ABSTRACT

Title of dissertation: DECONSTRUCTING THE MONOLITHIC EFFECT OF EMPLOYMENT ON CRIME: THE ROLE OF HETEROGENEITY IN JOB QUALITY

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Theorists and policy makers alike have relied on the overly parsimonious assumption that any employment should deter crime at the individual-level. Drawing on rational choice and dual labor market theories, the present study argues that this assumption is flawed for two reasons. First, it ignores vast heterogeneity in job quality which may fall under the guise of "employment." Second, the assumption that employment uniformly deters crime is theoretically inconsistent when workplace crime is considered as an outcome.

The present study evaluated the relationship between job quality and crime using a longitudinal sample of adults who had been adjudicated for a serious criminal offense in their youth. Focusing on individuals during their transition to adulthood provided a unique opportunity to evaluate the relationship between job quality and crime during a time in the life course when employment begins to take an especially prominent role. The robust employment-related measures available within the Pathways to Desistance data

also provided the opportunity to improve upon operationalizations of job quality used in prior literature through measuring job quality as a multidimensional construct consistent with theory as well as discourse outside of criminology.

While findings provided evidence of both between and within-individual variability in job quality amongst this high-risk sample, there was no evidence that transitioning from having no job to having a low-quality job was associated with a reduction in street or workplace offending. Among those who were working, there was also no evidence that a higher or high-quality job was associated with a reduction in street crime. Within the low-quality secondary sector, an increase in job quality was also not associated with a reduction in street crime.

With respect to workplace crime, higher quality employment was found to have a marginally significant association with offending. However, high (relative to low) job quality was not associated with a reduction in workplace crime. When employment remained within the low-quality secondary sector, an increase in job quality was not found to be negatively associated workplace crime.

In evaluating the street-workplace crime overlap, findings suggested that higher job quality was not significantly associated with committing no crime, committing workplace crime, or committing both (street and workplace crime), relative to committing street crime only. In addition, higher job quality was also not associated with committing workplace crime, or both, relative to committing no crime; nor was higher job quality associated with committing both types of crime relative to workplace crime only. Higher job quality was only found to be marginally associated with a reduced likelihood of committing both types of crime relative to committing street crime only.

In addition, high-quality primary sector employment was not associated with committing no crime, or committing workplace crime only, relative to street crime only. It was also not found to be associated with the likelihood of committing no crime relative to workplace crime only. High-quality primary sector employment was, however, found to be associated (at least marginally) with a reduced likelihood of committing both types of crime relative to no crime, workplace crime only, and street crime only.

Taken together, these results speak to the importance of avoiding the overly simplistic assumption that the relationship between employment and crime is uniformly negative. Overall, the study found little consistent support for its hypotheses which posited a negative relationship between job quality and offending. The lack of consistent support for the notion that higher and even high-quality employment serves an effective informal cost of crime throughout this study, especially with respect to street crime, speaks to the importance of further understanding cost-benefit considerations which influence offender decision-making.

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by

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CHAPTER 1: INTRODUCTION

For decades, criminology and criminal justice professionals have placed stock in employment as a panacea for criminal behavior, believing crime to be a problem among the unemployed. This is especially true among high-risk individuals in the transition to adulthood who are within the peak age range for criminal behavior just as employment becomes a normative role in the adult life course.[1] Because criminologists often propose the mere presence of employment is protective against crime, it is not surprising that practitioners have strived to find work for the criminally inclined. For instance, Crutchfield (2014) detailed his early career experiences as a parole officer, stating it was "considered a no-brainer... that a parolee with a job is far more likely to stay out of jail" (p. 29). Though he could not recall a single instance in which training specifically taught him of the importance of employment for his parolees, "it was certainly part of the lore and informal education in the office" (Crutchfield, 2014, p. 32).

Relatedly, many programs have targeted employment as a means of crime reduction; though experimental evaluations have consistently failed to demonstrate joint improvements in employment and reductions in offending (Bushway & Apel, 2012; Visher, Debus, & Yahner, 2008; but see also Duwe, 2012; Cook, Kang, Braga, Ludwig, & O'Brien, 2015, & Heller, 2014). The unexpected lack of empirical support for employment programs has led scholars to question why work doesn't work as a policy lever for crime reduction (Bushway & Apel, 2012). One explanation may be that high-risk individuals do not obtain the types of jobs which effectively deter crime. Cook et al. (2015) raised this concern when they questioned whether offering "a

[1] The present study focuses on the ambiguous relationship between employment and crime at the individual level. Though at the macro-level, there is little to no support for a relationship between unemployment and crime rates (see Freeman 1999 for an overview).

temporary low-wage menial job" was a strong enough treatment to reduce crime (p. 378). They conceded that generating desired recidivism outcomes may in fact "require a bigger 'dose' of legitimate opportunity" than work programs typically provide (Cook et al., 2015, p. 378).

Observational research focusing simply on whether or not an individual was employed within a given time period (measuring employment dichotomously) has also failed to provide conclusive evidence that work deters crime. Though some studies have found periods of employment to be negatively associated with crime (Laub & Sampson, 2003; Loughran, Nagin, & Nguyen, 2016a; Savolainen, 2009; Verbruggen, Apel, Van Der Geest, & Blokland, 2015; Verbruggen, Blokland, & Van Der Geest, 2012), others have failed to support the relationship entirely (Skardhamar & Savolainen, 2014) or found only limited crime-specific or race-specific support (Griffin & Armstrong, 2003; Piquero, MacDonald, & Parker, 2002). Horney, Osgood, and Marshall (1995) even found an unexpected positive association between employment and property crime.

However, many of these researchers have explicitly recognized their focus on the mere absence or presence of employment as a valid research limitation. For example, Horney et al. (1995) referred to their dichotomous measure as "crude" because it "did not even distinguish part-time from full-time employment, or temporary from permanent work" (p. 668). Nearly 20 years later, Skardhamar and Savolainen (2014) similarly recognized that their findings were limited by their inability to distinguish between good and bad jobs; highlighting that "key studies in this literature focus on within-individual changes in states of employment, ignoring variation in job characteristics" (p. 25).

The lack of consistent empirical support for a work-crime relationship may be the result of an overly parsimonious belief that employment uniformly deters offending. The search to uncover a monolithic effect of employment on crime has likely been allusive because it relies on two flawed assumptions. First, it assumes that all employment is created equal, ignoring vast heterogeneity in the types of jobs which may fall under the guise of "employment." Second, the assumption that all employment will uniformly protect against crime ignores the fact that employment and crime are not necessarily mutually exclusive (Fagan & Freeman, 1999; Grogger, 1998; Loughran, Nagin, & Nguyen, 2016a; Myers, 1983, 1984; Sviridoff & Thompson, 1983). In fact, some types of crime can only occur during periods of employment. For example, by definition, workplace crime cannot be negatively associated with employment. To better understand the relationship between employment and crime, the present study argues that scholars must move beyond theorizing that employment has a monolithic negative effect on crime by focusing on heterogeneity in the types of jobs obtained by high-risk individuals and acknowledging that crime may also occur in the workplace. The study specifically seeks to answer the following key research questions:

- **Research Question 1:** Among an employed high-risk sample, to what extent is there variability in job quality?
- **Research Question 2:** What is the relationship between job quality and crime?
- **Research Question 3:** Does the relationship between job quality and crime extend to crime in the workplace?
    - **Research Question 3a:** Is job quality associated with whether an individual commits no crime, street crime only, workplace crime only, or both?

Job Quality and Crime

Although the concept of choice is present within many criminological theories, rational choice theory (RCT) is now arguably the principle framework used to explain the decision to offend. The theory posits that because individuals are rational, calculating, and self-interested, they will always choose the course of action (or inaction) which is perceived to maximize benefits and minimizes losses (Becker, 1968).[2] Within RCT, employment can act as a deterrent through increasing the informal cost of crime; functioning as an attachment, commitment, and/or opportunity cost.[3] For example, if an individual is caught committing crime, he or she risks losing all of the valued benefits associated with employment if fired or unable to work. This has been referred to in the literature as commitment and attachment costs of crime (Williams & Hawkins, 1986; Nagin & Paternoster, 1994). In addition, an individual may incur opportunity costs even if his or her criminal behavior were to go undetected, simply by allocating time to criminal activity if the choice requires the individual to simultaneously forgo benefits that could have been earned by allocating time to alternative legitimate employment (Ehrlich, 1973;

[2] Although the field most often attributes RCT to the work of Cornish and Clarke (1986), the present inquiry does not focus on their rational choice perspective because it was intended to develop policies associated with Situational Crime Prevention rather than to serve as a theoretical model of offender decision-making.

[3] Within the present study, RCT is conceived of as broad theory which encompasses informal social control (Hirschi, 1969; Sampson & Laub, 1993). Although RCT was virtually synonymous with deterrence for decades (Paternoster, 2010), the theory has now developed to include a much more robust set of costs and benefits believed to influence behavior (see Loughran, Paternoster, Chalfin, & Wilson, 2016b for an overview). This tradition can be traced back the 1980s when deterrence scholars began to suggest that conformity may also be attributable to extralegal or "informal" sources of control (Paternoster, Saltzman, Waldo, & Chiricos, 1983). Williams & Hawkins (1986) then integrated theories of informal social control (e.g., Hirschi, 1969; Toby, 1957) with deterrence by adding informal sanctions to the cost side of a potential offender's decision-making calculus. This integration created a more robust utility model and brought cost considerations more in line with current conceptualizations of rational choice.

Lochner, 2004; Nguyen & Loughran, ND "Incorporating Rational Choice and Structural Theories").

Though an important reality which is too often overlooked within criminology is that not all employment is created equal in a potential offender's decision-making calculus – some jobs are simply more valuable than others. Therefore, "employment" will only serve as an effective deterrent within a rational choice framework to the extent that a specific job is valued. The total value (or potential cost of crime) associated with a job can be referred to as its *quality*. Because higher quality jobs are more costly to lose or allocate time away from, they should more effectively disincentivize criminal behavior. Conversely, lower quality jobs which are relatively worthless should have little to no deterrent effect, holding all other decision-making inputs constant. Lower quality jobs could arguably even be criminogenic if they directly provide high-benefit low-cost opportunities for crime (Mars, 1973; Sviridoff & Thompson, 1983) or indirectly facilitate crime through altering an individual's daily routines in a manner which is more conducive to crime (Apel & Horney, 2017; Osgood, Wilson, O'Malley, & Bachman, 1996).

An important question, then, remains as to what job characteristics increase its quality. Perhaps because wages are the most evident rewards of employment, they are also the most traditional and frequent measures of job quality; as higher paying jobs are suggested to be of greater value (Jencks, Perman, & Rainwater, 1988; Kalleburg & Sorensen, 1979). A potential offender's legal wage rate has particularly been emphasized within economic conceptualizations of choice for its ability to disincentivize financially motivated criminal alternatives (Gould, Weinberg, & Mustard, 2002; Grogger, 1998; Hansen & Machin, 2002; Machin & Meghir, 2004; Myers 1983, 1984; Witte, 1980). These rational choice models underscore pecuniary incentives,

suggesting that crime is the result of expected financial returns from crime exceeding the financial returns of legitimate work once potential illegal earnings are discounted by the perceived certainty and severity of sanctioning (see Freeman 1999 for an overview). Consistent with the notion that legitimate pecuniary returns disincentivize crime, a job with fringe benefits such as health insurance, paid vacation, sick leave, transportation compensation and/or employee discounts are also believed to be higher quality because of the tangible rewards associated with these benefits (Kalleberg, Reskin, & Hudson, 2000). Jobs with more available hours may also be superior in quality because they increase earnings or the likelihood of fringe benefits and are more frequently associated with stable employment.

These traditional economic indicators of job quality, however, can be criticized as "objective" in that they only incorporate aspects of employment *assumed* to be valued by the decision-maker. Given that RCT has a long history of recognizing the importance of perceptions (Geerken & Gove, 1975; Waldo & Chiricos, 1972), it is appropriate to also consider subjective indicators of job quality. Subjective valuations are important because they account for the fact that two individuals may have different perceptions of quality for the same job (Kalleberg, 1977).[4] This may be in part because of individual differences in the rewards people seek to obtain from work (Kalleberg, 1977). Literature has highlighted that there is not always perfect overlap between objective and subjective job quality indicators. For instance, Clarke (1998) used

[4] The theoretical argument for also considering subjective measures of job quality within the employment-crime literature parallels similar transitions in measurement within criminological theory. For example, decades ago deterrence scholars restated deterrence as a *perceptual* theory of sanction threats and transitioned from objective measures of sanction risk to subjective measures (Geerken & Gove, 1975; Waldo & Chiricos, 1972). An analogous shift also transpired within strain theory research when Agnew (2001) argued the stress literature had evolved to recognize that individuals differed in their subjective evaluations of a single objective strain.

data from 14,000 workers within 19 countries to evaluate the relationship between objective and subjective job quality. His findings demonstrated that although "most would agree that, *ceteris paribus*, a higher-paid job is a better job," the correlation between objective and subjective income was "not overwhelming" (0.40 or less) and the correlation between objective and subjective hours of work was much weaker; ranging from .17 to -0.09 (Clarke, 1998, p. 15, emphasis in original). This is not entirely surprising, as Clarke (1998) emphasized that we cannot learn workers' desired hours or income without asking workers themselves.

Traditional economic conceptualizations of RCT can also be criticized for failing to emphasize nonmonetary rewards of work, given that individuals also value intangible aspects of a job (Jencks, Perman, & Rainwater, 1988). These dimensions of job quality are most frequently emphasized by psychologists (Kalleberg & Vaisey, 2005). Years ago, Kalleberg (1977) identified dimensions of a job which may be valued in addition to its financial rewards: intrinsic benefits, convenience, adequate resources, relationships among co-workers, and career potential. The intrinsic dimension focuses on valuation of job specific tasks, reflecting an employee's desire for work to provide a sense of usefulness, accomplishment, and/or autonomy. Intrinsic job rewards are consistent with criminological discourse suggesting that work can promote "a sense of identity and meaning" in one's life (Laub & Sampson, 2003, p. 47). This is supported by Clarke's (1998) finding that respondents highly valued a job's interest, independence, ability to help others, and usefulness to society – all of which were ranked as more important than income. Shover (1996) also acknowledged intrinsic job rewards when he stated that not all jobs equally deter crime but "there is little surprise about the kinds that do. They return a decent income,

enable the individual to exercise intelligence and creativity, and allow for some autonomy in structuring the days' activities" (p. 127).

The convenience and resource adequacy dimensions suggest that "soft" jobs are perceived as more valuable (Kalleberg, 1977, p. 128). A soft job may be one with desirable hours, a convenient location, pleasant physical surroundings, and few safety hazards. Soft jobs also have comfortable workloads with sufficient resources to facilitate task completion. Adequate resources may include proper equipment, competent co-workers and appropriate guidance from supervisors. These job rewards are influenced by external organizational factors which are not specific to job tasks, contrasting intrinsic rewards.

Employees may also perceive a job as higher quality if it serves as an avenue to develop social relationships or attachments (Kalleberg, 1977). Through the course of employment, an individual may develop mutual feelings of respect, trust, admiration, amity and goodwill amongst co-workers and their employer. These attachments are not only valuable because they provide emotional rewards, but also because they may result in a social network that can be called upon in the future as a reference or connection to facilitate opportunities; consistent with Coleman's (1990) description of social capital. Sampson and Laub (1993) emphasized this type of employment reward in stating "that independent of the forms of physical and human capital available to individuals (for example, income, occupational skill), social capital is a central factor in facilitating effective ties that bind a person to societal institutions" (p. 140).

Additionally, jobs which do not meet many of the above characteristics may still be perceived as high quality if they are thought to be career track or difficult to replace. For instance, a job that is not currently meeting many quality characteristics but is believed to have

the potential to improve, such as an apprenticeship or internship, may be considered high quality (Kalleberg, 1977). It is for this reason that positions with strong chances for advancement, or "career track" jobs, may be considered especially valuable with enough foresight (Huiras, Uggen & McMorris, 2000; Nagin & Paternoster, 1994). Also, in job markets that are highly competitive with few jobs and many job seekers, even a job that is fairly low in quality in some dimensions could be considered valuable if it would be particularly difficult to replace. Relatedly, jobs which are believed to be "stable" with high job security are also typically considered of higher quality (Kalleberg, 2013; Sampson & Laub, 1993). An example of a subjective measure of job quality used within the literature is job satisfaction, defined as "a function of the range of specific satisfaction and dissatisfactions that [a worker] experiences with respect to the various dimensions of work" (Kalleberg, 1977, p. 127).

While it is possible for a job to have some aspects of quality and lack others, a job's total net value is the result of both objective and subjective dimensions which can provide utility or disutility. This perspective is not only consistent with a rational choice framework, but also with literature from outside of criminology which has placed significant emphasis on the measurement of job quality in an effort to improve cross-national welfare comparisons. For instance, Dahl, Nesheim, and Olsen (2009) noted that while different academic traditions have conceptualized job quality in different ways, "there has been a convergence in positions" which not only recognizes the utility of a multi-dimensional approach, but also advocates the use of both objective and subjective measures (p. 5, see also Burchell, Sehnbruch, Piasna, & Agloni, 2014; Cazes, Hijzen, & Saint-Martin, 2015). It is the total value associated with a job, or its "quality" which should influence the decision to offend.

Dual labor market theory (DLMT) raises an important caveat – that quality job characteristics tend to cluster together (Doeringer et al., 1972; Harrison 1972; Piore 1968, 1972). Therefore, rather than conceptualize job quality as a continuous spectrum, dual labor market theorists have proposed there is utility in conceptualizing the labor market as segmented with only two types of jobs: high-quality jobs and low-quality jobs. This polarization is argued to have been exasperated by structural constraints and major economic shifts such as deindustrialization (Kalleberg, 2012, see also Crutchfield, 2014). Within this framework, high-quality jobs are referred to as primary sector occupations. These jobs are located at the core of the economy and have most, if not all, of the quality characteristics previously described (Crutchfield, 1989; Kalleberg, 1977; Piore, 1972). Conversely, dual labor market theorists refer to low-quality jobs as secondary sector occupations, located at the outskirts of the economy. These jobs are believed to be consistently plagued with adverse quality characteristics contrasting primary sector employment. It is for this reason that they have been infamously dubbed "McJobs," as fast food employment has become a classic exemplar of low-quality work (Crutchfield, 2014, p. 38).

Additionally, DLMT emphasizes that high-quality jobs are not randomly distributed geographically nor across individuals. This means that those with low human capital tend to reside in communities geographically isolated from high-quality work opportunities and socially isolated from others capable of connecting them with a high-quality job (Coleman, 1990; Wilson, 1987). Those with low-quality jobs are restricted from leaving the secondary market because of the limited supply of high-quality jobs and institutional constraints. This results in low labor market mobility as workers are frequently structurally barred from leaving the

secondary sector. Relatedly, those with increased human capital (e.g., higher levels of education, high-skill, innate ability) oftentimes also reside in socioeconomically advantaged areas. It is these individuals who are more likely to obtain, as well as maintain, primary sector employment.

Although DLMT was originally developed by economists to explain persistently poor labor market outcomes among disadvantaged workers (Doeringer et al., 1972; Harrison 1972; Piore 1968, 1972), it has also been applied to the study of crime. Specifically, Crutchfield (1989, 2014; see also Crutchfield & Pitchford, 1997) drew on DLMT to develop his labor stratification and crime thesis which proposed that high-quality jobs within the primary sector serve as a protective factor against crime because these types of jobs offer sufficient rewards to incentivize conformity.[5] Correspondingly, low-quality jobs within the secondary sector are less valuable so they do not increase the costs of crime to the extent that they effectively deter crime. This suggests that *only* high-quality jobs within the primary labor market are capable of deterring crime. Consistently, a simple increase in quality – a higher quality job – may not be enough to deter crime, as long as the job remains within the secondary market.

The distinction as to whether a high-quality job is required to observe a negative effect on crime or whether an improvement in job quality is enough to yield protective effects, is particularly salient for the group of individuals criminologists are most interested in deterring: high-risk individuals with past criminal involvement (hereafter "high-risk" individuals). This is

[5] DLMT and RCT are arguably entirely consistent with one another. Although structural approaches have traditionally remained outside the scope of RCT within criminology (Matsueda, 2013), there are examples of scholars incorporated contextual structural factors into explanations of individual-level decision making (Nguyen & Loughran, ND, "Incorporating Rational Choice and Structural Theories"; Paternoster & Iovanni, 1989). Although DLMT emphasizes that structural barriers limit the availability of quality job opportunities, the present study adheres to the assumption that individuals make rational choices within their restricted choice sets. Therefore, agency exists in the presence of structural constraints (see Paternoster, 2017 for an overview of human agency).

because employment problems are a known "collateral consequence" of involvement with the criminal justice system (Mauer & Chesney-Lind, 2002). For example, those with a criminal record are less likely to receive callbacks for job interviews (Pager, 2003; Pager, Bonikowski, & Western, 2009) and are less likely to be hired (Holzer, Raphael, & Stoll, 2006). Not only are employers reluctant to hire past offenders, but those with a criminal record are also legally banned from many jobs (Travis, 2002). Research suggests that at the aggregate level, countries with higher levels of incarceration have increased levels of unemployment in the long term, theoretically due to the reduced job prospects among past offenders (Western & Beckett, 1999). At the individual level, self-reported criminal behavior, a criminal record, and time incarcerated have also been negatively associated with employment (Freeman, 1987). Studies also suggest that those who have been incarcerated experience slower wage growth (Western, 2002) and detrimental long-term effects such as decreased likelihood of formal employment, increased likelihood and duration of nonparticipation in the labor market, and negative effects on annual income (Apel & Sweeten, 2010).[6] An individual's number of arrests have also been found to be associated with non-employment (Grogger, 1992). Even relatively minor first-time arrests have been found to be associated with job stability, measured by the number of weeks worked at the major source of employment over the last year (Bushway, 1998). Recently, Augustyn and Loughran (2017) investigated the effect of a juvenile waiver on labor market outcomes among a

[6] Apel & Sweeten (2010)'s study examined first-time incarceration, so results may not be generalizable to subsequent periods of incarceration. What is particularly interesting with respect to the present inquiry is that Apel and Sweeten (2010) did not find a significant relationship among first-time incarceration and many additional quality job outcomes such as, whether an individual was employed for 35 hours or more, had job benefits, was employed in an unskilled industry, was employed in the secondary occupation sector, or job satisfaction ratings on a scale from 1-5.

sample of juveniles adjudicated for a serious offense; concluding that those who were transferred to adult court had significantly lower average monthly wages seven years post-adjudication. In addition, scholars have found negative labor market outcomes associated with past criminal involvement are aggravated by race/ethnicity (Pager, 2003; Western & Pettit, 2005) and/or the cumulative effects of low socioeconomic status (Bernstein & Houston, 2000; Petersilia, 2005; Visher et al., 2008; Visher, LaVigne, & Travis, 2004; Western & Wildeman, 2009; Wilson, 1987).

Because of the known employment difficulties afflicting high-risk individuals, this group is theoretically more likely to be employed within the secondary sector (Sullivan, 1989). Travis (2005) made this point when he described those who have had experience with the criminal justice system as "a class apart" because of their many labor market disadvantages even when compared to others in the secondary sector (p. 159). Given the low market mobility proposed by DLMT, an improvement in job quality (higher job quality) while remaining within the secondary sector may be much more attainable than a high-quality job within the primary sector. From a policy perspective, it then becomes important to consider whether higher quality employment within the secondary labor market is capable of disincentivizing crime.

While there is very little empirically known about the variability in job quality amongst high-risk individuals; heterogeneity in job quality within the secondary market is theoretically conceivable. Though, DLMT can be taken at face value to suggest there is not important variation in job quality within markets. Consistently, high-risk individuals who are more likely to be employed within the secondary market may simply have uniformly bad jobs. The primary and secondary market dichotomy however, has been referred to as an "oversimplified distinction"

because there is a wide variety of occupations and job characteristics which fall within sector classifications (Crutchfield, 2014, p. 36; see also Crutchfield et al., 2006). Even DLMT's lead spokesperson, Piore (1972), later suggested there was important heterogeneity within market sectors and divided the primary sector into upper and lower tiers.

There are also additional theoretical tenants contrasting the DLMT notion that job quality characteristics uniformly cluster together. For instance, Smith's ([1776] 2003) hypothesis of compensating differentials suggests pay is positively associated with undesirable working conditions (see also Smith, 1979; Brown, 1980). This has implications within the secondary sector, as a job may be dangerous with some unsatisfactory characteristics (e.g., long periods of travel, undesirable hours, harsh climate), but higher pay may compensate for these negative job characteristics. Secondary sector workers may also have relatively low pay and few benefits but prefer the inherent job flexibility associated with the opportunity to control their own schedules, experience a variety of jobs and increased free time (Belous, 1989).

Therefore, the present research suggests there may be important heterogeneity in job quality within the secondary labor market. While traditionally RCT would suggest higher quality employment should have a negative effect on crime regardless of the market sector, DLMT suggests that higher job quality with a sector will not reduce crime because even higher quality secondary sector jobs are not valued to the extent that they act as effective deterrents. Moreover, higher quality employment within the secondary labor market may actually increase crime, if the job itself is not valuable enough to disincentivize crime and working provides opportunities for crime or financial means to partake in routine activities conducive to crime (Apel & Horney, 2017, Briar & Pilliavin, 1965; Haynie & Osgood, 2005; Osgood et al., 1996).

The present research will first descriptively consider the extent to which there is variability in job quality amongst a high-risk sample (Research Question 1). It will then consider the relationship between job quality and crime (Research Question 2) through testing the following hypotheses:

- **Hypothesis 1:** Higher job quality will be negatively associated with crime.
- **Hypothesis 2:** High (relative to low) job quality will be negatively associated with crime.
- **Hypothesis 3:** Within the secondary market, higher job quality will be negatively associated with crime.

Job Quality and Workplace Crime

The long-held assertion that any employment is monolithically protective against crime has not only has caused scholars to ignore theoretically important variation in job quality, but also to overlook the potential for crime to occur *within* the workplace. As individuals transition into adulthood, employment often becomes a "central experience" in the adult life course as a greater proportion of time is spent at work (Wilson, 1996, p. 52). Scholars hypothesizing a protective effect of employment have suggested that simply spending time in the workplace will reduce crime through limiting criminal opportunities (Laub & Sampson, 2003). In this sense, work is thought to have an "incapacitation" effect. Economists have also similarly portrayed crime as an illegitimate choice *alternative* to legal work (Ehrlich, 1973). While these propositions may be logically consistent when focusing on street crime as an outcome, they become inconsistent when considering the potential for workplace crime, where workplace crime is defined within this study as conventional forms of crime committed at the workplace by employees (Friedrichs, 2002). By definition, employment cannot have a negative relationship with workplace crime because employment is required for this crime to occur.

However, crime within the workplace is not a new phenomenon within criminology, as white-collar crime has been studied for decades (Sutherland, 1949, see also Benson & Simpson, 2015 for an overview). White-collar crime is a broad topic which covers a range of socially harmful behaviors committed by individuals and corporations within the course of their occupational and organizational undertakings. Crime generally considered within offense-based definitions of white-collar crime such as antitrust violations, embezzlement, bribery, tax invasion, false claims, lending and credit fraud, and mail fraud will not be considered within this study (Benson & Simpson, 2015). Nor, on average, will individuals of "respectability and high social status" who commit crime during the course of their trusted occupation (Sutherland, 1949, p. 9). Rather, this study seeks to examine one specific type of workplace crime committed by fairly low-level workers—employee theft. Employee theft (hereafter used interchangeably with "workplace crime") is defined as "the unauthorized taking, control, or transfer of money and/or property of the formal work organization that is perpetrated by an employee during the course of occupational activity" (Hollinger & Clark, 1983b, p. 2).[7]

The lack of theoretical discourse surrounding employee theft is a key oversight given that research suggests the offense is highly prevalent. For instance, Hollinger and Clark (1983b) estimated that 25-50% of the work force within retail stores, hospitals, and electronics manufacturing firms were involved in employee theft; an estimate which has stood the test of time (Wimbush & Dalton, 1997; see also Sauser, 2007 for an overview). Estimates may be even higher within specific industries, given that 60% of employees in the fast food sector were found

[7] Though workplace violence is also an important area of inquiry, this type of crime will not be considered within the present study. This distinction is consistent with literature which has generally maintained workplace theft and violence as two separate spheres of theoretical discourse.

to have self-reported theft within the last 6 months (Hollinger, Slora, & Terris, 1992). Though there are no known estimates of employee theft prevalence with a high-risk sample, it is likely that the crimes prevalence may be even higher among those with past histories of criminal involvement.

Theories used to explain workplace crime are often consistent with criminological conceptions of RCT, though not often labeled as such. For example, Hollinger and Clark (1983, p. 8) noted that theories of workplace crime often assume "that everyone is basically greedy and dishonest by nature, with larcenous tendencies lurking only barely beneath the surface" – acknowledging underlying self-interest assumptions (see also Lipman, 1973). The benefits of workplace crime are also frequently emphasized, as employee theft has been referred to as "the hidden, shadow, secret, irregular, or black economy" because of the potential for financial rewards in addition to the legal job rewards (Cornford, 1982, p. V).

Theorists have also suggested one way to prevent workplace crime may be to "bolt everything down [reduce opportunities] and watch everyone [increase the certainty of sanctioning]" (Hollinger & Clark, 1983b, p. 69). Consistent with general criminological notions of deterrence, those studying workplace crime have found the perceived certainty of apprehension to be negatively associated with employee theft (Hollinger & Clark, 1983a). Though practitioners have voiced concerns regarding the high costs associated with increasing the certainty of detection in practice, questioning whether this is the most feasible and cost-effective policy (Leatherwood & Spector, 1991).

The perceived severity of sanctioning has also been shown to deter employee theft (Hollinger & Clark, 1983a). However, scholars have emphasized that formal severity, in

practice, is very low because employers do not frequently report theft nor file formal charges (Hollinger & Clark, 1983b). Instead, businesses often choose to handle employee theft cases with alternative non-criminal sanctioning to avoid negative publicity, retaliation, retribution, and appearing as if they are easy targets (Hollinger & Davis, 2006). Concordantly, the perceived severity of formal sanctioning is also quite low as employees "know that realistically the worst consequence which can happen to them is that they will be fired" (Hollinger & Davis, 2006, p. 219).

Therefore, employees will not be deterred by the threat of detection if the sanctions they expect to receive—primarily the loss of employment—are not costly enough. Hollinger and Clark (1983a) in particular, highlighted the threat of being fired as a weak punishment among those at the bottom of the status hierarchy with low-quality jobs which can be readily replaced. Consequently, while a formal "enforcement" approach emphasizing increasing the certainty and severity of formal sanctioning is consistent with RCT, an alternative and equally consistent approach focuses on improving the conditions of the job itself, thereby increasing the informal cost of crime (Leatherwood & Spector, 1991; see also Boye & Jones, 1997).

The theoretical emphasis on job quality within the employee theft literature can be traced back to early qualitative field studies which illustrated that employees with low-quality positions chose to supplement their wages with theft (Ditton, 1977; Mars, 1973). Hollinger and Clark (1983) drew on this work to stress that workplace crime may also be caused by internal job-related factors which, at the time, was a far less palatable explanation than assuming conditions

external to the workplace or traits of the employee were to blame for deviance.[8] They proposed that employee theft may be "a response to the perceived quality of the employment experience," as employees who were more satisfied with their jobs were the least likely to commit theft (Hollinger & Clark, 1983b, p. 79). Leatherwood and Spector (1991) similarly suggested that employee theft may be discouraged by improvements to job quality referred to as "inducements." They reasoned that jobs with superior inducements could not readily be obtained elsewhere and thus inspired strong commitment to the organization so that even low chances of termination would act as a powerful disincentive to theft (Leatherwood & Spector, 1991).

The idea that job quality inversely effects workplace crime has now become a fairly widely recognized articulation, which has been referred to generally as the marginality proposition (Greenberg & Barling, 1996). Akin to RCT and DLMT, the marginality proposition suggests that jobs of marginal quality will be associated with deviant behavior (Greenberg & Barling, 1996). In addition, temporary work has been described as more likely to result in employee theft because temporary workers do not have time to develop commitment to an organization or relationships with colleagues (Hollinger, 1986; Tucker, 1989).

Though theories of workplace crime have intimated that job quality influences employee theft in ways which are consistent with RCT, this literature has not yet drawn on DLMT to discuss how labor market segmentation influences this relationship. Though theoretically, job quality will deter workplace crime in the same manner which it deters street crime, consistent with Hypotheses 1–3. This expectation is consistent with previous scholarship which has argued

[8] This does not necessarily mean that external conditions do not influence the decision to commit crime in the workplace, but rather, that internal conditions *also* also influence the choice to offend. In fact, job quality may be heavily influenced by labor market conditions.

that RCT is a general theory which similarly applies to all types of crime (Loughran et al., 2016b). The present research will evaluate whether the proposed relationship between job quality and street crime extends to crime in the workplace (Research Question 3) by testing the following hypotheses:

- **Hypothesis 4:** Higher job quality will be negatively associated with workplace crime.
- **Hypothesis 5:** High (relative to low) job quality will be negatively associated with workplace crime.
- **Hypothesis 6:** Within the secondary market, higher job quality will be negatively associated with workplace crime.

Job Quality and The Street-Workplace Crime Overlap

There has been very little discourse within the employment and crime literature considering street and workplace crime within the same theoretical framework as these outcomes have generally remained separate academic traditions. This is problematic because when studying the employment-crime relationship, scholars tend to hypothesize a monolithically negative relationship without recognizing that when a high-risk criminally-involved individual becomes employed, at least four outcomes may actually result. First, as traditionally hypothesized, an individual may be deterred from crime generally, choosing not to commit crime in the street nor in the workplace. Second, an individual may be deterred from committing street crime, but choose to commit crime within the workplace. In this sense, employment may result in crime being displaced from the street into the workplace, where displacement is defined within the context of this study as an increased probability of committing workplace crime relative to street crime for an employed individual. Third, employment could result in crime be committed on the streets and within the workplace. Fourth, an individual may continue to

commit street crime, but choose not to commit crime at work, in which case employment has little-to-no effect on the likelihood of committing crime. The probability of any of these potential outcomes may be influenced by the quality of the job itself.

The first potential outcome, where an employed individual is deterred from crime both in the street and in the workplace, is the outcome most frequently hypothesized by criminologists who propose employment has a general protective effect (e.g., Sampson & Laub, 1993). As previously articulated, the likelihood that employment will deter crime is theorized to be influenced by the quality of the job. Specifically, the present study hypothesizes that a high(er) quality job will be negatively associated with both street and workplace crime. Therefore, those with high(er) quality jobs are more likely to be deterred from crime generally, both in the street and in the workplace.

The second potential outcome, where an employed individual is deterred from committing street crime but chooses to commit crime at the workplace, is referred to as displacement. Displacement can be problematic from a policy perspective because employment may solve one problem (street crime) while simply creating another (workplace crime). If employment is associated with a reduction in street crime solely due to an incapacitation effect (Laub & Sampson, 2003), one may expect simply having a job and working more hours to increase the likelihood of workplace crime relative to street crime without job quality having an influence. From a rational choice perspective, however, the decision to commit workplace crime relative to street crime may be somewhat complex.

For example, one may choose to commit workplace crime over street crime if they believe benefits of employee theft supersede the benefits associated with street crime, holding all

else constant. Though there are no known studies which have compared perceived benefits of street crime with those of workplace crime, there is some literature which suggests that illegal earnings are often comparable to legal minimum wages (Reuter, MacCoun, Murphy, Abrahamse, & Simon, 1990; Levitt & Venkatesh, 2000). Therefore, even if an individual is only able to earn the minimum wage at a legal job, the individual may choose to work and commit workplace crime over street crime because he or she can *simultaneously* earn rewards from legal work and theft, resulting in a larger financial net return. In this sense, there is a lower opportunity cost associated with workplace crime because the individual does not have to allocate time away from legal work (Lochner, 2004).

The certainty of apprehension and severity of formal sanctioning may also influence the decision to commit workplace crime relative to street crime. Though there are no known estimates of perceived certainty of apprehension for street crime relative to workplace crime, in theory an individual would choose to commit workplace crime if there is a lower certainty of apprehension (Hollinger & Clark, 1983a; Paternoster, 2010). Though severity of legal sanctioning is generally thought to be a fairly weak form of deterrence (Paternoster, 2010), the low punishment severity associated with workplace crime may make it a more favorable location for crime given the very low likelihood that an apprehended employee is formally sanctioned (Hollinger & Davis, 2006).

Additionally, the choice to commit workplace crime relative to street crime likely depends on the quality of the job. If apprehended committing theft in the workplace, employees are "almost certain to be terminated," resulting in a high informal cost of crime for those with valuable jobs (Hollinger & Clark, 1983b, p. 103). Individuals with high(er) quality jobs may

therefore be more likely to commit crime where they are least likely to lose their job: away from the workplace. Although no known literature provides estimates of the likelihood of job termination if an individual is apprehended for street crime relative to workplace crime, termination is arguably less likely for street crime. To be fired, an employer would not only have to find out about the indiscretion, but also decide the offense warrants termination. While this may happen if an individual is forced to miss work because of involvement with the criminal justice system (e.g., court dates, incarceration), or an employer is contacted as a result of sanctioning (e.g., probation), these outcomes are less direct and require a chain of events to result in termination. Workplace theft only requires apprehension for the offense to be known to the employer and likely result in being fired. For this reason, those with higher quality jobs are arguably less likely to commit crime in the workplace relative to the street, holding all other decision-making inputs constant.

Employment may also result in an increased likelihood of both street and workplace crime if an individual who had previously been committing only street crime chooses to now also commit workplace crime. This outcome suggests that when individuals become employed the job may provide additional opportunities for crime within the workplace in which they will take advantage. While some theories of crime may suggest that crime-prone individuals will commit crime indiscriminately in the streets and in the workplace (Gottfredson & Hirschi, 1990; Moffit, 1993), the present argument suggests that the likelihood of committing both will be influence by job quality. Specifically, those with low(er) quality jobs have little to lose and thus are more likely to commit both types of crime.

The fourth potential outcome, is one in which an employed individual continues to commit street crime but is unlikely to commit crime in the workplace. Consistently, there would be no evidence of a decreased likelihood of street crime, no evidence of an increased likelihood of committing workplace crime relative to street crime , nor any evidence of an increased likelihood of committing both types of crime relative to street crime. Those who continue to commit street crime during periods of employment likely have low(er) quality jobs which are not valuable enough to deter them from crime in the street, but their job quality may be just high enough to deter them from crime within the workplace, holding all else constant.

