Land 1993; Sampson and Laub 1993; Sampson and Laub 2003a), differences in adult and juvenile incarceration may substantively alter results in measured unemployment and labor force participation rates. Additionally, random effects models assume a normally-distributed error term for individuals that does not eliminate time-invariant effects.

In addition to the work cited above, Western's (2002) paper warrants additional discussion. That study's analysis of the effects of incarceration on wage trajectories using NLSY79 data addresses the effects of multiple periods of incarceration on labor market outcomes, while also employing fixed-effect models to analyze panel data. Western estimates models separately for blacks, whites, and Hispanics. While Western notes that labor market outcomes for employment and incarceration are modeled, he does not discuss these results. Western's analysis explores the possibility that criminal careers may disrupt earnings trajectories and finds that, on average, ex-offenders earn about 16% less than non-offenders, when controlling for differences in age, education, and multiple incarceration spells. Consistent with Grogger's (1995) findings that length of incarceration does not significantly alter the effects of incarceration, Western finds the effects of incarceration on wages are largely incurred after incarceration takes place. It is important to note that Western's approach to analysis (a) assumes that individuals "at risk" for incarceration are chosen from those with self-reported criminal behavior at the beginning of each sample; (b) includes analysis of all males, regardless of educational level; and (c) estimates separate trajectories for blacks, whites, and Hispanics. These assumptions deviate from assumptions in the audit surveys conducted by Pager and colleagues (Pager 2003; Pager and Western 2005).

In her dissertation analysis, Johnson (2003) utilizes NLSY79 data to estimate individual-level fixed effect models for wage and employment trajectories of individuals. Johnson's analysis estimates cumulative employment experience over the working lives of adult males, with educational level, current and prior incarceration status during each wave of interview, age, AFQT score, and race-prior incarceration interactions as independent predictors. On average, Johnson finds that prior incarceration decreased cumulative employment experience by 70 weeks by 2000 (ages 35-43), with uniform decreases in cumulative unemployment for prior incarceration across all racial groups. In Johnson's sample, blacks also experience two fewer weeks of employment than whites for every year of aging.

It is important to note that Johnson's sample omits data when zero or negative earnings are reported. Unlike Western (2002), Johnson's analysis does not restrict sample selection to offenders committing criminal activity at time of initial interview, and it includes all educational levels from 1979-1998. Her use of cumulative employment and wage trajectory models assumes that human capital increases with work experience, with incarceration marking periods where work experience does not accumulate. She interprets employment and wage penalties incurred from incarceration as resulting from discrimination by employers, with effects also occurring by race.

Recent work by Raphael (2006) analyzes NLSY79 data to examine the effects of incarceration on transition to adulthood. Raphael's findings suggest that a history of incarceration substantively decreases the likelihood of subsequent employment. Raphael incorporates two-way fixed effect models at the individual and year level, eliminating time-invariant characteristics for individuals and periods that may impact employment in the process. Raphael's statistical methodology addresses limitations of studies discussed above but lacks focus on two key issues. First, by using the full sample of men and incorporating racial differences into age-race interaction variables of his sample population, Raphael fails to test findings of audit studies where joint effects of race and incarceration are found. Second, Raphael assumes the effect of an incarceration is uniform over time and across racial groups.

Hypotheses for Analysis

In this subsection, I summarize the literature reviews for (a) theoretical links between race, incarceration, and employment and (b) prior work in the NLSY79 linking employment and incarceration. The section on theoretical hypotheses extends results from audit studies to analysis of panel data in this study, while also addressing neoclassical economic arguments. The section on prior work on the NLSY79 also frames the methodological hypotheses that extend prior research in data analysis.

Theoretical Hypotheses

Current statistical research has not fully examined the results from recent audit studies which find that penalties for race and incarceration may jointly impact employment outcomes among less-skilled men. In this study, I attempt to test for these joint effects in a longitudinal sample of men. To address neoclassical arguments that racial differences in skills may underlie observed differentials in unemployment, I also incorporate educational variables. I include measures of both history of incarceration and length since last prior incarceration to more closely model long-term employment outcomes of less-skilled men.

One key hypothesis is that, controlling for educational level, African American and Hispanic ex-felons will experience higher rates of unemployment and being out of the labor force relative to white ex-felons. By selecting my sample based on educational levels instead of race, I focus on observed differences among less-skilled ex-offenders in the labor market. This methodology more closely replicates audit studies.

Audit studies suggest that penalties in hiring exist for both African Americans and ex-felons in employment. The sociological literature suggests that African Americans operate in secondary labor markets, where little or no educational attainment and skill are normally required for employment. Employers also express strong preference against hiring ex-offenders. To test for independent effects, I test for employment differentials that may result due to incarceration. Within individuals, obtaining a history of incarceration should result in significant employment penalties. If race and incarceration effects on employment occur, significance of race and incarceration interaction should show that black and Hispanic ex-felons will have higher unemployment and lower labor force participation relative to non-incarcerated blacks and ex-felons. I also test for interactions between race and years since last incarceration to examine if, relative to whites, employment disparities linger among blacks and Hispanics with a history of incarceration.

To test if returns to education vary by race and incarceration for employment outcomes, I will include interaction terms for race and educational attainment. Significance of interaction for race and education in predicting employment suggests differential returns for low-skill workers. If race and incarceration exert joint effects on employment when controls for changes in racial variances of skills are included in analysis, this would suggest that racial variances in employment for ex-offenders are not simply skill-based.

Statistical Methods

Within sociology, lack of systematic analysis of panel data also has prevented full utilization of panel data within the discipline (Allison 1994, 2005b; Halaby 2004). As Hallaby (2004) notes, empirical analysis by Western (2002) and Western and Pettit

(1999) did not fully take advantage of the analysis of random and fixed-effect models in systematically analyzing panel data for incarceration effects. Fuller's usage of statistical models allows for comparison of effects in non-random assignments within individuals to assessment of causal inference. By analyzing populations where dependent variables mimic randomized experiments of incarceration effects given sets of control variables, quantitative research may further validate outcomes from randomized experiments (Heckman et al. 1998; Western et al. 2001). This paper will focus on the differential labor force participation and unemployment outcomes among minorities and those with histories of incarceration. This provides an empirical test of audit surveys that have been heavily criticized by some economists (e.g., Heckman 1998).

By using all available years of incarceration and dividing error components to include (a) individual-level fixed effects (u_i); (b) year fixed-effects (w_t); and (c) a random disturbance term that varies by year and individual (e_{it}), I will address limitations of Freeman's analysis. Individual-level (u_i) and year (w_t) fixed effects eliminate all unobserved time-invariant characteristics that may vary across individuals and periods. By eliminating these components in modeling, individual characteristics and period effects that may determine employment outcomes cancel out, avoiding issues associated with using the instrumental-variable approach.

Western (2002) and Johnson (2003) select individuals based on self-reports of delinquent acts in 1979. However, human capital theory and demographic studies suggest less-skilled individuals are much more likely to engage in criminal behavior and enter the criminal justice system (Becker 1968; Pettit and Western 2004). Audit studies examining the effects of incarceration and race also focus on less-educated groups. By selecting a sample of less-skilled men (i.e., with no more than a high school education) I analyze individuals most likely to be "at risk" for entry into the criminal justice system and face long-term employment outcomes similar to those studied in recent audit studies.

Western (2002) also estimates separate models for African Americans, whites, and Hispanics. This does not fully integrate findings that differences in skill are observed in analysis by economists (Neal and Johnson 1996) who analyze NLSY79 data to find that racial variation in skills (measured by AFQT scores) explains almost three-fourths of observed variance in wages. I will place all racial groups into the same data pool for analysis and use interactions to determine wage penalties for race and incarceration instead of estimating separate group models. Separate models for black, white, and Hispanic subpopulations will also be estimated for comparative outcomes.

Johnson's (2003) analysis focuses on differences in career wages and earnings, focusing on the differentials in human capital that occur over the life-course. My analysis differs by using count-data to model the effects of incarceration on *annual* weeks of unemployment and time out of the labor force to test if repeated cross-sectional data mimics audit studies. Johnson's analysis also assumes that differences in human capital determine wages; based on empirical and theoretical issues outlined above, I focus on whether or not increased formal education and training my benefit populations of low-skill ex-offenders and blacks[7]. I also test if differences in racial returns to education do not substantially impact race-incarceration effects on employment.

[7] Please note that the use of individual-level fixed-effect models assume away constant or unchanging education that an individual may have. The fixed-effect model thus measures the effect of changes in education on subsequent unemployment. By using interactions, I test if increased educational attainment for blacks and ex-felons differentially predicts unemployment.

Finally, the modeling techniques mentioned above closely resembles those used by Raphael (2006) in studying employment as an outcome variable for individuals transitioning to adulthood after undergoing incarceration. My analysis differs from Raphael in three ways. First, to model the effects of incarceration, I utilize negative binomial fixed effect models (vs. traditional GLS fixed models used in analysis by Raphael). This better the highly models the highly non-normal distribution count of weeks of unemployment and being out of the labor force during a calendar year. Second, I test for race and incarceration interactions to test for joint effects of race and incarcerations on employment. Raphael uses a pooled sample of racial groups, but assumes the effects of incarceration on employment are identical across groups. Third, Raphael uses a collapsed dependent variable for weeks unemployed during a given year. I aggregate raw counts of weeks unemployed during a calendar year from work histories of employers.

Data and Methods

Data

For analysis, I will analyze data from the 1979 National Longitudinal Survey of Youth. The NLSY79 is a nationally representative sample of 12,686 individuals ranging in age from 14-22. From 1979-1993, a comprehensive set of survey questions was asked annually for all participants in the sample; biannual surveys were also asked to all respondents from 1994-2004. As of 2004, approximately 7,661 of original respondents were interviewed (~79% of all original interviewees). Of the 5,025 cases not interviewed in 2004, approximately 2,600 were military and non-minority poor dropped from the sample, 421 individuals were deceased, 1134 refused interview, 800 were listed as being either difficult cases or “other,” and 80 individuals were listed as incarcerated. It should be noted that 2004 marks the first year in the NLSY79 that incarceration is listed as a reason for non-interview. While non-response and lost cases may also downwardly bias estimates due to disproportional representation among those living in poverty or incarcerated (National Longitudinal Survey of Youth 2004), panel data analyzed with fixed-effect models allows for testing within persons and observed time periods.

The structure of the NLSY79 allows for panel data analysis of respondents with twenty-one waves of data collection on social and economic variables. A NLSY79 appendix file for respondent work histories includes weekly data for unemployment and labor force participation. I utilize both the main dataset and work history appendix in my analysis.

Respondents

As discussed in the previous section, this paper focuses on the analysis of less-skilled men. Individuals selected for analysis were individuals who had completed no more than twelve years of education at age 22. This selection of individuals mimics audit studies such as Pager (2003) where college-age students have been used. By eliminating individuals who complete more than twelve years of education at age 22, this selection captures individuals who, as young adults, spend substantial time as “at risk” for incarceration relative to those who complete post-secondary education[8] (Pettit and

[8] While more highly educated groups commit crimes and undergo incarceration, eliminating individuals with higher education levels help reduce effects of incarceration on employment outcomes though mechanisms such as increased wealth, participation in the primary labor market, or obtaining jobs through social networks. Additionally, examination of individuals with lower levels of education selects a

Western 2004). Of the original sample of NLSY79 respondents, a sample of 4,610 men had completed no more than twelve years of education at age 22, including 2,599 white males, 1,244 black men, and 767 Hispanic men. The 1979 sample characteristics of the respondents are presented in Table 1.

Employment

As defined by the Bureau of Labor Statistics, unemployment arises when an individual does not have a job but actively seeks employment in the formal economy. Labor force participation occurs when an individual either works or seeks employment within the formal economy; those who do not participate in the labor force do not participate in the labor market of the formal economy. The disparities in unemployment and labor force participation among both minorities (Darity and Meyers 1998; Holzer and Offner 2006) and those with histories of incarceration (Raphael 2006; Western and Beckett 1999) have been documented. Hence, by analyzing measures of unemployment and labor force participation, this study more closely examines the dynamics of race and history of incarceration on the labor market outcomes of less-skilled men.

The number weeks of unemployment and labor force participation during a given calendar year in the general NLSY79 dataset are provided in categorical levels. To generate count data for annual weeks of unemployment and labor force participation, weekly labor force participation and unemployment of respondents are drawn from the NLSY79 "Work History" variables. Weekly work history variables are constructed in

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population where work is essential to generate earnings for survival. Racial differences in the effects of incarceration may also be obscured among more highly educated groups, due to the enforcement of equal opportunity laws among employers in the primary labor market. For such reasons, removing individuals with higher levels of education allows for better measurement of the effects of race and incarceration on employment.

the NLSY79 data from a respondent's self-reports on periods unemployed, not working, or employed in a given job. Details on the work history files may be found online at: http://www.nlsinfo.org/nlsy79/docs/79html/79text/workhist.htm.

Table 3.2 presents means and standard deviations for annual weeks of unemployment and weeks outside the labor force among less-skilled men in the sample. Annual weeks of unemployment and annual weeks out of the labor force reported in Table 3.2 are similar to those published by the NLSY for men during each calendar year of interview (for descriptive tables, see NLSY79 employment history summary tables available online at:
http://www.nlsinfo.org/nlsy79/docs/79html/79text/workhist.htm#descriptive).

History of Incarceration

The NLSY measures incarceration by noting if individuals are interviewed in jail or prison. While this form of incarceration data does not capture the full range of criminal justice contact (e.g., arrests, jail sentences served between interviews, etc.), this methodology provides certainty of incarceration, while providing conservative (downwardly biased) estimates of effects of incarceration on wages and unemployment[9] (Freeman 2000; Western 2002). While non-response and lost cases may also downwardly bias estimates due to disproportional representation among marginalized populations such as the homeless or the poor, data analysis by Western (2002) and

[9] The NLSY79 does not report spells of incarceration occurring between interviews. Consequently, downwardly biased estimates of a incarceration are likely to result due to the fact that the NLSY79 is likely to miss individuals serving sentences less than one year in prison, but suffering the effects of a criminal record.

Raphael (2006) find that incarceration patterns closely follow those of published Bureau of Justice statistics estimates.

Table 3.3 provides annual measures for the incarceration variables used in analysis for both the full sample and each racial group. “Currently Incarcerated” measures whether an individual is interviewed in jail during a particular interview-year. “Ever incarcerated” indicates an individual has been interviewed in jail/prison during a current or former interview-year. “Years since last prison interview” measures years since an individual was last interviewed in prison, where those with no prior history of incarceration have are coded as zero.

Education

In creating the sample, individuals who have completed more than thirteen grades of education at age 22 are removed from the dataset. However, some individuals in the dataset pursue postsecondary education in later life or obtain a G.E.D. To control for changes in education, I estimate models with indicators where individuals are high school dropouts, possess a GED, are high school graduates, or have completed postsecondary education in years after unemployment. A control variable is also used to control for periods when individuals are in school fulltime during a particular interview-year.

As Neal and Johnson (1996) and Neal (2005) document, racial differentials in AFQT scores explain about 75% of wage differences between blacks and whites in the NLSY79. Completing a G.E.D. versus a high school diploma is one key measure for measuring these differentials. If skill differentials explain racial differences in employment outcomes, these factors may explain racial variances in the effects of incarceration on employment outcomes. To control for this outcome, I estimate models with race and educational outcomes. My measures for educational attainment are variables for highest grade completed and completion of a G.E.D. Yearly means and standard deviations for educational attainment and the proportion of the sample with a G.E.D. are provided in Table3.2 2.

Control Variables

Table 3.1 provides variable definitions for control variables used in analysis, along with 1979 means and standard deviations for full sample population and by racial classification. These controls are measured across all interview-years in the sample and include region, urbanicity, welfare receipt, age, marital status, and work disability. Similar controls are used by Raphael (2006); like Raphael, I use second- and third-order polynomials to model nonlinear age patterns in employment.

Methods

Most empirical analyses of the effects of incarceration involve analysis of wage trajectories or wage penalties incurred as a result of incarceration (Grogger 1995; Holzer 2007; Johnson 2003; Western 2002; Western and Pettit 1999, 2005). But recent employer surveys (Holzer et al. 2004; Pager and Quillian 2005), audit studies (Pager 2003; Pager and Western 2005) and urban ethnographies (Anderson 1990b; Duneier 1999; Wilson 1996b) indicate that an inability to locate employment in the formal labor market also hinders ex-felons. By analyzing patterns of labor force participation and unemployment as dependent variables instead of wages, I examine if joint effects of race and history of incarceration are observed among less-skilled men.

As discussed above and in the previous section, my aim is to test the extent to which incarceration of individuals, measured by interviews occurring in prison or jail, affects unemployment and labor force participation. To conduct this analysis, I utilize the basic statistical model

$$\ln(Y_{it}) = \ln(\beta_{0t} + \Sigma(\beta_{jt} * X_{jit}) + + \Sigma(\gamma_{kit} * z_{kit}) + \varepsilon_{it},),$$

where i and t represent the ith individual at time t, Y_{it} is count data for number of weeks out of labor force participation or weeks of unemployment during a given year, β_{0t} is a constant, $\Sigma(\beta_{jt} * X_{jit})$ is the set of time-varying predictors and coefficients, $\Sigma(\gamma_{kit} * z_{kit})$ is the set of time-invariant predictors and coefficients, ε_{it} represents the sum of error terms in the equation such that

$$\varepsilon_{it} = e_{it} + u_i + w_t.$$

Here, ε_{it} is an error component consisting of a random disturbance term e_{it}, an individual-level error term u_i, and w_t is an error term for period effects at time t. Across all the models estimated, Hausman tests were found to be highly significant (e.g., *$p<0.001$*) versus random effects models, indicating a need for usage of a fixed-effect model to define u_i as a fixed effect term. Similarly, inclusion of fixed effect terms for sample years were found to be highly significant, indicating the need for w_t to control for unobserved period effects.

In analysis, the statistical model described above differs from similar work by Freeman, Johnson, Raphael, and Western in key ways. I describe these differences below, to summarize both current work and my contribution to existing literature.

Unlike Freeman (1993), the adoption of panel data allows for multiple years of incarceration and extending from 1979-2004. The use of repeated cross-sections to identify effects within individuals is one of the chief benefits of analyzing panel data (Halaby 2004). By incorporating incarceration in predicting employment outcomes (versus significance of only a single incarceration spell), I better capture if incarceration plays a role in labor market outcomes.

In analysis, I also make use of two-way fixed effects as repeated cross-sections and control for unobserved time invariant characteristics for individuals and time periods. Halaby (2004) and Allison (2005) note that fixed-effect models allow for repeated measurement for effects of a 'treatment variable" such as incarceration. Freeman's (1993) instrumental variable approach is problematic in that choice of instrument and error-bias cannot be measured. The analyses conducted by Western (2002) and Johnson (2003) utilize fixed-effect error components at the individual level, but lack year fixed-effect error terms controlling for time invariant period effects.[10] Time-invariant effects are utilized by Raphael (2006) to examine employment.

Prior research does not utilize fixed-effect regression modeling for count data. In prior research, Western (2002) examines real wages, Johnson (2003) examines

[10] Examples of time-invariant effects are individuals being more likely to be arrested in early years of the sample due to crime-age effects, a constant effect of economic growth/recession on individuals predicting unemployment, and increased job search times due to spatial mismatch.

cumulative weeks of employment, and Raphael (2006) examines employment. These studies all use fixed-effect modeling with a continuous dependent variable. As Allison (2005) documents, fixed effect modeling may be similarly utilized for analyzing Poisson and Negative Binomial regression techniques. Given employment in the labor force is measured in weeks out of labor force during a calendar year, I adopt negative binomial regression to model the repeated annual labor experiences of offenders and non-offenders.

