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Untangling the Interconnected Relationships between Alcohol Use, Employment, and Offending

Margaret M. Chrusciel
University of South Carolina

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Untangling the Interconnected Relationships between Alcohol Use, Employment, and
Offending

by

Margaret M. Chrusciel

Bachelor of Arts
University of South Carolina, 2009

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Accepted by:

Scott E. Wolfe, Major Professor

Robert Brame, Committee Member

Tia Stevens Andersen, Committee Member

Jeffrey Rojek, Committee Member

Cheryl L. Addy, Vice Provost and Dean of the Graduate School

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DEDICATION

This dissertation is dedicated to my dear friend Sasha Krupenko, the most curious and brilliant person I've had the honor to know. May your memory live on forever.

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As I stand at the finish line of this behemoth of an undertaking, I see more clearly than ever how essential the support of others has been to my academic success. I owe my sincerest gratitude to those who have supported me along the way and helped make this dissertation possible. First and foremost, thank you Dr. Scott Wolfe, my committee chair, whose mentorship has proven invaluable. You stepped up for me after a number of departmental changes left me flailing, without a mentor, and with no idea how to move forward. Because of you, I was able to get back on track and begin moving toward the light at the end of the tunnel. Thank you for your unrelenting patience, support, and guidance along the way and for challenging me to think critically and expand my perspective. I have learned so very much from you and will be forever grateful for all you have done for me.

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ABSTRACT

Both substance use and employment are correlates of crime that are heavily examined by criminological research. Efforts to explore these connections have produced two rich bodies of literature that provide insight into the nuances of the relationship between substance use and offending and the relationship between employment and crime. Research shows that while substance use increases subsequent criminal behavior, employment seems to reduce offending. Given the strong positive association between substance use and crime and the inverse effect of employment on offending, it is possible that drug use and employment interact in their impact on crime. In addition to potential moderation, the relationship between drug use, employment, and crime may be explained by mediation mechanisms. Thus, the current study uses data from the 1997 National Longitudinal Survey of Youth (NLSY97) to examine the possibility of moderation and/or mediation between substance use and employment in their impact on offending.

TABLE OF CONTENTS

DEDICATION	iii
ACKNOWLEDGEMENTS	iv
ABSTRACT	viii
LIST OF TABLES	x
LIST OF FIGURES	xii
CHAPTER 1 - INTRODUCTION	1
CHAPTER 2 - REVIEW OF EXTANT LITERATURE	5
CHAPTER 3 - DATA AND METHODS	53
CHAPTER 4 - RESULTS	87
CHAPTER 5 - DISCUSSION	127
REFERENCES	137
APPENDIX A - INDIVIDUAL EFFECTS OF VARIABLES CONSIDERED IN INTERACTIONS	162
APPENDIX B - SENSITIVITY ANALYSES	166

LIST OF TABLES

Table 3.1 Sample description and retention rates for both the cross-sectional, supplemental, and total sample at year survey round.	61
Table 4.1 Demographic Characteristics of NLSY97 Respondents and of the US Population Aged 17 to 27.	88
Table 4.2 Summary Statistics for Key Variables of Interest.	88
Table 4.3 Respondent Self-Reported Offending, Ages 18-27 ($n=8,738$).	89
Table 4.4 Differences in Drinking and Employment by Arrest, Ages 18-27 ($n=8,730$).	90
Table 4.5 Differences in Drinking and Employment by Conviction, Ages 18-27 ($n=8,730$).	91
Table 4.6 Differences in Drinking and Employment by Property Offenses, Ages 18-27 ($n=8,730$).	91
Table 4.7 Differences in Drinking and Employment by Violent Offenses, Ages 18-27 ($n=8,730$).	92
Table 4.8 The Effects of Drinking Alcohol and Employment on Arrest: Multilevel Logit Model Estimates ($n=8,340$).	96
Table 4.9 The Effects of Binge Drinking and Employment on Arrest: Multilevel Logit Model Estimates ($n=8,110$).	100
Table 4.10 The Effects of Alcohol and Employment on Conviction: Multilevel Logit Model Estimates ($n = 8,632$).	103
Table 4.11 The Effects of Binge Drinking and Employment on Conviction: Multilevel Logit Model Estimates ($n = 8,632$).	104
Table 4.12 The Effects of Alcohol and Employment on Property Offenses: Multilevel Logit Model Estimates. ($n = 8,632$).	108
Table 4.13 The Effects of Binge Drinking and Employment on Property Offending: Multilevel Logit Model Estimates. ($n = 8,632$).	109

Table 4.14 The Effects of Alcohol and Employment on Violent Offending: Multilevel Logit Model Estimates. ($n = 8,636$).	111
Table 4.15 The Effects of Binge Drinking and Employment on Violent Offending: Multilevel Logit Model Estimates ($n = 8,632$).	113
Table 4.16 The Effects of Employment and Offending on Alcohol Consumption: Multilevel Negative Binomial Model Estimates. ($n = 8,217$).	114
Table 4.17 The Effects of Employment and Offending on Binge Drinking: Multilevel Negative Binomial Model Estimates. ($n = 8,266$).	116
Table 4.18 The Effects of Drinking and Offending on Employment: Multilevel Generalized Linear Model Estimates ($n = 7,855$)	119
Table 4.19. Mediating effects in the relationship between alcohol and offending.	123
Table 4.20 Mediating effects of alcohol in the relationship between employment and offending.	124
Table 4.21 Mediating effects in the relationship between offending and employment.	126

LIST OF FIGURES

Figure 3.1 Distribution of Missing Values	65
Figure 3.2 Moderation in the Alcohol-Employment-Offending Relationship.....	78
Figure 3.3 Unmediated Model	80
Figure 3.4 Mediated Model.....	81
Figure 3.5 Mediation in the Alcohol-Employment-Offending Relationship.....	85
Figure 4.1 Theoretical Model of Interaction Effects of Days of Drinking and Weeks Worked on Arrest	97
Figure 4.2 Interaction Effects of Days of Drinking and Weeks Worked on Arrest	98
Figure 4.3 Individual Effects of Weeks Worked and Alcohol on Arrest	99
Figure 4.4 Theoretical Model of Interaction Effects of Binge Drinking and Weeks Worked on Arrest	101
Figure 4.5 Interaction Effects of Binge Drinking and Weeks Worked on Arrest.....	102
Figure 4.6 Theoretical Model of the Interaction Effects of Days of Binge Drinking and Weeks Worked on Conviction	105
Figure 4.7 Interaction Effects of Days of Binge Drinking and Weeks Worked on Conviction.....	106
Figure 4.8 Theoretical Model of the Interaction Effects of Conviction and Weeks Worked on Binge Drinking.....	117
Figure 4.9 Interaction effects of Conviction and Weeks Worked on Binge Drinking.....	118
Figure 5.1 Understanding the substance use, employment, and offending relationship	131

CHAPTER 1

INTRODUCTION

Both substance use and employment are correlates of crime that are heavily examined by criminological research. Efforts to explore these relationships have produced two rich bodies of literature that provide insight into the nuances of the relationships between substance use and crime and between employment and crime. Research suggests that while drug use increases subsequent criminal behavior, employment seems to reduce offending. These relationships have been heavily studied across disciplines, and as a result, have produced extensive theoretical and empirical work to support their relevance. Because these topics have been so closely scrutinized from a variety of approaches, the role of substance use and employment in shaping criminal behavior is clouded by the many complexities existing research has identified. One such nuance of particular importance suggested in the literature is that a relationship also exists between substance use and employment. However, these three separate bodies of work have not been integrated into a cohesive exploration of the multifaceted and multidirectional relationship between drug use, employment, and offending.

Given the strong positive association between substance use and crime and the inverse effect of employment on offending, it is possible that drug use and employment interact in their impact on crime. Should that be the case, the relationship between drug use and crime may depend on employment status – that is, the drug-crime link may be

moderated by employment. In that case, employed drug users may differ from unemployed drug users in their propensity toward crime. Conversely, the relationship between employment and crime may depend on substance use (i.e., substance use moderates the work-crime connection). In addition to moderation, the relationship between substance use, employment, and crime may be explained by mediation mechanisms. It could be that the apparent positive relationship between drugs and crime is mediated by employment such that the drug use reduces employment which in turn increases offending. In other words, it is not substance use that directly results in increased offending, but rather unemployment caused by substance use that is responsible for elevated levels of crime. Not only is it possible that employment mediates the drugs and crime relationship, but it could also be that substance use explains the relationship between employment and crime. In that case, it is possible that employment decreases the likelihood of drug use, which then reduces offending.

Considering potential moderation and mediation effects, it behooves policymakers to reconsider reentry programs for drug-involved offenders. Should employment moderate and/or mediate the relationship between drug use and crime, treatment programs targeting drug-involved offenders might be more effective if they also focus on creating employment opportunities. In contrast, should drug use prove relevant to the link between employment and crime, many employment-based reentry programs for drug offenders could be more effective if they included a treatment referral component. However, efforts to explore this phenomenon without a deeper understanding of the drugs-crime and employment-crime links would prove futile.

Thus, the following sections delve into the multifaceted relationships between drugs, employment, and crime. A comprehensive literature review is presented in Chapter 2 and begins with an exploration into the theoretical underpinnings of each individual relationship (drugs-crime, employment-crime) with careful consideration of how the two are tied together. The next section will address the connection between drugs and crime at length, providing insight into its relevance, occurrence, and related measurement concerns. This section will also include a thorough examination of existing empirical work regarding the drugs-crime link. The following section will provide an extensive exploration into the dynamic connection between employment and crime including its theoretical foundation as well as the current status of empirical research regarding the relationship. Once the key issues of both the drugs-crime and employment-crime link have been identified, the closing section will attempt to piece together these separate bodies of literature into a cohesive understanding of the drugs-employment-crime relationship and identify any gaps in our understanding of this crucial connection between substance use, employment, and offending.

Chapter 3 will follow the literature review and will introduce the reader to the data, methods, and analytic strategy adopted in the current study. First, a thorough explanation of the dataset will be presented followed by the sampling design and survey administration procedures utilized to gather the data. Next, a brief description of the sample will be provided including an overview of missing data issues and how these will be addressed. The following section presents the measures utilized in this study to gauge the relationships between drug use, employment and crime, including the strategy for

addressing the complex survey design. Finally, the analytic technique adopted by the current study will be explained.

Results from analyses will be presented in Chapter 4, which is organized into three main sections. Summary descriptive statistics will first be provided followed by multivariate regression analyses. A series of multilevel negative binomial models will be used to analyze the relationships among variables using longitudinal data. Interactions (i.e., moderation) will also be considered in these analyses. Finally, relationships will be more closely scrutinized to consider the potential of mediation among variables.

CHAPTER 2

REVIEW OF EXTANT LITERATURE

Theoretical Underpinnings of the Connections between Substance Use, Employment, and Offending

The ties between crime and both substance use and employment are complex and nuanced. As a result, it is imperative that efforts to examine the interconnectedness of these relationships are guided by theory. Further, a clear understanding of the theoretical foundation for each of the relationships individually is fundamental to understanding the multidirectional and multifaceted nature of the drugs-employment-crime connection.

A tripartite model of drug use and crime

In an effort to explain how drug use and crime may be related, Goldstein (1985) developed his now widely accepted drug-crime taxonomy. Although his model was formulated to explain violence specifically, the theoretical principles can be extended to inform our understanding of crime generally. Goldstein (1985) maintains that drug use may cause criminal behavior directly as a result of its psychopharmacological effects and indirectly by shaping the drug market and its competition. Thus, crimes related to drug use include both those committed by persons under the influence of drugs and/or alcohol as well as those committed in the context of the illegal selling, manufacturing, and/or trafficking of substances. Perhaps most importantly, drug use may also cause crime indirectly via its impact on financial resources and economic need. Drug users often pour whatever money they have into supporting their habit, fueling an addiction that urges the

user to then purchase more drugs. As a result of the addiction cycle, users quickly find themselves in need of more drugs and without the financial means to acquire them (Craddock, Collins, & Timrots, 1994; Dorsey & Middleton, 2008; SAMHSA, 2014). This financial need then drives users to commit crime for purposes of feeding their drug habit. Consequently, Goldstein (1985) posits three types of crime related to drug use including (1) psychopharmacological violence, (2) economically compulsive crime, and (3) systemic violence. His tripartite model has been corroborated by various studies that explore the relationship between drugs and crime (Akers, 1992; Boles & Miotto, 2001; Goode, 1997; Kuhns & Clodfelter, 2008; Oser, Mooney, Staton-Tindall, & Leukefeld, 2009; Parker & Auerhahn, 1998; Popovici et al., 2012; White & Dorman, 2000).

Psychopharmacological crime

Because drugs interact with the brain in such a way that results in both psychological and physiological changes, the effects of the drugs on the body may both indirectly and directly result in crime. From this perspective, the psychopharmacological effects of drug use may directly lead to crime when a person becomes inebriated and behaves criminally because of the mental state produced by drug use (Boles & Miotto, 2003; Goldstein, 1985; Kuhns & Clodfelter, 2009). In addition, some have argued that chronic drug users assume “revolving roles” in offending such that they are likely to be both the offender and the victim (French et al., 2004, p. 219). In many ways, the psychopharmacological effects of drug use can account for increased victimization risk, especially if intoxication reduces the users’ awareness of their surroundings and/or coordination to protect themselves (Felson & Staff, 2010; Kuhns & Clodfelter, 2009; Popovici et al., 2012). More commonly however, the criminogenic consequences of the

psychopharmacological effects of drugs center on addiction, which can change the temperament and priorities of users and subsequently lead to crime as a means of obtaining more drugs. This will be further explained in the following section regarding economic-compulsive crime.

Although the psychopharmacological effects behind the drug-crime link are best illustrated with illicit drugs, its influence can also be seen when considering alcohol abuse and crime. Indeed, the relationship between criminal behavior and the psychopharmacological effects of alcohol are perhaps better understood than any other drug (Boles & Miotto, 2003; Exum, 2006). Alcohol consumption is related to lowered inhibitions, poor judgment, and overemotional behavior, all of which are related to violent behavior (Cohen et al., 1994; Lane et al., 2003; White, 1997). Extant literature consistently supports a relationship between alcohol use and *violent* offending (Boles & Miotto, 2003; Bushman & Cooper, 1990; Exum, 2006; Pernanen, 1991) and suggests the effects of binge drinking are associated with an increased likelihood of perpetrating both property and predatory crimes (Popovici et al., 2012). In general, a similar trend is found, although less strongly correlated, in empirical research regarding the effects of using other drugs on violent offending (Dawkins, 1997; Kinlock, O'Grady, & Hanlon, 2003; Kuhns & Clodfelter, 2009; Phillips, 2012). Although alcohol is the most likely substance to be involved in psychopharmacological violence (Fagan, 1990; Friedman, 1998), the effect of illicit drug use on *offending in general* is stronger and persists longer throughout the life course than that of alcohol use (Schroeder, Giordano, and Cernkovich, 2007),

Economic compulsive crime

The second key explanation for the relationship between drugs and crime primarily focuses on the economic underpinnings of both drug use and offending. Accordingly, drug abuse drains financial resources and requires users to find ways to obtain money to support their drug habit. Because crime offers a quick and easy way to do so, many drug users commit crimes specifically for the purpose of purchasing more drugs. This could explain why drug users are more likely to commit crimes such as burglary, robbery, and theft (Bennett, Holloway, & Farrington, 2008). This impacts users regardless of drug type and its associated cost. It makes sense that more expensive drugs require more money to support users' drug habits and this may translate into a greater likelihood of economic-compulsive crime for those without the funding to do so (Silverman & Spruill, 1977). In contrast, this is also somewhat counterintuitive because to become addicted to a more expensive drug, one would have to be able to afford to do it in the first place. Furthermore, the user would have to have enough money to continue to use enough to grow dependent which would suggest the user has ample financial resources and would not need to turn to crime to afford the substance. Research that explores the effect of drug price on crime produce ambiguous results (Caulkins & Reuter, 1998; MacCoun, Kilmer, & Reuter, 2003). Higher drug prices are related to a lower prevalence of use, but also may promote property crime to be able to afford the substance. As a result, the number of people committing drug-related crimes may be reduced, but those who continue to use drugs may be offending at higher rates than when the prices were lower (Caulkins, Dworak, Feichtinger, & Tragler, 2000).

The economic-compulsive model is perhaps most relevant for the current discussion of how employment impacts the drugs-crime link. Through this lens, drug users commit crimes in order to obtain resources to support their drug habit (Felson & Staff, 2015). One study of state and federal inmates found that nearly 20% of inmates self-reported that supporting their habit was their motivation for the offense that landed them behind bars (Rahtz, 2012). However, findings from a systematic review of the literature regarding the link between drugs and crime committed for material gain provide no evidence of such an association (Chaiken & Chaiken, 1990). In contrast, a more recent meta-analysis of the research on the drugs-crime connection found that the odds of shoplifting, prostitution, burglary, and robbery were greater among drug users as opposed to non-users, with heroin, crack, and cocaine users at the greatest odds of committing such offenses (Bennett, Holloway, & Farrington, 2008). This is consistent with other work in the area that suggests a majority of property offenders were motivated to commit a crime by financial reasons (Sutherland et al., 2015).

Indeed, the extant literature indicates that not only does a correlation exist between drugs and crime, but drug use is associated with increased offending across gender (French et al., 2000; Friedman, 1998; Holloway & Bennett, 2004; Popovici et al., 2012), drug type (Bennett, Holloway, & Farrington, 2008; Pedersen & Skardhamar, 2009), and crime type (Bennett, Holloway, & Farrington, 2008; Nurco, Hanlon, & Kinlock, 1991; Phillips, 2012), with some variation by location (White & Gorman, 2000). Furthermore, substance use is associated with higher levels of violent offending (Carpenter & Dobkin, 2009; Phillips, 2012; Cohen et al., 1994), criminal victimization (Popovici et al., 2012), and recidivism (Spohn & Holleran, 2006).

Systemic crime

A large portion of drug-related crime involves the illicit drug market. This “systemic crime” is that which results from the business of manufacturing and distributing drugs. Because those involved in the business of illicit drug distribution cannot turn to traditional law enforcement to settle disputes or enforce drug agreements, violence or threats of violence become the primary tool by which to do so. This type of violence occurs between rival drug dealers and within individual dealing hierarchies (Goldstein, 1985) and is generally referred to as competition violence and disciplinary violence, respectively (Reuter, 2009). Thus, systemic crime includes violent confrontations such as those between rival gangs fighting over rights to sell in a certain area as well as a high level dealer assaulting one of his own lower level dealers for failing to sell a large enough quantity. Systemic crime associated with any given drug is dependent specifically upon its illicit market and thus is sensitive to changes in supply, demand, policy, and enforcement. It is also important to note that the drug market is expansive and by its very nature, requires user’s involvement (i.e., user’s cannot get illicit drugs any other way than via the illicit market). Thus, many users somehow adopt at least one role in the drug economy. In fact, one study of over a thousand low-income heroin and cocaine users in Baltimore, Maryland found that nearly half of the respondents reported having one role in the drug market (Sherman & Latkin, 2002). It follows that these users were more likely to use daily and had associated with a greater number of users than those who were not involved in the market.

Goldstein’s (1985) tripartite framework makes the fundamental assumption that drugs somehow caused the offense – either directly from its effects, indirectly by

producing dependence, or indirectly via the large drug market. However, it is also possible that the same causal forces underlying alcohol and drug use drive criminal behavior. In that case, substance use and offending are both a consequence of the same causal mechanisms (Fernandez-Montalvo, Lopez-Goni, & Arteaga, 2012; Goldstein, 1985; Boles & Miotto, 2003). Furthermore, some argue that drugs and crime are not in fact causally related but rather merely associated with each other as well as a number of other deviant behaviors. Many have criticized Goldstein's (1985) framework (Bennett & Holloway, 2009; MacCoun et al., 2003), noting the inability to explain causal mechanisms (Parker & Auerhahn, 1998) and an overemphasis on systemic crime (White & Gorman, 2000).

Theoretical connections between employment and crime

While Goldstein's tripartite approach informs our understanding of drug-related crime, no such model has been developed to describe crime that is related to employment. Rather, several major criminological theories offer insight into the relationships between employment, substance use, and crime, pointing to a multitude of different causal mechanisms (Aaltonen, 2013; Uggen & Wakefield, 2008). Because employment functions to affect drug use and offending similarly, this section will explore the primary criminological theories that are most relevant to understanding how employment operates to influence crime. Theoretical explanations of the drugs-crime link and its relevance in the employment-crime connection will be embedded within each perspective.

At the most fundamental level, work may reduce crime simply because people have less time to engage in criminal behavior when they are spending their time occupied at work (Ehrlich, 1973; Hirschi, 1969; Osgood et al., 1996). By the same token, time

spent at work reduces the time available to consume alcohol or use drugs. However, people do not work every hour of the day, every day of the week. Thus, some criminologists have sought to explain why employment might impact decisions to commit or resist deviance while off the clock. Legal income is one of the primary factors that directly influence the employment-crime link. According to rational choice and economic theories, legitimate earnings reduce the appeal of crime for financial gain and increase the cost of offending (Becker, 1968; Ehrlich, 1973; Matsueda, Kreager, & Huizinga, 2006). By maintaining a job and earning legal income, the need to commit crime to obtain drugs or money for drugs shrinks while the costs associated with such behavior rise. Consequently, economic compulsive crime should be lower among the employed than the unemployed (Hunt, 1991; Nurco & Shaffer, 1982).

For the employed, arrest may result in the loss of a valued job that provides regular wages. Presuming rationality, employment then serves as a type of deterrent for offending by which individuals with a job will be less likely to commit crime and to abuse alcohol and/or drugs for fear of consequences at work (McCarthy, 2002; Pidd & Roche, 2014; Sherman & Smith, 1992; Uggen & Shannon, 2014). Many employers drug test applicants and/or newly hired employees, and in effect, the potential employment may serve as a deterrent for drug use (Anglin & Westland, 1989; MacDonald, & Wells, 1994; Mehay & Pacula, 1999). In order to pass the drug screening mandated by the potential employer, the applicant must refrain from using until the time of the drug test. The employer does not even have to actually drug test the applicant to be an effective deterrent. Simply the possibility that the individual could lose the job opportunity because they tested positive for drug use is a great enough risk to deter further drug use

(DuPont, Griffin, Siskin, Shiraki, & Katz, 1995). In addition to the direct connections outlined above, a number of theories emphasize the *indirect* effects of employment on crime. The following sections explore the various theories that contribute to our understanding of such indirect effects.

Work as a Mechanism of Social Control

Other theoretical explanations of the employment-crime connection have offered a number of ways in which employment indirectly impacts offending behavior. Social control theories maintain that the bonds to conventional society, such as those formed at work, are the central mechanism involved in the work-crime relationship. Accordingly, it is when the bond to society is broken that individuals engage in crime (Hirschi, 1969). Thus, work should strengthen social ties to society, creating a “stake in conformity,” and subsequently reduce offending and drug use (Toby, 1975). Unlike previous theories of social control, Sampson and Laub’s (1993) theory of age-graded social control examined how the relationship between social bonds and criminality varies across the life course, recognizing that factors such as priorities, interests, relationships, and routines shift and adapt as a person matures. From this approach, social ties are the fundamental feature of the criminal desistance process – that is, social relationships including those formed at work are the reason that people eventually stop committing crime and quit using drugs (Uggen, 2000; Sampson & Laub, 1993; Schroeder, Giordano, & Cernkovich, 2007; Skardhamar & Savolainen, 2014).

Given that the life course framework accounts for changes and continuity throughout the life span, it follows that the effects of employment depend on the given stage of the life-course. For adults, employment plays a key role in reducing the

likelihood of offending, but its impact during adolescence is less definite. Hirschi (1969) maintains the importance of work in providing ties to conventional society for young adults, but contends that becoming too involved in work may conflict with commitment to and involvement in schooling. As a result, high levels of work intensity could undermine the social bonds that control adolescents and keep them from deviance. Research does seem to suggest that increased intensity of work during adolescence is associated with a greater likelihood of delinquency (Bachman & Schulenberg, 1993; McMorris & Uggen, 2000; Mortimer, 2003; National Research Council, 1998). From a strain perspective, a criminogenic effect of employment is counterintuitive. Working should provide adolescents a legitimate means to obtain the culturally valued financial resources and material goods, thus alleviating strain and reducing delinquency (Agnew, 1992). The next section will provide a more detailed exploration of the strain/anomie perspective on drug use, employment, and crime.

The Strain of Unemployment

Anomie and strain theories are one of the leading criminological branches relevant to the relationship between employment and crime. Although largely attributed to the work of Robert K. Merton (1938; 1949a; 1949b; 1957), the roots of strain theory trace back to the work of renowned sociologist Emile Durkheim. After reviewing the work French philosopher Jean-Marie Guyau (1887) regarding religious “anomy” (Durkheim, 1887), Durkheim (1893) introduced the concept of “anomie” into sociological and criminological thought (Besnard, 1988; Orru, 1983). In practical terms, Durkheim broadened the focus of criminology to recognize the role of the larger social system. More specifically, society functions to restrain people from the uninhibited pursuit of

self-interest because, as Durkheim (1951) argues, “The more one has, the more one wants, since satisfactions received only stimulate instead of filling needs” (p. 248). Failure of the social system to constrain such behaviors results in a state of normlessness or “anomie” during which there is an uninhibited pursuit of personal goals and desires.

Merton builds on Durkheim’s concept of anomie in an effort to explain why certain groups and cultures were more likely engage in deviance (Murphy & Robinson, 2008). His work has inspired a number of theoretical extensions (Agnew, 1992, 1997; Cloward & Ohlin, 1961; Menard, 1995, 1997; Messner & Rosenfeld, 1994) and has remained a salient criminological theory for nearly a century (Antonaccio et al., 2015; Baumer, 2007; Bernberg, 2002; Garfield, 1982; Murphy & Robinson, 2008; Orru, 1983). Merton’s (1938) framework posits that social norms influence both culturally valued goals and the ability to achieve those goals (Merton, 1938). He was critical of control theories that had been dominating criminological discussion and that centered on the functions of social structure. In response, his original conceptualization of anomie aimed to explore the dysfunctions of the system (Merton, 1997) and describe how the social structure may “exert a definite pressure” on individuals to behave criminally (Merton, 1938, p. 37). On one hand, Durkheim’s (1951) notion of anomie maintains an inherently voracious and self-serving aspect of human nature that must be regulated by social structures. On the other, Merton contends that anomie refers to “a literal demoralization, i.e. a deinstitutionalization of the means,” that results from an accentuation of cultural goals (i.e., money and prestige) and an inadequate emphasis on the appropriate institutionalized means (i.e., legitimate employment) for achieving those goals (Merton, 1968, p. 190). Merton (1938) explains, “Aberrant conduct, therefore, may be viewed as a

symptom of dissociation between culturally defined aspirations and socially structured means” (p. 38). According to Merton, opportunity structures encumber the ability of segments of the population from achieving culturally valued goals, such as legitimate employment. Accordingly, social structure does not control or restrain criminality as described by Durkheim, but rather serves as a driving force pushing people into crime.

Indeed, not all individuals who experience strain or who are unemployed inevitably break the law. Rather, people “adapt” to feelings of strain in a variety of ways, including committing crime and/or using drugs. Agnew (1992), who played a particularly salient role in the development and clarification of Merton’s strain theory (1938), furthers this idea and formulates a general strain theory (GST) to explain individual level offending. GST maintains that individuals are “constrained” by internal and external factors that limit their opportunity and/or willingness to commit crime (Agnew, 1992, p. 73; Broidy, 2001). Thus, unlike the structural strain associated with anomie outlined by Durkheim and Merton, GST approaches strain at a social psychological level – that is, “it focuses on the individual and his or her immediate social environment” (Agnew, 1992, p. 48). In other words, GST posits that people commit crime based on mental and emotional perceptions and reactions to interactions with their social environment. From this perspective, individuals experience strain, or feelings of anger, frustration, and stress, as a result of unemployment. In its simplest form, micro-level strain theories maintain that in order to cope with this strain, some individuals turn to crime.

Differential Opportunities for Legitimate and Illegitimate Earnings

Recognizing the important contributions of strain theory, Cloward and Ohlin (1960) propose a theory of differential opportunity that draws on Merton’s (1938)

concept of strain as well as notions from Chicago School theorists such as Sutherland and Shaw and McKay (Cullen, 1988). Unsatisfied with Merton's inability to explain *why* some resorted to crime while others did not, Cloward and Ohlin looked to ideas of differential association outlined by Sutherland (1937) whereby individuals learn the values, attitudes, and techniques for criminal behavior through interactions with others. Put simply, people learn how to be a criminal from those around them. Accordingly, experiencing strain does not translate into offending unless the individual has learned how to behave criminally. Not everyone has equal access to conventional coping mechanisms (i.e., familial support) nor are deviant opportunities equally obtainable for all individuals. Personal characteristics, values, and disposition to deviance then influence which of the available adaptations individuals will select. In other words, some individuals turn to drugs and crime to cope with stress because they have access and doing so does not conflict with their personal beliefs or priorities. At the same time, others do not turn to crime or drugs when experiencing the exact same strain because they do not have the option available, or if they did, doing so would contradict what they believe is right or fair.

This point is clearly demonstrated when framed in context of the employment and crime relationship. Certainly not all who are unemployed are criminals. However, the research discussed in the previous section does seem to suggest a considerable amount of crime is committed by those without a job. The findings of Baron (2008) are consistent with the notion that not everyone who is unemployed commits crime and emphasize the role of individual differences in reacting to strain that stems from unemployment. Specifically, Baron (2008) asserts that it is how individuals perceive their unemployment

and their experiences in the labor market that influences whether this translates into offending behavior. Those individuals who believe they have been not given a fair chance when applying for jobs or who felt wrongfully terminated are the same people who are more likely to respond to strain with deviant behavior.

Theoretical Explanations of the Reciprocal Influence of Offending on Employment and Drug Use

Given the substantial amount of empirical support for the negative relationship between incarceration and diminished employment opportunities, a number of theories have been utilized in an effort to understand the nuances of this association.

Life-course theories

Criminologists commonly use life course theories to understand the barriers to employment experienced by those with a criminal record. As noted earlier, life course theorists maintain that situational characteristics are equally as important as the aspects of the individual in understanding behavior. These contextual factors, which include both static and dynamic forces, impose constraints upon individuals' opportunities, expectations, and resources (Loeber & LeBlanc, 1990). As life circumstances oscillate during adolescence and the transition into adulthood, criminal behavior will fluctuate in accordance with the context of the life course.