The present research will evaluate whether job quality has an association with whether an employed individual commits no crime, crime in the street only, crime in the workplace only, or both (Research Question 3a), through testing the following hypotheses:

- **Hypothesis 7a:** High(er) job quality will be positively associated with committing no crime.
- **Hypothesis 7b:** Low(er) job quality will be positively associated with committing both street and workplace crime.
- **Hypothesis 7c:** High(er) job quality will be positively associated with committing street crime relative to workplace crime.

In sum, the present research seeks to contribute to the field's understanding of the employment-crime relationship through focusing on job quality as multidimensional potential deterrent and expanding crime outcomes to also consider crime in the workplace. Table 1 provides a summary of the proposed research questions and associated hypotheses. The following chapters will provide a review of the literature, describe the study sample and measures, detail the analytic plan for each research question, present results, and provide a discussion of the substantive findings, study limitations, and directions for future research.

CHAPTER 2: LITERATURE REVIEW

Variability in Job Quality Within a High-Risk Sample

Though previous research has demonstrated that involvement with the criminal justice system is associated with poor labor market outcomes, there has been very little research examining variability in job quality *within* a high-risk sample with past criminal involvement. Scholars have at least tacitly assumed that this group will obtain uniformly low-quality jobs, though there are no known descriptive statistics reporting the proportion of jobs which are low quality and/or within the secondary labor market amongst a high-risk sample. There is some evidence, however, that jobs obtained by this group are low in quality. For instance, recent study showed that the jobs obtained by released prisoners were highly concentrated in certain lower quality industries such as construction, food services, retail, and manufacturing, consistent with DLMT (Schnepel, 2018). A study of adult males released in 1978 from New York City's Rikers Island Correctional facility also provided evidence of low variability in job quality, given that nearly all of the respondents reported job titles in the bottom third of an occupational prestige scale and over half reported jobs in the bottom sixth of the scale (Sviridoff & Thompson, 1983). As a whole, Sviridoff and Thompson (1983) described the men's jobs as "uniformly low-level, unskilled or semi-skilled employment", with 32% at or below minimum wage (p. 199). Cook et al. (2015) also emphasized low average annual earnings reported by violent offenders who reentered the community from the Wisconsin Department of Corrections.

Visher et al.'s (2004) study of released inmates however, provided some evidence that job quality may not be uniformly low within a high-risk sample, particularly when subjective appraisals of quality were considered. Specifically, the study found that a high percentage of

those released from prison who found a job liked their work (86%), would be happy if they were still working there a year later (70%), got along with their coworkers (87%), and were treated fairly by their employer (89%) a month following release (Visher et al., 2004). However, about half of the respondents were not satisfied with their pay (Visher et al., 2004). Respondents also continued to be highly satisfied with their job at the second follow-up (4 to 6 months post release), though at slightly lower levels (Visher et al., 2004). This research suggests that even high-risk individuals may at least be somewhat satisfied with their jobs, such that even fairly low-waged jobs may have some quality characteristics. This provides evidence that there is likely variation in job quality within a high-risk group which may be capable of deterring crime.

In addition, Kalleberg et al. (2000) contributed evidence of important heterogeneity in job quality within the secondary sector when they examined the relationship between "nonstandard work" and job quality.[9] They found that although nonstandard work increased the likelihood that workers had bad jobs, some nonstandard work arrangements (i.e., temporary-help agency employment, regular part-time work, wage-and-salaried independent contracting) were worse than others. This research can be interpreted to suggest that even though those who are high-risk may be more likely to obtain nonstandard secondary sector jobs, there is variation in job quality within this low-quality sector where some jobs are better than others.

Job Quality and Crime

Though experimental program evaluations have generally failed to demonstrate joint improvements in employment and reductions in offending (Bushway & Apel, 2012; Visher et al.,

[9] Nonstandard work was defined as "employment relations other than standard, full-time jobs, including part-time employment in an otherwise standard work arrangement, day labor and on-call work, temporary help agency and contract-company employment, independent contracting, and other self-employment" (Kalleberg et al., 2000, p. 257).

2008), there have been three recent experimental program evaluations which warrant discussion because they demonstrated significant increases in employment and reductions in recidivism: The Minnesota Comprehensive Offender Reentry Plan (MCORP) pilot project (Duwe, 2012), the Milwaukee Safe Street Prisoner Release Initiative (PRI) (Cook et al., 2015), and One Summer Plus (OSP) (Heller, 2014).

MCORP was designed to reduce recidivism through establishing a dynamic case planning and management system which provided a range of services while individuals were incarcerated which continued into their release (Duwe, 2012). Within the first six months following release, MCORP participants were significantly more likely to obtain employment and less likely to be rearrested, reconvicted, and incarcerated. However, the study only considered whether participants obtained employment at any time following their release from prison and did not provide any descriptive information on the quality of the jobs which were obtained.

PRI was also designed to provide increased employment opportunities to incarcerated individuals reentering the community (Cook et al., 2015). PRI participants received extensive "reach in" (interventions while the offenders were still incarcerated) and "wrap-around" (services available to individuals after they were released) programming. These services included access to education, vocational assessments and training, assistance with finding jobs, and perhaps most influential, job creation through employer subsidies. Results from the PRI evaluation indicated that, one year after release, the treatment group had increased employment, increased earnings (largely due to an increase in hours worked because there was not much variation in hourly wage) and decreased rearrests.

The results from MCORP and PRI are promising in that they both demonstrated a joint increase in employment and decrease in offending, suggesting some aspect of the programs worked. Notably however, in both programs, participants' "treatment" included a wide range of additional services that were not solely employment-related, making it difficult to attribute reductions in recidivism directly to improvements in employment. In addition, these program evaluations focused on the effectiveness of the programs and were not designed to assess whether job quality influenced the relationship between employment and crime.

Heller's (2014) experiential evaluation of the OSP program for high risk youths in Chicago sheds some light onto the potential mechanisms in which employment may reduce crime, though the youth-specific effect of employment on crime is not a key focus of this study. The randomized controlled trial offered an 8-week summer employment program in which community organizations placed youths in jobs (e.g., summer camp counselors, working in community gardens, office assistants) and paid them the minimum wage. Findings indicated that OSP decreased violent crime arrests by 43% over 16 months. The results indirectly suggested that the findings were not attributable to an incapacitation effect (youths spending more time off of the streets) nor due to increasing pro-social educational values (youths spending more time in school). This program was likely effective because it increased youths' income, provided mentorship, provided job skills, facilitated connections with employers, and targeted the high-risk group early in the life course.

Although job quality was not a key focus of the evaluation's discussion (Heller, 2014), it is theoretically plausible that providing high-quality jobs influenced the success of the program. Even though the program offered uniformly minimum wage jobs ($8.25/hour), the jobs may

actually have been perceived as high in quality given that minimum wage is fairly typical for high-school aged youths. In addition, "the service providers locat[ed] jobs with the goals of aiding youth in exploring career interests and aptitudes, building vocational knowledge, developing team and leadership skills, and practicing creative thinking and problem-solving" (Heller, 2014, p. 10). This description does not match that of traditional low-quality secondary sector positions. If youths were given jobs which they felt were career track and that they were subjectively satisfied with, the job may have been high quality, and this may have been the mechanism deterring youths from crime.

Scholars evaluating the relationship between employment and crime using observational research have improved causal inferences through increasingly relying on quasi-experimental methods and longitudinal data to account for preexisting individual differences and temporal ordering (see Uggen & Wakefield, 2008 for an overview). Despite improved methodological sophistication, limitations within this literature persist, particularly with respect to a lack of emphasis on job quality. The studies which have considered the relationship between job quality and crime remain inconclusive, with some studies supporting a negative association as hypothesized and others failing to do so. While inconsistent findings may be attributed to factors associated with sample selection (e.g., the age, sex composition, historical time period, country, and risk-level of the sample), they are also likely attributed to a limited operationalization of job quality.

Higher Job Quality and Crime

Objective measures of job quality emphasizing wages, hours and benefits have produced the most inconsistent findings with respect to Hypothesis 1 which proposes that higher job

quality will be negatively associated with crime. This may be because these measures do not capture the additional, and often subjective, job characteristics which may influence a job's total value. While some studies have determined that higher wages were negatively associated with crime (Grogger, 1998; Myers, 1983, 1984; Uggen & Thompson, 2003; Witte, 1980), others have found no support for the relationship (Apel & Horney, 2017; Crutchfield & Pitchford, 1997; Sampson & Laub, 1993; Wadsworth, 2006; Williams & Sickle, 2002). Additionally, analysis of the National Supported Work Demonstration (hereafter "Supported Work") generated crime-specific results which did not support pecuniary motives, given that higher wages were only found to deter non-economic offenses (Uggen, 1999). Increased hours of work have also not been found to be reliably protective against crime in high-risk samples (Apel & Horney, 2017; Paternoster, Bachman, Kerrison, O'Connell, & Smith, 2016). While there is evidence that jobs with fringe benefits are negatively associated with property crime and marginally related to violence, only a single known criminological study has examined job benefits as a measure of job quality (Wadsworth, 2006). Notably, the study's benefits scale was comprised of only two (i.e., health insurance and paid vacation) of many potential job-related fringe benefits. The study also faced selection issues because respondents were only asked about employment characteristics if they were working full-time (Wadsworth, 2006).

Job stability has become one of the most common proxies for quality within the literature though it is rarely found to be associated with crime. Sampson & Laub's (1993) measure of job stability was a composite scale which included employment status (whether the individual was employed at the time of the interview), stability of most recent employment (the length of their most recent or present employment in months), and work habits (a three-point scale where

individuals were classified as having poor, fair, or good job habits). They demonstrated that job stability significantly explained variation in adult criminal behavior. Within the majority of scholarship however, stability has simply been operationalized as duration of employment, such as the length of time continuously employed with the same employer (Apel & Horney, 2017; Uggen, 1999), the length of time continuously employed at any job (Apel & Horney, 2017; Van Der Geest, Bijleveld, & Blokland, 2011; Verbruggen et al., 2012), the overall or average length of employment in a given time period (Apel & Horney, 2017; Blokland and Niuwbeerta, 2005; Wadsworth, 2006), or as expectations of continued employment (Crutchfield & Pitchford, 1997; Giordano, Cernkovich, & Rudolph, 2002). Regardless of the specific operationalization, results overwhelmingly suggest that duration is not associated with criminal outcomes. This inconsistency may be attributed to the fact that duration measures are naturally confounded with age, time, and external economic conditions and thus duration measures may not be measuring job quality as intended.

Higher occupational status based on job title has also not been found to be related to crime. For instance, Giordano, Schroeder, and Cernkovich (2007) evaluated the relationship between occupational prestige and crime among adults with a past history of offending; measuring prestige on a scale from 1 to 7 where executives, administrators and managers ranked the highest and service workers and laborers were coded the lowest. They did not find a relationship between occupational prestige and criminal involvement, perpetration of relationship violence, drug or alcohol abuse, nor life course offending patterns. Occupational prestige was, however, able to distinguish desisters from persisters and unstable offenders in supplemental models looking specifically at patterns of violence across the life course. Crutchfield and

colleagues (2006) also did not find an association between occupational status and crime using a similar 1 to 5 occupational classification scheme within a national representative sample. This lack of support is not surprising given that occupational status is not a strong approximation for job quality because the job titles used to create these categorizations do not reflect variation in job quality within occupational categories (Jencks et al., 1988).

Relatedly, scholars measuring job quality through quasi-subjective approximation scores drawn from pre-developed title-based scales have established mixed support. These quality measures are considered quasi-subjective because they were drawn from aggregate mean scores rather than established through an individual's subjective assessments of his or her particular job. For example, Uggen (1999) classified jobs into one of eight occupational groups (professional and technical, managerial and administrative, sales, clerical, craft, operative, laborer, and service) and then classified based on skill level and industry (Uggen, 1999). The jobs were then assigned a mean overall satisfaction rating from the 1977 Quality of Employment Survey (QES) (Quinn & Staines, 1979). The QES measures were based on an average score regarding work quality conditions within a nationally representative sample of 1,515 workers who were 16 and older (Quinn & Staines, 1979). However, QES only included those who were working for 20 or more hours per week (Quinn & Staines, 1979), so the survey may have missed sampling individuals who were more drastically underemployed. Nonetheless, Uggen's (1999) findings suggested that job quality was strongly related to economic and non-economic criminal behavior. Blokland & Nieuwbeerta (2005) also applied an occupational status score drawn from an outside classification scheme to their 1996 Dutch National Crime Survey (NCSR) data but did not find any significant relationship between occupational status and offending.

Despite the relevance of perceived job quality within RCT, very few studies have measured quality with subjective dimensions, though the studies which have produced promising results which most consistently support Hypothesis 1. To illustrate, in a relatively early study, Meisenhelder (1977) used unstructured interviews to evaluate the role of social bonds in facilitating desistance from property offending. He found that the likelihood of exiting a criminal career increased if an individual acquired what "they perceived as a good job... defined as subjectively meaningful, economically rewarding, and indicative of some career potential" (Meisenhelder, 1977, p. 326). More recently, Paternoster and colleagues (2016) studied a serious drug-involved sample of adult incarcerated offenders and found that those who self-reported getting a "good job" or "any job they liked" during the 6-month period following release had a significantly longer time until re-arrest over a nearly 20-year follow-up period. Additionally, Simons, Steward, Gordon, Conger, & Elder (2002) implemented a 23-item Job Satisfaction Scale created for the Iowa Youth and Families Project to measure individuals' perceived job rewards and frustrations, which they referred to as "job attachment." Their findings indicated that increased job attachment was found to be negatively associated with criminal behavior among males but not females (Simons et al., 2002). Interestingly however, strong job attachment moderated the chances that a female with a delinquent history would commit crime in adulthood (Simons et al., 2002). Wadsworth's (2006) study using data from the 1979 and 1980 waves of the National Longitudinal Study of Youth (NLSY) also measured "rewarding job attributes" as five-item subjective scale, capturing perceptions of promotional opportunities, chances to maximize ability, satisfaction with income, feeling a sense they were making a significant contribution, and job security. His findings indicated that rewarding job attributes in 1979 were

statistically associated with decreased property and violent crime in 1980. Notably, Wadsworth's (2006) study was one of the few to consider subjective dimensions of job quality as well as objective quality indicators (income and benefits), though these dimensions were evaluated as individual items rather than considering job quality as a multidimensional latent construct.

In addition, Apel and Horney (2017) measured job quality through asking respondents to rate their level of commitment to a job on a 5-point scale ranging from "just a job" to "a job I was very committed to" (Apel & Horney, 2017, p. 11). They found that employment had a direct negative effect on crime, but only when their respondents felt a strong commitment to work. They also found that commitment had a much more robust influence on crime than objective measures of job quality such as income and hours (Apel & Horney, 2017). Though notably, this subjective appraisal of commitment did not necessarily capture job quality. While job commitment may be influenced by the value of the job, these two constructs are not synonymous. There may be other factors (e.g., spouse, dependent children, financial problems, identity) which influence job commitment independent of job quality.

High Job Quality and Crime

Research has generally supported DLMT's propositions that high (as opposed to low) quality jobs deter crime, consistent with Hypothesis 2. For example, Crutchfield, Wadsworth, Groinger, and Drakulich (2006) analyzed a subsample of adults from the third wave of the 1997 NLSY cohort ("NLSY97") and classified jobs as primary or secondary based on their title. Controlling for individual-level demographics, parental socio-economic status, school, and census-tract neighborhood variables, they found that marginal employment at the time of interview (unemployed or employed in the secondary market) was negatively associated with

crime. They also found that unemployment and being employed within the secondary sector were each independently associated with crime, though these results were not consistent for rural census tracts. Crutchfield et al. (2006) noted that they "experimented with a great many possibilities for measuring the quality of employment" within their preliminary analyses, including measures of benefits and job satisfaction but they "did not perform as well as the two measures of quality that [were] retained" (p. 26). The preliminary analyses however were not reported and thus conclusions cannot be drawn as to how they substantively influenced crime. In addition, there was no theoretical discussion as to why certain measures did not perform as well as others. Answering these questions is a key contribution of the present study.

Crutchfield and Pitchford (1997) also examined the relationship between occupational sector and crime in an earlier study using the 1979 NLSY cohort. Their findings however, did not indicate that primary sector employment was significantly associated with criminal behavior, while controlling for individual and county-level factors. It is important to address that both studies (Crutchfield & Pitchford, 1997; Crutchfield et al., 2006) were limited in that they focused on employment at the time of the interview rather than across the entire follow-up period. In addition, analyses were cross-sectional, so individual differences likely contributed to selection and temporal ordering could not be established.[10] This research was also conducted using samples which were representative of the U.S. population. While a representative sample may be appropriate for examining the relationship between market sector and crime generally, it

[10] In fact, causal ordering is likely reversed given that employment measures were based on employment at the time of the interview and crime measures were retrospective self-reported crime in the prior year. This concern is exemplified in the following interpretation: "Those who were working were less likely to have reported that they had engaged in criminal behavior in the year prior to their interview" (Crutchfield et al., 2006, p. 33).

provides no insight into whether this finding is generalizable to a high-risk sample, especially if few reach primary sector employment.

Though not always referring to jobs as primary or secondary, additional studies have also supported that high-quality jobs deters crime. For instance, Farrington, Gallagher, Morley, St. Ledger, and West (1986) classified jobs as "low status" if they were semi-skilled or unskilled manual occupations and "high status" if they were skilled or non-manual, concluding that unemployment following a low-status job was associated with increased offending, whereas unemployment after a high-status job was not associated with offending. Additionally, Staff et al. (2010) found that men who worked "professional" jobs were less likely to drink heavily or use marijuana, but they drank with increased frequency. Van Der Geest, Bijleveld & Blokland (2011) compared temporary employment (employment through a temporary job agency which is often seasonal, or project based) to regular employment (on payroll, business ownership, sheltered workshops) among high-risk Dutch men and found that high-quality regular employment had slightly stronger effects on offending, although both types of employment reduced offending.

A recent study estimating the impact of labor market opportunities on recidivism also found results consistent with theories of labor market stratification which emphasize job quality by occupational sector (Schnepel, 2018). Schnepel (2018) specifically found that offenders released from prison in California were less likely to recidivate if there were more opportunities for jobs in construction and manufacturing at the time of release. Though not directly measured, this finding was attributed to the higher wages and other benefits associated with these types of jobs, relative to low-quality jobs in retail or fast food.

This body of literature has two key limitations which the present study seeks to address. First, segmented labor market operationalizations overwhelmingly rely on job title to classify jobs as high- or low-quality. This is problematic because title-based classifications do not consider the many job characteristics which are known to influence quality (Jencks et al., 1988). Title-based classifications also do not consider individuals' variation in objective dimensions of job quality nor variation in subjective appraisals of individuals' particular jobs, which are central to rational choice conceptualizations of the decision to offend. Additionally, this work tends to focus only on whether a high relative to low quality job reduces crime, without also considering whether simply a high*er* quality job is capable of deterring crime. Looking at these two possibilities in tandem is important from a policy perspective and a key contribution of the current study.

Higher Job Quality Within Labor Markets

Though there is some prior empirical support that both higher (Hypothesis 1) and high (Hypothesis 2) job quality are negatively associated with crime, as detailed above, there are no known studies which have directly examined the relationship between higher job quality and crime within high and low-quality labor markets. Therefore, there is no known evidence which explicitly supports nor opposes Hypothesis 3, which proposes that there will be a negative relationship between higher job quality and crime even within the secondary labor market. There is however, some indirect evidence suggesting that higher job quality within the secondary market may reduce crime. Particularly, the previously reviewed studies which investigated higher job quality within a high-risk sample may be capturing the effect of higher job quality within the secondary market (e.g., Apel & Horney, 2017; Paternoster et al., 2016). This of

course, relies on the assumption that all previously incarcerated individuals had jobs within the secondary sector. This empirical question will be explicitly evaluated within the present study.

Job Quality and Workplace Crime

Although none of the previously discussed studies examined crime in the workplace as an outcome, there is a body of literature which has evaluated the relationship between job quality and workplace crime. Findings from which, largely support Hypothesis 4 and 5, indicating that high(er) job quality will also be negatively associated with workplace crime; though support for these hypotheses is stronger when subjective measures of quality are used, consistent with conclusions from the street crime literature.

Studies focused on explaining workplace crime have failed to find consistent support for a relationship between objective job quality measures and employee theft. For instance, while early work showed that low wages or income had an association with employee theft (Mars, 1973; Ruggiero, Greenberg, & Steinberg, 1982), more recent research has failed to find an association (Hollinger & Clark, 1983; Huiras et al., 2000). There is some evidence however, that theft rates increased when pay was reduced amongst manufacturing employees (Greenberg, 1990). In addition to income, Huiras et al. (2000) considered additional measures of objective work quality using data from the Youth Development Study (YDS), a study which began in 1988 surveying adolescent in St. Paul, Minnesota and followed-up with respondents when they were 24–25 years old. They found that neither income, work sector, training, turnover, nor benefits

had an association with employee deviance.[11] In fact, the only objective indicator found to be positively associated with deviance was holding a position of authority (Huiras et al., 2000).

Contrasting the weak findings surrounding objective job quality and workplace theft, research has supported the relationship between subjective job quality and workplace crime. For instance, Hollinger and Clark (1983b) surveyed over 9,000 employees from 47 businesses in three sectors (retail, hospitals, and electronic manufacturing) located in Minneapolis- St. Paul, Cleveland, and Dallas-Fort Worth. They found a negative correlation between job satisfaction and workplace theft among retail and hospital employees, but not within the manufacturing sector. They attributed their lack of findings in the manufacturing sector to the low-value of manufacturing parts available to steal. Hollinger (1986) however, did not find above average job satisfaction to be associated with above average employee theft in a follow-up study using logistic regression, though currently looking for another job was found to be significant. Baumer and Rosenbaum (1984) found theft increased among employees who were dissatisfied with their supervisors, organization, opportunities for promotion and workload in a survey of over 1,400 employees within nine retail companies. Huiras et al. (2000) also demonstrated that jobs which corresponded to employees' long-term career goals and jobs with higher satisfaction were independently negatively associated with employee deviance. In addition, Bolin and Heatherly

[11] Notably, their measure of employee deviance was broader than employee theft. Employee deviance was measured as a 9-item summated frequency scale which combined theft of money or items (the number of times respondents gave away goods or services, took things from their employer or coworker, misused or took money, and claimed to have worked more hours than really did) and production deviance (the number of times participants got to work late without a good reason, called in sick when not sick, and been drunk or high at work), vandalism (number of times purposefully damaged property) and general deviance (number of times lied to get or keep job) (Huiras et al., 2000, p. 250).

(2001) found employee dissatisfaction to be associated with employee theft within a sample of entry-level restaurant and supermarket store employees.

Notably however, the literature evaluating the relationship between job quality and workplace crime has limitations. For instance, much of this research presented only bivariate correlations and did not control for omitted variables, individual differences which influence job selection, nor temporal order (e.g., Baumer & Rosenbaum, 1984; Hollinger & Clark, 1983). Huiras et al. (2000) was the only study reviewed which used longitudinal data. Their analysis however included employment and deviance variables from a single wave of the study (respondents age 24–25, 10th wave) and prior deviance lagged measures from when the respondents were in high school to account for preexisting individual differences which may result in selection into employment and crime. In addition, no known study has evaluated the relationship between job quality and workplace crime among a high-risk sample.

Job Quality and the Street-Workplace Crime Overlap

Street and workplace crime have traditionally been separated in theoretical discourse. Therefore, it is not surprising that there is only one known study which has directly considered both outcomes.[12] Svirdioff and Thompson (1983) directly examined the street-workplace crime overlap among adult male inmates released from the New York City Correlational Institution. Notably, they found evidence of rational decision-making among the men they interviewed,

[12] Some may consider literature evaluating the overlap between white collar crime and common offenders an exception. This literature overall concludes that white-collar offenders are not demographically similar to offenders convicted of street crimes (see Benson & Simpson, 2015 for an overview). Although, this literature does not consider the extent to which the same individual commits both types of crime. In addition, the white-collar offenses evaluated in these studies (e.g., bank embezzlement, mail fraud, bribery) are not consistent with employee theft as conceptualized within this study. Therefore, only a very limited parallel can be drawn.

stating a respondent was "the perfect 'economic man,' weighing the costs and benefits of alternative activities" (Sviridoff & Thompson, 1983, p. 210). They also concluded that although work and crime were mutually exclusive alternatives for many releases, for others, legitimate employment was a means to expand criminal activities. For instance, when an individual was asked how much he was making at his last job, he responded "$100 and all I can steal" (Sviridoff & Thompson, 1983, p. 202). Another respondent not only reported theft at every job he ever had, but also provided evidence of crime displacement given that, "when he was not working he became heavily involved with drugs, drinking, armed robbery, and street crime. When he was working, all his crime was job related" (Sviridoff & Thompson, 1983, p. 202).

In addition, there is some empirical evidence which indirectly suggests overlap may exist, contrary to traditional criminological notions that employment is monolithically protective. For instance, research has found known individual-level correlates of general crime such as age, low self-control, and past deviance to also be associated with employee theft (Hollinger & Clark, 1983; Hurias et el. 2000; Langton, Piquero & Hollinger, 2006), suggesting the same types of individuals who commit crime in the street may also commit crime in the workplace. Murphy (1993) also supported this notion in his book, *Honesty in the Workplace*, stating, "it is well known that individuals involved in employee theft are also often involved in a number of other deviant behaviors," emphasizing that low-end jobs not only attract crime-prone individuals but the nature of these jobs provide little disincentive for theft (Murphy, 1993, p. 33-34). These findings not only suggest there may be overlap between street and workplace crime, but also that the extent of this overlap may depend on job quality, as proposed in Hypotheses 7a and 7b

(high(er) job quality will be positively associated with committing no crime; low(er) job quality will be positively associated with committing both street and workplace crime).

There have also been unexpected empirical results within the traditional employment-crime literature which seem to provide evidence of crime displacement into the workplace or crime continuing to occur in both contexts. For instance, Horney et al. (1995) found an unexpected positive relationship between monthly employment and property crime which they suggested may be due to an increase in opportunities for theft, forgery, or fraud available at work. When Uggen (1999) did not find an association between higher pay and economic crime he theorized this finding may be associated with increases in economic offenses among workers in higher paying sales and management positions. As a robustness check, Apel and Horney (2017) attempted to test for the possibility that employment provided opportunities for crime within the workplace through isolating "theft" as an outcome (which included among many other things stealing from a till or cash register). Though they observed a similar pattern of results when compared to their other criminal outcomes, Apel and Horney (2017) nonetheless acknowledged their measure was not ideal because it was not restricted to theft in the workplace and therefore the possibility that employment provided opportunities for criminal offending could not be ruled out.

In sum, the criminological literature is limited in that only a single study has evaluated street and workplace crime within the same framework. This is problematic because there is evidence to suggest that employment may not be monolithically protective against crime, as some studies have found that employment may actually displace crime or encourage offending in both contexts. Moreover, the likelihood of these potential outcomes may be influenced by the

quality of the job itself – a theoretical articulation that has not been directly examined within the current literature. The present study seeks to fill this gap in criminological inquiry.

CHAPTER 3: THE PRESENT STUDY

Sample

To evaluate the relationship between job quality and crime within a high-risk sample, the present study used data from the Pathways to Desistance study (hereafter "Pathways") (Mulvey, 2012). The Pathways baseline sample consists of 1,354 youth ages 14-19 who were adjudicated in the juvenile court systems in Maricopa County (Phoenix, Arizona) and Philadelphia County (Philadelphia, Pennsylvania) from November 2000 to April 2003. These two sites were selected because of their high rate of serious juvenile crime, diverse racial/ethnic population, sizable female offending population, and contrasting operational systems. The two sites also had experienced research collaborators to assist in data collection and sufficient political support from practitioners within the juvenile justice systems to facilitate the study. For study inclusion, all felony offenses (with the exception of less serious property crime), misdemeanor weapons offenses, and misdemeanor sexual assaults were eligible. Although, the final sample consists of primarily felony adjudications. The study limited the proportion of males with drug offenses to 15% of the sample within each site because of the high number of eligible cases which fit this description. Cases were also excluded for practical reasons, as local interviewers became overloaded. These selection criteria resulted in 2,008 juveniles who were approached to participate in the study, of which, 1,354 consented and were enrolled at baseline (a 67% response rate).

Data collection began in November 2000 and continued until March 2010. After the baseline interview (W0), 10 follow-up interviews were conducted over the span of seven years. For the first three years, follow-up interviews were conducted every six months (W1-6 months;

W2-12 months, W3-18 months, W4-24 months, W5-30 months, W6-36 months). Within the final four years of the study, follow-up interviews were conducted annually (W7-48 months; W8-60 months; W9-72 months; W10-84 months). Of the 1,364 individuals enrolled at baseline, 864 (64%) at least partially completed all ten of the follow-up interviews and 1,232 (90%) at least partially completed 7 or more of 10 follow-up interviews. Only 17 individuals (1%) did not complete any follow-up interviews and 64 individuals (5%) completed less than half of the follow-up interviews. Throughout the study, 48 individuals (4%) died and 43 (3%) chose not to continue to participate in the study.

The Pathways study did not begin collecting the necessary information on job quality and workplace crime until the 7th wave (48 months past baseline). Because these constructs are essential for the present inquiry, only data from the last four follow-up interviews (W7, W8, W9, and W10) which were given the necessary survey instrument version (Version 1.16) were included within the present study's sample.[13] By the 7th wave, all individuals who at least partially completed an interview were also at least 18 years of age— making this an adult sample. An adult sample is ideal theoretically because scholars have argued the relationship between work and crime may differ amongst juveniles (Bachman & Schulenberg, 1993; Staff &

[13] Although about half of individuals received survey version 1.16 in the 6th wave, this wave is not included within the study for several reasons. The first is theoretical, as during the 6th wave, the sample had not yet reached age 18, which would no longer make this an adult sample. The second is methodological, as only half of the respondents in the 6th wave received survey version 1.16. The use of this wave would therefore result in a highly unbalanced panel for longitudinal analysis. The third reason is also methodological, as the 6th wave had a recall period of only 6 months, whereas all other recall periods included in the study (W7-W10) were annual. This would result in a different length of recall across the waves which could bias results. Aggregating the 6th wave (6-month recall) with the 5th wave (6-month recall) to create a year recall period is not conducive because in the 5th wave had even fewer respondents who received the correct survey version (≤15%).

Uggen, 2003; Wright, Cullen, & Williams, 2002, but see also Paternoster, Bushway, Brame, & Apel, 2003).

Of the 1,354 individuals enrolled at baseline, 1,032 (76%) at least partially completed all four follow-up waves included in the present study. An additional 154 (11%) at least partially completed three of four, 59 (4%) at least partially completed two of four, and 27 (2%) at least partially completed one of four. Only 82 (6%) individuals did not participate in any of the included follow-ups. The final study sample included 1,272 individuals who completed 1 or more interview waves– which is 94% of the baseline sample. Although there is precedent for using the later waves of data among those interested in labor-related outcomes (Nguyen, ND, "Getting By"), it is also important to note ways in which the individuals who remained in the analytic sample differ from those who were apart of the sample at baseline, as this is a form of selection. More specifically, those who were not included within the analytic sample because they did not at least partially complete one of the wave 7-10 interviews (missing in the study data) may be qualitatively different from those who remained in the study. Table 1A within the Appendix provides an analysis to compare individuals included within the study sample to those who were not included using baseline characteristics. The outcome variable was a dichotomous measure where all baseline study participants (N=1,354) were coded "1" if not included within the present study (N=82), or "0" if included within the present study (N=1,272). Results are presented as odds-ratios (OR). Findings suggest that those who were older at baseline, male, and located at the Philadelphia study site were less likely to be included within the sample. Results do not indicate that race/ethnicity, year of baseline interview, nor baseline criminal behavior (measured as age at first prior arrest, age at first self-reported offense, and self-reported

frequency of offending at baseline) influenced selection into the present study sample. In additional analyses, study site was also interacted with year of baseline interview. Results were not significant at conventional levels ($p \leq .05$), nor did findings substantively change. The nature of missing data associated with individuals failing to participate in some waves and/or failing to fully complete the survey items necessary for analysis will be discussed in forthcoming analytic plan sections.

It is important to emphasize that the proposed Pathways sample is "high-risk" in that all individuals were adjudicated for a serious crime during adolescence. This sample is also considered to have low human capital by most standards. For instance, by completion of the study (W10), only 19 individuals (2%) had earned a college degree at the level of associates of higher. Of those 19 individuals, 10 (53%) were white, 4 (21%) were black, 3 (16%) were Hispanic, and 2 (11%) self-reported as "other" race/ethnicity. Only 61% of the sample had a high school diploma or GED.

Demographically, the sample was 21% white, 40% black, 34% Hispanic and 5% self-reported "other" for their race and ethnicity. The sample was also 86% male. Across the study, respondent's age ranged from 18–26 making this is an entirely adult sample which observed individuals for four years during their transition to adulthood. Given that the present study's sample only captured individuals aged 18–24, forthcoming results do not generalize outside of this key period in the adult life course.

Measures

All variables were measured at the 48, 60, 72, and 84-month recall (waves 7, 8, 9 and 10 respectively). Table 2 (crime outcomes and employment related variables) and Table 3

(additional covariates) provide descriptive statistics for each measure included within subsequent analyses.

Self-Reported Offending

To measure criminal behavior, self-reported offending (SRO) measures were used which the Pathways study adapted from a common self-reported delinquency scale (Huizinga, Esbensen, & Weihar, 1991). Although various scaling techniques are used within the field to measure criminal offending, the present study elected to use variety scales throughout analyses. Variety scales provide a total number of the different criminal behaviors an individual self-reported during a given recall period. This scaling method was selected because when compared to frequency or dichotomous outcome measures, variety scales were found to be high in reliability, validity, and were not biased by relatively less-serious but high-frequency crimes (Sweeten, 2012).

Street Crime is an aggregate measure which encompassed 8 property crimes (entered a building to steal; shoplifted; bought, sold, or received stolen property; used credit cards illegally; stole a car or motorcycle; carjacked someone; robbed someone with or without a weapon; and entered a car to steal), 3 violent crimes (shot at someone regardless of whether the bullet hit; beat up and seriously injured someone; and beat up someone as part of a gang),[14] and 2 drug crime items (sold marijuana and sold other illegal drugs).

[14] Two items, 1) Forced sex upon another person and 2) Killed someone, were removed from the violent crime scale which has been used in past research (e.g., Loughran et al., 2016b). These items were removed due to their rare occurrence within the sample. Within the study, there were no reports of forced sex, and only 2 individuals who killed someone within the 7th wave, 2 within the 8th, 0 within the 9th, and 2 within the 10th. Across the study waves, only five individuals ever reported killing someone.

The study also included a measure of *Workplace Crime* which is largely consistent with measures established in the work of Hollinger & Clark (1983). Workplace crime was intended to capture the extent to which an individual was involved in employee theft and was comprised of 3 self-reported items: 1) Stole money from the place where you worked, 2) Stole things from work, such as office supplies, tools, or merchandise, and 3) Reported working hours or days (so that you could get paid) that you really did not work.

A measure of the self-reported street-workplace crime overlap was also used, hereafter referred to as *Overlap*. Because workplace crime included only measures of employee property theft, only street property crime was included for comparison. This measure was coded categorically, in which individuals were coded "0" if they reported participating in no street and no workplace property crime, "1" if they reported only participating in street property crime, "2" if they reported participating in only workplace crime, and "3" if they reported involvement in both types of crime.

Employment Status

Employment status was a dichotomous indicator coded "1" if individuals reported being employed in a recall period and "0" if individuals reported they were not employed in a recall period. This measure of employment included only regular community-based work and did not include employment which was reported as "sporadic," institutionally-based, or only available to those residing within a facility.

Job Quality

Given that job quality is theoretically a multidimensional construct, the measure included a variety of objective and subjective items in its operationalization. The objective items included

income and benefits. *Income* measured the total legal income an individual reported earning over a recall period. To measure *Benefits,* individuals were asked whether any of the following benefits were available through their job: 1) Paid sick leave, 2) Paid vacation, 3) Retirement plan, 4) Health insurance, 5) Child care and 6) Help paying for transportation/mileage. A 6-item variety score weighted average was then created to capture the extent of job benefits available to a respondent. The "weighted average" as described with this measure and all measures hereafter indicates that the item was weighted by the number of weeks within a recall period that an individual worked a particular job.[15] For instance, if an individual was offered 2 job benefits at Job A which was held for 8 weeks and was offered 1 job benefit at Job B which was held for 42 weeks, his or her weighted benefits score was equal to 1.16 as follows:

$$\frac{(benefits_{JobA} \times weeks_{JobA}) + (benefits_{JobB} \times weeks_{JobB})}{weeks_{JobA} + weeks_{JobB}} = \frac{(2 \times 8) + (1 \times 42)}{(8 + 42)} = 1.16$$

To capture subjective assessments of job quality, individuals were asked "How satisfied are you with [different job characteristics]" on a scale from 1 (very satisfied) to 5 (very dissatisfied). The items included satisfaction with: 1) *Salary*, 2) *Benefits*, 3) *Supervision*, 4) *Control,* 5) *Usefulness* (job provides a sense of usefulness and accomplishment), 6) *Advancement* (job provides opportunities for promotion), 7) *Status,* 8) *Security*, 9) *Colleagues*, and 10) *Workload*. The responses were then reverse coded, such that values ranged from 0 (very dissatisfied) to 4 (very satisfied). Individuals were also asked if their job was "in a field that you want to stay in as a career." This item was coded "1" if an individual indicated that they perceived a job to be career

[15] Respondents could report up to four jobs per month within the recall period, although the maximum number of jobs reported within any recall periods was 7. The modal number of jobs for every recall period was 1.

track and "0" if they did not think their job was career track. A weighted average of each item was then created.

The aforementioned 13 quality items were then used to create a *Job Quality* scale through confirmatory factor analysis. The items loaded on a single factor (Eigenvalue=4.72) and the scale demonstrated high internal consistency among those who were working (Cronbach's α=.83).[16] Table 4 provides descriptive statistics and factor loadings for each item included within the job quality construct.[17] Job quality originally had a minimum of -3.95 and a maximum of 2.26, however a value of 3.95 was added to the scale so that it ranged from 0 to 6.21. Within this scale, higher values indicated higher job quality and zero indicated the lowest observed job quality.

[16] Given that all items loaded as a single factor, the sample size was large, and internal consistency was high, all items with factor loadings greater than .30 were retained within the scale. Though there is no agreed upon threshold for factor loadings, .30 was selected because scholars have advocated this threshold may indicate practical significance (Hair et al., 1998). In addition, scholars have suggested that a factor can be considered reliable if it has at least four loadings greater than .60, in which the job quality scale satisfied (Field, 2005). As a robustness check, the models in the forthcoming chapter were also run with a job quality scale which only included items with factor loadings greater than .60 and results were substantively unchanged.