It should be noted that the use of two fixed-effect models is not without limitations. In contrast to the fixed-effect models cited above, random-effect models assume that the individual-level error term u_i is from a common distribution (e.g., normally distributed) and uncorrelated with disturbance terms and independent predictors.[11] Western and Pettit (1999) use a random-effect model to estimate effects of incarceration on employment by adding an error-component for individuals. This method generates increased efficiency in estimation and allows for covariance parameters to be estimated for incarcerated and non-incarcerated groups. However, unlike the fixed-effects models discussed above, unobserved heterogeneity for individuals is assumed to be uncorrelated with random disturbance term and independent predictors. In general, random-effect models are more efficient in estimation of predictors and standard errors, but are inconsistent when significant correlation exists between unobserved heterogeneity

[11] The distinction between either fixed effects or random effects is a somewhat artificial argument, though an assumption commonly made in the social sciences (Halaby 2004). In general, it may be shown that random-effects models are a special case of fixed-effects models when the random error term u_i is altered. In the random-effects model, time-invariant unobserved variables are unrelated to predictive variables and random disturbance terms, either by direct correlation or heteroskedasticity. When u_i is assumed to have correlation with predictor variables or disturbance terms, the model error structure alters to that of the fixed effect model (Allison 2005; Mundlak 1978).

across and independent predictors or disturbance terms in the specified regression model (Allison 2005; Halaby 2004). Hence, the fixed-effect models I utilize result in increased potential for Type II errors (falsely rejecting a true hypothesis), and lack of measurement of observed time-invariant characteristics that accompanies the fixed effect model are costs of estimation.

Results

Unemployment: Main Effects

Table 3.4 presents results for unemployment for the full sample of less-educated men and racial subgroups. In the estimated models, weeks unemployed during a calendar year are predicted by educational, incarceration, and demographic control variables. By estimating results for the pooled sample of all less-skilled men, along with whites, blacks, and Hispanic sub-samples, I examine if models estimated across racial groups by Western (2002) substantially impact results.

When controlling for years interviewed in prison, individuals with a history of incarceration are 1.65 ($p<0.001$) times more likely to spend additional weeks unemployed during a given year relative to those without history of incarceration. These effects do not differ significantly differ by race, with a history of incarceration increasing risk of weeks unemployed by a factor of 1.65 among whites, a factor of 1.53 among blacks, and a factor of 1.69 among Hispanics. For each consecutive year those with histories of incarceration remain out of prison, the effect of incarceration on weeks unemployed declines by 2.7% ($p<0.001$). White men see an annual decline of 4.1%, which contrasts with an annual decline of 2.5% among blacks and 0.1% of Hispanics.

The results for these incarceration variables do not substantially change when educational categories are substituted for years of education.

As neoclassical economists predict, variation in skills levels is a strong predictor of unemployment. In the pooled sample, relative to high school graduates, risk for experiencing additional weeks of unemployment is 29% higher for high school dropouts, 24.5% higher for those who have completed a G.E.D., and 12.7% lower for those who complete one or more years of postsecondary education. In an alternative model considering years of education with a dummy variable for obtaining a G.E.D., each year of education completed is associated with a 4.5% decline in risk for increased unemployment, while a G.E.D. is associated with a 16% increased risk in unemployment. In contrast to incarceration variables, the effects of education on unemployment vary substantially across racial groups. Educational attainment is found to vary most substantially among white men, with white high school dropouts and those obtaining a G.E.D. being ~45% more likely to experience additional weeks of unemployment. In contrast, blacks who have dropped out of high school or obtained a G.E.D. are only 12% more likely to experience unemployment, while Hispanic high school dropouts are 24% more likely to experience unemployment; no effect is found among Hispanics obtaining a G.E.D.

Control variables were also associated with significant changes in risks for unemployment. In the pooled sample and across all racial group subgroups, risk for married individuals declines by a factor of ~0.650 ($p<0.001$). Receipt of public assistance increases risk for unemployment by a factor of 1.95 ($p<0.001$) for the pooled sample; this risk varies by race, where risk of unemployment increases by a factor of 2.16

for whites, 1.71 for blacks, and 1.81 for Hispanics. Similarly, an increase in the level of neighborhood unemployment increases risk of unemployment by 12% ($p<0.001$) for the pooled sample; the effects of level of neighborhood unemployment on individual risk of unemployment vary by race, with increases of 15% for whites and Hispanics, and only 9% for blacks. Moving to the Midwest is associated with an increased risk of unemployment, with varying effects across racial groups.

The results above indicate that a history of incarceration increases risk of annual weeks of unemployment by a factor of 1.65 ($p<0.001$); the effect of incarceration does not vary significantly by racial group. For each year since an interview in prison, an individual's risk for unemployment declines by approximate 2.7% ($p<0.001$), with whites facing the most substantial declines (4.1% per year, $p<0.01$) relative to blacks (2.5% per year, $p<0.05$) and Hispanics (0.1% per year). These effects occur when controlling for marriage, age, neighborhood unemployment, receipt of public assistance, region and urbanicity, and educational attainment. In estimating alternative models of educational attainment, the effects of incarceration variables on unemployment remain similar across racial groups, suggesting that differential skills resulting from a G.E.D. do not mitigate the link between incarceration and unemployment.

Labor Force Nonparticipation, Main Effects

Table 3.5 presents results for unemployment for the full sample of less-educated men and racial subgroups. In the estimated models, annual weeks of unemployment are predicted by education, incarceration, and demographic control variables. As with the unemployment models discussed above, nonlinear effects for age are included in the model; a second-order polynomial, however, was found to better fit labor force trajectories in analysis. By estimating results for the pooled sample of all less-skilled men, along with whites, blacks, and Hispanics sub-samples, I examine if racial classification fundamentally biases results.

Relative to results from unemployment, a history of incarceration results in a much greater risk of spending time outside of the labor force; when controlling for interviews in prison, individuals with a history of incarceration are 2.31 times more likely to spend time outside of the labor force. These results vary substantially by race, with whites 1.89 times more likely to spend time outside of the labor force, African Americans 2.54 times more likely to spend weeks outside the labor force, and Hispanics 2.38 times more likely to spend weeks outside the labor force. For each additional year after release from prison, the risk of spending additional time outside the labor force declines by 1.9% ($p<0.001$). These results vary substantially by race, with no decline associated for whites, an annual decline in risk by 3.4% for blacks, and a decline of 3.6% for Hispanics. Hence, these results suggest that a history of incarceration significantly increases risk for spending time outside of the labor force, with outcomes differing significantly across racial groups.

Educational attainment is also associated with significant changes in weeks spent outside the labor force. In the pooled sample, dropping out of high school makes someone 44.6% ($p<0.001$) more likely than a high school graduate to spend time outside the labor force; individuals with a G.E.D. are 35.6% ($p<0.001$) more likely to spend time outside the labor force. These results vary significantly by race; relative to whites and Hispanics, African Americans with a G.E.D. are less likely to experience increased risk for spending time outside of the labor force. Relative to obtaining a high school degree, African Americans who drop out of high school are 38.3% more likely to spend time out of the labor force (49.5% for whites, 51.0% for Hispanics) and 18.5% more likely to spend time out of the labor force (50.5% for whites, 40% for Hispanics). In alternative models where highest grade of educational attainment is used, similar effects for a G.E.D. are observed.

Results for controls are similar to those obtained for unemployment. In the pooled sample, having a work disability ($p<0.001$), receipt of public assistance ($p<0.001$), level of neighborhood unemployment ($p<0.001$), and moving to an area outside of the Northwest ($p<0.05$) are all associated with an increased likelihood of being out of the labor force during a calendar year. Being married significantly decreases risk for spending time out of the labor force. Racial variances in risks are associated with neighborhood unemployment, welfare receipt, and regional location.

The results from Table 4B suggest that individuals with a history of incarceration are 2.31 times more likely not to participate in the labor force, with risk of being out of the labor force declining by 1.9% for every year since last interviewed in prison. In contrast to unemployment, increased risk for being out of the labor force varies substantially across racial groups, suggesting that joint effects for race and incarceration may hold. Even with controls for neighborhood unemployment, welfare receipt, age, marriage, work disability, urbanicity and regional location, and neighborhood unemployment, the effects of history of incarceration and years since last prison interview on labor force nonparticipation hold. When controlling for educational attainment and G.E.D. completion, the effects of incarceration on being out of the labor force also hold, suggesting that human capital skills do not mediate the link between incarceration and being out of the labor force.

Unemployment, Joint Race and Incarceration Effects

Table 3.6 presents interaction models for race and incarceration in predicting unemployment. For race and incarceration, interactions are tested for history of incarceration and years since last prison interview. Interactions for race and history of incarceration are not found to be significant, while being black or Hispanic is associated with lingering effects of incarceration on employment relative to whites ($p<0.05$). In testing interactions for (a) race and history of incarceration and (b) race and years since last interview, similar results are found. These results suggest that the effects of incarceration on increased unemployment are similar for whites, blacks, and Hispanics, but risk of unemployment in years since last interview remains significantly higher for blacks and Hispanics.

To examine if any differences are explained by differences in human capital, I test if racial differences for incarceration variables are mediated by changes in interactions of race and interaction models. Relative to whites, the baseline model shows that racial differences in educational attainment are significant; blacks are found to be at higher risk for unemployment than whites. These results are consistent with human capital theories advanced by neoclassical economists. When the interaction effects tested above are added to the models with race-education interactions, however, results for incarceration do not significantly change for race-incarceration variables. This suggests that lingering effects of incarceration faced by blacks and Hispanics are not explained by differences in racial returns to education.

Labor Force Nonparticipation, Joint Race and Incarceration Effects

Similar to the results presented above for unemployment, Table 3.7 examines the joint effects of race and incarceration on labor force participation. In models for race and incarceration, interactions show that blacks and Hispanics with a history of incarceration are much more likely to spend additional time out of the labor force ($p<0.001$) relative to whites. As a single set of interactions, blacks and Hispanics are statistically no different than whites in how years since last prison interview predict employment. However, when interaction terms for (a) race and history of incarceration and (b) race and years since last prison interview are considered, both terms become highly significant. Blacks and Hispanics with the history of incarceration are much more likely to spend additional time out of the labor force ($p<0.001$) relative to whites. As years since last jail interview increase, blacks and Hispanics are also significantly more likely ($p<0.01$) than whites to participate in the labor force. Thus, in order to explain the racial variances in how years since last year of prison interview predicts risk for being out of the labor force, controls are needed for the increased risks, relative to whites, faced by blacks and Hispanics with a history of incarceration in being out of the labor force.

To examine if any differences are explained by differences in human capital, I also test if racial differences for incarceration are mediated by changes in interactions of race and educational attainment. Relative to whites, the baseline model shows that racial differences in educational attainment place blacks at higher risk for being out of the labor force. These results are consistent with human capital theories advanced by neoclassical economists. When the interaction effects tested above are added to the models with race-education interactions, however, results for incarceration do not significantly change effects for race-incarceration variables. These findings suggest that increased, lingering effects of incarceration faced by blacks and Hispanics are not explained by differences in racial returns to education.

Conclusion

Recent audit studies suggest that incarceration and race play a role in the hiring of less-skilled workers by employers. While recent empirical research has documented that a history of incarceration plays a role in wages and employment, current work has not fully taken advantage of statistical methods and available data to investigate if race and history of incarceration determine employment outcomes. In this paper, I examine a panel of less-educated men from the NLSY79 using negative binomial regression models with fixed-effect error terms for individuals and periods. Controlling for marriage, education, urbanicity, welfare receipt, neighborhood unemployment, age, and work disability, I investigate the effects of race and incarceration on unemployment and labor force nonparticipation. Findings indicate that race and incarceration exert joint effects on employment outcomes.

Among less-skilled men, a history of incarceration increases likelihood of being unemployed during a calendar year by a factor of 1.65 ($p<0.001$), with the risk of unemployment declining by approximately 2.7% ($p<0.001$) for every consecutive year not interviewed in prison. Table 3.8 presents the results from interactions models estimated above for (a) race and history of incarceration and (b) race and years since last interview on unemployment. The effects of a history of incarceration do not vary significantly across racial groups; however, racial differences are seen for unemployment in years after last jail interview. For each consecutive year not interviewed in prison, the risk of unemployment declines by 4.5% per year among whites, 2.1% among African Americans ($p<0.10$), and 0.5% among Hispanics ($p<0.05$). These results suggest that being incarcerated has a similar impact on unemployment across racial groups, but that blacks and Hispanics are more likely to face increased risk for unemployment relative to whites in years after release from prison.

Among less-skilled men, a history of incarceration increases risk of being out of the labor force during a calendar year by a factor of 2.31 ($p<0.001$), with the risk of unemployment declining by approximately 1.9% ($p<0.001$) for every consecutive year not interviewed in prison. Table 3.9 presents the results from interactions models estimated above for (a) race and history of incarceration and (b) race and years since last interview on unemployment. Significant racial variances are observed for both history of incarceration and years after last jail interview. Given a history of incarceration, the risk for spending time out of a labor force during an interview year increases by a factor of 1.92 for whites, 2.59 for blacks ($p<0.001$), and 2.38 for Hispanics ($p<0.01$). For each consecutive year not interviewed in prison, the risk of unemployment declines by 0.1% per year among whites, 2.7% among African Americans ($p<0.01$), and 3.3% among Hispanics ($p<0.01$). These results suggest that blacks and Hispanics are significantly more likely to be out of the labor force during a given year relative to whites, but that risk for being out of the labor force declines more rapidly relative to whites in years since last jail interview.

In alternative model specifications, I also estimate alternative educational models to determine if racial differences may be explained by differences in human capital. These results find that racial differences in education explain unemployment and being out of the labor force, but that these differences do not impact the joint effects of race and incarceration on employment outcomes. Consequently, these models support arguments that other factors, such as discrimination or racial perceptions in ability to find work, may impact employment. Because less-educated men, in particular minorities, lack wealth (Conley 1997; Oliver and Shapiro 1995), stable family structures (Edelman et al. 2006; Mincy 2006; Western, Lopoo, and McLanahan 2004), and public assistance (Blank 2001), these individuals most likely must find some form of subsistence for survival. Work by urban ethnographers suggests that those with criminal histories, in particular minorities, wind up homeless and working in informal or illegal labor markets (Anderson 1990b, 1999; Duneier 1999; Edelman et al. 2006; Edin, Nelson, and Paranal 2004).

Limitations of this analysis point to potential avenues for future research. For example, measuring incarceration through interviews conducted annually in jail or prison from 1979-1994 and biannually from 1994-2004 underestimates criminal justice involvement. Data analysis of research where incarceration histories are supplemented through self-reports and background checks would lead to more precise measurement of incarceration effects. Additional variables, such as measuring delinquent activities across multiple waves and annual measures of substance abuse, would provide more robust controls and alternative sampling frames. Likewise, a more extensive set of racial categories (including multiracial identification) would provide a better context for measuring the effects of incarceration across racial groups.

Table 3.1: NLSY79 sample characteristics among less-skilled men for 1979 interviews, by race and for full sample

Variable	Full Sample	Whites	Blacks	Hispanics
Demographic Variables				
Age	17.80	17.95	17.69	17.5
	(2.31)	(2.37)	(2.27)	(2.24)
Respondent Married	(0.083)	0.107	0.0346	0.082
	(0.276)	(0.308)	(0.182)	(0.274)
Health Disability limiting work	0.0245	0.026	0.0241	0.0225
	(0.156)	(0.160)	(0.153)	(0.148)
Respondent Family Received Some form of Public Assistance [TANF, Food Stamps, AFDC, public housing, etc.]	0.036	0.031	0.041	0.043
	(0.185)	(0.172)	(0.198)	(0.203)
Respondent Lives in Urban Area	0.645	0.537	0.724	0.884
	(0.478)	(0.498)	(0.447)	(0.320)
Region of Country Respondent Resides				
North	0.198	0.202	0.186	0.202
	(0.398)	(0.402)	(0.388)	(0.402)
South	0.355	0.299	0.544	0.236
	(0.478)	(0.459)	(0.498)	(0.424)
West	0.204	0.180	0.088	0.471
	(0.405)	(0.387)	(0.286)	(0.499)
Midwest	0.224	0.297	0.164	0.073
	(0.414)	(0.454)	(0.374)	(0.253)
Education Variables				
High School Diploma	0.281	0.336	0.236	0.166
	(0.00)	(0.476)	(0.401)	(0.387)
G.E.D.	0.034	0.042	0.030	0.015
	(0.181)	(0.200)	(0.169)	(.121)
High School Dropout	0.216	0.195	0.204	0.307
	(0.408)	(0.393)	(0.431)	(0.461)
More Than High School	0	0	0	0
Proportion In School	0.482	0.441	0.544	0.526
	(0.499)	(0.496)	(0.498)	(0.499)
Highest Grade Completed in May 1979	9.98	10.25	9.98	9.35
	(1.83)	(1.78)	(1.72)	(2.01)
Incarceration Variables				
Respondent History of Incarceration	0.005	0.004	0.006	0.005
	(0.0691)	(0.063)	(0.079)	(0.072)
Respondent Interviewed in Jail	0.005	0.004	0.006	0.005
	(0.0691)	(0.063)	(0.079)	(0.072)
Years Since Prior Incarceration	0	0	0	0
Employment Variables				
Weeks Unemployed in Calendar Year	4.74	3.71	6.65	5.15
	(9.408)	(7.98)	(11.54)	(9.53)
Weeks Not in Labor Force	10.85	8.96	14.11	13.35
	(15.85)	(14.35)	(17.55)	(16.61)
Self-Employed in Unincorporated Business	0.014	0.019	0.0048	0.0138
	(0.118)	(0.136)	(0.068)	(0.116)
Local Unemployment Rate	2.56	2.63	2.384	2.635
	(0.736)	(0.710)	(0.601)	(0.921)
N	4610	2599	1244	767

Table 3.2: Mean and standard deviation for work & incarceration variables by year in full sample

Year	Sample Size	Ever Incarcerated	Interviewed in Jail	Years since last jail interview	Weeks Unemployed	Weeks no Labor Force Participation	High School Dropout	Obtained GED	Highest Grade Completed
1979	4610	0.005	0.005	0	4.74	10.85	0.216	0.034	9.98
		(0.070)	(0.070)		(9.40)	(15.85)	(.411)	(0.181)	(1.83)
1980	4340	0.015	0.012	0.003	6.84	11.82	0.243	0.050	10.47
		(0.121)	(0.109)	(0.052)	(11.6)	(16.62)	(0.429)	(0.217)	(1.61)
1981	4367	0.023	0.014	0.012	7.23	9.70	0.279	0.066	10.82
		(0.149)	(0.115)	(0.129)	(12.20)	(15.63)	(0.448)	(0.247)	(1.51)
1982	4335	0.031	0.019	0.021	7.84	8.28	0.290	0.081	11.08
		(0.172)	(0.134)	(0.204)	(12.80)	(14.67)	(0.453)	(0.272)	(1.48)
1983	4386	0.039	0.022	0.036	7.39	6.85	0.297	0.099	11.26
		(0.195)	(0.145)	(0.296)	(12.75)	(13.64)	(0.456)	(0.299)	(1.48)
1984	4311	0.047	0.021	0.058	5.33	5.51	0.296	0.111	11.34
		(0.210)	(0.143)	(0.413)	(10.73)	(12.27)	(0.456)	(0.315)	(1.53)
1985	3732	0.058	0.025	0.083	5.00	5.72	0.319	0.112	11.30
		(0.233)	(0.155)	(0.526)	(10.45)	(12.99)	(0.466)	(0.315)	(1.59)
1986	3631	0.067	0.0295	0.109	4.66	5.59	0.316	0.116	11.34
		(0.250)	(0.169)	(0 .644)	(10.35)	(12.86)	(0.464)	(0.320)	(1.62)
1987	3542	0.074	0.034	0.129	4.07	5.47	0.302	0.125	11.41
		(0.261)	(0.180)	(0.77)	(9.29)	(12.54)	(0.459)	(.331)	(1.63)
1988	3588	0.084	0.033	0.155	4.07	5.62	0.296	0.135	11.44
		(0.278)	(0.179)	(0.824)	(9.29	(12.91)	(.456)	(0.341)	(1.68)
1989	3621	0.089	0.034	0.210	3.26	5.47	0.292	0.137	11.46
		(0.284)	(0.181)	(1.06)	(8.25)	(12.54)	(0.454)	.344)	(1.69)
1990	3570	0.097	0.032	0.258	2.97	5.24	0.287	0.142	11.50
		(0.296)	(0.175)	(1.21)	(7.63)	(12.57)	(0.452)	(0.348)	(1.72)
1991	3117	0.100	0.0352903	0.277	4.11	5.92	0.262	0.140	11.63
		(0.300)	(0.184)	(1.30)	(9.42)	(13.46)	(0.439)	(0.346)	(1.66)
1992	3137	0.109	0.0380	.325	4.01	6.30	0.257	0.144	11.66
		(.311)	(0.191)	(1.45)	(9.53)	(14.25)	(0.436)	(0.351)	(1.67)
1993	3118	0.115	0.041	0.422	3.60	6.55	0.244	0.155	11.69
		(0.318)	(0.198)	(1.70)	(9.101)	(14.50)	(0.429)	(0.362)	(1.65)
1994	3092	0.120	0.043	0.410	2.77	6.83	0.238	0.158	11.74 (1.66)

