

Signaling Desistance: Identifying Long-Term Desisters Using Short-Term Signals

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## **ABSTRACT**

Using Pathways to Desistance (PtD) data, a longitudinal study of active youth offenders, the current study tests whether voluntary participation in a variety of programs – job training, mental health, and substance abuse, may be considered as a signal of an individual’s underlying intentions to desist from crime. Informed by a recent proposition by Bushway and Apel (2012) in the criminological literature, and the extant research on signaling stemming from the theory’s original proposition by Spence (1973), this analysis develops and tests a series of hypotheses consistent with either A) a signaling explanation for voluntary program participation or, B) a human capital explanation for the effects of program participation, regardless of its (in)voluntary nature. In order to adequately test these hypotheses against a suitable comparison group, a synthesized control group is created that is composed of randomly selected non-participants, with the time periods of their “participation” also being randomly selected.

Results suggest that voluntary program participation is a very weak signal of intentions to desist. Specifically, no strong differences exist between voluntary participants and the randomly selected control group with respect to attitudes associated with desistance pre- and post-program participation, though patterns in coefficient estimates suggest very mild, but insignificant differences in some attitudinal scales. Further, with respect to criminal behavior, analyses for offending frequency and offending diversity both suggest weak, insignificant differences between groups, even when model over-specification is corrected for. Therefore, voluntary program participation is, at its best, a weak, unfit signal of intentions to desist. Implications of these results are discussed with respect to future work on signals of desistance.

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Be forewarned, inconstant-reader (for those of you who get this reference, kudos!), for this section will likely be longer than the space I am allowed for it and filled with heaps of exuberant and plentiful praise upon the myriad of individuals who have helped me reach this point in my life and career. This is because no man is an island (no matter how much I attempt to be) and my success in academics (and life, in general) is a direct result of other people taking a chance on a young, brash, and egotistical know-it-all who seriously needed to calibrate the size of his britches considerably before he got here.

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**“We are what we pretend to be, so we must be careful what we pretend to be.”**

**– Kurt Vonnegut Jr.**

## **I. INTRODUCTION**

An oft-cited axiom of human behavior is that prior behavior is an excellent predictor of future behavior (Aarts & Verplanken, 1998; Brame, Bushway, & Paternoster, 2003; Farrington, 1987; Ouellette & Wood, 1998; Verplanken & Orbell, 2003). Whether this behavior includes prosocial activities (e.g. community activism, voting, etc..) or antisocial activities (e.g. gambling, excessive drinking) matters not. If one were to know that a person has a history of playing chess routinely, one could surmise, with a high degree of accuracy, that he or she will play chess in the future. This same principle can be applied to many other types of behaviors. Thus, without knowing much about a person, it is possible to predict, in a rather one-dimensional sense, his or her subsequent behavior at some undetermined point in the future.

Criminologists have found that almost all those who engage in criminal behavior in their adolescence go on to cease their criminal activity almost entirely<sup>1</sup> in later adulthood (Blumstein & Cohen, 1987; Blumstein, Cohen, & Farrington, 1988). How then might we reconcile this information with the contradictory principle that past behavior predicts future behavior? One explanation for this contradiction is that the referent time frames are different with respect to crime. The first fact is anchored in the present to near

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<sup>1</sup> Here, one might refer to other behaviors analogous to crime – gambling, drug use, excessive use of alcohol, etc... - that one might continue to engage in. Though not outright illegal, all of these behaviors are traditionally looked down upon socially.

future, i.e. a recent criminal offense predicts a criminal offense in the near future<sup>2</sup>, while the second fact is anchored in the long-term and far-off future, i.e. that criminal behavior is a “phase” one enters in adolescence and exits in early adulthood.

These are, of course, gross generalizations. Some adolescents never commit a crime, most of whom commit a small handful of trivial offenses, whereas some do not “mature” out of crime, but instead offend well into adulthood for manifold reasons. It is this last fact that creates a noteworthy amount of uncertainty for criminologists, particularly as it relates to early identification of those who desist from crime and those who do not. Though there are some indicators of life course-persistent criminal behavior (Kratzer & Hodgins, 1999; Terrie E. Moffitt, 1993; Terrie E Moffitt, 1997; Terrie E Moffitt, Caspi, Harrington, & Milne, 2002) these are far from deterministic, and even those individuals wholly encapsulating this constellation of risk factors still tend to cease criminal activity later in their lives (Laub & Sampson, 2001, 2003; Sampson & Laub, 1995).

Consider the dual desires of protection from potential dangerous criminals and offering a true and meaningful second chance for those who have been rehabilitated. There exist a host of precautionary measures meant to further the first stated goal, to protect the general public from individuals with criminal histories including: criminal background checks for employment or residence, as well as outright employment (Holzer, Raphael, & Stoll, 2003), job licensing (May, 1995), or residency restrictions (Socia Jr & Stamatel, 2010; Zgoba, Levenson, & McKee, 2008), which entirely ban

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<sup>2</sup> In fact, this may not be restricted to person-level events, either. One has to look no further than the spatial near-repeat literature to see that even criminal events in micro-places can spark additional criminal events nearby (Short, D'Orsogna, & Brantingham, 2009; Townsley, Homel, & Chaseling, 2003).

former offenders from select job markets and certain housing options (Burton, Cullen, & Travis III, 1987; Saxonhouse, 2004). Pushback against these kinds of restrictions is leveraged upon the fact that most individuals cease criminal activity, and that the current framework of post-prison “collateral” consequences is merely a secondary and extended form of punishment that serves to impede, rather than to accelerate, rehabilitation (Dale, 1976; Harris & Keller, 2005) and, further, their effects are felt disproportionately by minority populations (Uggen, Manza, & Thompson, 2006; Wheelock, 2005). As is almost always the case, it is in the gray area of the debate where the true answer lies: restrictions are sometimes helpful and sometimes detrimental, and the outcome could depend upon whether the individual whom these restrictions are placed upon has, in fact, “desisted” or not.

It is here that the crux and contribution of this study lies – in the identification of honest, early signals of desistance. Here, I do not use the term signal in its colloquial sense – that it predicts or prognosticates desistance – but in a more specific sense in that a signal represents an action or set of actions an individual purposely chooses in order to broadcast some underlying message to the signal’s intended recipient(s). Within the current context, this underlying message will be taken to be an individual’s desistance status – being a “desister” or not – with the recipients of this message being potential employers. The term “honest” is invoked to account for the fact that these actions, or signals, can be faked by “impostors” (i.e., those signaling they are a desister when they are not). Therefore, the potential “honesty” of a signal is related to its cost. Logically, the greater the cost of attaining and transmitting the signal, the lower the expected benefit for

the impostor and the greater the probability the signal is an honest reflection of the trait being signaled.

The current research will isolate the kinds of signals sent out by those who eventually go on to desist from crime, assessing the degree to which each of these signals actually corresponds to an individual's desistance status by applying the well-established economics theory of signaling (Spence, 1973) to the desistance process. Recent work by Bushway and Apel (2012) has integrated this theory into criminology, conceptualizing the completion of an employment-based reentry program as a signal sent to potential employers that the individual in question is ready to desist. However, program completion is not the only signal an offender may send. Rather, Bushway and Apel (2012) acknowledge there are a host of signals an intentioned desister may send, some to family and friends, others to potential employers, and still others to criminal justice practitioners. The nature, strength, and predictive power (or honesty) of these signals form an unanswered empirical question, the answer to which has important implications for both criminological theory and for criminal justice policy and practice.

In order to shed light on this question, the current dissertation will use data from Pathways to Desistance (PtD), a longitudinal study of youth convicted of serious felonious crimes in Philadelphia, PA and Maricopa, AZ counties. The unique nature of this data set – that all participants are convicted of a serious felony between the ages of 14 and 19 years – coupled with the vast breadth and depth of available measures including: various psychometric and attitudinal scales, exhaustive criminal, relationship, and employment histories, and event history data make these data ideal for a test of the micro-level assumptions of signaling.



This dissertation is organized around two distinct, but related, research questions. First, do signals correlate with underlying personal orientations we know/assume to be associated with desistance? Second, does the presence/absence of a signal alter an estimate of the effect of employment on desistance? I contend these are interrelated because the answer to the first question provides important context for the second. For example, if I find the proposed signals do, in fact, correlate with personal orientations theoretically associated with desistance, then we should expect these to influence desistance above and beyond the impact of legitimate work on crime, even within groups who do/do not obtain employment. However, if I find that no such correlation exists between the proposed signals and underlying orientations, then I would expect the estimated effect of work on crime to remain unchanged even when signals are accounted for.

The review of the extant literature centers on desistance and signaling beginning with a general overview of criminological interest in crime patterns as a function of age; and a review of Spence's (1973) signaling theory and the numerous theoretical refinements that have occurred since his seminal paper. Of primary interest are those qualifications to signaling theory related to the "fit" or "honesty" of signals sent by a signaler and also the characteristics of the signaling environment that may have an influence on how a receiver will interpret these signals. As I will argue below, there are numerous and quite plausible reasons one might not expect a macro-level interrogation of signaling theory to yield supportive findings, perhaps especially so when the sample of interest are those with criminal records. Instead of beginning at the macro-level, this dissertation will first explore the micro-level assumptions inherent to the theory – namely, that the signals

being transmitted are meant to communicate that the individual does, indeed, possess the underlying quality being signaled – that of being a desister from crime.

## II. REVIEW OF LITERATURE

Patterns of desistance have long held the attention of criminologists – this is best exemplified by the persistent attention paid to age-crime curves, research that began in the early to mid-1900s (Glueck & Glueck, 1950, 1968; Hall, 1904) and continues today (Bushway, Sweeten, & Nieuwbeerta, 2009; Liu, 2014; Shulman, Steinberg, & Piquero, 2013; Sweeten, Piquero, & Steinberg, 2013). These works indicate a desire for actionable knowledge and refinement of theory – i.e., how can we explain and act upon the fact that, while many individuals commit crime in their youth, only a small fraction of them continue to commit crime post-adolescence?

This small fraction of offenders has garnered the most academic attention, with researchers offering varied explanations for why some continue their criminal careers and others do not. Some contend that official criminal labeling is to blame (Becker, 1963; Lemert, 1951), that early childhood developmental deficiencies or constitutional differences are the cause (Gottfredson & Hirschi, 1990; Terrie E. Moffitt, 1993; Wilson & Herrnstein, 1985), that turning points redirect criminal paths toward more prosocial outcomes (Elder, 1986, 1985; Laub, Nagin, & Sampson, 1998; Laub & Sampson, 1988, 1993a, 2001, 2003; Sampson & Laub, 1995), or that cognitive transformations contribute to desistance (Giordano, Cernkovich, & Rudolph, 2002; Giordano, Schroeder, & Cernkovich, 2007; Maruna, 2001).

Far simpler explanations also abound. For example, one perspective on the age-crime curve is that age will inexplicably remain a significant predictor of crime (Gottfredson & Hirschi, 1990) regardless of the depth and breadth of the data in hand, an observation bolstered by a rich tradition of criminal careers research consistently establishing that almost all offenders go on to cease their criminal behavior in their mid- to late-thirties (Blumstein & Cohen, 1987; Blumstein, Cohen, Das, & Moitra, 1988; Blumstein, Cohen, & Farrington, 1988). Nascent work with the PtD sample, however, calls such a simple explanation into serious question – Sweeten et al. (2013) found that a full 69% of the drop in crime as these youth transitioned from adolescence to early adulthood could be explained by covariates associated with existing criminological theories (e.g., social learning, social control, and strain, among others), concluding that:

*“[T]hus, the relationship between age and crime may be difficult to explain, but it is by no means inexplicable. Our analyses show that, with the simultaneous application of multiple theoretical perspectives derived from developmental science, we can do a much better job of explaining the inexplicable than has been assumed” (p.936).*

The analysis in Sweeten et al. (2013) implies that an examination of desistance trends using the PtD data is not an effort in futility nor that unobservable developmental changes co-varying with age can largely account for the fact that many of these youth go on to cease their criminal offending or offend less frequently. Rather, Sweeten et al. (2013) demonstrate that much of the decline in criminal behavior is attributable to observables, implying that well-specified analyses can gainfully interrogate desistance using the PTD

data, absent the looming “inexplicability” criticism levied by Gottfredson and Hirschi (1990).

With this potential criticism effectively countered, it is appropriate to move forward with a concise review of the extant literature informing my dissertation. I will begin with a review of signaling theory in the tradition of economics, with a particular emphasis upon differentiating “signals” in the colloquial sense from signals under the more restrictive definitions proposed by Spence (1973). This review will culminate with a discussion of nascent research interrogating the various characteristics of signals, signalers, receivers, and the signaling environment that have been the focus of research interest since the original proposition of signaling theory.

## II-1. SIGNALING THEORY

Originally proposed by Spence (1973), signaling theory has recently been introduced into the criminological literature by Bushway and Apel (2012), who contend that the completion of job training reentry programs presents itself as a signal sent by the individual to potential employers that he or she is ready, willing, and able to participate in the formal employment market. Central to the original proposition of signaling theory is employment – namely, the uncertainty surrounding a firm’s hiring decision. Spence (1973) contends that the hiring process is akin to a lottery, whereby the wages paid to a job recipient represent the firm’s confidence in his or her productivity, which is inherently unobservable at the time of the hiring decision.

Given this, the employer must search for alternative indicators of productivity contained within the information an applicant provides them, such as previous employment, demographic information, educational achievements, and criminal history. To this point, Spence (1973, p.357) makes an important distinction between unalterable and alterable indicators:

*“Of those observable, personal attributes that collectively constitute the image the job applicant presents, some are immutably fixed, while others are alterable. For example, education is something that the individual can invest in at some cost in terms of time and money. On the other hand, race and sex are not generally thought to be alterable. I shall refer to observable, unalterable attributes as indices, reserving the term signals for those observable characteristics attached to the individual that are subject to manipulation by him.”*

Spence (1973) emphasizes that though some, or even all, of these indicators may be *predictors* of productivity, a subset of these are only *signals* in the purely colloquial sense. That is, the colloquial use of the term *signal* as synonymous with the term *predictor* (i.e., that an indicator “signals” or “predicts” some outcome) is inherently incompatible with the contours of signaling theory. A *signal* must represent something an individual retains control over (i.e., makes a *choice*), imparts some cost to themselves, and predicts the outcome of interest whereas the term may not also extend to fixed traits the individual may not themselves alter (e.g., *indices* such as sex and race) even though these may be equally, or even more, predictive of actual productivity. Simply stated, all *signals* are *predictors*, but not all *predictors* are *signals*.

Additionally, a signal need not be predictive in a *causal* sense. Rather, a signal, such as the voluntary enrollment, active participation in, or completion of a reentry job training program is merely a means an individual may use to calibrate or correct others' subjective views of their productivity or desistance status. Further, in order for a signal to effectively differentiate between individuals with respect to some outcome – whether it be productivity or desistance – the population sending the signal must be comparatively small when compared to the larger population not sending such signals and the costs of the signal must be sufficiently correlated with the outcome of interest.

As an example, Spence (1973) contends that education is a potential signal of productivity – that is, those applicants reporting a college degree must pay some cost to obtain this signal and thus symbolically differentiate themselves from other applicants. In the scenario where a minority of applicants report a college degree and the cost of obtaining a degree is indeed related to productivity, then an employer can effectively use a degree as an observable signal of the yet unobservable productivity of the applicant. If, however, the proportion of applicants reporting a college degree were to overtake the proportion of those that do not or if the costs of obtaining a degree were to significantly decrease, the strength of a college degree as a signal of productivity will decline, leading employers to seek alternative signals – e.g., if a large proportion of applicants possess a college degree, an employer might begin to select from among the applicants with an advanced degree, implying that signals are not necessarily fixed.

The current analysis seeks to empirically interrogate this proposition and the arguments of Bushway and Apel (2012) by using participation in a community-based job training program as a signal of desistance for serious youth offenders. Though the PtD

data do not contain indicators of whether an individual finished said program, it does provide a measure of the total number of sessions attended, as well as whether this attendance was required of them by the court or voluntary.<sup>3</sup> Naturally, only those voluntarily participating can be interpreted as sending a “signal” – theirs is a cost undertaken as the result of a personal choice (known as an “opportunity cost”) as opposed to the alternative, whose cost is imposed upon them by another entity (i.e., court-ordered participation). In addition to this possible signal, the current analysis will also examine other potential measures to ascertain whether they can be effectively used as desistance signals including 1) substance abuse program participation and, 2) mental health program participation – all under the assumption that only voluntary participants can be considered as sending a signal.

The potential benefits of integrating a signaling-based approach into the criminal desistance literature cannot be understated. As it currently stands, policymakers and employers have little advice to guide their decisions other than having to wait for an extended period of time – typically, 7 to 10 years (Kurlychek, Brame, & Bushway, 2006, 2007) – to truly ascertain whether an individual with a criminal record has definitively “desisted” from crime. The consequences of this approach are self-evident – the years an individual with a criminal record has to wait in order to be deemed eligible for gainful, formal employment (or, alternatively, that their risk is at an acceptable level), produce costs not just for themselves, but for society at large. This waiting period can see numerous individuals reoffend for lack of other options and, even for those who do not, a

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<sup>3</sup> Voluntary/involuntary participation is measured on a monthly basis, so it could be the case that an individual initially attends due to court requirements but continues to do so even when it is no longer required. This appears, at least initially, to potentially serve as an additional signal.

loss of potentially productive years spent without employment or being under-employed. Given that recent research (Kurlychek, Bushway, & Brame, 2012) has demonstrated that there exists a sizeable population of individuals leaving prison who immediately desist from crime (“instantaneous desisters”), it is of utmost importance that we make efforts to identify this population as early as possible for both practical and theoretical reasons.

## II-2. SIGNALING OR HUMAN CAPITAL – CONTRASTING THE APPROACHES

Before reviewing the evolution of signaling theory since its original proposition, it is important to begin by contrasting a signaling explanation for desistance with its prototypical counterpart – that of human capital. That is, Spence (1973) attributed wage premiums associated with a college degree to be reflective of an underlying signal of productivity, but an equally valid perspective on this wage premium is that it coincides with the individual with a college degree being more productive *because* of their education. From a signaling perspective, an education and its formal recognition (a degree) are *correlated* with a pre-existing level of productivity the individual was already capable of – the signal is merely a means the individual uses to differentiate themselves from other applicants and communicate their underlying worth. In contrast, a human capital explanation posits that education has a *causal* influence on productivity – working toward a college degree *enhances* an individual’s productivity, resulting in them being offered more lucrative employment.

Extending these arguments to the current context, one might counter that program participation (voluntary or not) should yield some premium to the individual in the form of higher rates of employment, marriage, or an increased wage rate. This relationship is



fairly straightforward for job training – these programs require the individual to attain a set of skills or general knowledge that employers are often willing to compensate with heightened wage rates. One may also view mental health or substance abuse treatment as contributing to human capital – though neither of these programs need have a positive *causal* effect on the outcomes above (i.e., the individual becoming employed *because* of the program) it is plausible that participation at least allows for the individual to realize their *actual* human capital value without the hindrance of a mental health issue or substance abuse problem (i.e., it *reveals* or *allows* dormant value to manifest itself).

Contributing to the lengthy polemic between these explanations is the simple fact that both perspectives yield the same outcome – i.e., an “educated” individual obtaining higher wages – but differ in how that outcome is achieved. Therefore, a true-to-form test comparing these perspectives is predictably difficult to conduct outside of a controlled laboratory environment (see Kübler, Müller, and Normann (2008); Posey and Yavas (2007); Potters, Sefton, and Vesterlund (2005, 2007) for more recent game-theoretic tests of these competing hypotheses). As such, researchers interested in testing these competing explanations have relied upon various assumptions to differentiate between circumstances in which signaling should or should not be evident and, at times, have relied on natural experiments, often involving changes to the minimum school leaving age.

One such prior assumption has been that the employment market is divided into two sectors: 1) the competitive sector (private sector/self-employed) and, 2) the uncompetitive sector (public sector/employees) and that the hypothesized returns to signaling (i.e., a wage rate) should be different across this distinction (Chevalier,

Harmon, Walker, & Zhu, 2004). Using such a framework, Brown and Sessions (1999) hypothesize that, since there is little incentive for the self-employed to use an education as a signal, the returns of education among the self-employed should be solely related to productivity, while those same returns for employees in the uncompetitive sector are a function of their human capital and the value of education as a signal. Their results confirmed this hypothesis – consistent with the “education as a signal” argument, employees in the uncompetitive sector were more likely to report higher levels of education but, also, a higher return for the same amount of education when compared to the self-employed (Brown & Sessions, 1999). Though additional research has also used this framework and found results suggestive of signaling (Layard & Psacharopoulos, 1974; Psacharopoulos, 1979), it has been argued that the distinction between competitive and uncompetitive sectors is plagued with selection bias (i.e., some individuals select into either market due to prevailing personal preferences) and that tests of signaling theory using this framework rest on the unsupported assumption that individuals make the choice to be in a particular market *and* obtain a specific level of education simultaneously (Chevalier et al., 2004).

Yet another trajectory of research into human capital and signaling arguments is an examination of the so-called “sheepskin effect.” Underlying this concept is the notion that “qualifications have a return that exceeds the return to the number of years spent acquiring them so that there are discontinuities in the returns to schooling at points associated with acquiring qualifications” (Chevalier et al., 2004, pp.508-509). Stated simply, the wage returns for education should exhibit clear jumps around years in which a qualification (the “sheepskin”) is typically given to the individual if the signaling

hypothesis is correct. Extant research suggests there is support for this hypothesis. Using the US Current Population Survey (CPS), Hungerford and Solon (1987) discovered large discontinuities in the estimated wage returns of education for individuals in certification years (i.e., years where they earn some type of qualification). Further, Chevalier et al. (2004) independently test this same hypothesis using data from a different period of time for Great Britain (the Labour Force Survey (LFS)) and also find that the estimated returns of education exhibit nonlinearities suggestive of a “sheepskin” effect and, therefore, provide some support for the signaling hypothesis.

An alternative perspective on the signaling hypothesis is that, since signals are not necessarily fixed, a better test of these competing theories would be to compare an individual’s level of education relative to their cohort (Johnes, 1998; Kroch & Sjoblom, 1994). This hypothesis operates under the signaling assumption that education is a means of differentiating oneself from other competing applicants for employment and, consistent with the signaling approach, that signals are apt to change depending upon context. If, for example, an individual within a highly-educated cohort wishes to send a signal through their education, they would necessarily have to undergo several more years of education than their average peer in order to do so. This argument is quite easily tested by computing the average education of a birth cohort, and then differencing each individual’s level of education against this average – if signaling is evident, those above this average should exhibit higher wages compared to those at or below this average.

Extant research testing this hypothesis has found weak or no support. Johnes (1998), using International Social Survey Programme (ISSP) data on workers in five countries (Australia, Britain, Germany, Italy, and the US) reported weak support for the

signaling hypothesis. While the effect for average education level was indeed negative (as would be predicted by signaling theory) it failed to reach statistical significance (but came close in one instance for females, specifically), thus providing very weak support for the signaling hypothesis (Johnes, 1998). A test conducted by Kroch and Sjoblom (1994) compared the relative rank of an individual to their cohort in respect to education levels, and assessed the influence of this variable as it predicted logged annual earnings when controlling for absolute levels of education in years. Under the signaling perspective, one would expect those at higher ranks would also report higher logged earnings while under the human capital perspective, one would expect rank to be insignificant and the absolute number of schooling years to be positive and significant. Their results supported the latter perspective, as education rank was rarely a significant predictor of earnings and in the case that it was, its effect was negative (i.e., individuals of higher rank had *lower* logged annual earnings) while absolute years of education were routinely and positively associated with higher earnings. These results notwithstanding, it has been noted that relative education rank might be an invalid indicator for testing signaling theory, as it can be influenced by cohort size, especially under conditions where educational institutions place limits on their admission cohorts (Chevalier et al., 2004).