[17] Two additional items, perceived difficulty of replacement and primary title occupation, were also considered for inclusion within the scale but were not incorporated because of low factor loadings (.16 and .21 respectively). With respect to replacement difficulty, respondents were asked, "If you wanted to leave this job, how difficult do you think it would be to find another one that was just as good?" on the scale of 0 (not difficult) to 2 (extremely difficult). A variable was then created as a weighted average for the recall period. Replacement difficulty may not have measured the quality of the job itself but rather captured characteristics of the employee. If for instance, the employee had personal characteristics which would make finding a job particularly difficult (e.g., a criminal record, little work experience). Occupational title was also considered for inclusion within the scale because it has been used as a measure of job quality in previous research (e.g., Crutchfield, 1989; Crutchfield et al., 2006; Crutchfield & Pitchford 1997). Jobs with responsibilities including: 1) Retail/cashier 2) Counter help, fast food, restaurant worker, 3) Babysitting, childcare, caretaker (adult or child), camp counselor, 4) Manual labor and 5) Other were considered low quality occupations. Jobs with responsibilities including: 1) Skilled labor, 2) Office work, clerical, telemarketing, 3) Managerial, administration, and 4) and Technical, Professional were considered high quality occupations. Job were coded as "1" if an individual held any job title classified as high quality during the recall period and "0" if he or she did not. Having an occupational title classified as "quality" was not a strong measure of job quality and thus it was not included in the scale. This is consistent with the present study's theoretical arguments as well as prior literature which suggests that occupational classifications are weak measures of job quality (Jencks et al., 1998).

To measure *High Job Quality* the job quality scale was dichotomized at the 70th percentile, where values greater than or equal to the 70th percentile (value of 4.39) were considered high quality and job quality values less than the 70th percentile were considered low quality. It is acknowledged that the selection of this threshold was highly discretionary. Therefore, robustness checks were carried out at alternative thresholds (50th, 60th, 80th, and 90th percentiles) to consider whether findings were sensitive to threshold selection. Results of this sensitivity check test suggest substantive conclusions were not influenced by the choice of threshold (results are available within Appendix Table 2A).

The number of *Hours* an individual worked, operationalized as a weighted-average of a respondent's reported hours worked per week over the recall period, was also considered for inclusion within the job quality scale. Though, it was not incorporated because it had a low factor loading (.20). Coding the variable as "1" full time (≥30 hours or ≥40 hours) and "0" less than full time" was also considered and similarly produced low factor loadings. This suggests the number of hours an individual worked was not an indicator of job quality. However, this measure was included within analysis as a single item control because it had the potential to confound the relationship between job quality and crime, particularly if working more hours influenced job quality (e.g., increased the likelihood of receiving job benefits) and also reduced the likelihood of crime through incapacitation. For inclusion in analysis, the weighted average hours measure was coded into 7 ordinal categories such that 0=[1, 10]; 1=(10, 20]; 2=(20, 30]; 3=(30, 40]; 4=(40, 50]; 5=(50, 60]; 6=(>60).

Additional Rational Choice Measures

Additional measures were also included to more fully specify the decision to offend as conceptualized in RCT. For instance, the *Certainty* of formal sanctioning was measured through asking individuals on a scale from 0 (no chance) to 10 (absolutely certain), how likely it is that they would be caught and arrested for committing each of the following seven crimes: 1) Fighting, 2) Robbery with a gun, 3) Stabbing someone, 4) Breaking into a store or home, 5) Stealing clothes from a store, 6) Vandalism, and 7) Auto theft. A 7-item mean was taken for each individual for each wave. The measure was found to have high internal consistency (Cronbach's $\alpha = 0.89$ at baseline).

The *Social Cost* of crime was measured through asking each individual if the police were to catch him or her breaking the law, how likely it would be that he or she would suffer social costs (Williams & Hawkins, 1986; Nagin, 1998). Six social costs were included: 1) Losing respect from close friends, 2) Losing respect from family members, 3) Losing respect from neighbors or other adults, 4) Losing respect from a girlfriend or boyfriend, 5) Being suspended from school, and 6) Finding it harder to get a job. Responses ranged from 1 (very unlikely) to 5 (very likely). This measure can be thought of as a broad perceptual measure of general social or extra-legal costs one would suffer if caught committing a crime (Cronbach's $\alpha = 0.76$ at baseline).

In addition to costs of crime, the proposed rational choice model also incorporates three perceived benefits of criminal behavior. *Social Rewards* were measured through asking individuals on a scale from 1 (strongly disagree) to 4 (strongly agree), how much they agreed or disagreed with statements regarding how others would react to three different crimes: 1) Stealing (e.g., "If I take things, other people my age will respect me more"), 2) Fighting (e.g., "If I beat

someone up, other people my age will respect me more"), and 3) Robbery (e.g., "If I rob someone, people my age will be afraid to mess with me"). The mean across all items was used for each individual within a recall period, and the item was found to have strong internal consistency (Cronbach's α= .82 at baseline). The *Personal Rewards* of crime was measured through asking respondents on a scale from 0 (no fun or kick at all) to 10 (a great deal of fun or kick), how much "thrill" or "rush" is it to commit the following 7 crimes: 1) Fighting, 2) Robbery, 3) Stabbing someone, 4) Breaking into a store or home, 5) Stealing clothes from a store, 6) Vandalizing, and 6) Stealing an automotive. If a respondent had self-reported to never have done any of those things, they were asked to give a rating based on how much thrill or rush they thought it would be to commit those behaviors. A summated average of all seven items was taken and the scale was found to have high internal consistency (Cronbach's α=.88 at baseline). To provide a measure of expected financial returns to crime, past *Illegal Earnings* were also included, measured as the total illegal earnings an individual self-reported within the previous recall period.[18]

Control Variables

In addition, the study included a variety of demographic and control variables. Time-constant variables included an individual's *race/ethnicity* coded as white, black, Hispanic, or other, an individual's sex (coded 1=*male*, 0=female), and the *study site* location (coded 1=Philadelphia, 0=Phoenix). Time-varying demographic (*Age*) and control variables were also included which were likely to co-vary with the employment variables of interest as well as

[18] Illegal earnings were top coded at $150,000/year. The variable's original maximum was $650,000/year. The top coding affected 26 observations.

criminal activity. For instance, perceived *Opportunities for Work* were also included, where individuals were asked to consider 5 items: 1) Employers around here often hire young people from this neighborhood, 2) If a young person from this neighborhood wants to find a job, he or she can, 3) If a young person from this neighborhood wants to find a job that pays well, he or she can, 4) If a young person from this neighborhood wants to find a job that teaches valuable skills, he or she can, and 5) Young people from this neighborhood who work can get promoted on the job if they want to. The mean of the 5 items was then taken to develop a summated average scale. The items were found to have high internal consistency (Cronbach's α= 0.76 at baseline).

To empirically consider an individual's financial need, two measures were used. First, an individual's level of *Financial Responsibility* was created through an additive 5-item scale in which an individual was asked to state whether they had 0 (no), 1 (partial), or 2 (full) financial responsibility for the following five items: 1) Paying bills such as electricity or phone, 2) Paying rent, 3) Helping support family members financially, 4) Supporting themselves financially, and 5) Buying groceries. Each item within the scale was found to have adequate to high internal reliability (Cronbach's α=.63 at W7, α =.80 at W8, W9, and W10). The second measure of financial need was *Other Income* which indicated how many of the following additional sources of income a respondent had: 1) Welfare, 2) Social security, 3) Money from parents or other relatives, 5) Survivor benefits, and 6) Money from a current or former spouse/partner.

Additional life circumstances of the respondent were also considered. For instance, a dichotomous measure of whether an individual was in a *Romantic Relationship* (1=Yes, 0=No) and a measure of the *Relationship Quality* which was created from a 7-item summated average measure adapted for the Pathways study which includes items such as "In general, how happy

are you with your relationship?" (Pierce, 1994; Piercce, Sarason, Sarason, Solky-Butzel, & Nagle, 1997). While the measure was originally continuous ranging from 1–5 (higher values suggest a higher quality relationship), 1 was subtracted from the measure and the variable was recoded into 5 categories such that 0=[0,.5), 1=[.5,1.5), 2=[1.5, 2.5), 3=[2.5, 3.5) and 4=[3.5, 4]. Measures capturing whether the respondent or respondent's partner was *Expecting a Child* (1=Yes, 0=No) within the recall period, and a cumulative total of the *Number of Children* the respondent has in each recall period were also included. An individual's level of *Education* (0=No high school or GED; 1= high school or GED; 2=College associates degree or higher), and a dummy variable indicating whether the individual was in *School* during the recall period were also included. In addition, a measure of *Routine Activities* thought to be conductive to crime was included (Osgood et al., 1996; see also Apel & Horney et al., 2017). The measure was comprised of four items: 1) Getting together with friends informally, 2) Riding around in a car for fun, 3) Going to Parties, and 4) Spending evenings out for fun and recreation. The items were measured on a scale from 1 (never) to 5 (almost every day), where higher values indicate more activities theoretically believed to be conductive to crime. A summated average scale was then created and items were found to have adequate internal consistency at baseline (Cronbach's α =0.62)

In addition, given Nagin and Paternoster's (1991) state dependence argument and Sampson and Laub's (1993, 1997) life course notion of cumulative disadvantage which propose that prior offending may have causal implications for future offending, perceptions of respondents' *Criminal Record* influencing employment decisions, their number of *Prior Arrests* (a summated total of an individual's number of priors from baseline until the start of the recall period) and the amount of time incarcerated within a given recall period (*Exposure)* are included

to control for past criminal experience influencing employment outcomes as well as criminal behavior. A measure of *Supervision* indicating whether an individual was on probation or parole (1=Yes, 0=No), was also included.

CHAPTER 4: RESEARCH QUESTION 1 ANALYTIC PLAN AND RESULTS

Analytic Plan

The study's first research question evaluated the extent of between and within individual variability in job quality within this high-risk sample. Substantively, between-individual variation considers how different an individual's job is from others within the sample, whereas within-individual variation describes job quality differences within the same individual over time.[19]

Tables 2 and 4 first describe variability in job quality through descriptive statistics. The descriptive statistics provide the mean, standard deviation, median, minimum and maximum values for each individual job quality item (Table 4) as well as the aggregate scale (Table 2). In addition, Figures 1-6 depict the mean level of each job quality item and the aggregate scale across waves. The study implemented one-way repeated measure analysis of variances (ANOVA) to test whether mean differences of each item were equivalent across waves. Substantively, this examined whether there was sufficient evidence to reject the null that the means at each wave were equivalent to one another for a given variable. Because ANOVA relies on a strong normality assumption, the study also implemented non-parametric analogous

[19] To provide a brief example of between-individual variation, assume that in the 7th wave there were only two respondents: Respondent A and Respondent B. Respondent A had a job quality score of 2 and Respondent B had a score of 5. The difference in job quality between Respondent A and B provide evidence of between individual variability in job quality because Respondent A had *lower* quality job than Respondent B. If, however, Respondent A and B would have had the same job quality score, there would be no between individual variability for that wave because the two respondents' quality scores were equivalent. With respect to within-individual variability, if Respondent A had a job quality score of 2 in the 7th wave and a score of 3 in the 8th, there would be within-individual variability in job quality for Respondent A given that her job quality *increased* from wave 7 to 8. Relatedly, if Respondent B had a job quality score of 5 in the 7th period and 5 in the 8th period, there would be no within-individual variability in job quality for Respondent B because she had no change in job quality across waves.

Skillings-Mack (SM) tests to examine whether there were systematic differences in distributions across waves (Chatfield & Mander, 2009; Skillings & Mack, 1981).[20] The bottoms of Figures 1-6 display the ANOVA and SM test results. Additionally, the study will provide conditional probabilities across wave 7 and 8 as an additional method of describing within-individual variation for each of these measures.

The last column of Table 2 also provides the within-individual proportion of variation to describe the extent of within and between individual variation in each job quality item and the aggregate scale. These values were calculated using a one-way ANOVA with each quality measure as the dependent variable and the individual respondent ID variable as the categorical predictor, following the methodology outlined by Allison (2009). The within-variation values can be interpreted as the proportion of variation that was within-individual for a given job quality item or the aggregate scale.

Within the study, about half of the respondents were employed within any given wave. Across waves, 25% of respondents were always employed, 25% were never employed, and 50% showed variability in their state of employment– working in some waves and not others. Unless otherwise stated, descriptive statistics for work-related variables are hereafter discussed for only working recall periods.

[20] The SM test's null hypothesis is that the distribution of the ranks for a variable at each wave are equal. A significant SM test statistic indicates that the distributions of a given variable across waves are different. The SM test is equivalent to the Friedman test when there are no missing data in a balanced design. The SM test was selected over the Friedman test given the nature of the unobservable data when an individual was not employed (Chatfield & Mander, 2009).

Results

Job Quality: By Item

To provide some context, the 2005–2010 continental U.S. poverty threshold ranged from $9,500 to $22,000 per year, conditional on year and household size (U.S. Department of Health & Human Services, 2005-2010).[21] The study respondents' reported income ranged from $40–$150,000, demonstrating non-trivial between-individual variability in earnings.[22] The sample's mean income was just over $16,000 per year, though the median annual income was only $12,000. While the median income was above the poverty line for a single individual, earnings were not much above this threshold. As one might have expected, the study's income distribution was heavily right skewed, with the 90th percentile reaching less than $34,000 per year. The study's average income range was $140,000, demonstrating a fairly high level of between-individual variability in earnings among study participants.

Figure 1 displays the mean income across waves to begin to illustrate within-individual variability. The average income increased across waves from $10,896 to $20,517 ($p \leq .001$); though 37–58% of jobs in any given wave had an average annual income below $10,000. The increase in income over time was likely associated with the individuals' increasing age and job experience throughout the course of the study. The distribution of reported earnings also differed across waves ($p \leq .001$). Descriptively, the income distribution became less right-skewed across waves as individuals' earnings increased on average, though the distribution always remained right-skewed. Table 4 highlights that 34% of income variation was within individuals and

[21] Estimate provided for one to four-person households.

[22] Income was top-coded at $150,000. This affected 10 observations. The original reported income maximum ranged as high as $872,000.

correspondingly 66% of the variation was between individuals. This substantively indicates that within this sample, there was relatively little change in an individual's income over time, as there was more variation between individuals. This is indicative of low earning mobility and is consistent with what is expected of a high-risk sample.

To further describe within-individual variation, Table 5 presents wave 8 income (coded into 9 ordinal categories for descriptive purposes: ≤$5000, $5000-$9,999; $10,000-$14,999; $15,000-$24,999; $25,000-$34,999; $35,000-$49,999; $50,000-$74,999; $75,000-$99,999; $100,000) conditional on wave 7 income. This suggests that 43% of those who were in the lowest income category in the first wave (≤$5,000) remained in this category in the following wave. A greater percentage however (57%), moved into a higher income category. These descriptive statistics indicate low income mobility between waves, given the relatively high conditional probabilities within the shaded on-diagonal cells of the table – suggesting no change in income category. There was however, also variation in within-individual income across waves, as indicated by the off-diagonal unshaded conditional probabilities.

With respect to benefits, the sample's number of reported job benefits ranged from 0 to 6, demonstrating between-individual variability. Notably, the average number of benefits was only 1.28 and the median was close to zero (0.05), suggesting the distribution was right-skewed with many individuals reporting no benefits at all. Specifically, 45–56% of the sample reported zero benefits within each wave. Table 6 provides the percentage of those working with each type of benefit by wave. Individuals most frequently reported health insurance, followed by paid vacation, sick leave, and a retirement plan, respectively. Employers rarely offered child care and transportation cost coverage, as only 4–9% of individuals reported these benefits within any

wave. Importantly, employers never offered any benefit type to a majority of the sample within a wave, which is consistent with the high-risk nature of the sample.

Figure 2 illustrates the mean number of benefits by wave to begin to describe within-individual variation. The average number of benefits ranged from 0.98 to 1.54. Like income, the average number of benefits also increased overtime ($p \leq .001$). The distribution also showed significant changes across waves ($p \leq .001$). Descriptively, this appeared to be associated with the increase in benefits as the distribution became less right skewed. Table 7 further describes within-individual variation through providing the likelihood of an individual reporting each number of benefits in wave 8 conditional on their number of benefits in wave 7. To aid in descriptive discussion, the weighted average number of benefits was recoded categorically within this table such that 0=[0, 1), 1=[1, 2), 2=[2, 3), 3=[3, 4), 4=[4, 5), 5=[5, 6) and 6=[6]. The first important finding is that those who had no benefits in wave 7 had a 68% likelihood of also having no benefits in the next wave. Interestingly, those with benefits in the 7th wave had a high likelihood of losing their benefits in the next wave, speaking to the capricious employment circumstances within this high-risk group. In sum, benefits showed fairly high levels of between individual variability, as well as within-individual change, given that 40% of the variability was within individuals (reported in Table 4).

Table 4 also presents descriptive statistics for each job quality item which was measured with a satisfaction scale. On average, individuals were not dissatisfied with any element of their job. Though they were the least satisfied with traditional economic indicators of quality (e.g., salary and benefits) and relatively more satisfied with intangible dimensions of the job such as their perception of control, usefulness, and collegiality. Table 4, further describes the distribution

of each of these items in the last five columns. To aid in descriptive discussion, the variable was recoded categorically (0 “very dissatisfied” [0, 1), 1 “dissatisfied” [1, 2), 2 “neither” [2, 3), 3 “satisfied” [3, 4), 4 “very satisfied” [4]). A reader can interpret these results such that, 6% of workers were very dissatisfied with their salary, 16% were dissatisfied, 26% were neither, 43% were satisfied and 9% were very satisfied. The first notable finding is that “satisfied” was the modal category for each of these items with the exception of satisfaction with job benefits. Additionally, the distribution of each item (again, with the exception of benefits) was slightly left skewed. It was relatively rare for individuals to be very dissatisfied with a dimension of their job. Though notably, for every dimension responses ranged from very dissatisfied to very dissatisfied, showing between individual variability.

Table 4 also suggests that a fairly high proportion of variation within each of these items was within individuals (43–50%). Figures 3 and 4 further illustrate the mean of each of these items across the waves to continue to investigate within-individual variation. Findings provide significant evidence that on average, satisfaction with supervision ($p \leq .001$) and benefits ($p \leq .001$) improved over time. The distributions of these items also changed over time ($p \leq .01$) Additionally, there was marginal evidence that the mean satisfaction with control and usefulness improved over time ($p \leq .10$). However, there was no evidence that satisfaction with salary, colleagues, workload, status or advancement changed on average or in distribution overtime. Table 8 further describes within-individual variation, through providing the conditional likelihood of the summated job satisfaction score (an average of all items measured using a satisfaction scale) in the 8th wave conditional on 7th. Within this table, the summated average was recoded categorically to aid in discussion (0=[0, 1), 1=[1, 2), 2=[2, 3), 3=[3, 4), 4=[4]). The first

notable element of this table is that there were very few individuals within the very dissatisfied row (N=3). This is due to the fact that only 5 individuals were very dissatisfied with their job in the 7th wave, 2 of which did not work in the 8th wave. Substantively, these conditional probabilities demonstrate that across waves, individuals who were dissatisfied tended to increase their overall satisfaction, while those who were neither dissatisfied nor satisfied were likely to remain there. Those who were satisfied were almost equally as likely to remain satisfied as they were to reduce their satisfaction to neither. Also, those who were very satisfied in the 7th wave were equally likely to reduce their total job satisfaction in the 8th wave to merely satisfied or neither.

Table 4 also provides descriptive statistics for the career track employment item. On average, individuals did not perceive their job to be career track (mean=.28). In fact, across the study, only about a third of individuals ever reported that their job was in a field they wanted to stay in as a career. Figure 5 displays the mean of career track employment across waves to illustrate within-individual variability for this item. Perceptions that a job was career track increased, on average, across waves from 0.26 to 0.29 ($p \leq .001$). The distribution of reported earnings also differed across waves ($p \leq .001$). As Table 4 notes, 45% of career track variation was within individuals. This substantively indicates that within this sample, there was nearly as much change within-individuals as there was between-individuals.

To further describe within-individual variation, Table 9 presents wave 8 career track employment (coded dichotomously for descriptive purposes where 0=[0,.5) and 1=[.5, 1]) conditional on wave 7 career track employment. This suggests that 77% of those who did not perceive their job to be career track in the 7th wave also did not perceive their job to be career

track in the following wave. Among those who felt their job was career track in the 7th wave, there was an equal likelihood of having career track employment or not in the following wave. These descriptive statistics indicate that while there was a high likelihood of within-individual stability between waves (.50 to .77) there was also evidence of within-individual change, particularly among those who felt their job was career track in the 7th wave. This also substantively suggests that having a career track job was fairly precarious, as only about half of those who had this type of job consistently maintained it.

Job Quality: Aggregate Construct

Though it was important descriptively to explore each individual item, the aggregate measure of job quality was the most essential to evaluate for evidence of between and within-individual variation as this measure will primarily be used in forthcoming analysis and is the most theoretically justified conceptualization of total job quality. Findings suggest that the mean job quality score was 3.95 with a median of 4.06, suggesting that although job quality was fairly normally distributed, it had some left-skew. As noted previously, job quality ranged from 0 to 6.21 across the study. The average range across waves was 6.11, providing evidence of between-individual variability.

To begin to look at within-individual variability in total job quality, Figure 6 illustrates the mean job quality across waves. Findings suggest that job quality increased, on average, across waves ($p \leq .01$) from a score of 3.82 in the 7th wave to 4.02 in the 10th. There was also evidence that the distribution of job quality systematically increased over time ($p \leq .01$). Table 4 notes that 38% of variation in job quality was within individuals. This substantively indicates that within this sample, there was relatively little change in an individual's job quality over time,

as a greater proportion of the variation was between individuals. This is consistent with what is expected of a high-risk sample.

To further describe within-individual variation, Table 10 presents wave 8 job quality (coded into 6 ordinal categories for descriptive purposes: (0=[0, 1), 1=[1, 2), 2=[2, 3), 3=[3, 4), 4=[4,5), 5=[5,6.21]) conditional on wave 7 job quality. The first notable finding was that very few individuals were in the lowest job quality category. These descriptive statistics provide evidence of both continuity and change in job quality over time. More specifically, there was evidence that those in lower quality categories (1 and 2) in the 7th wave were more likely to improve their job quality in the 8th wave relative to remaining within the same category. Those who had relatively higher job quality in the 7th wave (scores of 3, 4), however, were more likely to remain in this category in the next wave as indicated by the high conditional probabilities in the shaded-regions. Those who indicated the highest job quality in the 7th wave were notably more likely to reduce their job quality in the following wave, as indicative of the likelihood of the off-diagonal unshaded boxes. This suggests that even if high job quality was obtained, it was not necessarily maintained throughout waves – providing evidence of within-individual change.

CHAPTER 5: RESEARCH QUESTION 2 ANALYTIC PLAN AND RESULTS

Preliminary Analyses: Bivariate Relationships

To begin to evaluate the relationship between job quality and street crime, the present study first explored the bivariate associations between each item within the job quality operationalization and street crime. Figures 7–12 display the mean number of self-reported offenses (SROs) by individual job quality items as well as the aggregate scale within the pooled sample. The study conducted tests for mean differences in offending across categories along with the analogous non-parametric tests.[23] In addition, the study implemented bivariate pooled negative binomial regressions with SRO as the dependent variable and each job quality item as the independent variable. The negative binomial modeling strategy was appropriate because the dependent variable, SRO, was a variety count (Long, 1997). In addition, descriptive statistics within Table 2 indicate that the variance was greater than the mean within each of the crime variables– providing preliminary evidence of overdispersion. Formal tests for overdispersion also provided support for selection of a negative binomial model. All bivariate pooled models conditioned on working waves and clustered on the individual to account for dependence among observations across waves. Forthcoming results present the negative binomial model coefficients within these models, and all models hereafter, as incidence rate ratios (IRR). A reader can interpret these coefficients such that a one unit increase in the independent variable is associated with an IRR factor change in SRO. Each respective figure presents the results of these tests.

[23] Parametric ANOVA (F) and T-tests (T) were used. The respective non-parametric analogs, the Kruskal Wallis (KW) test for three category independent variables and the Wilcoxon-Mann-Whitney (MW) test for two category independent variables, were also conducted. These non-parametric tests do not assume the dependent variable is normally distributed. A significant test statistic indicates that the distributions of offending scores across categories are statistically different.

Figure 7 presents the preliminary bivariate relationship between income (coded categorically consistent with the previous chapter) and crime. Findings suggest that there were significant differences in mean levels of offending across categories ($p \leq .05$) as well as differences in the distributions ($p \leq .05$). The negative binomial model also provided significant evidence of a negative relationship between income and crime ($p \leq .05$). Within Figure 7 however, it was also notable that there did not appear to be monotonically negative relationship. Specifically, although the relationship between income and crime appeared to be negative across categories of income less than $49,999, the relationship between crime and income looked to become positive across higher income categories, suggesting perhaps that offending increased across high-levels of income.

Figure 8 depicts the bivariate relationship between job benefits and SRO. Findings did not indicate that there were significant differences in the mean number of SROs nor distributions of offending across the number of benefits. However, the bivariate negative binomial results did find marginally significant evidence of a negative relationship with street crime ($p \leq .10$). While there was some evidence of a relationship, the preliminary bivariate results did not suggest this relationship was strong.

The study also evaluated the bivariate relationships between each item which was measured on a satisfaction scale and crime. Figure 9 provides an illustration of the association between salary satisfaction and SRO. From this figure, it is notable that the mean number of SROs decreased as salary satisfaction increased. Findings suggest that the mean differences in SRO were different across salary satisfaction categories ($p \leq .10$) and that the distributions differed across categories ($p \leq .01$). In addition, the negative binomial regression results suggested

that salary satisfaction had a negative relationship with SRO, such that those with higher levels of salary satisfaction had a 14% decreased rate of offending ($p \leq .01$). Satisfaction with supervision, usefulness, status, security and workload were similarly found to have a negative bivariate association with offending (Appendix Figures 1A–5A). However, satisfaction with a job's benefits, control, chances for advancement, and colleagues, were not found have a bivariate association with offending (Figures 6A–9A within the Appendix).

Figure 10 illustrates the bivariate association between career track employment and SRO. These results visually demonstrate that the mean number of SROs was not found to be associated with career track employment. More specifically, the results of suggest that there was no significant evidence that mean differences in SRO (p=.43) nor differences in the distributions (p=.79) were associated with career track employment. Results of the bivariate negative binomial model also provided no evidence of a negative association between career track employment and SRO (p=.98).

Together, the item-specific results provided evidence that some dimensions of job quality were more strongly associated with street crime than others. However, from a rational choice perspective, one's overall job quality was the most important to consider as it was hypothesized that this total net value should be associated with crime. Figure 11 provides the bivariate association between the aggregate job quality construct and SRO, illustrating that the mean number of SROs decreased as job quality increased. Findings provided evidence of mean differences in SRO across job quality categories ($p \leq .05$) as well as systematic differences in the distributions ($p \leq .01$). Results of the bivariate negative binomial model also provided preliminary

evidence of a negative association between job quality and SRO ($p \leq .001$), in preliminary support of Hypothesis 1.

The bivariate association between high-quality employment (job quality dichotomized at the 70th percentile) and SRO was also assessed. Figure 12 demonstrates that a significant bivariate negative association between high job quality and crime was observed. Specifically, high-quality employment had significantly fewer mean SROS ($p \leq .001$) relative to low-quality employment. The distributions differed across the variable as well, suggesting that high-quality employment was associated with fewer offenses ($p \leq .001$). Additionally, the negative binomial bivariate regression provided evidence that relative to low-quality employment, primary sector high-quality employment was associate with a 37% reduction in the rate of offending ($p \leq .001$), providing some initial support for Hypothesis 2.

Together, these results provided preliminary evidence of a negative relationship between both higher and high job quality and SRO which warranted further scrutiny. Specifically, the forthcoming analysis focused on determining if results were consistent after theoretically relevant observable controls and unobservable time-stable heterogeneity was considered.

Analytic Plan

Pooled Multivariate Regression

The next stage of analyses implemented pooled multivariate negative binomial models clustered by the individual. Drawing upon RCT, the fully specified model estimated the following equation:

$$Y_{it} = \beta_0 + \beta_1 W_{it} + \beta_2 Q_{it} + \beta_3 X'_{it} + \varepsilon_{it} \quad (1)$$

Within this equation, Y_{it} represents a variety score of street crime committed by an individual (*i*) in wave (*t*). W_{it} indicates whether an individual was employed within the wave and Q_{it} represents job quality which was measured continuously in analysis examining higher quality, or as a dichotomy in models testing high job quality. X'_{it} represents a vector of both time-varying and time-constant controls, β_0 represents the constant, and ε_{it} represents the error term. Conditional expectations are used to demonstrate how to interpret the employment related coefficients within this equation and forthcoming equations as follows:

$$E(Y_{it} \mid W_{it} = 0, X'_{it}) = \beta_0 \quad (2)$$

$$E(Y_{it} \mid W_{it} = 1, Q_{it} = 0, X'_{it}) = \beta_0 + \beta_1 \quad (3)$$

$$E(Y_{it} \mid W_{it} = 1, Q_{it} \geq 0, X'_{it}) = \beta_0 + \beta_1 + \beta_2 \quad (4)$$

Following Apel and Horney's (2017) method of model specification, when an individual was not employed, W_{it} and Q_{it} were given values of zero (Equation 2). When an individual was working, W_{it} was given a value of 1, and Q_{it} took on a value which indicated the quality of the job. Therefore, Equation 3 demonstrates that the employment variable (W_{it}) captured the transition from not working to working at the lowest quality job (quality values coded "0"). As Equation 4 then indicates, the Q_{it} coefficient captured variation in job quality among those who were working. Importantly, while work (β_1) and all control variable estimates (β_3) generalize to the high-risk individuals included within the model's analytic sample, job quality estimates (β_2) generalize to only those who were working.[24]

Fixed Effects Regression

[24] Hours is a control variable exception, because like job quality, this variable's estimates generalize only to those who were working. This exception in generalizability is consistent in the forthcoming fixed effects discussion.

However, when researchers empirically assess the relationship between employment and crime, it is important to address the threat of selection bias as a key methodological hurdle. While the pooled regression model is able to control for many *observable* characteristics associated with selection, challenges to causal interference are created by *unobservable* individual differences which may influence the likelihood of employment, the quality of the job, and criminal behavior. This unobserved heterogeneity may include factors such as an individual's intrinsic motivation, ability, or even criminal propensity.

Gottfredson and Hirschi (1990) proposed the most well-known criminological argument suggesting a spurious work-crime relationship. They argued that any statistical association between employment and crime is the result of an individual's level of self-control. Within their theory, low self-control is a relatively time stable trait which causes individuals to think in the "here and now" and focus on immediate benefits of crime rather than long-term costs. Because individuals with low self-control focus on immediate benefits, they are more likely to commit crime which provides immediate gratification. Correspondingly, they are also less likely to focus on the long-term benefits associated with legal employment, making it unlikely that they will invest their time and effort into establishing quality employment. Others have proposed similar theoretical articulations suggesting offending is due to underlying differences in individuals' impulsivity and/or discount rate (Wilson & Herrnstein, 1985) or biological characteristics (Moffit, Lynam, & Silvia, 1994; Raine, 2013).

Ideally, a researcher could establish the treatment effect of employment through comparing the effect of employment on crime for an individual with the crime outcome for that same individual had he or she not been employed. Unfortunately, for a single individual, only

one of these counterfactual outcomes can be observed and researchers must instead compare an individual who is employed with specific job characteristics to different individuals who may not be employed or who are employed but may have an entirely different job. This is a methodological problem as those who are employed or work in higher/high quality jobs are likely very different from those who are not employed or have low quality positions. While one solution is to control for observable individual differences thought to influence selection, as within the pooled regression model, this becomes difficult when any of the traits thought to influence selection are difficult to measure empirically or are unobserved within the data– a form of omitted variable bias. As it is likely these factors are correlated with employment, job quality, and offending, there will be bias within the model if left unaccounted for.

The availability of panel data within the study provides a methodological opportunity to reduce selection bias. Given that a great deal of the selection bias is thought to be due to time stable (or fixed) unobserved traits, this analysis will employ a modeling approach which eliminates fixed unobservable heterogeneity through using only within-individual variation (Allison, 2009). This approach models unobserved heterogeneity by allowing each individual to be used as their own control within the model, capturing all individual effects that are constant over time. Those interested in examining the relationship between employment and crime (e.g., Paternoster et al., 2003), illegal earnings (Nguyen et al., 2015), and testing the generalizability of the rational choice model (Loughran et al., 2016b) have implemented similar modeling strategies to control for unobserved time-stable heterogeneity within the criminological literature.

The study estimated the following negative binomial model:

$$Y_{it} = \beta_0 + \beta_1 W_{it} + \beta_2 Q'_{it} + \beta_3 X'_{it} + \gamma_1 \overline{W}_i + \gamma_2 \overline{Q'}_i + \gamma_3 \bar{X}'_i + \rho_t + \varepsilon_{it} \qquad (5)$$

Where once again, Y_{it} represents a variety score of street crime committed by an individual (i) within wave (t), W_{it} indicates whether an individual was employed, Q_{it} represents job quality, and X'_{it} represents a vector of time-varying controls. Equation 5 differs from Equation 1 in that it controls for unobservable time-stable factors by including the means of each time-varying predictor for each individual ($\overline{W}_i,\ \bar{Q}_i,\ \bar{X}'_i$) as well as the time-varying deviations from those means (W_{it}, Q_{it}, X'_{it}).[25] Therefore, the β coefficients are analogous to fixed effect estimates relying only on within-individual variation. While work (β_1) and control variable estimates (β_3) generalize to all of the high-risk individuals included within the model's analytic sample, job quality estimates (β_2) continue to generalize to only those who were working. In addition, because fixed effects estimates rely solely on within-individual variation, results do not generalize to those who had no variation in the dependent variable across observed waves. While the γ coefficients are not entirely informative themselves (and thus are omitted from forthcoming output), they are included within the model for two reasons. First, without these means the model would not fully control for these factors. Second, these means allow for a Wald test examining the trade-off between the less-biased fixed effects estimator and the more-efficient random effects estimator. Within all forthcoming models in this chapter, a Wald test was performed with significant evidence in favor of estimates relying on only within-individual variation ($p \leq .001$). In addition, given that the data were collected across six years in two unique cities, the study site was interacted with year to control for time-variant trends within each location. To test the relationship between job quality and crime within the low-quality market sector (Hypothesis 3),

[25] Allison (2009) refers to this estimation method as the "hybrid approach" because it uses a random-effects estimator but produces β coefficients which can be interpreted as fixed effects estimates as they rely on only within-individual variation and hence, control for all time-stable unobserved heterogeneity.

the above equation was conditioned by removing all high-quality jobs (above the 70th percentile of job quality) from analyses.

Although this modeling approach takes steps to account for one threat to causal inference (omitted variable bias resulting from time-stable unobserved heterogeneity) it does not account for the possibility of a second threat– temporal ordering. Given that work and job quality were measured contemporaneously it is possible that crime influenced employment and job quality (Thornberry & Christenson, 1984). For this reason, it is essential to note that forthcoming estimates should not be interpreted as causal relationships, rather significant effects should be viewed only as statistical associations.

A Note on Missing Data

Of the 5,060 pooled observations (1,272 individuals) in the potential analytic sample described in Chapter 3, a total of 333 pooled observations (7% of potential study analytic sample) were omitted from analysis because they were missing data indicating whether or not an individual worked in a given wave, resulting in 4,727 pooled observations (1,265 individuals) with the potential to be included in analysis.

Of the 4,727 pooled observations with work-status indicated, 166 (4% of those with a work indicator) were missing information required to measure job quality (missing data on at least one of the necessary items required to compute the scale), resulting in 4,561 pooled observations (1,265 individuals) which could be included in analysis. The percentage of job quality missing data across waves was 10% in the 7th, 7% in the 8th, 5% in the 5th, and 6% in the 10th.

Of the remaining 4,561 observations, 160 were missing information on the dependent variable, limiting the potential sample to 4,401 observations (1,198 individuals) available for analysis. An additional 1,104 pooled observations were not included because information was missing on one or more of the control variables. This resulted in a final analytic sample of 3,297 observations (1,073 individuals) which could be used in this analysis. Across the panels, 714 observations were included from the 7th wave, 872 were from the 8th wave, 880 were from the 9th wave, and 831 were from the 10th wave.

Given that it is unlikely that data was missing at random, it is possible that the nature of the missing data within this study resulted in bias to the estimates (Allison, 2002). This is particularly problematic if there was unobservable time-varying heterogeneity (which was not accounted for by the fixed effects analytic strategy) which was also associated with street crime. Given that this cannot be ruled out, it is possible that missing data across the panel resulted in bias to the forthcoming estimates.

Higher Job Quality and Crime

Table 11 presents the results of negative binomial regressions evaluating the relationship between higher job quality and crime. Model 1 presents the pooled negative binomial results. With respect to control variables, findings suggest that an increase in the perceived certainty of sanctioning ($p \leq .001$) and the severity of perceived social costs ($p \leq .05$) both had a negative association with street crime. An increase in perceived personal rewards, perceived social rewards, and illegal earnings were also associated with an increased rate of offending ($p \leq .001$). An increased perception that there were opportunities for work was negatively associated with offending ($p \leq .01$) and increased financial responsibility was associated with an increased rate of

offending ($p \leq .05$). Transitioning from being single to the lowest quality relationship was found to be criminogenic ($p \leq .001$). However, an increase in relationship quality among those who were in a relationship had a negative association with street offending ($p \leq .001$). An increase in routine activities conductive to crime ($p \leq .001$), perception that a criminal record was hindering employment ($p \leq .01$), and the proportion of time spent incarcerated ($p \leq .001$) also had a positive association with self-reported offending. An increase in education level had a negative association with offending ($p \leq .01$). Expecting a child was marginally associated with an increase in offending ($p \leq .10$). A change in additional sources of income, the number of children, being in school, the number of prior arrests, criminal justice supervision, and the number of hours worked were all not found to be significantly associated with self-reported offending at conventional levels.

With respect to the key employment related variable of interest, findings suggest that there was not a significant association between transitioning from not working to working in the lowest quality job and street crime. However, the quality of the job was found to be associated with street crime, conditional on working. Specifically, an increase in job quality was associated with an 11% reduction in the rate of offending, holding all else constant ($p \leq .05$). This finding provided some additional preliminary support for Hypothesis 1.

Model 2 within Table 11 offers fixed effects estimates. Given that findings with respect to control variables were fairly consistent across models, these results will not be discussed in text. With respect to the key work-related variables of interest, once analysis controlled for time-stable unobserved heterogeneity, findings continued to suggest that transitioning from not working to working within the lowest quality job was not associated with street crime.

Interestingly, although insignificant, the coefficient was in the opposite direction of traditional theoretical assumptions, suggesting that transitioning from not working to working in the lowest quality job may be somewhat criminogenic. This supports the underlying argument that not all employment is uniformly protective. With respect to job quality, findings suggest that once time-stable unobserved heterogeneity was controlled for, there was no longer significant evidence of a negative association between job quality and street crime. Therefore, results failed to support Hypothesis 1.