		(0.324)	(0.202)	(1.75)	(7.96)	(15.08)	(0.426)	(.364)	
1996	3006	0.125	0.044	0.518	2.40	6.67	0.225	0.170	11.79
		0.331	(0.204)	(2.08)	(7.57)	(14.95)	(0.417)	(0.375)	(1.69)
1998	2860	0.127	.0423	0.615	1.88	6.27	0.208	0.169	11.85
		(0.333)	(0.201)	(2.43)	(6.58)	(14.81)	(0.405)	(0.375)	(1.70)
2000	2740	0.132	0.039	0.782	1.67	6.35	0.208	.172	11.89
		(0.338)	(0.193)	(2.86)	(6.24)	(14.97)	(0.405)	(0.377)	(1.74)
2002	2631	0.139	0.035	0.936	2.59	6.52	0.200	0.179	11.92
		(.346)	(0.184)	(3.29)	(8.02)	(15.10)	(0.401)	(0.383)	(1.72)
2004	2560	0.123	0.022	1.07	2.99	6.83	0.194	0.181	11.94
		(0.324)	(0.147)	(3.77)	(10.06)	(16.12)	(0.395)	(0.385)	(1.74)

Table 3.3: Mean and standard Deviation for population and incarceration variables for each interview year, by racial classification

	White				Black				Hispanic			
Year	Sample population	Ever Incarcerated	Interviewed in Jail	Years since last jail interview	Sample Size	Ever Incarcerated	Interviewed in Jail	Years since last jail interview	Sample Size	Ever Incarcerated	Interviewed in Jail	Years since last jail interview
1979	2599	0.004	0.004	0	1224	0.006	0.006	0	767	0.005	0.005	0
		(0.064)	(0.064)			(0.079)	(0.079)			(0.072)	(0.072)	
1980	2455	0.011	0.008	0.003	1179	0.023	0.023	0.001	706	0.0128	.008	0.004
		(0.106)	(0.089)	(0.057)		(0.152)	(0.149)	(0 .029)		(0.112)	(.091)	(0.064)
1981	2445	.019	0.009	0.013	1179	0.034	0.024	0.010	724	.0179558	0.011	0.011
		(0.135)	(.094)	(0.139)		(0.181)	(0.153)	(0.099)		(0.132)	(.104)	(0.138)
1982	2449	0.025	0.013	0.019	1198	0.044	0.028	0.025	711	0.030	0.020	0.02
		(0.154)	(0.115)	(0.198)		(0.205)	(0.165)	(0.202)		(0.169)	(0.139)	(0.227)
1983	2486	0.028	0.010	0.033	1181	0.067	0.047	0.042	719	0.036	0.0181	0.0389
		(0.164)	(0.101)	(0.275)		(0.249)	(0.212)	(0.314)		(0.186)	(0.133)	(.331)
1984	2423	0.034	0.014	0.051	1173	0.075	0.035	0.072	715	0.043	0.0021	0.056
		(0.181)	(0.119)	(0.402)		(0.263)	(0.183)	(0.418)		(0.203)	(0.143)	(0.439)
1985	1958	0.043	0.016	0.076	1080	0.089	0.041	0.109	694	0.0519	0.026	0.063
		(0.202)	(0.124)	(0.527)		(0.284)	(0.197)	(0.555)		(0.221)	(.159)	(0.473)
1986	1908	0.047	0.016	0.101	1051	0.108	0.053	0.145	672	0.061	0.031	0..076
		(0.212)	(0.124)	(.649)		(0.311)	(0.224)	(0.698)		(0.239)	(0.174)	(0.530)
1987	1864	.0494	0.019	0.112	1030	0.121	0.064	0.175	648	.068	0.029	0.106
		(0.216)	(0.135)	(.755)		(0.326)	(0.245)	(0.850)		(0.251)	(0.168)	(0.696)
1988	1886	0.056	0.017	0.131	1051	0.146	0.069	0.213	651	0.069	0.020	0.127
		(0.229)	(0.131)	(0.814)		(0.352)	(0.254)	(0.909)		(0.253)	(0.140)	(0.697)
1989	1887	0.057	0.019	0.180	1057	0.155	0.069	0.278	677	0.072	0.021	0.189
		(0.232)	(0.136)	(1.05)		(0.362)	(0.253)	(1.13)		(0.259)	(0.142)	(0.986)
1990	1871	0.061	0.018	0.204	1044	0.171	0.066	0.359	659	0.082	0.017	0.253
		(0.240)	(0.133)	(1.15)		(0.376)	(0.249)	(1.33)		(0.274)	(0.128)	(1.18)
1991	1419	0.044	0.011	0.168	1025	0.185	0.075	0.429	673	0.089	0.027	0.278
		(0.206)	(0.102)	(1.11)		(0.388)	(0.263)	(1.51)		(0.285)	(0.161)	(1.30)
1992	1428	0.047	0.009	0.208	1030	0.200	.0845	0.475	677	0.102	0.028	0.343
		(0.212)	(0.095)	(1.28)		(.400)	(0.278)	(1.64)		(0.302)	(0.165)	(1.48)

1993	1419	0.049	0.012	0.253	1035	0.208	0.086	0.643	664	0.110	0.033	0.438
		(0.216)	(0.109)	(1.44)		(0.405)	(0.280)	(1.95)		(0.313)	(.179)	(1.73)
1994	1407	0.052	0.014	0.241	1029	0.215	0.083	0.627	656	0.116	0.043	0.425
		(0.222)	(0.118)	(1.47)		(0.410)	(0.275)	(2.02)		(0.320)	(0.202)	(1.83)
1996	1374	0.059	0.015	0.353	1003	0.221	0.080	0.778	629	0.118	0.049	0.467
		(0.235)	(0.122)	(1.91)		(0.415)	(0.271)	(2.32)		(0.322)	(0.216)	(2.02)
1998	1316	0.060	.0137	0.432	937	0.224	0.083	0.907	607	0.124	0.041	0.565
		(0.237)	(0.116)	(2.26)		(.417)	(0.276)	(2.76)		(0.329)	(0.198)	(2.30)
2000	1259	0.065	0.015	0.507	900	0.241	0.078	1.17	581	0.131	0.031	0.774
		(0.246)	(0.121)	(2.57)		(0.427)	(0.267)	(3.21)		(0.337)	(0.173)	(2.81)
2002	1206	0.066	0.017	0.594	888	0.245	0.064	1.44	537	0.132	0.030	0.877
		(0.247)	(0.127)	(2.96)		(0.430)	(0.245)	(3.72)		(0.339)	(0.170)	(3.11)
2004	1164	0.060	0.013	0.658	860	0.210	0.040	1.65	536	0.119	0.015	1.05
		(0.237)	(0.112)	(3.28)		(.407)	(0.194)	(4.32)		(0.324)	(0.121)	(3.68)

Table 3.4: Negative binomial regression incidence ratios and standard errors for annual weeks of unemployment among less-skilled men

	Combined Sample		White Men		Black Men		Hispanic Men	
Variables	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained
Incarceration Variables								
Interviewed In Jail	0.430***	0.432***	0.477***	0.473***	0.421***	0.422***	0.411***	0.417***
	(0.026)	(0.026)	(0.054)	(0.053)	(0.034)	(0.034)	(0.061)	(0.062)
History of Incarceration	1.654***	1.689***	1.648***	1.687***	1.526***	1.541***	1.676***	1.726***
	(0.074)	(0.076)	(0.136)	(0.139)	(0.097)	(0.098)	(0.176)	(0.181)
Years since last jail Interview	0.973***	0.973***	0.959**	0.959***	0.975*	0.974*	0.998	0.999
	(0.007)	(0.006)	(0.012)	(0.012)	(0.010)	(0.010)	(0.015)	(0.015)
Education								
High School Dropout	1.294***	-	1.466***	-	1.176***	-	1.243***	
	(0.026)		(0.046)		(0.040)		(0.058)	
High School [Reference]								
GED	1.245***	1.161***	1.451***	1.320***	1.151**	1.117**	1.014	0.929
	(0.039)	(0.035)	(0.069)	(0.062)	(0.057)	(0.054)	(0.073)	(0.064)
More than High School	0.873***	-	0.879**	-	1.062	-	0.970	
	(0.033)		(0.048)		(0.054)		(0.083)	
In School Fulltime during interview	1.054+	0.926***	1.029	0.870***	0.830**	0.971	1.145*	1.005
	(0.030)	(0.025)	(0.043)	(0.035)	(0.054)	(0.047)	(0.076)	(0.061)
Highest Grade level completed	-	0.955***	-	0.928***	-	0.961***	-	0.980+
		(0.005)		(0.008)		(0.010)		(0.011)
Demographic Variables								
Age	1.472***	1.607***	1.413***	1.671***	1.642***	1.759***	1.398**	1.440***
	(0.077)	(0.087)	(0.112)	(0.137)	(0.145)	(0.160)	(0.167)	(0.176)
Age-Square	0.986***	0.983***	0.987***	0.981***	0.982***	0.980***	0.989*	0.988***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Age-cubed	1.0001***	1.0001***	1.0001***	1.0001***	1.0001***	1.0001***	1.0001*	1.0001**
	(0.000002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00005)	(0.000005)

Disabled	0.953	0.957	0.924	0.931	0.998	1.000	0.918	0.922
	(0.038)	(0.038)	(0.057)	(0.058)	(0.062)	(0.062)	(0.091)	(0.091)
Married	0.652***	0.957	0.666***	0.663***	0.700***	0.696***	0.644***	0.641***
	(0.012)	(0.038)	(0.018)	(0.018)	(0.026)	(0.026)	(0.028)	(0.028)
Received Public Assistance	1.942***	1.974***	2.160***	2.213***	1.708***	1.725***	1.806***	1.824***
	(0.042)	(0.043)	(0.070)	(0.072)	(0.062)	(0.063)	(0.092)	(0.094)
Resides in Urban Area	0.990	0.990	0.978	0.986	0.970	0.963	0.948	0.948
	(0.021)	(0.021)	(0.027)	(0.027)	(0.037)	(0.037)	(0.070)	(0.070)
Neighborhood Unemployment	1.127***	1.127***	1.147***	1.148***	1.093***	1.093***	1.154***	1.155***
	(0.010)	(0.010)	(0.014)	(0.014)	(0.018)	(0.018)	(0.021)	(0.021)
Region [Northeast]								
Northeast [Reference]								
South	1.032	1.026	0.976	0.985	1.066	1.058	0.879*	0.864*
	(0.027)	(0.027)	(0.041)	(0.042)	(0.045)	(0.045)	(0.055)	(0.054)
Midwest	1.152***	1.146***	1.170***	1.180***	1.240***	1.236***	0.984	0.972
	(0.033)	(0.033)	(0.048)	(0.049)	(0.062)	(0.062)	(0.083)	(0.082)
West	1.023	1.025	1.096+	1.117**	1.187**	1.182***	0.862**	0.851***
	(0.031)	(0.031)	(0.052)	(0.053)	(0.076)	(0.076)	(0.049)	(0.049)
Individual Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Number of Individual-Year Observations	62024	62024	30387	30387	19627	19627	12010	12010
Number of Individuals	3905	3905	2071	2071	1145	1145	689	689
Log-Likelihood	-95604.80	-108714.52	-42698.16	-42660.6	-34943.52	-34826.39	-17813.19	-17749.59

+p<0.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 3.5: Negative binomial regression incidence ratios and standard errors for annual weeks out of labor force among less-skilled men

	Combined Sample		White Men		Black Men		Hispanic Men	
Variables	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained
Incarceration Variables								
Interviewed In Jail	2.754***	2.760***	2.841***	2.864***	2.656***	2.653***	2.663***	2.657***
	(0.108)	(0.109)	(0.212)	(0.215)	(0.143)	(0.144)	(0.247)	(0.248)
History of Incarceration	2.310***	2.363***	1.885***	1.922***	2.541***	2.541***	2.377***	2.519***
	(0.087)	(0.089)	(0.129)	(0.133)	(0.136)	(0.136)	(0.208)	(0.221)
Years since last jail Interview	0.981***	0.980***	1.008	1.007	0.966***	0.966***	0.964***	0.965***
	(0.006)	(0.006)	(0.009)	(0.009)	(0.008)	(0.008)	(0.013)	(0.013)
Education								
High School Dropout	1.446***	-	1.495***	-	1.383***	-	1.510***	-
	(0.028)		(0.045)		(0.045)		(0.065)	
High School [Reference]								
GED	1.356***	1.245***	1.505***	1.366***	1.185**	1.146***	1.400**	1.204***
	(0.037)	(0.033)	(0.066)	(0.059)	(0.050)	(0.047)	(0.085)	(0.070)
More than High School	0.894***	-	1.031	-	0.755***	-	0.819**	-
	(0.028)		(0.045)		(0.043)		(0.062)	
In School Fulltime during interview	2.298***	1.941***	2.328***	2.012***	2.232***	1.891***	2.304***	1.805***
	(0.053)	(0.042)	(0.081)	(0.066)	(0.087)	(0.070)	(0.124)	(0.088)
Highest Grade level completed	-	0.935***	-	0.936***	-	0.911***	0.819***	0.954***
		(0.005)		(0.007)		(0.008)	(0.062)	(0.010)
Demographic Variables								
Age	0.881***	0.915***	0.884***	0.926***	0.886***	0.844***	0.922***	0.934**
	(0.011)	(0.012)	(0.017)	(0.019)	(0.019)	(0.019)	(0.026)	(0.027)
Age-Square	1.002***	1.001***	1.002***	1.001***	1.002***	1.003***	1.001*	1.001*
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)	(0.0003)	(0.001)
Disabled	1.880***	1.878***	1.929***	1.930***	1.751***	1.751***	2.069***	2.060***
	(0.052)	(0.052)	(0.085)	(0.086)	(0.076)	(0.076)	(0.128)	(0.128)
Married	0.546***	0.540***	0.552***	0.547***	0.586***	0.593***	0.535***	0.533***
	(0.010)	(0.010)	(0.015)	(0.015)	(0.021)	(0.021)	(0.021)	(0.021)

Received Public Assistance	2.132***	2.175***	2.095***	2.151***	2.069***	2.049***	2.225***	2.297***
	(0.042)	(0.043)	(0.066)	(0.068)	(0.065)	(0.065)	(0.096)	(0.099)
Resides in Urban Area	0.985	0.985	0.951+	0.954+	0.928*	0.925*	1.050	1.054
	(0.019)	(0.019)	(0.025)	(0.026)	(0.031)	(0.031)	(0.065)	(0.066)
Neighborhood Unemployment	1.021***	1.021***	1.005	1.005	1.036*	1.036*	1.049***	1.051***
	(0.008)	(0.008)	(0.012)	(0.012)	(0.015)	(0.015)	(0.017)	(0.017)
Region								
Northeast [Reference]	-	-	-	-	-	-	-	-
South	0.940**	0.934***	0.984	1.003	0.932+	0.923*	0.825***	0.795***
	(0.023)	(0.023)	(0.039)	(0.040)	(0.037)	(0.037)	(0.047)	(0.045)
Midwest	0.886***	0.881***	0.909*	0.911*	1.070	1.070	0.708***	0.698***
	(0.024)	(0.024)	(0.036)	(0.036)	(0.051)	(0.051)	(0.061)	(0.060)
West	1.057*	1.065*	1.237***	1.267***	1.372***	1.379***	0.763***	0.751***
	(0.029)	(0.029)	(0.053)	(0.055)	(0.079)	(0.080)	(0.039)	(0.039)
Individual Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Number of Individual-Year Observations	63013	63013	31034	31034	19750	19750	12512	12512
Number of Individuals	3941	3941	2079	2079	1138	1138	725	725
Log-Likelihood	-95604.80	-108714.52	-46490.32	-46421.36	-40223.60	-40054.54	-22136.19	-22086.7

+p<0.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 3.6: Negative binomial regression incidence ratios and standard errors for annual weeks of unemployment among less-skilled men, indirect effects

	Race and Incarceration Interactions			Race and Incarceration Interactions, with Race and Education Interaction Controls			
Variables	History of Incarceration	Years Since Last Incarceration	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained	Educational Categories
Incarceration Variables							
Interviewed In Jail	0.431***	0.434***	0.433***	0.429***	0.430***	0.430***	0.432***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
History of Incarceration	1.585***	1.647***	1.699***	1.644***	1.660***	1.641***	1.774***
	(0.101)	(0.074)	(0.119)	(0.074)	(0.108)	(0.074)	(0.126)
Years since last jail Interview	0.974***	0.957***	0.954***	0.975***	0.974***	0.963***	0.955***
	(0.007)	(0.010)	(0.011)	(0.007)	(0.007)	(0.010)	(0.011)
Interactions for Race and Incarceration							
History of Incarceration * Black	1.070	-	0.981	-	0.957	-	0.884
	(0.068)		(0.077)		(0.064)		(0.072)
History of Incarceration * Hispanic	1.038	-	0.893	-	1.084	-	0.932
	(0.085)		(0.092)		(0.092)		(0.098)
Years Since Last Jail Interview * Black	-	1.025*	1.027+	-		1.011	1.024
		(0.012)	(0.015)			(0.012)	(0.015)
Years Since Last Jail Interview * Hispanic	-	1.031*	1.043*	-		1.036*	1.043*
		(0.014)	(0.018)			(0.014)	(0.018)
Education							
High School Dropout	1.290***	1.290***	1.291***	-	-	-	
	(0.026)	(0.026)	(0.027)				
High School [Reference]							
GED	1.228***	1.228***	1.227***	1.251***	1.250***	1.258***	1.250***
	(0.039)	(0.039)	(0.039)	(0.058)	(0.059)	(0.059)	(0.059)
More than High School	0.906**	0.908*	0.908*	-	-	-	
	(0.034)	(0.035)	(0.035)				
In School Fulltime during interview	1.051+	1.051+	1.051+	0.924**	0.924**	0.925**	0.925**
	(0.030)	(0.030)	(0.030)	(0.025)	(0.025)	(0.025))	(0.025)
Highest Grade level completed	-	-	-	0.949***	0.949***	0.949****	0.949***