Incarceration interrupts the life cycle such that the transition from adolescence and young adulthood into stable, career employment is disrupted (Eden, Nelson, & Paranal, 2001; Petersilia, 2003; Pettit & Western, 2004; Sampson & Laub, 1990; Sampson & Laub, 1993; Uggen, 2000; Uggen & Shannon, 2014; Uggen & Wakefield, 2008). There are three primary explanations for why this disruption occurs. First, those individuals released from behind bars have few job skills and are often less educated than

the average adult on the job market, thus ex-inmates are usually the less qualified applicants. Secondly, many released offenders have lost touch with family and friends as a result of their incarceration, which decreases their ability to network and use connections to find employment. Finally, offenders face difficulty in gaining employment as a result of the stigma attached to their status as a convicted offender (Western, 2002). Whatever the reason, the inability to appropriately transition into employment can be consequential in terms of criminality. Research suggests that work can serve as a turning point away from offending and into conventional society (Skardhamar & Savolainen, 2014).

Labeling theory

Prior theoretical examinations of the consequences of the criminal stigma have primarily focused on labeling theory, which posits that the label associated with a criminal record causes the individual to not only be treated differently by others in society, but also changes their perception of self. Frank Tannenbaum (1938) was the first to discuss the ways in which punishment “dramatizes” the evil of criminals, ideas which later formed the foundation for the labeling theory. In his words,

“the process of making the criminal, therefore, is a process of tagging, defining, identifying, segregating, describing, emphasizing, making conscious and self-conscious; it becomes a way of stimulating, suggesting, emphasizing, and evoking the very traits that are complained of” (p.20).

Labeling theory thus argues that the stigma attached to labels such as “felon” or “drug addict” can indirectly result in changes in self-perception and subsequent behavior based on the expectations of others and of society.

Furthermore, because most drug use is illegal, those individuals who use illicit drugs (as opposed to alcohol) are not only labeled a “drug addict” but also a “criminal.” The apparent criminality of drug users is only exacerbated by the overrepresentation of drug users behind bars and their high rates of recidivism, which communicate to the public that drug users are “bad” people who commit an overwhelming amount of crime. Once users are identified as addicts and criminals, this label can further isolate them from friends, family, and conventional society in general and subsequently drive them into peer groups with other criminals.

The self-fulfilling prophecy of drug use and crime

Labels convey predictions about criminality and other social behaviors which can become reflexive in that they serve to create the behavior in which they forecast. This phenomenon was coined the self-fulfilling prophecy by Robert Merton (1948) who defined it as a falsely held belief that leads an individual to behave in a certain way specifically because they expected themselves to do so. He specifies that the predicted behavior does not merely occur subsequent to the prediction, but strictly because the prediction was made. Twenty years before the works of Merton, W. I. Thomas proposed, “If men define situations as real, they are real in their consequences” (1928: 527). In other words, Thomas suggests that humans’ response to a given situation is shaped not only by objective circumstances, but also by what the situation means from their own perspective. More specifically, interactions are shaped by perceived status of those involved and the role expectations ascribed to that status (Diekmann, Tenbrunsel, & Galinsky, 2003; Merton, 1968; Oeser & Harary, 1962; Wilson, 1970).

The self-fulfilling prophecy holds that the label not only impacts how the individual is perceived by society as suggested by labeling theorists, but that it also it influences the individual's perception of their own identity (Hare, 1987). This self-perception then defines self-expectations which, in turn, can result in deviant behavior that may not otherwise have occurred (Buck, 1963; Jussim, 2012; Knutsson, 1977). Thus, the self-fulfilling prophecy asserts that this internalization of the stigma posed from society is indirectly related to continued drug use and offending. More importantly perhaps, the self-fulfilling prophecy informs explanations regarding how drug use translates into crime. In other words, drug users commit crime and continue to use drugs specifically because they are expected to be "addicted" to a drug (i.e., they cannot stop) and addicts are expected to be criminals.

The Relevance of the Drugs-Crime Connection

Drug use in America has been a concern for over a century and its implications for crime have been speculated about for just as long. Scholars from a variety of fields such as sociology, criminology, public health, law, economics, and medicine have commented on this relationship, often providing conflicting results regarding the nature of the drugs-crime link (Friedman, 1998; Gadosy, Williams, Cohen, & Harwood, 1980). The relationship is dynamic and complex, adapting as new drugs are discovered or developed and the popularity and acceptance of different drugs rise and fall. Other social conditions impact the prevalence of drug use and levels of crime, including factors such as the economy, public policy, and law enforcement (Becker & Murphy, 1988; Benson, Kim, Rasmussen, & Zuehlke., 1992; Sloboda, 2002). As a result, research has approached the issue from various perspectives and posited a variety of multifaceted

questions regarding its causal nature. Before exploring this literature, there are fundamental terms that must be defined and a number of necessary distinctions that must be made regarding the drugs and crime relationship.

Defining Drug Use

First and foremost, a drug is usually defined as any chemical substance that alters the structure or functioning of the body in some way, excluding those vitamins and nutrients considered to be related to normal functioning (US Food and Drug Administration [FDA], 2012). Generally when people think of “drugs,” the substances that come to mind are those that act on the brain to alter mood, perception, thoughts, and/or behavior in an obvious way such as cocaine, marijuana, or heroin. However, by the FDA’s definition, a wide variety of substances are considered drugs, including both the more widely recognized illegal drugs and also legal everyday substances such as aspirin or caffeine. Because aspirin and caffeine are legal substances that are commonplace in American homes, many forget that they are in fact drugs. However, the legality of a substance is not involved in determining its status as a drug but rather is used to distinguish licit and illicit drug use. On one hand, licit drug use refers to the consumption of legal substances such as alcohol (for adults over the age of 21), nicotine, caffeine, and other prescribed medications. On the other hand, illicit drug use includes the use of illegal substances and the nonmedical use of prescription and over-the-counter (OTC) medications and household substances (US Department of Health & Human Services, 2015).

Throughout much of the early 1900s, there were few restrictions on drug use and as a result, drugs like cocaine, heroin, and marijuana were frequently used by citizens

across the US. In fact, some of these drugs were considered “cure-alls” and were often advertised to address a variety of ailments for Americans of all ages. For example, cocaine was used to numb the pain of toothaches and morphine was marketed as a cough suppressant and a sleeping aid for colicky babies. As more Americans began to suffer the consequences of drug addiction, the government stepped in and implemented a series of laws over the next 80 years to regulate the consumption, manufacture, sales, and transport of nearly every drug on the market. The first of such laws was the Pure Food and Drugs Act of 1906 which prohibited interstate transport of illegitimate and debased foods, beverages, and drugs (PL 59-384). The new law brought drug regulation under federal jurisdiction and was enforced by the Bureau of Chemistry in the Department of Agriculture, which later evolved into what is now known as the FDA (Donohue, 2006; FDA, 2015; Meadows, 2006).

For as long as the US has been addressing drug-related issues, one of the key justifications for government action has been the need to address the problems of drug abuse and addiction (Kane, 1917). Indeed, the addictive nature of illicit drugs such as cocaine is often considered a primary pretext for its illegality. Nonetheless, a great many number of drugs have addictive qualities that can lead to substance abuse and/or dependence, including legal drugs such as alcohol and caffeine.

Substance dependence disorders. Until recently, the Diagnostic and Statistical Manual of Mental Disorders (DSM), the universal psychiatric classification and diagnostic system used by mental health professionals, differentiated between substance abuse and substance dependence. However, the fifth edition (DSM-V) that was released in May 2013 recognizes “substance use disorders” which collapses these two categories

to clarify confusion caused by the terms that were used somewhat interchangeably. Substance use disorders are characterized by an inability to control use, failure to fulfill school, work, and/or home obligations, risky using behaviors, and pharmacological dependence. Although they vary in severity (mild, moderate, or severe), substance use disorders, in general, impact users' thoughts and behaviors and impede their daily lives, affecting them personally, socially, professionally, and physically (American Psychiatric Association, 2013). According to the website of the National Institute of Drug Abuse (2014), addiction is defined as "a chronic, relapsing brain disease that is characterized by compulsive drug seeking and use, despite harmful consequences."

Considering the dilapidated state of many drug dependent users' lives and the extent to which they will go to satisfy their addiction, the connection between drug use and criminal behavior starts becoming more evident - many of the criteria used to determine substance use disorders are also recognized correlates of criminal behavior. Thus, addiction is often cited as a crucial driving force behind drug-related crime (Greenberg & Adler, 1974; Gropper, 1985; Inciardi, 1981; Mark, Woody, Juday, & Kleber, 2001).

Defining Drug Crimes

Given that a number of drugs are illegal in this country, using such substances is, by definition, a crime. It follows that many crimes committed by illicit drug users somehow involve their drug use. To be sure, this is not to say that all drugs are illegal or that all crimes involve drug use, but that many drugs are illegal and that crimes by drug users frequently involve illegal drugs. On one hand, drug-defined offenses are "violations of laws prohibiting or regulating the possession, use, distribution, or manufacture of

illegal drugs” (Craddock, Collins, & Timrots, 1994, p. 1). Thus, the type of offense committed is “defined” by the illegal status of the drug. The term “drug offenders” is used to refer to those who were convicted of a drug-defined offense. On the other hand, drug-related crimes are those property or violent offenses that occur as a result of the offender’s drug use. In this case, although the offense is not a violation of drug laws, the underlying cause is somehow related to drug use. It follows that in addition to drug-defined offenses (i.e., possession, distribution, etc.), certain types of crimes are more likely to be committed by drug users than non-users.

A recent meta-analysis of the research on the drugs-crime connection found that the odds of crimes such as shoplifting, prostitution, burglary, and robbery were around three to four times greater for drug users as opposed to non-users, (Bennett, Holloway, & Farrington, 2008). Without recognizing the property and/or violent offenses that occur directly as a result of substance use, estimations include only those violations of drug laws and likely minimize the drugs-crime link. Nonetheless, looking at the frequency of drug and alcohol arrests provides some insight into the prevalence of substance use. However, the amount of drug crime in the US is inherently dependent upon the prevalence of American drug use, an estimate that proves much more difficult to identify than would be ideal.

Measuring Drug Use

As is the case in measuring criminal behavior, evaluating the prevalence of drug use is complicated. Nearly all of what is known about drug use in the general US population is based on the findings of large-scale national surveys and the efforts of the Substance Abuse and Mental Health Services Administration (SAMHSA), a federal

agency within the US Department of Health and Human Services. The National Survey on Drug Use and Health (NSDUH) is one of the leading annual surveys regarding legal and illegal drug use in America and is funded by SAMHSA and supervised by SAMHSA's Center for Behavioral Health Statistics and Quality (CBHSQ) (NSDUH, 2016; SAMHSA, 2016). Unlike a number of other sources that estimate the prevalence of drug use among those involved with the criminal justice system, NSDUH gauges the tobacco, alcohol, and drug use of the general US population by systematically interviewing individuals from randomly selected American households. By utilizing a random sample, each selected individual is intended to represent more than 4,500 US residents (NSDUH, 2016).

To capture the portion of the population that currently uses drugs or alcohol, respondents are asked whether they had used a variety of different substances within the last month. Thus, NSDUH considers those who indicate they had used in the last month to be current users. Accordingly, in 2013, about 10% of the US population ages twelve or older identified themselves as current illicit drug users (SAMHSA, 2014). Over 80% of self-identified illicit drug users reported using marijuana, the most commonly used illicit drug in 2013 and nearly 65% of current illicit drug users reported that marijuana was the only substance used in the month prior to survey administration. In contrast, slightly more than 15% of current users reported using both marijuana and some other drugs and nearly 20% reported using only other illicit drugs but not marijuana. Other than marijuana, the most commonly used illicit drugs were psychotherapeutics (i.e., prescription medications) and cocaine, used by 6.5% and 1.5% of the total population of Americans over the age of twelve, respectively. Less than 1% of respondents reported

using heroin in the last month. In sum, marijuana is overwhelmingly the most commonly used illicit drug followed by prescription medications, while typical street drugs such as cocaine and heroin are used than less than 2% of the population (SAMHSA, 2014).

Generally, substance use is highest among Americans in late adolescence through early adulthood. Over 20% of NSDUH respondents ages 18 to 25 identified themselves as current illicit drug users, while less than 9% of those 26 and older reported current drug use (CBHSQ, 2016; SAMHSA, 2014). With regards to gender, the rate of current illicit drug use is higher among males than females, with about 10% of males reporting current illicit drug use and about 7% of females. Rates of current illicit drug use were relatively comparable between whites (9.5%) and blacks (10.5%), but the highest rates were among smaller minority groups. Over 12% of American Indians or Alaska Natives, 14% of Native Hawaiians or Other Pacific Islanders, and over 17% of individuals reporting more than one race were considered current illicit drug users. However, whites were considerably more likely to self-identify as a current alcohol user than other racial groups.

Unlike drugs such as marijuana, cocaine, and heroin, alcohol is legal under federal law for consumption by adults over the age of 21 and therefore is much more commonly used. Although marijuana is legal in some states, alcohol is unique for its nationwide legality, albeit not without age restrictions. Because of this, it is readily available and easily accessible. As a result, a greater number of Americans report using alcohol more than any other drug. For the last fifteen years, alcohol has been the most common substance used in the US such that more Americans report drinking than report not having consumed any alcohol (CBHSQ, 2016). To gauge levels of current alcohol

and drug use, respondents were asked whether they had drank alcohol and/or used drugs in the last 30 days. The 10% of Americans ages twelve and older who report current illicit drug¹ use pales in comparison to the approximate 52% of Americans that report consuming alcohol in the last month (SAMHSA, 2014). Indeed, some estimates of alcohol consumption suggest that more than 70% of Americans consume alcohol (Chartier & Caetano, 2009; Esser et al., 2014). Further, NSDUH findings suggest that over 20% of American adults reported binge drinking within the past month (SAMHSA, 2014; National Center for Health Statistics [NCHS], 2015). NSDUH defines binge drinking as the consumption of five or more alcoholic beverages on at least one occasion in the last month (SAMHSA, 2014). However, it is generally accepted that definitions of binge drinking are dependent upon gender, identifying five or more drinks for men and four or more for women (CBHSQ, 2016; Center for Disease Control [CDC], 2015; Dwyer-Lindgren et al., 2015; National Institute on Alcohol Abuse and Alcoholism [NIAAA], 2015; United Health Foundation, 2016). Using these definitions, results from NSDUH suggest that approximately 1 in 4 people over the age of 12 identify themselves as current binge drinkers (CBHSQ, 2016).

Because alcohol is illegal for those under the age of 21, rates of consumption are highest among those in their twenties. According to NSDUH findings, nearly 60% of respondents between ages 18 and 25 and more than 55% of adults aged 26 or older were current alcohol users. Further, nearly 40% of young adults aged 18 to 25 and about 25% of adults aged 26 and older report binge drinking in the previous month. In other words, about 1 in 3 adults ages 18 and older currently consume four or five alcoholic beverages

¹ Specifically, this includes marijuana, cocaine, heroin, hallucinogens, inhalants, or non-medical use of prescription medications

(depending on sex) on one occasion. Although rates of alcohol use are similar for both genders, males report higher levels of both recent alcohol consumption and binge drinking (44.4%) than did females (47.5% and 31.4%, respectively). Other studies have suggested that as many as 75% of males are current drinkers and more than 65% of women report drinking in the past year (Chartier & Caetano, 2009; Esser et al., 2014). Current alcohol use is more common among whites than other racial groups with estimates of current drinking among white adults ranging from about 57% to nearly 70%. Rates of current alcohol use in black adults is around 50% and about 45% in other racial groups (Hispanic, Asian, Native American, Hawaiian/Pacific Islander, etc.) (Chartier & Caetano, 2014; SAMHSA, 2014). Binge drinking follows a similar pattern, although it is more common among “other” racial groups than blacks. Rates of current binge drinking are estimated at about 7% for whites, 4.5% for blacks, and about 6% for other racial minorities.

According to NSDUH data, over 8% of the population over the age of 12 can be classified as having what is now understood as a substance dependence disorder² (Esser et al., 2014; SAMHSA, 2014), nearly 95% of which are believed to be unaware of this dependence (Healthy People, 2015). Almost 70% of all those with a substance dependence disorder are classified as having an alcohol-only dependence disorder. In contrast, approximately 12% of those with a substance dependence disorder are classified as having both alcohol and drug substance disorders, while about 20% struggle only with illicit drugs (CBHSQ, 2016; SAMHSA, 2014).

² Recall that the DSM-V collapsed substance “abuse” and substance “dependence” previously defined in the DSM-IV. Thus, those individuals who were classified as having a problem with substance abuse and/or dependence are considered to have a substance use disorder according to the DSM-V.

Evaluating the Drug-Crime Connection

Self-report data and official records clearly demonstrate an association between drugs and crime. The extant literature suggests that not only does a correlation exist between drugs and crime, but that drug use is associated with increased offending for both males and females (Holloway & Bennett, 2004; Popovici, Homer, Fang, & French, 2012; French et al., 2000; French, Roebuck, & Alexandre, 2001), regardless of the type of drug used by the offender (Bennett, Holloway, & Farrington, 2008; Pedersen & Skardhamar, 2009). This is not to say that the drug-crime relationship looks identical for males and females across all drug types, but rather that using almost any type of illicit drug (including alcohol, marijuana, amphetamine, heroin and other opiates, benzodiazepines, cocaine, and crack cocaine³) is associated with higher odds of committing crime for both genders (Albrecht et al., 2014; Bennett, Holloway, & Farrington, 2008; Comiskey, Stapleton, & Kelly, 2012; Dawkins, Pedersen & Skardhamar, 2009).

A recent meta-analysis of the research on the drugs-crime connection found that the odds of predatory crime including shoplifting, prostitution, burglary, and robbery were greater among drug users as opposed to non-users, with heroin, crack, and cocaine users at the greatest odds of committing such offenses (Bennett, Holloway, & Farrington, 2008). Indeed, the extant literature indicates that not only does a correlation exist between drugs and crime, but drug use is associated with increased offending across gender (French et al., 2000; Holloway & Bennett, 2004; Popovici et al., 2012), drug type (Bennett, Holloway, & Farrington, 2008; Pedersen & Skardhamar, 2009), and crime type

³ MDMA, or 3,4-methylenedioxy-methamphetamine also known as ecstasy, appears to be the only drug that research has not reached a consensus regarding its relationship with offending behavior (Estevez & Emler, 2011; Hendrickson & Gerstein, 2005; Payne & Gaffney, 2012).

(Bennett, Holloway, & Farrington, 2008; Phillips, 2012), with some variation by location (White & Gorman, 2000). Furthermore, substance use is associated with higher levels of violent offending (Carpenter & Dobkin, 2009; Phillips, 2012; Cohen et al., 1994), criminal victimization (Popovici et al., 2012), and recidivism (Spohn & Holleran, 2006).

Measuring Drug Crime

In 2013, approximately 1.5 million Americans were arrested for drug law violations, comprising just over 13% of the total estimated arrests. Of those 1.5 million people, 80% were charged with drug possession, while about 17% were charged with drug manufacturing or sales (Federal Bureau of Investigation, 2014). The same year, over 1.1 million people were arrested for driving under the influence and another 800,000 were arrested for drunkenness or violations of liquor laws. In sum, over 3.5 million Americans were arrested for drug or alcohol offenses during 2013. The total number of drug-related offenses is much more difficult to estimate for a number of reasons. The primary issue is that determining if drug use is relevant to a crime often relies solely upon the offender to self-report their use unless a blood or urine test is administered upon arrest. The specific issues related to self-reported drug use will be discussed in the next section, but there is reason to question the reliability of what information offenders provide about their drug use.

The most recent, readily available estimations regarding drug-related offenses were gathered and calculated by the Bureau of Justice Statistics (BJS) using drug use and dependence information from state and federal prisoners in 2004. About 17% of state inmates and 18% of federal inmates reported that the offense that led to their incarceration was committed with the intent of obtaining money for drugs (BJS, 2004).

By the same token, the 2004 Survey of Inmates in State and Federal Correctional Facilities indicates that approximately 32% of state inmates and 26% of federal inmates reported that they were under the influence of a substance at the time they committed the offense (Mumola & Karberg, 2006). Because there is no way to be certain that these groups are mutually exclusive, there could be a considerable amount of overlap between these two types of drug-related offenders, or it is possible there is none at all. Consequently, the estimated number of inmates who committed drug-related offenses (i.e., motivated by money for drugs and/or were intoxicated at the time of the offense) may be as few as 32% of state inmates and 26% of federal inmates, or as great as nearly 50% of state prisoners and 44% of federal inmates. During this same year, drug offenders (i.e., violated laws prohibiting illicit drug use) made up about 21% of state prisoners and 55% of federal prisoners. However, approximately a quarter of these state and federal drug offenders were the same individuals who reported committing the offense either while on drugs or in order to obtain more drugs. Thus, it is difficult to pinpoint exactly how many state and federal inmates were serving time for offenses associated with drugs. This difficulty is only further complicated when considering crimes related to systemic crime. Specifically, most data sources classify homicides committed during a drug felony such as trafficking or manufacturing as drug-related. According to the Uniform Crime Reports (UCR), approximately 4% of the 14,000 homicides committed in 2004 are considered drug related (about 3,600 homicides) (Dorsey, Zawitz, & Middleton, 2008).

Measurement Concerns Regarding Substance Use and Crime

In measuring drug use and crime, the operationalization of terms is generally dependent upon the data source. As a result, different studies examining the same issue

may produce seemingly inconsistent results. Thus, it is always prudent to understand the source of the data and how it defines “use” and “crime.” In general, official records and databases measure offending in terms of arrest, conviction, and/or incarceration. This is because official data is only equipped to provide information about drug use and crimes that are identified and documented by law enforcement. Consequently, official data fails to account for the large amount of American drug use and related crime that never comes to the attention of the police or who were caught but only given a warning. Unlike official data, large national surveys ask individuals about their drug use and offending behavior regardless of whether or not they have been arrested. However, self-report data carries limitations of its own.

The primary concern regarding self-report data is that it may be biased depending on respondents’ honesty. In reporting whether or not they use illicit drug use, respondents are confessing to violating the law. This can create some unease or discomfort which can sway some people from telling the truth. Some respondents underestimate their substance use purposefully, to present themselves in a more positive light, while others overestimate their substance use to appear more “cool” and/or dangerous (Matt et al., 2003). However, some research suggests that self-reported drug use is moderately accurate but said accuracy is dependent on a number of individual characteristics including race, type of drug use, and history of arrests (Peters, Kremling, & Hung, 2015; Solbergdottir et al., 2004). In addition, humans can fall victim to their memory in that they simply may not recall every time they used. This is often exacerbated by the physiological effects of drugs that damage both short- and long-term memory (Lundqvist, 2005; Ornstein et al., 2000; Robbins, Ersche, & Everitt, 2008; Rosselli & Ardila, 1996).

Typically, surveys try to avoid this by asking about use in the last month or within a designated time period, rather than asking a respondent to recall every time they have used any substance over an indefinite period of time (Gaskell, Wright, & O'Muircheartaigh, 2000).

Issues in estimating the prevalence of “drug-related” crime. In general, the data suggests that drug offenses are more often drug-defined than drug-related – that is, more drug offenders break drug laws than commit some other type of crime (i.e., property or violent). However, there are a number of methodological concerns involved in estimating the amount of crime that is drug-related. First, most crimes are influenced by a multitude of factors and thus it is rarely the case that drugs are the only motivation for the offense. For example, a substance user who has lost their job as a result of their abuse may then resort to robbery to obtain money to purchase more drugs. Although the motivation centers on getting drugs, the offense is also impacted by the loss of employment and the lack of income. Thus, determining if the crime is “drug-related” is influenced by all of the other factors involved in the offense and it is often difficult to determine the relevance of drugs. This leads to the second methodological issue which is concerned with how “drug-related” is defined. Some research relies on a very narrow definition of drug-related while others define it much more broadly and consider the “mere presence” of drugs to be relevant to the offense (Craddock, Collins, & Timrots, 1994). Although the presence of drugs is not sufficient to charge an individual with possession or another drug offense, it may change the nature of the crime or may even have been the sole motivation for the offense committed. At the same time, it may be that the drugs are simply present and the offender(s) may not even have had knowledge of this. In that case, there are drugs

at the scene of an offense, but they are in no way connected to the incident that occurred. Depending on how “drug-related” is conceptualized, the frequency of drug-related crime will vary greatly.

Relatedly, how the term “drug” is defined also shapes estimates of prevalence. As noted earlier, most research does not categorize alcohol offenses with drug offenses. However, given that alcohol is in fact a drug and research supports its relevance for offending behavior, it is foolish to not consider these offenses when estimating the prevalence of drug and drug-related offenses. In addition, the prescription medication abuse further complicates the issue, especially given that nearly half of Americans used at least one prescription drug in the last month, including drugs with a high risk of dependence such as oxycodone (similar to morphine⁴) or Adderall (similar to amphetamines) (Drug Enforcement Administration, 2015; NCHS, 2015). Any number of cases may have occurred after an offender had taken a prescription medication, but whether it was related to that offender’s intoxication from that drug often remains unclear. Even if it is identified that the effects of the prescription drug led to the offense, there are questions related to whether that drug was prescribed by a doctor or if the user was abusing their prescription. If they were taking it as properly prescribed but it still produced a pharmacological effect that resulted in offending behavior, is that still a drug-related offense? Consequently, many sources do not recognize a crime as drug-related if the offender was under the influence of prescription drugs. Consider also those offenders who robbed a pharmacy to obtain prescription drugs – clearly a drug-related crime.

⁴ Oxycodone is an opioid drug that blocks the pain pathway to the brain thus reducing feelings of pain. It acts on the same parts of the brain as other opioids, including morphine and heroin. The molecular structures of oxycodone and heroin are nearly identical (Advocates, 2015). Considering only the psychopharmacological effects, the abuse potential of prescription opioids is equivalent to heroin (Cromer et al., 2008).

Although the DEA has approximated the frequency of such robberies, it is unclear if these are counted in estimations of the total number of drug-related crimes. A final challenge in measuring the amount of drug-related crime results from relying upon the word of those involved to determine if the offender was under the influence of a substance or trying to get money to buy drugs at the time of the offense. Unless the offender was given a blood or urine test, the primary means of determining if an offense was drug-related is whether or not the offender reports their own use.

Implications of the Relationship between Drug Use and Crime

Substance use and dependence among these millions of Americans translate into approximately \$600 billion in terms of productivity, health, treatment, and crime-related costs for the US (FBI, 2014; NIDA, 2012). According to the CDC (2014), excessive alcohol consumption alone costs more than \$160 billion in terms of lost work productivity, over \$24 billion in healthcare expenses, and approximately \$20 billion in criminal justice and law enforcement expenditures. Nearly 75% of these costs are related to binge drinking, the most common type of excessive drinking in the US. This total does not include social costs that cannot be monetarily defined such as child abuse/neglect, family disintegration, unemployment, and/or failures in educational attainment. The nearly \$200 billion spent responding to illicit drugs and the approximate \$235 billion spent addressing alcohol abuse does not reflect the damage done to families and friends whose loved ones' lives are consumed and, in some cases, eventually ended by substance-related problems (National Drug Intelligence Center [NDIC], 2011).

The mounting issues associated with substance use become even more evident when considering the prison population. According to the Federal Bureau of Prisons

(2015), nearly one-half of federal inmates were convicted of felony drug offenses such as trafficking or manufacturing. Although drug offenders comprise a smaller proportion of state prison inmates (Carson & Golinelli, 2013), they have consistently accounted for approximately one-half of the federal inmate population for over three decades (Beck & Harrison, 2001; Carson & Sabol, 2012). Because more than 75% of drug offenders recidivate within three years of release from prison (Cooper, Durose, & Synder, 2014; Langan & Levin, 2002), this large population of drug offenders behind bars is essentially self-sustaining, as former inmates cycle into and out of prison and new drug offenders enter the system. This is evinced in a study of over 1,000 convicted felons which found that incarcerated drug offenders were more likely to reoffend than other types of incarcerated offenders and did so within a shorter period of time after their release (Spohn & Holleran, 2002). Given the mounting concern regarding the size of the prison population, a great deal of attention has centered on the role drugs play in criminal behavior and what can be done to better enable drug users' recovery and reduce the likelihood they will offend again in the future.

The Relevance of the Employment-Crime Connection

In response to the accumulation of drug offenders behind bars, there has been an effort to determine not which factors are related to crime, but rather those associated with reduced recidivism. One manifestation of this focus on reducing recidivism among drug offenders has been the increasing prominence of inmate reentry programs. By targeting known correlates of reoffending, reentry programs are designed to facilitate inmates' reintegration back into conventional society and reduce the likelihood of subsequent offending. Employment has been both theoretically and empirically identified as a

consistent correlate of successful reentry, providing a source of income as well as ties to conventional society (Bushway, 2011; Lipsey, 1995; Pager, 2006; Travis, 2005; Uggen, 2000). Because it is more easily manipulated than other social factors such as marriage or parental status, criminologists and policymakers have focused on employment as a reentry strategy. Thus, many programs now include an employment component that tries to assist offenders in finding work after their release with the intent of reducing further offending (Uggen & Wakefield, 2008). This is rooted in the premise that changes in employment status are related to fluctuations in criminal behavior. Much research examining macro-level data supports this claim, pointing to the changes in crime rate corresponding to changes in the unemployment rate (Buonanno, Drago, & Galbiati, 2014; Cantor & Land, 1985; Chiricos, 1987; Gould, Weinberg, & Mustard, 2000; Greenberg, 2001; Hsieh & Pugh, 1993; Raphael & Winter-Ebmer, 2001). However, it is also contended that concentrations of poverty and homelessness have a criminogenic effect on an area and thus the crime rate is dependent on aggregate-level unemployment rates (Chiricos, 1987; Sampson, 1987; Sampson & Wilson, 1995; Wilson, 1987). In contrast, other studies suggest that both unemployment and crime are related to processes of selection – that is, some exogenous factor that increases the likelihood of unemployment also increases the likelihood of offending (Caspi et al., 1998; Gottfredson & Hirschi, 1990; Heckman, Stixrud, & Urzua, 2006). Nonetheless, the macro-level employment-crime connection appears relevant, but this relationship at the individual level has proven a bit more complex.

As addressed in the first section, nearly all of the classic criminological theories address the employment-crime relationship and generally agree that work should reduce

criminal behavior both directly, via informal social controls created by work, and indirectly, as a result of increased legitimate earnings (Agnew, 2001; Cohen & Felson, 1979, Laub & Sampson, 2003; Merton, 1938). There may also be gradual effects of employment such that holding down a job increases social capital over time, which strengthens the bond to conventional society (Laub & Sampson, 2003) and weakens identification with the criminal lifestyle (Maruna, 2001). As with macro-level explanations of crime, individual level theories usually either maintain that unemployment causes crime or that both unemployment and crime are caused by the same outside forces (Uggen & Wakefield, 2008; Wright, Cullen, Williams, 2002). Each theoretical framework centers on different primary factors that drive the relationship, but in general they all agree that employment should be accompanied by lower levels of offending.