High Job Quality and Crime

Table 12 presents the results from pooled (Model 3) and fixed effects (Model 4) negative binomial regressions evaluating the relationship between high-quality employment and crime to test Hypothesis 2. With respect to the control variables, the substantive findings within Model 3 and 4 are substantively similar to findings from Models 1 and 2, and thus are not discussed in text. The work-related findings suggest that although transitioning from not working to working in a low-quality secondary sector job was found to have a marginal association with a reduction in offending prior to accounting for time-stable unobserved heterogeneity (Model 3), there was no evidence that this transition was significant within Model 4 once fixed unobserved heterogeneity was controlled for. Additionally, while the pooled regression results (Model 3) suggested that an improvement in job quality from low to high was negatively associated with street crime ($p \leq .05$), once Model 4 analysis accounted for fixed unobserved heterogeneity there was no longer evidence of a negative association between high job quality and the street crime at conventional levels of significance. Therefore, results failed to support Hypothesis 2.

Higher Job Quality and Crime: Within the Secondary Sector

Table 13 presents the results of the pooled (Model 5) and fixed effects (Model 6) negative binomial regressions evaluating the relationship between higher job quality and street crime within the low-quality secondary market to test Hypothesis 3. Once again, findings with respect to the control variables are substantively similar to previous findings and thus will not be discussed. The pooled and fixed effect estimates consistently suggested there was no evidence that transitioning from not working to working within the lowest quality job in the secondary sector was associated with a reduction in street offending. In addition, there was no significant evidence that an improvement in job quality within the low-quality secondary sector was associated with a reduction in offending, even prior to controlling for fixed unobserved heterogeneity.

Sensitivity Analyses

To check the robustness of findings against alternative modeling approaches, analyses also implemented unconditional fixed effects negative binomial models for each fully specified model through incorporating dummy variable for all individuals except one. Results reached conclusions which were substantively the same and are reported within Appendix Table 3A.

Table 4A within the Appendix also disaggregated the outcome variable, street crime, by crime type to examine if findings were substantively similar across violent, property, and drug crimes. These results suggest that findings with respect to street crime were substantively similar when the outcome was disaggregated by crime types.

Additionally, given that employment is often a condition of criminal justice supervision, it is theoretically plausible that being supervised by the criminal justice system (on probation or parole) influenced the effect of job quality on crime. Therefore, Table 5A within the Appendix

presents results from the final models conditional on whether or not an individual was being supervised. Findings suggest that results were consistent among those who were not on supervision (supervision=0) (the first column of Table 5A). Among those who were on supervision (supervision=1), however, there was some evidence that transitioning from not working to working in the lowest quality job was marginally criminogenic ($p \leq .10$), and that having a higher quality job was associated with a reduced rate of offending ($p \leq .05$) consistent with Hypothesis 1. There was also some marginal evidence that an increase in job quality reduced that rate of offending within the low-quality secondary sector ($p \leq .10$), consistent with Hypothesis 3. There was however, no evidence that high relative to low quality employment was associated with reduced street crime among those supervised by the criminal justice system, failing to find support for Hypothesis 2.

CHAPTER 6: RESEARCH QUESTION 3 ANALYTIC PLAN AND RESULTS

Before evaluating the relationship between job quality and workplace crime, it was imperative to first provide descriptive information regarding the prevalence of workplace theft within this sample. Within each wave, the percentage of workers who reported committing workplace theft ranged from 6–12% (12%, 10%, 10%, and 6% in waves 7–10 respectively). Table 14 provides the percentage of those working who were committing each type of workplace crime. Across all waves, stealing money was the least reported type of workplace crime. Stealing things and reporting false hours was relatively more frequent and comparably so. Notably, the percentage of those reporting each type of theft decreased across the waves.

Figure 13 also illustrates the likelihood of committing one type of workplace crime conditional on committing another for the 7th wave. Substantively, this table suggests that while only 3% of workers reported stealing money, the likelihoods of stealing things or reporting false hours conditional on stealing money were quite high, 86% and 86% respectively. Conditional on stealing things, the likelihoods of stealing money or reporting false hours were lower at 40% and 42%, respectively. Similarly, conditional on reporting false hours, the likelihoods of stealing money or things were 44% and 46%, respectively. Interestingly, conditional on committing any two types of workplace crime, the likelihood of committing a third type was very high, ranging from 95–100% (reported within the third column of Figure 13).

Preliminary Analyses: Bivariate Relationships

The study first evaluated the bivariate associations between job quality workplace crime using the bivariate methodology outlined within the previous chapter. Figures 14 and 15 illustrate that there was no significant evidence in support of a relationship between income and

workplace crime or the number of job benefits and workplace crime. In addition, with respect to items measured on a satisfaction scale, overall, there was little-to-no evidence to support that any of the items (i.e., salary, benefits, supervision, control, usefulness, advancement, status, security, colleagues, and workload) were individually associated with workplace crime (results within Appendix Figures 11A–20A). Similarly, Figure 16 suggests there was no significant evidence of a bivariate association between career track employment and workplace crime.

Figure 17 provides an illustration of the bivariate relationship between workplace crime and higher job quality using the aggregate job quality construct. Visually, findings suggest there was not a clear negative association between job quality and workplace crime. Statistical tests also provided reached similar conclusions. For instance, there was no evidence that the mean number of self-reported crimes differed across job-quality categories. Additionally, the bivariate negative binomial regression suggested that there was no evidence of a negative association between job quality and workplace crime at conventional levels of significance.

Figure 18 provides an illustration of the bivariate relationship between workplace crime and high job quality. While the mean number of workplace crimes was lower (.13 relative to .17) for high-quality employment, the mean difference was not statistically significant at conventional levels of significance. There was, however, indication that high-quality jobs were systematically associated with fewer workplace offenses using non-parametric tests ($p \leq .05$). Though, the bivariate negative binomial model provided no significant evidence of a negative relationship between high job quality and workplace crime. In sum, preliminary bivariate results suggested that there was very little evidence that higher or high-quality employment was negatively associated with workplace crime, failing to provide support for Hypothesis 4 or 5.

Analytic Plan

The study next evaluated the relationship between job quality and workplace crime through employing multivariate negative binomial regression to account for observable variables believed to influence both job quality and workplace crime. Analyses first estimated the following pooled model and clustered by the individual (results are available within Appendix Table 7A):

$$y_{it}^{WP} = \beta_0 + \beta_1 Q_{it} + \beta_2 X'_{it} + \varepsilon_{it} \quad (6)$$

Within this model Y_{it} represents a variety score of workplace crime committed by an individual (*i*) within wave (*t*). Mirroring the modeling strategy in the past chapter, Q_{it} represents job quality and X'_{it} represents a vector of both time-varying and time-constant controls. The equation also includes a constant (β_0) and an error term (ε_{it}). Notably, Equation 6 differs from Equation 1 in that work (W_{it}) was no longer included. This is because this chapter's analyses conditioned on only working recall periods. This was appropriate because the dependent variable, workplace crime, was *unobservable* for those who were not working within the data. This means that these individuals could have committed workplace crime had they decided to work. This selection issue is well recognized in labor economics among those studying the legal wage rate as an outcome (Gronau, 1974). Including non-working individuals within the analyses and simply coding their workplace crime count as zero would have been produced biased estimates. Therefore, analyses restricted the sample to only waves in which an individual was working. One consequence of this decision was that it limited to whom all of the findings can be generalized. Thus, the forthcoming results generalize to only high-risk individual who chose to work.

To gain some insight into factors which influenced selection into work and sample generalizability, the study conducted a pooled logistic regression with work as the outcome and all study covariates (excluding job characteristics) as regressors. Appendix Table 6A presents these results. Findings suggest that those who worked within a recall period did not differ in terms of their perceived certainty of sanctioning, social cost, personal or social rewards of crime. Increased illegal earnings in the previous recall period, however, were associated with a reduced likelihood of working ($p \leq .001$). Having higher perceived opportunities for work was associated with an increased likelihood of working ($p \leq .01$), as was having a higher level of financial responsibility ($p \leq .001$), a higher level of education ($p \leq .001$) and being enrolled in school ($p \leq .01$). Having other sources of income was associated with a reduced likelihood of working ($p \leq .001$). In addition, being in a romantic relationship was associated with an increased likelihood of working. Having more children ($p \leq .001$), a higher number of prior arrests ($p \leq .001$), spending a greater proportion of recall period incarcerated ($p \leq .001$), and being at the Philadelphia site relative to Phoenix ($p \leq .001$) was associated with a reduced likelihood of working. Having a higher quality relationship (conditional on being in a relationship), expecting a child, partaking in routine activities conductive to crime, perceiving a criminal record may influence employment, being supervised by the criminal justice system, age, and sex were all not found to be statistically associated with the likelihood of working.

This chapter also continued to recognize the importance of controlling for unobserved time-stable heterogeneity which may influence results. Therefore, analysis employed a random effects negative binomial estimator as follows:

$$Y_{it}^{WP} = \beta_0 + \beta_1 Q_{it} + \beta_2 X'_{it} + a_i + \varepsilon_{it} \quad (7)$$

Within this equation, Q_{it} continued to represent job quality and X'_{it} represents a vector of time-varying and time-constant predictors thought to be associated with both job quality and crime. Once again, the study site was interacted with year to control for time variant trends within each location. a_i represents the combined effect on workplace crime of all unobserved variables that are constant over time, ε_{it} represents random variation at each point in time, and β_0 is a constant.

The key difference between a fixed effects estimates (presented within the last chapter) and a random effects estimates is that random effects models assumes a_i is normally distributed with a mean of 0, constant variance, and is independent of the observed explanatory variables within the equation across all waves. Literature suggests that the random effects estimator may not control for all time-stable unobserved heterogeneity because it relies on the assumption that there is no correlation between time-stable unobserved variables and observed variables (Halaby, 2004, see also Brame, Bushway, & Paternoster, 1999). It is recognized that this is a strong assumption which is inconsistent with previously described concerns that fixed unobserved heterogeneity is associated with selecting into employment and quality jobs. However, while the fixed effects estimator does not rely on this assumption and is thus less biased, the improved consistency comes at a high cost of reduced efficiency. Due to the fact the sample was restricted to only those who were working and the within-individual estimator requires within-individual variation in the dependent variable– excluding individuals with no variation in the dependent variable– the fixed effects estimator would be notably inefficient. The choice between the fixed and random effects estimator thus came down to a trade-off between bias and efficiency. To formally consider this trade-off, I conducted a Wald test after estimating the following equation

(analogous to the models implemented in Chapter 5, but restricting the model to only those working) as outlined by Allison (2009):

$$Y_{it}^{WP} = \beta_0 + \beta_1 Q_{it} + \beta_2 X'_{it} + \gamma_1 \bar{Q}_i + \gamma_2 \bar{X}'_i + \rho_t + \varepsilon_{it} \quad (8)$$

The Wald test tested the equality of the "mean," and "mean deviation" coefficients, providing a test of the random effects model compared to the fixed effects model.[26] This test was conducted for each of the forthcoming models and all results were unable to reject the null at $p \leq .10$, failing to provide evidence in favor of the fixed effects estimator. Each of the forthcoming tables report the results of the respective Wald tests at the bottom of the model. Given the results of these tests, a random effects estimator was selected. For comparison, the fixed effects estimates (modeled with Equation 8) are also available in Appendix Table 7A for all fully specified models within the chapter.[27]

A Note on Missing Data

Of the 5,060 pooled observations (1,272 individuals) in the potential analytic sample described in Chapter 3, a total of 2,357 observations (47% of original analytic sample) were omitted because they were non-working waves. This resulted in 2,370 pooled observations (926 individuals) with at least one working-wave to contribute to analysis (47% of original analytic

[26] This test is an analogous alternative to the Hausman (1978) test.

[27] Although the random effects estimates are more biased than the fixed effects estimates, I believe the increase in bias was minimal relative to the large gain in efficiency. In the forthcoming results the random effects coefficients are similar in magnitude to the fixed effects estimates. As expected, the most notable difference between the fixed and random effects estimates are the size of the standard errors, reflecting the lack of efficiency associated with the fixed effects estimator. Further, had I selected the fixed effects approach, there would have been no significant associations ($p \leq .05$) within the models – for the key independent variable or any of the control variables. This highlights lack of efficiency as a key problem given that even if none of the 20+ variables within a model had a true association with crime, at a p-value of .05, at least one variable was expected to be significant by chance alone if the model was sufficiently powered.

sample). 333 pooled observations (7% of originally analytic sample) were omitted because the work indicator variable was missing, though this did not result in the exclusion of any individuals given that they all had at least one wave where their work status was indicated. Of the 2,357 pooled observations with work-status indicated, 2,204 (94% of those with a work indicator) had all of the data required to compute a job-quality score (no missing data on individual job quality items). This resulted in 901 individuals with at least one wave of job quality information available to contribute to estimates.

Of the remaining 2,204 observations with work indicated and job quality information, only 53 (2%) were missing information on the dependent variable, resulting in 2,151 observations (86 to contribute to estimates). The analytic sample was further reduced by missing data in the control variables, resulting in a final sample of 1,979 pooled observations (830 individuals) who contributed to estimates. This final sample included 84% of all potential working observations (16% missing due to missing data on job quality, the dependent variable, or controls). Consistent with the previous chapter, give that it is not likely that the missing data was missing at random, it is possible that the missing data may result in bias to the random effects estimates. Particularly if the data is missing because of unobservable factors which are correlated with crime and are not effectively controlled for using the random effects modeling strategy. Because this cannot be ruled out, missing data is a valid study limitation that may result in bias to the estimates.

Results

Higher Job Quality and Workplace Crime

Table 15 provides the results of the random effects estimation evaluating the relationship between job quality and workplace crime. Specifically, Model 7 presents the results examining the relationship between higher job quality and workplace crime. With respect to the control variables, findings suggested that an increase in hours worked ($p \leq .05$), perceived personal rewards of crime ($p \leq .01$), perceived social rewards of crime ($p \leq .05$), financial responsibility ($p \leq .01$), other sources of income ($p \leq .01$), and going from being single to being in the lowest quality romantic relationship ($p \leq .05$) were associated with an increase in the rate of workplace offending. An increase in the perceived social cost of crime and being in a higher quality relationship ($p \leq .05$) were associated with a decrease in the rate of workplace crime. All other control variables were not associated with workplace crime. With respect to employment quality, findings suggest that job quality had a marginally significant association with workplace crime ($p \leq .10$), in support of Hypothesis 4. Specifically, an increase in job quality was associated with a 14% reduction in the rate of workplace offending.

Model 8 provides the results of the random effects negative binomial model assessing the relationship between high job quality and workplace crime. Given that the results with respect to the control variables are substantively consistent between Models 7 and 8, these results will not be discussed in text. Results provided no significant evidence of an association between high job quality and workplace crime, failing to find support for Hypothesis 5. Additionally, Model 9 assessed the relationship between higher job quality and workplace crime, conditional on being employed within the low-quality secondary sector. This model found no significant evidence that an increase in job quality while remaining within the secondary sector was associated with a reduction in workplace crime, failing to support Hypothesis 6.

Sensitivity Analysis

Given that working for a greater proportion of a recall period could create more opportunities to commit workplace theft and be associated with job quality, the proportion of a recall period an individual was employed (duration) was included within all fully specified models as a robustness check. Appendix Table 8A provides these results, which suggest that the duration of employment did not influence any substantive findings.

Additionally, given that criminal justice supervision may influence the relationship between job quality and crime, Appendix Table 9A presents sensitivity analysis conditioning on supervision status. These results indicate that there was little evidence to suggest that supervision status moderated the relationship between job quality and workplace crime.

Table 10A also considers the extent to which workplace crime and street crime are qualitatively different constructs. Within the first column, Model 7 (higher job quality and workplace crime as presented previously in this chapter) is provided to facilitate comparison. The second column then provides a model which used the same analytic strategy as Model 7 (random effects, conditioned on working waves) but changed the dependent variable to property street crime only (8 items: entered a building to steal; shoplifted; bought, sold, or received stolen property; used credit cards illegally; stole a car or motorcycle; carjacked someone; robbed someone with or without a weapon; and entered a car to steal) excluding violent crime and drug crime from the street crime dependent variable used in the previous chapter. The third column then further excludes items from the property crime dependent variable to create a measure of street theft (5 items: entered a building to steal; shoplifted; stole a car or motorcycle; and entered a car to steal) which excluded more violent property crimes (e.g., carjacked, robbery) or crimes

which were arguably less similar to the study's measure of workplace theft (e.g., used credit cards illegally). Across the three columns, the control variables are similarly predictive of all three crimes (in significance and direction). This is consistent with previous work which has suggested that rational choice is a general theory of crime in which costs and benefits similarly influence decision-making (Loughran et al., 2016b). Notably, the certainty of apprehension was not significant for workplace crime, though it was significant for street property crime and street theft. This is likely due to the fact that the measure was collected specifically for street crime and is thus less proximately measuring the certainty of apprehension for workplace crime. In addition, the number of hours was significant for workplace crime but not street property crime or street theft. This is likely because the number of hours worked was a measure of opportunity for workplace crime, whereas exposure may more similarly measure opportunity for street property crime and street theft. Most importantly, job quality was found to be marginally associated with workplace crime but was not significantly associated with street property crime or street theft. This may be because job quality is a more proximate consideration in the commission of workplace crime relative to street crime. Comparisons of the street-workplace overlap will further be explored in the following chapter.

CHAPTER 7: RESEARCH QUESTION 3A ANALYTIC PLAN AND RESULTS

Given that there is little known about the overlap between street and workplace crime, the study first evaluated the extent of overlap found within this data. Table 16 provides the percentage of employed offenders who were committing only street crime, only workplace crime, or both (street and workplace crime). Within any given wave, 46–62% of employed offenders were committing only street crime, 20–35% were committing crime only in the workplace, and less than 20% (12–19%) were committing both. There was a higher likelihood in any given wave that an individual was committing either street or workplace crime than committing both.

Analytic Plan

To evaluate whether job quality had an association with the likelihood of committing no crime, crime on the street only, crime in the workplace only, or both, the present study applied multinomial logistic (hereafter mlogit) regression to fit the unordered categorical outcome (Long, 1997). Consistent with the previous chapter, this analysis incorporated only waves in which an individual was working, because two of the categorical outcomes– workplace crime and both– were unobservable among those who were not working. Again, the consequence of this sample selection is that results only generalize to working high-risk individuals. To analyze the data, the study estimated a random effects mlogit model using generalized structural equation modeling to account for dependence of observations across time periods as follows: [28]

[28] The research fit generalized structural equation models in Stata with gsem using the procedure outlined in Stata's published example 41g "Two-level multinomial logistic regression (multilevel)." For comparison, the study also conducted pooled mlogit models clustered by the individual and reached results which were largely consistent (Appendix Tables 11A–12A).

$$\log\frac{p_{ij}}{p_{iJ}} = \beta_0 + \beta_{1j}Q'_{it} + \beta_{2j}X'_{it} + a_i + \varepsilon_{it} \qquad \mathrm{j} = 1, \ldots \mathrm{J} - 1 \tag{9}$$

Within this equation, j is an indicator of outcome category. The model continued to include job quality (Q_{it}) and a vector of both time-varying and time-constant controls (X'_{it}). ε_{it} represents random variation for individuals at each point in time and a_i represents the combined effect of all time-constant unobservables. The research also considered the conditional mlogit fixed effects estimator outlined by Chamberlain (1980) and Pforr (2014). However, the study elected to use the random effects estimator because it allowed for the retention of between-individual variation and thus provided more efficient standard errors, consistent with the rationale from the previous chapter.[29] For those interested in the comparison, fixed effects estimates are available within Appendix Tables 11A and 12A. There is no known statistical test which formally compares the trade-off between these two methods.

Results

Higher Job Quality and the Street-Workplace Crime Overlap

Table 17 provides the results from the mlogit analysis evaluating the relationship between higher quality employment and the street-workplace crime overlap. Findings with respect to the control variables, examining the likelihood of committing no crime relative to street crime only (column 1), suggest that an increase in the certainty of sanctioning ($p \leq .01$), perception of opportunities for work ($p \leq .05$), and romantic relationship quality ($p \leq .01$) were all associated with an increased likelihood of committing no crime relative to committing street crime only.

[29] In addition, the nature of the missing data across the study is largely consistent with the previous chapter and will thus not be described in detail here. The additional missing data within this chapter is due to the overlap of missing data in the dependent variable. Where the previous chapter only included work crime, this chapter also required measures of street property crime. This resulted in the loss of 14 observations (12 individuals.

Increased perceptions of personal rewards of crime ($p \leq .05$), perceptions of social rewards of crime ($p \leq .001$), illegal earnings ($p \leq .001$), having other income sources ($p \leq .05$), entering into a low-quality relationship ($p \leq .01$), expecting a child ($p \leq .01$), partaking in routine activities conductive to crime ($p \leq .001$), having more prior arrests ($p \leq .05$), perceptions of one's criminal record ($p \leq .001$) and (marginally) spending a greater proportion of the recall period incarcerated ($p \leq .10$) were all associated with a decreased likelihood of committing no crime relative to street crime among those who were working. Put another way, these factors were found to be criminogenic toward street crime. The perceived social cost of crime, financial responsibilities, number of children, a higher level of education, being in school and being under criminal justice supervision had no significant association with the likelihood of committing no crime relative to street crime. With respect to job quality, findings suggest that higher job quality was not significantly associated with the likelihood of committing no crime relative to street crime, failing to provide evidence in support of Hypothesis 7a.

Findings from the comparison between street crime only and workplace crime only (column 2) suggest that increased social costs ($p \leq .10$), perceived social rewards of crime ($p \leq .01$), expecting a child ($p \leq .01$), routine activities conducive to crime ($p \leq .05$), perceptions of criminal record ($p \leq .05$), and spending a greater proportion of the recall period incarcerated were significantly or at least marginally associated with a reduced likelihood of committing workplace crime relative to street crime. Results also suggest that increased financial responsibility was associated with an increased likelihood of committing workplace crime relative to street crime ($p \leq .05$). All other control covariates did not have an association with these relative likelihoods. With respect to the key job quality covariate, findings indicated that job quality did not have an

association with the likelihood of committing workplace crime relative to street crime, failing to provide support for Hypothesis 7c.

Findings from the comparison between committing street crime only and choosing to commit both street and workplace crime (column 3) suggest that increased perceived personal rewards of crime ($p \leq .10$), financial responsibility ($p \leq .01$), and being enrolled in school ($p \leq .10$) had a significant or marginally significant association with an increased likelihood of committing both types of crime relative to street crime only among those who worked. Having a higher number of prior arrests was marginally associated with a reduced likelihood that an individual would commit both types of crime relative to street crime only ($p \leq .10$). An increase in hours worked was also marginally associated with a higher likelihood that an individual would commit both types of crime relative to street crime only ($p \leq .10$). All other control covariates did not have an association with the likelihood of committing both crimes relative to street crime only. With respect to job quality, findings suggest that higher quality employment was marginally associated with a decreased likelihood of committing both types of crime relative to street crime only. More specifically, those with higher quality jobs had a 29% reduced likelihood of committing both types of crime relative to street crime ($p \leq .10$), lending support to Hypothesis 7b.

Table 18 presents a change in the reference category to facilitate additional comparisons. The findings within the top half (column 1) of the table suggest that job quality had no significant association with the likelihood of committing no crime, street crime only, or both, relative to workplace crime only. The bottom half of Table 18 also suggests that an increase in

job quality was not associated with a reduced likelihood of committing street crime only, workplace crime only, or both, relative to committing no crime; failing to support Hypothesis 7a.

High Job Quality and the Street-Workplace Crime Overlap

Table 19 provides the results of the mlogit analysis evaluating the relationship between high job quality and street-workplace crime overlap. Column 1 presents the relative likelihoods of participating in no crime relative to street crime only. With respect to the control variables, findings suggest that an increase in the perceived certainty of apprehension ($p \leq .05$), perceived opportunities for work ($p \leq .05$), and relationship quality ($p \leq .01$) were associated with an increased likelihood of committing no crime relative to street crime only. An increase in the level of education was also marginally associated with an increased likelihood of committing no crime relative to street crime only ($p \leq .10$). An increase in perceived personal rewards of crime ($p \leq .05$), social rewards of crime ($p \leq .001$), illegal earnings ($p \leq .001$), other sources of income ($p \leq .05$), transitioning from being single into the lowest quality relationship ($p \leq .01$), expecting a child ($p \leq .01$), participating in routine activities conductive to crime ($p \leq .001$), having more prior arrests ($p \leq .05$), increased perceptions of criminal record hindering employment ($p \leq .001$), and spending a greater proportion of the recall period incarcerated ($p \leq .10$) were all associated with a reduced likelihood of committing no crime relative to street crime only. The number of hours spent working per week, perceived social cost of crime, financial responsibilities, number of children, being enrolled in school, and being on criminal justice supervision were not associated with the likelihood of committing no crime relative to street crime only at conventional levels of significance.

With respect to job quality, having a high-quality job was not significantly associated with the likelihood of committing no crime relative to street crime only. In fact, although insignificant, the coefficient was in the opposite direction of what was hypothesized. Therefore, this finding failed to provide support for Hypothesis 7a given that high job quality had no association with the likelihood of committing no crime relative to street crime.

The second column of Table 19 compares the relative likelihoods of committing workplace crime only relative to street crime only. With respect to the control variables, findings suggest that an increase in the perceived social costs of crime ($p \leq .10$), social rewards of crime ($p \leq .05$), expecting a child ($p \leq .01$), partaking in more routine actives conductive to crime ($p \leq .05$), more prior arrests ($p \leq .10$), increased perceptions that a criminal record hinders employment ($p \leq .05$), and spending a greater proportion of the recall period incarcerated ($p \leq .01$) were all significantly or marginally associated with a reduced likelihood of committing workplace crime only relative to street crime only. Increased financial responsibilities were marginally associated with an increased likelihood of committing workplace crime relative to street crime ($p \leq .10$). All other control variables were not associated with the likelihood of committing workplace crime only relative to street crime only.

With respect to high job quality, there was no evidence to suggest that those with high quality jobs in the primary sector were less likely to commit workplace crime only relative to street crime only at conventional levels of significance, failing to support Hypothesis 7c, though the coefficient was in the expected direction.

The third column of Table 19 presents a comparison of the relative likelihoods of committing both street and workplace crime relative to committing only street crime. Findings

did not support that the certainty of apprehension, perceived social costs of crime or illegal earnings influenced the decision to commit street crime only relative to both, among those who were working. Perceived personal rewards of crime, however, was marginally associated with an increased likelihood of committing both relative to street crime only ($p \leq .10$). In addition, increased perceptions of the social rewards of crime was associated with an increased likelihood of committing both relative to only street crime ($p \leq .05$). Being enrolled in school and having more prior arrests were both marginally associated with an increased likelihood of committing both types of crime relative to street crime only ($p \leq .10$). Spending more hours per week working was also marginally associated with an increased likelihood of committing both types of crime relative to street crime only ($p \leq .10$). All other control variables did not have a significant statistical association with the relatively likelihood of committing street crime only relative to both types of crime.

With respect to high-quality employment, there was evidence that having a high-quality primary sector job served as a deterrent toward committing both types of crime relative to street crime only ($p \leq .05$). Specifically, among those who were working, having a high-quality job was associated with an 69% reduced likelihood of committing both street and workplace crime relative to street crime only. This finding supports Hypothesis 7b, given that those with low quality secondary sector employment had an increased likelihood of committing both types of crime relative to street crime only, among those who were working.

Table 20 provides output which varied the reference category to facilitate additional comparisons between high job quality and the street-workplace overlap. These findings suggest that high-quality employment did not have an association with the likelihood of committing no

crime relative to workplace crime only, failing to find support for Hypothesis 7a. However, those with high-quality employment were marginally less likely to commit both street and workplace crime relative to workplace crime only, providing support for Hypothesis 7b ($p \leq .10$).

Specifically, among those who were working, having a high-quality primary sector job was associated with an 61% reduced likelihood of committing both street and workplace crime relative to workplace crime only. Additionally, among those who were working, primary sector employment had a marginal negative association with committing both street and workplace crime relative to no crime such that those with a high-quality primary sector jobs were 58% less likely to commit both types of crime relative to committing neither of these crimes ($p \leq .10$). This finding supports Hypothesis 7b, given that having a low-quality secondary sector job was associated with an increased likelihood of commit both street and workplace crime amongst those who were working.

CHAPTER 8: DISCUSSION AND CONCLUSION

Criminologists have consistently proposed a negative association between employment and crime should exist such that individuals with a job are simply less likely to offend than those who are not working. However, employment programs and observational studies focusing solely on states of employment have failed to consistently find support for employment as a deterrent. Drawing on rational choice and dual labor market theories, this study proposed that inconclusive findings within the literature may be attributable to the lack of emphasis on heterogeneity in job quality which falls under the guise of "employment." Additionally, the study argued that the overly parsimonious belief that employment is uniformly protective against crime has led scholars to overlook workplace crime as a potential outcome among employed high-risk individuals. To date, there has been only a single known study, published 35 years ago, which considered street crime and workplace crime within the same theoretical framework (Sviridoff & Thompson, 1983).

The present research sought to fill these gaps in the literature through evaluating the relationship between job quality and crime amongst a sample of high-risk individuals during their transition to adulthood. The study's first research question examined the extent of variability in job quality amongst the high-risk sample. Drawing on those findings, the study's second research question then empirically evaluated the relationship between job quality and street crime. The study's third research question then considered whether the findings with respect to job quality and street crime similarly applied to workplace crime. Relatedly, the research also considered whether job quality had an association with whether an individual committed no crime, street crime only, workplace crime only, or both (Research Question 3a).

With respect to Research Question 1, there was a fairly large amount of both between and within individual variability in job quality within this high-risk sample. A greater proportion of variation in each individual job quality item was between as compared to within-individuals. Between individuals, the range in job quality within this study was notable. For instance, the self-reported income for a given wave ranged from $40 to $150,000. With respect to each job quality item as well as the aggregate scale, individuals self-reported at the high and low ends of the possible scale, indicating between individual variation in the types of jobs individual had– they were not all uniformly low-quality jobs.

Job quality outcomes from this sample may have been less uniformly low than outcomes reported by previous studies with high-risk samples, particularly studies of incarcerated offenders (e.g., Cook et al., 2015; Sviridoff & Thompson, 1983) because individuals within this study did not persist in offending to the extent that they were incarcerated to be included within the sample.

Consistent with expectations, there was relatively less variability in job quality within as compared to between individuals, though the study did find evidence of within-individual change in job quality during the transition to adulthood. On average, job quality was improving across waves, likely as a result of the sample aging and gaining job experience. Data suggested that individuals showed the least amount of within-individual variation with respect to quality dimensions such as income, which is consistent with notions of structural disadvantage. Overall, the study's findings with respect to Research Question 1 demonstrated variation in job quality between and within-individuals which had the potential to influence the decision to offend.

Research Question 2 then evaluated whether variability in job quality observed within Research Question 1 had an association with street crime. The study's first notable finding was that there was no evidence to support that transitioning from having no job to having the lowest quality job was associated with a reduction in street crime, consistent with the study's general theoretical argument that not all employment is protective. With respect to job quality, prior to accounting for unobserved heterogeneity, evidence supported an association between job quality and street crime. However, once analyses implemented a fixed effects modeling strategy to account for time-stable unobserved heterogeneity, there was no evidence to suggest that an increase in job quality reduced the rate of street crime among employed individuals; failing to support Hypothesis 1.

This finding diverged from previous scholarship which found an association between job quality and street crime (e.g., Grogger, 1998; Meisenhelder, 1977; Myers, 1983, 1984; Paternoster et al., 2016; Simons et al., 2002; Wadsworth, 2006; Witte, 1980). The difference in findings may be due to the fact that previous research did not effectively control for unobserved heterogeneity, given that the present study observed a relationship between job quality and street crime prior to controlling for unobserved time-stable individual differences. The divergence in findings may also be related to the fact that many of these studies used data from earlier time periods with different labor market contexts. Differences may also be due to varying sample demographics as some of the samples were not high-risk samples selected for past criminal involvement (e.g., Simons et al., 2002; Wadsworth, 2006), or had less race/ethnic diversity then the present sample (Simon et al., 2002).

Most importantly, the differences in findings may also have emerged because the prior literature did not use a measure of job quality which was multidimensional. Therefore, studies which did not include an equally robust measure of job quality may have found significant findings with respect to a single job quality dimension, but a single dimension is not an appropriate measure given that a job's total value is theoretically a net sum of many objective and subjective utilities and disutilities associated with the job (Burchell et al., 2014; Cazes et al., 2015; Dahl et al., 2009; Kalleberg, 1977; Kalleberg & Vaisey, 2005). Failing to take a multi-dimensional approach which accounts for the various factors which influence job quality may have resulted in a failure to effectively measure the construct which could have produced varying results.

The study also did not find support for its second hypothesis which proposed that high (as opposed to low) job quality would have an association with street offending. Analogous to the findings with respect to Hypothesis 1, although high-quality primary sector employment had an association with street offending prior to accounting for fixed unobserved heterogeneity, once time-stable individual differences such as ability, motivation, and criminal propensity were controlled for within the model, there was no evidence to support a negative association between primary sector employment and street crime. In addition, transitioning from having no job to having a job within the secondary sector was also not associated with a reduction in offending.

This finding notably differed from predictions associated with dual labor market theory. While the null results are consistent with some earlier findings (Crutchfield & Pitchford, 1997), this finding is inconsistent with Crutchfield et al.'s (2006) most recent work. Although the present study elected to use a multi-dimensional measure of dual labor market stratification, it is

unlikely that the lack of support was purely the result of this measurement choice given that sensitivity analysis also operationalized market stratification by occupational title (analogous to Crutchfield's previous work) and the results continued to fail to support an association between high quality employment and street crime. It is more likely that findings differed because the prior work was cross-sectional and did not account for time-stable individual differences influencing selection into high-quality occupations. All of the known research explicitly drawing on dual labor market theory to explain offending has also relied on samples which were at low-risk for criminal involvement and thus results do not necessarily generalize toward a high-risk population. It is possible that having a high-quality primary sector job simply is not a deterrent among high-risk offenders.

Additionally, when a job remained within the secondary sector and analysis accounted for unobserved heterogeneity, an improvement in job quality was also not associated with street offending. This is particularly interesting because it demonstrates that consistent with theory which emphasizes structural disadvantage and low-market mobility, when jobs remain in the secondary sector, an improvement in job quality did not serve as a deterrent.

With respect to the study's evaluation of the relationship between job quality and workplace crime (Research Question 3), a few interesting findings emerged. First, given that no known research has described the prevalence of workplace crime among a high-risk sample, the initial key findings are largely descriptive. For instance, the percentage of workers who reported committing workplace theft ranged from 6–12% across the four study waves. This prevalence finding was lower than Hollinger & Clark's (1983b) estimate that 25–50% of a workforce were participating in workplace theft. The study's reported theft prevalence may be lower for at least

three reasons. First, the two estimates have different denominators. Specifically, Hollinger & Clark's estimate reported the percentage of employees committing theft within a specific workforce (e.g., retail stores, hospitals, and electronics manufacturing firms). This study's estimate reports the percentage of individuals committing workplace theft among an employed high-risk sample with past criminal involvement, whom were employed in a variety of different types of jobs. Though further examining these estimate differences was beyond the scope of the study, it could be that high-risk individual who obtain work choose not to commit workplace crime as often as general samples of employees within a firm. Differences in workplace theft prevalence could also be due to differences in measurement, as Hollinger & Clark's (1983b) measure of employee theft included additional items such as misusing the discount privilege, purposely underring a purchase, and being reimbursed for more money than was actually spent on business expenses. Simply having a higher number of offenses which are considered as workplace crime could increase the prevalence of previous estimates relative to the present study. Finally, differences in workplace theft prevalence may also have be due to differences in time period. Many of the most common employee theft estimates are drawn from studies in the 1980s. It is possible that factors such as improvements in security technology and changes to workplace culture have reduced the prevalence of employee theft over time, though, empirically evaluating this possibly was beyond the scope of the present study.

Overall, the findings with respect to workplace crime were only somewhat consistent with findings with street crime as the outcome. Specifically, although job quality did not have an association with street crime, job quality had a marginal association with workplace crime. Specifically, higher job quality was marginally associated with a 14% reduction in the rate of

workplace crime. Therefore, there was some support that higher job quality had an association with workplace crime, in support of Hypothesis 4. This finding is consistent with theoretical articulations which have suggested that job quality is associated with theft in the workplace (e.g., Ditton, 1977; Greenberg & Barling, 1996; Hollinger & Clarke, 1983b; Mars, 1973).

However, the study did not find evidence to suggest that primary sector employment had a negative association with workplace crime, failing to support that high as opposed to low-quality employment had a negative association with workplace crime (Hypothesis 5). Consistent with findings with regards to street crime, the dual labor market conceptualization did not appear to have an association with the rate of workplace crime. Within the secondary sector, findings also did not find an association between higher job quality and workplace crime (Hypothesis 6). Substantively, this indicates that an improvement in job quality was not able to deter workplace theft when the job remained within the low-quality secondary sector.

Given that there are no known estimates surrounding the overlap between street and workplace crime. Within this high-risk sample, findings suggest that in any wave, 46–62% of employed offenders were committing only street crime, 20–35% were committing crime only in the workplace, and less than 20% were committing both. These descriptive results suggest less overlap than some theorists likely would have predicted (e.g., Gottfredson & Hirschi, 1990; Murphy, 1993). Substantively, this lack of overlap provides some evidence that offenders were selecting where they choose to commit crime, given that when these individuals had the opportunity to commit crime in both places– a large majority (80%) selected to commit crime on the street *or* in the workplace rather than both.

In evaluating whether job quality influenced the choice to commit no crime, street crime only, workplace crime only, or both, there was little-to-no evidence to support that higher job quality was associated with the relative likelihoods of being within any of the street-workplace crime overlap category as predicted. The only exception was that an increase in job quality was found to be marginally associated with a reduced likelihood of committing both types of crime relative to street crime only. This may substantively suggest that individuals with high-quality jobs who are committing crime on the street may be choosing not to commit crime at work to protect their valued employment, consistent with theoretical predictions.

With respect to high relative to low-quality employment, there was no evidence to support that high job quality influenced the decision to commit no crime relative to street crime or no crime relative to workplace crime, among those who were working. In addition, there was no evidence that high job quality influenced the decision to commit street crime relative to workplace crime. Rather, having high-quality primary sector employment appeared to at least marginally deter individuals from committing both types of crime relative to committing no crime, street crime only, and workplace crime only.