				(0.006)	(0.006)	(0.006)	(0.006)
Interactions for Race and Education							
Black*GED	-	-	-	0.916	0.925	0.913	0.925
				(0.060)	(0.061)	(0.060)	(0.061)
Hispanic*GED	-	-	-	0.764***	0.758**	0.751***	0.756***
				(0.063)	(0.063)	(0.062)	(0.063)
Black*Grade Level Attained	-	-	-	1.016***	1.017**	1.016***	1.017***
				(0.002)	(0.002)	(0.002)	(0.002)
Hispanic*Grade Level Attained	-	-	-	1.003	1.002	1.002	1.002
				(0.003)	(0.003)	(0.003)	(0.003)
Demographic Variables							
Age	1.470***	1.469***	1.470***	1.617**	1.614***	1.618***	1.615***
	(0.077)	(0.077)	(0.077)	(0.087)	(0.087)	(0.087)	(0.087)
Age-Square	0.986***	0.986***	0.986***	0.983**	0.983***	0.983***	0.983***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age-Cubed	1.0002***	1.0002***	1.0002***	1.0002**	1.0002***	0.957	
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.038)	
Disabled	0.954	0.954	0.954	0.958	0.958	0.958	0.958
	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)
Married	0.652***	0.652**	0.652***	0.662***	0.663***	0.663***	0.663***
	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Received Public Assistance	1.945***	1.946**	1.945***	1.957***	1.957***	1.958***	1.957***
	(0.042)	(0.042)	(0.042)	(0.043)	(0.043)	(0.043)	(0.043)
Resides in Urban Area	0.989	0.988	0.988	0.968	0.969	0.968	0.968
	(0.021)	(0.020)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Neighborhood Unemployment	1.127***	1.126***	1.127***	1.132***	1.132***	1.131***	1.131***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Region							
Northeast [Reference]							
South	1.031	1.031	1.031	0.993	0.993	0.994	0.993
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Midwest	1.151***	1.152***	1.151***	1.156**	1.156***	1.156***	1.156***
	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
West	1.024	1.022	1.023	1.054+	1.053+	1.053+	1.053+
	(0.031)	(0.031)	(0.031)	(0.033)	(0.033)	(0.033)	(0.033)
Individual Fixed Effects	YES	YES	YES	YES	YES	YES	YES

Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Number of Individual-Year Observations	62024	62024	62024	61885	61885	61885	61885
Number of Individuals	3905	3905	3905	3904	3904	3904	3904
Log-Likelihood	-95608.546	-95606.03	-95605.358	-95343.60	-95342.24	-95340.3	-95339.17

+p<0.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 3.7: Negative binomial regression incidence ratios and standard errors for annual weeks out of labor force among less-skilled men, indirect effects

Variables	Race and Incarceration Interactions			Race and Incarceration Interactions, with Race and Education Interaction Controls			
	History of Incarceration	Years Since Last Incarceration	Educational Categories	Highest Grade Attained	Educational Categories	Highest Grade Attained	Educational Categories
Incarceration Variables							
Interviewed In Jail	2.748***	2.754***	2.723***	2.754***	2.749**	2.748**	2.724***
	(0.108)	(0.109)	(0.108)	(0.109)	(0.109)	(0.109)	(0.108)
History of Incarceration	2.004***	2.311***	1.918***	2.339***	2.090**	2.344**	2.019***
	(0.101)	(0.087)	(0.100)	(0.089)	(0.108)	(0.089)	(0.108)
Years since last jail Interview	0.981***	0.985*	0.999	0.980***	0.981**	0.988	0.999
	(0.006)	(0.007)	(0.008)	(0.006)	(0.006)	(0.008)	(0.008)
Interactions for Race and Incarceration							
History of Incarceration * Black	1.251***	-	1.336***	-	1.189***	-	1.267***
	(0.060)		(0.072)		(0.061)		(0.072)
History of Incarceration * Hispanic	1.138*	-	1.241***	-	1.127+	-	1.167*
	(0.070)		(0.086)		(0.073)		(0.085)
Years Since Last Jail Interview * Black	-	0.997	0.974**	-		0.990	0.973**
		(0.009)	(0.009)			(0.009)	(0.009)
Years Since Last Jail Interview * Hispanic	-	0.984	0.968**	-		0.982+	0.966**
		(0.011)	(0.012)			(0.011)	(0.012)
Education							
High School Dropout	1.453***	1.451***	1.451***	-	-	-	
	(0.028)	(0.028)	(0.028)				
High School [Reference]	-	-	-	-	-	-	
GED	1.365***	1.367***	1.367***	1.301***	1.330***	1.294**	1.319**
	(0.038)	(0.038)	(0.038)	(0.056)	(0.058)	(0.056)	(0.058)

More than High School	0.946+	0.947+	0.950	-	-	-	
	(0.030)	(0.030)	(0.030)				
In School Fulltime during interview	2.328***	2.294***	2.294***	1.944***	1.945***	1.943**	1.991**
	(0.055)	(0.053)	(0.053)	(0.043)	(0.043)	(0.043)	(0.044)
Highest Grade level completed	-	-	-	0.930***	0.931**	0.930**	0.941**
				(0.005)	(0.005)	(0.005)	(0.005)
Interactions for Race and Education							
Black*GED	-	-	-	0.872*	0.839**	0.877*	0.841**
				(0.050)	(0.049)	(0.051)	(0.049)
Hispanic*GED	-	-	-	1.068	1.045	1.083	1.054
				(0.075)	(0.074)	(0.077)	(0.074)
Black*Grade Level Attained	-	-	-	1.013**	1.011**	1.013**	1.011***
				(0.002)	(0.002)	(0.002)	(0.002)
Hispanic*Grade Level Attained	-	-	-	1.003	1.003	1.004	1.003
				(0.002)	(0.003)	(0.002)	(0.0025)
Demographic Variables							
Age	0.881***	0.881***	0.881***	0.914**	0.914**	0.914**	0.910***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
Age-Square	1.002***	1.002***	1.002***	1.001**	1.001***	1.001**	1.001***
	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0. .0002)	(0.0003)	(0. .0002)
Disabled	1.881***	1.881***	1.878***	1.877**	1.877**	1.876**	1.875***
	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)
Married	0.547***	0.545***	0.547***	0.548**	0.548**	0.548**	0.547***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Received Public Assistance	2.136***	2.132***	2.135***	2.166**	2.171**	2.166**	2.169***
	(0.042)	(0.042)	(0.042)	(0.043)	(0.043)	(0.043)	(0.043)
Resides in Urban Area	0.979	0.986	0.980	0.967+	0.966+	0.967+	0.963+
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Neighborhood Unemployment	1.022***	1.022***	1.023***	1.022**	1.022**	1.022**	1.023**
	(0.008)	(0.008)		(0.008)	(0.008)	(0.008)	(0.008)

			(0.008)				
Region							
Northeast [Reference]	-						
South	0.938**	0.939*	0.939*	0.915**	0.917**	0.915**	0.922**
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Midwest	0.890***	0.885***	0.890***	0.894**	0.896**	0.894**	0.897**
	(0.025)	(0.024)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
West	1.064*	1.056*	1.066*	1.084**	1.089**	1.085**	1.086**
	(0.029)	(0.029)	(0.029)	(0.031)	(0.031)	(0.031)	(0.031)
Individual Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Number of Individual-Year Observations	62024	62024	62024	61885	61885	61885	61885
Number of Individuals	3905	3905	3905	3904	3904	3904	3904
Log-Likelihood	-108997.28	-109007.36	-108584.77	-108691.4	-108685.68	-108689.93	-108680.63

+p<0.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 3.8: Racial variation in the effects of prior incarceration and years since last jail interview on annual weeks unemployment (based on models presented in Table 3.6)

	Weeks Unemployed		
Race	Prior Incarceration [odds ratio]	Years since last jail interview [average decline per year]	Prior Incarceration and Years since last Jail Interview
White [baseline]	1.59	-4.3%*	1.70,-4.6%
Black	1.69	-1.9%*	1.67,-2.1%+
Hispanic	1.64	-1.4%*	1.52,-0.5%*

+p<0.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 3.9: Racial variation in the effects of prior incarceration and years since last jail interview on annual weeks out of labor force (based on models presented in Table 3.7)

	Weeks Out of Labor Force		
Race	Prior Incarceration [odds ratio]	Years since last jail interview [average decline per year]	Prior Incarceration and Years since last Jail Interview
White [baseline]	2.04	-1.5%	1.92,-0.1%
Black	2.51***	-1.8%	2.57***,-2.7%**
Hispanic	2.28*	-3.0%	2.38**,-3.3%**

+p<0.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

CHAPTER FOUR:

Genetic, Individual, Familial, and Community Mediators of the Association between Father's History of Incarceration and Adult Delinquency/Arrest

Introduction

As Gottfredson and Hirashi (1990) have noted, intergenerational patterns of crime and delinquency are well documented in existing literature. Early studies by Glueck and Glueck (1950) and Robins (1966) observed correlations between child's delinquency/arrest and parental incarceration with basic statistical correlations. More recent methods have replicated these findings in intergenerational studies of crime using more advanced statistical methods (Rowe and Farrington 1997; Thornberry 2005; Thornberry et al. 2003) and in national samples (Murray, Janson, and Farrington 2007; Roettger 2006).

While the correlation between intergenerational crime and incarceration is well established, a wide number of possible causal factors have been attributed to the phenomenon. Notable factors include pervasiveness of poverty and lack of opportunity in inner-city neighborhoods (Thornberry et al. 2003; Wilson 1996b), genetic propensities (Beaver 2006; Rowe and Farrington 1997; Rowe and Osgood 1984), issues of family (Beaver 2006; Rowe and Farrington 1997; Thornberry 2005; Thornberry et al. 2003), class-like structures (Hagan and Palloni 1990), and relative disadvantage of individuals (Sampson and Laub 1997; Sampson and Laub 2003a). Comparative studies examining intergenerational crime and arrest in the context of these associations may hence provide empirical insight into explaining observed father-child links in incarceration.

At the societal level, intergenerational incarceration may play a large role in perpetuating inequality. In recent decades, incarceration in the U.S. has become a lifecourse event among less-educated and minority men. At over five times the rate of other developed nations, the extent of incarceration in the U.S. is unique among Western societies (Gottshalk 2006; Pettit and Western 2004b; Western 2006; Western and Beckett 1999). As Harper and MacLanahan (2002) note, relatively little work has focused on the long-term effects of incarceration on children of incarcerated parents. Currently lacking in the literature is the use of contemporary and representative datasets to document if intergenerational patterns of crime/incarceration hold.

Using twin and nationally representative sub-samples from the National Longitudinal Survey of Adolescent Health (Add Health), I test if father's incarceration is linked to delinquency and arrest among young men. I use multilevel models to examine how the effect of father's history of incarceration may change when molecular genetic, individual, family, and community variables are introduced. Using a nationally-representative sample, I test how individual, family, and community variables also alter the effect of father's incarceration in predicting deviance and arrest. Through this analysis, I explore if father's incarceration remains a unique and robust predictor in explaining son's delinquency and arrest.

Intergenerational Crime & Incarceration: Theoretical Links

To date, a number of studies find intergenerational patterns of crime and delinquency. These studies find a link between paternal incarceration and adult son's delinquency and arrest but attribute these outcomes to a variety of mechanisms. The goals of this paper are (a) to demonstrate that father's incarceration is robust in predicting adult son's delinquency and arrest given a number of factors in a nationally-representative dataset and (b) to identify possible explanatory mechanisms for why intergenerational patterns of father-son delinquency and arrest may exist. In this section I discuss leading theories and their implications for the empirical analysis that follows.

Genetic Propensities for Crime

Prior research suggests that correlations in father-son arrest may result from genetic propensities to commit crime. Two major research initiatives that have addressed this potential issue are Robins' (1966) study of sociopathic behavior among children and analysis by David Rowe and colleagues (Rowe 1983; Rowe and Farrington 1997; Rowe and Osgood 1984).

To study delinquents with high rates of antisocial (sociopathic) behaviors, Robins (1966) analyzed a sample of delinquents referred to a St. Louis psychiatric clinic in the 1920's. Among Robins analysis were findings that, given a father's sociopathic diagnosis, 32% of her sample had a sociopathic diagnosis, compared with only 16% of delinquents diagnosed as sociopathic among non-sociopathic fathers. Among children with sociopathic diagnosis, 35% of fathers had a history of arrest. A strong correlation also existed between number of antisocial behaviors exhibited by fathers and sons. Robins interpreted this evidence as consistent with a genetic explanation of crime, along with other associations such as family structure, parental supervision, and neighborhood characteristics.

Using data from the Cambridge Study of Delinquent Development and twin datasets, David Rowe and colleagues have argued that genetic factors may underlie causes of crime and delinquency (Rowe 1983; Rowe and Farrington 1997; Rowe and Osgood 1984). Rowe and Osgoode (1984) analyzed a set of 530 school-age twins, estimating up to 47% of delinquency was attributable to heritability. Rowe and Farrington (1997) used a set of 344 families with two or more children from the Cambridge Study of Delinquent Development, finding a strong linkage between same-sex family convictions among fathers and sons, mothers and daughters, and sibling pairs. Work by David Farrington and colleagues has replicated the significance of family convictions between siblings in the U.S. and Sweden, with results suggesting neighborhood poverty, age of mothers, and other familial variables as possible explanatory variables, in addition to possible genetic causation (Farrington 1995; Farrington et al. 2001; Murray et al. 2007).

These studies suggest that genetic causation may underlie crime and delinquency found among some families. However, determining general heritability of intergenerational delinquency is highly problematic in empirical research. As Rowe and Osgood (1984) note, delinquency is likely a result of a complex set of genetic combinations, while gene-environment interactions are also important mediating mechanisms that remain poorly understood (Moffitt 2005). Because estimates of heritability among sibling and family-pairs are commonly estimated as proportions of total variance observed in samples, this methodology has been criticized as providing unreliable measures of heritability due to limitations of constructing heritability estimates in twin and sibling samples (Goldberger 1978, 1979). This may be illustrated due to the way heritability and environment are also mathematically derived by the general relationship:

$$\text{(sibling-type's shared genetic variance)} * (\text{heritability})^2 + (\text{environment})^2 = (\text{observed correlation})^2,$$

where sibling-type is MZ twin, DZ twin, full-sibling, half-sibling, etc. and the observed correlation is the respective correlation for a given sibling-type. Heritability and environment are assumed to explain between 0 and 100% of the total variance, with $0 \leq$

$(\text{heritability})^2 + (\text{environment})^2 \leq 1$. As Arthur Goldberger (1978) also notes, classic behavioral genetics models estimating heritability assume four conditions: a) no correlation of genes and environment, b) random mating, c) additive genetic effects, and d) identical similarity of environments across sibling types. Generating models with these four assumptions raises validity issues for heredity and environmental estimates. Assuming random mating and additive genetic effects, Goldberger also demonstrated that heritability and environmental estimates are largely functions of observed correlations, leading to analytical bounds that do not represent random error. The positions demonstrated by Goldberger, along with the general rarity of twin samples, have limited application of twin studies in many social science disciplines.

To deal with these criticisms of traditional behavioral genetics models, recent research on genetics and human behavior has turned to analyzing specific genotypes in human behavior through molecular genetics (Guo 2006). Molecular genetic studies of delinquency remain rare in humans, resulting, in part, from a lack of datasets containing genetic markers and a skepticism among criminologists and sociologists about traditional methods for estimating genetic influence (Beaver 2006). While testing for association of specific genotypes with deviance does not estimate total heritability, this methodology allows for testing specific genotypes associated with crime and the possibility of gene-environment interactions. Molecular genetics studies of crime/delinquency remain rare in human populations.

Capsi et al. (2002) analyzed a sample of New Zealand men, finding a relationship between violence and low-promoter activity (2, 3.5, and 4.5 repeat polymorphisms) in the monoamine oxidase A gene (MAOA). Guo, Roettger and Shih (2007) utilized multilevel models from a sample containing DNA markers in the National Longitudinal Study of Adolescent Health (Add Health) to empirically link delinquency and violence scale scores with dopamine transporter gene DAT1 and the dopamine receptor gene DRD2. Employing basic regression models for delinquent with Add Health data, Beaver (2006) explores associations with five genes (MAOA, DAT1, DRD2, DRD4, and 5HTT) across race and gender groups. Beaver's (2006) analysis found some evidence for direct effects of genes on environment, but gene-environment interactions were found to have much greater significance.[12]

Consistent with research from Caspi, et al. (2002), Haberstick et al. (2005), and Guang Guo and colleagues (Guo et al. 2007; Guo et. al 2008; Guo et al forthcoming), I will test for significance of the 10R/10R and 10R/9R genotypes of DAT1, the A1/A2 genotype for DRD2, and the 2R, 3R, and 5R genotypes of MAOA along with father's history of incarceration. Using genotypes and father's history of incarceration in predicting respondent's delinquency, effects may be observed through main effects. Statistical significance of main effects for genotypes and father's history of incarceration would suggest that both genotypes and father's history of incarceration are each significant predictors of a respondent's delinquency. In contrast, if significance of father's history of incarceration declines with inclusions of genotypes, this would suggest that an effect from father's history of incarceration may be explained by individual genotype. It should here be noted that thousand of single genes or combinations of genes may be responsible for intergenerational patterns of delinquency. This implies that

[12] It should be noted that Beaver's analysis reports over six hundred regression models across five dependent variables, raising the significant issues of type I errors (e.g., false positives) when he claims significant results at the $p<0.10$ and $p<0.05$ significance levels.

results for single molecular genetic markers should be interpreted as simply a genotype acting as a potential pathway for genetic transmission of genetic propensities for delinquency.

Father-Son Incarceration and Family Structure

From intergenerational studies of crime patterns within families and from studies of risk factors of delinquency, predictors of adult son's delinquency appear to be family structure, paternal involvement, and parenting styles. As discussed above, Robins (1966) found that father's arrest and antisocial diagnosis correlated highly with child's antisocial diagnosis. More recent work by David Farrington and colleagues has found that familial correlations in arrest and crime occur in wide variety of datasets over time and across nations, with trends showing strong father-son, mother-daughter, and sibling correlations (Farrington 1995; Farrington 2002; Farrington, Gundry, and West 1975; Farrington et al. 2001; Murray et al. 2007; Rowe and Farrington 1997). In related literature, the following factors may all play a role in creating an intergenerational father-son incarceration: father's absence, lack of emotional attachment, social labeling, and familial structures where parents are not present in the household.

Early researchers of delinquency, such as Glueck & Glueck (1950) and Robins (1966), examined general relationships between delinquency among adult men and familial variables from childhood, using basic statistical tables and correlation tables to identify trends. Robins (1966) identified relatively high degrees of correlation between parent-child rates of offending, along with risk factors of living in single-parent homes, residing with a large number of siblings, poverty, and a lack of parental discipline. In their classic study *Unraveling Juvenile Delinquency*, Glueck and Glueck (1950) identified abusive parenting relationships with delinquency in Boston youth, including attachment to parents, recognition and punishment of deviant acts, being raised in a single parent home, and residing in violent households.

Gottfredson and Hirschi (1990) have noted that family correlations in criminal behavior have been among the most robust findings in criminological research. Individuals such as Robins (1966) and West and Farrington (1977) have reported parent-child and sibling correlations in antisocial behavior and arrest. More recent work by Farrington and colleagues has extensively explored patterns of familial incarceration among national samples and datasets (Farrington 1995; Farrington et al. 1975; Farrington et al. 2001; Murray et al. 2007; Rowe and Farrington 1997). Analyzing data from the Pittsburg Youth Study, Farrington et al's (2001) study has found that incarceration of a father is a strong predictor of delinquency among adolescent males, mediated by variables indicating lack of awareness of behavioral problems, age of mother, family's socio-economic status, and parental supervision. In both the Pittsburg Youth Study and Cambridge Study in Delinquent Development, father's incarceration has been found to be a robust predictor of son's delinquency when controlling for familial outcomes (Farrington et al. 2001; Murray and Farrington 2005). Similarly, Thornberry and colleagues (Thornberry 2005; Thornberry et al. 2003) have used data from the Rochester Youth Development Study to report that father's incarceration is highly predictive of son's delinquent behavior, with paternal involvement and closeness as important mechanisms in transmitting delinquent acts.