Finally, perhaps the strongest tests of signaling versus human capital explanations for wage differences associated with education have used natural experiments, relying upon legislative changes to the minimum school leaving age to test these competing arguments. Changes to the minimum school leaving age (for example, raising it from 15 to 16, or 16 to 17) results in clear expectations for subsequent individual behavior informed by either theory. Under the signaling hypothesis, increasing the minimum

school leaving age should lead to more productive individuals pursuing higher levels of education to differentiate themselves from other potential applicants who now have one additional year of education (in the case where the minimum school leaving age is increased by one year). In contrast, if the human capital explanation is correct, the distribution of years of education should only change for low productivity workers who already choose the lowest level of education possible under any circumstance.

Capitalizing on state-to-state differences in compulsory attendance laws (CALs), Lang and Kropp (1986) used US Census data and the timing of these CALs to test these alternative predictions. Their results indicated that CALs extending the minimum school leaving age resulted in positive and significant effects in the enrollment rates of individuals unaffected by these changes (i.e., those already older than the leaving age), suggesting that the effects of CALs are borne out across the spectrum of years of education, and are not solely restricted to those who would have left at the minimum age, regardless of what this age was (Lang & Kropp, 1986).

In a slight adjustment to this framework, Bedard (2001) posited that constraints placed upon the higher end of the education spectrum (i.e., the lack of university access) should result in a greater degree of high school graduates and, therefore, lower dropout rates. This is because the benefits to be gained from obtaining a high-school diploma for those who would have otherwise dropped out are favorable to its costs when high-ability individuals cannot self-select into higher levels of education and, thus, create an equilibrium that generates a disutility for high-school dropouts to continue their education. In contrast, a human capital approach would assume that high school dropout rates should remain constant across areas where high ability individuals do and do not

have constraints placed upon their access to higher education (Bedard, 2001). That is, if education is not a signal, high school dropouts will choose to leave school regardless of whether high-ability individuals are constrained in their choices to further their education (i.e., to go to a university). Using longitudinal data on representative samples of youth in the United States (the National Longitudinal Survey of Young Men/Women (NLSYM/W), Bedard (2001) found support for the signaling explanation, indicating that in areas where university access was constrained there was, indeed, significantly lower rates of high school dropouts as individuals gravitated toward obtaining high school degrees and thus benefitting from high-ability individuals' inability to self-select into higher levels of education.

This evidence notwithstanding, the debate between human capital and signaling theories continues unabated. This is due to, as mentioned previously, the similar outcomes expected under both models. That is, the returns from education under either specification predict higher wages rates; these explanations merely differ in the reasons behind these wage premiums. As demonstrated above, numerous scholars have sought to craft "true" tests of these competing theories, and results appear to remain somewhat mixed. If this polemic seems familiar to a criminologist, there is an excellent reason for it. This same type of debate has existed in criminology with respect to the causes of desistance. Though the scholars and particulars might be different, the general characteristics of the argument are eerily similar, with one side arguing that desistance is a largely a mechanical process (reminiscent of the human capital perspective), and the other countering that the process of desistance is one of self-selection (the signaling perspective).

Within the realm of criminology, we already have a well-researched and theorized counterpart to the human capital and signaling debate. Namely, evoking the human capital argument is the age-graded theory of informal social control (Laub & Sampson, 1988, 1993a, 1993b, 2003; Sampson & Laub, 1995), while the clearest signaling equivalent is the cognitive transformation/identity change theory of desistance. Regarding the former, we might surmise that program participation, regardless of its (in)voluntary nature, increases an individual's opportunities for employment while, at the same time, heightens their human capital value, thereby increasing the (in)formal costs of offending through the loss of these gains if one were to be arrested and convicted of a new offense. Here, the relationship between program participation (and its ideal outcome – employment) and desistance is largely a mechanical one – participation adjusts an individual's rational calculus to favor law-abiding behavior.

In contrast, the cognitive transformation/identity change theories of desistance (Giordano et al., 2002; Giordano et al., 2007; LeBel, Burnett, Maruna, & Bushway, 2008; Maruna, 2001, 2004; Paternoster & Bushway, 2009) would hold that the process of desistance has already begun for those sending signals through their voluntary participation in a program. Specifically, criminally-involved individuals make a conscious choice to cease their offending and, akin to signaling theory, begin to act in such a way to convince others of their new identity/status as an desister. As is argued here, one such method they may use to convey this message is through their voluntary participation in job training, substance abuse, and mental health programs as this is an opportunity cost they take on of their own volition, striving to build a reformed life from the ashes of their past. The proceeding sections will provide a comprehensive review of

these theories, and will link them with the current research through their implied explanations for the relationship between (in)voluntary program participation and desistance.

### II-3. AGE-GRADED THEORY OF INFORMAL SOCIAL CONTROL & DESISTANCE

Building upon the life-course research of Elder (1986, 1985), Laub and Sampson (1988, 1993a) and Sampson and Laub (1995) proposed a theory of age-graded informal social control after analyzing the lives of the surviving members of the Glueck and Glueck (1950, 1968) delinquency study. Among the original cohort members they could locate these authors noted a trend – those who desisted from crime also happened to be those who experienced a turning point such as getting married, procuring gainful employment, or joining the military (Laub & Sampson, 1993a; Sampson & Laub, 1995). Explaining this pattern in desistance are changes in social control (Hirschi, 1969) exacted upon individuals experiencing these turning points – in each of the above three cases, an investment is made in some type of prosocial activity, an investment that would be lost if their criminal activity were to continue.

Further, their counterparts – those who continued to commit crime – were found to be living similar lifestyles to their younger delinquent selves. Laub and Sampson (1993a) contend this pattern reflects the fact that “delinquency incrementally mortgages the future by generating negative consequences for the life chances of stigmatized and institutionalized youths” (p. 306). Simply put, early consequences of crime (arrest and incarceration) generate more than short-term costs – these events reverberate across the



lives of those who experience them, leading to “failure in school, unemployment, and weak community bonds” (Laub & Sampson, 1993, p.306).

Figure 1 depicts a visual model of age-graded informal social control as proposed by Sampson and Laub (1995, pp. 244-245), adapted to explicitly include the influence of turning points on adult criminal behavior. Consistent with their emphasis on processes of social control, the model explains the initial onset of juvenile delinquency as an interactive function of structural background factors (e.g., socio-economic status of the family, family size, residential mobility) and individual differences (e.g., temperament, conduct disorder) both jointly influencing the degree to which an individual may be controlled by the social bonds with family and school, and the extent to which they are attached to delinquent influences. Juvenile delinquency reinforces weak attachments into adulthood, leading to crime and deviance as an adult, further eroding an individual’s bond with prosocial institutions (e.g., marriage and employment).

**\*\*\* FIGURE 1 HERE \*\*\***

Though weak adult attachments to prosocial institutions certainly influence an individual’s propensity to experience a turning point, the dashed vertical line between adult development and “desistance by default” is added to the figure to better exemplify Sampson and Laub’s (1995) contention that these structural catalysts (employment/marriage) are things that “happen” to an offender and are not events they intentionally endeavor to experience. Rather, a strict interpretation of their theory implies that those offenders experiencing turning points were practically no different than those who did not – both groups had extensive criminal histories, and continued to commit crime well into adulthood, showing little signs of cessation. In this, the turning point is

the fulcrum of their theory – those that become gainfully employed or get married are winners of a metaphorical lottery, thus going on to strengthen their attachments to prosocial institutions and increase the informal costs of their offending, leading toward desistance from crime. It is important to note, here, the absence of human agency – in no way, shape, or form are these offenders behaving or thinking in ways that lead toward crime cessation, hence the dashed-line “break” between adult development and desistance representing an exogenous process leading offenders toward desistance. The significance of this omission will be more thoroughly explored in the following section.

Subsequent research has bolstered but also conditioned these assertions. A bevy of research echoed the importance of turning points in the life-course (Edin, Nelson, & Paranal, 2001; Farrington & West, 1995; Horney, Osgood, & Marshall, 1995), even among particular sub-populations of offenders – e.g., sex offenders (Kruttschnitt, Uggen, & Shelton, 2000), cumulatively indicating that employment delays (Tripodi, Kim, & Bender, 2009) or practically eliminates (Wright & Cullen, 2004) recidivism and that marriage, controlling for other important factors, has a causal and negative effect on future crime (Beaver, Wright, DeLisi, & Vaughn, 2008; Laub et al., 1998; Sampson, Laub, & Wimer, 2006). Some studies have questioned the strength and nature of this relationship, noting that employment effects are dependent upon age (Uggen, 2000), the effect of marriage operates through its effect on peer associations (Warr, 1998), and that the desistance process is better thought of as a gradual decline in offending frequency and/or versatility (Bushway, Piquero, Broidy, Cauffman, & Mazerolle, 2001; McGloin, Sullivan, Piquero, Blokland, & Nieuwbeerta, 2011).

Applying their theory to the influence of program participation on desistance, it would appear that participation, regardless of its (in)voluntary nature, represents an investment in the future and, to a certain extent, an investment in the individual themselves. Even for those individuals ordered by the court to participate in a job training, substance abuse, or mental health program there is arguably some return to their human capital value, whether they explicitly recognize this or not. Further, if the end goal of these programs is gainful employment (explicitly for job training, perhaps implicit for substance abuse and mental health) this theory would suggest that desistance is conditional on these programs achieving their ends. For exemplars of program success (consistent, gainful employment) we would expect their criminal behavior to decline, either in its frequency (i.e., offending rate) or in its diversity (i.e., offense variety). In contrast, for those whom the program does not realize its end goals – i.e., the consistently under- or unemployed – we would then expect criminal behavior to amplify in frequency/diversity or, in the least, to return to pre-program levels.

#### II-4: COGNITIVE TRANSFORMATIONS & DESISTANCE

An alternative perspective on the desistance process proposes that cognitive transformations facilitate turning points but, even in their absence, lead to desistance from crime (Giordano et al., 2002; Maruna, 2001). Specifically, this perspective contends that feelings of hope for the future, shame and regret for past criminal actions, internalizing stigmas, and alternative identities (being a “good parent”, “provider”, or a “family man”) help to shape the desistance process, collectively enabling ex-convicts to

shed their past criminal identities in favor of non-criminal personas (Giordano et al., 2002; LeBel et al., 2008).

Figure 2 depicts a visual model of cognitive transformation theory, as exemplified by Maruna (2001) and Giordano et al. (2002). In contrast to Figure 1, this model pays little mind to the initial onset of crime, only indicating that prior criminal behavior interacts with a criminal self-identity (i.e., that one is a “criminal” and does not just commit “criminal” acts). This process then leads some offenders to undergo a cognitive transformation, whereby they intentionally distort prior events in their lives, interpreting past behavior as “bad” actions taken by an ultimately “good” person. In this, their underlying identity and personality remain unchanged – the past is merely intentionally misinterpreted to conform to the conceptualization of the self as an inherently prosocial person who, for a variety of reasons, engaged in actions counter to this perceived self. The cognitive transformation then leads to the adoption of prosocial roles – the “good” father or husband, or the “provider” – increasing the propensity for the individual to experience a turning point, thus leading toward desistance.

**\*\* FIGURE 2 HERE \*\*\***

However, it is here that this theory diverges most significantly from that depicted in Figure 1 – though turning points may play a role in desistance, this model treats them as an indirect influence, merely represent a “hook” for change that an individual can take advantage of. Further, a direct path to desistance extends from the cognitive transformation itself, meaning that, although a turning point may influence the propensity to desist, it is not a necessary event for desistance to take place. Additionally, the exogenous break in Figure 1 is absent here - this is because the cognitive transformation

model treats turning points as endogenous factors the individual prepares themselves for and intentionally pursues. That is, rather than playing a passive role in their lives (e.g., the desister by default from Figure 1) individuals having undergone a cognitive transformation play an active role in bettering their lives, seeking, engaging in, and performing the prosocial roles that are more attuned to their current perception of themselves.

Research has documented support for these arguments, finding that offenders who desist from crime indicate hope for their futures, expressing strong feelings of control over their future and “internal beliefs about their own self-worth and personal destinies” (Maruna, 2001). Similar research has also reported that offenders who express shame and regret concerning their past criminal actions also happen to be those who cease their criminal behavior (Leibrich, 1996; Paternoster, 1989), offenders who internalize stigma (regret criminal acts) while still preserving notions of their own self-worth fare better than their counterparts who also internalize, but fail to preserve self-worth (Ahmed, Harris, Braithwaite, & Braithwaite, 2001), and offenders who reshape their identities into non-criminal roles are better able to address difficulties in their lives without resorting back to crime (Maruna, 2001).

A particularly evocative exchange from Maruna (2001, p.154) perhaps best exemplifies the role of agency in redeeming oneself and creating the positive changes to one’s life that may not be credited to structural causes:

*“You know, like, I’m going to college and all that now, and do brilliant and I’m the best one on the course....And, em, it’s just amazing the turnaround you know what I mean? I just can’t believe. I’m buzzing with it. It’s not as if I’m doing it*

*'cause other people want me to do it. I'm doing it 'cause I want to do it. (male, age 33)''*

There exists no mention of a structural turning point but, rather, it is readily apparent that this individual is making determined strides on his own, by his own personal choice, to create a better future for himself through obtaining an education. Though one may argue that education presents a structural influence, it is not something one moves to obtain via passive participation. Instead, it requires motivated actions on the part of the individual to apply, attend classes, and study regularly to do well – one would be hard pressed to attribute this wholly to some kind of structural catalyst.

This evidence notwithstanding, research has also documented heterogeneous responses to these transformations, noting that the internalization of shame may produce depression and feelings of powerlessness over one's future (Maruna & Copes, 2005). Additionally, identifying the unique impact of cognitive transformations on desistance has thus far proved elusive, with most empirical assessments solely adopting either the cognitive transformation or turning point perspective. Farrall and Bowling (1999) argue that this has resulted in a false dichotomy, conceptualizing offenders as either “super agents” in total control over their lives and environment, or “super-dupes” who exert no agency of their own, awaiting the chance to react to opportunities as they come along in place of creating them. To date, just two studies have attempted to integrate these perspectives (LeBel et al., 2008; Rocque, Posick, & Paternoster, 2014), finding support for a dual-model explanation whereby cognitive transformations *and* turning points influence desistance. Unfortunately, limitations in both works prevented a) an examination of causal ordering (LeBel et al., 2008) or, b) a more thorough interrogation

of the influence of identity/cognitive transformations on desistance due to operationalization issues (Rocque et al., 2014).

However imperfect, this burgeoning evidence suggests that neither perspective wholly represents the process of desistance, but their total explanatory power might be greater than the sum of their respective parts. That is, it appears that weaknesses or gaps in one are complimented with strengths in the other, suggesting that an integrated approach might be the most fruitful path forward for desistance research.

## II-5: INTEGRATING STRUCTURE & AGENCY – AN IDENTITY THEORY OF CRIMINAL DESISTANCE

The complimentary nature of the turning point and cognitive transformation theories of desistance has bred a relatively nascent and limited literature on the manner in which the two competing theories may be integrated into one identity-based approach. As mentioned previously, LeBel et al. (2008) provide an empirical example of how these theories may be dually employed to explain criminal desistance, finding support for an integrated model whereby both structure (turning points) and subjective factors (cognitive transformations) simultaneously explain desistance. However, limitations prevented these authors from making strong causal inferences due to the fact that the observations used in the study were separated by ten years, the sample only consisted of 126 male property offenders, and that available covariates were restricted only to a small subset of relevant concepts – namely, criminal history, subjective mental states, and social problems. Nevertheless, LeBel et al. (2008) provided an integral first step toward the examination

of these competing perspectives, paving the way for future analyses to build upon their work (see Rocque et al. (2014) for a more recent analysis).

A theoretical example stems from the work of Paternoster and Bushway (2009), who proposed an identity theory of criminal desistance. Though their theory bears a number of similarities with prior conceptualizations of internal changes to self-identity leading to desistance (Giordano et al., 2002; Giordano et al., 2007; Maruna, 2001) it differs in several salient ways. First, prior theoretical propositions on cognitive transformations and desistance favored a more continuous interpretation of identity transformation, whereas Paternoster and Bushway (2009) view this process as more discrete, whereby an offender “casts off his old identity in favor of a new one” (p. 1107). For example, prior work by Giordano et al. (2007) posited that changes to self-identity are, in part, a socially driven process – individuals poised to undergo identity change are driven towards or away from their “potential self” by the interactions with those in their social circle, most notably their romantic partners. In contrast, Paternoster and Bushway (2009) contend that changes to one’s social circle – e.g., replacing delinquent/criminal associates with more conventional ones – are a result of an individualistic process of self-change, noting that the “conventional social relationships and role-taking” noted by Giordano et al. (2007) are only accessible “after offenders *first decide to change* [emphasis in original]” (p.1106).

Second, Paternoster and Bushway (2009) interpret the process of identity change as significantly more of a cognitive break with the past self than does Maruna (2001), who contends that desisting offenders cognitively distort their own views of past criminal behavior to align with their current prosocial views of themselves as an inherently “good”



person who has done “bad” things. The difference between these approaches is that, under the latter perspective, the conceptualization of the self is continual and unchanging due to deliberate distortions of past behavior where in the former perspective the individual intentionally sheds their past personality in favor of a new one after they come to the realization that their current behavioral trajectory will indelibly lead them to become a “feared self”. Further, the authors posit that this change comes about as a result of a “crystallization of discontent” whereby the individual realizes that the returns of criminal behavior are small, seldom, and precariously gained, thus desiring a new way of living, skirting the past in favor of a more promising future (Paternoster & Bushway, 2009).

Structural catalysts for change play a role in their theory, but they are of relatively lesser importance compared to the perspective proposed by Laub and Sampson (1993). In congruence with the cognitive transformation perspective, Paternoster and Bushway’s (2009) identity theory conceptualizes structural turning points such as employment or marriage as “hooks” for change, rather than direct causes. That is, rather than a turning point leading toward “desistance by default” (Laub & Sampson, 1993), an identity theory of desistance instead views the “upfront” work the individual undergoes to break with their past self as most critical – potential turning points absent these changes to the self merely influence behavior (crime) and not criminality (the propensity to commit crime) (Paternoster & Bushway, 2009).

Connecting the extant theoretical contributions of cognitive transformations and integrated identity theories of desistance to the current research yield several expectations for the impact of program participation on desistance. As opposed to the age-graded

informal social control approach, cognitive transformation/identity change theories have much more in common with signaling arguments and would therefore predict that those voluntarily participating (i.e., those sending a signal) in job training, substance abuse, or mental health programs have already begun the desistance process through a cognitive break with their past criminal self, and that the choice to participate is merely the individual now acting in accord with their new identity. This would indicate that attitudes associated with desistance – aspirations for employment, family, and law-abiding behavior, hope for the future, and the motivation to succeed – should be uncorrelated with program participation for voluntary participants, as the relevant changes to these attitudes should pre-date their participation or, in the least, be unaffected by it. Further, I would also expect that the outcome of the signaling process – here thought to be gainful employment – is not a necessary condition for desistance among voluntary participants, but might be for those ordered by the court to attend these programs. This is reflective of the notion that, though crime may be temporarily dampened for involuntary participants due to employment, their underlying criminality remains unchanged and, absent gainful work, they would be expected to return to their criminal careers post-participation.

## II-6. QUALITIES OF SIGNALS, SIGNALERS, AND THE SIGNALING ENVIRONMENT

Since its proposal, numerous qualifications and caveats to the pure form of signaling theory have been suggested, and these can be roughly separated into four categories regarding the qualities/characteristics of 1) signals, 2) signalers, 3) receivers, and 4) signaling environments (Connelly, Certo, Ireland, & Reutzel, 2011). Within each

category, one can easily isolate at least one quality or characteristic that may place those with criminal records at a pre-existing deficit when it relates to the likelihood of their signals to convey the information intended and further, to realize the ultimate goal of the signaling process in an employment context – the procurement of gainful employment.

Signals have a number of qualities that influence their effectiveness including, but not necessarily limited to, their observability (Lampel & Shamsie, 2000; Warner, Fairbank, & Steensma, 2006), cost (Bhattacharya & Dittmar, 2003; Certo, 2003), frequency (Baum & Korn, 1999; Carter, 2006), fit (Busenitz, Fiet, & Moesel, 2005; Zhang & Wiersema, 2009), and consistency (Chung & Kalnins, 2001; Fischer & Reuber, 2007; Gao, Darroch, Mather, & MacGregor, 2008). Of most direct importance for signaling in the criminological context, however, are the latter two qualities – the fit and consistency of signals.

The *fit* of a signal has been defined as the “extent to which the signal is correlated with unobservable quality” and is differentiated from the term *honesty* which is defined to be a characteristic of the signaler, and not the signal itself (Connelly et al., 2011, p.53). Signal fit has been investigated in the context of venture capitalism, examining the degree to which new venture teams (NVTs) pursuing lucrative opportunities receive investments from venture capitalist (VC) firms (Busenitz et al., 2005). Specifically, Busenitz et al. (2005) interrogated whether signals sent by members of the NVT – in the form of personal financial investment in the venture – were correlated with outcomes of said venture in the ten years following initial VC investment. Their results indicated that personal investment was an *unfit* signal, as greater levels of investment were uncorrelated with venture outcomes, suggesting that personal investment is either a) not a signal or, b)

a weak, or semi-fit signal that only partially sorts high and low quality firms (Busenitz et al., 2005).

Research has also examined the role of chief executive officer (CEO) certification of firm financial documents in regard to its potential stock market returns on a firm's publicly traded value (Zhang & Wiersema, 2009). Capitalizing upon a natural experiment, Zhang and Wiersema (2009) analyzed firms whose CEOs did and did not certify a firm's financial documentation prior to a deadline imposed by the Securities and Exchange Commission (SEC) after corporate scandals in the early 2000s (Enron, Worldcom, Tyco, etc...). Their results indicated that CEO certification by the deadline did, indeed, positively influence a firm's stock price, but this effect was conditional on attributes of the CEO themselves such as the amount of shares they held in the company and the number of external directorships they held (i.e., sitting on executive boards of other companies) (Zhang & Wiersema, 2009).

Collectively, these results imply that the *fit* of individual signals might not be enough to establish that they are, in fact, representative of some underlying quality in the signaler. Instead, it appears that receivers might search for additional signals to (dis)confirm the meaning of a singular signal and, that signalers are likely better served by sending multiple signals contemporaneously. Applied to the criminological context, this means that individuals with criminal histories are unlikely to be successful in their signaling endeavors if they are only sending one signal at a time. In fact, one might easily argue that reporting a criminal record and a signal of desistance (whatever form this may take) simultaneously communicates a lack of *consistency* in signals, even if the criminal signal is an older one (i.e., for an offense many years ago) or it stems from a trivial

incident. Therefore, multiple positive semi-fit signals might be necessary to counter this inconsistency.

The (in)consistency of signals has itself been the topic of scholarly inquiry (Chung & Kalnins, 2001; Fischer & Reuber, 2007; Gao et al., 2008), and is defined as “the agreement between multiple signals from one source” (Connelly et al., 2011, p.54). Gao et al. (2008) examined initial public offering (IPO) prospectuses of 57 biotech firms between the years 1997 and 2002, analyzing the consistency of signals communicated by new IPO firms as they related to stock market returns in their first 30-day windows post-IPO. Their results indicated that the type of signal – defender (narrow strategies targeted to specific markets) or prospector (broad strategies targeted to various opportunities in the market) – and the consistency of these signals across three dimensions of the firm (product/market, technological, administrative) influenced 30-day stock returns significantly, but in opposite directions for types of signals. In turn, inconsistent signals failed to produce desired results (level of stock underpricing during initial 30-day windows) for the different IPO strategies (defender/pro prospector) because they were not effective in reducing information asymmetry – i.e., convincing investors that the IPO has a consistent “defender” or “prospector” strategy (Gao et al., 2008).