Though the present study contributed the field's understanding of the relationship between job quality and crime, there were limitations of this research which must be acknowledged. First, with respect to measurement, the level of analysis was the individual-wave, with annual waves. Because individuals could have held more than one job within a given wave, many job quality measures captured the weighted average job quality for the wave. One consequence of this is that some of the measures do not necessarily capture all of the variation in an individual's job quality that may have occurred within a wave.

Second, there are limitations with respect to the generalizability of study findings. Given that the Pathways to Desistance data is a high-risk sample of individuals who were adjudicated for a serious crime in adolescence, findings from this sample many not generalize to low-risk samples drawn from the general population, or perhaps even other high-risk samples selected on current offending. In addition, job quality estimates only generalize toward high-risk individuals who selected to work, as job quality was only observable for those individuals. In addition, when the fixed effects estimator was applied, estimates generalize only toward individuals who had variability in the dependent variable.[30] The present study also captured individuals for four years during the transition to adulthood (ages 18-26). Therefore, these findings may only generalize towards this specific time in the adult life course. This is particularly relevant given that studies have found relationship between employment and crime may vary by the age of the sample (e.g., Bachman & Schulenberg, 1993; Uggen, 2000).

Third, it is also important to note that the study's measure of workplace crime encompassed only workplace theft and the measures of overlap included only workplace theft and street property crime. Therefore, results of the study can only generalize toward workplace theft and the street-workplace property crime overlap. I elected, however, to refer to these constructs broadly as workplace crime and the street-workplace crime overlap within discussion because these concepts theoretically apply to all types of crime. Although this data cannot speak to the generalizability of the findings across types of workplace crime, in theory, I believe there was no reason to restrict the theoretical discourse to only one type of crime. Evaluating the

[30] To contribute to a fixed effects estimate an individual must have persisted in offending into adulthood because offending could not be zero across all waves as this produces no within-individual variability for estimation.

generalizability of this study's findings to other types of workplace crime is a fruitful area for future scholarship.

Fourth, when studying the relationship between job quality and crime, although the study made an effort to rigorously control for time-stable individual differences, the modeling approach may relied on assumptions which may have been violated resulting in bias. For instance, there may have been time-varying factors which influenced both job quality and crime which were not included within the models, resulting in omitted variable bias. Given these limitations, it is important to address that these findings are purely associational and cannot take on causal interpretations.

Fifth, temporal ordering is also of key importance in causal inference and is a potential limitation of this research. Although the present study theoretically proposed higher job quality decreases the likelihood of criminal behavior, it is also possible that committing crime decreases the likelihood of having a quality job (Hagan, 1993; Thornberry & Christenson, 1984). The study included individuals' number of prior arrests (prior to the recall period), perception of criminal record influencing employment, and reported illegal earnings in the previous wave to try and account for ways in which past criminal behavior may influence job quality and crime. Nonetheless, the possibility of a reciprocal relationship between job quality and crime is a notable limitation of the study and all significant findings should only be interpreted as statistical associations rather than causal effects.

Although the study had limitations, it also provides fertile areas for future research. For instance, while the present study focused on the quality of the job itself, there may be individual level factors which influence one's commitment to the job or employment as a conventional line

of action which the present study did not capture. Understanding the relationship between job quality and commitment is a future direction for those interested in evaluation the relationship between employment and crime. In addition, given that there were differences in how job quality influenced street crime relative to workplace crime, there may be utility in continuing to disaggregate by crime type to try to understand how context can influence decision-making. Future research could also continue to explore whether dual labor market theory is an appropriate dichotomization for studying high-risk populations. There is little guidance within the literature as to how to most effectively measure the proposed stratification. Future research could focus on potential operationalizations most appropriate for testing this theory.

There are also methodological limitations associated with studying workplace crime, as it unobservable among those who are not working. This makes studying potential displacement affects associated with the transition into employment particularly difficult. Future scholarship should seek to advance methodology to more appropriately capture the employment transition and account for unobservable outcomes when an individual is not employed. Developing methods which do not require selecting on only working individuals is a key area for future research.

In sum, I believe the present study provided a strong test of job quality as an informal cost of crime which was consistent with the general tenants of rational choice and dual labor market theories as well as scholarship which has focused on measurement of job quality as multidimensional. The results of which, did not unanimously support that job quality influenced the decision to offend. Taken together these results provide a counterweight to overly simplistic conclusions that the relationship between employment and crime is monolithically negative. In

addition, these findings suggest that an improvement in job quality and even high-quality primary sector employment, does not necessarily deter crime among a high-risk sample. As such, I advocate for future studies of offender decision-making to continue to measure job quality as a multidimensional construct and to explore potential reasons why an improvement in job quality may not have been consistently associated with a reduction in offending.

TABLES AND FIGURES

Table 1. Summary of Research Questions and Hypotheses

Research Question 1: Among an employed high-risk sample, to what extent is there variability in job quality?

Research Question 2: What is the relationship between job quality and crime?

- Hypothesis 1: Higher job quality will be negatively associated with crime.
- Hypothesis 2: High (relative to low) job quality will be negatively associated with crime.
- Hypothesis 3: Within the secondary market, job quality will be negatively associated and crime.

Research Question 3*:* Does the relationship between job quality and crime extend to crime in the workplace?

- Hypothesis 4: Higher job quality will be negatively associated with workplace crime.
- Hypothesis 5: High (relative to low) job quality will be negatively associated with workplace crime.
- Hypothesis 6: Within the secondary market, job quality will be negatively associated with workplace crime.

Research Question 3a: Is job quality associated with whether an individual commits no crime, street crime only, workplace crime only, or both?

- Hypothesis 7a: High(er) job quality will be positively associated with committing no crime.
- Hypothesis 7b: Low(er) job quality will be positively associated with committing both street and workplace crime.
- Hypothesis 7c: High(er) job quality will be positively associated with committing street crime relative to workplace crime.

Table 2. Descriptive Statistics

Variable	Mean	SD	Med	Min	Max	Within[2]
Crime						
All Crime	0.61	1.38	0	0	12	0.39
Property Crime	0.27	0.81	0	0	8	0.43
Violent Crime	0.12	0.40	0	0	3	0.41
Drug Crime	0.22	0.56	0	0	2	0.49
Workplace Crime[1]	0.16	0.56	0	0	3	0.52
Overlap[1]	0.33	0.74	0	0	3	0.41
Employment						
Employment	0.50	0.50	1	0	1	0.40
Job Quality[1]						
Job Quality	3.95	0.95	4.07	0	6.21	0.38
Employment Related Control[1]						
Hours (categorical)	3.14	1.06	3	0	6	0.40
N=1,272						

1: Descriptives provided for only working recall periods
2: Proportion of within individual variation, all values significant at $p \leq .001$

Table 3. Descriptive Statistics Continued

Variable	Mean	SD	Med	Min	Max	Within[2]
Time Varying Controls						
Certainty	5.81	2.97	5.71	0	10	0.36
Social Cost	3.31	0.93	3.40	1	5	0.43
Personal Rewards	1.51	2.23	0.29	0	10	0.33
Social Rewards	1.86	0.52	2.00	1	4	0.40
Illegal Rewards ($10,000/year)	0.30	1.55	0.00	0	15	0.60
Work Opportunities	3.63	0.67	3.80	1	5	0.42
Financial Responsibilities	4.59	2.69	5.00	0	10	0.40
Other Income	0.38	0.64	0.00	0	4	0.50
Romantic Relationship	0.61	0.49	1.00	0	1	0.46
Relationship Quality[1]	3.04	0.76	3.00	3	5	0.47
Expecting a Child	0.09	0.28	0.00	0	1	0.70
Number of Children	0.74	1.00	0.00	0	6	0.18
Education	0.59	0.51	1.00	0	2	0.11
Enrolled in School	0.25	0.44	0.00	0	1	0.60
Routine Activities	2.88	0.95	3.00	1	5	0.42
Prior Arrests	2.13	2.48	1	0	23	0.16
Criminal Record	0.15	0.36	0	0	1	0.57
Proportion of Time Incarcerated	0.28	0.39	0	0	1	0.26
Supervision	0.26	0.44	0	0	1	0.50
Age	21.49	1.60	21	18	26	0.48
Year	2007	1.01	2007	2005	2010	0.66
Time Constant Controls						
White	0.21	0.41	0	0	1	--
Black	0.40	0.49	0	0	1	--
Hispanic	0.34	0.48	0	0	1	--
Other	0.05	0.21	0	0	1	--
Male	0.85	0.35	1	0	1	--
Study Site (Philadelphia)	0.50	0.50	0	0	1	--
N=1,272						

1: Descriptives provided for only recall periods in a relationship
2: Proportion of within individual variation, all values significant at $p \leq .001$

Table 4. Job Quality: By Dimension

	Mean[1]	SD	Median[1]	Within[1,2]	Very Dissatisfied	Dissatisfied	Neither	Satisfied	Very Satisfied	Factor Loadings
Income ($10,000/yr)	1.62	1.77	1.19	.34	--	--	--	--	--	0.33
Benefits	1.28	1.63	.05	.40	--	--	--	--	--	0.35
Satisfaction with Salary	2.48	1.03	3	.46	6	16	26	43	9	0.59
Satisfaction with Benefits	2.09	1.11	2	.45	11	24	31	27	7	0.55
Satisfaction with Supervision	2.74	0.88	3	.49	3	9	24	52	11	0.65
Satisfaction with Control	2.98	0.79	3	.46	1	6	18	57	18	0.69
Satisfaction with Usefulness	2.86	0.84	3	.47	2	7	21	54	15	0.74
Satisfaction with Advancement	2.57	0.99	3	.47	4	15	28	41	13	0.71
Satisfaction with Status	2.69	0.87	3	.43	2	11	27	49	11	0.78
Satisfaction with Security	2.72	0.92	3	.47	3	11	23	49	14	0.70
Satisfaction with Colleagues	2.87	0.85	3	.47	2	7	20	56	15	0.55
Satisfaction with Workload	2.68	0.89	3	.50	3	11	24	53	10	0.58
Career Track Job	0.28	0.42	0	.45	--	--	--	--	--	0.34

Note: Descriptives provided for only working recall periods, values listed under very dissatisfied, dissatisfied, neither, satisfied, and very dissatisfied are the percentages of observations within each category.
1: Descriptives provided before variable was recoded categorically for descriptive purposes (for satisfaction items)
2: Proportion of within individual variation, all values significant at $p \leq .001$

Table 5. Wave 8 Income Conditional on Wave 7

		Wave 8									
		0	1	2	3	4	5	6	7	8	N
Wave 7											
≤$5,000	0	.43	.18	.19	.09	.07	.02	.01	0	.01	116
$5,000- $9,999	1	.21	.23	.22	.22	.06	.02	.01	.01	0	94
$10,000- $14,999	2	.10	.12	.22	.38	.13	.03	.01	0	.01	69
$15,000-$24,999	3	.06	.06	.06	.51	.21	.07	.02	0	0	81
$25,000-$34,999	4	0	.10	10	.26	.29	.19	.03	.03	0	31
$35,000-$49,999	5	0	0	0	.11	.44	.22	.11	0	.11	9
$50,000-$74,999	6	0	.25	0	0	0	.50	0	.25	0	4
$75,000-$99,999	7	--	--	--	--	--	--	--	--	--	--
$100,000+	8	0	0	0	0	0	1	0	0	0	1

Note: Values presented in the cells are conditional probabilities. Values only provided for working recall periods.

Table 6. Percentage of Working with Each Job Benefit

	Wave			
	7	**8**	**9**	**10**
Sick leave	26	31	38	35
Paid vacation	33	37	45	46
Retirement plan	18	20	29	31
Health insurance	39	40	48	48
Child care	4	6	6	5
Transportation costs	6	6	9	9
N	624	615	605	527

Table 7. Wave 8 Benefits Conditional on Wave 7

		Wave 8							
		0	1	2	3	4	5	6	N
Wave 7	0	.68	.09	.09	.06	.07	.01	0	252
	1	.60	.11	.11	.11	.06	.02	0	47
	2	.32	.21	.11	.18	.11	.03	.05	38
	3	.35	.08	.06	.33	.17	0	0	48
	4	.14	.14	.11	.21	.29	.07	.04	28
	5	.25	.13	.25	0	.25	0	.13	8
	6	0	0	0	0	0	1	0	1

Note: Values in the cells are conditional probabilities. Probabilities provided only for working recall periods.

Table 8. Wave 8 Job Satisfaction Conditional on Wave 7

			Wave 8					
			0	1	2	3	4	N
Wave 7	Very Dissatisfied	0	0	.33	0	.67	0	3
	Dissatisfied	1	.02	.23	.63	.11	.02	47
	Neither	2	.01	.13	.63	.22	.02	246
	Satisfied	3	0	.03	.48	.47	.02	118
	Very Satisfied	4	0	0	.50	.50	0	4

Note: Values presented in the cells are conditional probabilities. Values provided only for working recall periods.

Table 9. Wave 8 Career Track Job Conditional on Wave 7

		Wave 8		
		No 0	Yes 1	N
Wave 7	No 0	.77	.23	309
	Yes 1	.50	.50	113

Note: Values presented in the cells are conditional probabilities.
Values provided only for working recall periods.

Table 10. Wave 8 Job Quality Conditional on Wave 7

		Wave 8						
		0	1	2	3	4	5	N
Wave 7	0	0	0	0	0	0	1	1
	1	0	.11	.22	.44	.11	.11	9
	2	0	.02	.22	.47	.22	.06	49
	3	.01	.02	.17	.43	.35	.03	126
	4	0	.01	.05	.28	.51	.16	167
	5	0	0	.03	.17	.53	.28	36

Note: Values in the cells are conditional probabilities. Probabilities provided only for working recall periods.

Table 11. Negative Binomial Models of Street Crime and Higher Job Quality

	Model 1: Pooled[1]		Model 2: Fixed Effects[2]	
Variable	**IRR**	**SE**	**IRR**	**SE**
Work	1.03	0.27	1.07	0.27
Job Quality	0.89*	0.05	0.97	0.05
Hours	1.01	0.00	0.99	0.05
Certainty	0.91***	0.01	0.95***	0.01
Social Cost	0.91*	0.04	1.07	0.05
Personal Rewards	1.12***	0.02	1.09***	0.02
Social Rewards	2.17***	0.18	1.64***	0.16
Illegal Earnings ($10,000/yr)	1.20***	0.03	1.04**	0.01
Work Opportunities	0.85**	0.05	0.91	0.06
Financial Responsibility	1.03*	0.02	1.00	0.02
Other Income Sources	1.04	0.07	0.97	0.07
Romantic Relationship	2.66***	0.52	1.28	0.24
Relationship Quality	0.81***	0.05	0.99	0.06
Expecting a Child	1.22†	0.14	1.07	0.12
Number of Children	1.06	0.04	1.05	0.08
Education	0.82**	0.06	0.80	0.14
School	1.09	0.10	1.22*	0.11
Routine Activities	1.45***	0.06	1.35***	0.06
Prior Arrests	1.02	0.02	0.96	0.03
Criminal Record	1.32**	0.12	0.97	0.08
Exposure	3.51***	0.56	1.54**	0.24
Supervision	1.13	0.10	0.85†	0.07
NP (N)	3,297	(1,073)	3,297	(1,073)
			χ^2=155.20	$p \leq .001$

Notes: † $p \leq .10$; $p \leq .05$ *; $p \leq .01$ **; $p \leq .001$ ***; Coefficients are incident rate ratios (IRR) computed (e^B-1) *100; Hypothesis tests are two-tailed. 1: Age, year, race/ethnicity, sex, site, year-site interactions and constants omitted from output. 2: Age, year, year-site interactions and constants omitted from output.

Table 12. Negative Binomial Models of Street Crime and High Job Quality

	Model 3 Pooled[1]		Model 4 Fixed Effects[2]	
Variable	**IRR**	**SE**	**IRR**	**SE**
Work	0.69†	0.14	0.97	0.17
High Job Quality	0.76*	0.09	0.89	0.11
Hours	1.01	0.00	0.99	0.05
Certainty	0.90***	0.01	0.95***	0.01
Social Cost	0.91*	0.04	1.07	0.05
Personal Rewards	1.12***	0.02	1.09***	0.02
Social Rewards	2.14***	0.18	1.63***	0.15
Illegal Earnings ($10,000/yr)	1.20***	0.03	1.04**	0.01
Work Opportunities	0.85**	0.05	0.91	0.06
Financial Responsibility	1.03*	0.02	1.00	0.02
Other Income Sources	1.03	0.07	0.97	0.07
Romantic Relationship	2.67***	0.52	1.27	0.23
Relationship Quality	0.81***	0.05	0.99	0.06
Expecting a Child	1.23†	0.15	1.08	0.12
Number of Children	1.05	0.04	1.05	0.08
Education	0.82**	0.06	0.81	0.14
School	1.09	0.10	1.21*	0.11
Routine Activities	1.44***	0.06	1.35***	0.06
Prior arrests	1.02	0.02	0.96	0.03
Criminal Record	1.33***	0.12	0.97	0.08
Exposure	3.47***	0.55	1.54**	0.24
Supervision	1.14	0.10	0.85†	0.07
NP (N)	3,297	(1,073)	3,297	(1,073)
			χ^2=154.08	$p \leq .001$

Notes: † $p \leq .10$; $p \leq .05$ *; $p \leq .01$ **; $p \leq .001$ ***; Coefficients are incident rate ratios (IRR) computed (e^B-1) *100; Hypothesis tests are two-tailed. 1: Age, year, race/ethnicity, sex, site, year-site interactions and constants omitted from output. 2: Age, year, year-site interactions and constants omitted from output.

Table 13. Negative Binomial Models of Street Crime and Job Quality: Within Low Quality

	Model 5: Pooled[1]		Model 6: Fixed Effects[2]	
Variable	**IRR**	**SE**	**IRR**	**SE**
Work	1.21	0.38	1.17	0.37
Job Quality	0.89	0.07	0.98	0.08
Hours	1.02	0.06	0.97	0.05
Certainty	0.91***	0.01	0.95**	0.02
Social Cost	0.90*	0.04	1.06	0.05
Personal Rewards	1.13***	0.02	1.09***	0.02
Social Rewards	2.08***	0.18	1.55***	0.16
Illegal Earnings ($10,000/yr)	1.18***	0.03	1.05***	0.01
Work Opportunities	0.87*	0.05	0.93	0.07
Financial Responsibility	1.02	0.02	0.99	0.02
Other Income Sources	1.04	0.07	0.98	0.07
Romantic Relationship	2.61***	0.54	1.36	0.27
Relationship Quality	0.82**	0.05	0.98	0.06
Expecting a Child	1.15	0.15	1.06	0.13
Number of Children	1.05	0.04	1.05	0.08
Education	0.91	0.07	0.83	0.15
School	1.08	0.10	1.22*	0.12
Routine Activities	1.45***	0.06	1.34***	0.07
Prior arrests	1.01	0.02	0.97	0.03
Criminal Record	1.32**	0.13	0.97	0.09
Exposure	3.48***	0.57	1.46*	0.24
Supervision	1.10	0.10	0.82*	0.08
NP (N)	2,699	(1,012)	2,699	(1,012)
			χ^2=134.94	p≤.001

Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed (e^B-1) *100; Hypothesis tests are two-tailed. 1: Age, year, race/ethnicity, sex, site, year-site interactions and constants omitted from output. 2: Age, year, year-site interactions and constants omitted from output.

Table 14. Percentage of Working Committing Each Workplace Crime by Wave

	Wave			
	7	**8**	**9**	**10**
Stole Money	3	4	4	1
Stole Things	7	7	7	5
False Hours	7	7	6	2
N	623	614	603	526

Table 15: Negative Binomial Models of Workplace Crime and Job Quality

	Model 7 Higher Job Quality		Model 8 High Job Quality		Model 9 Higher Job Quality: Within Low	
Variable	**IRR**	**SE**	**IRR**	**SE**	**IRR**	**SE**
Job Quality	0.86†	0.08	--	--	0.83	0.11
High Job Quality	--	--	0.78	0.16	--	--
Hours	1.19*	0.09	1.18*	0.09	1.14	0.11
Certainty	0.98	0.03	0.98	0.03	0.97	0.04
Social Cost	0.82*	0.08	0.82*	0.08	0.92	0.10
Personal Rewards	1.10**	0.04	1.10**	0.04	1.11*	0.05
Social Rewards	1.55*	0.29	1.52*	0.28	1.62*	0.36
Illegal Earnings ($10,000/yr)	1.04	0.06	1.05	0.06	1.04	0.07
Work Opportunities	0.97	0.13	0.96	0.13	0.92	0.14
Financial Responsibility	1.11**	0.04	1.10**	0.04	1.12**	0.05
Other Income Sources	1.46**	0.20	1.45**	0.20	1.60**	0.26
Romantic Relationship	2.26*	0.84	2.27*	0.85	1.96	0.84
Relationship Quality	0.76*	0.09	0.76*	0.09	0.72*	0.10
Expecting a Child	0.83	0.23	0.84	0.24	0.80	0.28
Number of Children	0.85	0.09	0.85†	0.08	0.83	0.10
Education	0.79	0.14	0.79	0.14	0.86	0.19
School	1.25	0.24	1.23	0.24	1.40	0.32
Routine Activities	1.08	0.10	1.08	0.10	1.20	0.14
Prior arrests	0.96	0.04	0.96	0.04	1.00	0.05
Criminal Record	1.23	0.26	1.24	0.26	1.19	0.29
Exposure	0.70	0.35	0.71	0.35	0.49	0.29
Supervision	1.21	0.25	1.20	0.25	1.09	0.27
NP (N)	1,979	(830)	1,979	(830)	1,378	(715)
	χ^2=13.74	p=.93	χ^2=14.11	p=.92	χ^2=18.82	p=.65

Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed (e^B-1) *100; Hypothesis tests are two-tailed. Age, year, race/ethnicity, sex, site, year-site interactions and constants omitted from output.

Table 16. Percentage of Working Within Overlap Categories by Wave

	Wave			
	7	**8**	**9**	**10**
Street Crime Only	56	52	46	62
Work Crime Only	33	32	35	20
Both (Street & Work Crime)	12	17	19	18
N	153	132	104	82

Table 17. MLogit of Street-Workplace Overlap and Higher Job Quality

Reference Category: Street Crime Only	No Crime		Workplace Crime Only		Both (Street and Workplace)	
Variable	**RRR**	**SE**	**RRR**	**SE**	**RRR**	**SE**
Job Quality	0.91	0.11	0.82	0.15	0.71†	0.20
Hours	0.97	0.10	1.07	0.13	1.37†	0.18
Certainty	1.10**	0.04	1.07	0.05	0.97	0.07
Social Cost	0.92	0.12	0.78†	0.15	0.76	0.21
Personal Rewards	0.90*	0.04	0.97	0.06	1.14†	0.07
Social Rewards	0.45***	0.22	0.55*	0.29	2.83	0.44
Illegal Earnings ($10,000/yr)	0.75***	0.08	0.93	0.09	0.79	0.19
Work Opportunities	1.47*	0.16	1.37	0.21	1.22	0.29
Financial Responsibility	1.06	0.04	1.12*	0.06	1.31**	0.08
Other Income Sources	0.68*	0.19	1.24	0.24	0.76	0.36
Romantic Relationship	0.24**	0.48	0.66	0.63	0.73	0.85
Relationship Quality	1.55**	0.15	1.11	0.19	1.09	0.26
Expecting a Child	0.43**	0.28	0.22**	0.51	0.99	0.49
Number of Children	0.96	0.11	0.82	0.16	0.74	0.22
Education	1.43	0.22	0.94	0.29	1.02	0.38
School	1.06	0.24	1.14	0.33	2.06†	0.43
Routine Activities	0.63***	0.11	0.69*	0.16	0.87	0.21
Prior arrests	0.89*	0.04	0.89	0.07	0.84†	0.09
Criminal Record	0.40***	0.23	0.42*	0.34	1.11	0.40
Exposure	0.40†	0.52	0.08**	0.93	3.38	0.94
Supervision	0.96	0.24	1.57	0.33	0.63	0.46
NP (N)	1,965	(818)				

Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are relative rate ratios (RRR) computed by exponentiating the coefficients; Age, sex, race/ethnicity, site, year, year-site interactions, and constants omitted from output. Hypothesis tests are two-tailed.

Table 18. MLogit of Overlap and Higher Job Quality: Part 2

Reference Category: Workplace Crime Only						
	No Crime		**Street Crime Only**		**Both (Street and Workplace)**	
Variable	**RRR**	**SE**	**RRR**	**SE**	**RRR**	**SE**
Job Quality	1.11	0.13	1.20	0.17	0.85	0.23
Reference Category: No Crime						
	Street Crime Only		**Workplace Crime Only**		**Both (Street and Workplace)**	
Variable	**RRR**	**SE**	**RRR**	**SE**	**RRR**	**SE**
Job Quality	1.11	0.12	0.90	0.12	0.80	0.19
NP (N)	1,965	(818)				

Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficient is relative rate ratios (RRR) computed by exponentiating the coefficient; Only key covariate included in output. Hypothesis tests are two-tailed.

Table 19. MLogit of Street-Workplace Overlap and High Job Quality

Reference Category: Street Crime Only	No Crime		Workplace Crime Only		Both (Street and Workplace)	
Variable	**RRR**	**SE**	**RRR**	**SE**	**RRR**	**SE**
High Job Quality	0.78	0.23	0.79	0.31	0.31*	0.48
Hours	0.98	0.10	1.06	0.13	1.39†	0.18
Certainty	1.10*	0.04	1.07	0.05	0.97	0.07
Social Cost	0.92	0.12	0.77†	0.15	0.76	0.21
Personal Rewards	0.91*	0.04	0.98	0.06	1.14†	0.07
Social Rewards	0.43***	0.22	0.54*	0.30	2.54*	0.44
Illegal Earnings ($10,000/yr)	0.76***	0.08	0.93	0.09	0.80	0.19
Work Opportunities	1.46*	0.15	1.34	0.21	1.22	0.29
Financial Responsibility	1.06	0.04	1.12†	0.06	1.31***	0.08
Other Income Sources	0.67*	0.19	1.24	0.24	0.73	0.36
Romantic Relationship	0.24**	0.48	0.7	0.63	0.68	0.85
Relationship Quality	1.54**	0.14	1.09	0.19	1.1	0.26
Expecting a Child	0.44**	0.28	0.22**	0.51	1.06	0.49
Number of Children	0.95	0.11	0.81	0.15	0.71	0.22
Education	1.43†	0.22	0.92	0.29	1.01	0.38
School	1.06	0.24	1.13	0.33	2.03†	0.42
Routine Activities	0.63***	0.11	0.68*	0.15	0.85	0.21
Prior arrests	0.89*	0.04	0.89†	0.07	0.84†	0.09
Criminal Record	0.41***	0.23	0.44*	0.34	1.11	0.40
Exposure	0.40†	0.52	0.08**	0.93	3.27	0.94
Supervision	0.96	0.24	1.56	0.33	0.64	0.46
NP (N)	1,965	(818)				

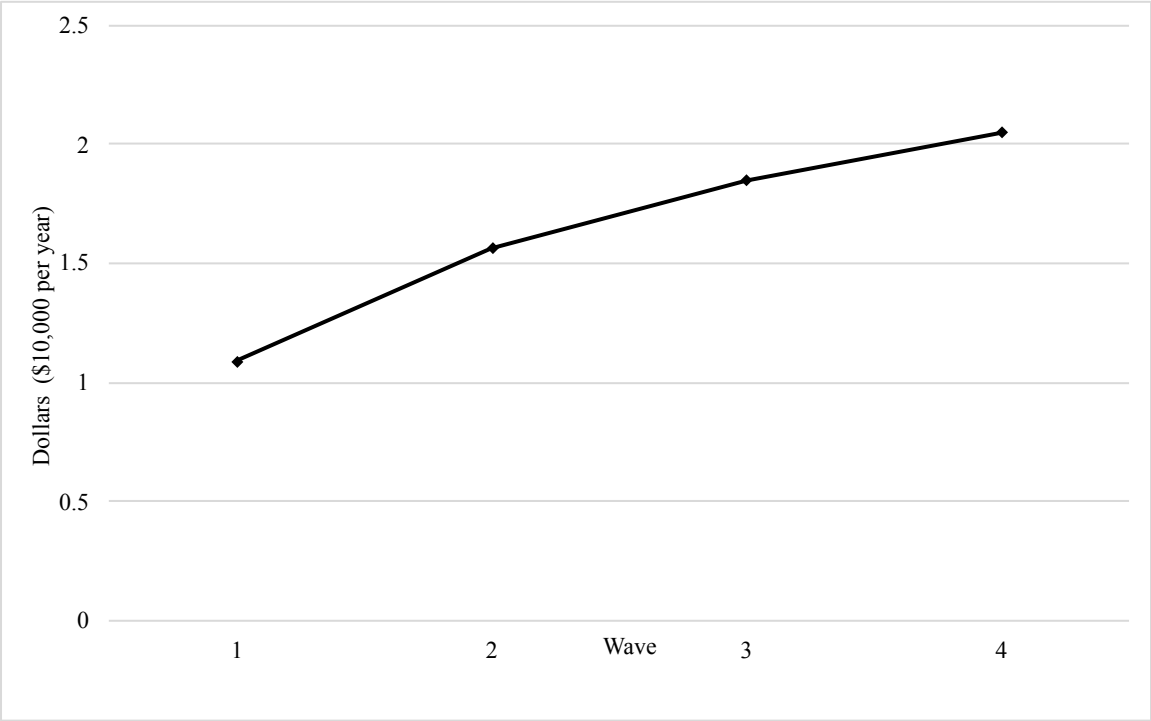
Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are relative rate ratios (RRR) computed by exponentiating the coefficients; Age, sex, race/ethnicity, year, site, year-site interactions, and constants omitted from output. Hypothesis tests are two-tailed.

Table 20. MLogit of Overlap and High Job Quality: Part 2

Reference Category: Workplace Crime Only						
	No Crime		**Street Crime Only**		**Both (Street and Workplace)**	
Variable	**OR**	**SE**	**OR**	**SE**	**OR**	**SE**
High Job Quality	0.96	0.26	1.25	0.34	0.39†	0.53
Reference Category: No Crime						
	Street Crime Only		**Workplace Crime Only**		**Both (Street and Workplace)**	
Variable	**OR**	**SE**	**OR**	**SE**	**OR**	**SE**
High Job Quality	1.33	0.24	1.01	0.24	0.42†	0.45
NP (N)	1,965	(818)				

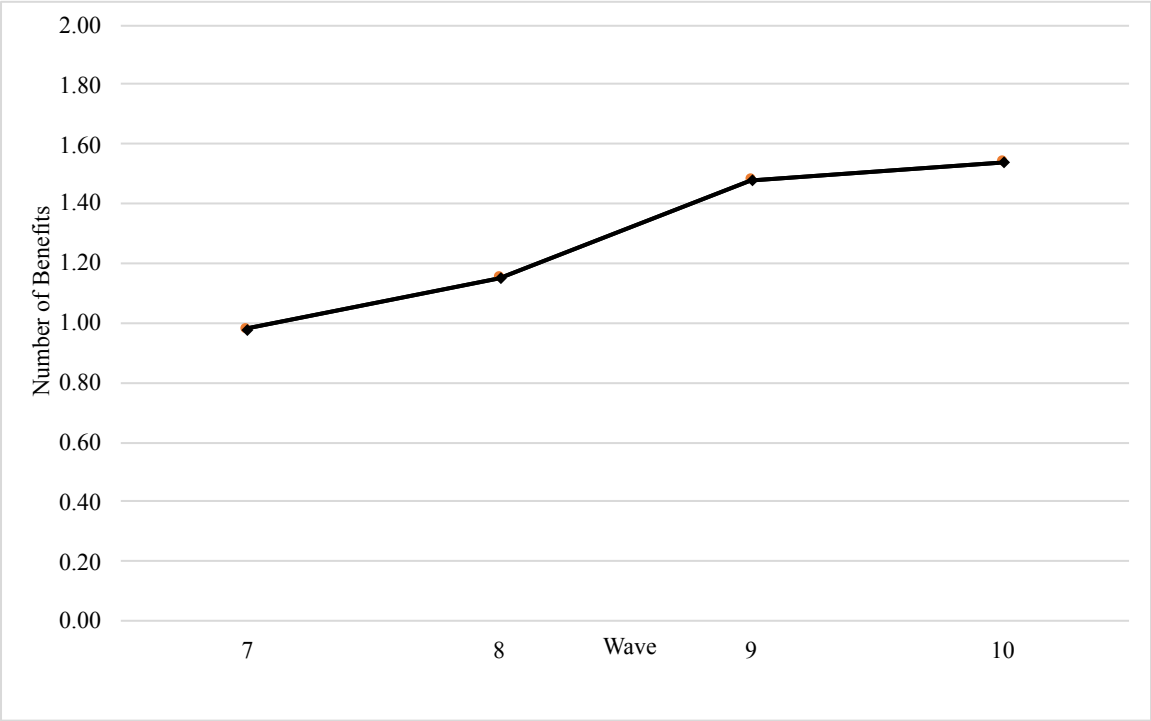
Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are relative rate ratios (RRR) computed by exponentiating the coefficients; Only key covariates included in output. Hypothesis tests are two-tailed.