Evaluating the Employment and Offending Connection

Empirical research generally supports theoretical claims regarding the protective effects of employment, albeit with mixed evidence. Although most research on the employment-crime link utilizes cross-sectional data and thus fails to provide causal information, studies that approach the relationship longitudinally provide inconsistent results (Cantor & Land, 1985, 1991; Bushway & Reuter, 1997; Uggen & Wakefield, 2008; Wright, Cullen, & Williams, 2002). Utilizing the classic Glueck and Glueck (1950) data which followed 1,000 subjects for eighteen years, the findings of Sampson and Laub (1993) suggest that job instability increases the likelihood of criminal behavior. Their research emphasizes the importance of social bonds to family and employment during the

transition to adulthood, but their concentration on late adolescence and early adulthood neglects the work-crime relationship for younger teenagers.

Consistent with the expectations of life course theorists, the work-crime relationship appears to vary across the life span. The impact of employment on adolescent offending is less consistent than among adults, and in general, runs inconsistent with theoretical expectations. Although some research indicates a protective effect of adolescent employment (Fergusson, Horwood, & Woodward, 2001), other studies suggest the opposite. From a strain perspective, a criminogenic effect of employment is counterintuitive. Working should provide adolescents a legitimate means to obtain the culturally valued financial resources and material goods, thus alleviating strain and reducing delinquency (Agnew, 1992). However, it appears that the effect of employment depends on a number of factors including characteristics of the job such as intensity (usually measured as 20 or more hours per week) (Apel, Paternoster, Bushway, & Brame, 2006; Bachman & Schulenberg, 1993; Staff & Uggen, 2003; Wright et al., 2002) and quality (Bellair, Roscigno, & McNulty, 2003; Steinberg Fegley, & Dornbusch, 1993; Uggen, 1999; Van der Geest, Bijleveld, & Blokland, 2011; Wadsworth, 2006). For example, the findings of Osilla et al. (2013, 2015) suggest higher levels of job intensity increase exposure to social networks and work environments that are conducive to risky behaviors. As a result, intense work can be criminogenic for adolescents. Further, research also indicates that low quality jobs are associated with higher levels of arrest than jobs that provide adequate hours and income (Allan & Steffensmeier, 1989).

Empirical research also indicates that perhaps some outside force is responsible for selection into both intensive work and delinquent behavior (Paternoster et al., 2003).

Other research has provided similar results suggesting that adolescents who are employed may be different from their unemployed counterparts in ways that existed prior to starting work (Apel et al., 2007; Brame, Bushway, Paternoster, & Apel, 2004; Staff & Uggen, 2003). However, research does seem to indicate that increased intensity of work during adolescence is associated with a greater likelihood of delinquency despite any selection processes (Bachman & Schulenberg, 1993; McMorris & Uggen, 2000; Mortimer, 2003, 2010; National Research Council, 1998). Nonetheless, the general consensus remains that employment has a protective effect on crime, specifically for adults. However, not all types of work reduce criminal behavior for every type of offender (Allan & Steffensmeier, 1989; Blokland & Nieuwebeerta, 2005).

Measurement Concerns Regarding Employment and Offending

Consistent with measuring drug use and criminal offending, there are a variety of methodological issues faced in measuring employment and its relationship to crime. Unlike drug use however, employment tends to be a much less private facet of people's lives and thus it is easier to access information about job status than about their drug use. Furthermore, it is much more difficult to lie about legal employment because there is usually some type of documentation as well as employers and/or coworkers that can confirm or deny the reported employment. Nonetheless, measuring employment is not without its challenges.

First and foremost, employment can mean any given number of things to different people. Without setting parameters, employment could include everything from babysitting the neighbor's children two nights a year, to selling Christmas trees during the month of December, to working full-time (40 hours per week) at an office job.

Technically, these are all forms of employment, but in practice, they are extremely different types of work. The more unstable jobs like babysitting are considered part of what economists refer to as the secondary labor market (Reich, Gordon, & Edwards, 1973; Wachter, 1974). Unlike the primary labor market which includes stable, high-wage jobs, the secondary labor market experiences high turnover and offers few, if any, opportunities for promotion (Reich, Gordon, & Edwards, 1973). There is very little mobility between these sectors due to institutional barriers and an insufficient amount of “good”, primary sector jobs (Wachter, 1974). Accordingly, individuals who are confined to the secondary labor market experience high levels of job instability and spend longer periods of time out of the labor market (Crutchfield, Wadsworth, Groninger, & Drakulich, 2006). Research suggests that expectations of job permanence and time spent in between jobs are essential aspects of employment in shaping its effect on subsequent offending (Crutchfield & Pitchford, 1997). Thus, simply having a job does not necessarily have a protective effect on crime. The quality of the job and its labor market sector (i.e., whether it is primary or secondary) matter in terms of consequences for offending. A simple dichotomous variable indicating whether an individual worked within the last year provides relatively little information about the quality and/or length of employment. Consequently, it is essential to understand exactly how employment is operationalized when considering its relationship to offending.

In particular, the extant literature suggests that variations in job stability and job quality are related to differential changes in criminal behavior (Johnson, 2004; Osilla, Miles, Hunter, & Damico, 2015). This suggests that these aspects of employment should be considered when evaluating its relevance to offending. Furthermore, some studies that

are often grouped into the employment and crime literature specifically examine the effects of *income* on offending (French, Zarkin, & Dunlap, 1998; Kaestner, 1991; Kandel, Chen, & Gill, 1995; Pettit & Lyons, 2009; Staff & Uggen, 2003). Indeed, these studies inform the employment-crime link, but the conceptualization of income as opposed to employment will likely produce substantively different results. Furthermore, the reciprocal effects of employment often confound our understanding of its impact on crime.

The Reciprocal Relationship between Work and Crime – How Crime Reduces Future Employment Opportunities

Despite the general consensus that employment facilitates reduced recidivism, individuals released from prison face barriers to employment opportunities as a result of their status as a criminal. Research consistently identifies a strong negative effect of incarceration on employment and wages (Pager, 2003; Nagin & Waldfogel, 1998; Pager, Western, & Sugie, 2009; Pager, Western & Bonikowski, 2009; Pettit & Lyons, 2007; Western & Beckett, 1999). However, the work of Nagin & Waldfogel (1995) suggests that conviction actually increases wages, but their later research indicates that this relationship varies across the life cycle. Specifically, conviction leads to increases in income for individuals under the age of 25, but also results in significant decreases in earning wages for ex-offenders over 30 years old. With the exception of this brief uptick in income for younger individuals, incarceration diminishes future employment and income (Nagin & Waldfogel, 1998).

Traditionally, the primary method by which this relationship was studied was through longitudinal data that followed the employment experiences of individuals released from prison. However, survey data does not permit isolation or clear

examination of the mechanisms underlying the relationship. In response, Pager (2003) expanded the audit methodology first developed by Schwartz and Skolnick (1962). The audit methodology is a field experiment in which matched pairs of individuals, called testers, apply for real jobs using applications formulated to test employers' responses to certain characteristics. Schwartz and Skolnick (1962) mailed applications to real employers looking to hire and found that applicants with criminal records were less likely to be considered by employers than those with no criminal background. Rather than sending in applications by mail, Pager (2003) pioneered the "experimental audit design" (p. 943) where testers applied for job openings in person to try to isolate the effect of criminal record while observing employers in their actual employment setting.⁵

This movement into experimental audit designs was centered on employer perceptions of applicants' criminal records and has spawned deeper exploration into employer attitudes toward hiring ex-offenders. Using surveys and in-depth interviews, research on employer attitudes found that a majority of employers would not knowingly hire an applicant with a criminal record (Holzer, 1999) and that employers were twice as willing to hire other low status groups such as welfare recipients than ex-offenders (Holzer, Raphael, & Stoll, 2003). Furthermore, even when employers reported a willingness to hire applicants with a criminal record, they were no more likely to actually hire them than their less willing counterparts (Pager & Quillian, 2005). As Pager (2006) points out, "Employers bear the burden of theft and violence in the workplace, as well as the more mundane problems of unreliable staff and employee turnover. With respect to each of these concerns, a criminal record is arguably a relevant signal" (p. 505).

⁵ See Pager (2007) for a thorough discussion of the audit experimental design, summary of findings from a number of audit experiments, and critiques of the methodology.

Accordingly, the relationship between past and future criminal hinders ex-offenders abilities to gain employment with some good reason; however, the work of Kurlychek, Brame, and Bushway (2006; 2007) and Blumstein and Nakamura (2009) suggests that perhaps this reasoning is based on flawed grounds. The results of these studies indicate that while past criminal history is positively associated with future criminality, this relationship decreases immensely over time.

According to Kurlychek, Brame, and Bushway (2006), the likelihood of future criminal behavior for those with offending histories decreases for approximately six or seven years post-arrest. In fact, they replicated these same results with different data, again finding support for the notion that the impact of past criminal behavior on future offending shrinks with time (Kurlychek, Brame, & Bushway, 2007). However, it is important to note that offending levels for those with criminal histories never become completely indistinguishable from the offending rates of the general population. That is, although the risk of offending dramatically declines during the six to seven years after arrest, individuals with criminal records remain slightly more likely to commit crimes than those who have never been arrested. Correspondingly, Blumstein and Nakamura (2009) also found a consistent decrease over time that leaves those with prior criminal histories with likelihoods of future offending about the same as those individuals their same age who do not have criminal records. Their contribution was to recognize that the age at offense and the crime type is related to the length of time necessary to become similar to those of the same age without a criminal record. Those whose offense occurred at a younger age and those with violence in their past require a longer time to become “close enough” to the general population (Blumstein & Nakamura, 2009, p.350).

Nonetheless, the existing empirical research suggests that the association between criminal history and future criminal behavior decreases over time until eventually the likelihood of offending is similar for those with and without a criminal record. This suggests that the risk involved in hiring applicants with criminal records decreases over time and thus, employers should be more open to hiring individuals whose criminal offense is far in their past. However, as discussed above, the research continues to indicate that individuals with a criminal record, no matter how recent or how long ago the offense occurred, experience significant decreases in employment and income. Thus, this challenges our understanding of the inverse relationship between employment and crime. Not only does employment seem to decrease offending, but also it appears that prior offending plays a substantive and powerful role in shaping future employment opportunities.

The Relationship between Employment and Drug Use

In addition to identifying both drug use and employment as correlates of offending, extant research also suggests that patterns of substance use may be related to employment. Results from the 2013 NSDUH suggest that illicit drug use varies in accordance with employment status (SAMHSA, 2014). The rate of current illicit drug use among the unemployed was notably larger than the rate among those with full-time and/or part-time employment. In fact, the rate of current illicit drug use for the unemployed was twice that of the rate of those who were employed full-time. Although this suggests an inverse correlation between employment and illicit drug use, the opposite is seen in trends of current alcohol use by differences in employment status. While about half of unemployed adult reported current alcohol use, nearly two-thirds of employed

adults reported current alcohol use. Rates of binge drinking were relatively consistent regardless of employment status (SAMHSA, 2014). Based on prevalence alone, it appears that although binge drinking is not related to employment status, illicit drug use is associated with lower levels of employment and alcohol use is associated with greater levels of employment.

A recent systematic review of the literature regarding drug use and employment provides insight into the key aspects of this relationship (Henkel, 2011). In regards to prevalence, Henkel (2011) found that employment was associated with rates of substance abuse. Specifically, unemployed individuals were more likely to drink excessive amounts of alcohol and abuse illicit and prescription drugs than were the employed. Since the publication of Henkel's extensive review, new and additional research has been conducted that concurs with this assertion. The work of Ionna Popovici and Michael French (sometimes in corroboration with others) has contributed greatly to our understanding of the connection between employment and substance use (French, Maclean, Sindelar, & Fang., 2011; Popovici et al., 2012; Popovici & French, 2013; Popovici & French, 2014; Popovici et al., 2014). They have primarily focused on the effects of alcohol, and found that alcohol is positively correlated with both the commission of crimes and criminal victimization (Popovici et al., 2012). Their subsequent work examined the effects of unemployment on alcohol consumption finding positive and significant effects of unemployment on drinking behaviors (Popovici & French, 2013). More recently, they have considered the effects of marijuana and contend that although it is related to antisocial behaviors in young adults, it does not appear to have the detrimental effects to

the labor market as previously anticipated (Popovici & French, 2014; Popovici et al., 2014).

Although research consistently identifies distinctions in drug use and criminal behavior based on employment status, it appears that the impact of employment varies by gender. For example, Bray et al. (2000) found that substance use and dependence in men is associated with decreased likelihood of being employed full-time, fewer weeks worked in the last year, and fewer hours worked in the last 30 days. In contrast, the effect of substance use and dependence on females' labor market behavior is less clear, with some aspects of employment affected by drug use but not others (i.e., number of weeks worked but not likelihood of full-time employment). Nonetheless, the general consensus is that the prevalence of drug use is greater among the unemployed than the employed, and thus, Henkel (2011) draws the conclusion that the need for substance prevention and treatment will be greater among the unemployed.

Although the evidence supports the inverse association between drug use and employment, research emphasizes important distinctions between types of users. Utilizing longitudinal cohort data from a large representative sample of injection drug users in Vancouver, Canada, Richardson, Wood, Li, and Kerr (2010) find that being male is positively associated with employment while drug users who are older, use crack and/or heroin daily, inject publicly, have unstable housing, or were recently incarcerated are less likely to be employed. In addition, the work of French, Roebuck, & Alexandre (2001) indicates that the relationship between employment and crime is related to the severity of drug use (i.e., chronic vs. non-chronic use). Accordingly, although chronic drug use was negatively related to employment for both males and females, their study

provides no evidence to support the idea that non-chronic drug use is significantly related to employment. Henkel's (2011) systematic review supports their findings and suggests that severe forms of substance abuse (heavy drinking, hard drug use, dependence, etc.) impact the likelihood of employment regardless of substance type. In contrast, the extant research appears in disagreement regarding the relationship between less severe substance use and employment.

Importantly, both adolescents and adults may be impacted by selection processes in each of the links between drugs and crime, drugs and employment, and employment and crime (Bachman & Schulenberg, 1993; Mortimer, 2010). Certain individual characteristics may influence their likelihood to use drugs, maintain employment, and/or be involved in crime. For example, considering the selection processes involved in finding a job, certain qualities that exist prior to employment may play a role in determining whether or not individuals will be employed (i.e., "selected" into the employment group) (Staff, Messersmith, & Schulenberg, 2009). The same factors that shape the likelihood of employment may also be related to the likelihood of committing a crime or using drugs (Paternoster et al., 2003; Sampson & Laub, 1993). For example, Gottfredson and Hirschi (1990) argue that low levels of self-control are responsible for both criminal involvement as well as employment instability. Mortimer (2010) supports the role of self-selection in the relationship between employment and delinquency among adolescents. His findings suggest that employment patterns among high school students are related to patterns of socioeconomic status and interest in their education. Specifically, students who worked more than 20 hours per week for an extended period of time had "lower educational aspirations and were less engaged in school" (Mortimer,

2010, p. 4). However, extant research indicates that job loss and unemployment are in fact risk factors for subsequent drug use and dependence (Henkel, 2011). Thus, although some selection processes may be present, employment status does appear relevant to the higher prevalence rate of drug use among those without work.

The Missing Pieces in the Substance Use, Employment, and Offending Puzzle

As evinced in the previous section's discussion of the theoretical links between substance use, employment, and offending, the relationship is complex and multidirectional. Each factor appears to directly affect the others: 1) drug use serves directly to increase criminality and reduce employment, 2) employment reduces drug use and offending, and, at the same time, 3) offending impacts the likelihood of drug use and employment. Both theoretical arguments and empirical evidence support this reciprocal relationship among these variables. What is left unknown, however, is how (if at all) each *indirectly* impacts offending via its effects on the other. More specifically, it is unclear how, on one hand, drug use may impact the employment-crime connection and on the other, employment may affect the substance use-crime connection.

Although many scholars from various disciplines have heavily researched these two relationships individually, there is an absence of scholarly work connecting them together. To be sure, this is not to say that research has failed to recognize their relevance to each other, but rather that there is a need to further explore how employment and substance use may interact in their effects on crime and to better understand the potential mediation occurring between employment, drug use, and offending. Thus, the present study considers the moderating effects of employment on the relationship between substance use and offending as well as the potential mediation of both

employment and drug use. Should empirical exploration of this issue support a protective effect of work, then employment could be a vehicle to reduce drug-related crime.

The Current Study

As noted above, the primary goal of the current study is to address the gap in the literature regarding the convoluted relationships between substance use, employment, and offending. To do so, this study focuses specifically on alcohol consumption and how it relates to employment and crime. Because this study is concerned with the consequences of substance use generally, alcohol serves as a suitable proxy by which the impact of substance use can be examined. Because alcohol is the most commonly used substance, any policy implications from studying alcohol will impact more people than would examining the effects of marijuana or cocaine and other hard drugs. Recall that for the last 15 years over half of the American population aged 12 and older report current drinking (i.e., they have consumed alcohol in the last 30 days). Thus, focusing on alcohol offers a greater number of cases to examine and can provide insight into a much more large-scale societal problem.

Another advantage to examining alcohol in relation to employment and offending is that rates of alcohol consumption are *higher* for those who are employed than those who are unemployed (SAMHSA, 2014). Specifically, in 2013, more than 65% of adults employed full-time were current drinkers while less than 54% of unemployed adults were current alcohol users. However, rates of binge drinking remain about the same regardless of employment status, with about 31% of adults reporting binge drinking behavior (National Council on Alcoholism and Drug Dependence [NCADD], 2015). It is important to note that of the approximate 58.5 million adults who were binge drinkers,

more than 75% had either part-time or full-time employment (about 44.5 million adults). The elevated rate of drinking among the employed may be a result of a number of factors. In contrast to the consequences for a failed drug test, alcohol consumption itself is generally not grounds for termination from employment. Although drinking before coming to work and/or while on the clock will likely lead to being fired, drinking after work or on a day off will not (that is unless heavy drinking the night before results in a hangover which affects work quality the next day) (NCADD, 2015). It follows that it will be easier to remain employed and continue drinking behaviors than to both work a job and maintain an addiction to more serious substances like cocaine or heroin. As a result, examining alcohol in the context of the relationships between substance use, employment and crime first broadens the amount of people identified as experiencing substance use and second, provides a greater number of people experiencing both key variables of interest (i.e., substance use and employment). However, centering analyses on alcohol is not without its limitations. Primarily, using alcohol as opposed to illicit drugs like marijuana or cocaine limits the ability of findings to appropriately apply to illicit substance use. Nonetheless, studying alcohol in this way can provide the foundation for future research regarding the drugs-work-crime link. Chapter 3 outlines the data used to examine these relationships, the variables used to measure key concepts, and the analytic strategy adopted to evaluate the potential mediation and moderation of the effects of both alcohol use and employment on offending.

CHAPTER 3

DATA AND METHODS

The following chapter presents the key aspects of the data and methods used in the current study. The first section introduces the National Longitudinal Survey program and provides information on the 1997 National Longitudinal Survey of Youth (NLSY97), the dataset used here. Next, the chapter explains sampling procedures, outlines survey administration and provides a description of participants. The third section presents the measures used in the current study and explains the survey questions used to operationalize the relevant theoretical constructs such as drug use, employment, and offending. Lastly, the chapter presents the analytic strategy implemented in this study.

The 1997 National Longitudinal Survey of Youth Data

In order to approach the complex relationship between drug use, employment, and crime, the current study uses data collected as part of the 1997 National Longitudinal Survey of Youth (NLSY97). The NLSY97 is the most recent survey of the National Longitudinal Surveys (NLS) program, a set of surveys funded by the Bureau of Labor Statistics (BLS) of the US Department of Labor to examine changes in labor market experiences of Americans over time. The original NLS program explored the employment decisions among four groups (older men, young men, older women, and young women) over a five-year period. These surveys were successful in providing useful insight to policymakers and thus the 1979 National Longitudinal Survey of Youth

(NLSY79) was created to capture much of the same information but to do so over a longer period of time (Aughinbaugh, Pierret, & Rothstein, 2015; BLS, 2006).

The NLSY79 includes nearly 13,000 respondents who were born between 1957 and 1964 and living in the US during the first survey period (1979). The initial purpose of the NLSY79 was to gauge employment and training programs among American youth, but the survey expanded over time to cover a range of topics including health, marriage, and education (Moore, Pedlow, Krishnamurty, & Wolter, 2000). By the early 1990s however, most of the NLSY79 participants were entering their 30s and the NLSY79 data were no longer relevant to young people of the time (BLS, 2006; Center for Human Resource Research [CHRR], 2003; National Opinion Research Center [NORC], 2014). As a result, the NLSY97 was created to again capture the experiences of people who were in late adolescence and early adulthood. The NLSY97 followed approximately 9,000 individuals from adolescence (12-16 years) into young adulthood. The survey is designed to probe into many aspects of the teenage experience, including health, relationships, sexual activity, criminal behavior, drug use, and the transition from school into employment (BLS, 2006; Moore et al., 2000). Taken together, the NLSY surveys provide “much of what we know about the return on investments in schooling, career progression, job turnover, hours of work, and wages in the U.S. labor force” (NORC, 2014). As a result, the NLSY97 captures the key concepts for the present study and offers a means by which the interworking of the relationships between drug use, employment, and offending can be examined. Furthermore, the complex survey design of the NLSY97 produced a representative sample that can provide insight into the experience of everyday American youth as they transition to adulthood.

Procedures

The NLSY97 utilizes multi-stage stratified random sampling to gather information on the general US population and also explore the experiences of minority groups such as blacks and Hispanics. As a result, the NLSY97 data includes one cross-sectional sample and an additional subsample intended to oversample Hispanic and non-Hispanic black individuals.⁶ The cross-sectional sample is intended to represent all individuals born between 1980 and 1984 living in the US during the first survey wave while the oversample is meant to represent Hispanics and African Americans who fit the same criteria used for the cross-sectional sample. The purpose of oversampling is to include enough respondents such that the responses of black and Hispanic/Latino individuals can be considered in statistical analysis (CHRR, 2003; Moore et al., 2000). Indeed, the ability to provide more accurate estimates for these minority groups is a key advantage to the NLSY97. After weighting to account for potential biases related to oversampling, selection processes, and non-random attrition, the NLSY97 is representative of the civilian, non-institutionalized US population who were 12 to 16 years old as of December 31, 1996. Because the population of interest in the greater American population, the generalizability of the NLSY97 makes it an attractive and appropriate source to explore the nuances of the relationships between drug use, employment, and criminal behavior in the US.

⁶ The NLSY97 uses the term “cross-sectional” to refer to the portion of the population represented by the sample. In contrast to the sample intended to overrepresent minorities, this sample is a “cross-section” of the population. Thus, the sample is cross-sectional but the survey itself is longitudinal in that it gathers data from this segment of the population overtime.

Sampling procedures

The sampling procedures for the NLSY97 involved two phases. First, a list of eligible housing units was created from which both the cross-sectional sample and oversample would be selected. Using the description provided by the Census Bureau, the NLSY97 defines a housing unit as “a single room or group of rooms intended as separate living quarters by a family, for a group of unrelated persons living together, or for a person living alone” (Moore et al., 2000; NLS, n.d.b). Thus, all housing units not used as group quarters (such as prisons, dormitories, and military barracks) within the 50 states and the District of Columbia were eligible for selection.

Because group quarters are not considered within the universe of eligible housing structures, effort was made to link youths living in such residences to households that fell within the scope of NLSY97 eligibility. For example, children who were living at boarding school or in a college dormitory were connected to the household of their living parent or guardian provided the parent’s or guardian’s housing unit was eligible. Children of separated or divorced parents and other youths who could be tied to more than one housing unit were linked to the mother’s household unless it was ineligible, in which case they were linked to the father’s.

A total of 96,512 eligible households were identified in the following manner. First, 100 primary sampling units (PSUs) for both of the NLSY97 samples were selected from NORC’s national sample. Each PSU represented either a metropolitan area or one or more non-metropolitan counties that had at least 2,000 housing units in the cross-sectional sample. Rather than following county lines, PSUs for the oversample merged together counties that were highly populated by minorities into areas with at least 2,000

housing units (CHRR, 2003). Next, segments containing one or more adjoining blocks with at least 75 housing units were selected from each PSU for both samples. From within these segments, subsets of housing units were identified to be interviewed and compiled into the NORC's list of eligible households. The second phase of the sampling process used this list to identify individuals ages 12 to 16 as of December 31, 1996 from within eligible households. To determine individual eligibility of every person linked to a given housing unit, short interviews were completed with the "household informant" at 75,291 housing units (NLS, n.d.b). The household informant is a single member of the household who is over the age of 18 that provided interviewers with information about those individuals who usually resided in their housing unit. Based on the information provided by the household informant, researchers identified 9,907 household members who were eligible for NLSY97 participation.

Base interviews were conducted between January and October 1997 and again between March and May 1998 with individuals who had been selected during the screening process. Of the identified 9,907 eligible household members who were selected for the interview, a total of 8,984 (91.6%) were interviewed at the first wave and are considered part of the NLSY97 cohort (CHRR, 2003). Specifically, the NLSY97 includes 6,748 respondents in the cross-sectional sample and 2,236 respondents in the oversample. Prior to completing the first interview, each participant, regardless of age, completed a consent form and parents of those who were 17 years or younger also completed a written Parental Permission to Interview Youth form. Beginning in spring 2002, all respondents were over 18 and thus consent procedures were adjusted to be more appropriate for adult participants (NLSY, n.d.). Not all of those individuals interviewed in round one

participated in all (or any) subsequent survey rounds. A majority of those who did not participate in subsequent waves had refused the interview, but others were not included for different reasons such as they could not be located or they were sick or deceased. There will be a more detailed discussion about missing data in a later section (see “Missing Data” p. 54).

Survey Administration

Survey administration occurred annually in rounds 1-15 (1997-2011), but beginning in round 16, data is collected biennially. NLSY researchers continue to conduct interviews and data for round 17 (2015-2016) is expected to be available in August 2017 (BLS, 2016). Interview sessions typically last about one hour and are conducted using a computer-assisted personal interview (CAPI) instrument. The interviewer administers the CAPI using a laptop computer at a time and location of the participants’ choosing (NORC, 2013). Respondents were given a small monetary incentive for their participation in each wave. The size of the incentive has varied across waves, but in general respondents received roughly \$20.

A general assessment of the interview is provided in the “interviewer remarks” section of each NLSY97 questionnaire. This includes objective information about the interview such as where and when it took place, how long it lasted, and who else may have been present. The interviewer was also asked to comment on the respondent’s general demeanor and situational aspects of the interview such as interruptions, distractions, or misunderstandings. Special circumstances such as mental and/or physical (i.e., hard of hearing or visual impairments) disabilities, intoxication, social deficiencies, and illness/injury led to the interview to be terminated and are thus detailed in this

section. Also, whether the survey was administered in English or Spanish is provided in the interviewer remarks. The survey instrument is available in both English and Spanish to prevent language barriers from impeding the accuracy of responses from Spanish-speaking participants. However, very few participants requested the Spanish version of the questionnaire (CHRR, 2003).

Survey administration usually begins in the fall and concludes in the spring of the next year. The time during which respondents are actively being interviewed (i.e., the survey is being administered) is referred to as the fielding period (Schonlau, Fricker, & Elliot, 2002). The methods employed for the initial wave (1997) is unique from the other waves for a few reasons, one of which is the use of two fielding periods to gather data for this round. Also, the first five waves administered surveys for a shorter time than the fielding periods used in the subsequent rounds. While survey rounds from 1997 through 2001 utilized a fielding period of approximately six months, the time allotted to conduct interviews in the remaining rounds is about 8 or 9 months. The length of the NLSY97 fielding period is primarily a result of the time and effort required to locate a large number of individuals. While some are found and interviewed quickly, others take the coordinated efforts of NORC offices and employees to find participants' contact information, search databases, and track down respondents. Furthermore, the time-consuming tasks of locating and contacting respondents are exacerbated by the large sample size of the NLSY97. Indeed, there are nearly 9,000 people for staff to locate, contact, and interview during the fielding period.

Data

Sample description

Table 3.1 presents the sample sizes, retention rates, and fielding periods for both samples at each wave from 1997 through 2011. Retention rates for each wave represent the percent of first wave respondents that are still eligible for NLSY97 participation who were interviewed during a given survey year. Also, because the fielding period spans two years, Table 3.1 also includes the “survey year” that corresponds with each. For example, despite that the most recent fielding period ran from September 2011 into June 2012, the survey year for wave 15 is 2011. As noted earlier, data collection continues, but survey administration now occurs every other year rather than annually.

Wave Selection

The current study will use data from both sampling segments from every survey round from 1998 to 2011 so as to include data on every respondent from ages 18 to 27. Although some youth may have been employed during their adolescence, the Fair Labor Standards Act (FLSA) mandates 14 as the minimum age for most non-agricultural work and restricts the number of hours youths may work, especially during school hours and at nighttime (US Department of Labor, 2016). Consequently, it is likely that very few participants were employed from ages 12-17 during the early waves. Further, participants’ ages varied at the initial survey round and thus there is not information on every respondent before age 17. For these reasons, the current study examines the data for everyone who were age 18 through 27 during the survey period. Respondents who were notably younger than the average were not included because of a lack of information on their late 20s and those who were older were also included due to an absence of data on the last adolescent years.

Table 3.1 Sample description and retention rates for both the cross-sectional, supplemental, and total sample at year survey round.

Wave	Survey Year	Fielding period	Cross-sectional sample		Supplemental sample		Total sample	
			Total	Retention rate (%)	Total	Retention rate (%)	Total	Retention rate (%)
1	1997	Feb-Oct 1997 and March-May 1998	6748	-	2236	-	8984	-
2	1998	Oct 1998-Apr 1999	6279	93.0	2107	94.2	8386	93.3
3	1999	Oct 1999-Apr 2000	6172	91.5	2036	91.1	8208	91.4
4	2000	Nov 2000-May 2001	6054	89.7	2026	90.6	8080	89.9
5	2001	Nov 2001-May 2002	5918	87.7	1964	87.8	7882	87.7
6	2002	Nov 2002-July 2003	5898	87.4	1998	89.4	7896	87.9
7	2003	Oct 2003-July 2004	5782	85.7	1972	88.2	7754	86.3
8	2004	Oct 2004-July 2005	5600	83.0	1902	85.1	7502	83.5
9	2005	Oct 2005-July 2006	5437	80.6	1901	85.0	7338	81.7
10	2006	Oct 2006-July 2007	5624	83.3	1935	86.5	7559	84.1
11	2007	Oct 2007-July 2008	5521	81.8	1897	84.8	7418	82.6
12	2008	-Oct 2008-July 2009	5560	82.4	1930	86.3	7490	83.4
13	2009	Sept 2009-Apr 2010	5616	83.2	1943	86.8	7559	84.1
14	2010	Oct 2010-June 2011	5544	82.1	1935	86.6	7479	83.2
15	2011	Sept 2011-June 2012	5501	81.5	1922	86.0	7423	82.6

(National Longitudinal Survey, n.d..b)

Although data is available from the most recent round (2013-2014), 2011 will be the last wave included in this study. Because of the shift to biennial data collection, the timeframe of reference also changes. Most of the questions presented to respondents depend on the previous interview as the reference point. As a result, responses from the 2013 will include behaviors that occurred over a two year span rather than the one year timeframe used in every other round. Excluding data gathered for 2013-2014 ameliorates this incongruence between survey questions.