In an evaluation of the Texas lodging industry, Chung and Kalnins (2001) discover similar results. Namely, hotels that consistently signal their quality through an affiliation with larger chains, their choice of location (rural/urban), their location relative to other chain-affiliated hotels, and in advertising expenses tend to report higher returns per room than do hotels sending inconsistent signals across these dimensions (Chung & Kalnins, 2001). Further, in a theoretical proposition of organizational reputation

development among new firms based on extant research on signal processing, Fischer and Reuber (2007) argued that a new firm's signals are likely to produce desired their desired effects (positive reputational gain) so long as these signals are collectively evocative of a pre-existing category for which receivers already have some point of reference. For example, "Silicon Valley start-up" is a well-known category for new firms, and is associated with a certain set of characteristics meant to define what such a "start-up" looks like and how it is expected to operate. Logically, those new firms who send numerous and consistent signals aligning themselves with the perceived characteristics of the prototypical "Silicon Valley start-up" (i.e., location, market strategy, organizational structure, etc...) should be more effective in reducing initial information asymmetry among investors, since these new firms will gain from pre-existing reputational development of similar firms.

Taken together, the extant research on signal *consistency* yields important implications for signaling in the context of employment for those with criminal histories. Though not necessarily an intentional signal, a criminal background assuredly transmits a negative message to potential employers, signaling that an applicant may be dangerous, untrustworthy, or otherwise unsuitable for employment. If this signal is received along with a positive signal (i.e., job training program completion, voluntary program participation, certificates of rehabilitation) it is quite clear that these disparate messages are inconsistent, and we would therefore expect the desired results of the signaling process in an employment context (i.e., gainful employment) to be achieved almost arbitrarily, dictated more by randomness than the influence of positive signals. However, it does seem possible for an individual with a criminal history to counteract this by

providing multiple positive signals of their underlying “desister” status to increase the consistency of their signals, which would require a great deal of effort and opportunity costs to obtain in order to effectively differentiate themselves and reduce information asymmetry for potential employers.

Further, signalers themselves have been the focus of analysis, examining their honesty (Arthurs, Busenitz, Hoskisson, & Johnson, 2009; Arthurs, Hoskisson, Busenitz, & Johnson, 2008; Ndofo & Levitas, 2004) and reliability (Busenitz et al., 2005; Sanders & Boivie, 2004) with respect to the signal(s) they transmit. Signal *honesty* is defined as “the extent to which the signaler actually has the underlying quality associated with the signal” (Connelly et al., 2011, p. 46) and has also been referred to as signal *veracity* in the extant signaling literature (Busenitz et al., 2005). In an investigation of lockup periods (i.e., the length of time IPO owners are prohibited from selling their stocks) in new IPO ventures, Arthurs et al. (2009) found that the length of the agreed upon lockup period influenced IPO underpricing. IPOs face a particular dilemma in regard to their signaling *honesty* going into a public stock offering – given their newness, they have had little opportunity to develop a positive reputation, and thus must signal their quality through publicly available prospectuses. Absent other available signals (prestigious stock underwriters or executive board members, for example) the IPO can use the length of the lockup period as a signal of owner confidence in the quality and future prospects of the firm. In this, owners can enhance their perceived *honesty* by taking on the opportunity cost of a lengthy lockup period, where they are forbidden from liquidating their positions in the firm absent express permission from the underwriter of their stock (Arthurs et al., 2009). Stated simply, by taking on a cost to themselves, owners of a firm can signal the

underlying quality of their firm in its IPO, thereby resulting in a lower degree of stock underpricing as a result of lesser information asymmetry for initial investors (i.e., the honesty of the signalers reduces uncertainty surrounding the quality of the firm).

Further, Ndofor and Levitas (2004) provide a conceptual method of understanding which kinds of signals are more effective under different levels of firm and environmental uncertainty and their unique combinations. As an example, they argue that strategies should differ for high- and low-uncertainty firms (i.e., their reputation in the industry) in varying levels of high- and low-uncertainty in their market environments. An intuitive example of a firm with high individual uncertainty operating within a highly uncertain environment would be a new technology firm during the “dotcom” boom of the late 1990s. Since this market was entirely new, and thus firms within it just as novel, there existed heightened levels of uncertainty for investors to consider involving not just the newness of individual firms, but of the future prospects of the market environment itself (Ndofor & Levitas, 2004). Therefore, it is argued that new firms within such an uncertain market would be best served by sending signals of their quality that may also influence environmental uncertainty, so as to enhance their own perceived *honesty* as signalers while at the same time reducing information asymmetry for the market as a whole.

Though it is difficult to envision a clear analogue of firm and market level uncertainty in the context of employment among individuals with criminal histories, it is a small step in logic to argue that individuals with criminal histories are a clear equivalent to new, high-uncertainty firms. For each, future prospects are shaky at best, and signals transmitted by either are apt to be interpreted with great deal of scrutiny, since their



“newness” (or in the case of individuals with criminal histories, their prior negative signals) prevents receivers from interpreting their behaviors as the signals they are intended to be. Additionally, it could easily be argued that the honesty of an individual with a criminal record is already in question, increasing the uncertainty a receiver attributes to them and, as a result, failing to effectively diminish information asymmetries regarding their true “desister” status.

Signaling scholars have also posited that signalers can have varying levels of *reliability*, defined as “the extent to which the signaler is honest and the signal corresponds with signaler quality” (Connelly et al., 2011, p.52) which has also been referred to as signaler *credibility* (Davila, Foster, & Gupta, 2003). As is evident in its definition, *reliability* is a function of both signaler (*honesty*) and signal (*fit*) qualities. Sanders and Boivie (2004), in their analysis of new internet firms and their valuations (i.e., their estimated worth on the stock market) determined that new firms could use a number of signals (stock-based incentives, firm ownership, board structure, etc...) to differentiate themselves from their competition, thus resulting in greater valuations for new internet firm IPOs than their counterparts sending fewer of these signals. Further, the age of a firm appeared to moderate the types of signals early investors prioritized post-IPO and this is arguably due to a firm’s perceived reliability. Specifically, certain signals when the firm is new (such as the degree of outside agents on their board of directors) are more important than when the firm is older, potentially indicating that prestigious board members from outside of the company (i.e., those that were not involved in starting the firm) are essential for establishing a firm’s *reliability* early on, but become less salient as more objective information about the firm becomes available (Sanders & Boivie, 2004).

Connecting this literature to signaling among those with criminal histories, it would appear that, since their signaler *honesty* is in question, the *fit* of their signals, no matter how strong, is likely to be diminished by receivers' perceptions of individuals with criminal records in the aggregate (i.e., that they lack honesty, are unreliable, and potentially dangerous). The findings of Sanders and Boivie (2004) would then imply that such individuals would benefit greatly from outside recognition of their unobservable status as “desisters” in the form of state-sponsored certificates of rehabilitation, whereupon these institutions “lend” a bit of their credibility to those lacking it, until they can accumulate enough to stand on their own (Maruna, 2012). Unfortunately, such practices are relatively rare in the United States, and when such certificates (or, alternatively, pardons) are available, they are hidden behind a “labyrinth of bureaucracy” thus making them nigh impossible for the average individual with a criminal record to obtain (Maruna, 2012, p. 78).

Finally, both signal receivers and the signaling macro-environment have characteristics that influence the signaling process – namely the amount of “noise” introduced by or into each of these signaling components (Branzei, Ursacki-Bryant, Vertinsky, & Zhang, 2004; Perkins & Hendry, 2005; Rynes, Bretz, & Gerhart, 1991; Zahra & Filatotchev, 2004). Receivers may introduce noise through their interpretation of the signals but also their perceptions of the broader category of signals/signalers being presented to them. As an example, Perkins and Hendry (2005) discovered that, among members of corporate remuneration committees (REMCOS) charged with dictating executive pay structures, members tended to weight different sources and kinds of information (including signals) differently when coming to their decisions. Similarly, in a

longitudinal study of job seekers, Rynes et al. (1991) found that signals sent by recruiters were interpreted through a lens of prior knowledge of their company or even the perceived “functional area” of the recruiter themselves. Prior positive knowledge of a company could offset the negative signaling experience of poor recruitment (and vice versa) and job seekers might discount signals entirely if the recruiter has no working knowledge of their field (e.g., an engineer being recruited by someone with no background in engineering).

In addition to the potential noise injected into the signaling process by receivers themselves, the macro signaling environment could be susceptible to other sources of noise either inside (Branzei et al., 2004) or outside of it (Connelly et al., 2011; Zahra & Filatotchev, 2004). In reference to the former, internal sources of noise might be other signalers – as Branzei et al. (2004) find, an organization’s orientation toward environmentally conscious business strategies can be influenced by signals sent by individuals at various levels in the organization’s hierarchy. For example, preliminary results (i.e., the initiative is thus far successful or a failure) might be interpreted and acted upon differently depending upon the actions of signalers in the upper echelons of the company. If an “individual champion” (e.g., an executive who supports the initiative) strives to push forward regardless of poor initial results, preliminary indications of failure are interpreted less negatively by others in the organization (Branzei et al., 2004).

In contrast, “external referents” might influence the signaling market itself (Carter, 2006; Sanders & Boivie, 2004; Zahra & Filatotchev, 2004), particularly by casting doubt over the “fit” of a family of signals or the “honesty” of a particular group of signalers. This could create a situation in which “certain signaler-receiver pairs may

interact to yield especially effective, or ineffective, signaling” (Connelly et al., 2011, p.62). Further, signaling scholars have argued that the level of noise introduced into the signaling environment could seriously undermine the value of signaling (Jiang, Belohlav, & Young, 2007; Zahra & Filatotchev, 2004) – this would, in the least, mean that the incentive to signal is reduced because the distortion introduced into the environment precludes receivers from acting upon them effectively. Alternatively, signalers may still make attempts to transmit messages of their unobservable qualities to receivers in the hopes that the repeated transmission of the signal eventually yields success in the long-term (one might even argue that this perseverance is a signal, in and of itself), even though short-term outcomes do not reinforce this behavior.

Given the manifold findings reviewed above, one may then expect to find little relationship between signals sent by desisting ex-offenders, perhaps even signals with support from legitimate institutions (e.g., a “certificate of rehabilitation” from the state or a state actor), and the intended effects of these signals (namely, employment) due to the prevailing effects of what one might argue are these individual’s “first” signals – their prior criminal behavior. First, even if voluntary program participation presents itself as a “fit” signal, an employer can receive both this signal and knowledge of the individual’s criminal record contemporaneously – though it is not completely deterministic that the receiver would prioritize the “criminal” signal, it yet remains that these two disparate pieces of information convey an inconsistent and confusing message for the employer to interpret. Further, popular media is flush with references to dangerous criminals, collectively contributing to a disparaging view of ex-offenders by the general public and, thereby, a “noisy” signaling environment. Probabilistically speaking, the criminological

world knows that these depictions are gross caricatures of reality and it might even be said that employers are (un)consciously aware of this as well – however, the proverbial “bell” cannot be un-rung, and the damage to the signaling environment (i.e., labor markets) is unlikely to be undone without greater effort than it took to damage it in the first place. Though, more recently, efforts have been made to correct these portrayals, and to reform our criminal justice system to a more balanced approach (i.e., more attention devoted to rehabilitation than has been true in the last few decades) these efforts have been counterbalanced by the wide-reaching stigmatic potential of internet criminal history databases which, as research has shown, are prone to errors and prove quite difficult to correct.

No matter the “fit” of the signal or the “honesty” of the signaler, then, does it seem as if individuals with criminal records will be largely successful in their signaling endeavors, at least in an aggregate sense. Intuiting from extant signaling literature, one would expect that their signals are inconsistent, their honesty as signalers is in question, that receivers interpret their signals through a tainted lens, and that their signaling environment is often too noisy for receivers to distinguish the “wheat from the chaff.” However, this does not indicate that signaling should not occur, nor that a test of signaling theory among individuals with criminal records is an effort in futility. Rather, this calls for a body of evidence concerning *if* receivers *should* interpret certain behaviors of those with criminal records as signals. This requires a thorough interrogation of the micro-level assumptions of signaling theory for, without first establishing that the proposed behaviors should be interpreted as signals, aggregate tests of the theory could

not determine if null effects are due to false signals or because these signals are distorted in some way by receivers or external referents to the signaling environment.

### **III. FULL SAMPLE**

This analysis uses data on participants involved in the Pathways to Desistance (PtD) study, a longitudinal study of seriously criminal youth from adolescence into early adulthood. At the time of sample selection, all youth were between the ages of 14 and 18 years old and were found guilty in juvenile or adult court of a serious offense (the majority of offenses being felonies, with a very small subset of cases adjudicated guilty of misdemeanor property crimes, sexual assault, or weapons-related charges) in Maricopa County, AZ, or Philadelphia County, PA. The total sample consists of 1354 adolescents, approximating roughly a third of all adjudicated 14 to 18-year-old youth during the sampling time frame of November 2000 through January 2003. Baseline interviews took place approximately one month after their adjudication and participants were initially re-interviewed every six months for the following three years, then annually for the proceeding final four years of the study, resulting in ten waves of data collection (Schubert et al., 2004).

In addition to interview data, life calendar data was obtained for each participant across several domains including antisocial activities, education, gainful activity, head injuries, living arrangements, making and spending money, romantic relationships, community based services participated in, contacts with the justice system, court monitoring services, medication, and out of community placements. Because of the finely tuned temporal nature of this life calendar data, the current analysis will be better able to pinpoint significant events during the course of these adolescents' transition to early

adulthood. For example, if a youth were to become gainfully employed for the first time between interviews and life calendar data were not available (specifically, the “making and spending money” calendar) this analysis could only treat the individual as employed from the point of that interview onward (provided of course they continue to report being employed) as is typical with most longitudinal data sets. However, the life calendar indicates the specific month during the follow-up period in which the individual became employed, allowing the analysis to control for the proportion of time between interviews that the individual was engaged in legitimate (or illegitimate) work. Perhaps most importantly, the out of community placements calendar will allow this analysis to control for street time – i.e., the time a youth is living in the community and, therefore, is at risk for criminal events. This will aid in inference by preventing erroneous conclusions that an individual has desisted or demonstrated a marked decline in criminal activity when in reality they have simply not been at risk for crime because they were under some form of correctional supervision throughout most or all of the follow-up period in question.

The baseline, follow-up, and life calendar data are also supplemented with official record information. These data were initially obtained through a hand-review of court documents in Philadelphia and an automated court reporting system in Phoenix (JOLTS – Juvenile On-Line Tracking System and ICIS – Maricopa County Superior Court database for adult court information). Official record information includes juvenile court appearances prior to the appearance initiating sample selection, information concerning the adjudication bringing the youth into the sample, any arrest or court petition occurring after the baseline interview, and court-ordered juvenile placements. Additionally, PtD

administrators supplemented this data with FBI records on a yearly basis in order to capture arrests occurring outside of Arizona and Pennsylvania (Schubert et al., 2004).

Table 1 provides summary statistics of the sample at baseline and for a subset of the follow-up interviews (12, 24, 48, and 84 month follow-ups) conditional on the completion of that specific interview (i.e., an individual who did complete the 6- and 18-month interviews but not the 12-month interview does not contribute to the 12-month descriptives). The average age at the outset of the survey is 16.04, most participants are male (86.41%) and a small majority of cases are initially from Philadelphia County (51.7%) but as participants leave the survey this majority is ceded to Maricopa County (51.23% at 84-month interview). A plurality of participants in any given follow-up interview are black (38.8 – 41.43%) with the second largest group being Hispanic (33.53 – 34.74%). The average proportion of one's peers (identified as the four closest friends) ever arrested in any given follow-up period tends to decline slightly over time, while the proportion ever jailed exhibits a larger, but similar, decline to arrests. Accordingly, Likert scales gauging the delinquency of one's peer group (none, some, most, all) decline as well throughout the study period.

**\*\*\* TABLE 1 HERE \*\*\***

In terms of the signal proposed for this analysis - voluntary participation in a job training, mental health, or substance abuse program - rates of participation (irrespective of the type of participation) in any given wave remain fairly low, whether it be court-ordered or voluntary, with the highest rates coming from mental health treatment programs (6.20% at 12-month follow-up). Naturally, when considering *only* voluntary participation, these rates decline, and, at times, do so considerably. In the realm of



signaling theory, however, this is preferred, as one of the necessary conditions put forth by Spence (1973) regarding signals is that the population sending them be small compared to the aggregate population they are situated within.

Additional statistics reported in Table 1 correspond to the average proportion of time spent at home (typically referred to as “street time”) and that spent in secure settings, including both juvenile and adult corrections institutions in addition to secure facilities dedicated to drug, alcohol, or psychiatric treatment. At the 12-month follow-up participants spend nearly the same amount of time in either type of setting (.571 at home, .426 in secure settings) but this diverges quickly into the 24-month follow-up, where participants spend an average of about 2/3 of their time at home and 1/3 of their time in secure settings, and stabilizes thereafter.

After the baseline interview, youth reported the approximate number of crimes they committed since the prior interview, statistics for these being represented in Table 1 in the form of counts and variety scores for all crimes, property crimes, and violent crimes.<sup>4</sup> It is important to note that the statistics for the 12- and 24-month follow-ups are based upon 6-month recall periods while the 48- and 84-month follow-ups use a 12-month recall. I would expect, a priori, that longer follow-up periods should yield elevated counts and variety scores, given the increased amount of time in the recall period but, this expectation is only partially supported. The average count of all crimes committed does

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<sup>4</sup> The crimes included for these statistics include destruction of property, setting fire to a building, entering a building to steal, shoplifting from a store, bought/received/sold stolen property, used checks/credit cards illegally, stole a car or motorcycle, sold marijuana, sold other illegal drugs, carjacked someone, drove drunk or high, paid someone for sex, forced sex on someone, shot someone, shot at someone, murdered someone, took something by force using a weapon, took something by force not using a weapon, beaten someone up badly enough for them to go to the hospital, been in a fight, beaten up someone as part of a gang, carried a gun, broke into a car to steal something, and stole a car to ride around. Statistics are further separated into property/violent distinctions.

increase from the 24-month follow-up into the 48-month follow-up but only by six crimes, the variety score of all crimes increases by .07, the average count of violent crimes increases by one, and the violent crime variety score increases by .02. In contrast, property crimes drop going into the longer recall period, the count going from 38.63, on average, to 35.64 and the variety score dropping by .013. Keeping in mind the longer length of later follow-up periods, it is important to recognize that average counts and variety scores exhibit a marked decline over time, hitting their lowest levels at the 84-month follow-up *even though* the recall periods double in size. It appears, then, that the average PtD participant is committing fewer crimes over time, in addition to declining in their offending versatility. An alternative explanation for this trend, however, might be that the highest frequency offenders and those who are most versatile in their offending habits are dropping out of the analysis, thus causing these averages to decline dramatically.

As with all longitudinal surveys, the issue of sample attrition could present considerable concerns regarding the validity of estimates, perhaps especially with a sample of serious youth offenders, a population not known for their long-term accessibility. Tables 2 and 3 provide summary statistics for interview completion from baseline to the final follow-up interview at 84 months. As is evident from Table 2, most of the baseline sample completes the interview at any given follow-up period but it is clear that this trends downward over time dipping below 90% for the 48-month follow-up and continuing downward from there, hitting a low of 83.53% at the final interview. Additionally, these losses are clearly not due to a rise in partial completion, but in the complete absence of baseline sample members from the follow-up interviews.

Regardless, survey administrators managed to ensure that a vast majority of the baseline sample completed their interviews during every follow-up, indicating that, while attrition may yet present itself as an issue the following analyses needs to address, this issue is, *a priori*, far less serious than this author had anticipated.

**\*\*\* TABLES 2 & 3 HERE \*\*\***

While Table 2 speaks to the veracity of general retention in any given interview, Table 3 instead examines the number of *total* interviews we might expect each baseline sample member to complete. Here, too, we see that attrition is less of an issue than one might anticipate with such a sample – a full 61.23% of participants completed all ten interviews, 78.81% completed 9 or more, and 86.12% completed 8 or more, and 94.98% of participants completed at least half of the scheduled follow-up interviews. Though it is clear that a subset of participants discontinued their participation either immediately after the baseline interview (1.33%) or shortly thereafter (1.33% missing at 6-month to 5.02% missing at 30-month) it is nevertheless the case that a substantial majority of participants completed 8 or more interviews.

## **IV. PRIMARY INDEPENDENT VARIABLES & HYPOTHESES**

### **IV-1. SIGNALING VARIABLES**

*Program Participation & Signals.* For the current analysis, I consider three types of program participation – job training, mental health, and substance abuse – this is because these three programs collectively account for 86% of program participants and, among the other types of programs included in the PtD data, the explicit purpose of these

programs is considerably less ambiguous.<sup>5</sup> For each respondent reporting participation at some point in the follow-up period, I isolate the first recall period during which participation begins and then evaluate the nature of their participation on a monthly basis. Nature of participation is then categorized into three groups – 1) voluntary (all months of participation are reported as voluntary), 2) involuntary (all months of participation are reported as involuntary) and, 3) mixed. The final category – mixed – consists of respondents who participate in multiple programs simultaneously with at least two of these programs being either A) entirely voluntary or, B) entirely involuntary.

The cost of participation (i.e., the opportunity cost) is measured by summing participation frequency – the number of times the respondent attended the program during each month of participation – across all types of programs within the first recall period the respondent reports participation. For simplicity in the figures in the proceeding section, this variable is split at the median – all respondents reporting participation frequencies above the median (13) are assigned a value of 1, while all below the median are assigned a value of 0.<sup>6</sup>

## IV-2. HYPOTHESES

In full consideration of the extant research on signaling, and research in the criminological realm on desistance, the influence of employment on crime, and reentry in

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<sup>5</sup> As opposed to job training, mental health, and substance abuse programs, the purpose of the remaining types of programs (community support groups, school guidance, mentoring, in-home counseling) are somewhat ambiguous. Regardless, the total numbers of participants in these programs are very small and their inclusion does not influence the results from the following analyses.

<sup>6</sup> Control group cases are randomly assigned a value of one or zero on this variable, then the random assignment is tested across all randomly selected control groups. This procedure is fully detailed in Appendix A.

general, the following sections will be primarily concerned with testing the micro-level assumptions of signaling theory, focusing centrally on four hypotheses:

**Hypothesis 1.** It is expected that program participation, voluntary or not, should exert some impact on the employability of participants, but not necessarily on attitudinal measures associated with desistance for those who participate of their own volition. Thus, pre- and post-signaling attitudinal measures should remain largely unchanged for voluntary participants, but should exhibit some change for involuntary or mixed participants as their orientations toward crime (and analogous behaviors) are redirected.

**Hypothesis 2.** The expectations of Hypothesis 1 should be most pronounced for those voluntary participants for whom the costs of acquiring the signal are larger. That is, the degree to which attitudinal measures associated with desistance remain unchanged pre- and post-program participation should be more evident among those individuals whose participation is comes with a higher opportunity cost. Thus, participants who spend comparatively little of their time in the program are paying less of a cost than an individual spending much more time in the program.

**Hypothesis 3.** Conditional on the rate of part-time or better employment (low, medium, and high), the effect of program participation on subsequent desistance patterns should vary according to whether program participation was voluntary, involuntary, or mixed. Specifically, I expect that main and interaction effects for the INVOL and MIX groups

will exhibit variation in accord with their level of post-program employment, while effects for the VOL group should be similar across all levels of employment, emblematic of their underlying intentions to desist as signaled by their voluntary program participation.