Figure 1. Mean Income Across Waves



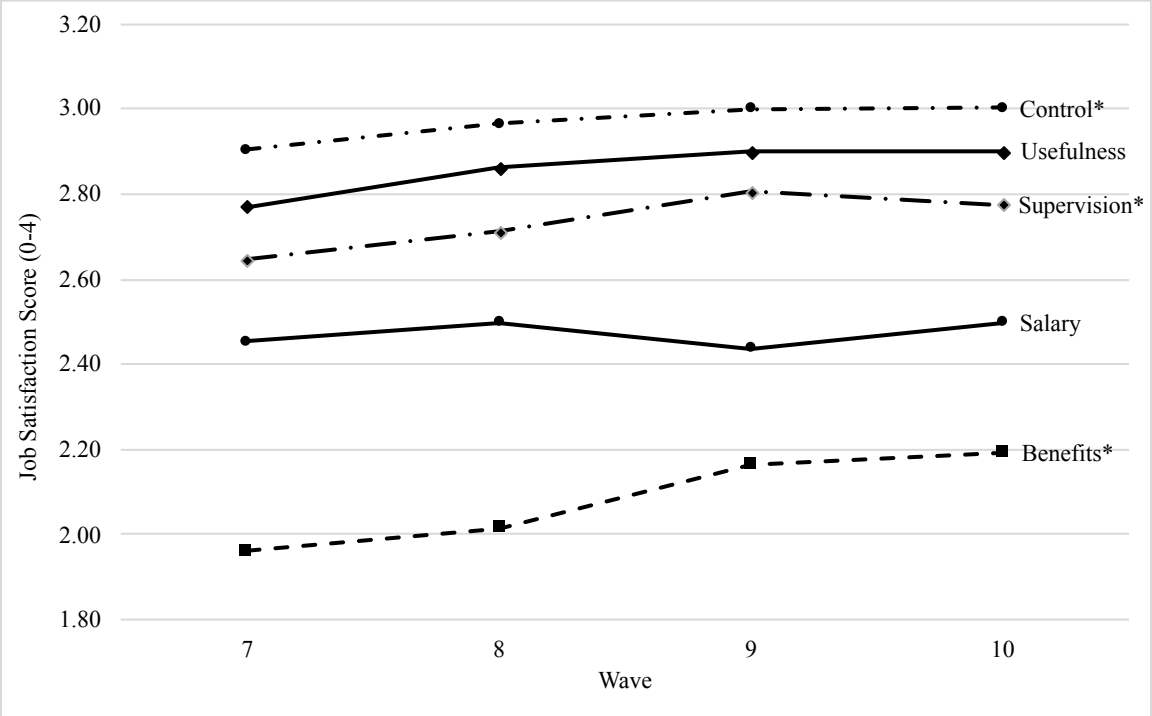
Means provided for only working recall periods
F=47.92, $p \leq .001$
SM=157.60, $p \leq .001$

Figure 2. Mean Benefits Across Waves



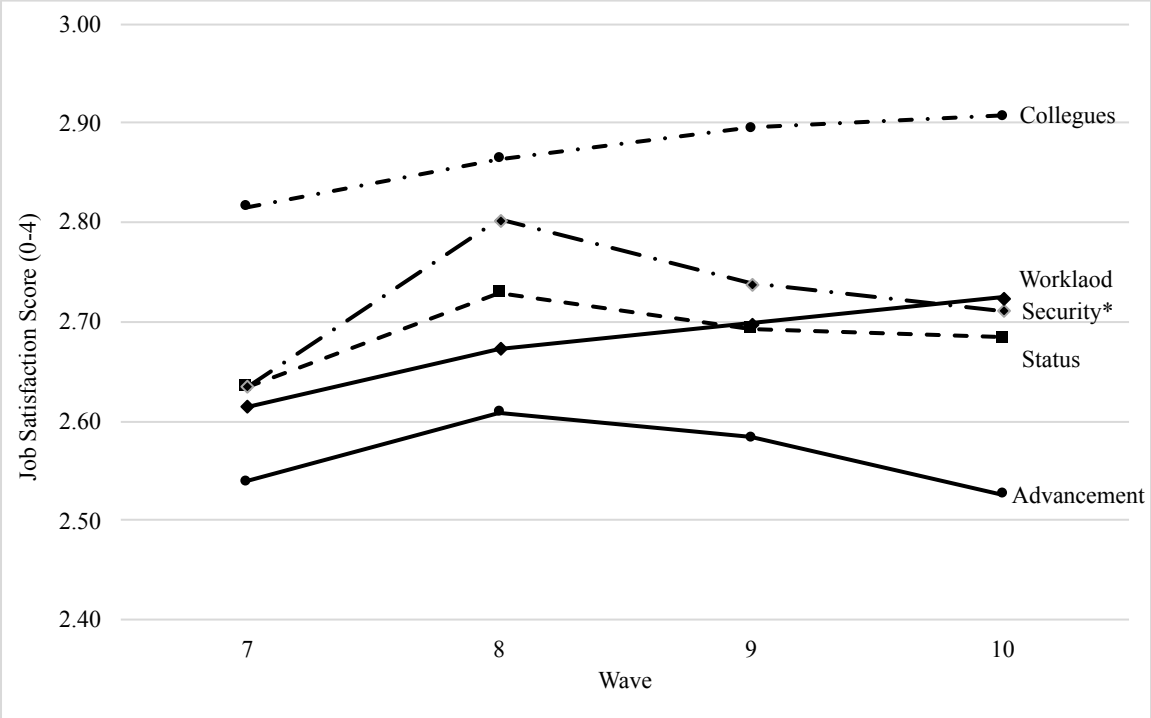
Means provided for only working recall periods
F=20.59, $p \leq .001$
SM=35.47, $p \leq .001$

Figure 3. Mean of Quality Dimensions Measured on Satisfaction Scale by Wave (Part 1)



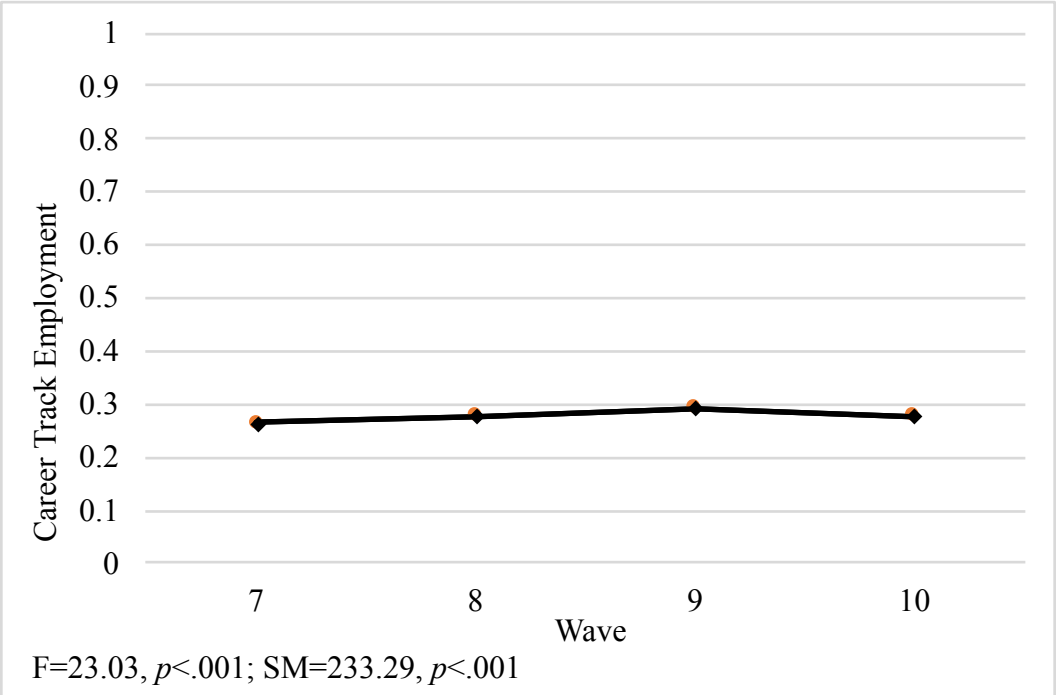
Note: Means provided for only working recall periods
*Denotes significant evidence ($p \leq .05$) of mean or distribution differences between waves
Salary: F=0.68, p=0.56; SM=0.72, p=.87
Benefits: F=6.55, $p \leq .001$; SM=15.92, $p \leq .01$
Supervision: F=5.68, $p \leq .001$; SM=11.69, $p \leq .01$
Control: F=2.08, $p \leq .10$; SM=8.76, $p \leq .05$
Usefulness: F=2.24, $p \leq .10$; SM=2.68, p=.44

Figure 4. Mean of Quality Dimensions Measure with Satisfaction Scale by Wave Continued



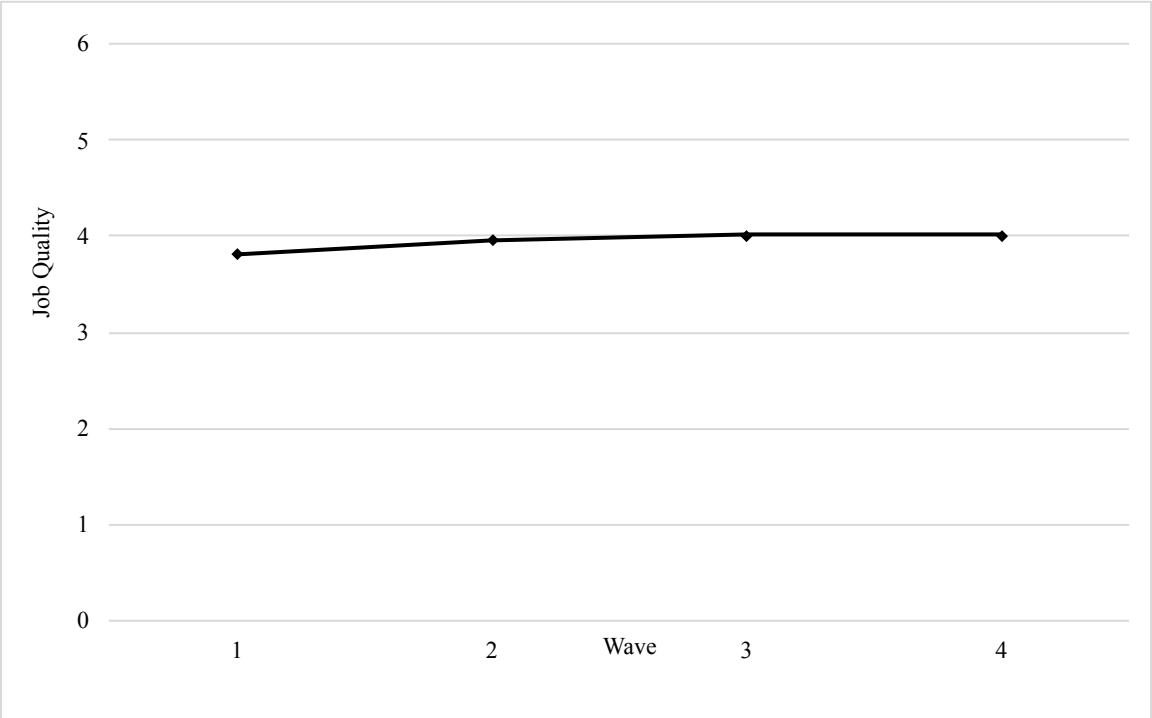
Means provided for only working recall periods
*Denotes significant evidence of mean or distributional differences between waves
Advancement: F=0.76, *p*=.52; SM=1.40, *p*=.71
Status: F=0.85, *p*=.47; SM=0.78, *p*=.43
Security: F=0.67, *p*≤.05; SM=0.79, *p*≤.01
Colleagues: F=2.00, *p*=.11; SM=3.96, *p*=.27
Workload: F=0.82, *p*=.48; SM=3.04, *p*=.39

Figure 5. Mean Career Track Employment by Wave



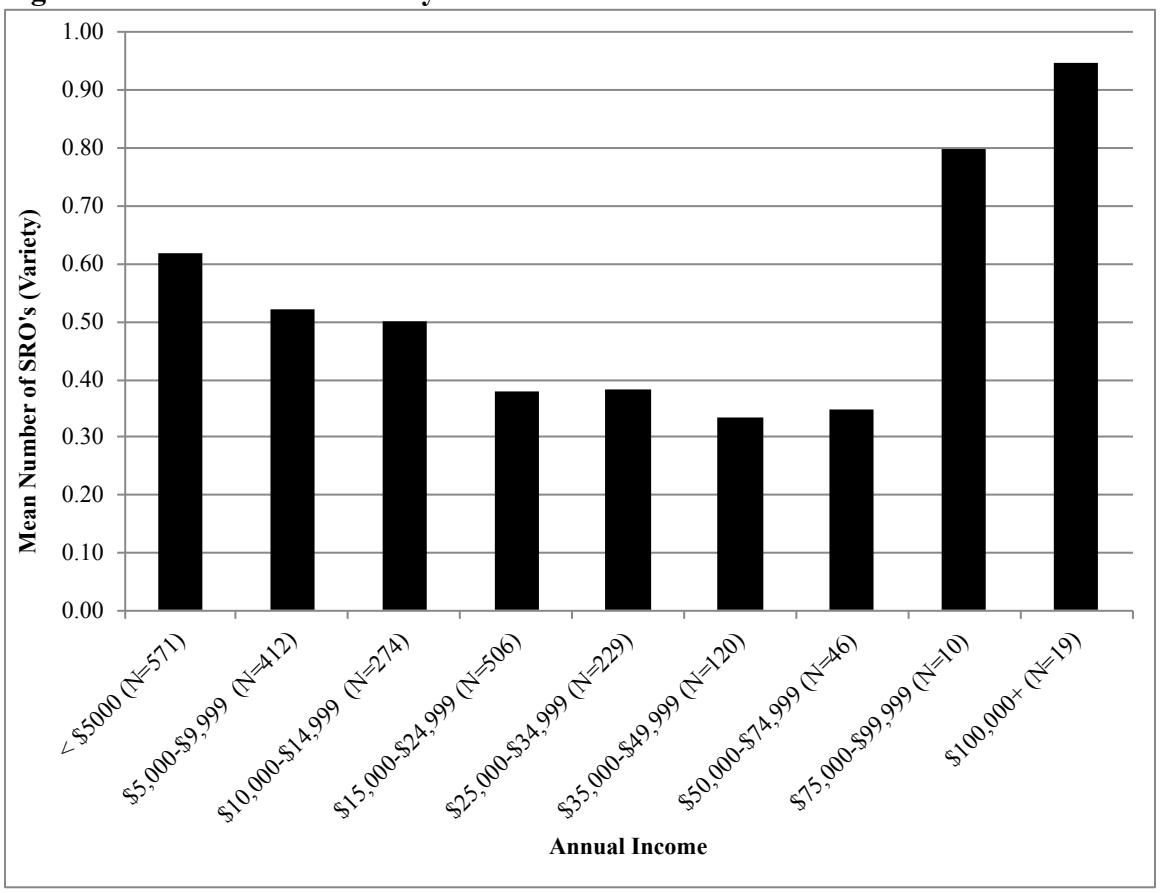
Means provided for only working recall periods
F: 3.21, $p \leq .05$
SM=7.01, p=.07

Figure 6. Mean of Job Quality by Wave



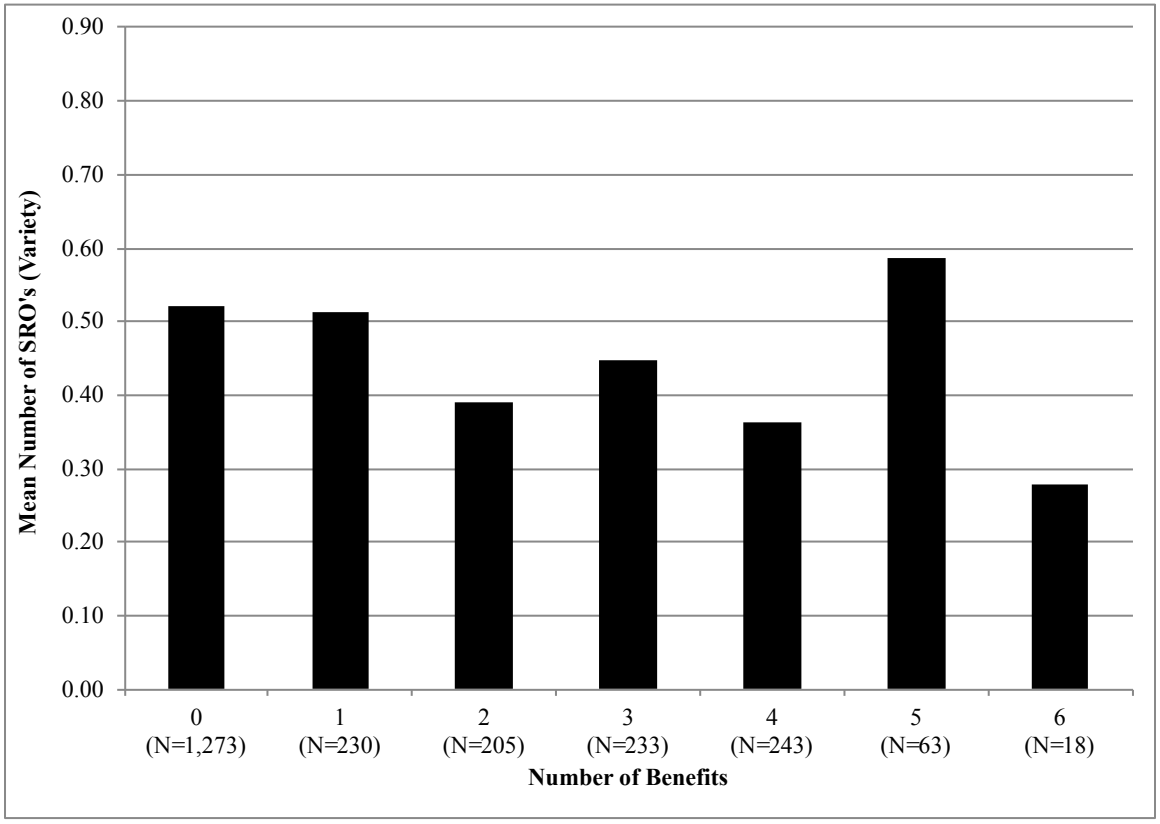
Means provided for only working recall periods;
F=4.69, $p \leq .01$;
SM=13.63, $p \leq .01$

Figure 7. Mean Street Crime by Income



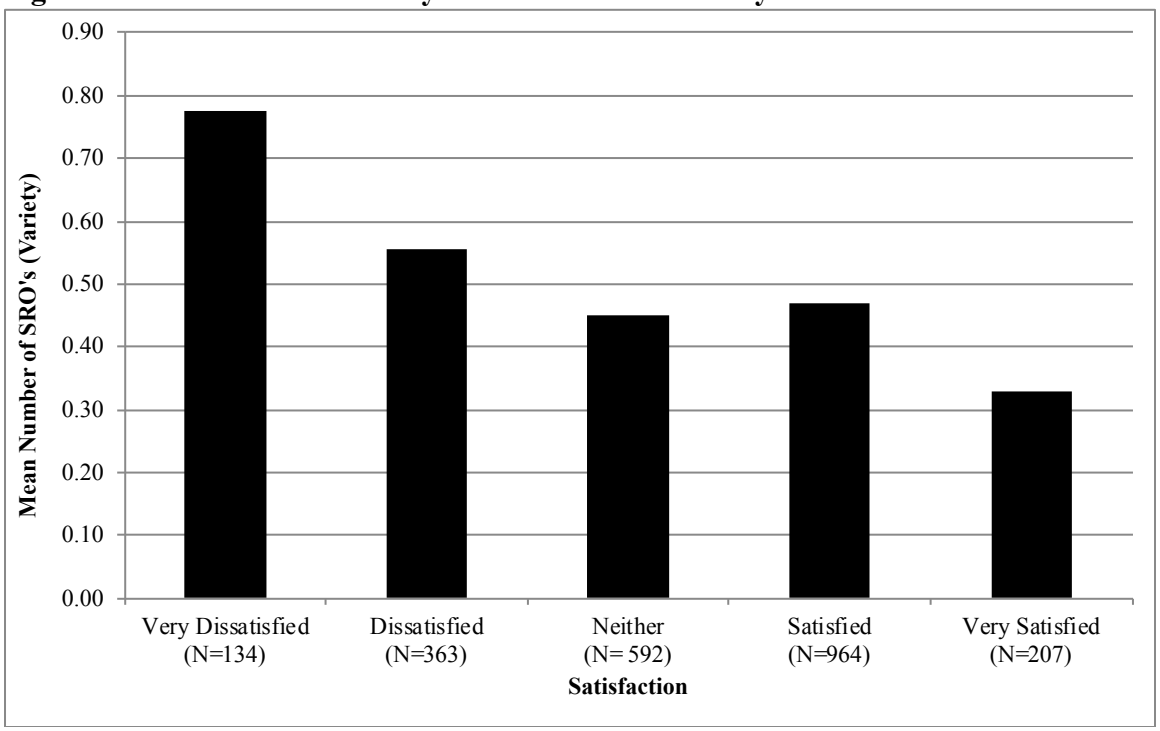
NP=2,185
F=2.21, $p \leq .05$
KW=19.58, $p \leq .05$
IRR=0.93, $p \leq .05$

Figure 8. Mean Street Crime by Benefits



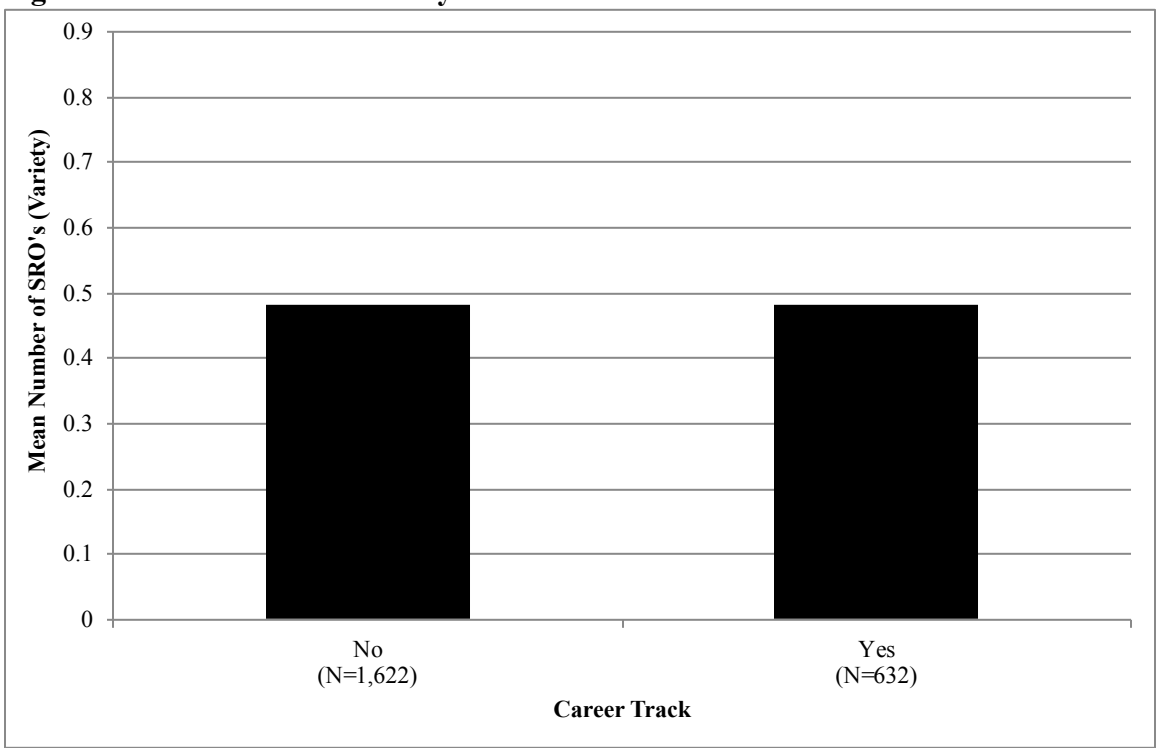
NP=2,255
F=0.98, *p*=.43
KW=3.47, *p*=.35
IRR=0.94, *p*=.10

Figure 9. Mean Street Crime by Satisfaction with Salary



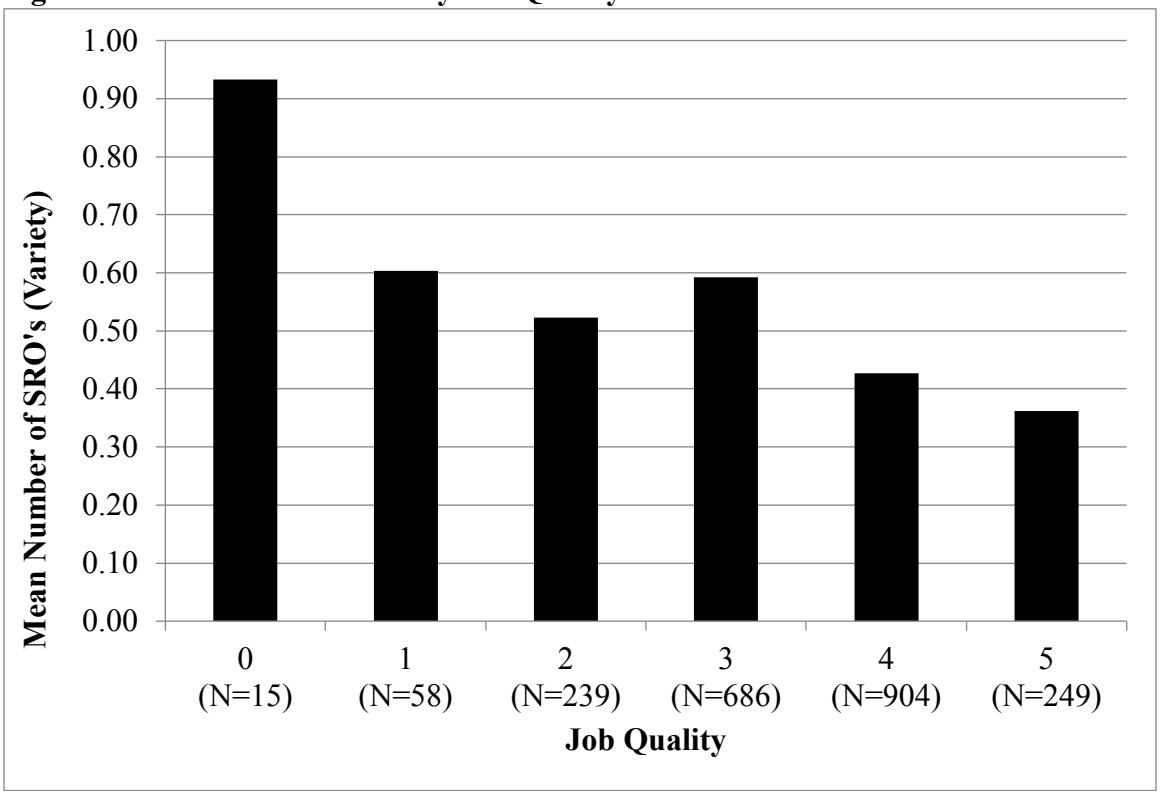
NP=2,260
F=3.14, $p \leq .01$
KW=13.99, $p \leq .01$
IRR=0.86, $p \leq .01$

Figure 10. Mean Street Crime by Career Track



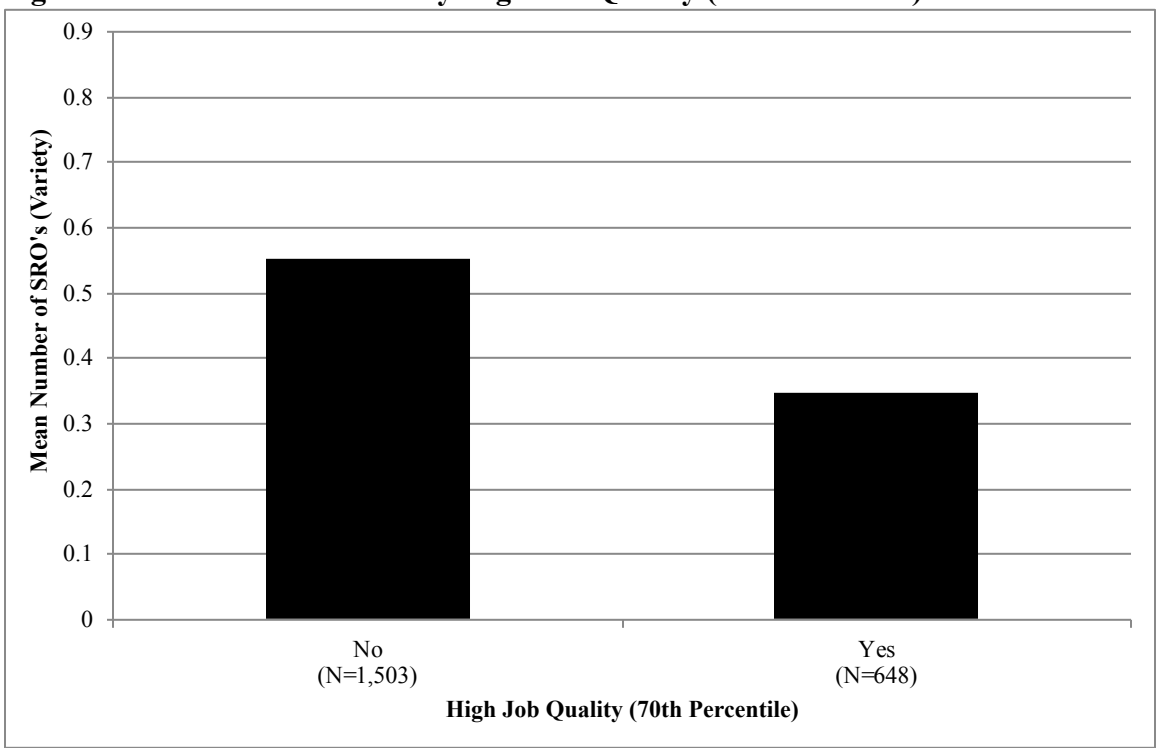
NP=2,254
T=0.02, *p*=.49
MW=-0.27, *p*=.79
IRR=1.00, *p*=.98

Figure 11. Mean Street Crime by Job Quality



NP=2,151
F=2.39, $p \leq .05$
KW=15.21, $p \leq .01$
IRR=0.85, $p \leq .01$

Figure 12. Mean Street Crime by High Job Quality (70th Percentile)



NP=2,151
T=3.50, $p \leq .001$
MW=3.57, $p \leq .001$
IRR=0.63, $p \leq .001$

Figure 13. Workplace Crime Conditional Probabilities: By Type

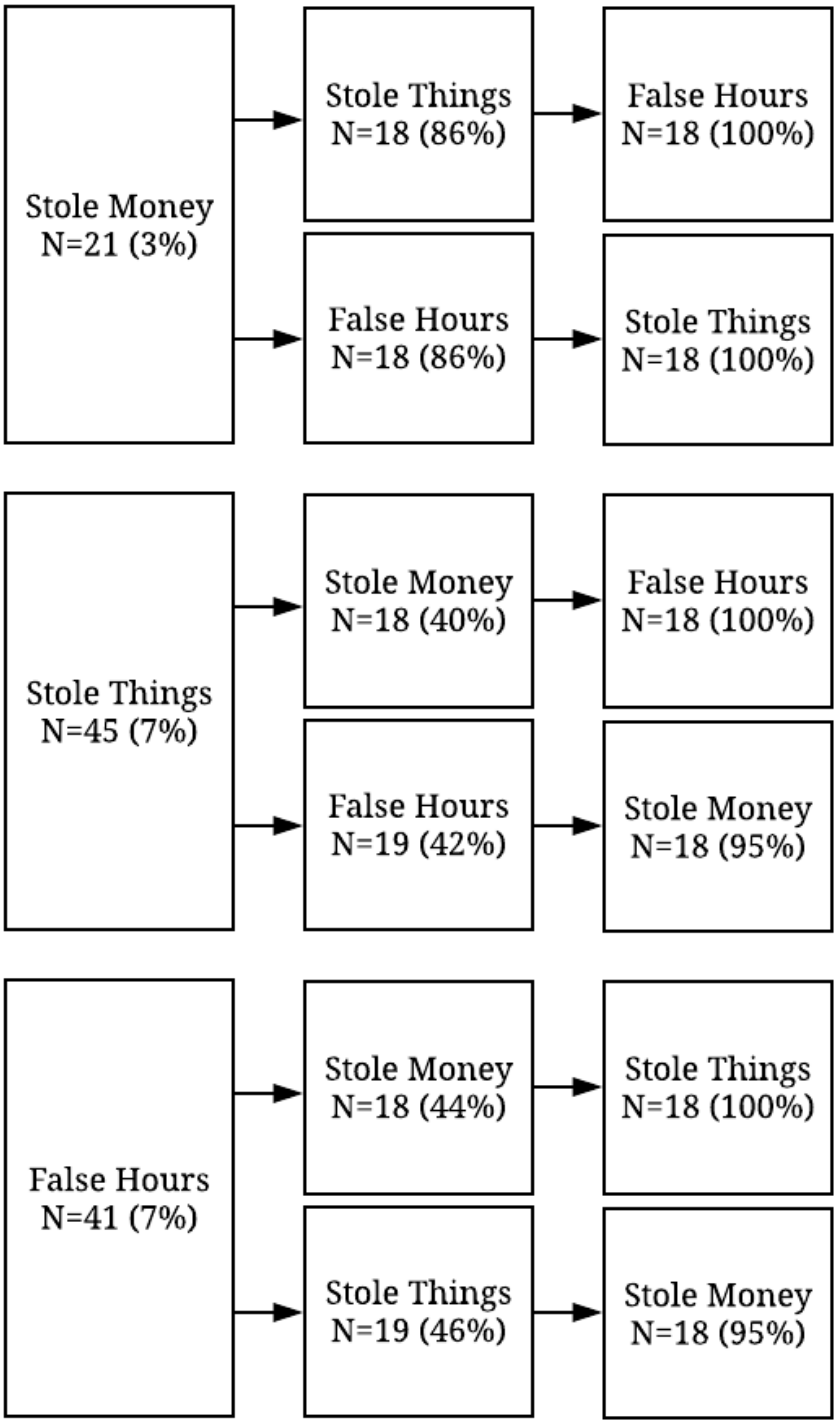
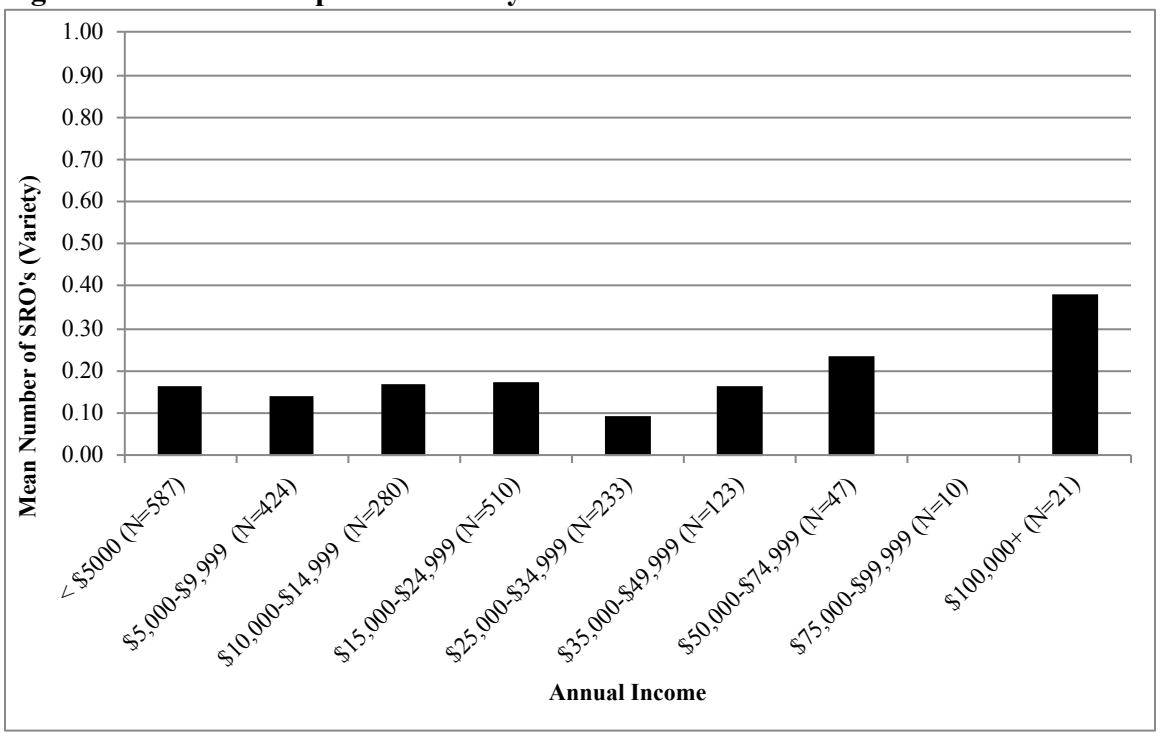
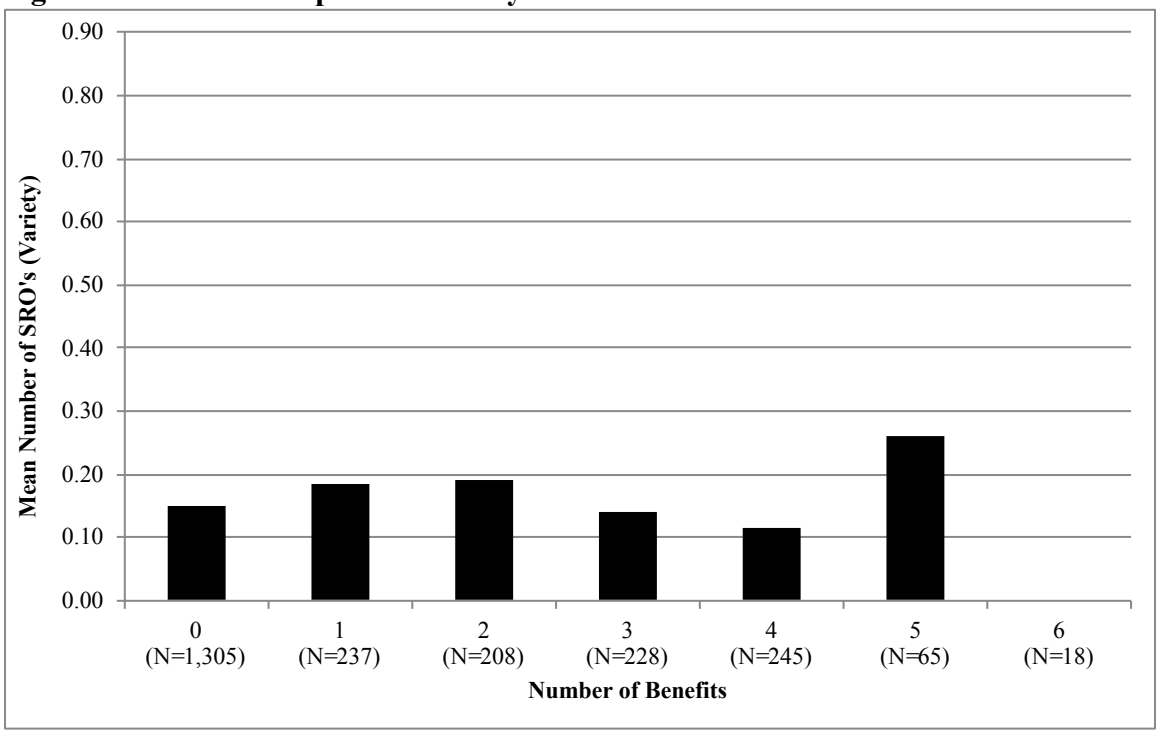


Figure 14. Mean Workplace Crime by Income



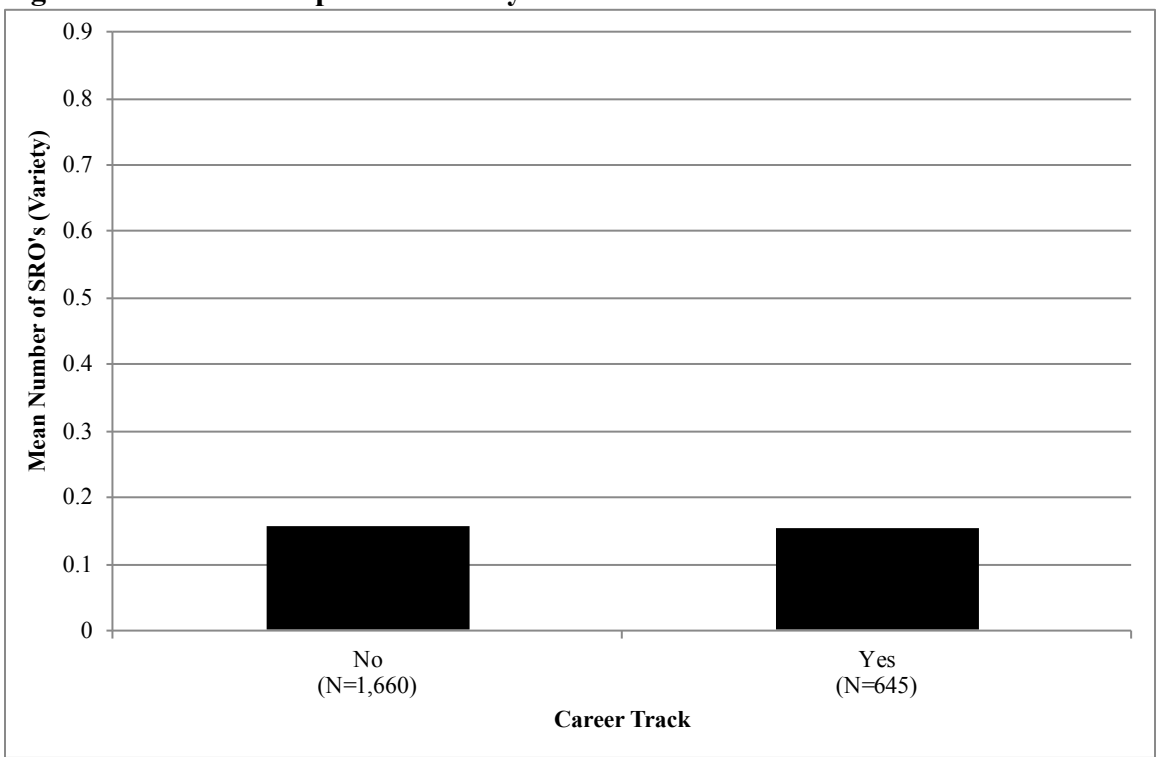
NP=2,235
F=1.15, *p*=.33
KW=7.06, *p*=.53
IRR=1.01, *p*=.76

Figure 15. Mean Workplace Crime by Benefits



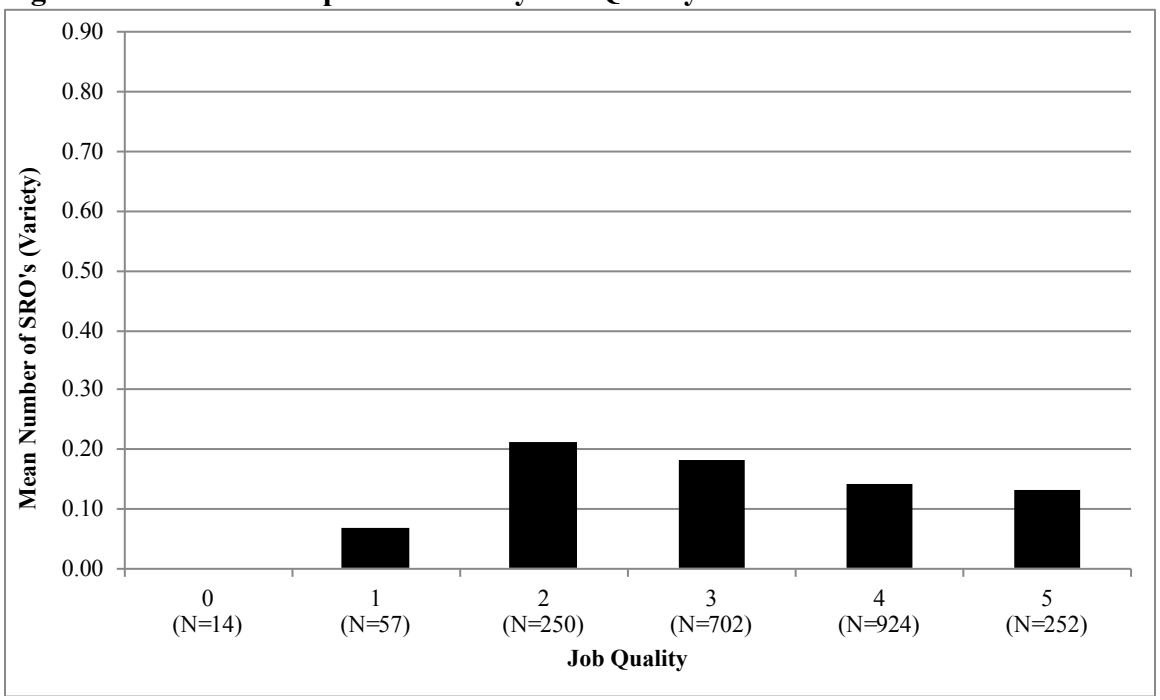
NP=2,306
F=1.20, *p*=.36
KW=7.96, *p*=.24
IRR=0.99, *p*=.84

Figure 16. Mean Workplace Crime by Career Track



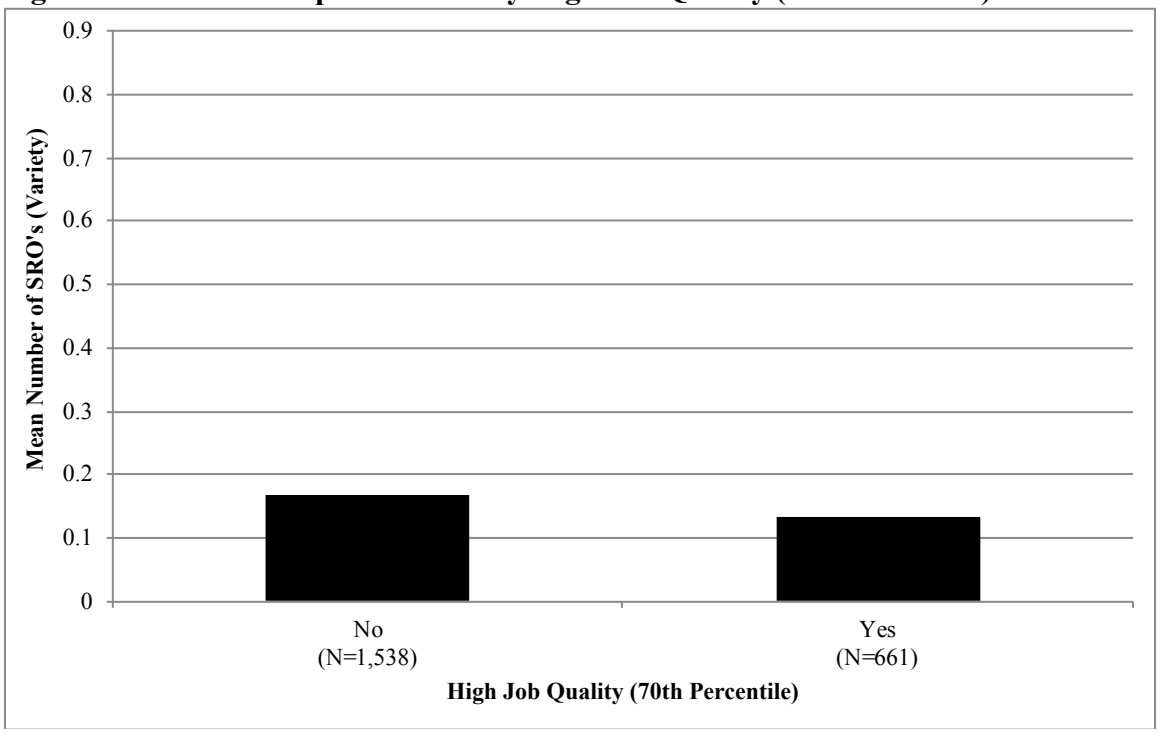
NP=2,305
T=0.17, *p*=.17
MW=0.14, *p*=.89
IRR=0.97, *p*=.87

Figure 17. Mean Workplace Crime by Job Quality



NP=2,199
F=1.52, p=.18
KW=1.11, $p \leq .05$
IRR=0.92, p=.28

Figure 18. Mean Workplace Crime by High Job Quality (70th Percentile)



NP=2,199
T=1.36, p=.17
MW=2.22, $p \leq .05$
IRR=0.79, p=.21

APPENDIX

Table 1A. Attrition Analysis for Analytic Sample

Variable	OR	SE
Age at Baseline	1.30**	0.14
Male	3.02*	1.60
Black	1.16	0.45
Hispanic	1.11	0.45
Other	2.07	1.20
Age at first prior	0.96	0.07
Age at First SRO	1.03	0.07
Baseline SRO Frequency	1.00	0.00
Study Site (Philadelphia)	3.22**	1.06
Year 2001	0.67	0.33
Year 2002	0.48	0.25
Constant	0.00	0.00
N=1,343		

$* \, p \leq .05, ** \, p \leq .01$

Table 2A. High Job Quality Threshold Sensitivity Check

	70th		90th		80th		60th		50th	
	IRR	S.E.	IRR	S.E.	IRR	S.E.	IRR	S.E.	IRR	S.E.
Model 4										
Work	0.97	0.17	0.96	0.16	0.96	0.17	0.97	0.17	0.98	0.17
High Job Quality	0.89	0.11	1.10	0.23	0.96	0.14	0.97	0.11	0.94	0.10
NP (N)	3,297	(1,073)								
Model 8										
High Job Quality	0.78	0.16	0.79	0.25	0.75	0.18	0.72†	0.13	0.72†	0.12
NP (N)	1,979	(830)								

Note: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed by (e^B-1) *100; Only treatment coefficients included in output. Hypothesis tests are two-tailed.