It is important to note that although every wave after 1997 asks about behaviors “since the date of the last interview,” the time between interviews varies among respondents and across waves. The previous interview may have been conducted exactly a year prior, but it also may have been as recent as four months. As a result, some responses will indicate occurrence over the last year while others at the same wave will reference a four month time span. Differences in reference periods within waves are even more exaggerated when considering those who missed a survey round. When asked how often they had engaged in a given behavior since the date of the last interview, those who missed a survey wave will be estimating their behavior after a date potentially much further in the past than the rest of the respondents. Consequently, their responses will include the number of times they engaged in a certain behavior in a longer timeframe than other respondents, potentially inflating their estimation. Further, using an arbitrary reference point such as the last interview date blurs the endpoints of the timeframe and thus increases the risk that respondents may include in their estimations instances that occurred outside of the reference period (Gaskell, Wright, & O’Muircheartaigh, 2000). This “telescoping effect” is further exacerbated by the length of the reference period used

in operationalizing the offending variables. For alcohol and marijuana use, respondents only have to recollect the past thirty days, but in reporting how many times they were arrested, convicted, or committed any offenses (property, violent, and drug sales), respondents must recall events that occurred many months prior. Because the date of interview is available for each survey round, the number of months between interviews can be calculated. Thus, to address inconsistencies in time between interviews, new variables are created to indicate frequency (for example, number of property offenses) per month rather than since the date of the last interview.

Missing Data

There are a series of issues related to missing data that must be addressed prior to conducting statistical analyses. Indeed, nearly 20% of respondents from the first round did not participate altogether in later rounds (see Table 3.1). Given that nearly all statistical methods assume that every case has a value for every item, failure to properly handle missing data may create biased estimations (Allison, 2002; White, Royston, & Wood, 2009). In large datasets such as the NLSY97, incomplete data are almost inevitable. Data may be missing for numerous reasons such as respondents' inability or unwillingness to participate, interviewer errors, and issues related to the survey instrument itself. In some cases, respondents have passed away or have moved and cannot be located, while in other cases the interviewer may have failed to document the response or forgot to ask a given question. Further, it is likely that a number of participants who were selected opted not to participate in any of the survey rounds. Unfortunately, information regarding these individuals is limited.

There are three primary reasons for missing data in the NLSY97. First, some respondents may not participate in a given wave and as a result, are missing all items for that particular survey year. These individuals were coded as missing because of a “non-interview” situation. The NLSY97 includes reasons for not interviewing certain respondents at each wave (refused to participate, unable to be located, too ill to participate, deceased, etc.). Another reason that some data is missing in the NLSY97 is that respondents provided an invalid response. These data are coded based on the interviewers’ discretion as either a “refusal” (“I don’t want to answer that,” or “I do not feel comfortable sharing that”) or a “don’t know” (“I have no idea” or “I’m not sure how to answer that”) (NLSY, n.d.a). Finally, the design of the NLSY97 questionnaire does not require all participants to answer each question. In some cases, if the respondent answers “no” to a lead-in question, then they are instructed to skip the next set of questions that only apply to those who answered “yes.” Participants who skip a question that does not apply to them are coded as a “valid skip” and those who wrongly skip a question that they should have answered are coded as an “invalid skip” (NLSY, n.d.a). Thus, some data may appear missing on a variable because their answer to an earlier question had indicated that the subsequent question was inapplicable to some respondents. Those cases with such “valid skips” were recoded to 0 and “invalid skips” were recoded as missing. However, because survey administration utilizes CAPI, the number of invalid skips is greatly reduced (NORC, 2013). Figure 3.1 provides a visual depiction of the amount of missing data for the variables included in the study. Race, sex, and age do not have any missing values and as such are not included in Figure 3.1.

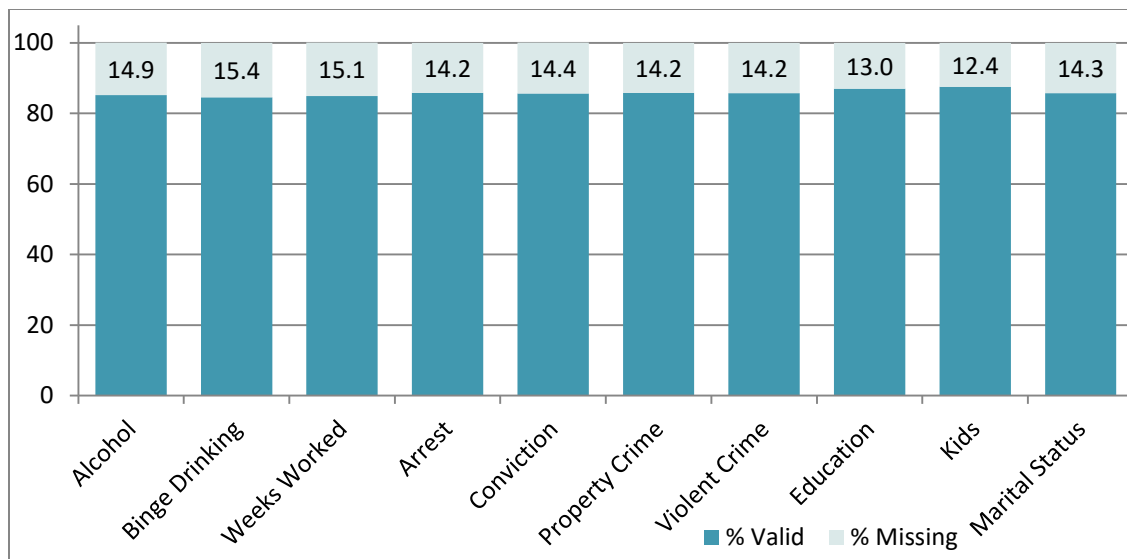


Figure 3.1 Distribution of Missing Values.

There were 111 respondents (1.24%) that did not participate in any survey round after the initial 1997 interview. The current study only uses data from waves from 1998-2011, and thus these 111 participants were removed from the dataset. There were another 80 respondents who only participated in one of the 14 waves included in this study. Because they account for less than 1% of the total dataset and were missing data for 13 waves, the decision was made to also remove these respondents from the dataset. After careful consideration of potential strategies for addressing missing data, the decision was made to use listwise deletion and remove all cases from the dataset that were missing on all of the key measures used in this study. Although there are numerous advantages to missing data methods such as multiple imputation, these strategies also have their pitfalls. First, Stata, the statistical software used for this study, is ill-equipped to perform negative binomial models on complex survey data that is both multilevel and multiply imputed data. One concession would have to be made. Analyses could:

1. Allow a second level intercept appropriate for longitudinal data and include imputed data, but not account for selection bias that results from the complex survey design of the NLSY97,
2. Include imputed data and account for the survey design but treat the data as cross-sectional rather than longitudinal, or
3. Address the complex survey design and treat the data as longitudinal but not include the imputed data.

Thus, the decision was made to account for the complex survey design and use multilevel modeling to treat the data as longitudinal (option 3), but this was not before spending time weighing the costs and benefits of each option. The primary issue of multiple imputation that influenced the decision is related to missing data on the dependent variable. Before elaborating, a brief overview of multiple imputation (MI) is required to better understand the reasons for opting against its use.

The process of MICE involves a cycle of four steps that sequentially predict values for each variable with missing data points (Rubin, 1987; von Hippel, 2007). First, each missing value is replaced with a “place holder” value, such as the mean, that is generated using a simple imputation. After all missing values in the data have been replaced, one variable is reverted back to its pre-imputation form (i.e., the “place holder” values are reset to missing). Next, a regression model is ran using the variable with missing data as the outcome and all other variables in the model as the independent variables. Finally, the values predicted from the regression model then replace the missing values of that model’s outcome variable. The process is repeated for each variable with missing values using the variables with their imputed values rather than their “place holder” values (Azur et al., 2011; Stuart et al., 2009). As a result, the missing values of each variable are replaced by the predictions of regression models that reflect

observed relationships in the data. Once the missing values of each variable have been predicted, one “cycle” or “iteration” is complete and one imputed dataset is produced. The process is repeated over a number of iterations to produce a predetermined number of datasets which are then recombined, accounting for variation between and within the datasets (Rubin, 1987).⁷

Because MICE predicts the values for missing data based on the values of data points that are available, the dependent variable is imputed based off of the same variables upon it will be regressed. To combat this, von Hippel (2007) makes the case for what he deems “multiple imputation then deletion” (MID) by which MI is used to impute all missing values, and then imputed outcomes are deleted prior to analyses. The problem with that strategy for this study is that because this study is concerned with reciprocal relationships, all of the key variables (i.e., those relating to employment, alcohol use, and crime) are treated as dependent variables in at least one model. Moreover, the key variables have more missing data than any of the controls. Of the 8,208 remaining after removing cases discussed above, there were 3,514 cases missing data on at least one of the seven key variables (*alcohol*, *binge*, *weeks worked*, *arrest*, *conviction*, *property crime*, and *violent crime*) for at least one wave. As a result, MID will produce nearly the same dataset as listwise deletion. Further, multilevel modeling does not require equal groups in order to perform analyses. For example, if data is missing on one independent variable, analyses will still be performed using the dependent variable and any of the other

⁷ It is generally recommended that ten iterations are performed (Raghunathan et al., 2001) although others argue that more are preferable (White, Royston, & Wood, 2011). Likewise, it has been common practice to create small numbers of imputed data sets (Rubin, 1987; Schafer & Olsen; von Hippel, 2005), but more recent work has discovered that greater numbers produce less biased results (Azur et al., 2011; Bodner, 2008; Graham, Olchowski, & Gilreath, 2007).

independent variables that are available in the dataset. In other words, multilevel models use all available data, even if the amount of available data varies among respondents. Consequently, it was decided that listwise deletion would be used to remove those cases missing on all seven of the key variables of interest (*alcohol, binge drinking, weeks worked, arrest, conviction, property, violence*). This resulted in the elimination of 403 cases leaving a sample of 8,730 that were used for analyses.

Variables

Given that the purpose of this study is to examine the interconnected relationships between employment, substance use, and crime, there are three key concepts of interest – alcohol use, employment, and offending behavior. However, there are a number of other factors that may influence the key variables of interest (drug use, employment, and offending) such as age, gender, and parental status (Richardson et al., 2010; Uggen, 2000). This section outlines the conceptualization and operationalization of the measures used in the current study.

Outcome Variables

The outcomes of interest in the present study are respondents' criminal offending captured by four outcome variables – days drank alcohol, days binge drank, number of arrests, and number of convictions. Respondents were asked about crimes committed that resulted in involvement with the criminal justice system as well as offending in general (i.e., those offenses for which they were not caught). Because not all offenses result in arrest or conviction, many offenses do not involve the criminal justice system. Consequently, analyses that consider only those offenses that result in arrest and/or conviction fail to acknowledge those that are not identified, reported, and/or documented

(Thornberry & Krohn, 2003). Thus, asking people directly about their offending behaviors is perhaps one of the best ways to expose some of the “dark figure” of unrecorded crime (Biderman & Reiss, 1967; Skogan, 1977).

The NLSY97 asks respondents about offenses that result in criminal justice system involvement as well as those which do not come to the attention of law enforcement. *Arrest* and *conviction* are used to measure contact with the criminal justice system. A continuous variable indicates the number of arrests since the date of the last interview. If respondents reported any arrests, they were asked follow-up questions regarding any convictions that may have resulted from the arrest(s). Thus, a continuous variable is used to indicate the number of the aforementioned arrests that resulted in conviction. In sum, offending that came to the attention of the criminal justice system is measured via continuous arrest and conviction variables.

As noted earlier, because not all crimes come to the attention of the criminal justice system, it is useful to also utilize self-reported criminal behavior. At each interview, the NLSY97 asks respondents to report various property and violent crimes. Participants were asked how many times they had committed each of the following since last being interviewed: “Purposely damaged or destroyed property that did not belong to you” (*property damage*), “Stolen something from a store, person, or house, or something that did not belong to you worth less than \$50?” (*petty theft*), “Stolen something from a store, person, or house, or something that did not belong to you worth \$50 or more including stealing a car” (*grand theft*), and “Committed other property crimes such as fencing, receiving, possessing or selling stolen property, or cheated someone by selling them something that was worthless or worth much less than what you said it was” (*other*

property crime). These four variables were then summed to create *property crime*, a continuous variable that indicates the total number of property offenses respondents' report having committed since the last interview.

Self-reported offending is also captured by a second continuous variable that indicates the number of violent offenses committed since the most recent interview. Respondents were how frequently they had "attacked someone with the idea of seriously hurting them or have had a situation end up in a serious fight or assault of some kind" (*violence*). The distributions of all of the offending variables are highly skewed toward 0.

Independent variables

Because the research questions in this study are concerned with the employment-crime and drugs-crime links, the primary independent variables of interest are intended to capture alcohol use and employment. The operationalization of these concepts is described in the following sections.

Alcohol use

Alcohol consumption is measured through a continuous variable indicating the number of days (0-30 days) in the thirty days prior to the interview that respondents consumed alcoholic beverages ("A can or bottle of beer, a glass of wine, a mixed drink, or a shot of liquor"). It is important to note that participants are included in this study beginning at age 18, a time at which they are legally prohibited from drinking alcohol. However, the purpose of the alcohol measures is not with regards to whether this behavior was against the law. Rather, the goal is to gauge if participants drank, regardless of whether that behavior was illegal, and how such behavior is related to employment and offending. Furthermore,

Extant research suggests that it is *excessive* alcohol consumption (i.e., binge drinking) that is associated with offending, as opposed to alcohol consumption in general (Boles & Miotto, 2003; Craig, Morris, Piquero, & Farrington, 2015). Thus, in addition to the aforementioned variable used to capture alcohol consumption generally, a second variable is used to operationalize binge drinking. Consistent with the NSDUH definition⁸ (SAMHSA, 2014), the NLSY asks respondents about the number of days they drank five or more alcoholic beverages in the thirty days prior to the interview. Using this question, *binge drinking* indicates how often respondent's binge drank in the last month (0-30 days).

Employment

Employment is operationalized by a continuous variable that represents the number of weeks each respondent was employed since the date of the last interview. However, because the time between rounds was not the same for all participants, this measure uses a different reference point for each respondent. For example, a respondent who reported working 24 weeks since the last interview may appear to have worked less than a different respondent who reports having worked 30 weeks since the last round. However, the first respondent was interviewed 6 months prior and thus had worked nearly 95% of the weeks since the previous round while the second was interviewed 12 months ago and thus had worked less than 60% of the weeks since their last interview. To avoid such issues, the number of weeks since the previous survey round was used to calculate a continuous variable (*weeks worked*) that ranges from 0 to 1 to indicate the proportion of weeks worked since the last interview date.

⁸ Although SAMHSA defines binge drinking differently for males and females, the NSDUH questionnaire does not make this distinction. In the NLSY97, respondents are not asked about "binge drinking" by name, but rather they are specifically asked the number of days they consumed five or more drinks.

Demographics

Additional variables are included to account for other factors that may influence the relationships between employment, alcohol use, and crime. Respondent sex is captured by a dummy variable (*male*) that is coded 1 for male and 0 for female. Race is coded as a series of dummy variables indicating whether respondents are each of the listed races. For example, *white* is coded 1 for white respondents and 0 for respondents of any other race. The same strategy is done to create *black* (1=black, 0=non-black), and *other* (1=other race, 0=not other race). A continuous variable indicates respondent age and is measure in years.

During the years of emerging adulthood, many people make the transition to settling down and starting a family (Arnett, 2000). The responsibilities associated with having a family changes priorities and schedules and thus may lead to differences between respondents' likelihood of arrest that is not related to their job or drug use. Thus, the number of children respondents have will be controlled for by a count variable to measure parental status. Respondents were asked about the number of biological children they had living both in and out of their residence. These two variables were then merged to create *parental status*, a continuous variable that indicates the total number of biological children.

Finally, a continuous variable is used to control for income. This variable is included in analyses because the purpose of this study is not to examine how job related earnings influences the alcohol-work-crime connection, but rather if having a job in general impacts these relationships.

Complex Survey Design

As discussed in the section regarding sampling procedures, the NLSY97 adopts a complex survey design such that the sample is representative of the population. As a result, it is essential to account for strata and primary sampling units (PSU) involved in the sampling strategy. It is also imperative to include sampling weights, which are “used to reflect the differing probabilities of selection of the sample elements” (Lee & Forthofer, 2006, p. 7). Failure to account for survey design features will result in biased point estimates as well as biased estimation of means and standard errors (Rabe-Hesketh & Skrondal, 2012). To address this, the following analyses of the NLSY97 data are “design-based” such that they account for randomness produced by the clustered nature of the data. Sampling weights were added to the analyses such that respondents are “weighted” with a value that represents the portion of the larger population they are intended to represent. The proportions presented throughout the following sections are weighted in order to account for the sampling design.

Analytic Strategy

Multilevel modeling for longitudinal data

The network of relationships between alcohol use, employment, and offending in a longitudinal dataset will be analyzed using multilevel modeling. “Multilevel” refers to the structure of the data and the appropriate analytical models for handling parameters at different levels of analysis (i.e., individual changes over time and differences between individuals) (Hox, 2000). When factors at a higher level of analysis influence those at a lower level, it is inappropriate to use single-level analytical techniques that fail to address higher level features and/or processes (Luke, 2004; Raudenbush & Bryk, 1986; Schnabel,

Little, & Baumert, 2000). In the case of longitudinal data such as the NLSY97, repeated measures across years can be understood as nested within the individual (Goldstein, 2005; Luke, 2004; Rabe-Hesketh & Skrondal, 2012; Rasbash & Goldstein, 1994). As described by Hox (2000), “one way to model data [with repeated measures for each individual] is to view the series of repeated measures as a separate level below the individual level. The individual level becomes the second level, and it is possible to add a third and higher levels for possible group structures [such as classrooms within schools or blocks within a city]” (p. 19). Indeed, this ability to estimate one or more effects as either fixed or random is a distinguishing feature of multilevel modeling. On one hand, fixed effects have only one value in the model that is applied to each level 1 observation despite the higher level(s) in which it is nested. On the other hand, random effects are given a value that is allowed to vary in accordance with level 2 units (Hayes, 2006; Rabe-Hesketh & Skrondal, 2012).

Ignoring the multilevel nature of longitudinal data likely violates the assumption of independent observations (Bauer, Preacher, & Gil, 2006). For example, whether or not an individual has consumed alcohol at the first wave is likely associated with use at each subsequent wave. Failing to account for such clustering produces negatively biased standard errors and leads to non-significant effects being falsely identified as significant (Type I error) (Maas & Hox, 2004; Rabe-Hesketh & Skrondal, 2012). Consequently, approaches to longitudinal datasets like the NLSY97 must be equipped to account for group effects and within-group correlation (Hayes, 2006). Multilevel modeling is one such approach that allows observations to be correlated such that measures at each wave are not independent from measures at other waves (Luke, 2004). Consequently,

multilevel modeling is the appropriate analytic strategy to examine these relationships. For multilevel mediational analyses to be appropriately conducted, the data must be clustered with positive ICC and the outcome variable must be at the lowest level.

The overarching purpose of the current study is to better understand the nuanced and reciprocal relationships between employment, alcohol use, and offending. As a result, there are three general research questions involved. First, this study examines the direct effects of alcohol use and employment on offending. Also, because research suggests offending also exerts influence on subsequent drinking and employment (Kurlychek, Brame, & Bushway, 2007; Nagin & Waldfogel, 1998; Pager, 2003), the relationships will then be approached from the opposite direction (i.e., the effect of offending on alcohol use and the effect of offending on employment). Second, the study considers potential interactions between drinking and employment to evaluate whether the effect of alcohol use and/or employment on offending is contingent upon the other. In other words, the possibility of an interaction between alcohol use and employment is considered. Third, analyses examine the potential that either employment or alcohol use act as mediators in the others' relationship to crime. Specifically, this study tests whether employment mediates the effect of drinking on crime and whether alcohol use mediates the effect of employment on crime. In sum, this study involves analyses of direct effects as well as both moderation and mediation effects. Each of these will be further explained in the following sections.

Direct effects

Prior to assessing any moderation or mediation involved in the relationships between alcohol, work, and crime, it is necessary to individually evaluate the effects of

alcohol use and employment on offending. As presented in the previous section, there are two employment variables, two alcohol use variables, and five continuous crime variables that operationalize the key concepts involved. A series of mixed effects negative binomial regression models are used to evaluate the effects of alcohol use variables (*alcohol* and *binge drinking*) on the frequency of arrest and conviction. Two additional mixed effects negative binomial regression models are used for the self-reported offending variables (*property* and *violence*). Lastly, a multilevel general linear model is used to estimate the proportion of weeks worked. In sum, four mixed effects models are used to approach the impact of employment and alcohol use on offending.

Additional steps must be taken to consider the possibility of mediation and moderation among drug use, employment, and offending. Hypotheses involving mediation and moderation require that drug use and employment are not only related to crime, but also that they are related to each other. Although extant literature suggests a relationship between employment and alcohol use (Henkel, 2011; SAMHSA, 2014), the nature of the relationship is not entirely clear. For example, it is possible that the effect of drinking on offending depends on employment (i.e., employment moderates the alcohol-crime link), but it also may be that the effect of employment depends on alcohol use (i.e., alcohol use moderates the employment-crime link). The multidirectional relationship and potential for either variable to serve as the moderator proves challenging when interpreting results from analyses. The Stata command `-margins-` offers an analytic strategy to evaluate these interactions by allowing for significance testing of the effect of X on Y at different levels of M. Therefore, the effect of alcohol use on offending will be evaluated at different levels of employment (i.e., proportion of weeks worked) and the

effect of employment on offending at different values of alcohol use variables will also be considered.

To further complicate the matter, it may also be that alcohol use operates through employment. In that case, employment mediates the alcohol-crime link such that drinking affects employment which then impacts offending. While the effect of employment on offending may depend on alcohol use (moderation), it may also be that the effect of employment on offending is mediated by alcohol use – that is, employment affects alcohol use which then impacts offending. For example, having a job may reduce the likelihood of drinking which then results in lower levels of offending. The following sections outline moderation and mediation and describe the analytic approach used to model each.

Moderation

Figure 3.2 presents a graphic depiction of the possible moderation involved in the interconnected relationships between alcohol use, employment, and offending.

Moderation, or interaction, occurs when the effect of one variable on an outcome is not constant but rather varies at different values of a third variable. In other words, the effect of one variable on a second is determined by the value of a third. It follows that a key assumption of moderation is that both the independent variable and the moderator are related to the outcome variable.

The extant literature provides mixed findings regarding the impact of employment on offending. Some research suggests a protective effect of employment on alcohol use such that those who are employed are less likely to consume alcohol than those who are not employed (Henkel, 2011; Popovici et al., 2012). At the same time, NSDUH results

indicate rates of drinking are higher among adults employed full-time (SAMHSA, 2014). It also appears that using drugs and alcohol reduces the likelihood of employment (Bray et al., 2000; French, Roebuck, & Alexandre, 2001; Richardson et al., 2010). Given that there is research to suggest that employment reduces alcohol use and that alcohol use reduces employment, it is possible that moderation may go in either direction. That is, alcohol use may moderate the relationship between employment and crime, but at the same time, employment may moderate the relationship between alcohol use and crime.

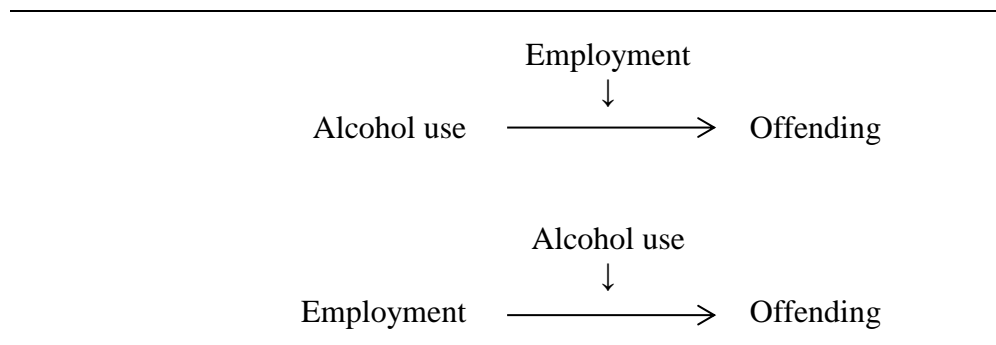


Figure 3.2 Moderation in the Alcohol-Employment-Offending Relationship

On one hand, the effect of alcohol use on offending may be contingent on employment such that alcohol users who have a job differ in their offending from alcohol users who are not employed. Despite their drinking, users who are employed have obligations, responsibilities, and a source of legitimate income. As a result, they may be less likely to commit crime because they have less free time during which they could break the law and the legitimate earnings reduce the need to commit crime to gain financial resources. Moreover, places of employment provide opportunities to form ties to conventional society and build social bonds with others at work. Thus, the social bonds formed as a result of employment can also serve to decrease offending regardless of alcohol use. On the other hand, the effect of employment on offending may be dependent

on alcohol use. Although employment may have a protective effect on subsequent crime for non-users, employment may increase offending for those who drink. More specifically, the connection between employment and crime may vary based on the severity of alcohol use. Thus, the present study will examine the effect of employment based on how many times respondents consumed alcohol in general in the previous 30 days, as well as how often the binge drank in the last month.

Results from analyses will indicate whether either of the alcohol use variables interact with the proportion of weeks worked in their effect on subsequent offending. For example, analyses will examine how *alcohol* influences the effect of the proportion of weeks worked since the last interview (*weeks worked*) on offending. This will be repeated for the binge drinking variable (*binge*) to consider how it interacts with *weeks worked* to impact offending. Because there are five variables used to operationalize offending, the moderating effects of each of the two alcohol use variables on employment will be considered with regards to each of the five offending variables.

Mediation

In addition to examining direct effects and conducting moderation analyses, this study will examine the indirect effects of employment and alcohol use using multilevel mediation analyses. A fundamental assumption of mediation is that the first two variables are correlated. Thus, a multilevel regression model will be used to examine the relationship between alcohol use and employment using both alcohol use variables and *weeks worked*. Provided a relationship exists between alcohol use and employment, a subsequent mediation analysis will be conducted that considers the possible mediation effects of employment on the alcohol-crime link and of alcohol use on the employment-

crime connection. The purpose of mediation analyses is to determine if the relationship between two variables is, at least to some extent, due to a mediating variable (Krull & MacKinnon, 2001). When mediation is present, the effect of a variable operates, in part or entirely, through its effect on a mediating variable (i.e., alcohol use affects employment and employment subsequently impacts offending).

For relationships that involve complete mediation, it is the mediating variable that directly impacts the outcome variable. It is also possible however, that a relationship may involve partial mediation. In such a case, the causal variable directly impacts both the mediator and the outcome. Thus, the relationship is explained partially by the direct effects of the causal variable and partially by the effect of the mediator. Figure 3.3 presents the unmediated model which captures the *total effect* (c) of one variable (X) on another (Y). The total effect is equal to the sum of the direct (c') and indirect effects (ab) (see Figure 3.4). However, without considering the possibility of mediation, the nuances of indirect effects remain indiscernible.

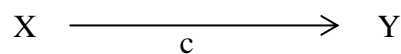
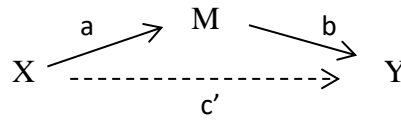


Figure 3.3 Unmediated Model

Figure 3.4 presents a diagram of mediation where the relationship between one variable (X) and the outcome (Y) is mediated by a third variable (M). There are two causal pathways involved, one directly to the outcome (path c')⁹ and the other through the mediating variable (path a and b). Similar to path c in Figure 3.3, path c' represents

⁹ The superscript (c') is used to indicate the *direct* effect and to distinguish this causal pathway from the *total* effect which is represented by c .

the effect of X on Y. However, unlike the unmediated model, c' accounts for the effect of a mediating variable (M). As a result, c' represents the *direct effect* of X on Y when controlling for the mediator (M). In contrast, the path through the mediator (a and b) indicates the amount of mediation, or the *indirect effect* of X on Y (Baron & Kenny, 1986).



* Note: Solid arrows indicate mediation while dashed arrows denote direct effects.

Figure 3.4 Mediated Model

The most commonly used strategy to test mediation is the causal-steps approach, which outlined four steps to establish mediation that involve three regression equations. The following procedure is clearly outlined by Baron and Kenny (1986) and is widely accepted across disciplines (Fairchild & MacKinnon, 2009; Judd & Kenny, 1981; MacKinnon, Fairchild, & Fritz, 2007). Many scholars have explored mediation effects (James & Brett, 1984; Judd & Kenny, 1981; Kenny, Kashy, & Bolger, 1998; MacCorquodale & Meehl, 1948; Wright, 1934), but Baron and Kenny's (1986) work clearly articulates the process by which mediation can be analyzed. First, it is necessary to establish that a correlation exists between the causal variable (X) and the outcome (Y) (i.e., that path c exists). That is, the first step of mediation analysis is to ascertain whether there is an effect to be mediated. Second, there must be a correlation between the key independent variable (X) and the mediator (i.e., establishes path a). To demonstrate the relationship, the mediating variable (M) is treated as the outcome variable in the equation. Third, M must be related to Y when controlling for the X. Failure to include the

independent variable may result in falsely identifying them as correlated (type I error) because they are both caused by X. Using the same equation from step three, the final step is to determine whether the mediator variable completely or partially mediates the relationship between the causal and outcome variables (Baron & Kenny, 1986). *Complete* mediation occurs when the effect of the causal variable on the outcome controlling for the mediator is 0. In other words, once the mediator is controlled for, there is no longer any effect of the causal variable on the outcome. However, partial mediation (i.e., c' is reduced when X is included but remains different from 0) is more common than complete mediation (Kenny, 2016). That is, when mediation is involved in a relationship, it more commonly explains some, but not all, of the relationship between the variables. In sum, the four steps of the causal-steps approach are:

1. establish the relationship between X and Y
2. establish the relationship between X and M
3. establish a relationship between M and Y controlling for X
4. determine whether M partially or completely mediates the relationship between X and Y

Thus, the current study will apply these steps to consider first how employment mediates the connection between alcohol use and crime, and second, whether alcohol use mediates the employment-crime link. However, a shortcoming of this approach is the failure to test the significance of the indirect effect of X on Y through the mediator via path *a* and path *b*. Further, the causal-steps approach often fails to identify true mediation effects (i.e., Type II error) (MacKinnon et al., 2007). Thus, to address these issues, a regression coefficient is calculated for the indirect effect and tested for significance. The indirect effect coefficient (B_{indirect}) indicates the effect on Y for every one unit change in X that is mediated by M. This is done using the Sobel product of coefficients approach

which multiplies the partial regression effect of M predicting Y (B_2), and the coefficient for X predicting M (B) to calculate B_{indirect} .