The father-son correlations in literature suggest that familial processes play a large role in generating incarceration, but the effects of incarceration may result from issues associated with basic family structure. Currently, research has begun to examine the collateral consequences of the unprecedented expansion of the criminal justice system in the U.S. Given that average incarceration in state prisons is approximately 60 months (Bonczar 2003b) and that nearly one half of incarcerated inmates are parents (Mumola 2000), incarceration removes young fathers from interaction with children for extended periods. Recent research on family structure and single-parent relationships has brought increased focus on the social role of the father in improving outcomes of children (Ginther and Pollak 2004; Lamb and Tamis-Lemonda 2003; McLanahan and Sandefur 1994; Mincy 2006). Incarceration of a father is known to break apart marriages and increase separation between parents and children (Harper and McLanahan 2002; Johnson and Waldfogel 2004; Western et al. 2004). Ex-felons also face required alimony payments that, when paired with decreased net earnings, create legal barriers for visitation rights and father-child involvement (Edelman et al. 2006; Holzer, Offner, and Sorenson 2005). Studies of criminal behavior among adolescents link single-parent families, father-child attachment, low social control (e.g., parenting) and abusive father-child relationships with antisocial and delinquent behaviors in children (Harper and McLanahan 2002; Jaffee et al. 2003; Johnson and Waldfogel 2004; Sampson and Laub 1993). It should be noted that a number of factors may mediate father's history of incarceration, including father's absence, emotional detachment, and lack of involvement are associated with factors such as dropping out of school, 'being idle' (not working and out of school), and living in poverty. (Harris and Ryan 2003; King, Harris, and Heard 2004).

The research above suggests that familial structures and processes should be considered in research. However, even though spending one year or more in jail or prison has become a life-course event (Bonczar 2003b; Pettit and Western 2004b), empirical research on long-term consequences of father's incarceration is notably lacking in the context of the U.S.'s rapidly expanding prison population (Johnson and Waldfogel 2004).

Thus, by examining familial factors with father-child incarceration and arrest patterns in a nationally-representative sample, it is possible to gain insights on how delinquency and arrest in young adults relate to family structure, paternal relationships, and intergenerational patterns of crime/arrest. To capture the effect of paternal relationships, I will use an index measuring father's involvement and closeness to the respondent, receipt of financial support, and Wave I family structure [in the form of two-parent biological, remarried, single parent, and other family structures]. Testing characteristics of the respondent's biological father (e.g., a father's attachment index measuring involvement and reported closeness to child, along with alimony payment) and Wave I reported family structure serve as proxies for father's absence. To test the role of family structure and processes, I will include variables for adolescent familial structure in Wave I, family receipt of food stamps, parental strictness, and family structure. These variables will help to identify correlations that may help to contextualize how a father's incarceration may explain son's adult delinquency and arrest.

Neighborhood Control Variables

The link between neighborhood factors and crime has had a long history of research in social science research. Du Bois (1998 [1898]) found that disproportional arrest and incarceration among blacks occurred among Southern migrants living in impoverished areas of Philadelphia's seventh ward. Similarly, by investigating the plight of blacks centralized in Chicago's "Bronzville" during the 1930's, Drake and Cayton (1993 [1945], pp. 200-210) observed neighborhood associations between high concentrations of male juvenile delinquents, illegitimate births, high rates of disease and poor public health, and percentage of families on public relief. In studies of deviance, Glueck and Glueck (1950) and Robins (1966) noted that neighborhood poverty was found to substantially correlate with delinquent activities. The correlation between neighborhood deprivation and spatial relative inequality has been extensively documented in criminological research (Blau and Blau 1982; Land, McCall, and Cohen 1990a; Morenoff, Sampson, and Raudenbush 2001; Sampson et al. 2005).

In recent decades, one of the more controversial and debated of causes of crime has been Robert Sampson and John Laub's (1997) proposal that individual, familial, and community factors have additive effects on deviant behavior through the process of "cumulative disadvantage." Factors such as familial attachment, school quality, and neighborhood characteristics additively insulate or expose individuals to generating criminal propensities and committing criminal acts. Sampson, Morenoff, and Raudenbush (2005) have recently demonstrated that racial disparities in violent crime rates decline substantially when residential segregation and neighborhood socioeconomic status are taken into account with individual and family-level variables in a sample of Chicago neighborhoods. Based on Sampson and Laub's theory of cumulative disadvantage, father's incarceration would mediate community and individual demographic factors listed above to generate an individual's propensity to commit crime.

If father's incarceration is a measure of intergenerational poverty or disadvantage, then intergenerational father-son links may spuriously capture disadvantage in neighborhoods. Finding a nearly complete absence of upward mobility for both aspiring and non-aspiring groups of inner-city teenagers, MacLeod (1995a) argues that social and community variables create nearly impermeable barriers towards economic or social success with general society. Individuals in these environments lack opportunities to earn decent wages and escape poverty, generating antisocial behaviors that become criminal behavior. As similar work by Wilson (1987; 1996b) and Anderson (1990a, 1999) on inescapable poverty and disadvantage that pervades inner city ghettoes and minority communities suggests, criminal behavior is assumed to be a function of widespread and inescapable social and cultural structures. Hannon (2003) labels this position as "disadvantage saturation," wherein social variables exclusively determine the social outcomes of individuals.

Analyzing data on father-son arrest and incarceration from the Cambridge Study of Delinquency Development, Hagan and Palloni (1990) conclude that social labeling of fathers and sons as "criminal" results in reproduction of a criminal class across generations. From a structural standpoint, Thornberry (2005) similarly proposes that father's unemployment and poverty are negatively associated with intergenerational patterns of crime and incarceration observed between father and son. Farrington et al. (2001) suggest that increases in father-son correlations observed in their U.S. sample may be the extensiveness of deprivation within inner city areas. If true, such studies would imply that "class-like" behaviors underlie father-son incarceration, with presence in poor inner cities explaining father-son links in crime. Class-like structures are beyond the scope of this study, but they would imply a strength of neighborhood and familial variables explaining most of father-son correlations in crime and arrest.

Land, McCall, and Cohen's (1990a) landmark study of neighborhood effects and violence suggests that community deprivation, population structure, and concentration effects provide three primary dimensions for capturing exogenous neighborhood effects. This methodology has been widely utilized in research (e.g., Rosenfeld et al. 2001; Sampson et al. 2005). While factorial indices may be constructed to capture issues of deprivation and concentration (e.g., 2006), I adopt Sampson et al.'s (2005) usage of residential segregation and population density, along with a similar measure of professional educational attainment at the census tract level. This helps to minimize potential endogeneity bias which may occur in correlated neighborhood measures such as the number of two-parent families and percentage of households headed by single mothers.

By testing if the effect of a father's history of incarceration is reduced when neighborhood effects are introduced, it is possible to see if father's history of incarceration is mediated by neighborhood influences. Significance of neighborhood variables and a father-child relationship in increasing behavior are more consistent with theories of cumulative advantage, while insignificance of father-son incarceration when community level variables are present would suggest evidence that external factors such as saturation disadvantage might provide a more parsimonious explanation. For class-like behaviors, familial and community variables might come into play, but economic and structural variables would be more consistent with theories discussed above.

Individual Demographics and Crime

Individual variables also play an important role in predicting deviant behavior and arrest. Basic demographic variables associated with delinquency and arrest include racial classification (Bonczar 2003), age (Hirschi and Gottfredson 1983), lack of educational training or classification as a low-skilled worker (Edelman et al. 2006; Pettit and Western 2004b), low social attachment to education (Nagin et al. 2003; Pagani et al. 2001), delinquent peer networks (Haynie 2001, 2003; Haynie et al. 2006), and a history of alcohol or substance abuse (Dawkins 1997; Ford 2005). Conversely, general research has found that marriage, military service, and stable work have been associated with decreased delinquency (Sampson and Laub 2003b; Sampson and Laub 1993; Sampson, Laub, and Wimer 2006; Uggen 2000). These individual effects exert a large influence on individual propensities to engage in criminal behavior.

To account for individual-level effects, I include basic controls for age, dropping out of high school, grade retention, and history of substance and/or alcohol abuse. To minimize endogeneity issues for grade retention and substance abuse with other predictors of adult delinquency/arrest, these measures are defined as occurring during Wave I (five years prior to interview at Wave III). Measures of dropping out of high school, romantic relationships, military service and job tenure are incorporated as contemporary influences on delinquent acts.

The importance of peer networks in delinquency has been well documented in existing research, but analysis of peer effects with Add Health data in young adulthood is problematic. Haynie (2001, 2003) used Add Health data to measure how adolescent delinquency was impacted by peer respondent behaviors such as smoking and painting graffiti. Haynie's analysis is insightful, but this methodology for measuring peer influence does not capture more serious delinquent behavior; furthermore, it focuses on pooled samples of male and female respondents to fully exploit friendship data. Delinquency in peer networks has been hypothesized to both help or harm individuals in later life, based on class status and type of delinquent activity (Hagan and Foster 2003; McCarthy and Hagan 2001). Given that friendship data in Add Health is limited to measuring minor delinquency, while delinquent acts in adolescence may have an ambiguous impact on delinquency, I exclude measurements of peer networks from analysis.

Data and Methods

As stated in the above section, the goals of this paper are (a) to test if significance of biological father as a predictor of son's deviance and arrest may be explained by molecular genetic, community, familial, and individual variables and (b) to provide a contextual framework for using father's incarceration as a risk factor in adult deviance and arrests. To accomplish these goals, I make use of sibling and nationally representative samples of respondents in Add Health for analysis using: (i)

HLM/multilevel models for delinquency using twins samples to estimate family, community, and individual-level effects with molecular genetics markers and (ii) evaluation of a nationally-representative sample with community, familial, and individual-level models in predicting adult delinquency and arrest through usage of, respectively, ordered logistic and logistic regression models. The use of the twins samples allows testing of molecular genetics markers along with individual, family, and community variables to test comparative influence of father's incarceration. The limitations of the twin data in non-random selection and relatively small sample size prevent generalization of results. Usage of a representative population of adult men from the general Add Health sample helps address non-random selection and small sample size in the twins sample, while also serving as an additional check of individual, family, and community models. The data and methods employed for these sets of analysis are outlined below.

Data

For this paper, I utilize data from the National Longitudinal Study of Adolescent Health (Add Health) to test if an incarcerated father increases propensity to engage in criminal behavior and incur arrest in later life. Add Health is currently a three wave sample. Initially, Wave I interviews consisted of approximately 90,000, 7^{th}-12^{th} grade adolescents in-school interviews and a subsequent, in-home interview of approximately 20,000 students and separate interviews for parents. Two follow-up studies of the in-home sample were conducted a year later in the second wave of data collection and five years later during the third wave of data collection. During the second wave of Add

Health, approximately 15,000 interviews were conducted with respondents from Wave I enrolled in grades 8-12. This sub-sample removed older individuals from the longitudinal sample, creating a cohort with five years of age variance in the process. During the third wave of Add Health, approximately 15,000 individuals were interviewed. Respondents ranged predominately between ages 18 and 27 and were interviewed on all occasions when found by the funding agency. Out of 20,000 interviews, the number of non-missing interviews for three waves consists of 11,600 individuals used to form the weighted longitudinal sample (Harris et al. 2003).

Add Health, as a data set with sections devoted to deviant behavior in both adolescence and young adulthood, provides the opportunity for measuring propensities for arrest and engaging in criminal delinquency in addition to containing data on father-child incarceration. Its measures of health, family variables, and community-level measures also make it a unique resource for analyzing individual, family, and community level predictors of delinquency. Additionally, Add Health contains both a genetic sub-sample that allows for estimates of heritability and environmental influences and a nationally-representative sample of respondents to test familial and structural variables generalized at the level of U.S. society (Harris et al. 2003). Below, I describe the characteristics of both the twins and national data samples.

Twins Data

The Add Health genetic sub-sample consists of 3,129 twins and full-siblings during Wave IIII interviews. Of these, approximately 1175 males with non-missing data were found to be usable for analysis. The twin and sibling pairs represent a national sample, with completed questionnaires identical to those utilized for the full sample.

Although a national sample, small sample size, and lack of non-zero sample weights create increased risk for type I and type II errors. Issues of sample stratification and non-random sampling are also present. Thus, the sibling sample provides a mechanism for determining the extent to which molecular genetic markers and basic environmental variables play a role in father-child delinquency correlations. Unfortunately, due to sample size, generalization of father's incarceration with Wave III biological child delinquency and arrest are uncertain. Table 4.11 provides descriptive statistics for the genetic sample.

National Sample

The Add Health household sub-sample consists of 15,000 respondents in the 7th-12th grade sample during Wave I who possess complete data for a set of self-reported criminal behaviors for waves one and three. Of these individuals, 7,050 males ages 18-27 were present at time of the Wave III interview.[13] The available community and family variables present during period of first interview allows for a detailed assessment of how early family and community variables may explain links in father-child delinquency and incarceration. The data structure of this design creates a sample of individuals ages 18-27 at time of interview. However, because of significant decline in the sample size of individuals over age 24 and a decline in mean probability of ever being arrested after age 23,[14] cases older than age 24 were removed from the dataset. This action reduces sample

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[13] As Hagan and Foster (2003) document, the Add Health sample is a multi-stage clustered design that implies individual cases are neither randomly selected nor independent. Using STATA 9's survey commands, regional and school clustering are combined with individual probability weights to generate a representative population of males ages 18-23 for the U.S. STATA 9's 'survey' regression commands are used in data analysis to derive regression results for this population.

[14] Mean probabilities of ever being by arrested for ages 18-24 are, respectively, .123, .113, .166, .141, .169, .161, and .141. The probability of ever being arrested for the sample population should increase

bias, but selection factors downwardly biasing estimates of arrests and criminal behavior still remain.[15] The sample population also decreased to 6,552 males, or 6,182 males with nonzero weights in the representative survey sample. The resulting sample is evenly distributed by age, representing a male cohort ages 18-24 at time of the Wave III interview. Means and sample deviations may be viewed in Table 4.2.

Methods

For this paper, I wish answer the following question: "How do community, familial individual, and molecular genetic constructs explain correlations between father's incarceration and biological child's delinquency/arrest?" The combined usage of the sibling sample and full samples of Add Health allows for tests of significance and magnitude of effects among twins and young adult men. The Add Health sample is unique in containing self-reported delinquency for both a sibling and nationally-representative sample. I attempt to make optimal use of Add Health data by testing if intergenerational patterns may result from spurious clustering in the twin sample, while the nationally-representative sample allows testing of significance in social and familial structures in father-child delinquency and contact with the criminal justice system. For comprehensive analysis, I adopt separate strategies for analysis of the sibling and national

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monotonically by age, as does the cumulative probability of incarceration for the general population (Bonczar 2003).

[15] Chantala, et al (2004) estimate that selling drugs, carrying a weapon and shooting or stabbing someone are underrepresented by ~5% in the Wave III data relative to the Wave I population. In general violence measures and criminal activities are, respectively, underestimated in the Wave III sample by an average of 2.5% and 1%. Approximately one-hundred individuals in Wave III were not interviewed due to incarceration. Sample attrition due to non-response is disproportionately likely for chronic offenders and those incarcerated as adults, causing an age truncation of this population in the sample. Our models of arrest and criminal behavior are hence downwardly biased in the sample for independent variables in the regression analysis that positively correlate with criminal behavior and arrest.

sub-samples of the Add Health dataset. I then proceed to discuss analytic strategies for each sample.

It is important to note that measures of delinquency and arrest rely on self-reports of respondents. Official measures are traditionally associated with underestimation of delinquency and crime (Harris et al. 2003; Hood and Sparks 1970; Murphy, Shirly, and Witmer 1946; Robison 1936; Thornberry and Krohn 2000) because official measures often reflect not only the behavior of offenders, but also by external factors such as policing, prosecution, and the judicial system. For these reasons, many criminologists have turned to self-reports since the late 1970's (Hindelang 1981; Hindelang, Hirschi, and Weis 1979; Thornberry and Krohn 2000). Self-reports are a common method for sampling delinquency (Thornberry and Krohn 2000). However, while generally considered reliable, racial differences in self-reports of delinquency and arrest have been observed in studies such as the 1979 National Longitudinal Survey of Youth (Freeman 2000a). To increase validity and reliability of findings, Add Health interviews of respondents were conducted using computer-assisted self-interview (CASI) technology/methods.

Measuring Respondent Delinquency & Arrest

Respondent Delinquency. The questions used to measure delinquent activities are given in 31 and capture activities potentially leading to sanctions of arrest and/or incarceration. Because of separate modeling strategies and conventions incorporated into analysis of the twin and nationally-representative datasets, I adopt separate measures of delinquency. To determine correlates leading to a wide variety of exhibited criminal behavior, I adopt a delinquency scale based on scales for Add Health data on delinquency utilized by Haynie (2001, 2003) and Hagan and Foster (2003). These scales are a variation of a more-widely used set of 13 questions tested and used in contemporary research on criminal behavior (Farrington et al. 1996; Hannon 2003) and used in the 1979 National Longitudinal Survey of Youth.

For analysis of the twin's sample, I adopt scales used by Guo et al. (2007) for analyzing the genetic sample in Add Health using a set of 12 items as a measure of serious delinquency along with a subset of eight items as a measure of violent delinquency. By adopting Guo et al.'s scales and methods, my analysis uses genetic models that have been empirically demonstrated to *not* result from natural stratification within the sample population. For serious delinquency, these items include stealing amounts larger or smaller than $50, breaking and entering a home, selling drugs, serious physical fighting that resulted in injuries needing medical treatment, use of weapons to get something from someone, involvement of physical fighting between groups, shooting or stabbing someone, deliberately damaging property, and pulling a knife or gun on someone. For violent delinquency, the scale excluded stealing, breaking and entering a home, and selling drugs. A listing of the questions utilized for this analysis may be viewed in Appendix 3.

For serious delinquency, Crombach's alpha CAS score for Waves I-III were, respectively, 0.81, 0.79, and 0.73. For violent delinquency, the Crombach alpha correlations for Waves I-III were 0.75, 0.74, and 0.66. These alpha-values compare to criminal behavioral reliabilities for scales utilized by Hagan and Foster (2003), Haynie (2003), and Hannon (2003) in analysis of NYSL79 and Add Health data. While Hagan and Foster (2003) use a scale with a Cronbach's alpha of α=0.86, their scale includes minor vandalism and lying to parents/guardian, acts more typically viewed as part of common adolescent deviance. Both Hagan and Foster and Haynie utilized violence scales with Cronbach's alpha of α =0.64. By limiting measures to felony or misdemeanor-type behaviors, Guo et al.'s scale focused on criminal behavior potentially leading to arrest and conviction within the criminal justice system. By adopting this methodology, I hope to capture criminal behavior "at risk" for adult arrest and conviction in the developmental trajectories of the twins genetic sample. Individuals with two or more missing responses were excluded from analysis to increase data reliability.[16]

For the nationally-representative sample, use of survey weights in STATA helps to create a random and representative population sample of individuals included in both Wave I and Wave III interviews. To capture a broader range of delinquent acts, I expand the list of items from Guo et al.'s serious delinquency scale to include fifteen items by including whether or not an individual has held stolen property, used someone else's credit or debit card without permission or knowledge, or deliberately written a bad check. A listing of the items used in this scale may be viewed in Appendix 3.