Table 3A. Unconditional Negative Binomial Fixed Effects Models

	Model 2 Higher		Model 4 High Quality[1]		Model 6 Higher (Within Low)[1]	
	IRR	SE	IRR	SE	IRR	SE
Work	1.11	0.24	1.01	0.15	1.13	0.25
Job Quality	0.96	0.05	--	--	0.96	0.05
High Job Quality (70th)	--	--	0.83	0.10	--	--
NP (N)	3,299	(1,073)				

Note: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed by (e^B-1) *100; Only treatment coefficients included in output. Hypothesis tests are two-tailed. 1: Work opportunities omitted to facilitate convergence.

Table 4A. Negative Binomial Fixed Effects by Crimes Type

Property Crime						
	Model 2 Higher Quality		**Model 4 High Quality**		**Model 6 Higher (Within Low)**	
	IRR	SE	IRR	SE	IRR	SE
Work	1.36	0.44	1.29	0.28	1.02	0.41
Job Quality	0.98	0.07	--	--	1.08	0.11
High Job Quality	--	--	0.80	0.14	--	--
NP (N)	3,301	(1,074)	3,301	(1,074)	2,703	(1,013)
Violent Crime	**Model 2 Higher Quality[1]**		**Model 4 High Quality[2]**		**Model 6 Higher (Within Low)[1]**	
	IRR	SE	IRR	SE	IRR	SE
Work	0.51	0.26	0.71	0.25	0.67	0.42
Job Quality	1.10	0.12	--	--	1.11	0.17
High Job Quality	--	--	0.91	0.23	--	--
NP (N)	3,298	(1,073)	3,298	(1,073)	2,700	(1,012)
Drug Crime	**Model 2 Higher Quality**		**Model 4 High Quality**		**Model 6 Higher (Within Low)**	
	IRR	SE	IRR	SE	IRR	SE
Work	1.30	0.48	0.85	0.22	1.76	0.78
Job Quality	0.88	0.07	--	--	0.85	0.09
High Job Quality	--	--	0.87	0.17	--	--
NP (N)	3,330	(1,101)	3,330	(1,101)	2,729	(1,037)

Note: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed by (e^B-1) *100; Only treatment coefficients included in output. Hypothesis tests are two-tailed. 1: Year-site interactions omitted to facilitate convergence; 2: Year and year-site interactions omitted to facilitate convergence; 2: Conditional Poisson fixed effects model with bootstrapped standard errors implemented to facilitate convergence.

Table 5A. Negative Binomial Fixed Effects for Job Quality and Street Crime: by Supervision

Model 2 Higher	**No Supervision**		**Supervision**	
	IRR	SE	IRR	SE
Work	1.15	0.44	2.23†	1.00
Job Quality	0.96	0.08	0.78*	0.09
NP (N)	2,310	(869)	987	(549)
Model 4 High	**No Supervision**		**Supervision**	
	IRR	SE	IRR	SE
Work	1.01	0.26	1.09	0.35
High Job Quality	0.94	0.11	0.96	0.13
NP (N)	2,310	(869)	987	(549)
Model 6: Higher (Within Low*)*	**No Supervision**		**Supervision**	
	IRR	SE	IRR	SE
Work	1.33	0.68	2.25†	1.14
Job Quality	0.93	0.11	0.78†	0.11
NP (N)	1,850	(811)	849	(501)

Note: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed by (e^B-1) *100; Only treatment coefficients included in output. Hypothesis tests are two-tailed.

Table 6A: Pooled Logistic Regression of Work and Study Covariates: Examining Selection into Work

	Model 19	
Variable	**IRR**	**SE**
Certainty	1.01	0.02
Social Cost	1.04	0.06
Personal Rewards	1.03	0.02
Social Rewards	0.87	0.08
Illegal Earnings ($10,000/yr)	0.87***	0.03
Work Opportunities	1.20**	0.08
Financial Responsibility	1.32***	0.03
Other Income Sources	0.52***	0.04
Romantic Relationship	2.17**	0.56
Relationship Quality	0.93	0.07
Expecting a Child	0.86	0.12
Number of Children	0.81***	0.04
Education	1.70***	0.18
School	1.36**	0.16
Routine Activities	1.00	0.05
Prior arrests	0.91***	0.02
Criminal Record	1.05	0.12
Exposure	0.13***	0.03
Supervision	1.16	0.13
Age	0.95	0.04
Black	0.61**	0.10
Hispanic	0.91	0.13
Other	0.64†	0.17
Male	1.13	0.17
Philadelphia	0.49***	0.11
NP (N)	3,475	(1,107)

Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Year and year-site interactions omitted from output. All hypotheses tests are two-tailed.

Table 7A: Negative Binomial Models of Workplace Crime and Job Quality: Pooled and Fixed Effects

	Model 7 Higher Job Quality		Model 8 High Job Quality		Model 9 Higher Job Quality: Within Secondary Sector	
Variable	**IRR**	**IRR**	**IRR**	**SE**	**IRR**	**SE**
Pooled						
Job Quality	0.91	0.08	--	--	0.87	0.11
High Job Quality	--	--	0.91	.17	--	--
NP (N)	1,979	(830)	1,979	(830)	1,378	(715)
Fixed Effects						
Job Quality	0.89	0.42	--	--	0.81	0.17
High Job Quality	--	--	0.84	0.23	--	--
NP (N)	1,979	(830)	1,979	(830)	1,378	(715)

Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed by (e^B-1) *100; Only key theoretical coefficients included in output. Hypothesis tests are two-tailed.

Table 8A: Duration Included in Models of Job Quality and Workplace Crime

	Model 7 Higher Job Quality		Model 8 High Job Quality		Model 9 Higher Job Quality: Within Secondary Sector	
Variable	**IRR**	**IRR**	**IRR**	**SE**	**IRR**	**SE**
Job Quality	0.84†	0.08	--	--	0.81	0.11
High Job Quality	--	--	0.75	0.15	--	--
Duration	1.76†	0.54	1.72†	0.52	1.84†	0.66
NP (N)	1,967	(823)	1,967	(823)	1,367	(708)

Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed by (e^B-1) *100; Only key theoretical coefficients included in output. Hypothesis tests are two-tailed.

Table 9A. Negative Binomial Random Effects for Job Quality and Workplace Crime: by Supervision

Model 2: Higher Job Quality	**No Supervision**		**Supervision**	
	IRR	SE	IRR	SE
Job Quality	0.86	0.10	0.87	0.16
NP (N)	1,473	(647)	506	(335)
Model 4: High Job Quality	**No Supervision**		**Supervision**	
	IRR	SE	IRR	SE
High Job Quality	0.65†	0.16	1.39	0.53
NP (N)	1,473	(647)	506	(335)
Model 6: Higher Job Quality (Within Low*)*	**No Supervision**		**Supervision**	
	IRR	SE	IRR	SE
Job Quality	0.89	0.15	0.76	0.16
NP (N)	1,012	(550)	366	(272)

Note: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are incident rate ratios (IRR) computed by (e^B-1) *100; Only treatment coefficients included in output. Hypothesis tests are two-tailed.

Table 10A: Random Effects Negative Binomial Models Comparing Workplace Theft to Street Theft

	Model 7 Higher Job Quality Workplace Crime		Higher Job Quality Street Property Crime		Higher Job Quality Street Theft	
Variable	**IRR**	**IRR**	**IRR**	**SE**	**IRR**	**SE**
Job Quality	0.86†	0.08	0.98	0.07	0.90	0.10
High Job Quality	--	--	--	--	--	--
Hours	1.19*	0.09	1.09	0.07	1.07	0.10
Certainty	0.98	0.03	0.91***	0.02	0.87***	0.04
Social Cost	0.82*	0.08	1.08***	0.09	1.02	0.13
Personal Rewards	1.10**	0.04	1.12***	0.03	1.16***	0.05
Social Rewards	1.55*	0.29	2.23***	0.35	2.30***	0.57
Illegal Earnings ($10,000/yr)	1.04	0.06	1.17***	0.05	1.10	0.09
Work Opportunities	0.97	0.13	0.84	0.09	0.92	0.15
Financial Responsibility	1.11**	0.04	0.98	0.03	0.93	0.04
Other Income Sources	1.46**	0.20	1.51***	0.18	1.70**	0.31
Romantic Relationship	2.26*	0.84	2.00*	0.63	1.65	0.85
Relationship Quality	0.76*	0.09	0.81*	0.08	0.88	0.14
Expecting a Child	0.83	0.23	1.39†	0.24	1.53	0.44
Number of Children	0.85	0.09	0.98	0.08	0.89	0.12
Education	0.79	0.14	0.73*	0.12	0.70	0.16
School	1.25	0.24	1.18	0.18	1.24	0.29
Routine Activities	1.08	0.10	1.35***	0.10	1.42**	0.17
Prior arrests	0.96	0.04	1.06†	0.03	1.02	0.04
Criminal Record	1.23	0.26	1.36*	0.20	1.62*	0.37
Exposure	0.70	0.35	3.05***	0.95	10.70***	5.07
Supervision	1.21	0.25	0.93	0.14	0.73	0.19
NP (N)	1,979	(830)	1,968	(818)	1,982	(830)

Notes: † $p \leq .10$; $p \leq .05$ *; $p \leq .01$ **; $p \leq .001$ ***; Coefficients are incident rate ratios (IRR) computed (e^B-1) *100; Hypothesis tests are two-tailed. Age, year, race/ethnicity, sex, site, year-site interactions and constants omitted from output.

Table 11A. MLogit of Street-Workplace Crime Overlap and Job Quality: Model 10 with Pooled and Fixed Effects

Pooled

	No Crime		Street Crime Only		Both (Street and Workplace)	
Variable	**OR**	**SE**	**OR**	**SE**	**OR**	**SE**
Job Quality	0.90	0.09	0.82	0.12	0.72†	0.12
NP (N)	1,965	(818)				

Fixed Effects

	Street Crime Only		Workplace Crime Only		Both (Street and Workplace)	
Variable	**OR**	**SE**	**OR**	**SE**	**OR**	**SE**
Job Quality	0.84	0.21	0.61	0.22	0.86	0.61
NP (N)	544	(176)				

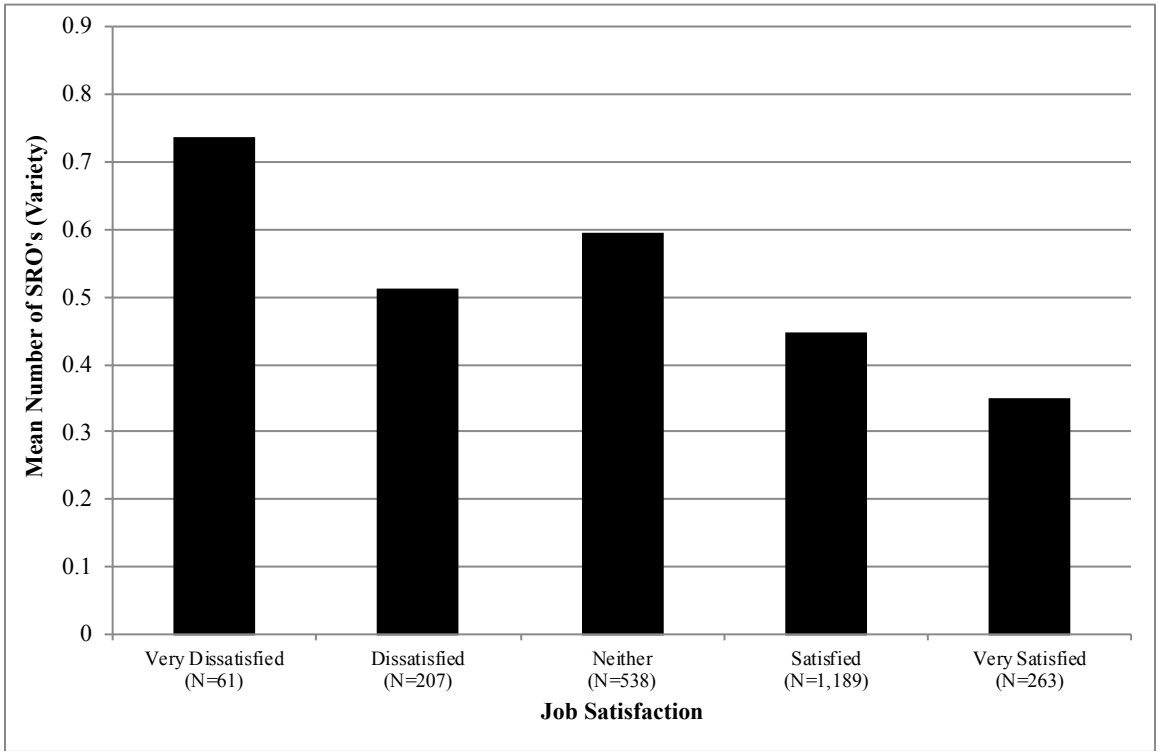
Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are odds ratios (OR) computed by exponentiating the coefficients; Only key covariates included in output. Hypothesis tests are two-tailed. Reference category is street crime only.

Table 12A. MLogit of Street-Workplace Crime Overlap and Job Quality: Model 11 Pooled and Fixed Effects

Pooled	**No Crime**		**Workplace Crime Only**		**Both (Street and Workplace)**	
Variable	**OR**	**SE**	**OR**	**SE**	**OR**	**SE**
High Job Quality	0.76	0.14	0.78	0.23	0.31**	0.14
NP (N)	1,965	(818)				
Fixed Effects						
	No Crime		**Workplace Crime Only**		**Both (Street and Workplace)**	
Variable	**OR**	**SE**	**OR**	**SE**	**OR**	**SE**
High Job Quality	0.64	0.33	0.62	0.44	0.44	0.57
NP (N)	544	(176)				

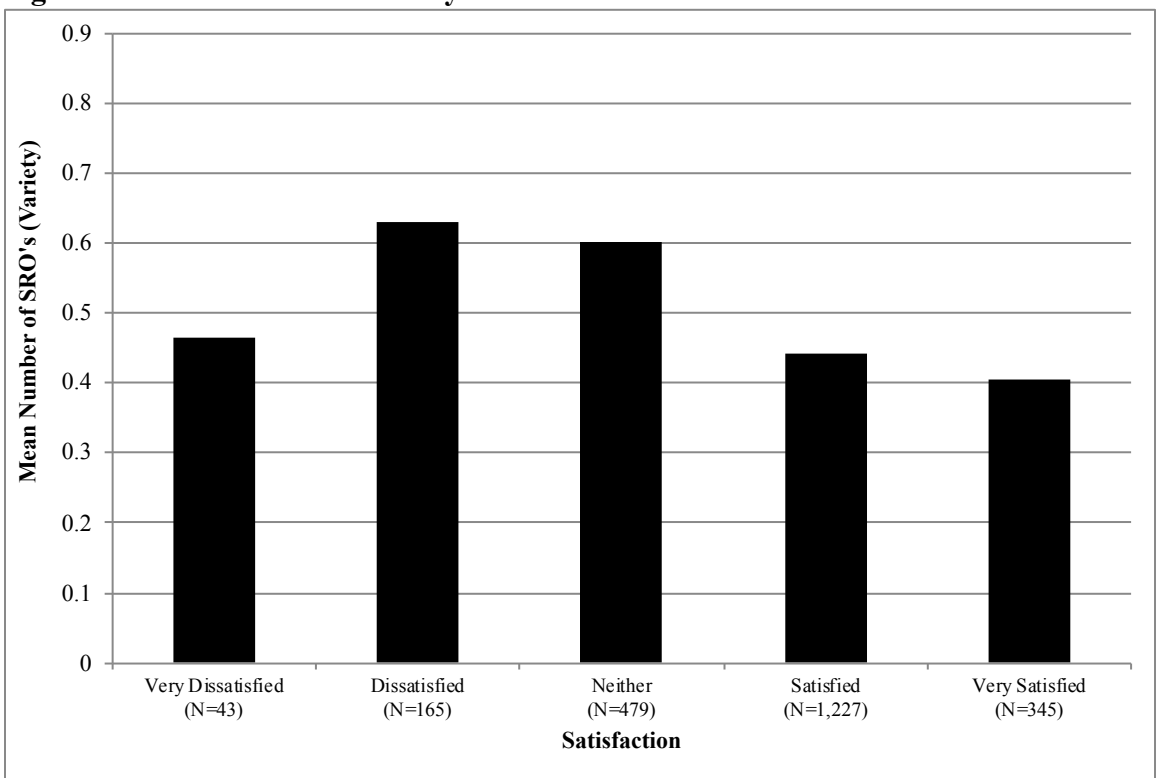
Notes: † $p \leq .10$; $p \leq .05$ $*$; $p \leq .01$ $**$; $p \leq .001$ $***$; Coefficients are odds ratios (OR) computed by exponentiating the coefficients; Only key covariates included in output. Hypothesis tests are two-tailed.

Figure 1A. Mean Street Crime by Satisfaction with Supervision



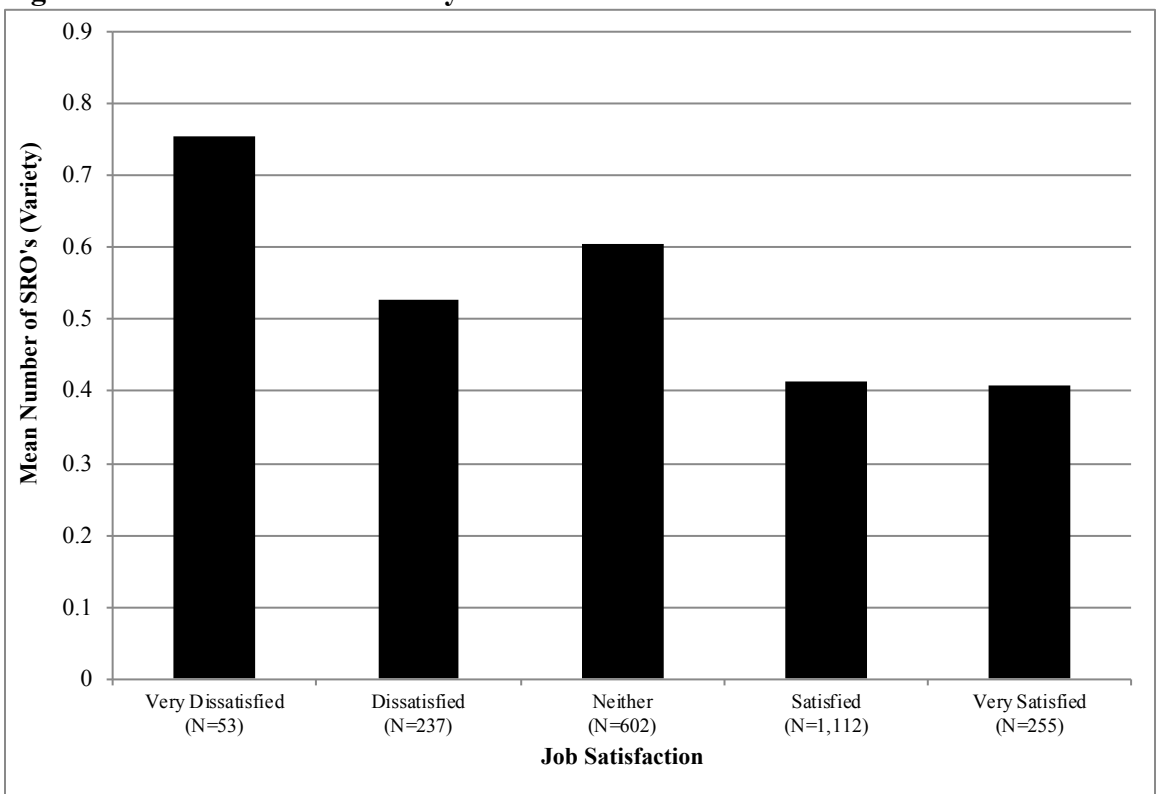
NP=2,258
F=2.80, $p \leq .05$
SM=7.27, $p \leq .01$
IRR=0.84, $p \leq .01$

Figure 2A. Mean Street Crime by Satisfaction with Usefulness



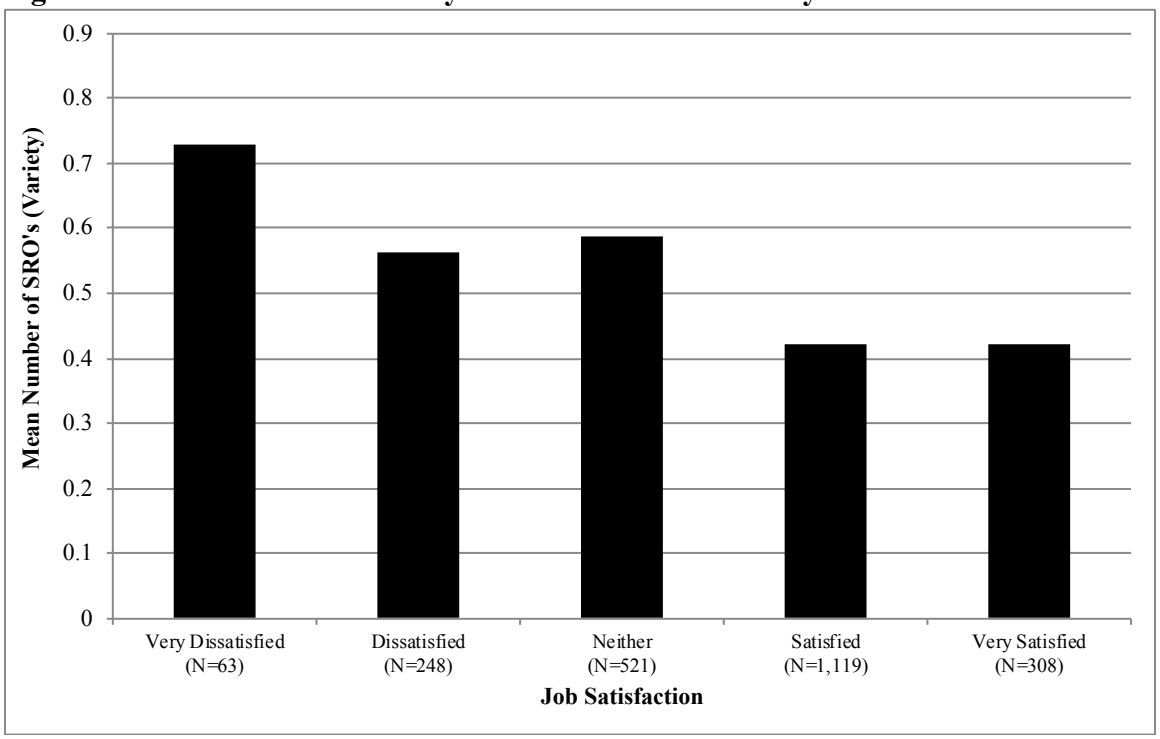
NP=2,259
F= 2.36, $p \leq .05$
KW=13.76, $p \leq .001$
IRR=0.86, $p \leq .05$

Figure 3A. Mean Street Crime by Satisfaction with Status



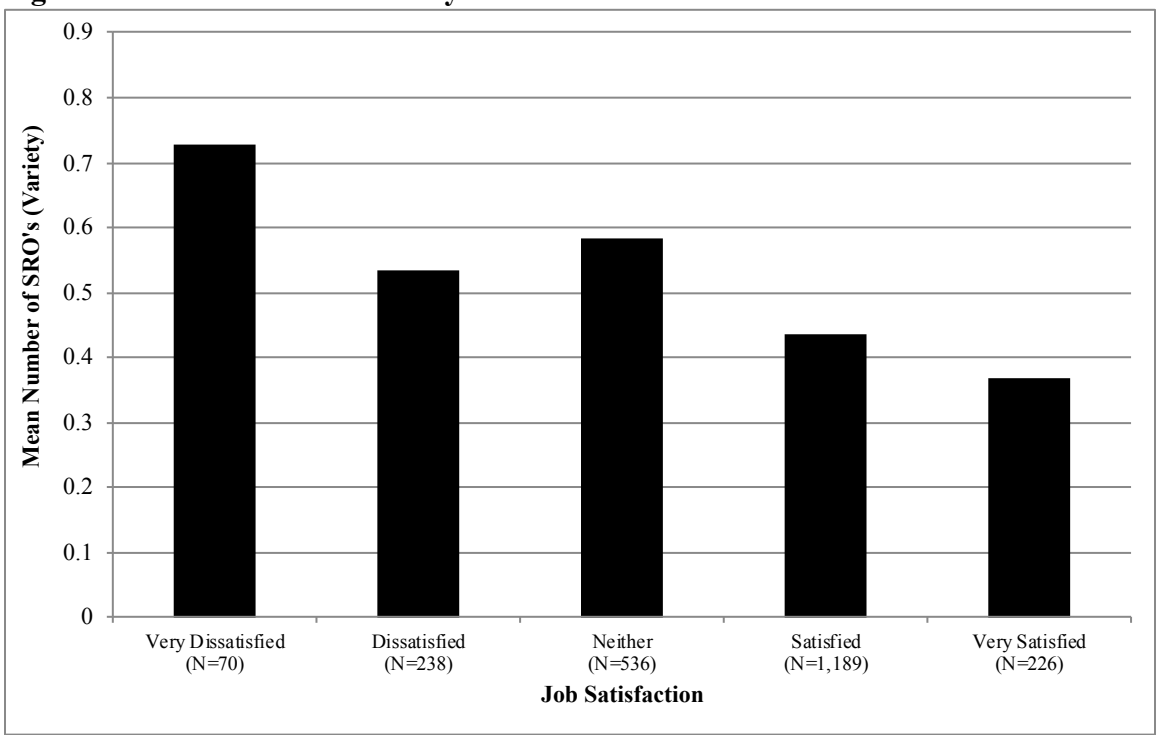
NP=2,259
F=3.33, $p \leq .01$
KW=9.25, $p \leq .01$
IRR=0.85, $p \leq .01$

Figure 4A. Mean Street Crime by Satisfaction with Security



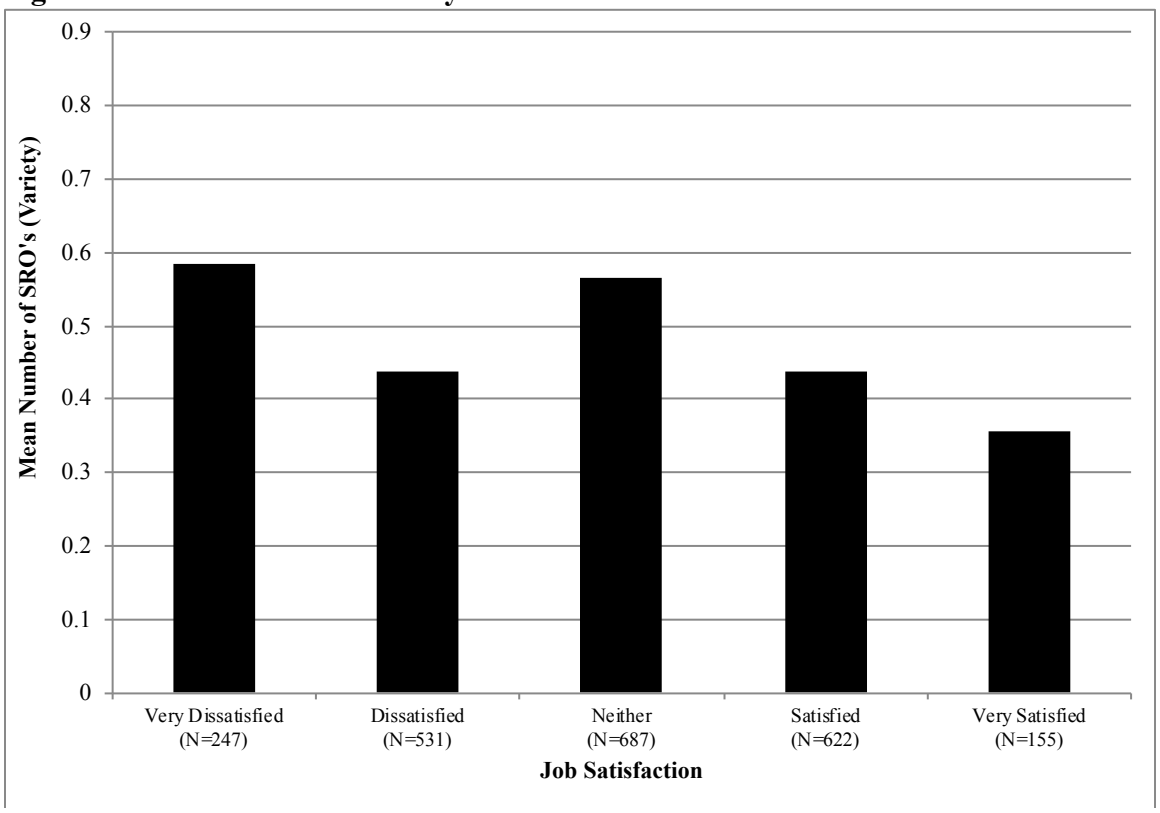
NP=2,259
F=2.70, $p \leq .05$
KW=6.65, $p \leq .05$
IRR=0.86, $p \leq .01$

Figure 5A. Mean Street Crime by Satisfaction with Workload



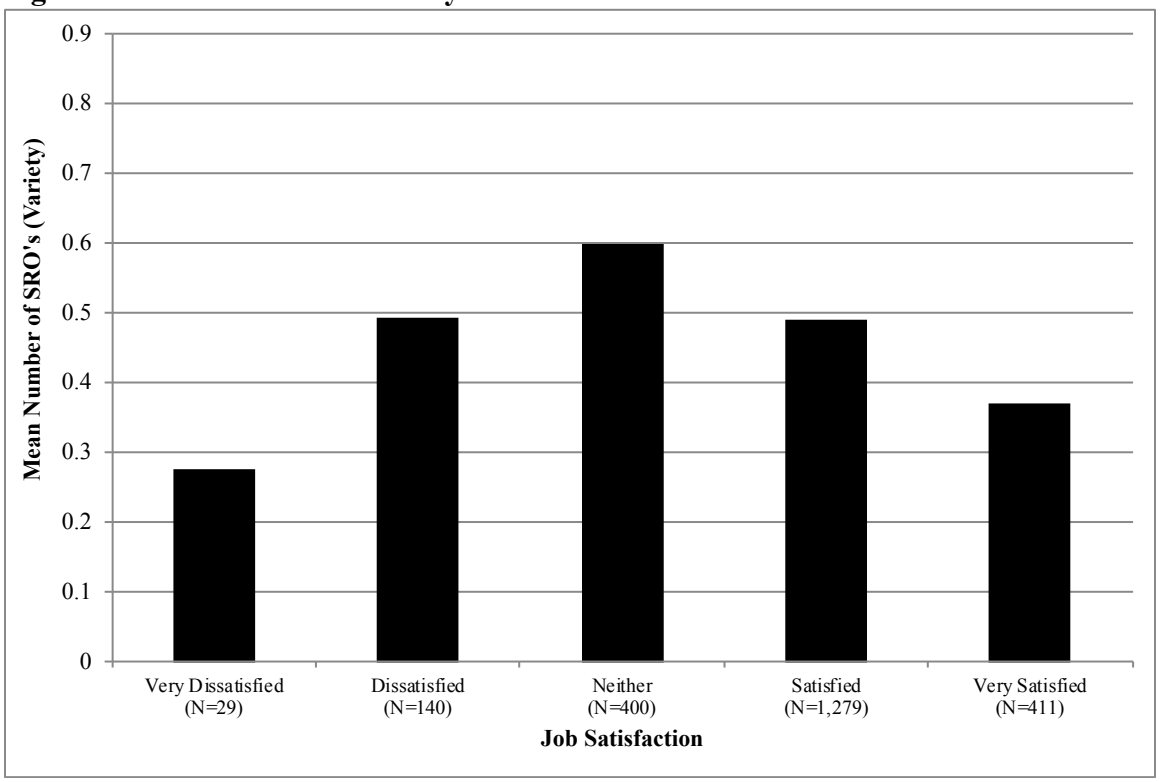
NP=2,259
F=2.58, $p \leq .05$
KW=4.26, p=.09
IRR=0.85, $p \leq .05$