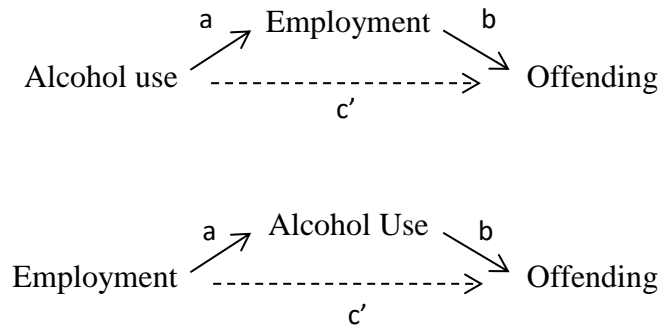
Figure 3.5 presents a diagram of these mediation processes that may be involved in the alcohol, employment, and crime connection. To examine the possibility that employment acts as a mediator, the first step is to demonstrate that a relationship exists between alcohol use (causal variable) and offending (the outcome). Recall that the first section of analyses evaluates the total effects involved in the network between drinking, work and crime. Thus, the equation for this step is the same as that used to evaluate the effects of alcohol use on offending that was formulated earlier. Second, a correlation between alcohol use and employment is estimated using a regression equation where alcohol use is the predictor and employment is the dependent variable. The third step requires an evaluation of the relationship between employment (mediator) and offending. To do so, the regression equation includes both alcohol use (casual variable) and employment (mediator) as independent variables to determine their effect on offending. Alcohol use must be controlled for in the equation to clearly understand any correlation that may exist between employment and offending. Without including alcohol use, employment and offending may appear correlated, but it cannot be determined if that correlation exists because they are both dependent on alcohol use or because they are truly associated with one another. Finally, the fourth step determines whether employment partially or completely mediates the alcohol-crime link. Employment completely mediates the relationship if the effect of alcohol use on offending is zero when employment is controlled for – that is, employment completely explains the relationship between alcohol use and crime. In contrast, employment partially mediates

the relationship if the effect of alcohol use identified in step one is larger than the effect of alcohol use identified in step three. After using employment as the mediator, the same process is repeated to consider alcohol use as the mediator in the employment-crime link.

However, there are a number of weaknesses of the causal steps procedure that are imperative to recognize. First, an independent variables indirect effects may be in the opposite direction of their direct effects. If there are effects going in two different directions and they are approximately equal, then the first mediation condition may not be satisfied, despite a mediating connection existing between variables. Another key shortcoming of the causal steps process occurs at step 3. It has been argued that controlling for the independent variable at this step may reduce the link between the mediator and the outcome to nonsignificance. Likewise, controlling for the mediator may result in a minor reduction in a relationship between the IV and DV that reduces the relationship to nonsignificance. Lastly, the causal steps approach does not result in an estimated effect size for the mediator variable (i.e., indirect effect).

To address these shortcomings, there has been a recent shift in the literature where authors are urging others to adopt another mediation strategy and developing alternative approaches (MacKinnon, Cheon, & Pirlott, 2011; MacKinnon, Lockwood, & Williams, 2004; MacKinnon, 2004; MacKinnon, 2002; Preacher & Kelly, 2011). Specifically, the importance of indirect effects are emphasized and researchers are encouraged to test the significance of the indirect pathway (that X affects Y through path A to the mediator and then through path b). Indirect effects are equal to the total effect minus the direct effects. These effects can be calculated as the difference between paths c (total effects) & c' (direct effects) – $IE = c - c'$ – or as the product of paths a and b ($ab = c - c'$). Thus, in

accordance with modern mediation techniques, the indirect effect will be calculated and evaluated to determine the extent and significance of mediation processes.



Note: Solid arrows indicate mediation while dashed arrows denote direct effects.

Figure 3.5 Mediation in the alcohol-employment-offending relationship

Upon completion of both mediation analyses, sensitivity analyses are conducted to assess the impact of violations of key assumptions. Testing the sensitivity of statistical models is essential because it “[quantifies] the degree to which empirical findings rely upon the key assumptions” (Imai, Keele, Tingley, & Yamamoto, 2011, p. 766). Stata includes a mediation package that easily facilitates such sensitivity analyses.

In sum, the analytic approach adopted for this study is intended to address three overarching research questions. First, how are substance use and employment related to offending and to each other? Second, do these factors interact in their effect on offending? Lastly, are there mediation mechanisms at work in the network between substance use, employment, and offending? To answer these questions, the current study explores seven hypotheses. The first four hypotheses are concerned with the individual effects in the alcohol-employment-crime connection. The fifth focuses on possible interactions between alcohol use and employment (i.e., does alcohol use or employment operate as mediators in the other’s relationship to offending?). Finally, the last two

questions explore potential mediation of the effects of alcohol use and employment on crime. The hypotheses are as follows:

- H1. Alcohol use affects the likelihood of offending.
- H2. Employment affects the likelihood offending.
- H3. Substance use and employment are related to each other.
- H4. Offending has reciprocal effects on alcohol use and employment.
- H5. Alcohol use and employment interact in their impact on offending.
- H6. Employment mediates the relationship between alcohol use and offending.
- H7. Alcohol use mediates the relationship between employment and offending.

The following chapter presents the results from analyses that were conducted using Stata, statistical software that has the capacity to adequately account for complex survey designs. Collinearity diagnostics were conducted prior to analyses which confirmed that many variables had variances much larger than their means. Thus, the decision was made to use negative binomial models in place of Poisson models. The data has been weighted to be representative of the population.

CHAPTER 4

RESULTS

This chapter first presents descriptive summary statistics of the sample which have been weighted to be representative of the US population. These summary statistics will guide decisions regarding which variables to consider for the interaction models. The distribution of alcohol, employment, and offender variables are then examined. Next, a series of mixed effects logit models and mixed effects negative binomial models are conducted. Also, this section will explore potential interactions to evaluate the moderating effects of alcohol consumption, binge drinking, and employment on offending. Reciprocal effects will also be considered. The final section of this chapter involves mediation analyses that consider both alcohol consumption and employment as potential mediators in the others' relationship with offending.

Summary Statistics

Table 4.1 summarizes the demographic characteristics of NLSY97 respondents included in this study. The values presented in Table 4.1 are weighted to be representative of the population. Approximately 51% of respondents included in this study are male and just over 70% of the sample is white. Black respondents comprise slightly more than 15% of the sample and the remaining 14% of respondents were categorized as part of some “other” racial group including Hispanic, Asian, Hawaiian/Pacific Islander, and Native American or identified as having more than one

race. The average age of the NLSY sample used in this study is about 22 years old, which is markedly younger than the average age of the US population (37 years old) (US Census Bureau, 2015). However, the goal here is to explore key aspects of emerging adulthood (i.e., employment and alcohol use) and thus the younger average age is preferable for purposes of this study. The NLSY97 sample is more diverse than the US population of the same age, with fewer whites and greater numbers of those who are of some “other” race (i.e., not white or Black).

Table 4.1 Demographic Characteristics of NLSY97 Respondents and of the US Population Aged 17 to 27.

	Sample %	US Population ¹⁰ %
<i>Race</i>		
White	72.4	62.3
Black	14.5	14.5
Other	13.1	23.2
<i>Sex</i>		
Male	47.0	49.2
Female	53.0	50.8

Note: Estimates are weighted to account for the complex survey design of the NLSY97

Table 4.2 presents the means and standard errors for the alcohol and employment variables (*alcohol*, *binge drinking*, and *weeks worked*). Respondents averaged about 4 days of drinking and 1-2 days of binge drinking in the 30 days prior to the interview. Respondents worked on average approximately 70% of the time since their last interview.

Table 4.2 Summary Statistics for Key Variables of Interest.

	Mean	SE	Minimum	Maximum
Days drank ($n = 8,217$)	4.55	0.09	0	30
Days binge drank ($n = 8,266$)	1.80	0.05	0	30
Proportion of weeks worked ($n = 7,855$)	0.69	0.05	0	1.00

¹⁰ Demographics of the US population are based on those provided on the US Census Bureau website (US Census Bureau, 2015).

Note: Estimates are weighted to account for the complex survey design of the NLSY97.

Table 4.3 presents the percent of respondents who have been arrested and/or convicted, as well as those that report committing property and/or violent offenses. Approximately 16% of respondents report having been arrested at least once. Of those who have been arrested, over 85% report a conviction. With regards to offenses that did not necessarily come to the attention of the criminal justice system, property crimes and violent crimes were reported by about 15% of the sample.

Table 4.3 Respondent Self-Reported Offending, Ages 18-27 ($n=8,738$)

	%
<i>Arrest</i> ($n = 8,340$)	
Not arrested	84.0
Arrested	16.0
<i>Conviction</i> ^a ($n = 8,632$)	
Not convicted	14.5
Convicted	85.5
<i>Property Offenses</i> ($n = 8,632$)	
Did not commit any property offenses	84.0
Committed property offenses	16.0
<i>Violent Offenses</i> ($n = 8,636$)	
Did not commit any violent offenses	85.2
Committed violent offenses	14.8

^a Among those who were arrested ($N=1,319$)

Note: Estimates are weighted to account for the complex survey design of the NLSY97

Table 4.4 presents the mean and standard error of the alcohol use and employment variables for those who have been arrested and for those who have not been arrested. Among those who have been arrested, the average number of days of drinking is notably greater than among those who have not been arrested. While those who have not been arrested report about 4 days of drinking, respondents who have been arrested average

about 6 days of drinking in the month prior to the interview. A similar pattern emerges in days of binge drinking, with those who have been arrested averaging a little more than 5 days of binge drinking and those who have not been arrested averaging about 3 days of binge drinking. Table 4.3 also suggests that respondents who have been arrested work less than those who have not. Specifically, those who have not been arrested worked about 70% of the weeks since their previous interview while those who were arrested worked a little more than 55% of the weeks. In other words, respondents who have not been arrested worked about 36 weeks (255 days) per year while those who have been arrested worked about 28 weeks (200 days).

Table 4.4 Differences in Drinking and Employment by Arrest, Ages 18-27 ($n=8,730$)

	<u>Arrested</u>		<u>Not Arrested</u>		Test Statistic
	Mean	St. Error	Mean	St. Error	
Days of drinking	6.23	0.19	4.46	0.09	92.73**
Days of binge drinking ^a	5.29	0.17	3.04	0.06	169.85**
Weeks worked	0.56	0.01	0.70	0.00	196.94**

* $p < .05$, ** $p < .01$

^a Among those who report drinking at least one day ($N=4,323$)

Note: Estimates are weighted to account for the complex survey design of the NLSY97

Table 4.5 presents the mean values for the alcohol and employment variables for respondents by conviction and indicates that rates are relatively similar for the two groups. Both respondents who have been convicted as well as those who were not averaged almost 5 days of drinking and 2 days of binge drinking in the 30 days preceding the interview. In contrast, those who have been convicted report working about 60% of the weeks since the prior interview, which is 10% less than the 70% of weeks worked by those who were not convicted.

Table 4.5 Differences in Drinking and Employment by Conviction, Ages 18-27
(*n*=8,730)

	<u>Convicted</u>		<u>Not Convicted</u>		Test Statistic
	Mean	St. Error	Mean	St. Error	
Days of drinking	4.69	0.13	4.51	0.09	1.94
Days of binge drinking	3.67	0.11	3.09	0.06	27.05 **
Weeks worked	0.60	0.01	0.70	0.00	66.74**

* $p < .05$, ** $p < .01$

Note: Estimates are weighted to account for the complex survey design of the NLSY97

The results in Table 4.6 ascertain statistically significant differences between those who have committed property crime and those who have not. However, with regards to drinking and binge drinking, the differences are minimal. Although those who have commit at least one property crime report slightly greater numbers, both groups report drinking about five days and binge drinking about three days. Differences in employment between those who have committed property crimes and those who have not are more notable. Respondents who have not committed a property crime work 70% of the weeks since the last interview, while those who have committed a property crime work about 10% less.

Table 4.6 Differences in Drinking and Employment by Property Offenses, Ages 18-27 (*n*=8,730)

	<u>Committed</u> <u>Property Crime</u>		<u>Did Not Commit</u> <u>Property Crime</u>		Test Statistic
	Mean	St. Error	Mean	St. Error	
Days of drinking	5.08	0.14	4.46	0.09	23.63**
Days of binge drinking	3.96	0.13	3.04	0.60	49.68**
Weeks worked	0.60	0.01	0.70	0.00	141.91**

* $p < .05$, ** $p < .01$

Note: Estimates are weighted to account for the complex survey design of the NLSY97

Table 4.7 presents the differences in alcohol consumption and weeks worked between respondents who report having committed one or more violent crimes and those who have not. Respondents drink alcohol approximately 5 days in the month prior to the email, regardless of whether violent crimes were committed. As was the case with each of the offending variables, the average number of days drank is slightly higher for those who have been violent than those who have not. Respondents who were violent and reported drinking at least once reported binge drinking over three days in the last month. In contrast, those who were not violent but had reported drinking at least once in the month prior to the interview averaged less than 3 days of binge drinking. The groups are more clearly distinguished with regards to weeks worked. Respondents who have been violent work about 56% of the time since the previous interview date, while those who have not been violent work about 70% of the weeks since the last survey round.

Table 4.7 Differences in Drinking and Employment by Violent Offenses, Ages 18-27. (n=8,730)

	<u>Committed Violent Crime</u>		<u>Did Not Commit Violent Crime</u>		Test Statistic
	Mean	St. Error	Mean	St. Error	
Days of drinking	4.94	0.14	4.59	0.09	12.27**
Days of binge drinking	3.48	0.11	2.66	0.06	51.66**
Weeks worked	0.56	0.01	0.70	0.01	194.67**

* p < .05, ** p < .01, *** p < .001

Note: Estimates are weighted to account for the complex survey design of the NLSY97

Multivariate Analyses of Alcohol, Employment, and Offending

Based on the preliminary results from bivariate analyses, it appears moderation and mediation analyses are appropriate. Prior to conducting such analyses, the

relationships among variables will be explored using multivariate analyses. The literature suggests that on one hand alcohol use may be directly related to increases in offending, while on the other, employment is directly related to reductions in offending behaviors. To consider these relationships, the effects of alcohol consumption, binge drinking, and weeks worked on the number of arrests, property crimes, and violent offenses will each be examined. Also in this section, the potential interaction between alcohol and employment will be explored. Because extant research also suggests a reciprocal effect of offending on subsequent drinking and employment, analyses will be conducted utilizing the offending variables as the independent variables. Finally, mediation analyses will be performed considering both alcohol and employment as potential mediators in the others' effect on offending. Results from the earlier models will inform which variables are explored in mediation analyses.

It is important to note that the large sample size used in this study provides high statistical power. This means that small, subtle effects can be easily detected and determined to be statistically significant despite having minimal substantive consequences (Cohen, 1990). Thus, while statistical significance will be reported, the focus of the following analyses emphasizes the magnitude of effects and not merely that effects exist. One way to do so is by providing the odds ratio (OR) for logit models and the incidence-rate ratio (IRR) for negative binomial models in addition to the coefficients. The IRR is the occurrence of an event, such as arrest or days drank alcohol, during a given time. It is useful for interpreting results from mixed effects negative binomial regression models and provides information about the size of the identified effects. For purposes of the current study, the timeframe used to determine the IRR would

have varied from person to person had questions relying on the previous interview date not been transformed to indicate frequency per month (see “Wave Selection” p. 56). A variable that indicates the proportion of weeks since the last interview date (*weeks*) is included as an exposure variable in the following models.

Multivariate Analyses of Arrest

Table 4.8 presents the results from three multilevel logistic regressions using the dichotomous arrest variable as the outcome. Because days drank and binge drinking are strongly correlated ($r=0.70$, $p<0.01$), they are analyzed using separate models to avoid issues of collinearity. For each of the outcome variables, results from the model using days drank will be presented first followed by the model for binge drinking. Model 1 presents the null model (also referred to as the intercept only model) with no parameters. The null model does not explain or account for the variance in the outcome variable, but rather it separates the variance into that which occurs between groups (at Level 2) and that which occurs within them (at Level 1). Thus, in the context of the current study, Model 1 provides information about the differences among respondents – that is, it indicates the between-individual variance¹¹ (Level 2) (Robson & Pevalin, 2016). Model 2 examines the effects of days drank alcohol on arrest. Model 3 includes the same variables as Model 2 but also considers the interaction between the days drank alcohol and the proportion of weeks worked since the last interview.

There are a number of patterns in the relationships among variables that emerge from multivariate analyses and can be observed in Table 4.8. The effects of sex, being married, number of kids, and level of education are all significantly related to arrest, a

¹¹ Identified as “Individual Level Variance” in all subsequent tables.

trend that will continue throughout the following analyses. Males ($b=1.05$, $p<0.01$) have notably more arrests than females such that the odds of being arrested among males is three times the odds of being arrested among females. In contrast, being married and having more education are inversely related to arrest. Being married reduces the odds of arrest to 40% that of those who are single and have never been married. Similarly, having attending some college reduces the odds of arrest by more than half and the odds of those who have graduated from college being arrested is about 20% the odds of those who never finished high school.

Perhaps most noticeably, nearly all variables included in the models are significantly related to the arrest outcome, with the exception of age. Recall that the large sample size of this study provides high statistical power which allows for detection of relatively small effects. As a result, statistical significance provides little insight into the substantive effects of the model and the focus must be on the coefficients and incident rate ratios rather than significance. The proportion of weeks worked since the last interview has a significant effect on arrest, although the size of the effect is minimal. For every one percent increase in the percent of weeks worked there is a reduction in odds of arrest by 0.7%. Consider a respondent who had exactly 52 weeks between interviews. A one percent increase weeks worked would translate into about half of a week. Thus, if that person were to increase the percent of weeks worked by an additional two weeks, their odds of arrest would be reduced by about 3%. Conversely, increasing days drunk by one day increases odds of arrest by about 5%. These findings are consistent with what the literature suggests regarding the protective effect of employment on offending and the criminogenic effect of alcohol use, albeit in this case the effect is quite small. Model 3

indicates that a significant ($p<0.05$) interaction exists between weeks worked and days of drinking.

Table 4.8 The Effects of Drinking Alcohol and Employment on Arrest: Multilevel Logit Model Estimates ($N=8,340$)

	Model 1 <i>b</i> (Std. Error)	Model 2 <i>b</i> (Std. Error) OR	Model 3 <i>b</i> (Std. Error) OR
Days drank		0.05 (0.01)** 1.05	0.06 (0.01)** 1.06
Weeks worked ^a		-0.70 (0.08) ** 0.99	-0.60 (0.09) * 0.99
Age		-0.08 (0.01)** 0.92	-0.08 (0.01) 0.92
Black		0.18 (0.06) 0.75	-0.29 (0.10) 0.75
Hispanic or mixed		-0.15 (0.11)* 0.74	-0.30 (0.11) 0.74
Male		1.15 (0.08)** 3.31	1.14 (0.08)** 3.31
Some high school		-0.86 (0.11)** 0.32	-1.14 (0.11)** 0.32
Some college		-0.83 (0.07)** 0.47	-0.75 (0.07)** 0.47
College graduate or more		-1.64 (0.08)** 0.20	-1.62 (0.07)** 0.20
Married		-0.90 (0.11)** 0.43	-0.85 (0.11)** 0.43
Separated, Divorced, or Widowed		0.53 (0.14) ** 1.24	0.22 (0.14) ** 1.24
Number of Kids		0.26 (0.04)** 1.25	0.23 (0.04)** 1.25
Weeks Worked x Alcohol ^a			0.02 (0.01)* 1.01
Fixed Intercept	-2.49 (0.04)**	-5.85 (0.28)**	-7.27 (0.12)**
Individual-Level Variance	6.52 (0.22)	1.70 (0.12)	1.63 (0.11)

* $p<0.05$; ** $p<0.01$, *** $p<0.001$

^a Regression coefficient and standard error have been multiplied by 100.

Table 4.8 shows a significant interaction exists between weeks worked and the number of days drank. Specifically, the impact of drinking days on arrest is reduced as the proportion of weeks worked increases. In other words, weeks worked is inversely related to the positive relationship between alcohol use and arrest. Figure 4.1 provides a conceptual illustration of the relationship similar to Figure 3.2 presented in the previous chapter. It should be noted that this could also be interpreted that alcohol use impacts the relationship between weeks worked and arrest. Considering it this way, days of drinking positively impacts the negative effect of weeks worked on arrest. That is, the inverse relationship between weeks worked and arrest becomes smaller the more days drank. This is also presented in Figure 4.1.

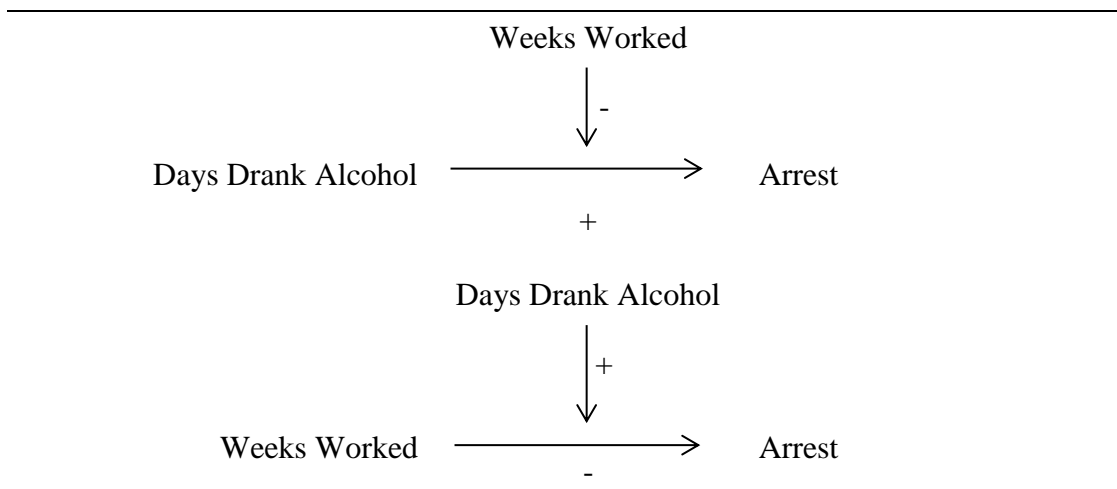


Figure 4.1 Interaction Effects of Days of Drinking and Weeks Worked on Arrest

Figure 4.2 presents a more detailed look at the nature of the interaction. Although increasing the percent of weeks worked reduces predicted arrest, the rates of arrest in general are higher for those who drink more frequently. In contrast, rates of arrest are notably lower for those who report having not drank at all in the previous month. Specifically, the effect of days drank is the greatest for those who did not work at all and

the smallest for those who worked every week since the previous interview. Thus, failing to account for an interaction between weeks worked and drinking days skews perceptions of the relationship.

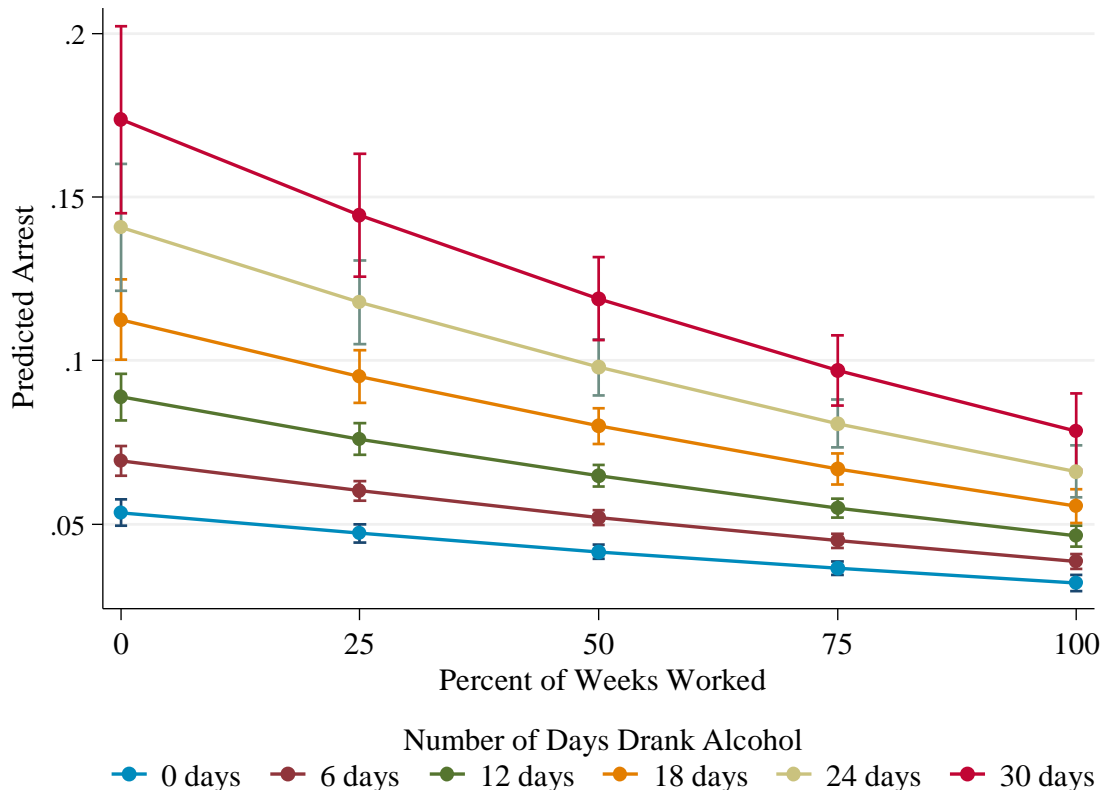


Figure 4.2 Interaction Effects of Days of Drinking and Weeks Worked on Arrest

To further illustrate the importance of exploring interaction effects, Figure 4.3 provides the effect of weeks worked and drinking days individually. Without considering the effect of drinking days, weeks worked appears to have a sizeable impact on arrest such that those who have not worked at all have notably higher rates of arrest than those who have worked at least some in the year prior. The converse is true for drinking frequency such that those who have not drunk any days of the previous month are markedly less likely to be arrested. Figure 4.3 clearly illustrates the importance of considering the potential interaction between variables.

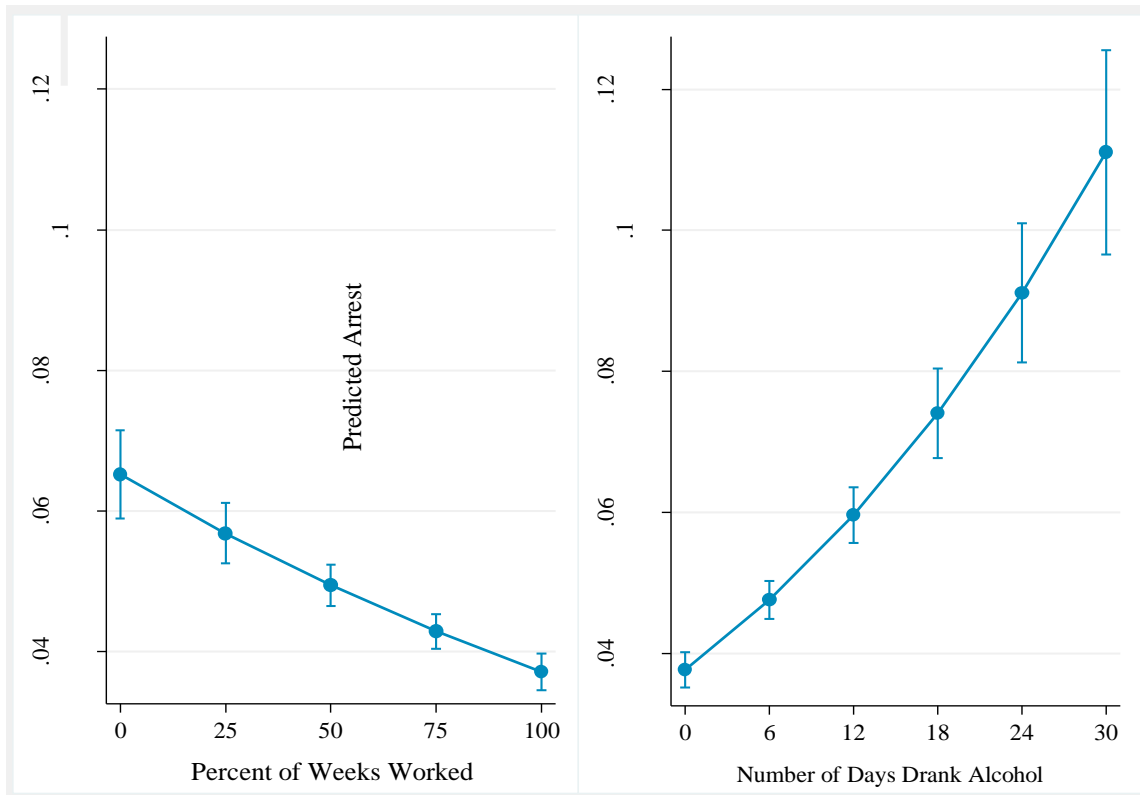


Figure 4.3 Individual Effects of Weeks Worked and Alcohol on Arrest

Table 4.9 presents the regression results for the two-level logistic regression models for the effects of binge drinking and weeks worked on arrest. Model 1 presents the null model without any parameters and Model 2 presents the main effects of each variable on arrest. Similar to results in Table 4.7, weeks worked is inversely related to arrest ($b=-0.66$, $p<0.01$) while binge drinking is directly related to arrest. For each additional day of binge drinking, the odds of arrest rise by about 7%.