When raw frequency of events is available for analysis, count-based regression models are commonly used to model the dependent variable (Haynie 2001; Long 1997; Matsueda, Kreager, and Huizinga 2006); however, self-reported delinquency items in Add Health are collapsed frequency counts, with right censoring for occurrences either

[16] The scores of individuals with missing responses were also proportionally rescaled to fit a 12-item metric. Individuals with more than two missing responses (those who either gave no-response or refused to answer the given question) were dropped from the sample. These procedures, though more stringent than rescaling methods used by Hagan and Foster (2003), attempted to minimize cases with missing data while reducing bias due to non-response. Comparing mean deviance between rescaled and non-adjusted scale scores, the mean rescaled deviance score is 1.407 while the mean non-adjusted scale score was 1.395 or a difference of 0.8%.

occurring more than one time (pulling a knife or gun on someone, shooting or stabbing someone) or more than five times (other thirteen items). To address the lack of frequency counts, I use ordered logistic regression methods to more conservatively represent frequency and occurrence of deviant behavior. To facilitate this analysis, I adopt a classification scheme where violent acts are classified more seriously than non-violent acts. The occurrence of both violent and non-violent activity was rated as a more serious category of offending, but reports of high overall rates of delinquency were classified as most serious.[17] The classification for Wave III delinquency is numerically designated given as follows:

'0' for respondents reporting no delinquency in the fifteen items

'1' for respondents reporting one or more non-violent acts only, low incidence

'2' for respondents reporting one or more violent acts only, low incidence

'3' for respondents reporting one to three non-violent acts *and* one to three violent acts

'4' for respondents reporting high incidence and/or non-violent acts.

Respondent Arrest. In addition to self-reported delinquency, an important measure of intergenerational crime and incarceration is contact with the U.S. criminal justice system. Unlike delinquency, arrest is a discrete event. Using logistic regression, I look at how behaviors in adolescence may explain arrest incurred in adulthood. By using data from self-reports, it is possible to look at behaviors beyond a 12-month time frame

[17] Here, high incidence of delinquency was categorized when (1) a respondent reported three or more violent and non-violent acts or (2) a deviance score of nine or more when a score of nine or more was found using a scale similar to Guo et al. (2007) for fifteen Wave III items.

and to examine how adolescent variables may explain entrance into the criminal justice system.

Sibling Data

For analysis of sibling data in Add Health, I adopt the methodology developed and utilized by Guang Guo and colleagues (Guo and Wang 2001; Guo and Tong 2006; Guo et. al 2007; Guo et. al forthcoming), with random error components for individuals, sibling type, and interview wave. These models are extensions of basic multilevel models that treat kinship type among monozygotic (MZ), dizygotic (DZ), and full-siblings as unique random error components contributing to delinquent activities (Guo and Wang 2001). The modeling strategy is adapted from traditional mixed/multilevel models (Raudenbush and Bryk 2002; Searle 1971). Using panel data with multiple observations (e.g., “clustering”) occurring both at the individual and sibling-type, this statistical model may be written as a three-level multilevel model denoted as:

$$\text{Delinquency}_{tij(s)} = \text{л}_{0ij(s)} + \text{л}_{1ij} * \text{Age}_{tij} + \text{л}_{2ij} * (\text{Age}_{tij})^2 + \Sigma(\text{л}_{kij} * X_{kij}) + e_{tij(s),}$$

where:

$\text{Delinquency}_{tij(s)}$ denotes observed delinquency for the individual i, at time t, in sibling-pair j. (s) denotes the type of sibling-relationship classified as MZ twins, DZ twins, or full-siblings.

$\text{л}_{0ij(s)}$ is the mean delinquency of individual ii in sibling-pair j.

л_{1ij} and л_{2ij} are coefficients, respectively, for the linear and quadratic age values at time t for respondent i in sibling type s.

$\Sigma($ л$_{kis} * X_{kis})$ represents the sum of a set of individual-level predictors $X_{kij,}$ where k represents k=3,…,m, and corresponding regression coefficient л$_{kis}$ that occur at the individual-level. Individual level predictors include genotype, father's incarceration, and control variables.

$e_{tij(s)}$ is the random disturbance term for deviance from respondent tij(s)'s deviation from the overall mean score. The disturbance term is normally distributed with mean zero.

At the second level, the model specification is given as:

л$_{0is}=\beta_{00j(s)} + r_{0ij(s)}$

л$_{1ij}= \beta_{10j(s)}$

л$_{2ij}= \beta_{20j(s)}$

and for all other coefficients:

л$_{Lis}= \beta_{L0s}$, where L=1,…,m

where:

$\beta_{00j(s)}$ represents the mean individual delinquency within sibling-pair j

r_{0ij} represents a random effect for individual i score from the mean predicted by each sibling-pair j.

л$_{Lis}= \beta_{L0j}$, where L=2,…,m, represents the general level-2 (e.g., individual-level) coefficient for the age, age², and the predictor variables $\Sigma(\beta_{k0j} * X_{kis})$, where k=3,..,m . Here, the level one and level-two coefficients are identical due the assumption that age and control variables are time-varying covariates at level-one and fixed effects at levels 2 and 3 (e.g., age and controls vary by time, but are fixed effects at the individual level and sibling-type level).

At the third-level, the model is given as follows:

β_{00s}=γ_{o00} + $u_{00(s)}$= + $u_{00(MZ\ twins)}$ + $u_{00(DZ\ twins)}$ + $u_{00(full\text{-}siblings)}$

and for all other coefficients:

β_{L00} = γ_{L00}, where L=1,…,m

where:

γ_{00s} represents the grand mean delinquency score for the sample,

$u_{00j(s)}$ represents a random effect for sibling j given for each sibling type—MZ twins, DZ twins, and full-siblings. Hence, random effects are calculated for three sibling types.

γ_{lis}= β_{L0j}, where L=1,…,m, represents the general level-3 (e.g., sibling-level) coefficient for the age and control variables in the model. Here, level-two and level-three coefficients are identical due the assumption that age, age², and the predictor variables $\Sigma(\beta_{k0j} * X_{kis})$, where k=3,..,m, are time-varying covariates at level-one and fixed effects at levels 2 and 3 (e.g., age and controls vary by time, but are fixed effects at the individual level and sibling-type level).

Combining time, individual, and sibling-pair levels, I obtain the following conditional, three-level model:

$$Delinquency_{tij(s)} = \pi_{0ij(s)} + \pi_{1ij(s)}*Age_{tij} + \pi_{3ij}*(Age_{tij})^2 + \Sigma(\pi_{kij} * X_{kij}) + e_{tij(s)}$$

$$= \beta_{00j(s)} + r_{0ij} + \beta_{10j}{*}Age_{tij(s)} + \beta_{20j}{*}(Age_{tij})^2 + \Sigma(\beta_{k0j(s)} * X_{kij}) + e_{tij(s)}$$

$$= (\gamma_{000} + u_{00j(s)}) + \gamma_{10j}{*}Age_{tij} + \gamma_{20j}{*}(Age_{tij})^2 + \Sigma(\gamma_{30j} * X_{kij}) + r_{0ij} + e_{tij(s)}$$

$$= \gamma_{000} + \gamma_{10j(s)}{*}Age_{tij(s)} + \gamma_{20j(s)}{*}(Age_{tij})^2 + \Sigma(\gamma_{k0j} * X_{kij}) + u_{00j(s=\text{MZ twins})} + u_{00j(s=\text{DZ twins})} + u_{00j(s=\text{full-siblings})} + r_{0ij} + e_{tij(s)}$$

where the error component is given as

$$u_{00j(s=\text{MZ twins})} + u_{00j(s=\text{DZ twins})} + u_{00j(s=\text{full-siblings})} + r_{0ij} + e_{tij(s)}$$

These error components contain a random disturbance term, $e_{tij(s),}$ and four random effects. These four random effects contain four normally-distributed random intercepts with residuals, one at the individual-level and three at the sibling-level for MZ twins, DZ twins, and full-siblings. The random-intercept terms have been found to significantly vary by sibling type (Guo and Wang 2001; Guo et al. 2007; Guo et. al 2008a Guo et. al 2008b). Given the individual and sibling-type random effects, the dependent variable is assumed to be normally distributed, with independent response categories. These assumptions are consistent with the conditional three-level model outlined in Raudenbush and Bryk (2002) and employed by Guo et al. (2007). As previously noted, I also assume that age and control variables are time-varying covariates that have fixed effects at the individual and sibling-type levels in the model.

As discussed above, traditional twin studies have been met with heavy criticism, most notably by Goldberger (1978, 1979). Goldberger's basic critiques of twins studies concern assumptions of random mating, identical dispersion of genes within twins, additive effects of genes, and the range of outcomes associated with how heritability is calculated as a function of variance.[18] Random effects models such as employed by Guo and Wang (2001) utilize random intercepts for individuals and sibling types, creating error components that adjust for complex problems such as differential treatment by sibling type and measurement of gene/environmental components, but still estimate results based on total heritability and environment as a function of variance. The methodology employed by Guo et al. (2007) and outlined above assumes differences by individual and sibling type, while testing how individual genotypes may influence behavior. This eliminates the possibility of estimating total heritability, but main genetic effects and interactions allow for testing if intergenerational patterns of father-son deviance may be explained as transmission of genetic propensities for criminal behavior.

National Sample

Unlike the sibling sample, Add Health's longitudinal sample lacks genetic markers that might allow for estimates of heritability of father's incarceration on adult child's delinquency and arrest. The national sample, however, contains a rich set of measures that allow for tests of community and familial variables in determining arrest. As discussed above, the national sample also contains a much larger set of individuals

[18] Goldberger's (1978) paper specifies the standard mathematical model utilized in calculating heritability and environmental factors associated with outcomes among twins. Goldberger also estimates potential ranges in variance for environmental effects, given calculations on heritability among twins. The author's conclusion is that social science should not rely on twin studies to inform research and social policy, citing examples of eugenicists in psychology such as Richard Herrnstein and Arthur Jenson. Citing Goldberger as an example, Freeze and Powell (2003) argue widespread misunderstanding and fear of such labeling has prevented interaction between sociology and genetics. Given that psychologists argue for some genetic basis of delinquency and violence (Caspi et al 2002; Moffitt 2005), my goal in estimating genetic effects is to understand the range of potential influence of heredity and environment in explaining the biological father-child empirical link. Given that a number of genes likely influence deviance (Rowe and Osgood 1984) and that deviance results from complex sets of gene-environment interactions (Moffitt 2005), I do not believe it is possible to aggregately estimate the role genetics plays in intergenerational crime/delinquency. With potentially hundreds of genes as candidates, this is beyond existing research.

arrested as an adult and with an incarcerated father than the sibling sample. By analyzing the national sample, it is hence possible to (1) test how community variables (neighborhood employment, poverty, segregation, etc.) and familial variables (parental attachment, biological father's absence, etc.) in adolescence may explain the link in father-son arrest patterns and (2) use population weights to determine the likelihood of respondent's adult arrest given community and familial variables.

The use of population weights allows for a general comparison of how community and familial variables may influence arrest when father's incarceration is included as a predictor. Given the rapidly increasing incarcerated population in the U.S., empirical analysis of how a father's incarceration may influence son's arrest within the context of adolescent family and community structures contributes to understanding what factors place individuals at risk for entering the criminal justice system. Thus, I will utilize Wave III delinquency scores to examine how father's incarceration may be mediated by individual, familial, and community-level effects traditionally associated with deviant activity.

The arrest of individuals has the benefit of being a much easier measure to categorize and measure as a discrete event. As Freeman (2000) notes, some researchers consider self-reported criminal behavior less accurate than arrest and conviction in measuring criminal behavior. For the 1979 National Longitudinal Survey of Youth (NLSY79), Freeman notes that blacks underreport criminal behavior in self-reports, while whites tend to more closely self-report actual levels of criminal behavior. In general, Freeman argues that self-reports do not appear substantially more biased than other forms of measurement of criminal behavior. In previous analysis looking at both arrest and self-reported delinquency, significance of effects of father's incarceration and other controls were found to be largely similar (Roettger 2006). To model arrest, I hence utilize binary logistic regression in analysis.

Since adult arrest is coded for any arrest above age eighteen, I also eliminate concurrent, time-dependent variables from the previous regression that may alter criminal behavior, such as adult relationship status and work history. These variables also violate assumptions of time-order (if marriage at age 19, for instance, deters future arrest of an individual previously arrested at age 18). This eliminates potential time-bias in the logistic regression (Long 1997), but removes potential explanations of concurrent explanations for crime. I limit analysis to variables from adolescence, testing if Wave I individual or family variables predict adult arrest. With these variables, I test to see if father's incarceration is linked to increased probability of adult arrest.

Results

Twins Sample

Analysis of the genetic sample contains basic genotype, individual, family, and community estimates for serious and violent delinquency. The fixed effects estimates for these models are presented in Table 4.3. While not presented here, all estimated random effects slopes and intercepts between sibling pairs and repeated measures were found to be highly significant across the estimated models and to replicate general patterns reported by Guo et al.'s (2007) usage of sibling and repeated measures for the Add Health sample.

To facilitate interpretation, I discuss the results for serious and violent delinquency across the baseline demographic, individual, family, and community models. Following existing research, along with father's incarceration, main effects for the DAT1 dopamine transporter, the DRD2 dopamine receptor, and the Monoamine oxidase A (MAOA) genotypes in individuals are simultaneously presented. The significance of a genotype predicting in predicting individual delinquency is compared to a reference genotype. The reference genotypes are 9R/9R or 9R/other for DAT1, A1/A2 for DRD2, and 3.5R, 4R for MAOA.

In the baseline demographic model, father's incarceration, race, age, and genotypes are presented. For both violent and non-violent delinquency, father's incarceration positively correlates with delinquency at the $p<0.001$ level. Consistent with prior research (Hirashi and Gottfredson 1983, Gottfredson and Hirashi 1990; Haynie et al. 2006), a non-nonlinear relationship exists for age. The 2R genotype for MAOA and the 9R/10R genotype in DAT1 are associated with increases in delinquency at the $p<0.05$ level, while the A2/A2 genotype is associated with a significant decrease in delinquency at the $p<0.05$ level. These genetic effects are consistent with Guo et al.'s (2007) findings. Controlling for these genotypes, I thus find that father's incarceration remains a robust predictor of serious and violent delinquency in the dataset.

In the individual-level model, a likelihood ratio test with the additional variables yields a p-value of $p<0.00001$ for both violent and serious delinquency, suggesting that individual-level variables are significant additions to the baseline demographic model. For both violent and serious delinquency, the effect of father's incarceration declines by ~45% in magnitude but remains significant at the $p<0.05$ level. Drug use at Wave I, dropping out of high school, and cohabitation are associated are associated with large increases in both violent and serious delinquency, which are significant at the $p<0.001$ level. The index for Wave 1 school attachment is also associated with a significant decrease of serious delinquency ($p<0.001$) and violent delinquency ($p<0.01$), and may be interpreted as a respondent's delinquency declines and as satisfaction and involvement in education increases. Excluding the relatively rare 2R genetic sequence, results for MAOA, DRD2, and DAT1 become largely insignificant. These results suggest the importance of individual-level variables in predicting delinquent trajectories. Caution is encouraged to remain skeptical about causal inference for all genetic results, as random effects models focus on correlations between individuals and sibling types.

With the addition of family variables added to baseline demographic variables, likelihood ratio tests for the estimated family models tests yielded p-values of $p<0.004$ and $p<0.016$, respectively, for serious and violent delinquency. For both violent and serious delinquency, the effect of father's incarceration decreased by ~20% in magnitude relative to the baseline demographic model, with p-values significant at the $p<0.01$ level. This suggests that father's incarceration is a significant factor when family-level. Among family variables, father attachment, a scale measuring father's involvement and closeness to the respondent, was found to significantly decrease delinquency ($p<0.001$). Being removed from a home by social services was also associated with higher levels of delinquent activity ($p<0.05$). Measures of parental strictness, repeated incidence of child abuse, family structure, father's payment of alimony, and receipt of public assistance were not associated with statistically significant changes in delinquency. The 10/9R genotype for DAT1 and 2R genotype for MAOA were found to be significant predictors of increased delinquency ($p<0.05$), while the A2/A2 genotype for DRD2 was associated with decreased delinquency ($p<0.05$). These data suggest that genotypes and father's incarceration are significant predictors of delinquency when family variables are considered.

For both serious and violent delinquency, the set of Wave I community variables [measured at the census tract level] were found to have no significant correlation with deviance, while father's incarceration and genetic effects remain similar to those of the baseline demographic model. Models with one community variable and similar measures were not found to be significant predictors of delinquency. Weak community neighborhood effects are commonly observed in quantitative research (Guo and Zhao 2000) and are also documented in Haynie et al.'s (2006) analysis of neighborhoods. The school-based sample design and relatively small number of individuals in the sample may also contribute to lack of significance within the model.

The results of these regression models illustrate that father's incarceration is a robust predictor of young adolescent men along with statistically significant main genetic effects of three known genes. These results do not rule out the possibility that genetic effects explain intergenerational patterns of crime; instead, they suggest that, in the three genes analyzed, genetic factors exert independent effects on delinquency along with father's history of incarceration. Moreover, the statistical significance of father's incarceration on delinquency remained significant across all models, even when individual genotypes became marginally significant or non-significant. Thus, the importance of father's incarceration in explaining serious and violent delinquency held when individual, genetic, family, and community-level predictors were introduced.

Conversely, individual and family predictors were found to reduce overall significance by 50% and 20%, respectively, suggesting that individual-level and family predictors explain a relationship between father's incarceration and son's deviance. Significance of individual and family variables also suggests that individual characteristics and family relationships are associated with differing levels of offending among individuals. Factors such as father involvement, possession of A2/A2 genotype, and school attachment are associated with significant declines in deviance, while events such as adolescent substance abuse, possession of a 2R genotype, and cohabitation are associated with increases in deviance. The coexistence of these predictors with father's incarceration in the regression model suggest that additive factors are associated with increased and decreased levels of respondent delinquency. Thus, the results from the twin's model broadly support Sampson and Laub's (1997, 2003) model of cumulative disadvantage.

In assessing results, some cautions are in order. While variables with large correlations were not used in this model to minimize endogeneity, caution should be made about causal inference, as nonlinear-growth models focus on correlations between individuals and sibling types. The highly significant, nonlinear effects of age in predicting delinquency also emphasize that delinquency remains a developmental process that varies tremendously among individuals. Significance of father's incarceration and genetic effects along with cohabitation are predictors of delinquency, and are not causal in the sense of changing individual trajectories in offending. Finally, small sample size increases likelihood of type II errors and limits generalization of results. To address these last two issues, I present analysis from a nationally-representative sub-sample of Add Health.

National Sample

Adult Delinquency

While lacking data genetic data, the nationally-representative subsample of males allows for comparative testing of the influence of individual, family, and community factors in determining delinquency. Using the ordered logistic regression model outlined in the methods section above, I test the assertion that parental incarceration is associated with adult delinquency for young men. These results provide an additional means of measuring influence of individual, family, and community-level variables. A combined model of individual, family, and community effects also attempts to gauge comparative impact of factors. Results are presented in Table 4.4.

In the model with individual-level characteristics, father's incarceration is associated with a 61.7% ($p<0.001$) increase in delinquency, representing an 18% decline from the model estimating father's incarceration only. For other predictors in the individual model, respondent's military service ($p<0.05$), Wave I incidence of binge drinking ($p<0.001$), and substance abuse ($p<0.001$) were also found to be significant. Relative to single individuals, marriage was associated with a 49% decrease ($p<0.001$) in incarceration relative to single individuals. School attachment was also marginally statistically significant ($p<0.10$) and associated with a decline in offending rates. Consistent with existing research, each year of age was associated with an annual decline of 15% in deviance (*p<0.001*), though no significant nonlinear effects were found in models not presented here.

For the family-level models, father's incarceration was associated with a 75% overall increase in deviance relative to respondents with a father lacking a history of incarceration. For the family variables, familial measures include characteristics of the father's relationship (father attachment index and payment of alimony to mother at Wave I), Wave I family structures, parental strictness, reported abuse and removal from home by social services, and Wave I receipt of food stamps. Among these variables, living in a single household at Wave I was associated with a 26% (*p<0.05*) increase in deviance relative to individuals living with both biological parents. In addition, physical abuse by a parent or caregiver was associated with a 40% increase in deviance, while those removed from a home by social services were associated with a ~300% increase in deviance scores (*p<0.01*). No significant association was found for Wave I food stamp receipt, father's attachment or alimony payment, and a measure of parental strictness. This suggests that father's history of incarceration is not heavily mediated by a respondent's relationship to his biological father, low socioeconomic status, family structure, physical abuse by a parent or caregiver, or removal from home by social services.