Figure 6A. Mean Street Crime by Satisfaction with Benefits



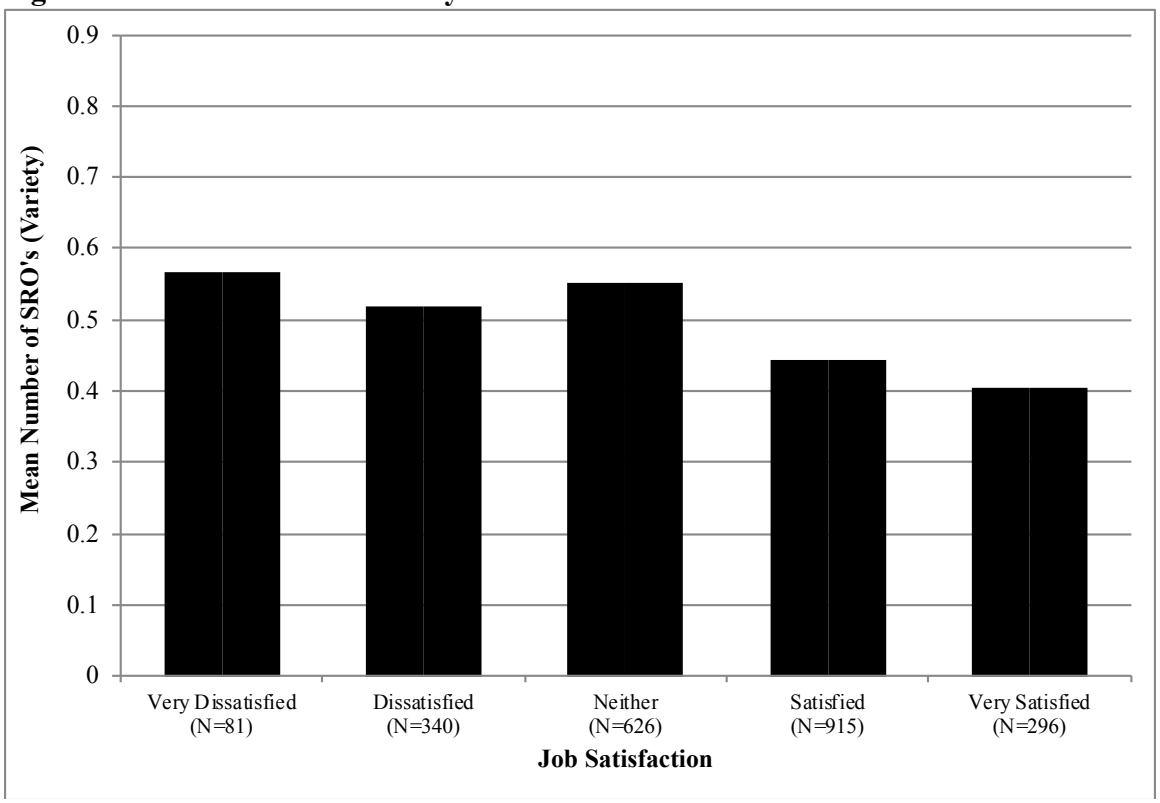
NP=2,242
F=1.93, *p*=.10
KW=3.69, *p*=.13
IRR=0.93, *p*=.20

Figure 7A. Mean Street Crime by Satisfaction with Control



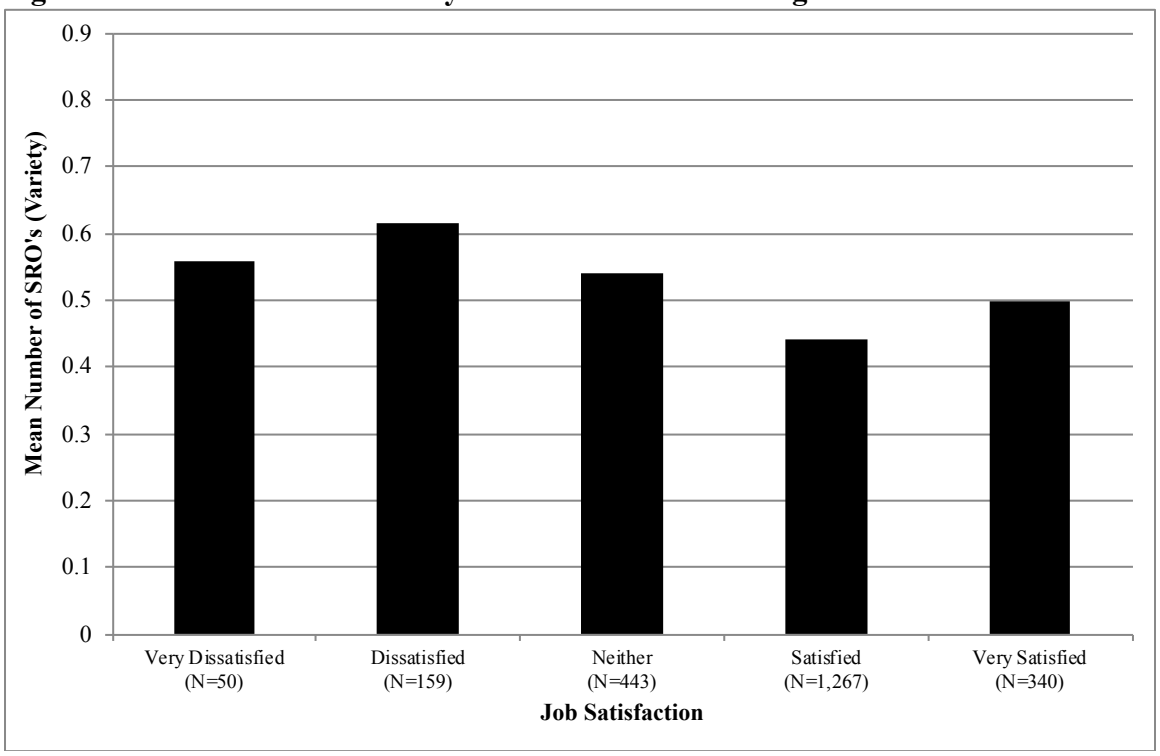
NP=2,259
F=1.93, *p*=.10
KW=3.77, p=.12
IRR=0.90, *p*=.08

Figure 8A. Mean Street Crime by Satisfaction with Advancement



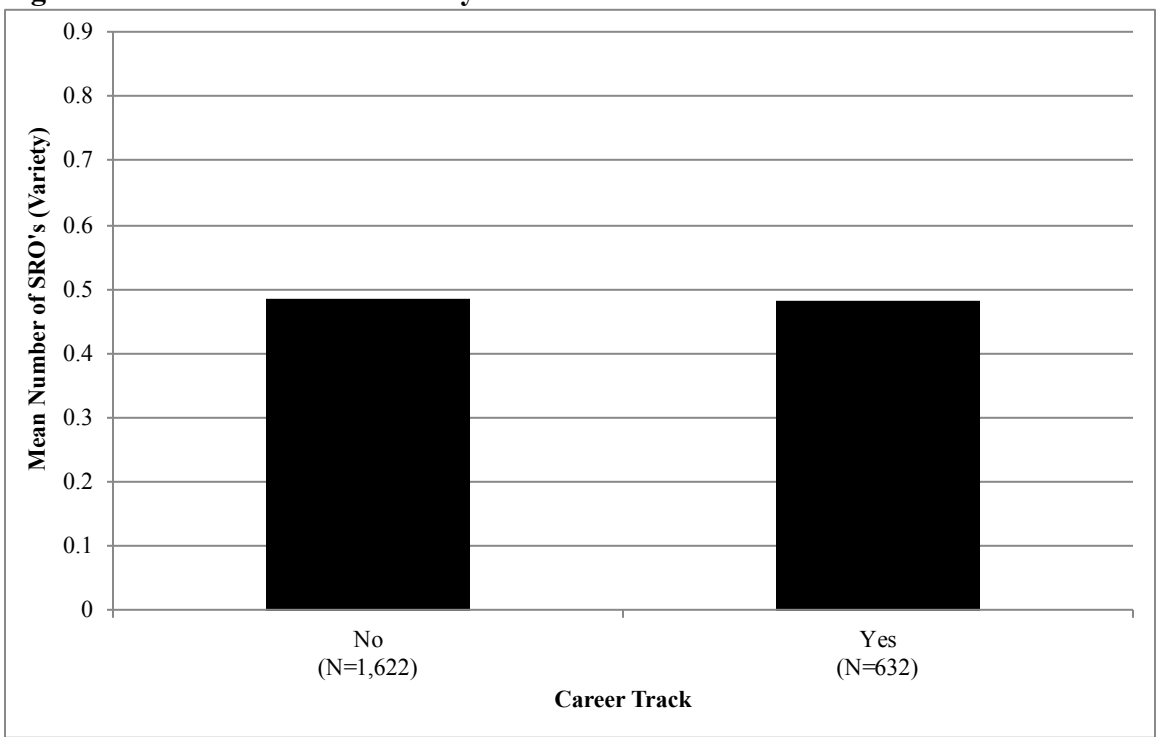
NP=2,258
F=1.19, p=.31
KW=7.03, $p \leq .01$
IRR=0.91, p=.08

Figure 9A. Mean Street Crime by Satisfaction with Colleagues



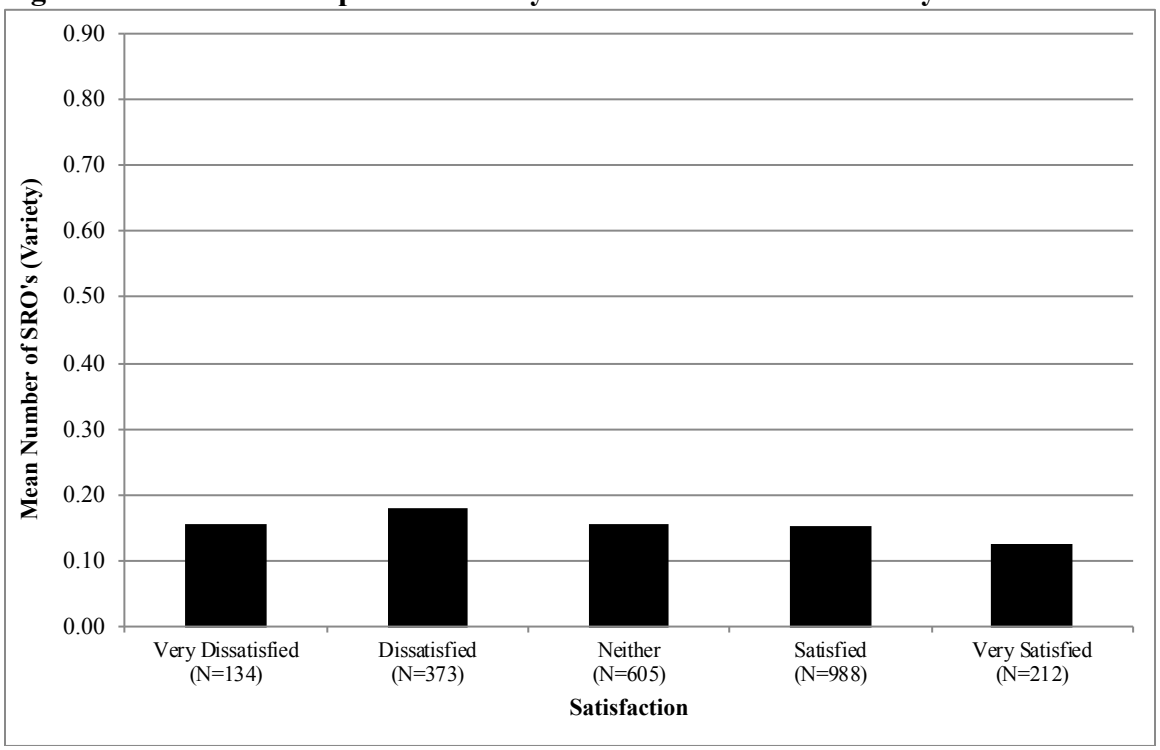
NP=2,259
F=1.15, p=.33
KW=5.87, $p \leq .05$
IRR=0.92, p=.20

Figure 10A. Mean Street Crime by Career Track Job



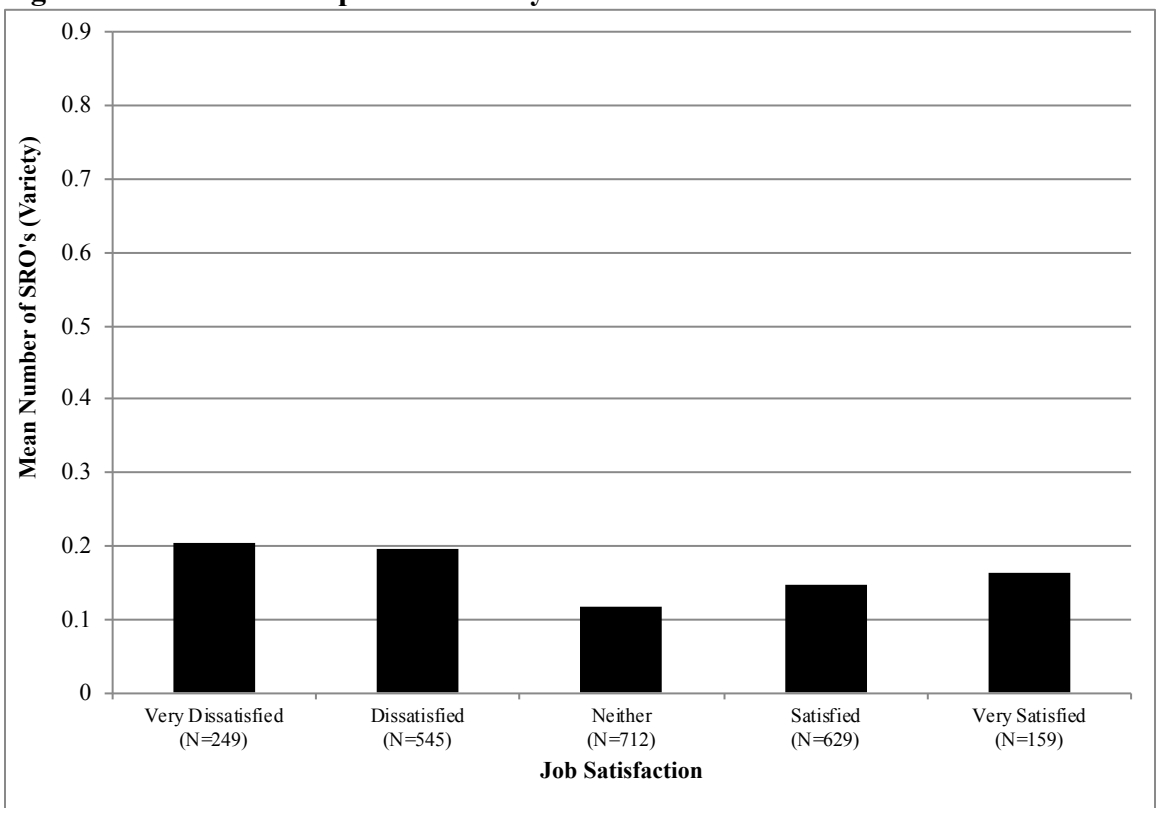
NP=2,254
T=0.02, *p* =.49
MW=-0.27, *p* =.79
IRR=1.00, *p* =.98

Figure 11A. Mean Workplace Crime by Satisfaction with Job Salary



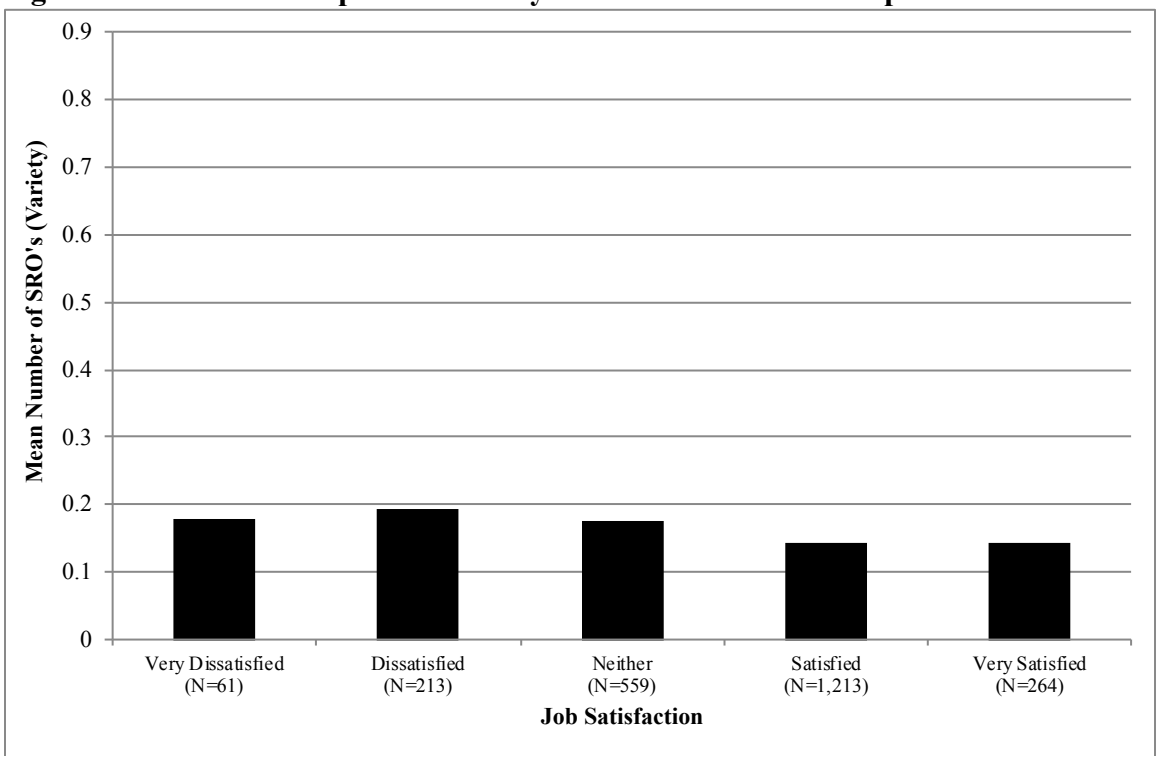
NP=2,260
F=0.32, *p*=0.86
KW=4.03, *p*=0.40
IRR=0.94, *p*=0.37

Figure 12A. Mean Workplace Crime by Satisfaction with Benefits



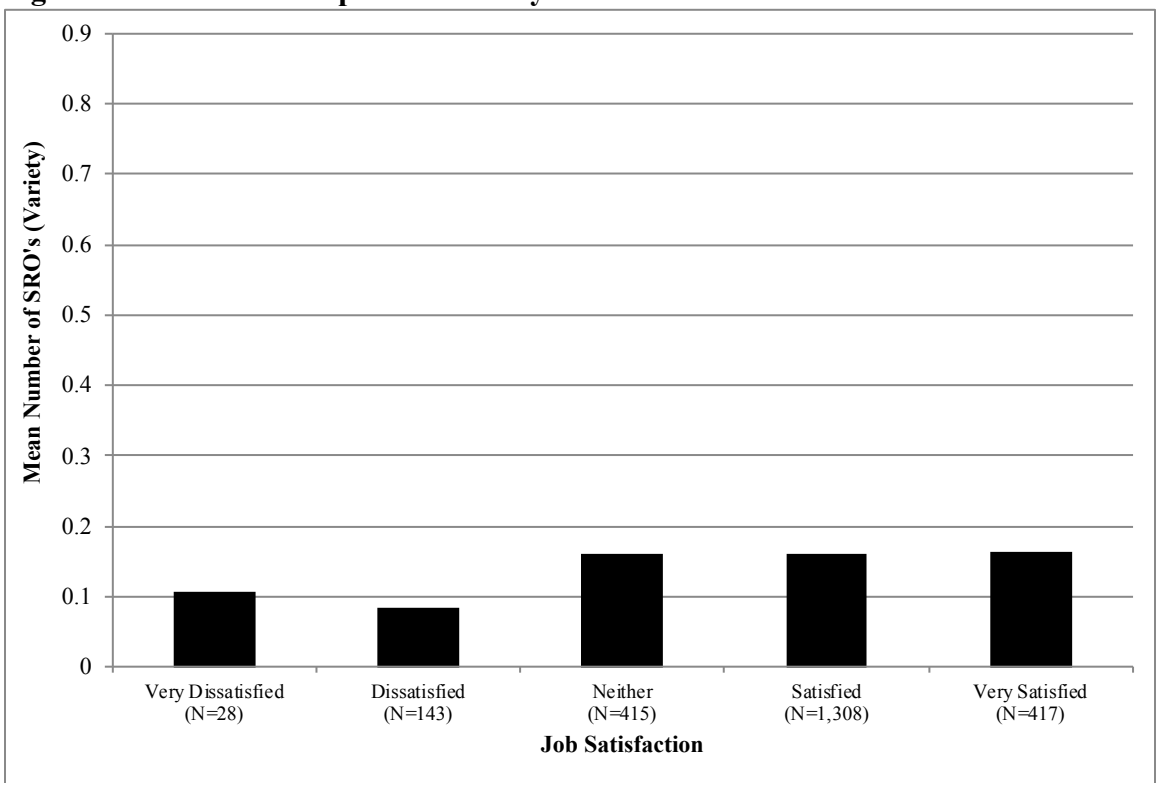
NP=2,294
F=2.08, $p \leq 0.10$
KW=10.92, $p \leq .05$
IRR=0.90, p=0.14

Figure 13A. Mean Workplace Crime by Satisfaction with Job Supervision



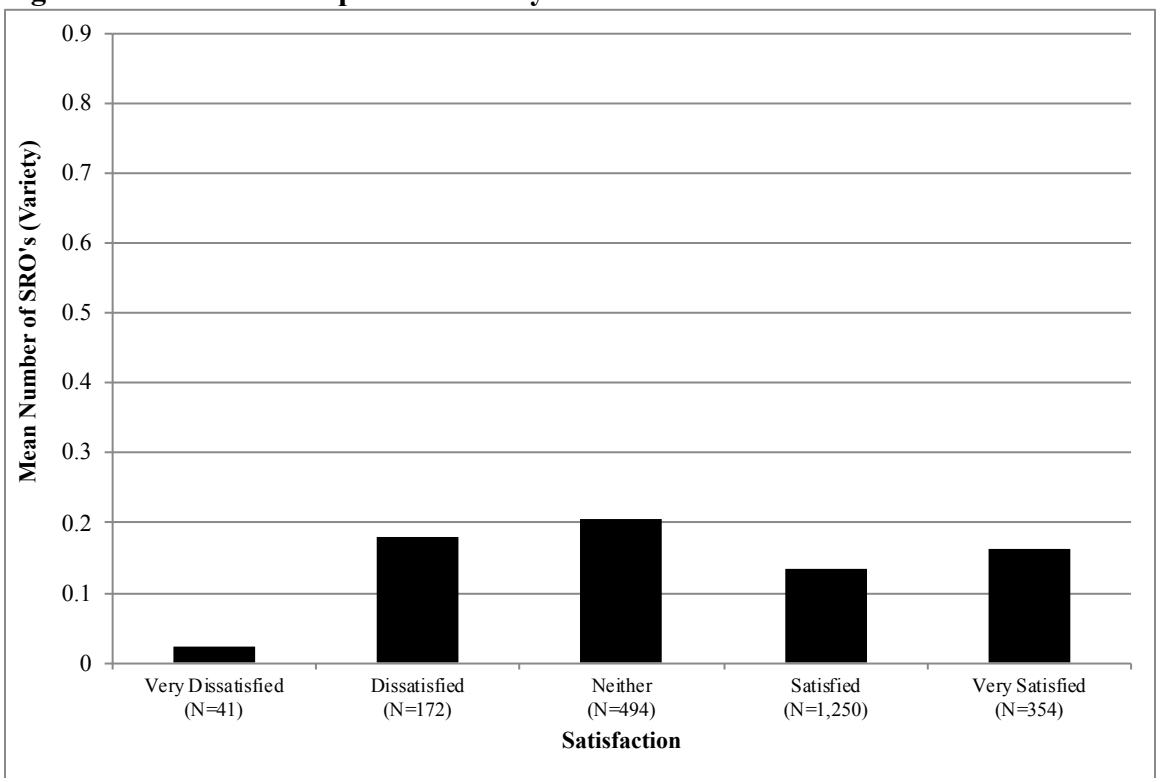
NP=2,310
F=0.64, p=0.63
KW=7.27, $p \leq .01$
IRR=0.89, p=0.17

Figure 14A. Mean Workplace Crime by Satisfaction with Job Control



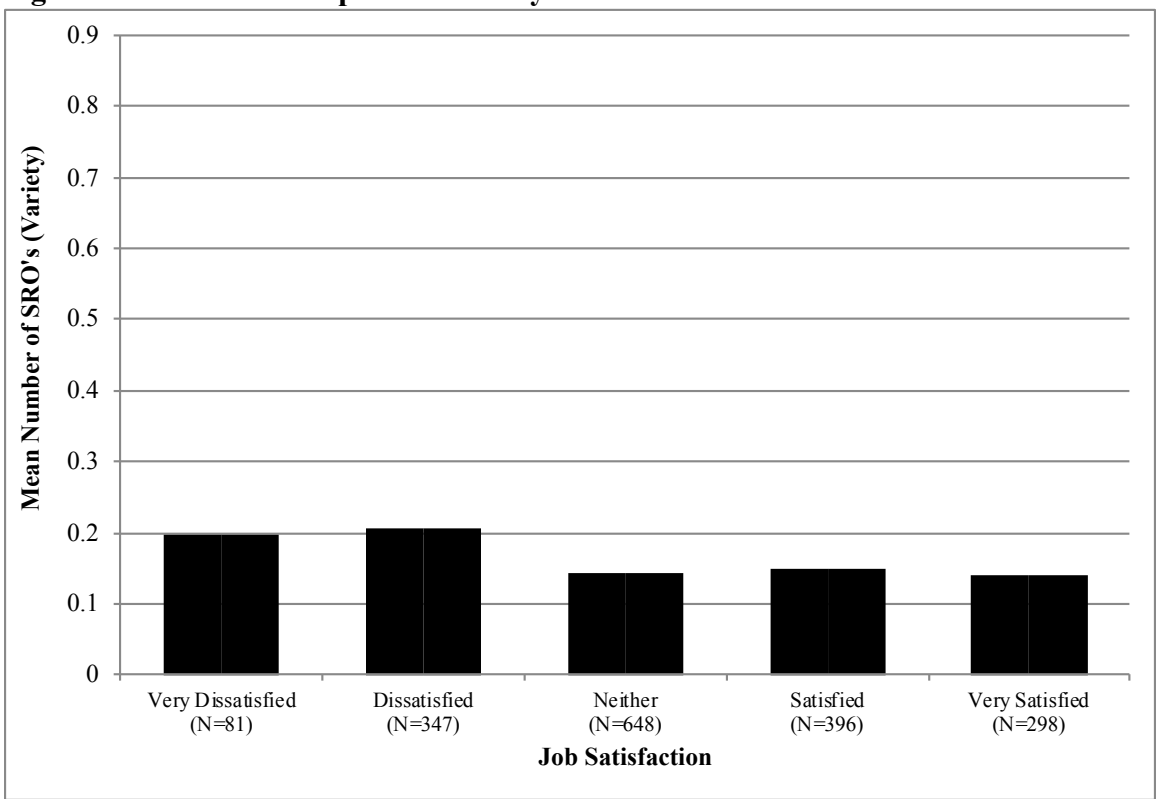
NP=2,331
F=0.72, *p*=0.58
KW=1.49, *p*=0.83
IRR=1.12, *p*=0.21

Figure 15A. Mean Workplace Crime by Satisfaction with Job Usefulness



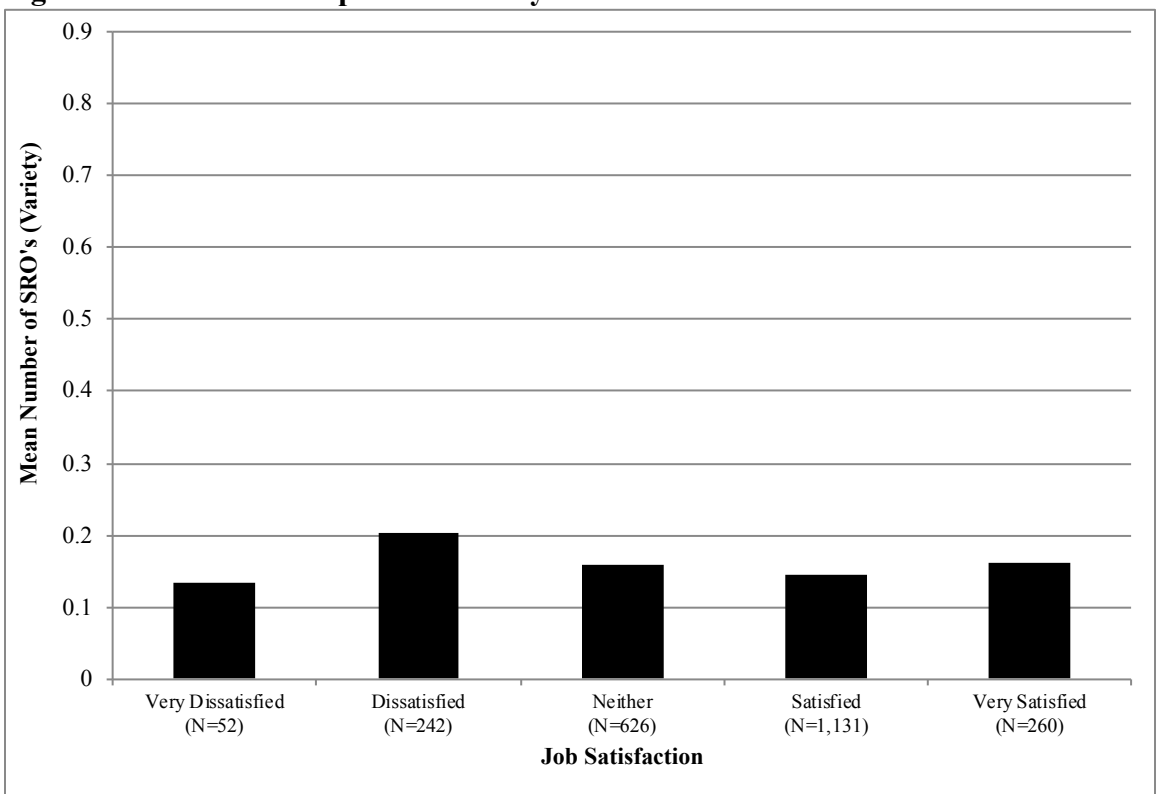
NP=2,331
F=2.16, $p \leq .10$
KW=12.74, $p \leq .01$
IRR=0.95, p=0.52

Figure 16A. Mean Workplace Crime by Satisfaction with Job Advancement



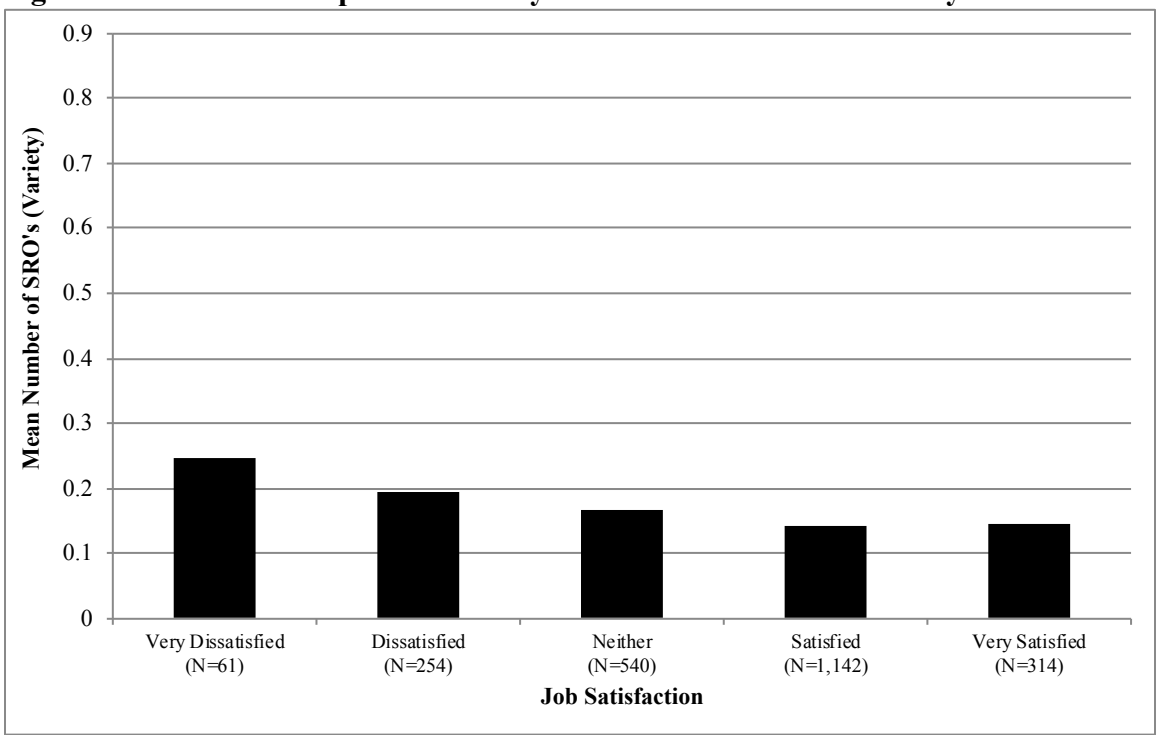
NP=2,310
F=0.97, p=0.42
KW=8.39, $p \leq .10$
IRR=0.90, p=0.17

Figure 17A. Mean Workplace Crime by Satisfaction with Job Status



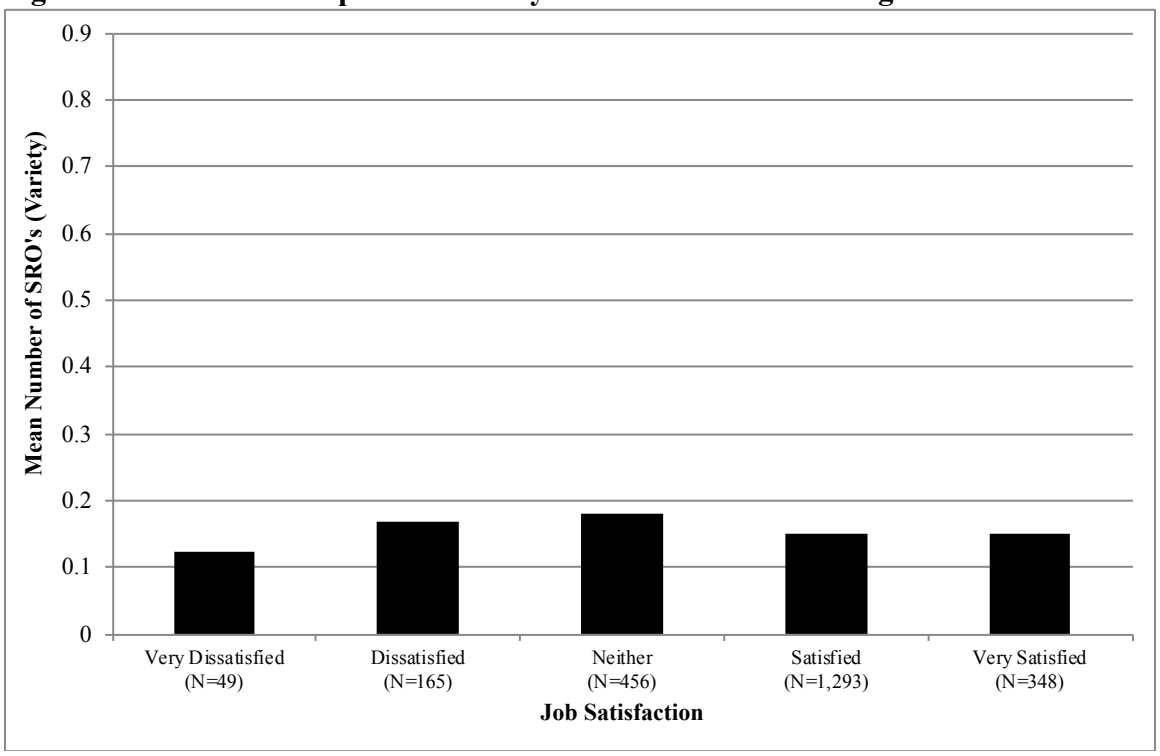
NP=2,294
F=0.57, *p*=0.67
KW=6.40, *p*=0.17
IRR=0.94, *p*=0.45

Figure 18A. Mean Workplace Crime by Satisfaction with Job Security



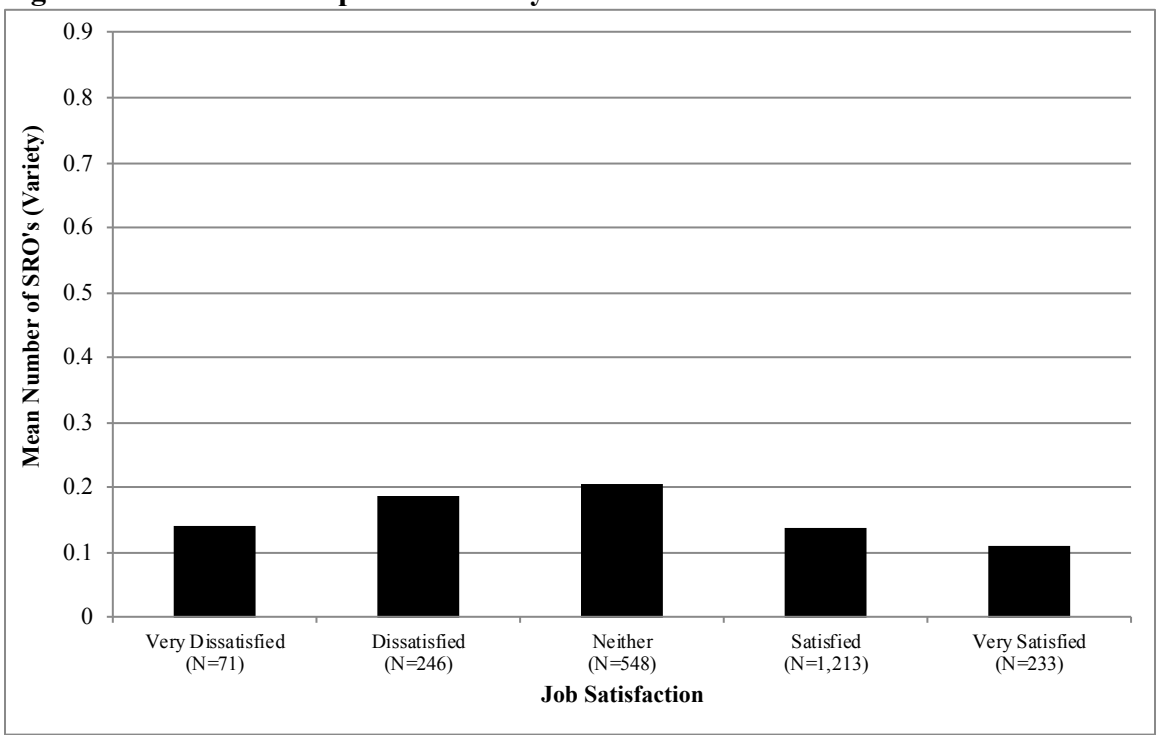
NP=2,311
F=0.97, *p*=0.42
KW=7.23, *p*=0.11
IRR=0.88, *p*=0.11

Figure 19A. Mean Workplace Crime by Satisfaction with Colleagues



NP=2,349
F=0.34, *p*=0.85
KW=2.35, *p*=0.67
IRR=1.00, *p*=0.60

Figure 20A. Mean Workplace Crime by Satisfaction with Job Workload



NP=2,311
F=2.08, $p \leq .10$
KW=4.03, $p \leq .05$
IRR=0.86, $p \leq .05$

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