Table 4.9 The Effects of Binge Drinking and Employment on Arrest: Multilevel Logit Model Estimates ($n=8,110$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) IRR	<u>Model 3</u> <i>b</i> (Std. Error) IRR
Days binge drank		0.07 (0.04)** 1.07	0.09 (0.01)** 1.10
Weeks worked		-0.01 (0.00) ** 0.99	-0.01 (0.00) ** 0.99
Age		-0.07 (0.01)** 0.94	-0.07 (0.01)** 0.94
Black		0.09 (0.07) 1.10	0.10 (0.07) 1.10
Hispanic or mixed		-0.20 (0.09)* 0.82	-0.20 (0.09)* 0.82
Male		1.11 (0.07)** 3.05	1.12 (0.08)** 3.05
Some high school		-0.87 (0.11)** 0.42	-0.87 (0.11)** 0.42
Some college		-0.75 (0.07)** 0.47	-0.76 (0.11)** 0.47
College graduate or more		-1.52 (0.07)** 0.22	-1.53 (0.07)** 0.22
Married		-0.89 (0.11)** 0.41	-0.90 (0.11)** 0.41
Separated, Divorced, or Widowed		0.52 (0.13) 1.68	0.52 (0.14) 1.68
Number of Kids		0.21 (0.04)** 1.23	0.21 (0.04)** 1.23
Weeks Worked x Binge ^a			-0.04 (0.01) **
Observation-Level Intercept	-3.05 (0.04)**	-7.15 (0.12)**	-7.20 (0.13)**
Individual-Level Variance	7.04 (0.25)	1.60 (0.10)	1.60 (0.10)

* $p<0.05$; ** $p<0.01$

^a Regression coefficient and standard error have been multiplied by 100.

Model 3 in Table 4.9 indicates a significant interaction effect between binge drinking and weeks worked on arrest. For each additional day of binge drinking, the protect effect of work on arrest is reduced. Conversely, for every additional week

worked, the negative effect of binge drinking on arrest is lessened. This interaction is presented in Figure 4.4.

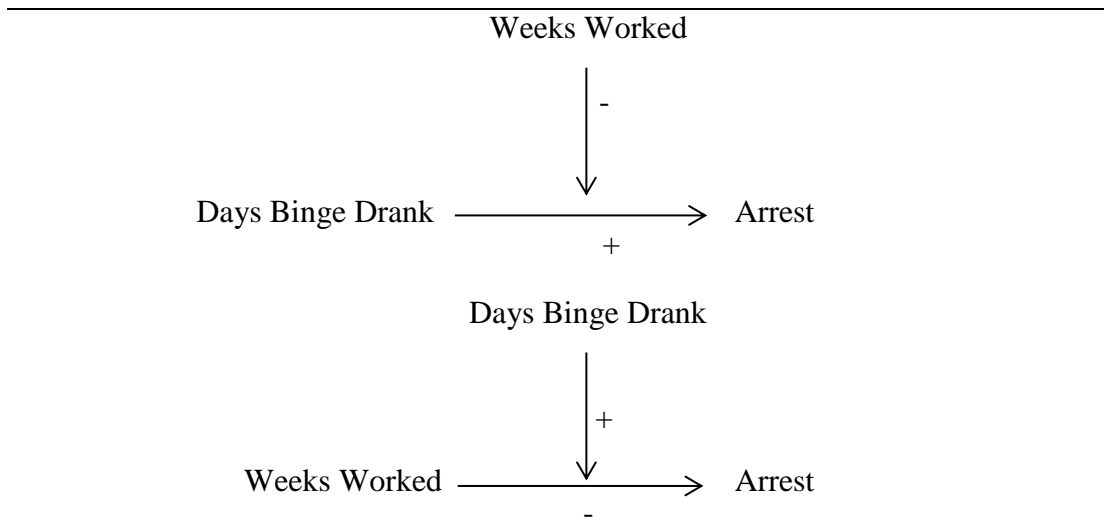


Figure 4.4 Theoretical Model of Interaction Effects of Binge Drinking and Weeks Worked on Arrest

Figure 4.5 further demonstrates the nature of the interaction between binge drinking and weeks worked. The pattern is similar to that observed in the interaction between drinking alcohol and the percent of weeks worked (see Figure 4.3) such that the effect of alcohol is greatest for those who drank more frequently. Graphs of the individual effects of binge drinking and weeks worked are provided in Appendix A.

Multivariate Analyses of Conviction

Table 4.10 and 4.11 present the results from analyses of the effects of days drank, binge drinking, and weeks worked on conviction. Model 1 in both tables presents the null model with no parameters and Model 2 evaluates the direct effects. In both Table 4.10 and 4.11, weeks worked is significant and related to a reduction in arrest by about 0.5% for every 1% increase in the percent of weeks worked. Both days drank ($b=0.03$, $p<0.01$) and binge drinking ($b=0.04$, $p<0.01$) are significantly related to conviction. For each additional day of drinking or binge

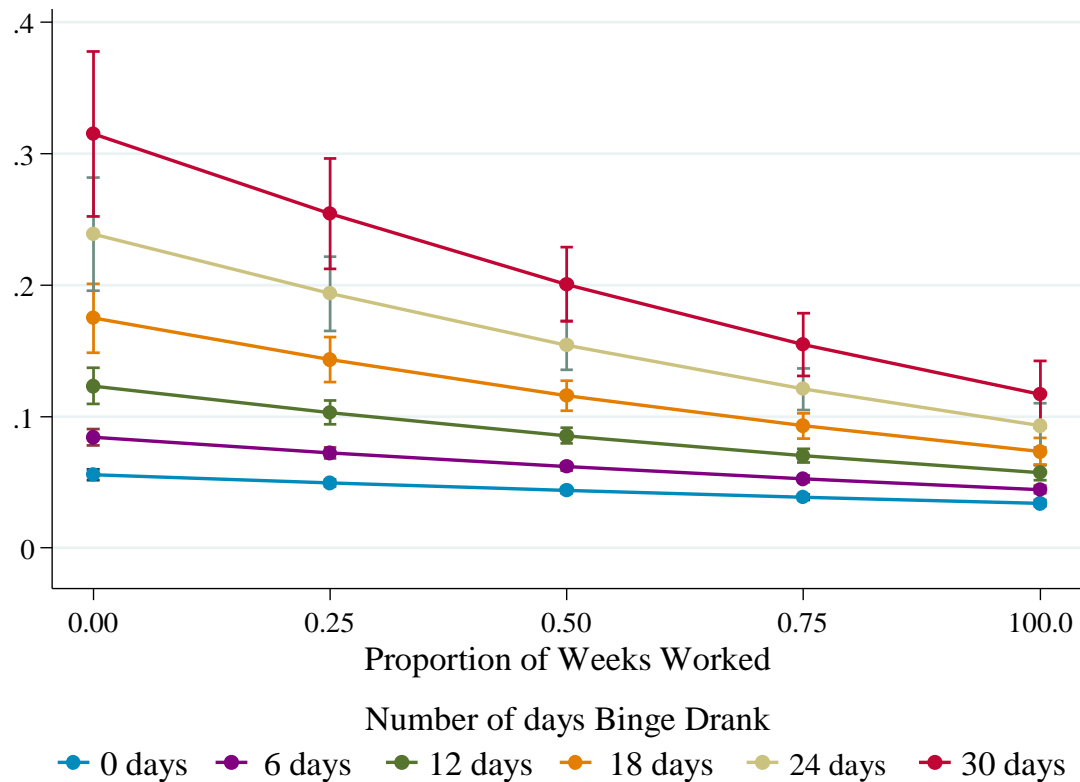


Figure 4.5 Interaction Effects of Binge Drinking and Weeks Worked on Arrest.

drinking, the odds of conviction increase by 3% and 4%, respectively. Model 2 indicates that the odds of conviction for males are more than 200% greater than the odds of conviction for females. Married respondents ($b=-1.00$, $p<0.01$) and those who have graduated college ($b=-1.59$, $p<0.01$) have markedly fewer convictions than those who are single and have less education. However, increasing respondents' number of children by one actually increases the odds conviction by 25%.

Table 4.10 The Effects of Alcohol and Employment on Conviction: Multilevel Logit Model Estimates ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) IRR	<u>Model 3</u> <i>b</i> (Std. Error) IRR
Days drank		0.03 (0.01)** 1.03	0.04 (0.01)** 1.04
Weeks worked ^a		-0.56 (0.07)** 0.99	-0.66 (0.07)** 0.99
Age		0.01 (0.01) 1.01	0.01 (0.01) 1.01
Black		-0.30 (0.10)** 0.75	-0.30 (0.10)** 0.75
Hispanic or mixed		-0.30 (0.10)** 0.74	-0.30 (0.10)** 0.74
Male		1.20 (0.10)** 3.31	1.20 (0.10)** 3.31
Some high school		-1.14 (0.15)** 0.32	-1.14 (0.15)** 0.32
Some college		-0.75 (0.09)** 0.47	-0.75 (0.09)** 0.47
College graduate or more		-1.62 (0.11)** 0.20	-1.63 (0.11)** 0.20
Married		-0.85 (0.11)** 0.43	-0.85 (0.11)** 0.43
Separated, divorced, or widowed		0.22 (0.22) 1.24	0.22 (0.22) 1.24
Number of kids		0.23 (0.04) ** 1.25	0.22 (0.04) ** 1.25
Weeks worked x Alcohol ^a			-0.02 (0.01) 0.99
Observation-Level Intercept	-2.86 (0.04)	-7.83 (0.14)**	-7.88 (0.15)**
Individual-Level Intercept	7.42 (0.24)	2.45 (0.15)	2.45 (0.15)

* $p < 0.05$; ** $p < 0.01$

^a Regression coefficient and standard error have been multiplied by 100.

One key distinction from the arrest analyses is that race is significant in these models. Being black, Hispanic, or mixed races reduces the odds of conviction by about 25% compared to being white. The effect of race is slightly greater in the model that

includes binge drinking than in the days drank model. Indeed, all of the measures have a slightly larger effect on conviction in the binge drinking model (see Table 4.11).

Table 4.11 The Effects of Binge Drinking and Employment on Conviction: Multilevel Logit Model Estimates ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error) OR	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Binge drinking		0.05 (0.01)** 1.06	0.07 (0.01)** 1.08
Weeks worked ^a		-0.01 (0.00)** 0.99	-0.01 (0.00)** 0.99
Age		0.01 (0.01) 1.01	0.01 (0.01) 1.01
Black		-0.26 (0.10)** 0.78	-0.25 (0.10)** 0.78
Hispanic or mixed		-0.29 (0.11)** 0.75	-0.29 (0.11)** 0.75
Male		1.16 (0.09)** 3.20	1.16 (0.09)** 3.20
Some high school		-1.11 (0.15)** 0.33	-1.11 (0.15)** 0.33
Some college		-0.75 (0.08)** 0.47	-0.75 (0.08)** 0.47
College graduate or more		-1.61 (0.10)** 0.20	-1.62 (0.10)** 0.20
Married		-0.82 (0.11)** 0.44	-0.83 (0.11)** 0.44
Separated, divorced, or widowed		0.24 (0.21) 1.27	0.24 (0.21) 1.27
Number of kids		0.22 (0.04) ** 1.24	0.22 (0.05) ** 1.24
Weeks worked x binge drinking ^a			-0.03 (0.02)* 0.97
Observation-Level Intercept	-7.80 (0.14)	-7.80 (0.14)**	-7.85 (0.14)**
Individual-Level Intercept	2.39 (0.15)	2.37 (0.15)	2.40 (0.15)

* $p < 0.05$; ** $p < 0.01$

^a Regression coefficient and standard error have been multiplied by 100.

In addition to the direct effects observed in Table 4.11, it appears that the proportion of weeks worked interact with binge drinking to significantly impact conviction. That is, the proportion of weeks worked lessens the impact of binge drinking

days on conviction. At the same time, additional days of binge drinking reduces the protective effect of weeks worked on conviction. This relationship is presented in Figure 4.6 and Figure 4.7 provides a more detailed look at the nature of the interaction. As was the case for arrest (see Figure 4.5), the effect of working is more salient for those who binge drink the least frequently. More specifically, the likelihood of conviction for respondents who never binge drank is dramatically reduced with increases in weeks worked. On the other hand, for those who report no binge drinking, the impact of weeks worked on conviction is nearly indiscernible.

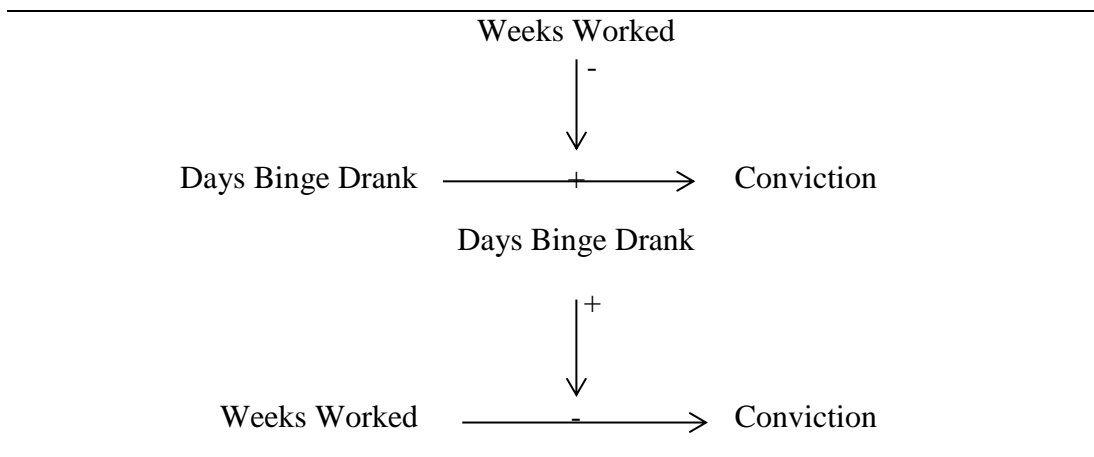


Figure 4.6. Theoretical Model of the Interaction Effects of Days of Binge Drinking and Weeks Worked on Conviction

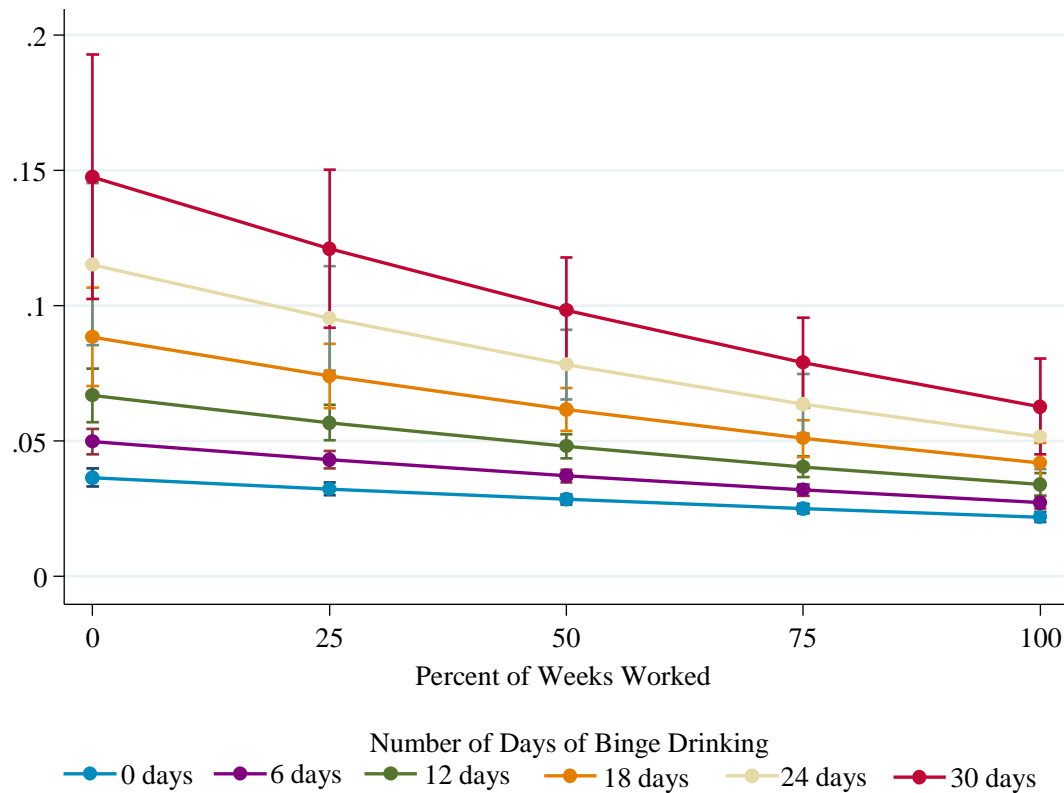


Figure 4.7 Interaction Effects of Days of Binge Drinking and Weeks Worked on Conviction.

Multivariate Analyses of Property Offenses

Table 4.12 and Table 4.13 present the results of the two-level logistic regression model to examine the effect of alcohol and employment on property offenses. Model 1 presents the intercept-only model with no parameters. Model 2 presents the main effects of each measure and Model 3 considers the potential interaction between days drank alcohol and weeks worked (Table 4.12) and binge drinking and weeks worked (Table 4.13). Both days drank ($b=0.06$) and binge drinking ($b=0.08$) are significantly related to property offenses ($p<.01$). Every one additional day of drinking alcohol is accompanied by an increased likelihood of property offending. Specifically, the odds of property offenses are 5% higher for each additional day of drinking. Likewise, the likelihood of

property offenses increases by about 8% for every additional day respondents report binge drinking.

Weeks worked impacts the likelihood of property offenses such that a 1% increase in weeks worked (about half a week or 3.5 days) results in a 0.03% reduction in the odds of property offenses. That is, every additional week worked decreases the number of property crimes by about 0.6%. To put this in clearer terms, working an additional month (4.25 weeks) reduces the odds of property offending by slightly more than 2.5%. Unlike previous models, weeks worked is significant at the .05 level rather than the .01 level. This means that there is a 5% chance that an effect was detected that does not actually exist, or in other words, of making a Type I error. Variables that are significant at the .01 level have a 1% chance of a Type I error. Thus, the results presented in Table 4.12 and 4.13 indicate that there is a 5% chance that the proportion of weeks worked is unrelated to property offending.

Tables 4.12 and 4.13 also indicate a significant effect of marital status on property offenses. Accordingly, being married is associated with a 70% decrease in the odds of property crimes ($p < 0.01$). In addition, being a parent is significantly related to property offending, such that more children actually *increase* the odds of committing a property offense by more than 30% for each additional child ($b = 0.27$, $p < 0.01$). Parental status is also significant in the binge drinking model (see Table 4.13), with each child increasing the odds of property offending by slightly less than 28% ($b = 0.25$, $p < 0.01$). Finally, as was the case for both arrest and conviction, being male is associated with a significant increase in odds of property crimes. Specifically, being male nearly doubles the odds of property offenses compared to being female. There were no significant interaction effects

for either days drank and weeks worked or days binge drank and weeks worked (i.e., the relationships are not moderated).

Table 4.12 The Effects of Alcohol and Employment on Property Offenses: Multilevel Logit Model Estimates. ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Days drank		0.06 (0.00)** 1.06	0.07 (0.00)** 1.07
Weeks worked		-0.27 (0.08)** 0.99	-0.19 (0.08)* 0.99
Age		-0.43 (0.02)** 0.65	-0.43 (0.02)** 0.65
Black		-0.14 (0.09) 0.87	-0.13 (0.09) 0.88
Hispanic or mixed		0.11 (0.10) 0.90	0.11 (0.10) 0.90
Male		0.71 (0.07)** 2.03	0.71 (0.07)** 2.03
Some high school		-0.33 (0.12) ** 0.72	-0.33 (0.12) ** 0.72
Some college		-0.36 (0.11) ** 0.70	-0.37 (0.11) ** 0.69
College graduate or more		-0.55 (0.11)** 0.57	-0.56 (0.11) ** 0.57
Married		-0.70 (0.14)** 0.50	-0.70 (0.14)** 0.50
Separated, divorced, or widowed		-0.03 (0.28) 0.97	-0.03 (0.28) 0.97
Number of Kids		0.28 (0.06)** 1.32	0.28 (0.06)** 1.32
Weeks Worked x Alcohol ^a			-0.02 (0.01) 0.99
Observation-Level Intercept	-2.43 (0.03)**	-8.85 (0.15)**	-8.90 (0.16)**
Individual-Level Intercept	5.61 (0.17)	3.30 (0.20)	3.30 (0.20)

* $p < 0.05$; ** $p < 0.01$

^a Regression coefficient and standard error have been multiplied by 100.

Table 4.13 The Effects of Binge Drinking and Employment on Property Offending: Multilevel Logit Model Estimates. ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error) OR	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Binge drinking		0.08 (0.01)**	0.09 (0.01)**
		1.08	1.08
Weeks worked ^a		-0.24 (0.08) **	-0.21 (0.08)*
		0.99	0.99
Age		-0.42 (0.02)**	-0.42 (0.02)**
		0.66	0.66
Black		-0.11 (0.09)	-0.11 (0.09)
		0.90	0.90
Hispanic or mixed		0.10 (0.10)	0.10 (0.10)
		0.91	0.91
Male		0.68 (0.07)**	0.68 (0.07)**
		1.98	1.98
Some high school		-0.32 (0.12)**	-0.32 (0.12)**
		0.73	0.73
Some college		-0.37 (0.11)**	-0.37 (0.11)**
		0.69	0.69
College graduate or more		-0.54 (0.11)**	-0.55 (0.11)**
		0.58	0.58
Married		-0.68 (0.14)**	-0.68 (0.14)**
		0.51	0.51
Separated, divorced, or widowed		0.02 (0.28)	0.02 (0.28)
		1.02	1.02
Number of kids		0.25 (0.06)**	0.25 (0.06)**
		1.28	1.28
Weeks worked x Binge drinking ^a			-0.01 (0.01)
			0.99
Observation-Level Intercept	-2.43 (0.03)**	-8.75 (0.15)**	-8.75 (0.16)**
Individual-Level Intercept	5.61 (0.17)	3.30 (0.19)	3.30 (0.19)

* $p < 0.05$; ** $p < 0.01$

^a Regression coefficient and standard error have been multiplied by 100.

Multivariate Analyses of Violent Offenses

Results from the two-level negative binomial models for violent offenses are presented in Tables 4.14 and 4.15. Model 1 presents the null model with no parameters, thus providing only the random intercept for violent offending. Table 4.14 indicates that

both days drank alcohol and the percentage of weeks worked is significantly related to violent offending. Accordingly, for every additional day of drinking, the odds of violent offending increase by about 6% ($b=0.06$, $p<0.01$). Conversely, each additional week worked reduces violent offending by 1% ($b=-0.45$, $p<0.01$). Age, race, sex, education, and parental status also significantly affect violent offending. Every additional year in age is related to a 26% decrease in the odds of violent offending ($b=-0.30$, $p<0.01$). Having attended some college reduces the odds of violence by more than 40% ($b=-0.57$, $p<0.01$) and graduating college is related to a 75% decrease in property crime ($b=-1.40$, $p<0.01$). Further, odds of property offending for those enrolled in high school are half of those who are not enrolled and do not have a high school degree or GED ($p<0.01$).

In contrast to the protective effects of age and education, being black ($b=0.45$, $p<0.001$), male ($b=0.81$, $p<0.01$), and having children ($b=0.17$, $p<0.01$) are all positively related to violence both statistically and substantively. Being black increases the odds of violence by more than 55% and being male more than doubles those odds. Although parental status affects violence less strikingly, the nearly 20% increase in odds of violence with each additional child is not without substantive significance. Only being divorced, separated, or widowed and being Hispanic or mixed race is not significantly related to violence. Table 4.14 also shows that the proportion of weeks worked and the number of days drank do not significantly interact to affect violence.

Table 4.15 presents the results from the two-level negative binomial model of the effects of binge drinking and weeks worked on violent offending. Model 1 is the null model with no parameters. Model 2 indicates a significant positive effect of binge drinking on violence ($b=0.09$, $p<0.01$) such that one additional day of binge drinking

Table 4.14 The Effects of Alcohol and Employment on Violent Offending: Multilevel Logit Model Estimates. ($n = 8,636$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Days drank		0.06 (0.00)** 1.06	0.06 (0.00)** 1.07
Weeks worked ^a		-0.45 (0.09) ** 0.99	-0.45 (0.09)** 0.99
Age		-0.30 (0.02)** 0.74	-0.30 (0.02)** 0.74
Black		0.45 (0.10)** 1.57	0.45 (0.10)** 1.58
Hispanic or mixed		0.14 (0.12) 1.15	0.14 (0.11) 1.15
Male		0.81 (0.10)** 2.24	0.81 (0.10)** 2.24
Some high school		-0.68 (0.13)** 0.51	-0.68 (0.13)** 0.51
Some college		-0.57 (0.10) ** 0.57	-0.57 (0.10)** 0.57
College graduate or more		-1.40 (0.12)** 0.25	-1.40 (0.12)** 0.25
Married		-0.60 (0.12) ** 0.55	-0.60 (0.12) 0.55
Separated, divorced, or widowed		0.04 (0.25) 1.04	0.04 (0.24) 1.04
Number of kids		0.17 (0.07)** 1.18	0.17 (0.06)** 1.18
Weeks worked x Days drank ^a			-0.01 (0.01) 1.00
Observation-level intercept	-2.79 (0.05)**	-8.78 (0.17)**	-8.79 (0.17)**
Individual-level intercept	7.16 (0.24)	3.44 (0.18)	3.44 (0.18)

* $p < 0.05$; ** $p < 0.01$

^a Regression coefficient and standard error have been multiplied by 100.

increases violent offending by almost 10%. In contrast, weeks worked is inversely related to violence, but the relationship is not significant. Age and education level are related to lower levels of violence while race, sex, and parental status are positively related to violent offending. Most notably, being male ($b=0.75$, $p<0.01$) and being black ($b=0.53$, $p<0.01$) substantially increase. Model 3 includes an interaction term to capture possible

moderation between days of binge drinking, proportion of weeks worked, and violent offending. Accordingly, the number of weeks worked and days of binge drinking do not interact to significantly impact violent offending.

Table 4.15 The Effects of Binge Drinking and Employment on Violent Offending: Multilevel Logit Model Estimates ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error) OR	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Binge drinking		0.09 (0.01)**	0.09 (0.01)**
		1.09	1.09
Weeks worked		-0.46 (0.09)**	-0.46 (0.09)**
		0.99	0.99
Age		-0.29 (0.02)**	-0.29 (0.02)**
		0.75	0.75
Black		0.49 (0.10)**	0.49 (0.10)**
		1.63	1.63
Hispanic or mixed		0.16 (0.11)	0.16 (0.11)
		1.17	1.17
Male		0.78 (0.10)**	0.78 (0.10)**
		2.18	2.18
Some high school		-0.67 (0.13)**	-0.67 (0.13)**
		0.5	0.51
Some college		-0.56 (0.10)**	-0.56 (0.10)**
		0.57	0.57
College graduate or more		-1.37 (0.12)**	-1.37 (0.12)**
		0.25	0.25
Married		-0.54 (0.12)**	-0.54 (0.12)**
		0.58	0.58
Separated, divorced, or widowed		0.09 (0.23)	0.09 (0.23)
		1.10	1.10
Number of Kids		0.14 (0.07)**	0.14 (0.07)**
		1.15	1.15
Weeks worked x Binge drinking ^a			0.00 (0.01)
			1.00
Observation-Level Intercept	-5.92 (0.10)**	-8.65 (0.17)**	-8.65 (0.17)**
Individual-Level Intercept	1.82 (0.25)	3.35 (0.17)	3.35 (0.17)

* $p < 0.05$; ** $p < 0.01$

^a Regression coefficient and standard error have been multiplied by 100.

Multivariate Analysis of Days Drank Alcohol

Table 4.16 presents the results of the two level negative binomial models for days drank alcohol. Model 1 is the null model with no parameters to estimate the between individual variance. Model 2 indicates all four offending variables significantly impact the number of drinking days, with violence having the largest effect ($b=0.32$, $p<0.01$). For each additional violent offense, the number of days drank increases by nearly 40%. The number of arrests ($b=0.24$, $p<0.01$) and property offenses ($b=0.23$, $p<0.01$) also result in an increase in drinking, but the effects are smaller (about 25%). In contrast, conviction actually reduces drinking days by about 11% per conviction ($b= -0.11$, $p<0.01$).

Following the trend of earlier models, nearly every measure is statistically significant with the exception of having attended some college and being separated, divorced, or widowed. Being black ($b= -0.77$, $p<0.01$) and being male ($b=0.40$, $p<0.01$) have the largest effects of all the control measures. While being black lessens the number of days drank by about 55%, being male increases drinking by about 50%.

Model 3 also presents the interactions between offending variables and weeks worked on the number of drinking days in the month prior to the interview. The results show no significant interaction between arrest and weeks worked or between violent offending and weeks worked.

Table 4.16 The Effects of Employment and Offending on Alcohol Consumption:
Multilevel Negative Binomial Model Estimates. ($n = 8,217$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) IRR	<u>Model 3</u> <i>b</i> (Std. Error) IRR
Weeks Worked ^a		0.24 (0.03)** 1.01	0.24 (0.02)** 1.00
Arrest		0.24 (0.04)** 1.27	0.28 (0.07)** 1.32
Conviction		-0.11 (0.04)** 0.89	-0.11 (0.04)** 0.89
Property Offense		0.23 (0.03)** 1.26	0.23 (0.03)** 1.26
Violent Offense		0.32 (0.03)** 1.37	0.34 (0.06)** 1.41
Age		0.07 (0.00)** 1.07	0.07 (0.00)** 1.07
Black		-0.77 (0.05)** 0.46	-0.77 (0.05)** 0.46
Hispanic or mixed		-0.26 (0.04)** 0.77	-0.26 (0.04)** 0.77
Male		0.40 (0.04)** 1.49	0.40 (0.04)** 1.49
Some high school		-0.39 (0.06)** 0.67	-0.39 (0.05)** 0.67
Some college		0.13 (0.05)** 1.14	0.13 (0.05)** 1.14
College graduate or more		0.35 (0.05)** 1.42	0.35 (0.05)** 1.43
Married		-0.23 (0.02)** 0.80	-0.23 (0.02)** 0.80
Separated, Divorced, or Widowed		-0.04 (0.06) 1.04	-0.03 (0.06) 1.04
Number of Kids		-0.15 (0.02)** 0.86	-0.15 (0.02)** 0.86
Arrests x Weeks Worked ^a			-0.06 (0.07) 1.00
Violent Offenses x Weeks Worked ^a			-0.05 (0.07) 1.00
Observation-Level Intercept	0.92 (0.03)**	-3.37 (0.14)**	-3.38 (0.13)**
Individual-Level Intercept	1.73 (0.06)	1.45 (0.05)	1.45 (0.05)

* $p < 0.05$; ** $p < 0.01$

^a Regression coefficient and standard error have been multiplied by 100

Multivariate Analysis of Days Binge Drank

Table 4.17 present the results of a two-level negative binomial model of binge drinking days reported in the month prior to the interview. Model 2 indicates that violence has the largest effect on binge drinking, increasing the number of binge drinking days by a factor of 1.5 with every one additional violent offense ($b=0.39$, $p<0.01$). In other words, for each additional violent offense, the number of binge drinking days increases by 50%. The number of arrests and property offenses also increase binge drinking, albeit with somewhat smaller effect sizes. Each additional arrest increases binge drinking days by more than 30% ($b=0.29$, $p<0.01$) and each additional property offense increases the number of binge drinking days by nearly 40% ($b=0.33$, $p<0.05$).