**Hypothesis 4.** The degree to which this estimate varies should be correlated with signaling costs. That is, VOL participants whose involvement in the program has greater intensity (i.e., more frequent attendance) should exhibit more pronounced differences in desistance patterns when compared to those who participate more infrequently. Additionally, this should hold true regardless of the success of the signal (i.e., at all rates of employment). I would also expect coefficients to differ between low and high frequency participants in the INVOL and MIX groups, but these estimates should be conditional on the level of employment, consistent with a human capital explanation (i.e., age-graded theory of informal social control) for desistance.

## **V. MEASURES AND METHODS – EFFECT OF PROGRAM PARTICIPATION ON ATTITUDINAL MEASURES (HYPOTHESES 1 & 2)**

### **V-1. DEPENDENT VARIABLES**

***Aspirations for Work, Family, and Law (AWFL).*** Measured at baseline and all following interviews, the AWFL, originally developed by Menard and Elliott (1996), taps into the respondent's perceptions of their future success. There are a total of 14 items that measure two the two dimensions of the AWFL – 1) aspirations for work, family, and law

abiding behavior and (7 items), 2) expectations for work, family, and law abiding behavior (7 items).

The first sub-dimension involves the importance the respondent places upon several facets of their perceived adult life including – 1) having a good job or career, 2) graduating from college, 3) earning a good living, 4) providing a good home for their family, 5) having a good marriage, 6) having a good relationship with their children and, 7) staying out of trouble with the law. The second sub-dimension uses the same facets as above, but instead asks the respondent to indicate how likely they are to reach these goals and, in this, is more a measure of expectations than of an individual's desire. All 14 items use a 5-point Likert scale (from 1 “Not at all important/Poor” to 5 “Very important/Excellent”, and final measures are averages computed across the individual items in the sub-dimension resulting in an average measure for each dimension (respondents must have at least 5 valid values on these items for a mean to be computed). These scores are then differenced across the recall periods coming directly before ( $t-1$ ) and after ( $t+1$ ) the recall period where initial participation occurs ( $t=0$ ) in order to assess if, and to what extent, program participation exerts an impact on a respondent's aspirations or expectations about their future.

I would expect, a priori, that the first sub-dimension should remain stable for signalers, and perhaps display some change for non-signalers as program participation redirects their orientation toward their adult life (if this is indeed true). In contrast, expectations might be similarly changed across signaling and non-signaling samples – this is because expectations might be more grounded in reality than are aspirations. If program participation exerts some effect on an individual's employability or marriage

prospects and if expectations are at least in part grounded in reality, then it stands to reason that expectations may change for both signalers and non-signalers alike.

***Future Outlook Inventory (FOI).*** The FOI is measured at baseline and all following interviews, and was originally developed by Cauffman and Woolard (1999) which used as a foundation for this scale research conducted by Scheier and Carver (1985), Strathman, Gleicher, Boninger, and Edwards (1994), and Zimbardo (1990). Respondents were asked to indicate, across 15 items, the extent to which each statement (e.g., “I will keep working at difficult, boring tasks if I know they will help me get ahead later”) corresponded to their typical behavior (1 “Never True” to 4 “Always True”). Final scores on this variable are computed from 8 items that reliably coalesce into a single factor across recall periods – a mean is computed across these items so long as the respondent has valid data on at least six of them. Items were reverse coded as necessary such that a higher score on this measure indicates a greater orientation toward the consideration of the future and planning for it. These scores are then differenced across the recall periods coming directly before ( $t-1$ ) and after ( $t+1$ ) the recall period where initial participation occurs ( $t=0$ ) in order to assess if, and to what extent, program participation exerts an impact on a respondent’s orientation toward their future.

***Motivation to Succeed (MtS).*** The motivation to succeed items, originally developed by Eccles, Wigfield, and Schiefele (1998), are administered at the baseline and all following interviews. The MtS consists of six items that tap into respondents’ level of agreement



with the following statements: 1) “In my neighborhood it is easy for a young person to get a good job”, 2) “Most of my friends will graduate from high school”, 3) “In my neighborhood it is hard to make money without doing something illegal”, 4) “College is too expensive for most people in my neighborhood”, 5) “There is not much opportunity to succeed as kids from other neighborhoods” and, 6) “My chances of getting ahead/being successful are not very good.” Scores from all items are combined into a singular measure computed by taking the average of these six individual measures. These scores are then differenced within the recall periods coming directly before (to t-3 recall periods) and after (to t+3 recall periods) the recall period where initial participation occurs (t=0) in order to assess if, and to what extent, program participation exerts an impact on a respondent’s motivation to succeed.

## V-2. ADDITIONAL INDEPENDENT VARIABLES

***Brief Symptom Inventory (BSI).*** The BSI is administered at the baseline and all following interviews. It is derived from research by Derogatis and Melisaratos (1983), and consists of 53 self-report items involving the extent to which participants have been “bothered” (on a Likert scale from 0 “not at all” to 4 “extremely”) by various psychological symptoms in the week leading up to the interview. Within the 53 items are nine subscales corresponding to somatization, obsessive-compulsive, interpersonal sensitivity, depression, anxiety, hostility, phobic anxiety, paranoid ideation, and psychoticism symptoms. Since these items exhibit fairly high intercorrelation, a factor score was computed across all nine subscales (eigenvalue=5.4, all factor loadings exceed

.65)<sup>7</sup> in order to measure overall psychological distress.<sup>8</sup> As is noted by the PtD administrators, a bug in the programming code of the survey resulted in some participants not being asked these questions (about 27% of person-recalls missing, including sample attrition) – in order to retain these cases for the measures that are available among them, a score of 0 is imputed on the BSI factor, and a dummy variable flagging missing cases is included in all analyses.

***Contact with Caring Adults (CCA).*** The CCA inventory, implemented at the baseline and all following interviews, was derived from multiple sources (cites here), and contains eight domains involving contact with 1) adults you admire and want to be like, 2) adults you could talk to if you needed information or advice about something, 3) adults you could talk to about trouble at home, 4) adults you would tell about an award or if you did something well, 5) adults with whom you can talk about important decisions, 6) adults you can depend on for help, 7) adults you feel comfortable talking about problems with and, 8) special adults who care about your feelings. Respondents are asked to report the number of adults within each domain, then three categories of measurement are produced – 1) domains of social support, 2) diversity of caring adults and, 3) depth of social support. The first category – domains of social support – measures the number of different dimensions in which the respondent nominates at least one caring adult (0 = no

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<sup>7</sup> Factor scores were first tested to ascertain whether the subscales coalesced into a singular factor for each recall period. Given that eigenvalues and factor loadings remained quite similar across recall periods principal components factor analysis was conducted without respect to the recall period in question (i.e., over all recall periods as opposed to within each recall period).

<sup>8</sup> Extant research suggests this is the appropriate approach, since the discriminant validity of individual subscales of the BSI is currently suspect (Benishek, Hayes, Bieschke, & Stöffelmayr, 1998).

adults in any dimension, 8 = at least one adult in each dimension); the second category – diversity of caring adults – is measured twofold: a) the number of unique adults mentioned across all domains and, b) the number of unique adults mentioned in the domain with the highest number of adults reported. Finally, the third category – depth of social support – is also measured twofold: a) the number of caring adults who are mentioned in more than two domains and, b) the number of non-family adults present in two or more domains.

For simplicity, this analysis will use three of these measures to summarize dimensions of social support – 1) the total number of adults across all domains (individuals appearing in multiple dimensions are counted as many times as they appear), 2) the count of the number of domains where at least one adult was mentioned and, 3) the number of unique adults mentioned across all domains.<sup>9</sup> Since initial sensitivity analyses demonstrated a strong positive skew in the first measure, it is logged for use in this analysis, and higher-order terms (i.e., squared terms) are tested for the latter two measures to account for any potential nonlinearity (no nonlinearity found to be present). Additionally, preliminary sensitivity analysis suggested these measures were tapping into unique domains of social support, as variance inflation factor values remained low and stable throughout the initial series of interviews.

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<sup>9</sup> Youth who mentioned no adults in any domain were assigned a missing value for these variables – in order to retain these individuals in the following analyses, zeroes are imputed for these measures and are then flagged with a dummy indicator, equal to one if the youth reported no contact with caring adults (16% missing person-recalls).

***Criminal History and Self-Report Offending (SRO).*** In addition to the SRO inventory described in a prior section I also include several variables that characterize offending behavior during the recall period as well as an indicator for the age at which the respondent first began committing crime (the minimum age at first offense across all SRO offenses, measured at the baseline interview). An offending rate is computed by dividing the total number of SRO offenses reported during the recall window by the total number of days in the recall period in which the respondent was in the community.<sup>10</sup> As is convention, this variable is then logged in order to account for potential outliers (i.e., very high offending rates). Two forms of this variable are tested for each of the proceeding models – one including drug offenses (namely, the sale of various illegal substances) and the other excluding them. Since results remained largely unchanged using either variable, I retain only the offense rate excluding drug offenses in the model because including drug sales tends to inflate offense rates, and it is arguable that the selling of drugs is measured on a somewhat different metric than the remaining SRO items (e.g., burglary, assault, robbery, etc...). Also included in these models are two variables tapping into offending diversity – one a variety score of property offenses committed in the recall window and the other a variety score of violent offenses in this time period. A single higher-order term (i.e., a squared term) is tested for each to account for potential nonlinearities (none present).

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<sup>10</sup> Individuals who spent the entirety of the recall window in a secure setting were not asked these questions, thus resulting in a large number of missing values for these variables. To account for this, zeroes are imputed for all SRO items, and a dummy indicator is then added to flag these missing cases so as to retain these person-recalls in the analysis.

**Demographics.** Along with traditional demographic indicators (ethnicity, sex, age, age-squared, survey site – Philadelphia/Phoenix), also included in this analysis as a control is a measure for socioeconomic status (SES). SES is measured at the baseline interview, and is a function of the highest level of education of the respondent’s parents and their current occupations (as of the baseline interview).<sup>11</sup> Scores are assigned for both occupation status and education level and, if both parents are present, these scores are then averaged, producing an overall socioeconomic status level of the respondent’s parents. Since many of these measures are time-stable constants (i.e., ethnicity, sex, survey site, SES), either by definition or as a consequence of how they are measured, those variables will be interacted with time (i.e., recall period) in order to retain them in the fixed effect models.

**Employment – Rates, Wages, & Interruptions.** As a part of the “Making and Spending Money” life calendar, all participants indicated the type (legal, under-the-table, illegal), degree (i.e., number of hours worked), and returns (i.e., wages) of employment for each month of the most recent recall period. This information is used to compute three variables – 1) an employment rate, 2) an average wage rate for the recall period and, 3) the number of interruptions in employment during the latest recall period. The first of these is measured by computing the proportion of community recall-months (i.e., months not spent in a secure setting) where the participant indicated they were employed at least part-time – this is then transformed into a percentage for ease of interpretation. It is

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<sup>11</sup> If a parent is missing from the household, socioeconomic status is still computed using just one parent’s information.

important to note that employment in this context excludes illegal work (e.g., gambling, drug selling, etc...) but can include under-the-table work which is not strictly legal, but does not involve the same level of criminal activity as does illegal work. Unfortunately, PtD administrators only began to make the specific distinction between legal and under-the-table work after several recall periods had already passed, so the current measure combines both types of work, since it is available for all recall periods.

The second measure is computed by dividing the participants' total reported wages by the total hours worked for each month of the recall period. This value is then averaged across all months, resulting in an average wage rate over the entirety of the recall window – as is common in most measures of wages, there exists considerable positive skew in this measure, so a logged version is included in all analyses to better approximate a linear relationship. The final measure is simply taken from respondents' reporting of the total number of times within the recall period where their legal employment experienced some disruption (i.e., fired, laid off, etc...).

***Gang Membership and Contact Frequency.*** At baseline and all following interviews, respondents are first asked whether they are currently a member of a gang and, if they answer affirmatively, are then asked the degree of frequency with which they are in contact with fellow members of the gang on a scale from 1 “Less than monthly” to 7 “Daily”. Two variables are then included in this analysis – 1) a dummy indicator for gang membership during the recall period and, 2) an ordinal indicator of contact frequency with fellow gang members. Those who are not gang members are retained in this analysis by imputing a 0 for the latter variable, which is accounted for through the inclusion of the

dummy indicator (i.e., those that are not gang members invariably also have 0s on frequency of contact with fellow members of the gang).

These variables are included to supplement other measures of peer influence, most particularly to account for the effect that fellow gang members may have on individual attitudes toward law-abiding behavior and in fostering contexts where criminal behavior becomes more likely. Considering that extant research suggests that selection effects (i.e., individuals select into gangs because of their predispositions toward crime) cannot account for the totality of the effect of gang membership on crime (Pyrooz & Densley, 2015), it is important to include these variables as additional controls for potentially criminal peer influences.

***Illegal Work Calendar (IWC).*** The IWC is not implemented at baseline, but is for all following interviews and requires the participant to indicate whether they had engaged in income-generating illegal activities during the most recent recall period. These activities are separated into 6 categories – 1) sold stolen property, 2) sold drugs, 3) stole merchandise, 4) gambling, 5) prostitution and, 6) other. Respondents first indicate whether they had engaged in any of these activities, and then record the months in which these activities occurred during the most recent recall period. Two measures are then produced from their answers – 1) an illegal work variety score, counting the number of unique types of activities engaged in during the recall period and, 2) a monthly rate of illegal work activity, computed by counting the number of months in the recall period where the respondent engaged in illegal work, and dividing this by the total number of months in the recall period in which the respondent spent at least some of their time in

the community. This latter rate is then transformed into a percentage for a simpler interpretation of its associated coefficients (i.e., a 1 percentage point increase in the monthly rate of illegal work is associated with a “X” change in the outcome variable).

***Interview/Recall Period Information.*** Two measures are included to account for the setting of the interview and the length of the recall period. The first measure is a dummy indicator equal to 1 if the participant undergoes their interview in a secure setting or court-ordered placement restricting their access to the community and 0 otherwise (i.e., interviewed at home or in a public setting). The second measure accounts for the total number of months in the recall period. PtD administrators made their best attempts to interview participants at particular intervals – every six months for the first six waves of the PtD then annually for the final four waves – but scheduling difficulties often created departures from this. For the first six waves, the number of months in the recall period can vary between two to eight months while the final four recall periods can be as short as six months and as long as fourteen months. Since the variation in the time since their last interview can likely effect estimates of the effect of program participation (i.e., longer intervals between interviews allow more time for attitudinal change, shorter periods vice versa), it is important to control for this variation so that these estimates are not influenced by the length of the recall window.

***Mechanisms of Moral Disengagement (MMD).*** The MMD was implemented for the baseline and all following interviews, and was originally developed by Bandura,



Barbaranelli, Caprara, and Pastorelli (1996). The MMD consists of 32 items measuring an individual's attitudes towards the treatment of others, and taps into eight sub-dimensions: 1) moral justification (e.g., "It is alright to beat someone who bad mouths your family"), 2) euphemistic language (e.g., "Slapping and shoving someone is just a way of joking"), 3) advantageous comparison (e.g., "It is okay to insult a classmate because beating him/her is worse"), 4) displacement of responsibility (e.g., "Kids cannot be blamed for using bad words when all their friends do it"), 5) diffusion of responsibility (e.g., "A kid in a gang should not be blamed for the trouble the gang causes"), 6) distorting consequences (e.g., "Teasing someone does not really hurt them"), 7) attribution of blame (e.g., "If kids fight and misbehave in school it is their teacher's fault") and, 8) dehumanization (e.g., "Some people deserve to be treated like animals"). Respondents were asked to indicate their agreement with each of the 32 statements on a scale from 1 "Disagree" to 3 "Agree" – an overall measure is then computed as the mean across all items, requiring that the participant responded to at least 24 of the 32 items in order to have a valid value on the aggregate measure.<sup>12</sup>

***Peer Criminal Influences.*** Also included in this analysis will be a measure tapping into the criminal behavior of each participant's close peer group. Respondents are asked, at baseline and all following interviews, to provide information on their four closest friends, including their criminal activity and whether they have spent time incarcerated since the last interview. It is important to note that this "core" peer network is subject to change

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<sup>12</sup> Internal consistency of the 32 items was found to be at or above .90 for initial follow-up periods (the 6, 12, 18, and 24-month interviews), demonstrating adequate agreement between items over time.

over time – i.e., the “close” friends a participant reports in an interview at time “t-1” may not be the same individuals as those reported at time “t=0” or time “t+1.” However, since the measures used in this analysis correspond only to the criminal behavior of the current peer network (and, thus, the peers one is arguably most attached to) this presents less of an issue than if I were to assess the changing nature of the peer group as it relates to desistance. Given that the measure is proportional in nature (i.e., what proportion of these peers have ever been arrested or jailed) it is required that participants indicate at least two friends as being members of their “core” network.<sup>13</sup>

***Psychosocial Maturity Inventory (PSMI).*** The PSMI is administered at the baseline and all following interviews. Developed by Greenberger, Josselson, Knerr, and Knerr (1975), the PSMI includes 30 items tapping into three domains – self-reliance (e.g., “Luck decides most things that happen to me”), identity (e.g., “I change the way I feel and act so often I wonder who the ‘real’ me is”), and work orientation (e.g., “I hate to admit it, but I give up on my work when things go wrong”) – all measured on a 4-point Likert scale (“Strongly Agree” to “Strongly Disagree”). An overall measure of psychosocial maturity is computed by taking the average value of responses across all items<sup>14</sup> – in order for this mean to be computed, however, the respondent must have entered a response for at least 22 of the 30 items.

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<sup>13</sup> In order to retain cases not reporting at least two “close” friends, zeroes are imputed for these measures and are then flagged by a dummy variable, equal to 1 if the individual did not report at least two friends for these measures. The dummy variable flag indicated that approximately 16% of person-recalls are missing on these measures.

<sup>14</sup> The items within this measure display high internal consistency, as Chronbach’s alpha values were all at or above .90 by recall period.

***Perceived Social & Personal Rewards from Crime.*** At the baseline and all following interviews, participants are asked about the perceived social rewards from crime – specifically, the social rewards they might obtain if they were to commit 1) robbery, 2) to fight someone, and 3) theft. Respondents were asked to indicate their level of agreement with statements such as “If I take things, other people my age will respect me more” on a scale from 1 “Strongly Disagree” to 4 “Strongly Agree” Additionally, participants were also asked about their perceived personal rewards from crime, indicating how much “thrill” or “rush” it is for them to commit crime (e.g., “breaking into a store or home to steal”) on a scale from 0 “No fun or kick at all” to 10 “A great deal of fun or kick”.

Principal components factor analysis suggested that perceived social rewards from crime coalesced well into a singular factor (eigenvalue=2.57, all factor loadings above .91) and that perceived personal rewards from crime should be considered as a separate variable, since it did not load well into this factor in any recall period.

***Resistance to Peer Influence (RPI).*** The RPI is administered at baseline and all following interviews. It consists of 10 items and was developed by Steinberg exclusively for the PtD survey (see Cauffman and Steinberg (1995); Mounts and Steinberg (1995); Steinberg, Fletcher, and Darling (1994); Steinberg and Silverberg (1986) for earlier foundations of the RPI) – the original intent of the measure was to assess the degree to which an individual expresses their autonomy when interacting with their peer group. All participants were presented with two scenarios – 1) “Some people “....” just to keep their

friends happy” and, 2) “Other people refuse to “...”, even though they know it will make their friends unhappy.” Completing these statements were ten possible scenarios – 1) go along with friends, 2) fit in with friends, 3) change their mind, 4) knowingly do something wrong, 5) hide their true opinion, 6) break the law, 7) change the way they usually act, 8) take risks, 9) say things they do not really believe and, 10) go against the crowd. Respondents were asked to choose the statement that best reflected their behavior, and to then rate the extent to which they believed the statement was true of them (i.e., “sort of true” to “really true”).

Each statement was then given a score of 1 to 4 depending upon the initial scenario choice and respondents’ ratings. As an example, a respondent who chose 1 for the initial scenario (“Some people “...” just to keep their friends happy”), and then “really true of me” are assigned a value of 1 for that item (thus indicating lesser resistance to peer influence). In contrast, an individual who initially chose the 2<sup>nd</sup> scenario (“Other people refuse to “...”, even though they know it will make their friends unhappy”) and then “really true of me” are assigned a value of 4, thus indicating a greater degree of resistance to peer influence. The measure used in this analysis is an overall mean computed across all ten scenarios, which demonstrated high internal consistency (Chronbach’s  $\alpha > .75$ ) across the initial four waves of interviews.

***Substance Abuse Inventory (SAI).*** Implemented at baseline and all following interviews, participants are asked to report whether they had used the following drugs during the recent recall period: 1) alcohol, 2) marijuana/hashish, 3) sedatives/tranquilizers, 4) stimulants/amphetamines, 5) cocaine, 6) opiates, 7) ecstasy, 8) hallucinogens, 9)

inhalants, 10) amyl nitrate/odorizers/rush, 11) other drugs, 12) prescription medication (abuse, not regular use). For alcohol use, participants are also asked how often during the recall period they had been drunk, and I use this measure, as opposed to simple alcohol use, since regular alcohol use is arguably somewhat normative amongst teenagers (though still illegal) and does not present itself as “abuse” as well as more frequent and serious events of intoxication.

For each of these drugs (or getting drunk) the respondent is then asked, on an 8-point scale (0 “Not at all” to 8 “Every day”) how often they used these substances. Given that the eleven individual items fail to correlate adequately (Chronbach’s  $\alpha \approx .48$ ) I sum scores across all items to produce an overall substance abuse severity scale ranging from 0 (did not use any drugs in the recall period) to 88 (used all eleven drugs every day during the recall period). In practice, however, the highest score on this scale was 75, and average values within subgroups was quite low ( $\sim 3$  to 4) depending upon the recall period. For this reason, I chose to cap values at the 99.9<sup>th</sup> percentile (a score of 46) since scores above this range present themselves as extreme outliers and are quite difficult to believe.<sup>15</sup>

***Percentage of Time Spent in the Community (“Street Time”).*** Using life calendar data, the current analysis is able to also control for the time a respondent spends in secure facilities or on the street during the most recent recall period. A “secure setting” is defined as a setting in which the respondent does not have access to the community, and

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<sup>15</sup> This results in 93 person-recalls being missing on this variable of 13,443 total person-recall observations.

could be one or more of the following settings: 1) inpatient drug/alcohol treatment unit, 2) inpatient psychiatric hospital or psychiatric unit of a general hospital, 3) jail or prison, 4) temporary detention facility while awaiting adjudication or placement in a secure facility, 5) a youth development center (YDC, only in Philadelphia setting) or the Arizona Department of Juvenile Corrections (ADJC, only in Phoenix setting), 6) contracted residential treatment facilities for general treatment and, 7) contracted residential treatment facilities for mental health treatment. The total number of days spent in any of these types of facilities is summed within each recall period, and is then divided by the total number of days in the recall period. This proportion is then used as a weight in all following analyses, and is reported as a percentage in all descriptive tables for ease of interpretation.<sup>16</sup>

***Youth Psychopathic Traits Inventory (YPI).*** The YPI is not implemented at baseline, but is for all following interviews. Originally developed by Andershed, Kerr, Stattin, and Levander (2002), the YPI consists of 50 items that measure three subscales of psychopathy – 1) grandiose manipulative (e.g., “I’m better than everyone on almost everything” or “I can make people believe almost anything”), 2) callous unemotional (e.g., “I think that crying is a sign of weakness, even if no one sees you” or “I usually feel calm when other people are scared”) and, 3) impulsive irresponsible (e.g., “I consider myself a pretty impulsive person” or “I have often been late to work or classes in school”). Respondents are asked to respond, on a 4-point Likert scale (1 “Does not apply

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<sup>16</sup> Since the STATA functions used for this analysis do not allow for weights to vary within person, these proportions had to be averaged across recall periods.

at all” to 4 “Applies very well”) to how each of the 50 statements correspond to their behavior or orientations.