For community variables, no significant relationship was found between adult son's delinquency/arrest and the following Wave I variables: the proportion of African Americans within a respondent's census tract, the proportion of households holding a college degree, and a log transformation of urban density. As discussed above, these variables reflect population structure, measures of residential segregation, and relative affluence or deprivation. Though not presented, a factor score similar to one used by Haynie et al. (2006) to measure of relative deprivation was also attempted, but was similarly non-significant. The 1990 community measures used to measure adult behavior, along with the general weak magnitude of community variables (Guo and Zhou 2000) may explain these general results. These findings generally contradict theory and evidence that neighborhood factors greatly influence individual criminal behavior.

In the final model, I use individual, family, and community variables to test their combined effects on father's history of incarceration as a predictor of adult deviance. In the combined model, respondents with an incarcerated father are 59% ($p<0.001$) more likely to have increased deviance scores relative to those without father's deviance. These results hold when controlling for 1) individual-level variables for substance and alcohol abuse, age, race, military service, school attachment and relationship status; 2) family-level variables for father attachment and alimony payments in childhood, receipt of food stamps (low socioeconomic status), family structure, and repeated child abuse by adult or caregiver; and 3) community variables including residential segregation, community deprivation, and population structure. In the combined model, age, substance abuse, repeated physical abuse, and marriage were also associated with significant changes in deviance scores. These results broadly align with prior empirical research, while suggesting father's history incarceration is found to be a highly significant and robust factor in predicting increased deviance among young men.

Adult Arrest

While delinquent activity measures propensity for criminal behavior, issues such as racial profiling, differential sentencing, criminal aptitude and transmission of disadvantage my substantially affect a link between father's history of incarceration on adult sons. Arrest and incarceration also remain discrete events that lead to establishing having a 'criminal record' that forms the basis for differential treatment in later life. Hence, testing if incarceration by a father increases probability of a son incurring an adult arrest provides additional means for observing how incarceration influences adult arrest. To avoid issues of endogeneity, I exclude variables that may have occurred after arrest, such as marriage, military service, or work. Thus, respondent's history of adult arrest allows for measurement of how Wave I variables at the individual, family, and community levels influence entrance into the adult criminal justice system. The results of these regression models are presented in Table 5.

Among Wave I individual-level predictors, father's incarceration was found to increase by 86% ($p<0.001$), a decline of approximately 12% relative to the baseline model. Among individual-level variables, substance abuse increased likelihood of incurring arrest by a factor of 2.54 ($p<0.001$), binge drinking increased risk of arrest by a factor of 60% ($p<0.001$), and dropping out of high school increased probability of arrest by 58% ($p<0.01$). Native Americans were marginally less likely to be arrested than whites, but blacks and Hispanics showed no difference in odds of arrest. No significant effects were observed for both school attachment and age. Given that approximately 100 individuals were not interviewed due to incarceration at time of the Wave III interview, selection bias may explain lack of significance for age and race.

For Wave I family variables, a respondent with a father having a history of incarceration was associated with an 89% ($p<0.001$) increase in incurring adult arrest. Among family variables, an increase in parental strictness score was associated with a 9% ($p<0.05$) decrease in likelihood of arrest, while a history of child abuse was associated with a 38% increase in incurring adult arrest. Adult arrest was not found to be statistically associated with Wave I low socioeconomic status (measured by receipt of food stamps), the respondent's relationship to his biological father, alimony payment, or being raised in a household without both biological parents, though living with a single father was found to be marginally significant.

Among community-level variables, father's incarceration was found to increase likelihood of incurring adult arrest by a factor of 2.15 ($p<0.001$) among respondents. Among community variables, no significant relationship existed between proportion of African Americans in a census tract, the proportion of individuals in census tract holding a college degree, and a log transformation of urban density. As in the ordered logistic regression models for deviance, I found no significance in models with computed factor scores used by Haynie et al. (2006) to measure relative deprivation.

Combining individual, family, and community variables with father's incarceration into a final model, father's history of incarceration increased a respondent's probability of incurring adult arrest by approximately 85% ($p<0.001$), a decline of approximately 13%. Hence, respondent's individual, family, and community variables were not associated with a major decline in overall deviance. In the combined model, individual level variables were found to be of primary statistical significance in increasing likelihood of incurring adult arrest. These factors include Wave I binge drinking ($p<0.001$), a history of Wave I substance abuse ($p<0.001$), and dropping out of high school ($p<0.001$). Among racial groups, Hispanics and Native Americans were associated with marginally significant risks of arrest relative to whites. Family structure, low socioeconomic status, respondent's relationship with father, and history of child abuse were non-significant. Community measures were also similarly found to remain non-significant in the combined model for predicting arrest.

Discussion

This paper has analyzed data containing molecular genetic, individual, familial, and community-level variables to explain possible correlations between father's history of incarceration and son's delinquency and arrest. In both a national twins sample of males ages 12-23 and a nationally-representative sample of adults ages 19-24, father's incarceration was found to be robust predictor of self-reported delinquent behavior in the presence of individual, family, and community variables. For the adult sample, father's history of incarceration increased deviance by 58% ($p<0.001$) and increased likelihood of incurring arrest by 85% ($p<0.001$) when controlling for individual, family, and community variables. These results broadly suggest that father's incarceration is both a robust and significant predictor of adult delinquency, controlling for biological, individual, family, and community-level variables.

In the baseline demographic models, father's history of incarceration was found to be an independent predictor when compared with genetics samples. These results align with prior research linking 2R genotype in MAOA, the 10R/9R and 10R/10R genotypes in DAT1, and the A1/A1 and A2/A2 genotypes in DRD2 as predictors of delinquency (Guo et al. 2007; Guo et al 2007). Along with father's history of incarceration, genetic effects also remain statistically significant co-predictor of serious and violent delinquency. These results suggest that father's incarceration remains an independent predictor along with these three genotypes. Additionally, the results are consistent with Rowe and Osgoode's (1984) postulate that many genotypes have an effect on delinquent behavior and that the three genotype predictors are independent of father's history of incarceration. The latter indicates that father's history of incarceration is a strong predictor of delinquency not explained by three biomarkers directly linked to deviance in humans.

Across both samples, individual-level variables showed the most explanation in reducing the effect of father's incarceration, decreasing the magnitude by ~50% in explaining deviance in the twins sample, which was a ~20% decline in deviance among young adults and a ~12% decline in adult arrest. In the twin sample, individual-level variables that were highly significant included dropping out of high school, school attachment, drug use, and cohabitation. Among the national sample, alcohol and substance abuse, marriage, and age proved most significant. For adult arrest, adolescent alcohol/substance abuse and dropping out of high school were highly significant individual-level predictors. Empirically, differences in findings may result from differences in age of sample frame, scale construction, differences in factors associated with arrest and deviance, and/or usage of random effects and logistic regression models. Also, usage of variables such as marriage and dropping out of high school are empirical associations and do not indicate causality. Usage of fixed effect models such as those utilized by Sampson et al. (2006) are useful in determining causation, but they eliminate time-invariant characteristics such as individual genotypes and the Wave III measure of father's incarceration that do not change over time in the sample.

For family-level variables, father's incarceration was associated with an approximately 20% decline in significance in the twins dataset, a 10% decline in predicting adult deviance, and a 10% decline in predicting adult arrest. No single variable stood out as uniformly significant across all sets of analysis, though being removed by social services was significant in all three sets of analysis. For the twins sample, father's attachment scale was significant in decreasing overall delinquency, while removal from home by social services was associated with an increase in delinquent activities. For deviance among young adult respondents, being raised in a single parent household, a history of repeated child abuse, and removal from home by social services were associated with increased likelihood of deviant behavior. For adult arrest, a history of child abuse and parental strictness were significant at the $p<0.05$ level, while being removed from a home by social services was significant at the $p<0.10$ level. Across these models, father's attachment was found only to be significant in the twins sample while low socioeconomic status and father's characteristics were generally not associated with deviance and arrest. These results suggest that a father's history of incarceration is not a proxy for transmission of socioeconomic status or adolescent relationships of the biological father to son. Instead, father's history of incarceration is a strong and unique predictor among family-level variables in predicting adult male delinquency and arrest.

In regression models, community-levels were found to be fairly insignificant in predicting delinquent behavior in both the sibling sample and in adult arrest and delinquency. Though not presented, use of factor scores based on Haynie et al's (2006) measures of relative deprivation also were found to be non-significant. These results may stem from the fact that community variables are relatively weak predictors of arrest (Guang and Zhou 2000); existing analysis linking crime and arrest in spatial studies normally examines violent crimes such as homicide and sexual assault (e.g., Sampson et al 2005; Land et al 1990). While Haynie et al.'s (2006) models do link Wave I community variables with Wave I and Wave II delinquency, their measures use pooled male and female data and negative binomial regression models to measure categorized count data for delinquent acts. These differences in Haynie's research design may hence explain differences with my findings.

In final models for adult delinquency and arrest, I estimated models with individual, community, and family variables. Having a father with a history of incarceration was associated with a 59% increase in Wave III delinquency (*$p<0.001$*) and an 89% increase in likelihood of arrest (*$p<0.001$*). Individual-level predictors were found to be most significant, with strongest results found for alcohol and/or substance abuse in predicting both delinquency and arrest, age and marriage in predicting delinquency, and dropping out of high school in predicting arrest. Among family-level variables, a history of repeated child abuse and being removed from a home by social services were significant predictors in delinquency, but were found to be non-significant in predicting arrest. As in the other models estimated, community variables were found to be non-significant in predicting delinquency and arrest.

When considering variables in the national twins sample and nationally-representative adult sample, analysis indicates that individual-level predictors are most strongly associated with reducing significance of father's incarceration in predicting adult arrest. Molecular genetics markers remained significant predictors when tested alongside father's history incarceration and empirically suggest that some biological factors operate independently from father's incarceration in predicting delinquent behavior. Family variables are also found to be significant, with a history of child abuse and family disruption through removal of child by social services as predictors. Community variables were found to exert no influence on delinquency in the models estimated. The significance of 1) genetic influences with individual and family influences and 2) individual and family variables also suggest that processes of cumulative advantage play a role in creating individual propensities for arrest. The significance of having an incarcerated father remained across all models tested, indicating father's history of incarceration is a robust predictor of delinquency and arrest that holds in a nationally representative sample of the U.S. population.

As with all studies, this analysis has limitations. With the twins sample, only a small number of genes were tested on a relatively small population. Currently, a Wave IV survey is being funded, which will increase the number of genes available for analysis with the entire Add Health sample, thus allowing the possibility of testing genotypes with a nationally-representative population. This will also allow for testing significance of genotypes as predictors of delinquency with more basic statistical methods. The Wave IV questionnaire also asks for greater detail on father's history of incarceration to better test for timing, while placing greater emphasis on interviewing individuals serving time in jail or prison. This analysis will provide a means of testing if father's history of incarceration may hold across latent classes of offenders (Moffitt 1993a; Nagin and Land 1993). Data on timing of father's incarceration may also be used in fixed effect models to determine if changes in delinquency occur within individuals. This form of analysis would suggest that father's incarceration might change behavior and not simply capture time-invariant correlations in father's socioeconomic status, societal discrimination, and societal inequality.

Table 4.1: Add Health twins sample, descriptive statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
Serious Delinquency Score				
Violent Delinquency Score			0	21
Father's Incarceration	0.141	0.348	0	1
Missing Father's Incarceration	0.050	0.219	0	1
Individual-Level Respondent Variables				
White	0.571	0.159	0	1
Black	0.173	0.378	0	1
Hispanic	0.147	0.345	0	1
Asian	0.081	0.273	0	1
Age at time of Wave III Interview	17.61	2.88	0	1
Age-squared	318.5	104.7	144	529
Substance Abuse, Wave 1	0.279	0.481	0	1
Respondent High School Dropout	0.132	0.339	0	1
Job Tenure, Wave III	14.2	19.8	0	190
Military Service	0.070	0.256		
School attachment Index	3.78	0.832	1	5
Missing School attachment Index	0.015	0.121	0	1
Marriage	0.122	0.328	0	1
Cohabitation	0.271	0.444	0	1
Family-Level Variables				
Two Parent Biological	0.635	0.481	0	1
Single Mom	0.181	0.385	0	1
Single Dad	0.026	0.161	0	1
Non-Biological Two Parent	0.134	0.341	0	1
Parental Strictness Scale, Wave 1	1.55	1.21	0	5
Missing Parental Strictness Scale, Wave 1	0.010	0.099	0	1
Child's Wave 1 Household Receipt of Food Stamps			0	1
Biological Father Provided Alimony Payments to Respondent Household	0.116	0.320	0	1
Missing Information on Whether Biological Father Gave Alimony	0.014	0.119	0	1
Wave I Father Involvement Scale	5.10	2.51	1	10
Missing Wave I Father Involvement Scale Score	0.118	0.323	0	1
Respondent Self-Report of Repeated Physical Abuse by Parent or Caregiver	0.081	0.273	0	1
Respondent removed from home by social services	0.013	0.114	0	1
			0	1

Community Level Variables				
Log Transformation of Population Density	0.247	0.302	0.001	2.610
Proportion of Census Tract classified as African American	0.124	0.137	0	1
Proportion of Census Tract with college degree	0.251	0.079	0	1
Genotype Variable				
DAT1				
9R/other,10R/other	0.049	0.209	0	1
9R/9R,	0.051	0.220	0	1
9R/10R	0.334	0.472	0	1
10R/10R	0.576	0.494	0	1
MAOA				
2R	0.010	0.098	0	1
3R,5R	0.427	0.494	0	1
3.5R, 4R	0.563	0.468	0	1
1DRD2				
A1/A1	0.547	0.498	0	1
A1/A2	0.371	0.483	0	1
A2/A2	0.081	0.273	0	1

Table 4.2: Add Health nationally-representative sample, sample means and individual-weighted standard deviations

Variable	Mean	Standard Deviation	Minimum	Maximum
Delinquency, Wave I	0.781	1.110	0	4
Adult arrest	0.150	0.358	0	1
Father's Incarceration	0.128	0.334	0	1
Missing Father's Incarceration	0.0577	0.233	0	1
Individual-Level Respondent Variables				
White	0.659	0.474	0	1
Native American	0.021	0.144	0	1
Black	0.152	0.359	0	1
Hispanic	0.116	0.320	0	1
Asian	0.040	0.197	0	1
Other Racial Category	0.012	0.109	0	1
Binge Drinking, Wave 1	0.117	0.332	0	1
Substance Abuse, Wave 1	0.254	0.456	0	1
Respondent's Job Tenure (in months)	14.22	20.46	0	190
Respondent With History of Marriage	0.118	0.322	0	1
Respondent With History of Cohabitation Only	0.294	0.456	0	1
Respondent's Military Service	0.060	0.237	0	1
Age at time of Wave III Interview	21.65	1.65	19	24
Respondent High School Dropout	0.165	0.372	0	1
School Attachment Index, Wave I	2.204	0.864	1	5
Missing School Attachment Index, Wave I	0.014	0.117	0	1
Family-Level Variables				
Two Biological parents	0.587	0.492	0	1
Single Mom	0.172	0.378	0	1
Single Dad	0.036	0.187	0	1
Non-Biological Two Parent	0.158	0.365	0	1
Other Family Arrangement	0.046	0.210	0	1
Parental Strictness	1.498	1.223	0	5
Child's Wave 1 Household Receipt of Food Stamps	0.103	0.303	0	1
Biological Father Provided Alimony Payments to Respondent Household	0.132	0.339	0	1
Missing Information on Whether Biological Father Gave Alimony	0.119	0.324	0	1
Wave I Father Involvement Scale	5.262	2.368	1	10
Missing Wave I Father Involvement Scale Score	0.117	0.321	0	1
Respondent Self-Report of Repeated Physical Abuse by Parent or Caregiver	0.079	0.269	0	1
Respondent removed from home by	0.017	0.128	0	1

social services				
Community Level Variables				
Log Transformation of Population Density	0.255	0.372	0.001	2.610
Proportion of Census Tract classified as African American	0.134	0.135	0	1
Proportion of Census Tract with college degree	0.251	0.081	0	1

Table 4.3: Twins sample, HLM regression estimates and standard errors for genetic, individual, family, and community-level predictors of serious and violent delinquency

	Serious Delinquency Scale				Violent Delinquency Scale			
Variable	Base Model	Individual	Family	Community	Base Model	Individual	Family	Community
Intercept	-3.50 2.15	-0.287	-2.20	-3.72**	-1.19 (1.52)	0.5124	-0.490	-1.15
		(2.14)	(2.20)	(2.16)		(1.52)	(1.56)	(1.53)
Father's Incarceration	0.912***	0.4960*	0.734***	0.927***	0.629***	0.358*	0.515**	0.625***
	(0.220)	(0.202)	(0.224)	(0.221)	(0.155)	(0.144)	(0.157)	(0.156)
Missing Father's Incarceration	-0.587+	-0.708*	-0.686+	-0.6047	-0.472+	-0.571*	-0.566	-0.478*
	(0.347)	(0.315)	(0.354)	(0.346)	(0.243)	(0.225)	(0.248)	0.243
Individual-Level Respondent Variables								
Black	0.477*	0.451*	0.379	0.705*	0.389*	0.378*	0.312+	0.476
	(0.230)	(0.206)	(0.240)	(0.282)	(0.161)	(0.147)	(0.169)	(0.198)
Hispanic	0.569*	0.338 (0.217)	0.575**	0.511*	0.414*	0.244	0.404*	0.426
	(0.244)		(0.244)	(0.258)	(0.171)	(0.156)	(0.172)	(0.182)
Asian	0.347*	0.753**	0.1968	0.248	0.1534	0.425* (0.194)	0.055	0.155
	(0.306)	(0.273)	(0.308)	(0.320)	(0.214)		(0.215)	(0.224)
Age	0.672**	0.430+	0.6291	0.667**	0.335*	0.191	0.317+	0.334*
	(0.238)	(0.237)	(0.239)	(0.238)	(0.168)	(0.168)	(0.169)	(0.168)
Age-Squared	-0.023***	-0.016*	-0.021***	-0.022***	-0.012**	-0.009+	-0.012*	-0.012
	(0.006)	(0.006)	(0.007)	(0.006)	0.0046	(0.005)	(0.005)	(0.005)
Substance Abuse, Wave 1	-	1.724	-	-	-	1.054***	-	-
		(0.159)				(0.113)		
Respondent's Job Tenure (in months)	-	0.004	-	-	-	0.003	-	-
		(0.003)				(0.002)		
Respondent With History of Marriage	-	-0.123	-	-	-	-0.031	-	-
		(0.213)				(0.153)		
Respondent With History of Cohabitation Only	-	0.521**	-	-	-	0.391***	-	-
		0.159				0.113		
Respondent's Military Service	-	-	-	-	-	0.119	-	-
						(0.192)		
Respondent High School Dropout	-	1.169***	-	-	-	0.944***	-	-
		(0.211)				(0.151)		
School Attachment Index, Wave I	-	-0.394***	-	-	-	-0.196**	--	-
		0.0848				(0.060)		
	-	-	-	-	-	-	-	-