Although there are fewer significant demographic predictors of binge drinking than for drinking in general, three of the four offending variables significantly impact binge drinking. Conviction is the only offending measure that is not statistically significant. Further, it is not only insignificant, but it is in the opposite direction from the other offending variables. To see if perhaps conviction is crucial in some manner to understanding binge drinking, interaction effects between conviction and weeks worked are explored. Further, because violent offending has the largest effect on days of binge drinking, violence is also considered as a potential moderator. Model 3 in Table 4.17 presents results from the analysis. Accordingly, violence and weeks worked do not interact in their impact on binge drinking. In contrast, it also indicates that despite a lack of a significant main effect, a significant interaction effect does exist between conviction and weeks worked on the number of binge drinking days. The interaction between conviction and weeks worked will be discussed below.

Table 4.17 The Effects of Employment and Offending on Binge Drinking:
Multilevel Negative Binomial Model Estimates. ($n = 8,266$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) IRR	<u>Model 3</u> <i>b</i> (Std. Error) IRR
Weeks Worked		0.21 (0.04)** 1.23	0.22 (0.04)** 1.00
Number of arrests		0.29 (0.05)** 1.34	0.29 (0.05)** 1.32
Convictions		-0.11 (0.05) 0.89	-0.11 (0.10) 0.89
Property offenses		0.33 (0.04)* 1.39	0.33 (0.04)** 1.39
Violent Offenses		0.39 (0.04)** 1.48	0.34 (0.08)** 1.41
Age		0.02 (0.01) 1.02	0.02 (0.01)** 1.02
Black		-1.26 (0.07)** 0.29	-1.26 (0.07)** 0.29
Hispanic or mixed		-0.31 (0.0)** 0.74	-0.31 (0.06)** 0.74
Male		0.94 (0.05)** 2.55	0.94 (0.05)** 2.55
Some high school		-0.62 (0.07)** 0.54	-0.61 (0.07)** 0.54
Some college		-0.09 (0.06)* 1.09	0.09 (0.06) 1.10
College graduate or more		0.22 (0.08)** 1.24	0.22 (0.06)** 1.25
Married		-0.50 (0.04)** 0.61	-0.50 (0.06)** 0.61
Separated, Divorced, or Widowed		0.09 (0.10) 1.10	0.09 (0.10) 1.10
Number of Kids		-0.13 (0.02)** 0.88	-0.13 (0.03)** 0.88
Conviction x Weeks Worked ^a			-0.25 (0.12)* 0.78
Violence x Weeks Worked ^a			-0.09 (0.10) 1.10
Observation-Level Intercept	-0.63 (0.05)	-4.96 (0.02)**	-4.97 (0.10)**
Individual-Level Intercept	3.58 (0.11)	2.84 (0.10)	2.84 (0.10)

* $p < 0.05$; ** $p < 0.01$

As presented in Figure 4.8, the effect of conviction on binge drinking is dependent upon the percent of weeks worked (and vice versa). More specifically, conviction lessens the positive effect of weeks worked on binge drinking and weeks worked reduces the negative effect of conviction on binge drinking (see Figure 4.8). This is further illustrated in Figure 4.9 which provides a graphical depiction of the interaction.

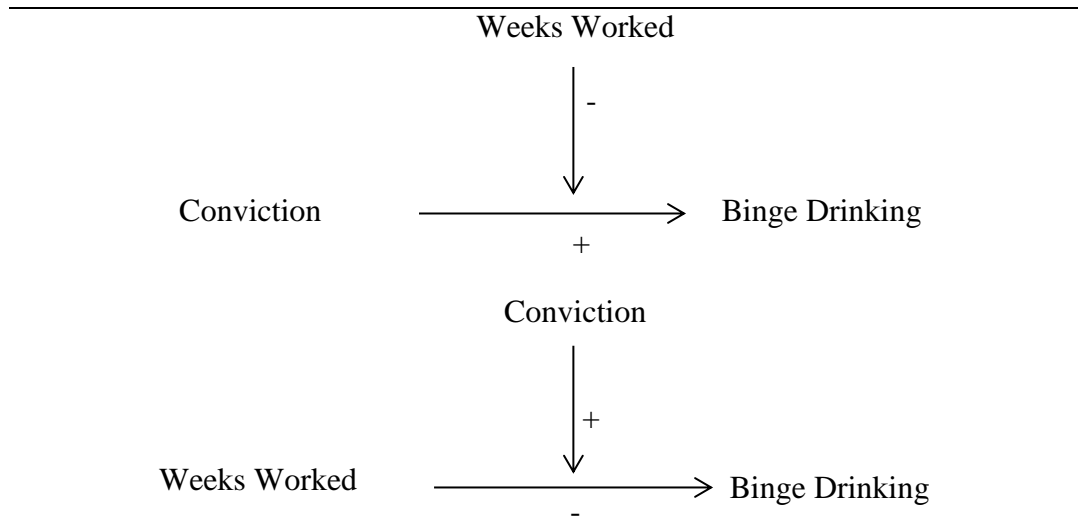


Figure 4.8 Interaction Effects of Conviction and Weeks Worked on Binge Drinking

Figure 4.9 indicates that the percent of weeks worked has a relatively small impact on binge drinking for those who have been convicted of at least one crime. For the convicted, increasing the amount of weeks worked since the last interview from 0% to 100% reduces the frequency of binge drinking by about 0.1 days. In contrast, respondents who were not convicted and worked all of the weeks since the last interview had higher levels of binge drinking than those who did not work at all. Increasing the percent of weeks worked from 0% to 100% among those without a conviction also increases the days of binge drinking about one day.

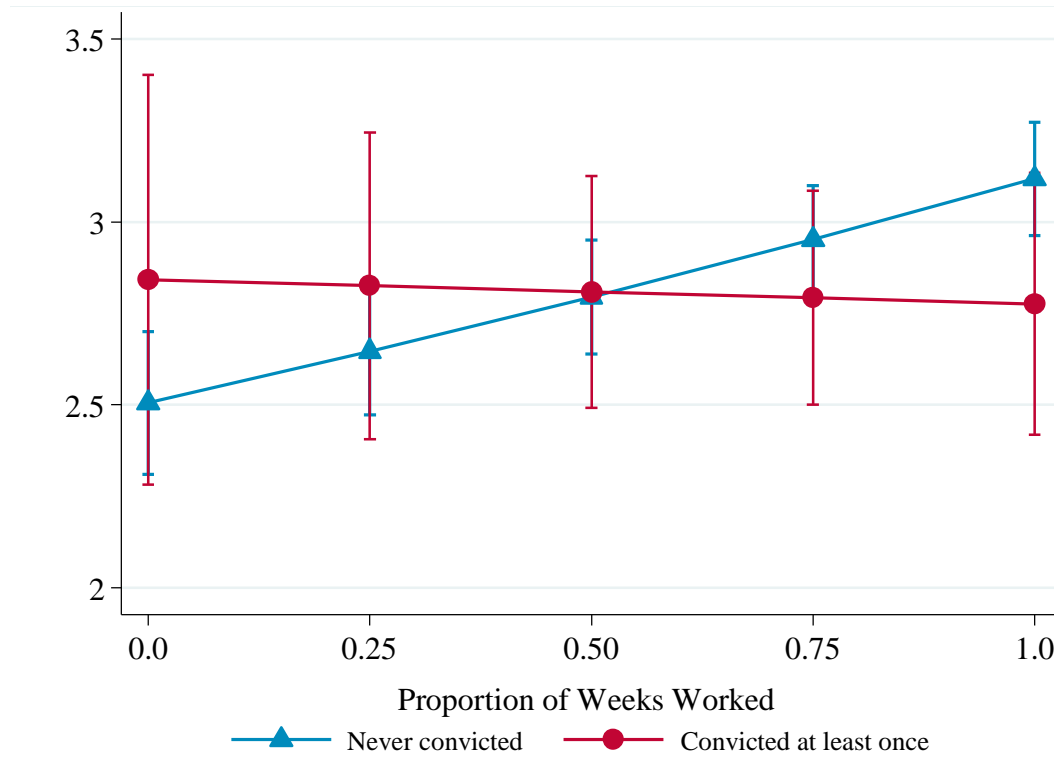


Figure 4.9 Interaction effects of Conviction and Weeks Worked on Binge Drinking

Table 4.18 presents results from the two-level generalized linear model for weeks worked. Model 1 presents the null model with no parameters. Model 2 includes the effect of the number of days of drinking alcohol, while Model 3 includes the effect of days of binge drinking. The number of days drank significantly effects weeks worked ($b=0.01$, $p<0.01$), which equates to an increase of 2%, or approximately one week for an additional day of drinking. The effect of binge drinking is smaller, but is nonetheless statistically and substantively significant. Specifically, one additional binge drinking day results in an increase of less than 1 week ($b=0.00$, $p<0.05$). In addition, the number of arrests is significantly related to weeks worked such that a one unit increase in arrest results in a decrease of 2% ($b=-0.02$, $p<0.05$). There were no significant effects of conviction, property offending, or violent offending on weeks worked. Because

Table 4.18 The Effects of Drinking and Offending on Employment: Multilevel Generalized Linear Model Estimates ($n = 7,855$)

	<u>Model 1</u> <i>b</i> (Std. Error) IRR	<u>Model 2</u> <i>b</i> (Std. Error) IRR	<u>Model 3</u> <i>b</i> (Std. Error) IRR
Number of arrests		-0.02 (0.01)* 0.98	-0.02 (0.01)* 0.98
Convictions		-0.03 (0.02) 0.97	-0.03 (0.02) 0.97
Property offenses		0.02 (0.01) 1.02	0.02 (0.01) 1.02
Violent Offenses		-0.03 (0.03) 0.97	-0.03 (0.03) 0.97
Days Drank		0.01 (0.00)** 1.01	
Binge Drinking			0.00 (0.00)* 1.00
Age		0.05 (0.00)** 1.05	0.05 (0.00)** 1.05
Black		-0.19 (0.03)** 0.83	-0.20 (0.03)** 0.82
Hispanic or mixed		-0.05 (0.03) 0.95	-0.05 (0.03)* 0.95
Male		-0.01 (0.01) 0.99	-0.01 (0.01) 0.99
Some high school		0.05 (0.04) 1.06	0.05 (0.04) 1.05
Some college		0.20 (0.04)** 1.22	0.20 (0.04)** 1.22
College graduate or more		0.19 (0.04)** 1.20	0.19 (0.04)** 1.20
Married		-0.05 (0.02)** 0.96	-0.05 (0.02)** 0.96
Separated, Divorced, or Widowed		-0.08 (0.03)* 0.92	-0.09 (0.03) 0.92
Number of Kids		-0.14 (0.01)** 0.87	-0.14 (0.01)** 0.87
Observation-Level Intercept ^a	3.54 (0.01)**	-1.51 (0.06)**	-1.52 (0.06)**
Individual-Level Intercept ^a	0.21 (0.02)	0.19 (0.02)	0.19 (0.02)

* $p < 0.05$; ** $p < 0.01$

^a Only the coefficient and standard error are provided for the intercepts.

the effects of alcohol, binge drinking, and arrest are relatively minor and the other offending variables are not significant, exploration into interaction effects are not warranted for weeks worked. However, the effects of a few variables are worth noting.

Unlike previous models where nearly all variables were significant, the results in Table 4.18 indicate that few measures significantly affect the outcome variable (weeks worked). Specifically, age ($b=0.05$, $p<0.01$) and education level (having attending some college ($b=0.20$, $p<0.01$), and having graduated college, ($b=0.19$, $p<0.01$) significantly increase weeks worked. In contrast, marital status, parental status, and race significantly reduce weeks worked. Being married ($b=-0.05$, $p<0.01$) results in a 5% decrease in weeks worked and for each additional child, weeks worked decreases by about 13% ($b=-0.14$, $p<0.01$). Being black significantly affects weeks worked, reducing it by over 15% ($b=-0.19$, $p<0.01$) compared to being white. The effect of being male on weeks worked is not significant, thus suggesting rates of work are about the same for males and females.

Mediation Analyses

It is possible that the relationships observed in the previous analyses are explained by an intervening variable. To consider this, mediation analyses are performed following the four-step approach outlined by Barron and Kenny (1986) to evaluate the relationship between:

1. the independent variable (X) and the dependent variable (Y) (path c),
2. X and the mediating variable (M) (path a),
3. M and Y without X in the model (path b), and
4. M and Y controlling for X (path c').

In addition, a regression coefficient is calculated for the indirect effect which indicates the effect on Y for every one unit change in X that is mediated by M. It is tested for significance using the Sobel product of coefficients approach.

Drawing on the theoretical foundation that underlies the alcohol, employment, and offending connection, mediation analyses are performed using a series of variables as the mediator. Recall that both theory and research have suggested that each of the relationships may be reciprocal. Consequently, it is possible that alcohol use may mediate the work-crime connection and that the relationship between alcohol and crime may be mediated by employment.

First, the role of weeks worked as a potential mediator between alcohol consumption and offending is considered. Next, the mediating effects of alcohol will be explored in the relationship between employment and offending. Last, alcohol as a mediator of the reciprocal effects of offending on employment will be evaluated. It is important to note that the mediation steps should not be defined in terms of statistical significance, but rather in terms of zero and nonzero coefficients. Mediation analyses are sensitive to sample size and thus trivial coefficients can be significant strictly because of statistical power rather than magnitude of effect (Kenny, 2016). Thus, while significance can inform our understanding of a relationship, there are other strategies to more effectively evaluate partial mediation. To quantify the magnitude of partial mediation, both the percentage reduction from c' to c and the size of the indirect effect (ab) should be involved. More specifically, partial mediation exists when $0 < c' < c$ (assuming $c > 0$), and/or the indirect effect is significantly greater than zero ($0 < ab$). Table 4.19 presents the results from the Sobel-Goodman tests used to test whether employment carries the effect of drinking and/or binge drinking to offending. The percent of the total effect mediated by weeks worked is provided in the far right column. Accordingly, weeks worked does not appear to substantially mediate any of the drinking variables effects on

any of the offending variables. Weeks worked accounts for about 5% of the total effect of alcohol on conviction and alcohol on arrest. However, employment accounts for about 2% or less of the total effects in the rest of the alcohol-crime relationships.

Table 4.20¹² presents the analyses of the alcohol variables as mediators between employment and offending and suggests that alcohol mediates the effect of weeks worked on all four offending variables at least to some extent. The smallest mediation effect is that of binge drinking on arrest (6.8%) while in the case of the effect of weeks worked on property crime, days drinking accounts for almost 40% of the total effect. According to the results from the bootstrap test put forth by Preacher and Hayes (2004, 2008), the number of days drank significantly mediates the effect of the percent of weeks worked on arrest, conviction, property offending, and violent offending. Specifically, the number of drinking days carries over 10% of the effect of weeks worked on arrest, slightly less than 10% on conviction, nearly 40% of the employment's effect on property crime, and more than 25% of the effect on violent crime. Likewise, the number of binge drinking days significantly mediates the effect of weeks worked on property and violent offending accounting for more than 25% and 16% (respectively) of the effect of weeks worked. Binge drinking also significantly mediates the effect of weeks worked on arrest and conviction, but the effects are notably smaller.

¹² Note that all values in Table 4.20 except path *a* have been multiplied by 100 to facilitate interpretation and clarity. The mediated effect is initially provided as a proportion and thus multiplying it by 100 allows for discussion in terms of percentage.

Table 4.19. Mediating effects in the relationship between alcohol and offending ^a

	Path <i>c</i> X → Y	Path <i>a</i> X → M	Path <i>b</i> M → Y	Path <i>c</i> ' X → Y	Path <i>ab</i> X → Y	Mediated Effect (%)
	<i>Total effect</i>	β_a	β_b	<i>Direct Effect</i>	<i>Indirect Effect</i>	
Alcohol → Weeks Worked → Arrest	0.28	0.397	-0.034	0.294	-0.014	4.8
Alcohol → Weeks Worked → Conviction	0.14	0.397	-0.019	0.149	-0.008	5.3
Alcohol → Weeks Worked → Property Crime	0.33	0.397	-0.013	0.328	-0.005	1.6
Alcohol → Weeks Worked → Violent Crime	0.27	0.397	-0.015	0.275	-0.006	2.2
Binge → Weeks Worked → Arrest	0.55	0.375	-0.033	0.558	-0.0012	2.3
Binge → Weeks Worked → Conviction	0.34	0.375	-0.019	0.348	-0.007	2.1
Binge → Weeks Worked → Property Crime	0.64	0.375	-0.012	0.641	-0.005	0.7
Binge → Weeks Worked → Violent Crime	0.52	0.375	-0.014	0.527	-0.005	1.0

^a Coefficients for path *c*, path *b*, path *c*', and path *ab* have been multiplied by 100.

Table 4.20 Mediating effects of alcohol in the relationship between employment and offending. ^a

	Path <i>c</i> X → Y	Path <i>a</i> X → M	Path <i>b</i> M → Y	Path <i>c</i> ' X → Y	Path <i>ab</i> X → Y	Mediated Effect (%)
	<i>Total effect</i>	β_a	β_b	<i>Direct Effect</i>	<i>Indirect Effect^b</i>	
Weeks worked → Alcohol → Arrest	-0.03	0.011	0.294	-0.031	0.003	10.5
Weeks worked → Alcohol → Conviction	0.02	0.011	0.149	-0.019	0.002	9.4
Weeks worked → Alcohol → Property Crime	-0.01	0.011	0.328	-0.013	0.004	38.6
Weeks worked → Alcohol → Violent Crime	-0.01	0.011	0.275	-0.015	0.003	25.1
Weeks worked → Binge → Arrest	-0.03	0.004	0.558	-0.033	0.002	6.8
Weeks worked → Binge → Conviction	-0.02	0.004	0.348	-0.019	0.001	7.5
Weeks worked → Binge → Property Crime	-0.01	0.004	0.641	-0.012	0.002	25.5
Weeks worked → Binge → Violent Crime	-0.01	0.004	0.527	-0.014	0.002	16.0

^a Coefficients for path *c*, path *b*, path *c*', and path *ab* have been multiplied by 100.

Findings from the final set of mediation analyses are presented in Table 4.21. These analyses consider the reciprocal effects of offending on employment and how alcohol use may mediate those connections. The top half of the table uses the number of days drank in the 30 days prior to the interview as the mediator, while the lower half considers the number of days of binge drinking in the month prior. Perhaps the most distinguishing aspect of Table 4.21 are the notably larger effect sizes for each of the paths. This can be deceiving though, because all four offending variables are dichotomous (coded 0=no, 1=yes) while the weeks worked variable is a continuous variable of percentage points. Therefore, it is imperative to stay focused on the portion of the total effect that is mediated as well as the size of the indirect effect (path *ab*) with consideration of the size of the direct effect (path *c'*).

Based on the findings in Table 4.21, it appears as though the relationship between property crime and weeks worked is accounted for by alcohol consumption. The number of days drank carries almost 40% of the effect of conviction on work while days of binge drinking carry over 25%. The effect of weeks worked on violent crime can also be accounted for in part by alcohol use. The number of days drank mediates a quarter of employment's effect on violence and binge drinking accounts for over 15% of the effect. Although the impact of employment on arrest and conviction is largely explained by the direct effects of weeks worked, alcohol mediates about 10% of the effects and binge drinking accounts for around 7%.

Table 4.21 Mediating effects in the relationship between offending and employment. ^a

	Path <i>c</i> X → Y <i>Total effect</i>	Path <i>a</i> X → M β_a	Path <i>b</i> M → Y β_b	Path <i>c'</i> X → Y <i>Direct Effect</i>	Path <i>ab</i> X → Y <i>Indirect Effect^b</i>	Mediated Effect (%)
Arrest → Alcohol → Weeks Worked	-9.11	2.30	0.43	-10.08	0.98	10.7
Conviction → Alcohol → Weeks Worked	-8.0	1.80	0.41	-8.69	0.73	9.2
Property Crime → Alcohol → Weeks Worked	-4.82	2.74	0.41	6.05	1.12	39.2
Violent Crime → Alcohol → Weeks Worked	-4.82	2.99	0.41	-6.05	1.23	25.6
Arrest → Binge → Weeks Worked	-9.14	1.63	0.43	-9.383	0.70	7.6
Conviction → Binge → Weeks Worked	-8.01	1.57	0.40	-8.65	0.64	7.9
Property Crime → Binge → Weeks Worked	-2.90	196	0.40	-3.68	0.78	27.0
Violent Crime → Binge → Weeks Worked	-4.98	2.11	0.41	-5.83	0.85	14.6

^a Coefficients in this table have *not* been multiplied by 100.

CHAPTER 5

DISCUSSION

In an effort to better understand why some people engage in crime, a number of correlates have been identified in the extant literature. Although criminologists have identified both substance use and employment among such correlates, it has been unclear how exactly these two forces fit together, if in fact they do at all. Thus, the overarching purpose of the current study is to bring together theoretical and empirical understandings of how drugs and employment impact crime. To do so, this study has two main goals: 1) contribute to the extant literature regarding the individual effects of alcohol consumption and employment on offending, and 2) tie research on alcohol and crime together with research on employment and crime by exploring interactions between these key variables and considering how mediation forces may be underlying the relationships.

Using a nationally representative and longitudinal sample of Americans aged 18 to 27, this study focuses on disentangling the nuances of the multidirectional relationships between alcohol consumption, employment, and offending. In the pages that follow, relevant research findings will be summarized, offering a foundation for beginning to understand the nature of the alcohol-employment-crime connection. Limitations of the current research will then be discussed, followed by recommendations for future research. The chapter will conclude with a brief discussion of policy implications. For purposes of clarity, this chapter will be organized around the seven

hypotheses outlined at the end of Chapter 3. To reiterate, the seven hypotheses posited by this study include:

- H1. Alcohol use¹³ affects the likelihood of offending.
- H2. Employment affects the likelihood offending.
- H3. Substance use and employment are related to each other.
- H4. Offending has reciprocal effects on alcohol use and employment.
- H5. Alcohol use and employment interact in their impact on offending.
- H6. Employment mediates the relationship between alcohol use and offending.
- H7. Alcohol use mediates the relationship between employment and offending.

Hypotheses 1 and 2 – The effects of alcohol and employment on offending

Consistent with previous work exploring the connection between substance use and offending (French et al., 2000; Holloway & Bennett, 2004; Pedersen & Skardhamar, 2009; Popovici et al., 2012; White & Gorman, 2000), alcohol appears to be a significant predictor of offending. However, results from multivariate analyses indicate that although alcohol use is statistically significant, it explains only a fraction of the variance in all four offending outcomes. Thus, alcohol use seems to have more statistical significance than substantive significance. Each alcohol variable explains anywhere from 3% to 10% of the variation within the offending variables. Considering Goldstein's tripartite model of drug use and crime, it follows that alcohol consumption should account for a notable portion of violence. As the above analyses indicate, this was not the case. Alcohol consumption (both drinking and binge drinking) did not explain much of the variance in violence.

Further, the effects of binge drinking and drinking e about the same on violence, which again suggests the psychopharmacological violence outlined by Goldstein (1985) is a

¹³ Alcohol use is used throughout the discussion to include both days drank and days binge drank. When referring to only the number of days drank (i.e., separate from binge drinking) will be referred to as "days drank" or "drinking days."

minute portion of drug-related crime, or that alcohol is not a substance which greatly contributes to crime committed by those acting under the influence

The proportion of weeks worked is also statistically significant, but similar to alcohol use, accounts for a small amount of the variation in each of the outcome measures. Because extant literature consistently supports an effect of substance on crime (Bennet, Holloway, & Fairrington, 2008; Holloway & Bennet, 2004; Henkel, 2011; Pedersen & Skardhamar, 2009; Popovici et al., 2012), an alcohol effect was expected. In contrast, the evidence for the effect of employment is frequently contradictory (Cantor & Land, 1985, 1991; Uggen & Wakefield, 2008; Wright, Cullen, & Williams, 2002). Thus, the current study adds to the extant body of literature by offering support for a subtle, yet important effect of employment on offending. As will be discussed in the sections that follow, the significance of employment on crime may be a result of its relationship to alcohol use.

Hypothesis 3 – The relationship between alcohol and employment

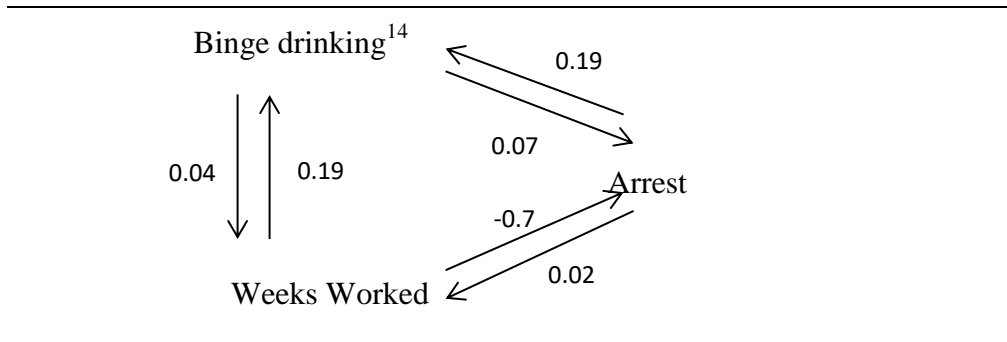
The findings discussed in the previous chapter provide evidence that a relationship exists between drinking alcohol and weeks worked. Consistent with the literature, analyses found that both drinking days and binge drinking days were significantly associated with weeks worked. Prior research has indicated that alcohol consumption is associated with lower levels of employment (Henkel, 2011) and also that unemployment is associated with greater levels of drinking (Popovici et al., 2012). However, the current study found the opposite to be true. Specifically, increasing the amount of weeks worked results in an increase in the number of days where alcohol is consumed. There are a number of theoretical explanations for this, pointing to

mechanisms from rational choice and social bond theories. First, because working more often generally means a greater and steadier income, people may drink more when they work more specifically because they have the money to do so. Alcohol is not free and depending on one's preferences can be quite costly. Thus, having a job may provide the income necessary to purchase whichever type(s) of alcoholic beverages one wishes to consume. Second, spending a greater amount of time at work often means closer ties to coworkers. Depending on who those people are, more intimate relationships may mean spending time with them partaking in activities you both enjoy, such as drinking. "Happy hour" is common practice for many young professionals during the time immediately after work (usually from about 4 to 7) and often involves consuming many alcoholic beverages for a discounted price (Babor, Mendelson, Greenberg, & Kuehnle, 1978). Further, alcohol may serve as a coping mechanism for the increased stress that comes with working more frequently. If coworkers are also drinkers, this could result in drinking as a shared coping mechanism by which coworkers can unwind from their day and "vent" about their employer, other coworkers, and other work-related stressors.

Hypothesis 4 – The reciprocal effects of offending on alcohol use and employment

All four of the offending variables significantly affect drinking behaviors. For the most part, the effects are minimal, although there does appear to be one exception. Violent offending is the strongest of all the offending variables in terms of being able to explain variation in the alcohol variables, but it does not explain any of the variation in weeks worked. Arrest was the only offending variable that had a significant effect on weeks worked and it accounts for a 20% change in binge drinking and just slightly less for alcohol consumption. Results from multivariate analyses suggest that the variables do act reciprocally on each other. Keeping in mind that *weeks worked* is coded as a percent,

Figure 5.1 provides a clear demonstration of the reciprocal forces at work. This is but one example – all of the offending variables and both alcohol variables could be inserted and although the coefficients would change, the significance and directions of effects would remain.



Note: Each arrow is indicative of total effects

Figure 5.1 Understand the substance use, employment, and offending relationship

These findings are consistent with existing theoretical and empirical evaluations of employment and crime and of substance use and crime. The criminogenic effect of alcohol is supported throughout the existing literature in this area as is the negative relationship between alcohol and employment. The current study adds to extant research regarding the employment-crime link,

Hypothesis 5 – Alcohol use and employment interact in their effect on offending.

A total of twelve possible interactions were considered, and only four had significant effects. Both drinking and binge drinking interact with the proportion of weeks worked to affect arrest. In other words, the extent of the criminogenic effect of alcohol on arrest depends on how much of the previous year the respondents spent at work. Likewise, the size of the protective effect of employment depends on how

¹⁴ For purposes of this illustration (Figure 5.1), the coefficients related to weeks worked have been multiplied by 100.

frequently alcohol is consumed. Respondents who did not drink at all experience a much larger protective effect of work while respondents who drank most of the previous 30 days benefit relatively little from the protective effects of work.

Binge drinking and the amount of weeks worked also interact in their effect on conviction such that the effect of each individually is dependent on the value of the other. More specifically, frequent binge drinking reduces the protective effect of employment. In other words, the amount of time spent working matters less for conviction with each additional day of binge drinking. Conversely, the effects of weeks worked and conviction interact in their impact on binge drinking. While those who work often are more likely to binge drink, those who have been convicted are less likely to consume five or more alcoholic beverages on one occasion. Thus, the interactive effect between conviction and binge drinking means that having been convicted lessens the extent to which work increases binge drinking.

Although all of the relationships among alcohol and employment indeed appear to be reciprocal, interaction does not seem to be the vehicle by which most of these reciprocal relationships affect outcomes. Using the economic-compulsive understanding of drug-related crime, respondents who drink more frequently and work less frequently should then be more likely to commit property crime. In other words, an interaction effect between the alcohol variables and weeks worked was expected with regards to property offenses. Because no such interaction was found, it appears that inadequate employment and excessive alcohol consumption do not intensify the effect of each other of property offending. This challenges Goldstein's (1985) economic-compulsive model of crime. Rather, it appears the life-course approach and social bond theory can better inform our

understanding of property crime, especially when considering the effects of marriage and education.