Scores are then summed within each dimension (grandiose-manipulative – 20 items, callous-unemotional – 15 items, impulsive-irresponsible – 15 items) thus producing three overall measures for each domain. Principal components factor analysis was conducted to determine whether these domains tapped into an overarching general psychopathy measure – though the eigenvalue was somewhat low (2.27) as compared to other factors in this analysis, all factor loadings exceeded .85, and results remained stable when considered within each recall period. Therefore, the factor was retained for use in this analysis and, since items were reverse-coded as necessary, higher factor scores on this general psychopathy factor indicate the presence of more (or more severe) psychopathic characteristics.

### V-3. METHODS

The current analysis employs a random-intercept panel model due to the nested nature of the data – i.e., time-variant measures are nested within persons, themselves having time-invariant personal characteristics that may influence coefficient estimates. Since the primary concern of this analysis is to estimate the influence of selection – i.e., the extent to which individuals sending signals are systematically different from those who do not – it is essential that the model be able to ascertain.... Since all dependent variables have been differenced, and are thus no longer in their original ordinal format, a random-intercept OLS model is fit to the data, taking on the following form:

$$(Y_{it} - Y_{it-1}) = \alpha + \delta D_i T_{it} + \beta X_{it} + \gamma W_i + u_i + e_{it}$$

Where  $(Y_{it} - Y_{it-1})$  is a differenced outcome variable, representing the recall-to-recall change in the various attitudinal measures used in this analysis,  $\alpha$  is the model intercept,  $\beta$  a vector of coefficients for level-1 regressors and  $X_{it}$  are independent variables for person  $i$  measured at time  $t$ ,  $\gamma$  a vector of coefficients,  $W_i$  are time-stable regressors for each person  $i$ , and  $u_i$  the person-level (level-2) deviation from the model intercept with the person-recall (level-1) equivalent being  $e_{it}$ . Finally,  $\delta$  is a vector of coefficients for the cross-level interaction between  $D_i$  - a vector of person-stable indicators of treatment group (control, only voluntary, only involuntary, mixed) - and  $T_{it}$  a time-varying dummy indicator for whether the recall period for person  $i$  at time  $t$  is pre- or post-program participation.

In order to adequately compare the effect of program participation, it was necessary to create a simulated control group to compare voluntary, involuntary, and mixed participation individuals to. As such, I implemented a random selection procedure designed to both 1) randomly select individuals and, 2) randomly select recall periods within them for comparison. This procedure, explained in full detail in Appendix A, is completed ten times, resulting in ten potential control groups for comparison. Fixed-Effect OLS models are estimates using each of these ten groups, and then coefficients are compared using seemingly unrelated estimation (using the “suest” command in STATA). For all of the following models in this section, there existed no significant differences in coefficients using any of these control groups, essentially indicating that the choice of control group for the models presented below is arbitrary. Descriptive statistics are



presented using a weighted average of these groups, while the random-intercept models reported below use just one of these groups for estimation.

An underlying issue with a random-intercept panel model specification is that the number of observations within level-2 clusters (persons) is minimal (the “low t” problem), here being a maximum of 10. The attendant complication becomes having to effect a precarious balance between producing efficient slope estimates in the guise of random-effects or more consistent ones in the form of fixed-effects. Therefore, two versions of each model are estimated – 1) a random-intercept model and, 2) a fixed-effects model. Results are then compared using Hausman specification tests, which take the general form:

$$H = (\beta_{RE} - \beta_{FE})' \hat{\Sigma}^{-1} (\beta_{RE} - \beta_{FE})$$

Where  $\beta_{RE}$  and  $\beta_{FE}$  are a vector of coefficients from the random-effects and fixed-effects models, respectively and  $\hat{\Sigma}^{-1}(\beta_{RE} - \beta_{FE})$  represents their covariance matrix. The test is asymptotically distributed chi-square, with the null hypothesis that coefficient estimates across models are not systematically different and, therefore, that either 1) the random-intercept model is both efficient and consistent or, 2) that the fixed-effects model is so inefficient as to outweigh any of its gains in consistency. If results are significant, alternative models are then estimated expressing certain coefficients as fixed-effects within the random-effects estimator (accomplished by adding a level-2 regressor representing that variable’s person-level mean over all recalls) and Hausman specification tests are then re-estimated. If significant results persist even after these

coefficients are estimated as fixed-effects, it is determined that the fixed-effects model is preferred to the random-intercept model due to the former's consistency.

## **VI. RESULTS - EFFECT OF PROGRAM PARTICIPATION ON ATTITUDINAL MEASURES (HYPOTHESES 1 & 2)**

### **VI-1. DESCRIPTIVE ANALYSIS**

Before delving into analysis results, it is prescient to begin this section by exploring descriptive statistics for each group over the recall periods directly adjacent to the recall period in which program participation occurs (for descriptive statistics extending to  $t-3$  and  $t+3$ , please see Table B-1 in Appendix B). Table 4 presents these statistics, below.

**\*\* TABLE 4 HERE \*\***

Beginning with outcome variables, AWFL perceived opportunity and expectations scores tend to remain stable across both groups (control (CTRL)<sup>17</sup>, only voluntary (VOL), only involuntary (INVOL), mixed (MIX)) and recall periods. Values for the former (opportunity scores) tend to hover around values of 4.45 and 4.5, thus indicating that youth have fairly high perceptions regarding the opportunity for them to achieve their occupational, familial, and educational goals in the future. Further, values for the latter are slightly lower – between 3.6 and 3.72 – since both opportunities and expectations are measured on similar scales, this indicates that, although youth perceive high opportunities, they are, on average, slightly more pessimistic as it regards their own

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<sup>17</sup> Statistics for this group represent a weighted average across all 10 simulated control groups.

expectations for themselves to achieve these goals. Moving to FOI scores, we again see only slight differences between groups and over recall periods, suggesting that these scores are somewhat stable over time. Additionally, the substantive meaning behind scores -  $\sim 2.5$  – means that the average respondent tends to fall between “Rarely True - 2” and “Often True - 3” scores on FOI items, representing a fairly agnostic response pattern across items. Finally, MtS scores, in contrast to the remaining outcomes, does exhibit some dissimilarity between groups and over recall periods. Namely, CTRL and VOL groups have the lowest scores, the INVOL group scores are slightly higher, and the MIX group has the highest scores over all recall periods. Further, there is a small upward trend among all groups, thus potentially indicating that program participation may have some effect on an individual’s underlying motivations, but it remains to be seen whether this apparent effect holds when controlling for a host of other factors.

Social and personal rewards from crime exhibit dissimilar patterns across groups and recall periods. While VOL and MIX groups tend to have the lowest scores on the social rewards factor score, this is not also true for personal reward from crime (i.e., “thrill” or “kick”) as the VOL group becomes more similar to the CTRL group (with scores  $< 1$ ) and the MIX group approximates the INVOL group (with scores  $> 2$ ).

Employment rates (measured as the percent of community-months employed at least part-time) tend to increase for all groups over recall periods but, most notably for the INVOL group, which begins at a low of 20.38% and ends at 31.29, the highest rate for all groups in recall  $t+1$ . Logged average wage rates tend vary considerably over time – some groups see a period-to-period change moving from  $t-1$  to  $t=0$ , but often return to  $t-1$  values in the following recall period. This could indicate that employment and wages

tend to increase during program participation, but for most groups this benefit appears to have a fairly short half-life. Interruptions in employment increase in the recall period, but tend to return to pre-program levels afterward – this is likely suggestive of something akin to an Ashenfelter dip whereby program participants experience a dip in employment leading into the program (which could be caused by a variety of sources) which then corrects itself after participation ends.

Offending rates (measured as the expected number of offenses per day in the community) exhibit dissimilarities between groups – the INVOL and CTRL groups tend to have the highest levels of offending for both rate measures (drug selling including/excluded) and, though the VOL group starts at a level higher than all other groups this decreases quickly into  $t=0$  and  $t=1$ . As was expected, the offending rate including drugs is universally higher than the rate excluding drugs and this difference can be quite profound – e.g., the offending rate including drugs can be over 7 times as high as the offending rate excluding drugs (see INVOL group in recall  $t+1$ ). Considering this, proceeding models will only include the former measure, as drug sales tend to inflate offending rates, likely due to the fact that they are as instances of drug sales (i.e., a transaction), as opposed to discrete periods of time where the individual sells drugs (i.e., the number of times they actively sold drugs for X period of time). Both sets of variety scores are fairly stable over time, and all below a value of one, indicating that no group, in none of the recall periods depicted here, tend to commit more than one type of property or violent crime in any given recall window. Finally, the average person in this sample, regardless of group, committed their first SRO at or around the age of 10.25 to 10.5 years old.

In regard to peer influences, gang membership is relatively rare for all groups over all recall periods, but the MIX group stands out as having the lowest number of current gang members (as low as 1.6%) while the CTRL group is often the highest (as high as 10.8%). Among current gang members, contact frequency with fellow gang members ranges between values of 4 (“Once per week”) and 5 (“Twice per week”) for all groups, and tends to remain stable over the recall window. RPI scores vary somewhat between groups and over time, but this variation is fairly minimal, as the average individual across all groups and time periods tend to have a score of approximately 3.2, indicating on the RPI scale (1 “It’s really true I’m influenced by my peers” to 4 “It’s really true I prefer to be an individual”) that they mostly prefer to be an individual, but can be influenced by their peers to a small degree. The proportion of one’s peers who have ever been arrested or jailed differs slightly between groups – namely, that the VOL group has the lowest values, on average – and tends to remain fairly stable over time. This could indicate that the 2 or more “close” friends an individual reports either stay the same over time (since the language used in the survey pertains to “ever” arrested or jailed) or, alternatively, individuals choose friends with similar characteristics as others leave their peer group.

Moving to the various psychological scales used in this analysis, BSI factor scores tend to remain stable within groups over recalls, with the VOL group reported the highest levels of acute psychological distress (however, these values - .27 to .45 - are still somewhat low relative to the maximum score on this factor is 7.73). PSMI scores are stable across groups and over recalls, and all values correspond to greater levels of psychosocial maturity (as the maximum on this scale is a score of 4). Finally, YPI factor

scores exhibit some slight irregularities within groups over recall periods, but there is a slight overall trend downward toward the average values (i.e., a value of 0, since this inventory has undergone factor analysis) for all but the VOL group, whose values increase over recalls.

As it pertains to the presence of caring adults (Social Support indicators) in these youths' lives, most youth across groups and over recall periods report a similar number of caring adults across the various domains of social support, resulting in a logged value of approximately 2.5. However, these adults are distributed differently across groups – the MIX group tends to report higher numbers of unique adults within domains while the CTRL group typically has the lowest values. Similarly, while most youth report adults in roughly 5 unique domains, only the INVOL and MIX groups report an average at or above 6, indicating that there are caring adults in their lives within all but two of the social support domains in this analysis.

As it pertains to substance abuse measures, it appears that while many youth report no variety of substance used, enough indicate using at least one substance so as to push averages toward a value of 1, but in lesser degrees for each group. Interestingly the variety of substances abused appears to increase over time for the VOL and INVOL groups, but remains stable for the MIX group. This being said, all substance use variety scores remain below zero, indicating that substance abuse remains relatively rare for all groups in all the recall depicted in the table. Consistent with these statistics, the frequency of substance abuse increases for the VOL group, but remain stable for the INVOL, MIX,

and CTRL groups<sup>18</sup>. Considering that all scores remain below a score a 9 (approximately), however, it seems as though that substance abuse remains relatively infrequent, even if it increases over the recall window for some groups.

Finally, groups tend to begin participation at slightly different ages, with the INVOL and MIX groups being slightly younger at recall *t-1* than are individuals in the VOL and CTRL groups. Interestingly, compared to the simulated CTRL group, actual program participants are more often white, while the CTRL group is typically black, Hispanic, or another race. There also exists a slight gender disparity between groups – both the simulated CTRL group and the INVOL group are more often male, while the VOL and MIX groups, while still a majority male, contain comparatively more females.

An interesting pattern arises when examining survey site statistics (i.e., whether the respondent was in Philadelphia (0) or Phoenix (1)). While CTRL, VOL, and INVOL groups are roughly evenly split between sites, the MIX group predominantly comes from Philadelphia. This suggests that there may be some underlying differences in the assignment to involuntary programs and availability of voluntary programs in Philadelphia as opposed to Phoenix, but other group distributions would imply these are somewhat similar. Whatever the reason for this disparity, it will be presumably important for all following analyses to control for site in order to ascertain whether this difference influences outcomes. Lastly, parent SES is typically highest for the CTRL and INVOL groups, and lowest for the VOL and MIX groups.

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<sup>18</sup> Frequency of abuse statistics are only computed for those reporting a substance abuse variety score of one or more so that average values are not drawn toward zero by nonusers.

The final set of statistics pertain to characteristics of the recall period (i.e., percent of time in the community and the number of months in the recall period) and the setting of the interview for each recall. Fortunately, the number of months in the recall period is largely similar across groups and over recalls, with the slight exception of the INVOL group in recall  $t+1$ . Both the simulated CTRL and INVOL groups are more often interviewed in some type of secure placement as compared to individuals in the VOL and MIX groups. This trend is reflected in the percent of time these groups spend in the community – the CTRL and INVOL groups spend the least amount of their time outside of secure settings, but it is notable that, for all but the CTRL group, time spent in the community tends to increase over recall periods.

***Figures for Outcome Variables Over Extended Recall Periods.*** In order to provide a better picture of how the various outcome variables in the following analysis behave over time, several figures were produced plotting average group values over recall periods between  $t-5$  and  $t+5$ . These figures are presented below.<sup>19</sup>

**\*\* FIGURES 3 TO 6 ABOUT HERE \*\***

*AWFL Scores – Opportunities.* As was demonstrated in the Table 4, AWFL opportunities scores are fairly stable over time (Figure 3), and no clear group differences were discovered in the recall periods directly adjacent to program participation. These trends are largely the same when extending the observation window to 5 recall periods pre/post

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<sup>19</sup> Since the seemingly unrelated estimation procedure determined that the choice of control group was essentially arbitrary, the following figures use just one group (Control Group 3) to produce trend lines. Please see Appendix A for further details on this procedure.



participation. The only departure from this are the scores among the MIX group – the scores within Table 4 exhibited little variation, but scores outside this time period demonstrate that the MIX group appears to have higher scores, on average, than all other groups, but for some reason these scores drop considerably going into the periods directly surrounding program participation. Both the VOL and INVOL groups overlap considerably, with both group's values falling within a tight range between 4.45 and 4.55 over all recall periods.

*AWFL Scores – Expectations.* Expectations scores for all treatment groups (i.e., VOL, INVOL, MIX) tend to cluster together over all recall periods (Figure 4). In fact, all groups experience a drop in expectations in the recall periods leading into program participation, but differ slightly in the timing, magnitude, and recovery from this drop. Specifically, expectation scores begin to decline in  $t-3$  for the MIX group, then increase steadily before dropping again to pre-program levels after  $t+2$ . In contrast, the VOL group begins their decline in  $t-2$ , recovers into  $t+1$ , then declines thereafter. Finally, the INVOL group experiences a similar decline to the VOL group in terms of timing and magnitude, but recovers comparatively quicker before again declining into  $t+5$ .

*FOI Scores.* With the exception of the MIX group, all groups tend to follow a fairly flat trend with respect to FOI scores (Figure 5), suggesting that participants' outlook for their futures remains somewhat constant over time, regardless of program participation. While the MIX group slightly departs from this trend, this is mostly a function of low scores in

earlier recall periods, as this group's trend is similar to that of the other groups after  $t=0$ . The only other notable pattern is that the VOL group typically has higher FOI scores than the INVOL group, but these differences are quite small ( $<.10$ ).

*MtS Scores.* Both the VOL and INVOL groups exhibit a similar drop in motivation (Figure 6) at  $t-2$ , but these groups recover differently – the VOL group maintains low MtS scores until  $t+3$  but the INVOL group almost immediately returns to pre-drop levels in  $t-1$ . Both groups, however, meet at the very same MtS score at  $t+5$ . In contrast, the MIX group maintains comparatively higher MtS scores throughout most of the observation window, only dropping to levels similar to other groups in  $t+4$ .

*Discussion.* In contrast to my prior expectations, the VOL group does not appear to be largely different from the INVOL group with respect to their attitudinal trends pre- and post-program participation. Though it does not appear that attitudes are necessarily influenced by program participation, it was at least expected that future orientations, aspirations, and motivations for success would be noticeably higher among those who participate in programs voluntarily, especially as compared to those whose participation is solely court-ordered. However, it does appear that the MIX group exhibits trends substantially different from other groups with respect to their perceived opportunities and motivation for success. It remains to be seen, though, whether these non-differences and differences hold when controlling for other factors. The following section will explore

this more thoroughly, estimating a series of random-intercept models estimating the influence of program participation on these attitudinal scales.

## VI-2. RANDOM-INTERCEPT RESULTS

As aforementioned, recall-to-recall changes in attitudinal measures (AWFL – Opportunities/Expectations, FOI, and MtS) are predicted using cross-level interactions between program participation, participation type, and the opportunity cost of participation as well as manifold salient independent variables that might otherwise account for these attitudinal changes. Table 5, below, presents the results from these four analyses along with model auxiliary statistics.<sup>20 21</sup>

### **\*\* TABLE 5 HERE \*\***

*AWFL – Opportunity Scores.* Beginning with the AWFL opportunity scores, I find that no interactions between program participation (or, treatment, equal to 1 if post-participation), participation type (CTRL, VOL, INVOL, and MIX), and participation category (=1 if above median participation in the sample) yield significant findings.

However, some patterns do comport with Hypotheses 1 & 2 – namely, that program

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<sup>20</sup> Reliabilities and intraclass correlations are computed using non-differenced outcome variables, since the difference transformation disallows the random-intercept model from estimating the amount of variation in these outcomes that is due to level-2 clusters (i.e., the person-level). In addition, all observations are weighted by the proportion of time during the recall period in which the respondent was not in a secure facility. Since the xtreg function in Stata does not allow for weights to vary within observations, these proportions are averaged across all recall periods, and then entered as weights using the “iweight” option for the xtreg command.

<sup>21</sup> Out of concern for over-controlling the effect of selection, alternative models are estimated on non-differenced outcomes only controlling for demographic variables and the treatment/group type interactions. Though results remain largely unchanged some group type and treatment interactions do emerge as significant. Namely, it appears that future orientations for the INVOL and MIX groups are influenced by program participation in a positive direction. Consistent with Hypotheses 1 and 2, main and interaction effects for the VOL group are never significant.

participation appears to have no effect for the VOL and MIX groups (nor for the INVOL group, for that matter) as evidenced by the non-significance of the “Group X Treatment” interactions.

In contrast, both measures for illegal work appear to be related to opportunities scores. In regard to the illegal work rate, every 1 percentage point increase in the rate of illegal work is associated with a .0018 ( $p < .01$ ) drop in perceived opportunities. Since values on this variable tend to be somewhat clustered around common values (i.e., 10, 25, 33, 50%, etc...) it is better to interpret this variable in higher increments, such that a ten percentage point increase in the illegal work rate is associated with a .18 decline in perceived opportunities, representing about a fifth of a point decline in the AWLF – Opportunities scale. Interestingly, the variety of illegal work (i.e., the number of different types of illegal work engaged in during the recall period) is associated with an increase in perceived opportunities. Specifically, for each additional type of illegal work, the opportunities score is expected to change by roughly .08 ( $p < .01$ ) points. Given this pattern, it is likely the case that these variables are tapping into two different dimensions of illegal work – one of persistence (the rate) and the other of breadth (variety) and it might be the case that greater breadth in illegal work instills the individual with a belief that they are better positioned to achieve future success than other, perhaps more persistent individuals, who do not branch out into other money-generating illegal activities.

It is of little surprise that PSMI scores – psychosocial maturity – are related to changes in AWFL Opportunity scores. It is important to first note, however, that initial Hausman specifications tests indicated that models without a level-2 (person-level)

average PSMI score failed significance tests (i.e., coefficients between random-intercept and fixed-effect models were systematically different). Therefore, a within-person average PSMI measure is incorporated into each model so that Hausman specification tests resulted in accepting the null hypothesis (i.e., that no systematic differences in coefficients was present). The inclusion of this variable means that the recall-to-recall change in PSMI may be interpreted akin to a fixed-effect since person-level means are controlled for. In this case, every one-unit increase in the PSMI scale results in approximately a .0621-point increase in the expected AWFL Opportunity score. Considering, however, that the PSMI scale ranges from just 1 to 4, it is apparent that, though these results are statistically significant, their substantive impact is somewhat limited – i.e., even an individual beginning at a value of 1 in a prior wave that then moves to the maximum in the current wave (4) would only be expected to have an AWFL Opportunity score that is only .1863 points higher.

In addition, being interviewed in a secure setting appears to exert a significant and positive influence ( $b=.0736$ ,  $p<.01$ ) on AWFL Opportunity scores, as might be counter to expectations. While it is somewhat difficult to make sense of this finding, it might be the case that juveniles in secure settings are benefitting from programs not available to them in the community, resulting in heightened perceptions of their future opportunities for their life after they leave the facility.

Finally, auxiliary statistics indicate that 35% of the total variation in AWFL Opportunity scores is associated with person-level (level-2) characteristics. Further, the reliability estimate – i.e., the degree to which cluster means reliably estimate population

means – is quite high, suggesting that 1) cluster means are reliable and, 2) they vary substantially between clusters (persons).

*AWFL – Expectations Scores.* As was true for AWFL Opportunity scores, none of the cross-level interactions between treatment, group type, and participation category are significant but patterns differ. The group-level effects for the VOL and MIX groups are instead positive, while the group type and post-program (treatment) interactions are negative. This is likely representative of the differences exhibited in Figures 3 & 4, whereby these groups tended to have some disagreement between their perceived opportunities and perceived expectations for themselves – perhaps representing an underlying pessimism for their futures.

The rate and variety of illegal work exert similar effects as for AWFL Opportunities. Again, we see that a 10 percentage point increase ( $p < .05$ ) in the rate of illegal work is associated with a .20-point decline in perceived expectations and an increase in illegal work variety is expected to increase expectations ( $b = .0819$ ,  $p < .05$ ). In contrast to perceived opportunities, resistance to peer influence is a significant predictor of perceived expectations – for every one-point increase in the RPI scale (1 to 4) there is a .0644-point increase in perceived expectations for the future. However, the range of the RPI scale means that, substantively, this relationship lacks strength – an individual reporting complete resiliency to peer pressure is only expected to differ from someone completely susceptible to peer influence by approximately .19 points on the AWFL Expectations scale – just 1/5<sup>th</sup> of a single point.

As was also true for AWFL Opportunities, PSMI scores are significantly related to AWFL Expectations, but appear to exert greater influence on perceived expectations than they do on perceived opportunities. Specifically, the fixed-effect estimate of PSMI indicates that every one-point increase results in a .2071-point increase in perceived expectations, controlling for person-level average PSMI. Further, individuals interviewed in a secure setting are again seen to have higher expectations scores ( $b=.0737$ ,  $p<.05$ ) than their counterparts who are interviewed in the community. Finally, individuals missing data on the BSI scale – mostly as a result of a programming error – are expected to have perceived expectation scores that are roughly .07 points higher than those with valid data, on average and all else equal. Though one might be immediately alarmed that a missing indicator variable is significant, its effect is fairly insubstantial – a .07-point difference is quite small with respect to the total range of perceived expectation scores (1 to 5).