Family-Level Variables	-	-	-	-	-	-	-	-
Single Mom	-	-	0.1156	-	-	-	0.077	-
			(0.310)				(0.218)	
Single Dad	-	-	0.6078	-	-	-	-0.366	-
			(0.505)				(0.356)	
Non-Biological Two Parent	-	-	0.3530	-	-	-	0.2029	-
			(0.291)				(0.204)	
Parental Strictness	-	-	-0.059	-	-	-	-0.032	-
			(0.0622)				(0.043)	
Child's Wave 1 Household Receipt of Food Stamps	-	-	-0.1479	-	-	-	0.050	-
			(0.282)				(0.199)	
Biological Father Provided Alimony Payments to Respondent Household	-	-	-0.1173	-	-	-	-0.1261	-
			(0.314)				(0.221)	
Wave I Father Involvement Scale	-	-	-0.142***	-	-	-	-0.084**	-
			(0.0441)				(0.031)	
Respondent Self-Report of Repeated Physical Abuse by Parent or Caregiver	-	-	0.3640	-	-	-	0.2387	-
			(0.265)				(0.186)	
Respondent removed from home by social services	-	-	1.757*	-	-	-	1.296*	-
			(0.714)				(0.509)	
	-	-	-	-	-	-	-	-
Community Level Variables					-	-	-	-
Log Transformation of Population Density	-	-	-	0.1804	-	-	-	0.037
				(0.295)				(0.207)
Proportion of Census Tract classified as African American	-	-	-	-0.965	-	-	-	-0.390
				(0.748)				(0.527)
Proportion of Census Tract with college degree	-	-	-	1.27	-	-	-	-0.0246
				(1.14)				(0.805)
DAT1								
9R/other,10R/other	-	-	-	-	-	-	-	-
9R/9R,	-	-	-	-	-	-	-	-
9R/10R	0.726*	0.420	0.517+	0.729*	0.481*	0.2858	0.479*	0.479*
	(0.288)	(0.260)	(0.277)	(0.288)	(0.202)	(0.186)	(0.203)	(0.202)
10R/10R	0.523+	0.203	0.734*	0.528+	0.312	0.1125	0.2930	0.3104
	(0.276)	(0.249)	(0.289)	(0.276)	(0.194)	(0.179)	(0.195)	(0.194)
MAOA								
2R	1.642*	1.28+	1.55* (0.765)	1.54*	1.4220**	1.179*	1.3650*	1.39*

	(0.772)	(0.694)		(0.779)	0.550	(0.505)	(0.547)	(0.555)
3R,5R	-0.1174	-0.135	-0.122	-0.116	-0.026	-0.036	-0.019	-0.026
	(0.161)	(0.114)	(0.161)	(0.160)	(0.112)	(0.103)	(0.113)	(0.112)
3.5R, 4R	-	-	-	-	-	-	-	-
1DRD2								
A1/A1	-0.249	-0.087	-0.2763+	-0.249	-0.251*	-0.1543	-0.269*	-0.2482
	(0.167)	(0.151)	(0.167)	(0.167)	(0.118)	(0.108)	(0.117)	(0.118)
A1/A2	-	-	-	-	-	-	-	-
A2/A2	-0.7318*	-0.411	-0.713*	-0.721*	-0.523*	-0.329+	-0.500*	-0.527*
	(0.304)	(0.275)	0.302	(0.304)	(0.213)	(0.196)	(0.212)	(0.213)
N	3189	3189	3189	3189	3189	3189	3189	3189
-2log Likelihood	16644.1	16413.1	16615.8	16639.9	14429.8	14239.9	14404.5	14429.2

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 4.4: Nationally-representative sample, ordered logistic regression odds ratios and standard errors for individual, family, and community-level predictors of Wave III delinquency

Variable		Individual	Family	Community	Combined
Father's Incarceration	1.838***	1.617***	1.756***	1.856***	1.588***
	(0.187)	(0.158)	(0.183)	(0.187)	(0.162)
Missing Father's Incarceration	0.995	0.921	0.956	0.982	0.974
	(0.143)	(0.143)	(0.163)	(0.144)	(0.176)
Individual-Level Respondent Variables					
Native American		0.790			0.833
		(0.190)			(0.209)
Black		1.168			1.154
		(0.142)			(0.156)
Hispanic		0.999			0.986
		(0.101)			(0.102)
Asian		0.955			0.902
		(0.161)			(0.166)
Other Racial Category		1.231			1.192
		(0.3293)			(0.310)
Binge Drinking, Wave 1		1.526**			1.486**
		(0.175)			(0.177)
Substance Abuse, Wave 1		1.648***			1.627***
		(0.151)			(0.148)
Respondent's Job Tenure (in		0.997			0.997
months)		(0.002)			(0.002)
Respondent With History of		0.512***			0.503***
Marriage		(0.066)			(0.067)
Respondent With History of		1.119			1.134
Cohabitation Only		(0.094)			(0.096)
Respondent's Military Service		1.462*			1.495*
		(0.237)			(0.246)
Age at time of Wave III Interview		0.823***			0.813***
		(0.020)			(0.021)
Respondent High School Dropout		1.206+			1.235+
		(0.108)			(0.135)
School Attachment Index, Wave I		1.084+			1.089+
		(0.049)			(0.048)
Family-Level Variables					
Single Mom			1.267*		1.067
			(0.142)		(0.119)
Single Dad			1.237		0.964
			(0.246)		(0.194)
Non-Biological Two Parent			1.139		1.027
			(0.133)		(0.125)
Other Family Arrangement			0.757		0.707
			(0.215)		(0.209)
Parental Strictness					0.936+
					(0.033)
Child's Wave 1 Household Receipt			1.041		0.971
of Food Stamps			(0.180)		(0.164)
Biological Father Provided			0.993		0.995
Alimony Payments to Respondent			(0.116)		(0.117)
Household					
Missing Information on Whether			0.895 (0.091)		0.922

Biological Father Gave Alimony					(0.104)
Wave I Father Involvement Scale			1.016		0.971
			(0.022)		(0.164)
Missing Wave I Father Involvement Scale Score			0.945		0.960
			(0.157)		(0.163)
Respondent Self-Report of Repeated Physical Abuse by Parent or Caregiver			1.404*		1.535**
			(0.197)		(0.219)
Respondent removed from home by social services			2.999**		2.765*
			(1.225)		(1.205)
Community Level Variables					
Log Transformation of Population Density				1.121	1.182+
				(0.118)	(0.102)
Proportion of Census Tract classified as African American				1.273	1.167
				(0.411)	(0.389)
Proportion of Census Tract with college degree				2.000	1.476
				(1.108)	(0.817)
N	5947	5947	5947	5947	5947
Pseudo Log-Likelihood	-11487288	-11182800	-11433396	-11478561	-11125360

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Table 4.5: Nationally-representative sample, logistic regression odds ratios and standard errors for individual, family, and community-level predictors of respondent incurring adult arrest

Variable		Individual	Family	Community	Combined
Father's Incarceration	2.12***	1.866***	1.894**	2.155***	1.846***
	(0.241)	.223	(0.217)	(0.247)	(0.223)
Missing Father's Incarceration	1.455+ (0.292)	1.290 (0.255)	1.285	1.439+	1.283
			(0.276)	(0.292)	(0.275)
Individual-Level Respondent Variables					
Native American		0.531+			0.541+
		(0.185)			(0.191)
Black		1.191			1.277
		(0.168)			(0.232)
Hispanic		0.779			0.749+
		(0.130			(0.130)
Asian		0.733			0.683
		(0.201)			(0.193)
Other Racial Category		0.790			0.767
		(0.609)			(0.602)
Binge Drinking, Wave 1		1.601**			1.587**
		(0.228)			(0.222)
Substance Abuse, Wave 1		2.536***			2.503***
		(0.301)			(0.307)
Age at time of Wave III Interview		0.958			0.946
		(0.033)			(0.034)
Respondent High School Dropout		1.577**			1.669***
		(0.195)			(0.219)
School Attachment Index, Wave I		0.971			0.970
		(0.066)			(0.065)
Family-Level Variables					
Single Mom			1.205		0.928
			(0.199)		(0.167)
Single Dad			1.576+		1.104
			(0.394)		(0.284)
Non-Biological Two Parent			1.139		0.981
			(0.187)		(0.170)
Other Family Arrangement			1.331		1.063
			(0.392)		(0.323)
Parental Strictness			0.907*		0.929
			(0.040)		(0.045)
Child's Wave 1 Household Receipt of Food Stamps			0.924 (0.204)		0.829 (0.184)
Biological Father Provided Alimony Payments to Respondent Household			0.973 (0.155)		0.949 (0.150)
Missing Information on Whether Biological Father Gave Alimony			0.970 (0.149)		0.923 (0.152)
Wave I Father Involvement Scale			0.973		1.000
			(0.030)		(0.030)
Missing Wave I Father Involvement Scale Score			0.989 (0.204)		1.111 (0.234)
Respondent Self-Report of Repeated Physical Abuse by Parent or Caregiver			1.379* (0.222)		1.301 (0.209)
Respondent removed from home by social services			1.929+ (0.657)		1.476 (0.552)

Community Level Variables					
Log Transformation of Population Density				1.001	0.838
				(0.192)	(0.356)
Proportion of Census Tract classified as African American				1.305	1.053
				(0.449)	(0.205)
Proportion of Census Tract with college degree				2.550	2.987
				(1.59)	(1.974)
N	5936	5936	5936	5936	5936
Pseudo Log-Likelihood	-4080649.3	-3870699.7	-4045654.2	-4076910.1	-3847579.7

+p<.10 *p<.05 **p<.01 ***p<.001 [two-tailed test]

Appendix 1:

List of Data Sources Used for Analysis in Chapter Two

Segregation Data:

Iceland, John, Daniel H. Weinberg, and Erica Steinmetz. 2004. "Racial and ethnic residential segregation in the united states, 1980-2000. Available online at: *Http://www.Census.Gov/hhes/www/resseg.Html*." vol. Special Report Series, CENSR # 3.: U.S. Census Bureau.

Population Data:

Ruggles, Steven , Matthew Sobek, Trent Alexander, Catherine A. Fitch, Ronald Goeken, Patricia Kelly Hall, Miriam King, and Chad Ronnander. 2005. "Integrated public use microdata series: Version 3.0 [machine-readable database] available online at: Www.Usa-ipums.Org." Minneapplois, MN: Minnesota Population Center, University of Minnesota.

F.B.I. Arrest Data:

Chilton, Roland and Dee Weber. 2000. "Uniform crime reporting program [united states]: Arrests by age, sex, and race for police agencies in metropolitan statistical areas, 1960-1997 [computer file]." Amherst, MA: University of Massachusetts [producer], 2000. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2000.

U.S. Dept. of Justice, Federal Bureau of Investigation. 2006. "Uniform crime reporting program data [united states]: County-level detailed arrest and offense data, 2000 [computer file]." ICPSR03451-v4: Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor], 2006-01-16.

Appendix 2:

Listing of MSA and Years Included in Chapter Two Dataset

MSA	Census Years included in dataset
Abilene, TX MSA	1980, 1990, 2000
Akron, OH PMSA	1980, 1990, 2000
Allentown-Bethlehem-Easton, PA MSA	1990, 2000
Altoona, PA MSA	1980, 1990, 2000
Anchorage, AK MSA	1990, 2000
Anniston, AL MSA	1980, 2000
Bakersfield, CA MSA	1980, 1990, 2000
Baltimore, MD PMSA	1990, 2000
Bellingham, WA MSA	1990, 2000
Benton Harbor, MI MSA	1990, 2000
Bergen-Passaic, NJ PMSA	1990, 2000
Billings, MT MSA	1980, 1990, 2000
Bloomington, IN MSA	1990, 2000
Brazoria, TX PMSA	1980, 1990, 2000
Brownsville-Harlingen-San Benito, TX MSA	1980, 1990, 2000
Bryan-College Station, TX MSA	1990, 2000
Buffalo-Niagara Falls, NY MSA	1980, 1990, 2000
Canton-Massillon, OH MSA	1980, 1990
Cedar Rapids, IA MSA	1980, 1990, 2000
Champaign-Urbana, IL MSA	1980, 1990
Chicago, IL PMSA	1980, 1990, 2000
Chico-Paradise, CA MSA	1990, 2000
Cleveland-Lorain-Elyria, OH PMSA	1980, 1990
Colorado Springs, CO MSA	1980, 1990, 2000
Columbia, MO MSA	1980, 1990, 2000
Columbia, SC MSA	1980, 1990
Columbus, OH MSA	1990, 2000
Daytona Beach, FL MSA	1980, 1990
Decatur, IL MSA	1980, 1990
Detroit, MI PMSA	1980, 1990, 2000
El Paso, TX MSA	1980, 1990, 2000
Elkhart-Goshen, IN MSA	1980, 1990, 2000
Eugene-Springfield, OR MSA	1980, 1990, 2000
Fayetteville, NC MSA	1980, 1990, 2000
Flint, MI PMSA	1980, 1990
Florence, SC MSA	1980, 1990
Fort Collins-Loveland, CO MSA	1990, 2000
Fort Lauderdale, FL PMSA	1980, 1990
Fort Myers-Cape Coral, FL MSA	1980, 1990
Gainesville, FL MSA	1980, 1990
Galveston-Texas City, TX PMSA	1980, 1990, 2000

Gary, IN PMSA	1980, 1990, 2000
Greeley, CO PMSA	1980, 1990, 2000
Greensboro--Winston-Salem--High Point, NC MSA	1990, 2000
Hagerstown, MD PMSA	1990, 2000
Hamilton-Middletown, OH PMSA	1980, 1990, 2000
Honolulu, HI MSA	1980, 1990, 2000
Houston, TX PMSA	1990, 2000
Jacksonville, NC MSA	1990, 2000
Jamestown, NY MSA	1990, 2000
Jersey City, NJ PMSA	1980, 1990, 2000
Kenosha, WI PMSA	1980, 1990
Lakeland-Winter Haven, FL MSA	1980, 1990
Lancaster, PA MSA	1980, 1990, 2000
Lincoln, NE MSA	1980, 1990, 2000
Los Angeles-Long Beach, CA PMSA	1980, 1990, 2000
Lubbock, TX MSA	1980, 1990, 2000
Madison, WI MSA	1980, 1990
McAllen-Edinburg-Mission, TX MSA	1990, 2000
Medford-Ashland, OR MSA	1980, 1990, 2000
Melbourne-Titusville-Palm Bay, FL MSA	1980, 1990
Merced, CA MSA	1990, 2000
Middlesex-Somerset-Hunterdon, NJ PMSA	1990, 2000
Mobile, AL MSA	1980, 1990, 2000
Modesto, CA MSA	1980, 1990, 2000
Monroe, LA MSA	1980, 1990, 2000
Montgomery, AL MSA	1980, 2000
Muncie, IN MSA	1980, 2000
New York, NY PMSA	1980, 1990, 2000
Newark, NJ PMSA	1980, 1990, 2000
Oakland, CA PMSA	1990, 2000
Olympia, WA PMSA	1990, 2000
Orange County, CA PMSA	1990, 2000
Philadelphia, PA-NJ PMSA	1980, 1990, 2000
Phoenix-Mesa, AZ MSA	1980, 1990, 2000
Pittsburgh, PA MSA	1980, 2000
Provo-Orem, UT MSA	1980, 1990, 2000
Pueblo, CO MSA	1980, 1990, 2000
Racine, WI PMSA	1980, 1990
Reading, PA MSA	1980, 1990, 2000
Redding, CA MSA	1990, 2000
Reno, NV MSA	1980, 2000
Riverside-San Bernardino, CA PMSA	1990, 2000
Rochester, MN MSA	1990, 2000
Sacramento, CA PMSA	1980, 1990
Salt Lake City-Ogden, UT MSA	1980, 2000
San Diego, CA MSA	1980, 1990, 2000
San Francisco, CA PMSA	1990, 2000
San Jose, CA PMSA	1980, 1990, 2000
Santa Rosa, CA PMSA	1980, 1990, 2000
Sarasota-Bradenton, FL MSA	1980, 1990
Seattle-Bellevue-Everett, WA PMSA	1990, 2000
Sharon, PA MSA	1990, 2000
South Bend, IN MSA	1980, 1990, 2000
Spokane, WA MSA	1980, 1990, 2000
Springfield, IL MSA	1980, 1990

State College, PA MSA	1990, 2000
Stockton-Lodi, CA MSA	1980, 1990, 2000
Tacoma, WA PMSA	1980, 1990, 2000
Tampa-St. Petersburg-Clearwater, FL MSA	1980, 1990
Tucson, AZ MSA	1980, 1990, 2000
Tyler, TX MSA	1980, 1990, 2000
Vallejo-Fairfield-Napa, CA PMSA	1990, 2000
Ventura, CA PMSA	1990, 2000
Vineland-Millville-Bridgeton, NJ PMSA	1980, 1990, 2000
Visalia-Tulare-Porterville, CA MSA	1990, 2000
Waco, TX MSA	1980, 1990, 2000
Waterloo-Cedar Falls, IA MSA	1980, 1990, 2000
West Palm Beach-Boca Raton, FL MSA	1980, 1990
Wichita Falls, TX MSA	1980, 1990, 2000
Yakima, WA MSA	1980, 1990, 2000
York, PA MSA	1990, 2000

Appendix 3:

Delinquency Scales Utilized in Chapter Four for Twins and Nationally-Representative Sub-samples

<u>List of self-reported delinquency items used for twin's sample</u>
(based on Haynie 2003; Hannon 2003; Hagan and Foster 2003; Guo et. al 2008)

1. In the past 12 months, how often did you use someone else's credit card , bankcard , or automatic teller card without their permission or knowledge? [B]
2. In the past 12 months, how often did you hurt someone badly enough in a physical fight that he or she needed care from a doctor o r nurse?[A]
3. In the past twelve months, how often did take part in a fight in which you were so badly injured that you were treated by a doctor or nurse?[A]
4. In the past twelve months, how often did you use or threaten to use a weapon to get something from someone?[A]
5. In the past twelve months, how often did you take part in a physical fight where a group of your friends was against another group?[A]
6. In the past twelve months, how often did you steal something worth more than $50?[B]
7. In the past twelve months, how often did you steal something worth less than $50?[B]
8. In the last twelve months, how often did you deliberately damage property that didn't belong to you?[B]
9. In the past twelve months how often did you carry a handgun to school or work?[A]
10. In the past twelve months, how often did you go into a house or building to steal something?[B]
11. In the past twelve months, how often did you sell marijuana or other drugs?[B]
12. In the past twelve months, how often did you buy, sell, or hold stolen property?[B]
13. In the past twelve months, how often did you deliberately write a bad check?[B]
14. In the past twelve months, have you shot or stabbed someone?[A]
15. In the past twelve months, have you pulled a knife or gun on someone?[A]

[A]For this question, positive score values are coded as violent behaviors
[B]For this question, positive scores values are coded as non-violent behaviors

<u>List of self-reported delinquency items used for nationally-representative sample</u>
(based on Haynie 2003; Hannon 2003; Hagan and Foster 2003)

1. In the past 12 months, how often did you use someone else's credit card, bankcard, or automatic teller card without their permission or knowledge? B
2. In the past 12 months, how often did you hurt someone badly enough in a physical fight that he or she needed care from a doctor o r nurse? A
3. In the past twelve months, how often did take part in a fight in which you were so badly injured that you were treated by a doctor or nurse? A
4. In the past twelve months, how often did you use or threaten to use a weapon to get something from someone? A
5. In the past twelve months, how often did you take part in a physical fight where a group of your friends was against another group? A
6. In the past twelve months, how often did you steal something worth more than $50? B
7. In the past twelve months, how often did you steal something worth less than $50? B
8. In the last twelve months, how often did you deliberately damage property that didn't belong to you? B
9. In the past twelve months how often did you carry a handgun to school or work? A
10. In the past twelve months, how often did you go into a house or building to steal something? B
11. In the past twelve months, how often did you sell marijuana or other drugs? B
12. In the past twelve months, how often did you buy, sell, or hold stolen property? B
13. In the past twelve months, how often did you deliberately write a bad check? B
14. In the past twelve months, have you shot or stabbed someone? A
15. In the past twelve months, have you pulled a knife or gun on someone? A

A For this question, positive score values are coded as violent behaviors
B For this question, positive scores values are coded as non-violent behaviors.

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