Being married and educated reduces the likelihood of property offending more than all of the other predictors. Indeed, this is true for all of the offending variables. Married respondents and those who have graduated college are markedly less likely to have an arrest, a conviction, and to commit either a property or violent offense than those who are single and have less education. This suggests social bonds to society matter for offending – that is, bonds to society such as a spouse or education system serve a protective function for individuals, lessening the likelihood of offending behavior. However, in stark contrast, being a parent appears to be criminogenic such that each additional child increases the odds of arrest and conviction as well as property and violent offenses. Because children generally offer a connection to mainstream society, this undermines the social bond explanation of the alcohol-employment-crime connection. At the same time, it may suggest that the stress of personal responsibility and financial burden that comes with being a parent overrides the influence of social bonds. Given that respondents included in the current study are aged 17 to 27, they can generally be considered younger parents. In the context of Laub & Sampson's (2003) age-graded theory of social control, the stress of being a parent is intensified by the lack of financial stability that occurs during emerging adulthood. Further, parental obligations may conflict with the lifestyle of others the same age and may prohibit the parent from engaging in the types of behaviors they would prefer if they did not have a child. Thus, being a younger parent may foster drinking to relieve the strain of the financial burden or as a means to escape their obligations by becoming intoxicated.

Hypotheses 6 - Employment mediates the effect of alcohol use on offending

The mediation analyses conducted in the previous chapter provide insight into how exactly these three key factors – alcohol use, employment, and offending – are interconnected. Although alcohol use accounts for less than 10% of the variation in each of the four offending variables, it appears that effect results from alcohol directly. That is to say, the relationship between alcohol use and offending does not operate through employment. This implies then that attempts to tackle alcohol-related offending are futile if they intend to do so by increasing employment. Instead, the focus should be on reducing how frequently people drink alcohol both in general and to excess.

It is also worthwhile to note that employment mediates the largest portion of alcohol's effect on offending in the conviction model and only slightly less in the arrest model. While the effect is minimal, it suggests that employment may be more important to decisions made by the criminal justice system rather than by the offenders. In other words, how frequently one works may not matter for whether or not they choose to commit a given offense, but it may shape how police officers and the courts interact with or perceive a person. Should that be the case, then it would appear working more frequently improves perceptions by the criminal justice system and subsequently reduces the likelihood that an officer will respond to an incident with an arrest or that a court will convict someone who has been arrested.

Hypotheses 7 –Alcohol use mediates the relationship between employment and offending

Although employment appears to be in insignificant mediator in the alcohol-crime connection, it appears that alcohol plays an important role in mediating the connection between work and crime. Further, it serves as a mediator in both directions – that is, it

mediates both the effect of work on offending and of offending on work. More specifically, drinking alcohol in general mediates a substantial amount of the effect of weeks worked on whether a person commits a property or violent offense. Conversely, alcohol consumption mediates a slightly larger portion of the effect of property and violent crime on how frequently a person works. Both of these mediation effects hold true for binge drinking as well, but somewhat surprisingly, the effect is larger for the number of days drinking in general (as opposed to excessively). While intuition may suggest that more intense drinking patterns would have a greater impact, the findings from the current study indicate they have a more muted effect regardless of whether employment or offending is considered as the outcome.

Directions for Future Research

Given the importance of alcohol use in shaping the relationship between employment and crime and the absence of employment in the alcohol-crime relationship, efforts to curb offending behaviors would be wise to target alcohol consumption rather than employment. However, because alcohol use accounts for such a small portion of offending, it appears there must be more effective avenues to reduce offending than through the reduction of alcohol consumption. Thus, future research in this area could build on the foundation of this study by exploring the effects of other types of drugs such as marijuana, cocaine, and/or heroin in the context of the drugs-work-crime network. Further, there may be important differences by the types of job worked, the quality of employment, and satisfaction with the job.

In addition, there is a need to examine how drug use and employment impact arrest frequency among females specifically. Given that the frequency of arrest and drug

use among females and the presence of women in the workforce have been on the rise for half a century, it is relevant to consider how these may interact with each other for females in a way that is different from the male experience. Thus, although sex was controlled for in the present study, future work should explore the nuances of the relationships between substance use, employment, and offending specifically within the context of gender.

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APPENDIX A

INDIVIDUAL EFFECTS OF VARIABLES CONSIDERED IN INTERACTIONS

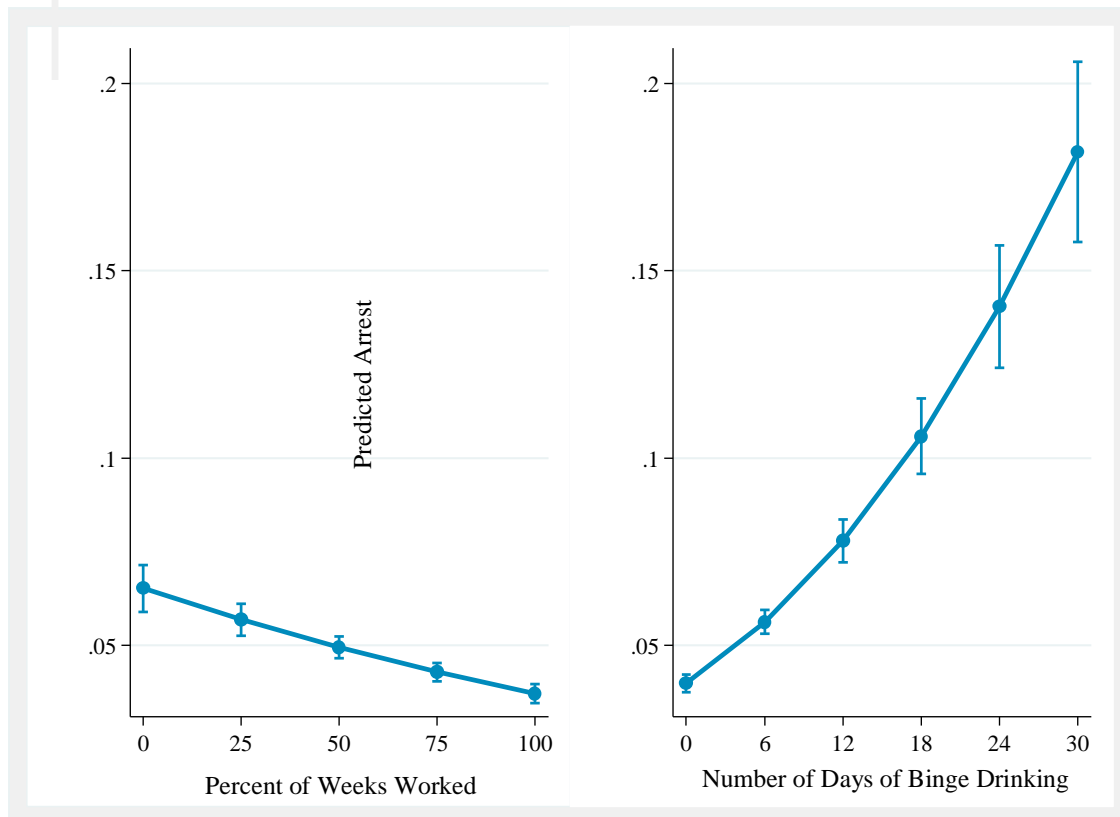


Figure A.1. Individual Effects of Days Drank Alcohol and Weeks Worked on Arrest.

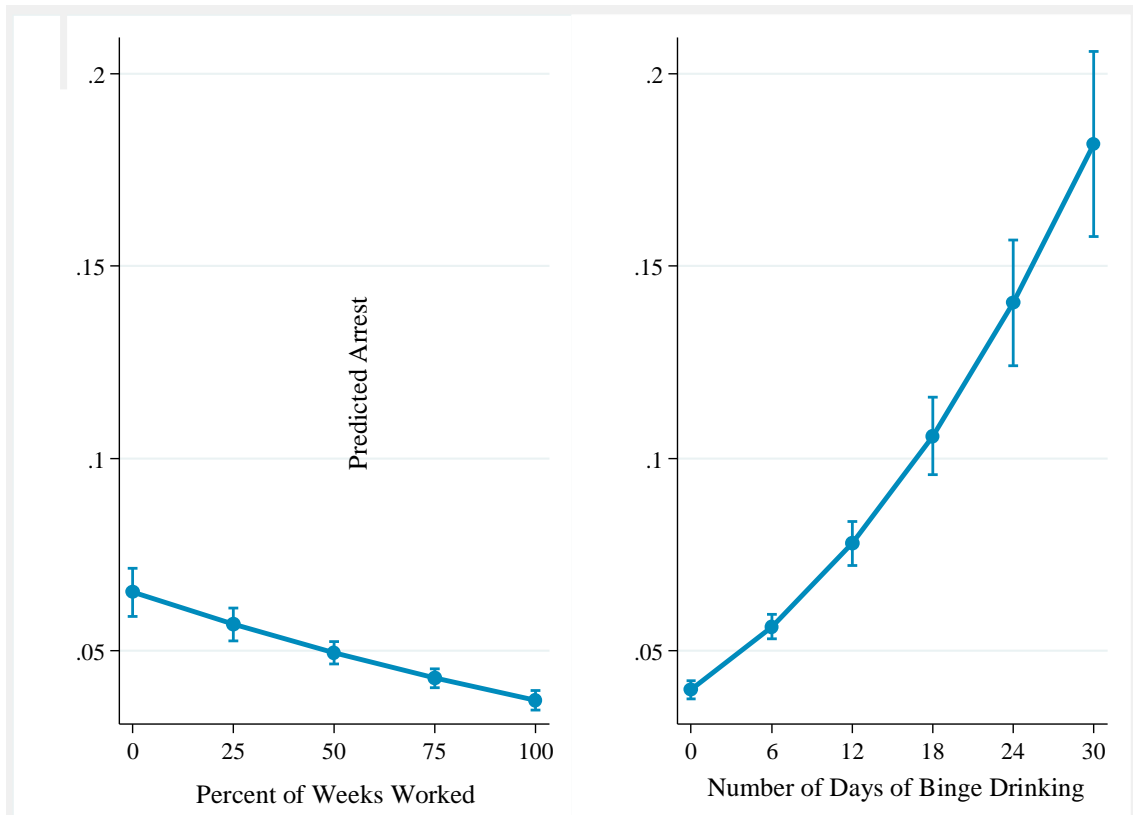


Figure A.2. Individual effects of Binge Drinking and Weeks Worked on Arrest.

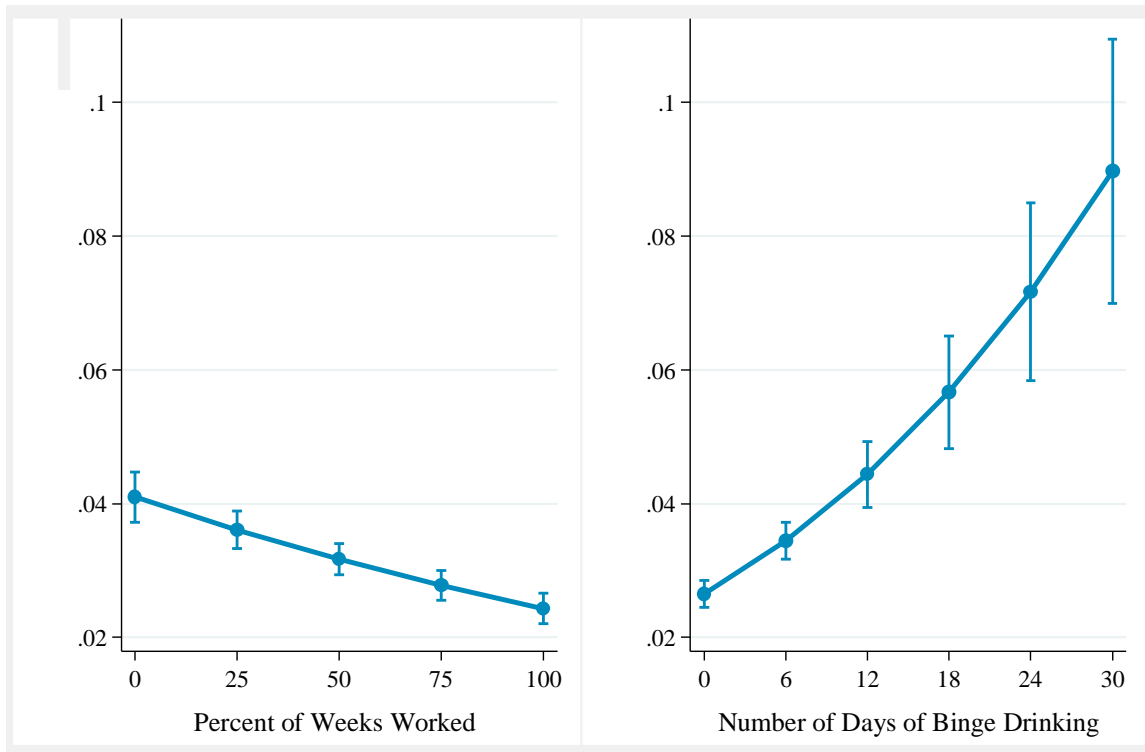


Figure A.3. Individual Effects of Binge Drinking and Weeks Worked on Conviction.

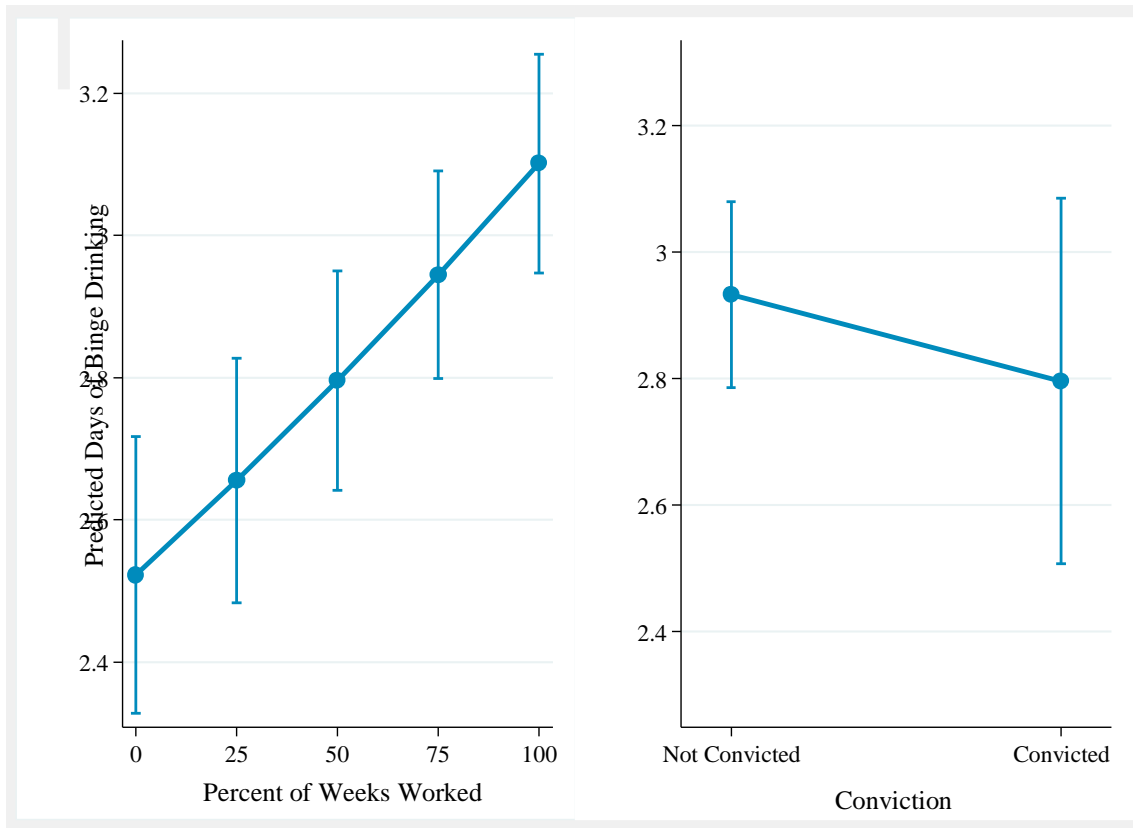


Figure A.4. Individual Effects of Conviction and Weeks Worked on Binge Drinking.

APPENDIX B

SENSITIVITY ANALYSIS

Table B.1. The Effects of Drinking Alcohol and Number of Jobs Held on Arrest:
Multilevel Logit Model Estimates ($N=8,340$)

	Model 1 <i>b</i> (Std. Error)	Model 2 <i>b</i> (Std. Error) OR	Model 3 <i>b</i> (Std. Error) OR
Days drank		0.05 (0.01)** 1.04	0.04 (0.01)** 1.04
Number of jobs held		0.08 (0.03) ** 1.08	0.08 (0.03) * 1.08
Age		-0.08 (0.01)** 0.92	-0.08 (0.01)** 0.92
Black		0.18 (0.06)*** 1.20	-0.18 (0.07)** 1.20
Hispanic or mixed		-0.15 (0.11)** 0.86	-0.15 (0.09)** 0.86
Male		1.15 (0.08)** 3.17	1.16 (0.08)** 3.17
Some high school		-0.86 (0.11)** 0.42	-0.86 (0.11)** 0.42
Some college		-0.83 (0.07)** 0.44	-0.83 (0.07)** 0.44
College graduate or more		-1.64 (0.08)** 0.20	-1.64 (0.07)** 0.20
Married		-0.90 (0.11)** 0.41	-0.90 (0.11)** 0.41
Separated, Divorced, or Widowed		0.53 (0.14) ** 1.70	0.53 (0.14) ** 1.70
Number of Kids		0.26 (0.04)** 1.29	0.29 (0.04)** 1.29
Number of Jobs Held x Alcohol			-0.01 (0.00) 1.00
Fixed Intercept	-2.50 (0.04)**	-5.85 (0.28)**	-7.76 (0.12)**
Individual-Level Variance	6.52 (0.22)	1.70 (0.12)	1.70 (0.12)

* $p<0.05$; ** $p<0.01$, *** $p<0.001$

Table B.2. The Effects of Binge Drinking and Number of Jobs on Arrest: Multilevel Logit Model Estimates ($n=8,110$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) IRR	<u>Model 3</u> <i>b</i> (Std. Error) IRR
Days binge drank		0.07 (0.00)** 1.06	0.08 (0.01)** 1.08
Number of Jobs Held		0.08 (0.03) ** 1.08	0.10 (0.00) ** 1.10
Age		-0.08 (0.01)** 0.93	-0.07 (0.01)** 0.93
Black		0.21 (0.07)** 1.23	0.21 (0.07)** 1.23
Hispanic or mixed		-0.15 (0.09) 0.86	-0.15 (0.09) 0.86
Male		1.13 (0.08)** 3.09	1.13 (0.08)** 3.09
Some high school		-0.85 (0.11)** 0.43	-0.85 (0.11)** 0.43
Some college		-0.82 (0.07)** 0.44	-0.83 (0.11)** 0.44
College graduate or more		-1.61 (0.07)** 0.20	-1.61 (0.07)** 0.20
Married		-0.88 (0.11)** 0.42	-0.88 (0.11)** 0.41
Separated, Divorced, or Widowed		0.55 (0.14) ** 1.74	0.55 (0.14) 1.74
Number of Kids		0.24 (0.04)** 1.27	0.24 (0.04)** 1.27
Number of Jobs Held x Binge			-0.01 (0.01) 0.99
Observation-Level Intercept	-2.50 (0.04)**	-7.67 (0.12)**	-7.70 (0.13)**
Individual-Level Variance	6.52 (0.22)	1.66 (0.12)	1.66 (0.10)

* $p<0.05$; ** $p<0.01$

Table B.3. The Effects of Alcohol and Number of Jobs Held on Conviction: Multilevel Logit Model Estimates ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) IRR	<u>Model 3</u> <i>b</i> (Std. Error) IRR
Days drank		0.03 (0.01)** 1.03	0.02 (0.01)** 1.02
Number of Jobs Held		0.03 (0.04) 1.03	0.01 (0.04) 1.00
Age		-0.01 (0.01) 0.99	-0.01 (0.01) 0.99
Black		-0.18 (0.10) 0.84	-0.18 (0.10) 0.83
Hispanic or mixed		-0.26 (0.10)* 0.78	-0.25 (0.10)* 0.78
Male		1.21 (0.09)** 3.35	1.21 (0.09)** 3.35
Some high school		-1.10 (0.15)** 0.33	-1.11 (0.15)** 0.33
Some college		-0.82 (0.09)** 0.44	-0.82 (0.09)** 0.44
College graduate or more		-1.69 (0.11)** 0.19	-1.69 (0.11)** 0.19
Married		-0.83 (0.11)** 0.44	-0.83 (0.11)** 0.44
Separated, divorced, or widowed		0.27 (0.21) 1.27	0.24 (0.21) 1.27
Number of kids		0.25 (0.04) ** 1.28	0.25 (0.04) ** 1.28
Number of Jobs Held x Days Drank			0.00 (0.00) 1.00
Observation-Level Intercept	-2.86 (0.04)	-8.29 (0.13)**	-8.29 (0.13)**
Individual-Level Intercept	7.42 (0.24)	2.45 (0.15)	2.50 (0.15)

* $p < 0.05$; ** $p < 0.01$

Table B.4. The Effects of Binge Drinking and Number of Jobs Held on Conviction:
Multilevel Logit Model Estimates ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error) OR	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Binge drinking		0.05 (0.01)** 1.06	0.06 (0.01)** 1.06
Number of Jobs Held ^a		0.02 (0.03) 1.02	0.06 (0.00) 1.04
Age		0.00 (0.01) 1.01	-0.01 (0.01) 0.99
Black		-0.14 (0.10)** 0.87	-0.14 (0.10)* 0.87
Hispanic or mixed		-0.25 (0.11)** 0.78	-0.25 (0.11)** 0.78
Male		1.17 (0.09)** 3.21	1.17 (0.09)** 3.21
Some high school		-1.07 (0.15)** 0.34	-1.07 (0.15)** 0.34
Some college		-0.81 (0.08)** 0.44	-0.81 (0.08)** 0.44
College graduate or more		-1.68 (0.11)** 0.19	-1.68 (0.11)** 0.19
Married		-0.79 (0.11)** 0.45	-0.79 (0.11)** 0.45
Separated, divorced, or widowed		0.26 (0.21) 1.30	0.26 (0.21) 1.30
Number of kids		0.24 (0.04) ** 1.29	0.24 (0.04) ** 1.27
Number of Jobs Held x binge drinking			-0.03 (0.02) 0.99
Observation-Level Intercept	-7.80 (0.14)	-8.24 (0.12)**	-8.26 (0.13)**
Individual-Level Intercept	2.39 (0.15)	2.44 (0.15)	2.45 (0.15)

* $p < 0.05$; ** $p < 0.01$

Table B.5. The Effects of Alcohol and Number of Jobs Held on Property Offenses:
Multilevel Logit Model Estimates. ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Days drank		0.06 (0.00)**	0.06 (0.00)**
		1.06	1.05
Number of Jobs Held		0.11 (0.03)**	0.10 (0.03)**
		1.12	1.11
Age		-0.43 (0.02)**	-0.43 (0.02)**
		0.65	0.65
Black		-0.14 (0.09)	-0.14 (0.09)
		0.95	0.95
Hispanic or mixed		-0.07 (0.10)	-0.07 (0.10)
		0.94	0.94
Male		0.71 (0.07)**	0.73 (0.07)**
		2.06	2.06
Some high school		-0.31 (0.12) **	-0.31 (0.12) **
		0.72	0.73
Some college		-0.41 (0.11) **	-0.41 (0.11) **
		0.70	0.69
College graduate or more		-0.59 (0.11)**	-0.59 (0.11)**
		0.57	0.57
Married		-0.68 (0.14)**	-0.68 (0.14)**
		0.50	0.50
Separated, divorced, or widowed		-0.01 (0.28)	-0.01 (0.28)
		0.97	0.97
Number of Kids		0.28 (0.06)**	0.30 (0.06)**
		1.32	1.35
Number of Jobs Held x Alcohol			0.00 (0.00)
			1.00
Observation-Level Intercept	-2.43 (0.03)**	-9.17 (0.15)**	-9.16 (0.15)**
Individual-Level Intercept	5.61 (0.17)	3.22 (0.20)	3.22 (0.20)

* $p < 0.05$; ** $p < 0.01$

Table B.6. The Effects of Binge Drinking and Number of Jobs Held on Property Offending: Multilevel Logit Model Estimates. ($n = 8,632$)

	<u>Model 1</u>	<u>Model 2</u>	<u>Model 3</u>
	<i>b</i> (Std. Error)	<i>b</i> (Std. Error)	<i>b</i> (Std. Error)
	OR	OR	OR
Binge drinking		0.08 (0.01)**	0.08 (0.01)**
		1.08	1.08
Number of Jobs Held		0.11 (0.03) **	-0.12 (0.08)**
		1.12	1.12
Age		-0.42 (0.02)**	-0.42 (0.02)**
		0.66	0.66
Black		-0.03 (0.09)	-0.11 (0.09)
		0.90	0.90
Hispanic or mixed		-0.08 (0.10)	0.10 (0.10)
		0.91	0.91
Male		0.68 (0.07)**	0.68 (0.07)**
		1.98	1.98
Some high school		-0.32 (0.12)**	-0.32 (0.12)*
		0.73	0.73
Some college		-0.37 (0.11)**	-0.37 (0.11)**
		0.69	0.69
College graduate or more		-0.54 (0.11)**	-0.55 (0.11)**
		0.58	0.58
Married		-0.68 (0.14)**	-0.68 (0.14)**
		0.51	0.51
Separated, divorced, or widowed		0.02 (0.28)	0.02 (0.28)
		1.02	1.02
Number of kids		0.25 (0.06)**	0.25 (0.06)**
		1.28	1.28
Number of Jobs Held x Binge drinking			0.00 (0.00)
			1.00
Observation-Level Intercept	-2.43 (0.03)**	-9.06 (0.15)**	-9.06 (0.16)**
Individual-Level Intercept	5.61 (0.17)	3.22 (0.19)	3.30 (0.19)

* $p < 0.05$; ** $p < 0.01$

Table B.7. The Effects of Binge Drinking and Number of Jobs Held on Violent Offending: Multilevel Logit Model Estimates. ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error) OR	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Binge drinking		0.08 (0.01)** 1.08	0.08 (0.01)** 1.08
Number of Jobs Held		0.03 (0.03) 1.03	0.03 (0.03) 1.03
Age		-0.42 (0.02)** 0.66	-0.31 (0.02)** 0.66
Black		-0.11 (0.09)** 0.90	-0.11 (0.09)** 0.90
Hispanic or mixed		0.10 (0.10) 0.91	0.10 (0.10) 0.91
Male		0.68 (0.07)** 1.98	0.68 (0.07)** 1.98
Some high school		-0.32 (0.12)** 0.73	-0.32 (0.12)** 0.73
Some college		-0.37 (0.11)** 0.69	-0.37 (0.11)** 0.69
College graduate or more		-0.54 (0.11)** 0.58	-0.54 (0.11)** 0.58
Married		-0.68 (0.14)** 0.51	-0.68 (0.14)** 0.51
Separated, divorced, or widowed		0.02 (0.28) 1.02	0.02 (0.28) 1.02
Number of kids		0.25 (0.06)** 1.28	0.25 (0.06)** 1.28
Number of Jobs Held x Binge drinking			0.00 (0.01) 1.00
Observation-Level Intercept	-2.43 (0.03)**	-9.05 (0.17)**	-8.965 (0.17)**
Individual-Level Intercept	5.61 (0.17)	3.39 (0.17)	3.39 (0.17)

* $p < 0.05$; ** $p < 0.01$

Table B.8. The Effects of Alcohol and Number of Jobs Held on Violent Offending: Multilevel Logit Model Estimates. ($n = 8,636$)

	<u>Model 1</u> <i>b</i> (Std. Error)	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Days drank		0.06 (0.00)** 1.06	0.06 (0.00)** 1.06
Number of Jobs Held		0.03 (0.03) 1.03	0.03 (0.03) 1.03
Age		-0.30 (0.02)** 0.74	-0.30 (0.02)** 0.74
Black		0.45 (0.10)** 1.57	0.45 (0.10)** 1.57
Hispanic or mixed		0.14 (0.12) 1.15	0.14 (0.12) 1.15
Male		0.81 (0.10)** 2.24	0.81 (0.10)** 2.24
Some high school		-0.68 (0.13)** 0.51	-0.68 (0.13)** 0.51
Some college		-0.57 (0.10) ** 0.57	-0.57 (0.10) ** 0.57
College graduate or more		-1.40 (0.12)** 0.25	-1.40 (0.12)** 0.25
Married		-0.60 (0.12) ** 0.55	-0.60 (0.12) ** 0.55
Separated, divorced, or widowed		0.04 (0.25) 1.04	0.04 (0.25) 1.04
Number of kids		0.17 (0.07)** 1.18	0.17 (0.07)** 1.18
Weeks worked x Days drank			0.01 (0.00) 1.02
Observation-level intercept	-2.79 (0.05)**	-9.10 (0.17)**	-8.79 (0.17)**
Individual-level intercept	7.16 (0.24)	3.38 (0.18)	3.44 (0.18)

* $p < 0.05$; ** $p < 0.01$

Table B.9. The Effects of Binge Drinking and Number of Jobs Held on Violent Offending: Multilevel Logit Model Estimates ($n = 8,632$)

	<u>Model 1</u> <i>b</i> (Std. Error) OR	<u>Model 2</u> <i>b</i> (Std. Error) OR	<u>Model 3</u> <i>b</i> (Std. Error) OR
Binge drinking		0.09 (0.01)** 1.09	0.09 (0.01)** 1.09
Number of Jobs Held		0.03 (0.03) 1.02	0.02 (0.09) 1.02
Age		-0.29 (0.02)** 0.75	-0.29 (0.02)** 0.75
Black		0.49 (0.10)** 1.63	0.49 (0.10)** 1.63
Hispanic or mixed		0.16 (0.11) 1.17	0.16 (0.11) 1.17
Male		0.78 (0.10)** 2.18	0.78 (0.10)** 2.18
Some high school		-0.67 (0.13)** 0.5	-0.67 (0.13)** 0.51
Some college		-0.56 (0.10)** 0.57	-0.56 (0.10)** 0.57
College graduate or more		-1.37 (0.12)** 0.25	-1.37 (0.12)** 0.25
Married		-0.54 (0.12)** 0.58	-0.54 (0.12)** 0.58
Separated, divorced, or widowed		0.09 (0.23) 1.10	0.09 (0.23) 1.10
Number of Kids		0.14 (0.07)** 1.15	0.14 (0.07)** 1.15
Number of Jobs Held x Binge drinking			0.00 (0.01) 1.00
Observation-Level Intercept	-5.92 (0.10)**	-8.97 5 (0.17)**	-8.96 (0.17)**
Individual-Level Intercept	1.82 (0.25)	3.35 (0.17)	3.29 (0.17)

* $p < 0.05$; ** $p < 0.01$