Auxiliary statistics again indicate that person-level characteristics account for a sizable percentage of variation in perceived expectations – approximately 41% - and that cluster means are sufficiently reliable (.896). Further, the expected value of the AWFL expectations score for an individual with average random-effect about the intercept is expected to be .4816, holding all else constant.

*FOI – Future Orientation Inventory Scores.* In a repeating pattern, none of the cross-level treatment, group type, and participation category interactions are significant when predicting FOI scores. Though the direction of coefficients somewhat agree with prior models, there are some small departures. Most notable is that the sign of the treatment

effect (post-participation) is negative, suggesting that, though the underlying effect is no different from 0, the balance of estimated effects trends toward being slightly negative.

As was true for AWFL scores, the rate of illegal work exerts a significant effect – specifically, every 10 percentage point increase in the rate of illegal work is associated with a .19-point decline in the FOI scale. Once again, a one-unit increase in the RPI scale suggests that higher levels of resistance to peer influence are associated with greater orientations/expectations toward the future ( $b=.0428$ ,  $p<.01$ ).

Interestingly, greater levels of moral disengagement are associated with higher levels of future orientation ( $b=.0055$ ,  $p<.01$ ). Once again, however, the substantive effect of this measure is questionable – ranging from 0 to 32, the expected difference in the change in FOI between two individuals at the minimum and maximum of moral disengagement – i.e., super-morality v. near amorality – is only .1760, just under 1/5<sup>th</sup> of a single point in the FOI scale. In regard to the remaining psychological scales, both recall-to-recall changes in PSMI and person-level PSMI averages are significantly correlated with future orientations ( $b=.0922$ ,  $p<.001$ ;  $b=-.1181$ ,  $p<.001$ ; respectively). Against personal expectations, the strength of the relationship between psychosocial maturity and future orientations lies somewhere between the strength of its relationship with perceived opportunities and expectations – I would have surmised that psychosocial maturity be more directly related to future orientations, as this measure involves actively planning for the future as opposed to simply perceiving what may yet come to pass. Regardless, every unit increase in the PSMI scale is associated with approximately a 1/10<sup>th</sup> of a point increase in future orientations, holding person-level PSMI average constant.



Lastly, in the only instance of this variable being significant across the models presented here, survey site appears to be significantly related to future orientations. Specifically, youth in Phoenix (site=1) have marginally lower scores ( $1/20^{\text{th}}$  of a point,  $b=-.0490$ ,  $p<.05$ ) than do youth in Philadelphia. Auxiliary statistics indicate that 36.49% of the remaining variation in FOI scores (absent that already explained by included person-level variables) may be attributed to unmeasured person-level characteristics, suggesting that differences between youth account for more than a third of the variation in future orientations. Once again, reliability estimates (.896) are of sufficient level to determine that cluster means reliably estimate the population mean and the Hausman specification test indicates no systematic differences between random-intercept and fixed-effect coefficient estimates.

*MtS – Motivation for Success Scores.* As was true in all prior models, no main effects for, or interactions between, treatment, group type, and participation category indicators are significant when predicting MtS scores. Patterns of the signs of coefficient suggest that all groups have lower MtS scores than the randomly selected control group, and these scores appear to be made *worse* through program participation (except for the MIX group, which gains from participation). Regardless, all coefficients are insignificant, and quite small, at that – this suggests, overall, that program participation, irrespective of the type or intensity of participation, exerts little to no effect on an individual's underlying motivation for legitimate success.

As might have been expected, an individual's perceptions of the social rewards from crime has some impact on their motivations for success. Specifically, every one-unit

increase in the social rewards factor score is associated with a .0318-point decline in the MtS scale. Further, since the variable is a factor score, every one-unit increase may be interpreted as a standard deviation increase in the underlying factor and its associated effect on the outcome variable. As such, it seems that a fairly large change in perceived social rewards from crime is necessary for a comparably small change in motivation. Considering that the maximum of the factor score is approximately 7, this means that we would expect an individual seven standard deviations above the expected score on this factor to have an MtS score that is just  $1/5^{\text{th}}$  of a point ( $\sim .22$ ) lower than an individual at the average value (0). Therefore, while statistically significant, it appears that the substantive influence of perceived social rewards from crime is somewhat negligible.

Recall-to-recall changes in PSMI and the person-level average PSMI are also significantly related to MtS scores, and in the same direction as prior models would imply. Namely, every one unit change in the PSMI scale is associated with a .1209-point increase in motivations for legitimate success, controlling for the within-person average of PSMI. Considering that the AWFL and MtS scores are measured within the same range (1 to 5), we can place the substantive impact of PSMI on motivations as somewhere between its effect on perceived opportunities and perceived expectations. Further, we can estimate the expected difference in changes in MtS between individuals at the minimum (1) and maximum (4) of the scale as .3627. Though still small relative to the range of the MtS scale, a  $1/3^{\text{rd}}$  point difference in motivations for legitimate success is still sizable on its own.

Auxiliary statistics indicate that the intercept for changes in MtS (.7829) is fairly high relative to the intercepts for the first three models (.2221 to .6339). Additionally, it

is evident again that unmeasured person-level characteristics account for a good deal (~36%) of the unexplained variation in MtS scores and reliability estimates are again sufficiently high as to determine that cluster means are reliably estimated.

### VI-3. DISCUSSION OF RANDOM-INTERCEPT MODELS

*Repeat of Hypotheses Tested.* To provide context for the following discussion, Hypotheses 1 and 2 are repeated below:

**Hypothesis 1.** It is expected that program participation, voluntary or not, should exert some impact on the employability of participants, but not necessarily on attitudinal measures associated with desistance for those who participate of their own volition. Thus, pre- and post-signaling attitudinal measures should remain largely unchanged for voluntary participants, but should exhibit some change for involuntary or mixed participants as their orientations toward crime (and analogous behaviors) are redirected.

**Hypothesis 2.** The expectations of Hypothesis 1 should be most pronounced for those voluntary participants for whom the costs of acquiring the signal are larger. That is, the degree to which attitudinal measures associated with desistance remain unchanged pre- and post-program participation should be more evident among those individuals whose participation is comes with a higher opportunity cost. Thus, participants who spend comparatively little of their time in the program are paying less of a cost than an individual spending much more time in the program.

*Discussion.* In comport with Hypothesis 1, I expected that all main effects for the VOL and MIX groups would be insignificant, since their voluntary participation in a program is conceptualized as a signal of their underlying intentions to desist from crime. As such, though these programs may exert an effect on their employability (to be examined in the following section), participation should yield no effect on attitudes associated with these intentions. Namely, their perceived opportunities and expectations for the future, orientations toward future success, and motivations for success should remain relatively constant pre- and post-participation. In contrast, I expected that programs may have some influence on the attitudes for the INVOL group, since the programs included in this analysis appear, at least at face value, to have some implications for their futures. Namely, that job training, mental health, and substance abuse programs could enable these individuals to increase their level of human capital or, in the least, to realize its true value absent the impediment of a mental health or substance abuse problem.

These expectations are only partially met, as main effects and treatment interactions are indeed insignificant for the VOL and MIX groups, but it is also apparent that program participation has no observable effect for the INVOL group either. Instead, the only significant findings from these analyses indicate that changes in the rate and variety of illegal work, resistance to peer influence scores, and psychosocial maturity influence attitudes that extant research (LeBel et al., 2008; Maruna, 2001; Rocque et al., 2014) suggest are related to cognitive transformations/identity changes leading toward desistance from crime.

Since an analysis of signaling is, essentially, an examination of selection effects, it might be the case that the current analyses are over-specified. Specifically, that variables that appear to be related to attitudinal changes are accounting for the stable differences between the VOL, INVOL, and MIX groups. As such, alternate models are specified predicting non-differenced outcome variables and including only group, treatment, and participation category main and interaction effects along with simple demographic indicators. Results from this sensitivity analysis (Table B-# in Appendix B) illustrate slightly stronger support for Hypothesis 1, but do not indicate any significant differences between groups with respect to participation category interaction effects (Hypothesis 2). Specifically, both the INVOL and MIX groups exhibit significant and positive group and treatment interaction effects, suggesting that participation has some influence on their attitudes, while all main and treatment interaction effects for the VOL group remain insignificant.

Perhaps the most important overall finding from this analysis is that, among the VOL group, attitudes do not appear to be altered through their voluntary participation in programs. Rather, results presented in the preceding tables and the aforementioned sensitivity analysis suggest that the apparent effect of program participation on attitudes is restricted only to those whose participation is either A) wholly involuntary (INVOL) or, B) partially involuntary (MIX). This provides at least some support for the notion that voluntary program participation is a signal of intentions to desist (albeit a signal with weak fit), at least with respect attitudes associated with desistance. It remains to be seen, however, whether these findings also hold true when the outcomes of interest pertain to criminal behavior, which will be tested in the proceeding section.

## **VII. MEASURES AND METHODS – ESTIMATING THE EFFECT OF PROGRAM PARTICIPATION ON DESISTANCE (HYPOTHESES 3 & 4)**

### **VII-1. DEPENDENT VARIABLES**

Considering that program participation, type of participation, and participation intensity (i.e., opportunity costs) appear to exert no detectable influence on attitudes associated with desistance, the analysis now moves forward to estimate the effect of employment on two key offending measures – changes in offending rates and changes in offending versatility. These two measures are chosen, as opposed to a simple dummy indicator because desistance is largely not regarded as a discrete event, but is increasingly thought of as a gradual process whereby an individual slowly ceases offending over a period of time (Bushway et al., 2001; Paternoster & Bushway, 2009).

*Change in the Rate of Offending.* As detailed in section V-2 of this manuscript, respondents are asked at all recall interviews to indicate the number of times they had engaged in 21 types of SRO offenses (these are repeated for posterity below).<sup>22</sup> An offending rate is then calculated by dividing the total number of self-report offenses by the total number of days in the recall window where the individual was not in a secure

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<sup>22</sup> The crimes included for these statistics include destruction of property, setting fire to a building, entering a building to steal, shoplifting from a store, bought/received/sold stolen property, used checks/credit cards illegally, stole a car or motorcycle, sold marijuana, sold other illegal drugs, carjacked someone, drove drunk or high, paid someone for sex, forced sex on someone, shot someone, shot at someone, murdered someone, took something by force using a weapon, took something by force not using a weapon, beaten someone up badly enough for them to go to the hospital, been in a fight, beaten up someone as part of a gang, carried a gun, broke into a car to steal something, and stole a car to ride around.

facility. There were two versions of this rate – 1) a rate including drug offenses and, 2) a rate excluding drug offenses. Considering that drug offenses are measured on a slightly different metric than the remainder of self-report offenses, it was determined that prior analyses would only include a logged version of the latter, since the former was prone to inflation (and, further, that additional independent variables already measure drug sales in a different manner – illegal employment rates/ variety of illegal employment). For the following analyses, I will use a non-logged version of both outcomes that are differenced across recall periods.

*Change in the Versatility of Offending.* Though not a traditional measure as it pertains to desistance trends, offending versatility is, at least in part, a measure of an individual's willingness to offend when an opportunity presents itself. Granted, a part of the changes in offending versatility over the course of all recall periods might be the result of offending specialization – but extant literature on specialization (Mazerolle, Brame, Paternoster, Piquero, & Dean, 2000; Piquero, Paternoster, Mazerolle, Brame, & Dean, 1999) suggests that it is fairly atypical<sup>23</sup>, and that many active offenders exhibit a “cafeteria” style of offending (Braga, McDevitt, & Pierce, 2006; Klein, 1995) choosing to offend when they see opportunities for it with little regard for the type of offense entailed (save for, say, the distinction between property and violent offenses<sup>24</sup>). For the

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<sup>23</sup> This research also suggests offending diversity is higher (albeit very slightly) among early-onset offenders (Mazerolle et al., 2000). With an average at first offense of about 10 years old, this sample almost certainly fits the definition of early-onset offenders.

<sup>24</sup> Extant research suggests that the probability of future violent offenses among those who have already committed a violent offense is greater than one would expect if offending were random, which would suggest that these variety scores should justifiably be split into property and violent dimensions (Brennan, Mednick, & John, 1989).

proceeding analyses, offending versatility is measured as the sum of all SRO dummy indicators within each recall period, further separated into property and violent crime variety scores. These scores are then differenced across recall periods.

## VII-2: ADDITIONAL INDEPENDENT VARIABLES

Independent variables from the first set of analyses are largely repeated here (the exception being several employment and criminal offending measures), but two additional measures are incorporated into the following analyses to account for the influence of impulse control, suppression of aggression, general temperament, and consideration of others as they collectively relate to offending behavior. Additionally, the dependent variables from preceding analyses will now be included as regressors (i.e., AWFL Opportunities/Expectations scores, FOI, MtS). Further, the employment rate used in prior analyses is now categorized (into tertiles<sup>25</sup>), and will be used to run estimation models within categories of post-program participation employment rates (i.e., lower, middle, and upper tertiles). This procedure is adopted in order to ascertain whether offending outcomes are conditional on the success of programs in enabling individuals to obtain gainful employment. In regard to the signaling hypothesis (i.e., consistent with cognitive transformation/identity change theories), I would predict that group main and interaction effects for the VOL group are not systematically different with respect to the level of employment, suggesting that their voluntary participation is representative of

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<sup>25</sup> A median split is also conducted – all models are estimated again for this alternative categorization. Since substantive results are similar, the tertile split is used in the following analyses since it provides a better distinction between very low levels of employment (0 to 3.1%), average values (3.11 to 35%) and above average values (35.1 to 100%).



their underlying intentions to desist, regardless of whether they are able to obtain consistent, gainful legal employment. In contrast, if the human capital explanation (i.e., consistent with the age-graded informal social control theory) is correct, then group main effects and interactions should be different across levels of post-program employment for all groups. This would suggest that differences between individuals in the success of program participation influence offending, regardless of the nature of their participation.

***Weinberger Adjustment Inventory (WAI).*** The WAI is administered at the baseline and all following interviews. Originally developed by Weinberger and Schwartz (1990), the WAI consists of 22 items corresponding to 4 subscales: impulse control (e.g., I say the first thing that comes into my mind without thinking enough about it”), suppression of aggression (e.g., “People who get me angry better watch out”), and consideration of others (e.g., Doing things to help other people is more important to me than almost anything else”) Participants ranked the extent to which each of the 22 statements represented their behavior over the six months leading into the interview (from 1 “False” to 5 “True”).

Responses were reverse-coded when necessary so that higher values within each subscale correspond to more impulse control, suppression of aggression, and consideration of others. Initial sensitivity checks revealed a troubling degree of collinearity between the computed scores for two of these subscales – impulse control suppression of aggression – so principal components factor analysis was conducted to combine these measures into one factor (eigenvalue=1.57, all factor loadings at or above

.88).<sup>26</sup> The remaining subscale – consideration of others – did not coalesce well into this factor, and is included as a separate measure in the following analysis.

### VII-3: METHODS – ESTIMATING THE EFFECT OF PROGRAM PARTICIPATION ON DESISTANCE (HYPOTHESES 3 & 4)

The current analysis employs a random-intercept panel model due to the nested nature of the data – i.e., time-variant measures are nested within persons, themselves having time-invariant personal characteristics that may influence coefficient estimates. Since the primary concern of this analysis is to estimate the influence of selection – i.e., the extent to which individuals sending signals are systematically different from those who do not – it is essential that the model be able to ascertain the extent to which person-level characteristics account for variations in offending. Further, in accord with Hypotheses 3 & 4, models estimates are estimated conditional on employment levels post-program participation. Specifically, the employment rate after participation is separated into three roughly equal-sized tertiles representing low rates of employment (0 to 3.1%), low to medium levels of employment (3.11 to 35%), and medium to high levels of employment (35.1 to 100%). Three models for each dependent variable are then estimated within these categories. This procedure is adopted in order to provide a comparative test of the signaling and human capital explanations for desistance. If VOL participants are indeed sending signals (and MIX participants a partial signal) I would expect that coefficient estimates would follow a similar pattern (i.e., lower offending

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<sup>26</sup> Factor analysis was first conducted for each recall period separately to assess the degree to which the two subscales coalesced into one factor over time. Since factor analysis results remained largely the same across recall periods, the factor itself was computed over all recall interviews.

rates/variety scores) regardless of the level of employment the individual attains post-participation. In contrast, I expect that INVOL participants will be more susceptible to the influence of employment and, therefore, the main effects of being in the INVOL group and their interaction with time-variant treatment indicators should exhibit changes in accord with their modal level of employment after program participation.

Since all dependent variables have been differenced, and are thus no longer in their original ordinal format, a random-intercept OLS model is fit to the data, taking on the following form:

$$(Y_{it} - Y_{it-1}) = \alpha + \delta D_i T_{it} + \beta X_{it} + \gamma W_i + u_i + e_{it}$$

Where  $(Y_{it} - Y_{it-1})$  is a differenced outcome variable, representing the recall-to-recall change in the various attitudinal measures used in this analysis,  $\alpha$  is the model intercept,  $\beta$  a vector of coefficients for level-1 regressors and  $X_{it}$  are independent variables for person  $i$  measured at time  $t$ ,  $\gamma$  a vector of coefficients,  $W_i$  are time-stable regressors for each person  $i$ , and  $u_i$  the person-level (level-2) deviation from the model intercept with the person-recall (level-1) equivalent being  $e_{it}$ . Finally,  $\delta$  is a vector of coefficients for the cross-level interaction between  $D_i$  - a vector of person-stable indicators of treatment group (control, only voluntary, only involuntary, mixed) - and  $T_{it}$  a time-varying dummy indicator for whether the recall period for person  $i$  at time  $t$  is pre- or post-program participation.

In order to adequately compare the effect of program participation, it was necessary to create a simulated control group to compare voluntary, involuntary, and mixed participation individuals to. As such, I implemented a random selection procedure

designed to both 1) randomly select individuals and, 2) randomly select recall periods within them for comparison. This procedure, explained in full detail in Appendix A, is completed ten times, resulting in ten potential control groups for comparison. Fixed-Effect OLS models are estimates using each of these ten groups, and then coefficients are compared using seemingly unrelated estimation (using the “suest” command in STATA). For all of the following models in this section, there existed no significant differences in coefficients using any of these control groups, essentially indicating that the choice of control group for the models presented below is arbitrary.

An underlying issue with a random-intercept panel model specification is that the number of observations within level-2 clusters (persons) is minimal (the “low t” problem), here being a maximum of 10. The attendant complication becomes having to effect a precarious balance between producing efficient slope estimates in the guise of random-effects or more consistent ones in the form of fixed-effects. Therefore, two versions of each model are estimated – 1) a random-intercept model and, 2) a fixed-effects model. Results are then compared using Hausman specification tests, which take the general form:

$$H = (\beta_{RE} - \beta_{FE})' \hat{\Sigma}^{-1} (\beta_{RE} - \beta_{FE})$$

Where  $\beta_{RE}$  and  $\beta_{FE}$  are a vector of coefficients from the random-effects and fixed-effects models, respectively and  $\hat{\Sigma}^{-1}(\beta_{RE} - \beta_{FE})$  represents their covariance matrix. The test is asymptotically distributed chi-square, with the null hypothesis that coefficient estimates across models are not systematically different and, therefore, that either 1) the random-intercept model is both efficient and consistent or, 2) that the fixed-effects model is so

inefficient as to outweigh any of its gains in consistency. If results are significant, alternative models are then estimated expressing certain coefficients as fixed-effects within the random-effects estimator (accomplished by adding a level-2 regressor representing that variable's person-level mean over all recalls) and Hausman specification tests are then re-estimated. If significant results persist even after these coefficients are estimated as fixed-effects, it is determined that the fixed-effects model is preferred to the random-intercept model due to the former's consistency.

In all models presented below, initial Hausman tests were significant and indicated that several variables exhibited large differences in their random- and fixed-effects. Cluster means (i.e., within-person averages over all recall periods) were then added for these variables and Hausman tests were re-estimated. Since all resulting tests were insignificant, random-intercept models were used instead of fixed-effects.

## **VIII. RESULTS – ESTIMATING THE EFFECT OF PROGRAM**

### **PARTICIPATION ON DESISTANCE (HYPOTHESES 3 & 4)**

#### **VIII-1. DESCRIPTIVE ANALYSIS**

*Offending Rates (Incl. Drugs).* In contrast to the figure (Figure 8, below) depicted for offending rates excluding drugs, offending rates including drug sales (Figure 7) tend to be twice as high as those excluding them. This is likely due to the different metric with which these crimes are measured – as opposed to a burglary, car theft, or an assault, drug sales take fairly little time to occur, and respondents can report many drug sales in short periods of time. It is of no surprise, then, that offense rates including drugs are much

higher than those excluding them. Some interesting patterns do emerge. Namely, that offending rates excluding drugs are much higher for the INVOL group than we see in Figure 8, suggesting that a sizable proportion of their SRO offenses are related to the selling of drugs. The VOL group exhibits two jumps in offending rates leading into treatment, then their rates drop dramatically leading into treatment and remain somewhat stable thereafter (rates are almost the mirror-image in Figure 8, below). Finally, the MIX group are set apart from all others in their low, stable rates of offending over all recall periods. Interestingly, the pattern depicted for offending rates including drugs differs very little from that excluding drugs, suggesting that the MIX group is unique in respect to their relatively minimal levels of offending, regardless of the offenses being considered.

**\*\* FIGURE 7 HERE \*\*\***

*Offending Rates (Excluding Drugs).* Rates of offending per day in the community tend to remain stable and low (0 to .5 offenses per day) throughout the observation window for all but the VOL group. This group exhibits a jagged, climbing pattern into the period of program participation, when rates drop precipitously and remain that way for all following time periods. Most groups, in fact, seem to drop at least slightly in their offending rates going into the period of program participation. After participation, the VOL and INVOL groups are nearly indistinguishable, while the MIX group always maintains the lowest values of offending after  $t=0$ .

Here, it might be argued that there could be signaling emanating from the MIX group. Though they are ordered by the court to participate, they do engage in some voluntary participation either a) at the same time as their court-ordered program or, b) after they are no longer required by the court to participate. The low, almost stationary

offending rates among this group distinguish them somewhat from other groups, but it remains to be seen whether this difference holds after accounting for salient control variables that might influence these rates. There is also slight evidence for signaling among VOL participants reminiscent of a “structural break” (Paternoster & Bushway, 2009) in their offending leading into their voluntary participation, as their offending rates, including (Figure 7) or excluding (Figure 8) drugs drop precipitously into the recall period of their initial participation.

**\*\* FIGURE 8 HERE \*\***

*Property Offending Variety Scores.* Each of the VOL, INVOL, and MIX groups follow somewhat dissimilar patterns with respect to the variety of property crimes they engage in. The VOL group exhibits some of the lowest scores across all recall periods, but paradoxically increase their scores (i.e., increase the variety of their property offending) leading into program participation, then scores decline to pre-participation levels after  $t+2$ . In contrast, the INVOL group begins with mid-level variety scores that largely decline over the observation window, but most particularly so in the recall periods directly after participation. The MIX groups begins at the highest variety scores for any group over the entire observation window, and begins a large decline leading into program participation, followed by a fairly stable score of approximately .85 into  $t+4$ . It is important to note, however, that all scores are relatively low – just two points cross a threshold of 1, while the maximum possible score is 11.

**\*\* FIGURE 9 HERE \*\***

*Violent Offending Variety Scores.* Violent offending is a rarity for all groups, as variety scores all remain below 1 (out of a total possible score of 8). All groups exhibit overall declines across the observation window but display slightly different patterns. The VOL group begins low, having scores between .4 and .5, increases slightly into the period of program participation, then declines through  $t+5$  to pre-participation levels of violent offending. The INVOL group, in contrast, begins at somewhat higher scores (roughly .6), increases into the period of program participation, then declines considerably after  $t+1$ . Finally, the MIX group exhibits an irregular pattern – they begin at high levels, decline precipitously into  $t-1$ , then increase in the period of program participation and  $t+1$ , thereafter declining to join the remaining groups at scores around .4.

In addition to the overall trend, there is slight evidence here that program participation might have some influence on violent offending variety scores. Pre-participation trends are slightly irregular across groups, particularly in the recall periods directly before program participation. This changes after  $t=0$ , as all groups exhibit a decline in violent offending, just differing slightly in the speed and magnitude of this decline. Attributing this decline to participation, however, would likely be erroneous, as the control group displays a similar downward trend over time.

**\*\* FIGURE 10 HERE \*\***

## VIII-2: DESCRIPTIVE ANALYSIS – DISCUSSION

Trends in offending rates and offense diversity (separated into property and violent dimensions) exhibit patterns suggestive of the notion that signaling among the



VOL or MIX group is occurring but, also, that program participation may exert some influence on criminal behavior. Namely, the VOL group exhibits sharp declines in their offending rates leading into program participation, which may be emblematic of a structural break in their offending patterns. Meanwhile, the MIX group, sending arguably a partial signal, exhibit some of the lowest offending rates and offense diversity among all groups, irrespective of the timing of program participation. Additionally, there is some slight evidence that the INVOL group may be influenced by these programs, as their offending rates and offending diversity tend to see moderate to sharp declines post-program participation. It is important to note for violent offense diversity, however, that all groups, including the randomly selected CTRL group, demonstrate declines in violent offending over time, so these trends are likely unrelated to programs and more representative of an overall pattern of decreased violent offending with age.

### VIII-3: RANDOM-INTERCEPT MODELS

*Offending Rates (Incl. Drugs).* Reported in Table 6, below, are the results from three random-intercept models predicting changes in offending rates (including drug offenses) within each category of post-participation employment rates.<sup>27 28</sup> With the exception of one interaction effect, all main and interaction effects in this model are insignificant. In

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<sup>27</sup> As was the case for earlier tables reporting random-intercept results, only those variables having a significant coefficient in at least one category of post-program employment rates are reported in these tables. Full results are available in Appendix B.

<sup>28</sup> Reliabilities and intraclass correlations are computed using non-differenced outcome variables, since the difference transformation disallows the random-intercept model from estimating the amount of variation in these outcomes that is due to level-2 clusters (i.e., the person-level). In addition, all observations are weighted by the proportion of time during the recall period in which the respondent was not in a secure facility. Since the xtreg function in Stata does not allow for weights to vary within observations, these proportions are averaged across all recall periods, and then entered as weights using the “iweight” option for the xtreg command.

regards to this exception, it appears that the INVOL group experiences a decline ( $b=-1.113$ ,  $p<.05$ ) in offending rates post-participation so long as their opportunity costs are sufficiently high (i.e., where participation category equals 1) and they are in the upper tertile of post-program employment rates. This being said, there are some patterns in the direction of group type, treatment, and participation category coefficients. Namely, all main effects for the lower tertile of employment are positive and treatment interactions negative, suggesting that post-program employment (or lack thereof) might exacerbate offending rates, while program participation probably attenuates this, but not to a significant degree.

**\*\* TABLE 6 HERE \*\***

Both gang membership and contact frequency with fellow gang members significantly influence changes in offending rates, but there is evidence of cluster confounding, as initial sensitivity tests suggested the need to include cluster averages for both terms in these models. Since cluster averages are stable within person, they may be interpreted as departures from the model predicted intercept holding all other regressors constant. In the case of gang membership, the expected difference in intercept values between a respondent never reporting gang membership and a respondent always reporting gang membership is 6.153 ( $p<.01$ ), but this is only relevant for individuals in the upper tertile of post-program employment. Contrary to what might be expected, contact frequency with fellow gang members is not universally associated with higher offending rates. While it appears that the fixed-effect estimate of contact frequency is positively related to offending rates for some individuals (lower tertile of employment,  $b=1.727$ ,  $p<.05$ ) it also appears to negatively influence rates as well (middle tertile,  $b=-$

.993,  $p < .01$ ), and its cluster average effect is also negative for the upper tertile of employment ( $b = -1.164$ ,  $p < .05$ ).

Moving to characteristics of illegal work, the fixed-effect estimate of illegal work variety is universally associated with increases in offending rates across all levels of employment, indicating that more diversity in illegal work is, as is to be expected, resultant in higher rates of daily offending. This being said, there were cluster confounding issues for illegal work variables, and this was particularly the case for the upper tertile of employment. Opposite of what was anticipated, the cluster average effect for the variety of illegal work is negative ( $b = -10.584$ ,  $p < .001$ ) for individuals in the upper tertile of post-program employment. Congruent with expectations, however, is the estimated effect of the cluster average for the rate of illegal work, which indicates higher offense rates for those with higher person-level average rates of illegal employment ( $b = .157$ ,  $p < .001$ ).

Finally, both peer influences and substance abuse are related to offending rates. In regard to the former, as the percentage of one's peers that have ever been arrested increases, individual offending rates are expected to decline ( $b = -1.460$ ,  $p < .01$ ) while the opposite effect is reported for the percentage of peers ever having been jailed ( $b = 1.662$ ,  $p < .01$ ). Since the frequency of substance abuse exhibited cluster confounding issues in sensitivity analyses, its person-level cluster average is included in the model, and indicates that individuals having higher average frequency of abuse scores tend to also have higher offending rates ( $b = .115$ ,  $p < .05$ ), but this effect is only significant for those in the upper tertile of post-program employment.

Model auxiliary statistics illustrate that level-2 variation accounts for a greater proportion of variation among those in the upper tertile of employment ( $\rho=.346$ ), relative to the lower and middle tertiles ( $\rho=.236$  and  $.176$ , respectively). In addition, all reliability values are of adequate degree (all above  $.88$ ) and Hausman specification tests all returned as insignificant after cluster confounding variables were included in the model (for full list of covariates, see Table B-7 in Appendix B).

*Offending Rates (Excl. Drugs).* Reported in Table 7, below, are the results from three random-intercept models predicting changes in offending rates (excluding drug offenses) within each category of post-participation employment rates. Of immediate note is that, across all models, the coefficients for group type and their interactions with time-varying treatment indicators are universally insignificant. However, there are some important patterns of which to take note. First, main effects for the VOL group are negative under most levels of employment ( $b=-.052$ ,  $b=-.648$ ) or very close to zero (upper tertile,  $b=.006$ ). The INVOL group exhibits a more pronounced negative pattern over all levels of employment ( $b=-.037$ ,  $b=-.197$ ,  $b=-.096$ ) and main effects for the MIX group are positive under low and high levels of employment ( $b=1.978$ ,  $b=.048$ ) and negative under medium levels ( $b=-.327$ ). Though there may be slight evidence for signaling here (in that most main effects for the VOL group are negative and all treatment interactions insignificant) it is very weak, as the INVOL group's estimates would be more representative of what we might expect if that group were signaling.

**\*\* TABLE 7 HERE \*\***

In examining marginal effects, though, we find more evidence for a signaling explanation. Computed for the average individual in the analysis (i.e., average values on continuous regressors, zeros on dummy indicators) the marginal effects associated with group category suggest that the main effects for the VOL group are indeed routinely negative ( $b=-.004$ ,  $b=-.174$ ) or indifferent from zero in a positive direction ( $b=.004$ ) whereas the directional patterns for other groups are either inconsistent or seem to be related to the level of employment. There is also very slight evidence that higher opportunity costs differentiate signalers. In respect to the participation category and treatment indicator, we see that members of the VOL group have lower expected offense rates ( $b=-.260$ ,  $b=-.033$ ) so long as their participation level (i.e., opportunity cost) is of a sufficient level and employment is at or above medium levels post-participation. Nevertheless, all these interactions are insignificant, indicating that if signaling is occurring here, the measures used for signaling (voluntary participation, opportunity costs) have a very weak fit.

Turning to the remaining regressors, we see that being a gang member appears to influence offending rates, but in a negative direction ( $b=-5.634$ ,  $p<.05$ ). This is counterbalanced somewhat by contact frequency with gang members ( $b=1.262$ ,  $p<.01$ ), indicating that the criminal influence of gang membership is limited to those members having more routine contact with the gang. These effects, however, are limited solely to those within the low employment category. For all other categories, the influence of gang membership and gang contact frequency are negligible.

As was true in almost all prior analyses, engaging in illegal work is associated with poorer offending outcomes – namely, that every unique type of illegal work engaged

in during the recall period appears to coincide with a change in the offending rate of approximately 2.763 crimes *per day* in the community, controlling for an individual's average rate of illegal work ( $b=.014$ ,  $p>.10$ ) over the observation window and within the recent recall period ( $b=-.030$ ,  $p>.10$ ). Since a quarter of the illegal work variety score has to do with the sale of illicit drugs (2 items out of 8), this is likely what drives this coefficient, as average offending rates including drugs are almost twice as high as those excluding them. Interestingly, these covariates also exert influence over the offending rates of those in the highest post-program employment category. Although the direction of the relationship remains the same, effect sizes differ. For example, every additional type of illegal work engaged in appears to result in a .240 increase in the offending rate, which is a tenth of the expected increase among those with lowest levels of employment ( $b=2.763$ ,  $p<.001$ ).

Regarding psychological scales, both the moral disengagement and Weinberger Adjustment Inventory (WAI) scales are significantly associated with offending rates. Consistent with what one may expect, greater levels of socio-emotional development – in the form of higher levels of impulse control and suppression of aggression – are associated with lower offending rates ( $b=-.980$ ,  $p<.05$ ), but only for those with low levels of post-program employment. In contrast to expectations, higher levels of moral disengagement (e.g., it is morally permissible to treat others poorly) coincide with lower rates of offending ( $b=-.050$ ,  $p<.05$ ), but only for those with somewhat average levels of employment. However, though this coefficient might be significant, its substantive impact is questionable. Given that the moral disengagement scale is an average of 32 items measured on a 3-point Likert scale, its maximum influence on offending is quite

low – the expected difference in the change in offending rates between an individual at the minimum (1) and maximum (3) of this scale is only roughly .10 offenses per day. Further, the limited range of this measure might obscure important differences between individuals at the middle of this scale (2) and its outer extremes. Taken together, it is prescient to interpret any coefficients associated with the moral disengagement scale with a healthy dose of skepticism.

Moving to social support measures, both the logged number of caring adults and domains of social support appear to influence offending rates, but in opposite directions. Consistent with what one may expect, the larger the number of caring adults a respondent reports, the lower their rate of offending is expected to be ( $b=-1.453$ ,  $p<.01$ ), on average and all else equal. Conversely, as the number of different social support domains in which a respondent reports at least one caring adult increases, so too does the offending rate ( $b=.401$ ,  $p<.05$ ).

Again, there is a clear disagreement between these findings, but the scaling of both variables might help in reconciling this disparity. Specifically, the total number of potential domains is 8, while the (un)logged number of caring adults can range from a minimum of 0 (1) to a maximum of 6.29 (541). This suggests that the aggregate impact of the domains variable is limited, and could easily be overcome with greater numbers of caring adults. Further, it is possible within the domains measure that a small number of unique adults are mentioned across all items – this could indicate that the respondent relies on fewer caring adults for a higher variety of social support, potentially indicative of a resource-strained social network. Finally, the significance of this relationship, as was

true for gang involvement and illegal employment, is limited solely to individuals with the lowest levels of post-program employment.

In regard to model auxiliary statistics, it is apparent that person-level unobserved characteristics account for a good deal of the unexplained variation in offending rates excluding drugs ( $\rho=.3480$  to  $.4034$ ). However, the model predicting offending rates for those with low levels of employment accounted for all of this person-level variation (i.e.,  $\rho=0$ ), suggesting that included covariates explain all of the important person-level disparities in offending rates excluding drug offenses. Further, all reliability statistics were of adequate level (.88 or above) and all Hausman specification tests returned as insignificant after including person-level averages for several cluster confounding regressors.

*Property Offending Variety Scores.* As was true for offending rates, all group, treatment, and participation category main effects and interactions are insignificant. However, several marginal effects are significant, and suggest that treatment exerts a negative impact on property offense variety ( $b=-.059$ ,  $p<.05$ ) and that the INVOL group is expected to have lower variety scores ( $b=-.106$ ,  $p<.05$ ), but these effects are only present for those with high levels of post-program employment.

**\*\* TABLE 8 HERE \*\***

Perhaps most important is that patterns of the direction and magnitude of coefficients for the main effects of group type bear similarities to those from the preceding model. Specifically, coefficients for the VOL group, though they shift slightly



in direction from low- to medium-levels of employment, remain very low, almost indifferent from 0. In contrast, it is evident that other groups exhibit clear differences in the size and direction of their coefficients with respect to levels of employment. Albeit insignificant, the coefficients for the INVOL group vacillate between positive ( $b=.317$ ), near-zero ( $b=-.068$ ), and negative ( $b=-.156$ ) in accord with increasing levels of post-program employment. Similarly, the MIX group has significantly greater negative changes in the variety of their property offending only under medium levels of employment – this same coefficient under different levels of employment is either positive ( $b=.291$ ), or negative and insignificant ( $b=-.055$ ). Therefore, though the majority of these results are insignificant, their direction conforms to what we might expect if voluntary program participation is a signal (albeit, an obviously weak one if so) – main effects for the VOL group imply that they are not affected by employment to the extent exhibited by other groups, possibly indicating that their apparent resiliency is indicative of signaling.

Predictably, the number of interruptions in legal work influences property offending diversity, with every interruption being associated with a .257 or .112 increase ( $p<.05$ ) in the variety score. The fact that this effect is only evident for those in the medium- or high-level employment categories makes practical sense. That is, individuals in the low category are likely to have very few interruptions (i.e., since they are serially unemployed) or to have none at all (i.e., constant unemployment) so the majority of the movement in this measure is likely contained within the medium- or high-employment categories.

In terms of more illicit types of employment, all variables measuring illegal work are almost universally significant within each level of legal employment. As it regards the fixed-effect portion of the illegal employment rate, those within the low employment category are expected to have positive changes in their property offense diversity ( $b=.012, p<.01$ ) for every 1 percentage point increase in the rate of illicit employment. Conversely, this effect is in the opposite direction for those in the highest employment category ( $b=-.007, p<.01$ ), paradoxically indicating that an increase in illegal employment *reduces* property offending diversity. In contrast, increases in the variety of illegal work appear to exert significant and positive effects on property offense variety ( $b=.657, p<.001$ ;  $b=1.377, p<.001$ ;  $b=1.411, p<.001$ ) controlling for person-level average illegal work variety and regardless of the level of post-program employment.

Both moral disengagement and the frequency of substance abuse are associated with property offense diversity, but differ in respect to the distribution of their effects across employment categories. The former's influence is positive ( $b=.032, p<.05$ ) under low levels of employment, but negative ( $b=-.015, p<.05$ ) under high levels. As before, however, this effect is substantively minimal – the moral disengagement scale ranges from 1 to 3, meaning that individuals at the opposite ends of the scale are only expected to have changes in their property offense diversity that are about .06 or .03 points apart. In contrast, the substantive influence of substance abuse is much more profound – having a positive influence on changes in offending diversity for all employment categories, the anticipated differences between individuals with minimal and substantial substance abuse problems is much larger. For those with medium levels of employment, even those with relatively average levels of substance abuse (see Table 4) would be expected to have

variety scores .45 higher than individuals with little or no substance abuse issues. Though this effect is somewhat smaller among those with higher levels of post-program employment, this same statistic among this group would be .25, still a sizable difference in expected offending diversity.

Finally, model auxiliary statistics demonstrate that unobserved person-level characteristics account for a minimal amount of the unexplained variation in property offense diversity (ICCs between .0725 to .1592) and also that reliability estimates are of adequate level (above .89). In addition, all Hausman specification tests return as insignificant after person-level averages of cluster confounding variables are included in these models.

*Violent Offending Variety Score.* With the exception of the marginal effects for treatment and the INVOL group among those with high levels of post-program employment, all main effects for, and cross-level interactions between, group, treatment, and participation category indicators are insignificant. Departing from the findings of prior models, the main effect for the VOL group varies across employment levels and is at times positive ( $b=.241$ ,  $b=.116$ ) and negative ( $b=-.033$ ). However, all marginal effects are negative, suggesting that when the influence of being in the VOL group is evaluated for the average individual, it is associated with lower violent offense diversity. Further, cross-level interactions between group type and post-treatment periods indicate a varied trend for the VOL and MIX groups, with negative effects at low and medium levels of employment and positive effects at high levels. In contrast, treatment interactions for the INVOL group are negative, perhaps suggesting that treatment exerts a very weak

influence on their violent offense diversity post-participation. Additionally, participation category interactions illustrate that higher opportunity costs are typically associated with lower violent offense diversity for the VOL group, thus yielding weak evidence in support of Hypothesis 4.

**\*\* TABLE 9 HERE \*\***

As was true for property offense diversity, interruptions in legal employment are associated with greater diversity in violent offending ( $b=.176$ ,  $p<.05$ ), but only for individuals having a medium-level of post-program employment. Additionally, illicit work displays nearly the same pattern of results as it did with property offending, differing only in the magnitude of its effect. Specifically, the coefficients for the variety of illegal work and cluster average of the illegal employment rate are in the very same direction, but are approximately one-quarter to one-half the size of these same coefficients in the model predicting property offense diversity.

As opposed to prior models, information regarding the recall period (the number of months in the recall period) and a missing indicator for BSI are all significant for at least one employment group. In reference to the former, as the number of months in the recall period increase, the expected violent offense diversity among minimally-employed respondents increases by .041 crimes ( $p<.05$ ). Regarding the latter missing indicator, an individual who is missing information on the BSI factor score is expected to have a violent offense variety score that is .116 ( $p<.01$ ) points lower than their counterpart with valid BSI information, controlling for the person-level average of this missing indicator.

In regard to the various psychological inventories in this analysis, PSMI scores and WAI factor scores are significantly related to violent offense diversity, conditional on employment level. Specifically, increases in the PSMI inventory are, contrary to expectations, positively related ( $b=.213$ ,  $p<.05$ ) to violent offense variety and this effect is restricted to those with low levels of post-program employment. In reference to the latter, WAI factor scores are negatively associated with violent offense diversity ( $b=-.108$ ,  $p<.001$ ) controlling for person-level average WAI values across all recall periods and this effect is only evident for those with high levels of post-program employment. Finally, the frequency of substance abuse is routinely associated with higher levels of violent offense diversity, regardless of the level of employment and controlling for person-level averages of substance abuse frequency. In addition, these affects are fairly similar across levels of employment, suggesting that the effect of substance abuse is felt similarly for all individuals, regardless of how (in)consistent their employment might be.

Model auxiliary statistics suggest that most of the unexplained variation in violent offense diversity is accounted for by person-level regressors included in these models, as all intraclass correlation coefficients are below .20, thus indicating that time-variant influences are responsible for nearly 80% of the remaining variation in violent offending. Further, all reliability estimates are of adequate level ( $>.89$ ) and all Hausman specification tests returned as insignificant after person-level averages of cluster confounding variables were included in these models. Interestingly, cluster confounding issues were most severe for violent offending relative to property offending and offending rates, suggesting that time-stable person-level characteristics are responsible for much of the differences in violent offending between individuals. This largely

comports with extant research on violent offense specification and theories explaining persistent and adolescent-limited offending.

As was true for models predicting attitudes and orientations, sensitivity analyses were conducted to correct for potential over-specification of these models. That is, since an analysis of signaling is, at its core, an examination of *selection*, the inclusion of a wide variety of regressors in the model might be accounting for the very selection effects that differentiate the VOL group from the others. In light of this, auxiliary models were estimated including only group, treatment, and participation category main and interaction effects along with simple demographic indicators.<sup>29</sup> Results for offending rates including drugs were largely similar, and no significant differences between groups emerged with respect to main or interaction effects. In contrast, results for offending rates excluding drugs indicated that the VOL group had a significant and positive main effect and a significant, negative treatment interaction under certain levels of employment, suggesting that program participation and post-program employment levels exert an effect on their offending rates. In contrast, results for property and violent offense diversity were more representative of a signaling argument supporting the notion that the VOL group is systematically different from the INVOL and, in part, the MIX group. Specifically, several main effects for the INVOL and MIX groups indicated significant and positive coefficients, while treatment interactions were either a) negative for the INVOL group and significant or, b) positive for the MIX group and significant. Once again, these results differed substantially across levels of post-program employment,

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<sup>29</sup> All tables for these analyses are available in Appendix B, beginning with Table B-11 for offending rates including drugs. Models estimating offense rates and offense diversity within the low employment category could not be estimated due to

suggesting that the influence of programs on these individuals is conditional on their success in enabling these individuals to procure consistent, gainful employment.

#### VIII-4 DISCUSSION OF RANDOM-INTERCEPT RESULTS

*Repeat of Hypotheses Tested.* To provide context for the following discussion,

Hypotheses 3 and 4 are repeated below:

**Hypothesis 3.** Conditional on the rate of part-time or better employment (low, medium, and high), the effect of program participation on subsequent desistance patterns should vary according to whether program participation was voluntary, involuntary, or mixed. Specifically, I expect that main and interaction effects for the INVOL and MIX groups will exhibit variation in accord with their level of post-program employment, while effects for the VOL group should be similar across all levels of employment, emblematic of their underlying intentions to desist as signaled by their voluntary program participation.

**Hypothesis 4.** The degree to which this estimate varies should be correlated with signaling costs. That is, VOL participants (and to a lesser degree, MIX participants) whose involvement in the program has greater intensity (i.e., more frequent attendance) should exhibit more pronounced differences in desistance patterns when compared to those who participate more infrequently. Additionally, this should hold true regardless of the success of the signal (i.e., at all rates of employment). I would also expect coefficients

to differ between low and high frequency participants in the INVOL and MIX groups (less so for the latter, since they send a partial signal), but these estimates should be conditional on the level of employment, consistent with a human capital explanation (i.e., age-graded theory of informal social control) for desistance.

*Discussion.* In regard to Hypothesis 3, it was expected that offending rates and offense diversity should exhibit little to no changes for the VOL group conditional on their level of post-program employment as is consistent with a signaling interpretation of their voluntary participation status. Additionally, this may also be true for the MIX group, given that they also participate in a program voluntarily at some point, even if their initial reason for participation was in response to a court order. In contrast, I would expect that the INVOL group exhibits differences in offending patterns across levels of post-program employment as is consistent with a human capital explanation for the influence of program participation on this group.

Although very few group main effects and treatment interactions are significant (namely, marginal effects in the offense diversity models) some patterns do emerge suggesting that individuals in all groups are, to some degree, influenced by their success in obtaining employment in recall periods after their program participation. Under the low employment category, it was routinely true that group main effects were positive, suggesting that signaling groups are not differentiated well when post-program employment is minimal. Further, effects often shifted from negative to positive as post-program employment increased, indicating that, regardless of the nature of participation, medium- to high-levels of post-program employment may be necessary in order for



individuals from any group to decline in their offending, irrespective of why they participated in the first place. Also inconsistent with the expectation from Hypothesis 3 are the estimated effects of treatment. Though it was again true that no treatment interactions were significant, it was often the case that interaction effects were negative under low levels of employment and either indifferent from zero or positive under higher levels of post-program employment (the exception being offending rates excluding drugs, where all estimated effects were positive, but still insignificant). This suggest that the influence of treatment is also conditional on post-program employment, inconsistent with the notion that VOL or MIX groups are sending a signal of their intentions to desist with their voluntary participation.

In regard to Hypothesis 4, it was expected that the opportunity costs associated with program participation (i.e., the frequency of participation in the program), might better differentiate those sending poorly *fit* signals and those sending signals of a better *fit*. That is, higher opportunity costs should correlate with greater signal *fit* in the respect that the signal being sent is better representative of the underlying unobservable quality meant to be signaled for those paying a higher cost to transmit this signal. Additionally, the estimated effects of these interaction effects should not differ substantially over levels of post-program employment for those sending a signal, but should for individuals participating only involuntarily. As was true for Hypothesis 3, these expectations are largely not met. Rather, the estimated effects for participation category interactions appear to also be conditional on the level of post-program employment for all groups, thus suggesting that a human capital explanation is better supported by these data. This being said, it was often the case that estimates for the VOL group were negative at

medium- or high-levels of post-program employment, providing weak support for the notion that these individuals are sending a signal of their intentions to desist through voluntary participation, but that failure to obtain employment after the program may influence a signaler's resolve to stay committed to these intentions.

Over concern for over-specification, auxiliary models were estimated only including group, treatment, and participation category main and interaction effects along with simple demographic indicators. In regard to offending rates, results were either inconclusive (i.e., no significant main or interaction effects) or suggested that VOL participants were influenced by program participation (for offending rates excluding drugs). In contrast, sensitivity analysis results for offense diversity suggested that the INVOL and MIX groups had significantly higher offense diversity, and were influenced in different directions by program participation. Most importantly, however, is the fact that estimated group and main effects for the VOL group were insignificant, but did largely suggest that their offense diversity was typically lower than that of other groups and they were not influenced by program participation. In this, there is slight evidence for a signaling explanation of these apparent group differences, indicating that the VOL group is different with respect to offense diversity and the influence of programs. However, effects were again conditional on post-program employment for all groups, again indicating slight support for a human capital explanation.

## IX. CONCLUSION AND DISCUSSION

A central thesis in criminological research is that, while many individuals offend at some point in their lives, most only do so temporarily, and to a limited extent. Those that persist in crime into their early- and late-adulthood have been the topic of rigorous academic debate concerning the anticipated length of their criminal careers (Blumstein & Cohen, 1987; Blumstein, Cohen, & Farrington, 1988; Farrington, 1987), and the factors that eventually contribute to their desistance from crime (or lack thereof) (Laub & Sampson, 1993b, 2001, 2003; LeBel et al., 2008; Maruna, 2001; Paternoster & Bushway, 2009; Sampson & Laub, 1995; Sampson et al., 2006; Warr, 1998; Wright & Cullen, 2004). While this debate will undoubtedly continue into the near future, there exists a call for actionable knowledge concerning how we might identify those who intend to desist earlier in this process, rather than waiting for a period of 7 to 10 years to deem an individual “safe” with respect to their risk of arrest for a new crime (Kurlychek et al., 2006, 2007; Kurlychek et al., 2012).

One such method of identifying desisters earlier in the process is to look for signals they are sending related to their unobservable intentions to desist. Originally proposed by Spence (1973), signaling theory suggests that individuals may engage in behaviors that come at some cost to themselves in order to transmit a message to potential receivers of some underlying quality that the receiver may not directly observe, at least at that time. In this sense, a signal is not simply a characteristic of the individual or behavior that predicts some outcome, but it must come at some cost to the individual, such that transmitting the signal itself is onerous for those not having the unobservable quality (i.e., dishonest signaling). More recently, Bushway and Apel (2012) provided a

means for signaling theory to be integrated into the extant literature on criminal desistance. Namely, their argument rests on a common finding that job program participants who *complete* the program are often quite different from those who participate but fail to achieve all program milestones with respect to their subsequent criminal offending. In this, program completion is a costly signal, for the requirements can be strict, and prove too difficult for those lacking full intentions to desist to achieve.

In the economics literature on signaling, tests of the theory universally place signaling in opposition to its antithetical counterpart – human capital theory (Bedard, 2001; Chevalier et al., 2004; Johnes, 1998; Kroch & Sjoblom, 1994; Lang & Kropp, 1986). That is, an alternative perspective on the arguments put forward by Bushway and Apel (2012) is that job training completion is not a signal, but instead increases an individual's level of human capital such that subsequent offending comes at a greater cost to them as compared to individuals who merely participated in the program. This is evocative of a pre-existing polemic within criminology – namely, that desistance is largely “accidental” and happens to an individual without specific effort on their part due to turning points in the life course (Laub & Sampson, 2003; Sampson & Laub, 1995) or, alternatively, that these turning points are merely “hooks “ for change, and that individuals express personal agency in making the decision to quit offending and to change their identities to match their reimagined status as an individual who has left the criminal lifestyle (Giordano et al., 2002; Giordano et al., 2007; LeBel et al., 2008; Maruna, 2001).

As is argued in the preceding sections, these theories, also informed by the signaling versus human capital debate, may be used to inform a test of these explanations

for desistance as it relates to another proposed “signal” of an individual’s unobservable intentions to desist – the voluntary or involuntary nature of program participation.

Conceptually, voluntary program participation presents itself as a potential signal due to the fact that it is resultant of a personal choice on the part of the participant and that participation comes at some cost to the individual. Using data on 1,354 active offenders in the PtD data, the current dissertation tested the extent to which voluntary participation in job training, mental health, and substance abuse programs may be conceptualized as a “signal” of an individual’s intentions to desist.

Results suggest that if voluntary participation is a signal, it is one of poor fit. That is, voluntary participation often failed to significantly differentiate those whose participation was entirely (VOL group) or partially (MIX group) voluntary from those whose participation was solely the result of a court order. This being said, estimates of group main effects and their interactions with treatment periods and the cost of participation were at times consistent with what one may expect if voluntary participation is a signal. Namely, that program participation appeared to exert no influence on attitudes or orientations associated with desistance for VOL participants, and that sensitivity analyses correcting for potential over-controlling implied that programs influence these attitudes and orientations for those whose participation was, at least in part, involuntary (i.e., the INVOL and MIX groups). Therefore, though significant results were largely absent, there was some, albeit very weak, support for the notion that voluntary program participation is a signal of underlying intentions to desist.

Turning to offending outcomes conditional on post-program levels of employment, there existed comparatively less support for the signaling hypotheses.

Specifically, offending rates (including and excluding drug sales) and offending diversity (separated into property and violent crime dimensions) appeared to be conditional on post-program employment, such that low levels of employment exacerbated offending for all groups, regardless of the (in)voluntary nature of their program participation. However, as was true for attitudes and orientations associated with desistance, weak patterns did emerge suggesting that VOL participants were somehow different from INVOL and MIX participants in respect to lower rates of offending and offense diversity, but only if post-program employment was at a sufficient level. Additionally, sensitivity analyses correcting for the potential over-specification present in the models presented in this analysis implied again that both the INVOL and MIX groups appear to differ from the VOL group in respect to the main effects of being in these groups (positive and significant for offense diversity) and the estimated effect of treatment on offense diversity (negative effects at varying levels of post-program employment).

In light of these weak findings, it is perhaps important to recognize that desistance is not necessarily a process generated solely by human agency. Rather, it is arguably the case, and is evident here, that “hooks” for change likely exert some impact on an individual’s success in desisting from crime. Thus, it appears that neither the signaling nor human capital explanations for desistance, and their criminological counterparts, may fully explain patterns in desistance. Logically, even the most steadfast individual intending to desist from crime would be deterred from repeated failures in achieving their goals (here posited to be gainful employment) nor will the most criminally prone youth cease their offending as a result of turning points in their lives that substantially increase the informal costs of their offending.

In fact, in respect to the former, desistance under the cognitive/identity transformation perspective is not wholly the result of personal agency, although its foundation certainly resides there. Rather, desistance from this perspective is in part a process of symbolic interaction whereby the individual experiences a change to their own self-identity, begins to act in ways that support it, but are still susceptible to the views of important stakeholders in their life (Giordano et al., 2007; Maruna, 2012; Mead & Mind, 1934). In this, the self (or, identity) is not a concept borne just from personal transformation, but also how this transformation is perceived by others (Maruna, 2012). This notion may help to explain the fact that even voluntary program participants exhibited increased offending rates and offense diversity under low levels of employment. Absent some formal recognition of their changing status (or, at least, their intentions for change) it is quite reasonable that an individual may abandon their new goals and forfeit their new lease on life if these changes fail to be reinforced through outside recognition.

There are also important limitations that may have prevented this study from (dis)verifying whether voluntary program participation could be conceptualized as a signal of intentions to desist. First, the initial round of recruiting for the PtD sample targeted youth adjudicated guilty of felony offenses in juvenile court, resulting in an initial sample whose age ranged from a low of 14 to a high of 19. Though youth were then tracked over a period of approximately seven years, this yet implies that the oldest an individual could be in this data set is 26. Given that extant research on the effect of randomized job training programs has found that only older offenders appeared to gain from the program (Uggen, 2000), this potentially implies that the PtD sample is simply

too young for meaningful signaling results to be discovered. This notion is also bolstered by existing research on the development of the brain's capacity for decision-making which finds that the prefrontal cortex, the area of the brain responsible for higher reasoning functions, is not fully developed until the mid-twenties so many of the youth in the PtD sample are, quite literally, less capable of making informed decisions about their future than their older selves (Steinberg, 2007, 2008).

It may also be argued that an examination of singular signals is unlikely to result in strong findings for or against the argument that the behavior in question is a signal. This is because extant work on signaling in laboratory experiments and natural experiments or observational studies find that individual signals are typically only semi-sorting, at best (Cho & Kreps, 1987; Densley, 2012; Gambetta, 2009; Pyrooz & Densley, 2015). This provides further context for the weak findings for signaling presented in the following sections – I would expect, based upon this literature, that voluntary program participation would only differentiate signalers and non-signalers to a small degree. Further, although this analysis attempted to control for the cost of signals through the frequency of program participation, this cost remains somewhat ambiguous. For example, different programs may have multiple, short meetings that do not equate to programs with fewer, but much longer sessions. Therefore, the unclear assumption that an individual pays a higher cost with greater frequency of participation (i.e., the number of sessions attended in the program) also further weakens the ability of these analyses to differentiate individuals with respect to the cost of the signals they transmit. Given the above, a more fruitful way forward might be to develop a signaling scale that an individual can contribute to over a period of time. That is, rather than using program participation as a



treatment intervention, voluntary program participation, interacted with participation frequency, would contribute to a cumulative signaling scale over recall periods, measuring in addition other prominent achievements – i.e., obtaining a job license, high school degree, GED, or college diploma – that come at some cost to the individual and may be interpreted as signals (see Pyrooz and Densley (2015) for a recent example of such a method). In fact, nascent work on signaling supports this argument, as individuals or groups presenting a wider variety of strong, consistent signals tend to experience better outcomes than those whose signals are few and inconsistent. Given that individuals with criminal histories have already unintentionally transmitted a negative signal through their prior criminal behavior, it makes logical sense that receivers would require multiple signals before entertaining the idea that the individual truly intends to desist.

Finally, the current analysis assumes that receivers of these signals are employers, and initial outcome variables (AWFL, FOI, MtS) were specifically chosen oriented around this assumption. While employers are likely typical receivers of signals for those with criminal histories in the process of turning their lives around, it stands to reason that these signals may also be meant for other stakeholders in their lives. Namely, some signals may be sent to potential/current romantic partners, spouses, or friends and family, suggesting that conditioning offending outcomes on employment is an error in these cases, as the intentions of their signals are not to demonstrate unobservable intentions to desist to employers, but to friends, family, and romantic partners.

Though the preceding analyses failed to find strong support for the notion that voluntary program participation is a signal of underlying intentions to desist, there is some, albeit fairly weak, support for this argument. However, this manuscript hardly

presents the last word on the potential impact of integrating economic signaling theory into desistance research. The ultimate importance of Bushway and Apel's (2012) contribution to this literature lies in the fact that it restores some control over the desistance process to the individuals doing the work to desist, and provides a framework for practitioners and researchers alike to recognize the signals these individuals have already been sending for quite some time. In this, a signaling perspective on desistance provides a clearer channel for their messages to be transmitted, and future work in this burgeoning literature will only help to reduce the static that has prevented us from seeing these messages for what they truly are.

## DISSERTATION TABLES

Table 1 - Descriptive Statistics for Pathways Sample at Baseline and 12, 24, 48, and 84 Month Follow-up Interviews

<i>Variable</i>	<i>Baseline</i>		<i>12 Month</i>		<i>24 Month</i>		<i>48 Month</i>		<i>84 Month</i>	
	<i>Avg. (%)</i>	<i>Std. Dev.</i>	<i>Avg. (%)</i>	<i>Std. Dev.</i>	<i>Avg. (%)</i>	<i>Std. Dev.</i>	<i>Avg. (%)</i>	<i>Std. Dev.</i>	<i>Avg. (%)</i>	<i>Std. Dev.</i>
<i>Demographics</i>										
Age (in Years)	16.04	1.14	17.05	1.15	18.02	1.14	20.03	1.15	23.03	1.15
% Male	86.41		86.13		86.19		85.76		84.83	
% Philadelphia	51.7		51.58		49.88		49.71		48.77	
% Maricopa	48.3		48.42		50.12		50.29		51.23	
<i>Ethnicity</i>										
% White	20.24		20.84		20.88		20.91		21.6	
% Black	41.43		40.97		40.29		39.84		38.8	
% Hispanic	33.53		33.52		34.2		34.73		34.74	
% Other	4.80		4.68		4.63		4.53		4.85	
<i>Peer Group</i>										
Avg. Prop. of Peers Ever Arrested	0.454	0.377	0.505	0.387	0.478	0.398	0.478	0.412	0.451	0.406
Avg. Prop. of Peers Ever in Jail	0.617	0.426	0.387	0.381	0.392	0.393	0.402	0.41	0.386	0.400
Antisocial Behaviors (Likert)	2.32	0.926	1.83	0.835	1.72	0.799	1.71	0.782	1.60	0.696
<i>Signals</i>										
<i>% Participate in...</i>										
Job Training	-----	-----	1.85		1.26		0.89		0.52	
Substance Abuse Treatment	-----	-----	1.48		1.26		0.74		1.03	
Mental Health Treatment			6.20		4.28		3.47		2.58	
Anger Management Treatment	-----	-----	1.48		0.96		0.30		0.59	
<i>% Voluntary Participation in...</i>										
Job Training	-----	-----	0.52		1.18		0.66		0.52	
Substance Abuse Treatment	-----	-----	0.22		0.30		0.07		0.15	
Mental Health Treatment			1.33		2.07		2.07		1.55	
Anger Management Treatment	-----	-----	0.22		0.44		0.15		0.37	
<i>Avg. Proportion of Time Spent ....</i>										
At home	-----	-----	0.571	0.432	0.704	0.414	0.714	0.39	0.695	0.409
Secure settings	-----	-----	0.426	0.432	0.292	0.413	0.282	0.388	0.299	0.407
<i>Self-Report Offending</i>										
Avg. Count of All Crimes	-----	-----	34.29	164.44	51.92	255.67	58.21	216.29	38.51	153.88
Avg. Variety Score (All Crimes)	-----	-----	1.55	2.71	1.25	2.43	1.32	2.43	0.839	1.8
Avg. Count of Property Crimes	-----	-----	26.09	150.38	38.63	200.41	35.64	154.08	25.32	123
Avg. Variety Score (Property)	-----	-----	0.69	1.54	0.591	1.43	0.578	1.36	0.374	1.03
Avg. Count of Violent Crimes	-----	-----	2.13	8.9	1.57	7.91	2.57	17.92	1.34	13.55
Avg. Variety Score (Violent)	-----	-----	0.627	1.01	0.436	0.858	0.456	0.882	0.279	0.654

Table 2 – Interview Completion Status

Table 2 - Interview Completion Status - All Follow-up Interviews										
	6	12	18	24	30	36	48	60	72	84
% Complete	92.54	92.54	89.59	90.32	90.55	90.77	89.66	88.85	86.78	83.53
% Partial	0.89	0.66	1.18	0.59	0.59	0.22	0.07	0.3	0.3	0.22
% Incomplete	6.57	6.79	9.23	9.08	8.86	9.01	10.27	10.86	12.92	16.25

Table 3 – Number of Total Interviews Completed

<b>Table 3 - Number of Total Interviews Completed</b>		
<b># of Interviews</b>	<b>% Completed this #</b>	<b>% Completed At Least this #</b>
0	1.33	100
1	0.74	98.67
2	0.81	97.93
3	0.81	97.12
4	1.33	96.31
5	1.55	94.98
6	2.88	93.43
7	4.43	90.55
8	7.31	86.12
9	17.58	78.81
10	61.23	61.23

Table 4: Descriptive Statistics for All Treatment and Control Groups

Variable	1 Recall Period Pre-Participation (n=1)								Recall Period of Participation (n=0)								1 Recall Period Post-Participation							
	Control (n=426)		Only Vol. (n=135)		Only Invol. (n=142)		Mixed (n=66)		Control (n=520)		Only Vol. (n=158)		Only Invol. (n=199)		Mixed (n=73)		Control (n=585)		Only Vol. (n=142)		Only Invol. (n=190)		Mixed (n=63)	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
<b>Aspirations for Work, Family, &amp; Law (AWFL)</b>																								
Perceived Opportunities	4.452	.576	4.466	.563	4.525	.563	4.503	.452	4.453	.577	4.513	.575	4.505	.502	4.538	.482	4.453	.572	4.504	.608	4.495	.552	4.507	.583
Perceived Expectations	3.617	.882	3.584	.921	3.718	.887	3.664	.824	3.633	.884	3.642	.901	3.650	.819	3.778	.869	3.617	.886	3.753	.870	3.721	.837	3.736	.909
Future Outlook Inventory (FOI)	2.601	.580	2.553	.591	2.540	.591	2.512	.485	2.608	.592	2.630	.579	2.533	.516	2.477	.578	2.610	.574	2.631	.581	2.579	.488	2.605	.561
Motivation to Succeed (MS)	3.391	.624	3.384	.604	3.485	.594	3.594	.538	3.396	.621	3.394	.662	3.480	.592	3.593	.560	3.395	.621	3.458	.609	3.506	.625	3.667	.689
<b>Criminal Attitudes</b>																								
Social Rewards from Crime Factor Score	-.001	1.018	-.099	1.073	.051	.964	-.060	.924	.012	1.019	-.098	.997	.115	.816	-.053	.900	-.018	1.022	-.084	1.012	.042	.973	.057	.996
Personal Rewards from Crime	1.745	2.272	1.757	2.464	1.998	2.091	2.358	2.477	1.767	2.256	1.783	2.419	2.218	2.552	2.375	2.580	1.768	2.270	1.708	2.365	2.381	2.718	2.400	2.688
<b>Employment</b>																								
Employment Rate (% Months in Recall Empl.)	26.656	39.110	27.041	37.447	20.376	35.086	26.853	39.268	27.284	39.561	26.168	34.829	31.436	40.247	28.853	35.435	27.541	39.611	30.569	39.480	31.289	40.310	28.148	37.232
Logged Average Legal Wage Rate (By Recall)	2.679	5.964	2.215	3.367	3.727	21.455	2.240	4.992	2.793	5.933	2.700	4.361	2.628	4.139	2.285	2.803	2.885	6.616	3.322	4.554	3.434	6.224	3.104	4.134
Number of Interruptions in Legal Work	.507	.724	.578	.738	.479	.702	.379	.602	.500	.725	.658	.755	.508	.724	.658	.768	.504	.717	.592	.791	.547	.663	.460	.618
<b>Offending</b>																								
Offending Rate (Drugs Incl.)	1.314	13.008	2.454	18.466	.649	2.482	.195	.506	1.341	13.914	.456	1.493	.461	2.026	.208	.764	1.671	18.661	.458	1.884	1.889	15.232	.228	.816
Offending Rate (Drugs Excl.)	.458	3.479	1.968	17.901	.506	1.893	.080	.226	.394	2.599	.214	.898	.172	1.051	.163	.623	.486	3.218	.196	.947	.256	.749	.062	.147
Property Variety Score	.640	1.416	.511	1.105	.831	1.856	.864	1.607	.629	1.410	.747	1.659	.874	1.800	.849	1.934	.598	1.337	.754	1.590	.763	1.627	.937	1.908
Violent Variety Score	.535	.953	.526	.929	.662	.944	.500	.789	.532	.957	.563	1.137	.658	.966	.603	1.064	.535	.961	.465	.928	.658	1.020	.698	1.131
Age at First Offense	10.466	1.832	10.370	1.819	10.270	1.651	10.530	1.561	10.472	1.835	10.222	1.758	10.249	1.646	10.603	1.579	10.478	1.846	10.232	1.765	10.277	1.670	10.587	1.572
<b>Peer Influences</b>																								
Member of Gang?	.098	.298	.096	.296	.077	.268	.061	.240	.108	.310	.076	.266	.101	.301	.041	.200	.106	.308	.070	.257	.095	.294	.016	.126
Contact with Gang	4.056	2.575	4.154	2.609	4.091	2.663	5.250	1.708	4.201	2.581	4.333	2.498	4.737	2.232	4.000	4.243	4.250	2.598	4.700	2.710	4.944	2.287	.016	.126
Resistance to Peer Infl. (RPI)	3.234	.575	3.243	.530	3.212	.576	3.164	.613	3.245	.572	3.239	.564	3.164	.528	3.069	.605	3.246	.567	3.237	.562	3.171	.548	3.173	.590
% of Friends Ever Arrested	.437	.411	.356	.380	.450	.395	.392	.398	.424	.409	.349	.390	.380	.399	.413	.418	.427	.408	.383	.336	.402	.390	.436	.414
% of Friends Ever Jailed	.349	.399	.273	.355	.345	.382	.269	.337	.339	.395	.280	.366	.287	.352	.290	.352	.344	.395	.295	.360	.312	.360	.368	.388
<b>Psychological Scales</b>																								
Brief Symptom Inventory (BSI) (Factor Score)	-.084	.729	.270	1.241	.012	.769	.181	.744	-.079	.743	.425	1.435	-.073	.717	.330	1.157	-.077	.736	.442	1.438	-.018	.882	.107	.939
Psychosocial Maturity (PSM)	3.187	.462	3.143	.510	3.110	.536	3.080	.439	3.184	.474	3.138	.455	3.108	.459	3.189	.452	3.186	.470	3.176	.469	3.150	.456	3.227	.423
Psychopathic Traits (PTI) (Factor Score)	.050	1.016	-.004	1.018	.159	1.026	.106	1.021	.043	1.017	-.041	1.000	.152	.929	.054	.940	.017	1.018	.106	.969	.063	.961	.084	.848
<b>Social Support</b>																								
Number of Caring Adults (Logged)	2.249	1.077	2.378	.993	2.579	1.056	2.528	.898	2.203	1.066	2.444	1.032	2.529	.975	2.641	.949	2.184	1.077	2.304	1.014	2.412	.894	2.537	.891
Diversity of Caring Adults	1.886	1.254	2.037	1.312	2.338	1.347	2.409	1.436	1.851	1.258	1.937	1.092	2.206	1.284	2.370	1.339	1.828	1.240	1.873	1.148	2.095	1.231	2.079	1.209
Domains of Social Support	5.514	2.672	5.756	2.535	6.204	2.249	6.273	2.195	5.482	2.685	5.911	2.419	6.085	2.236	6.575	2.147	5.431	2.706	5.739	2.638	5.979	2.321	6.302	2.276
<b>Substance Abuse</b>																								
Frequency of Abuse	8.286	6.155	6.841	6.566	8.651	7.593	7.208	4.344	8.347	5.916	9.240	6.700	9.522	7.598	7.800	3.969	8.289	6.041	9.373	7.485	8.420	7.270	7.429	5.210
Variety of Substances Used	.684	1.072	.563	1.055	.507	1.009	.758	1.096	.586	1.053	.797	1.235	.678	1.351	.795	1.130	.571	1.025	.859	1.509	.732	1.220	.746	1.031
<b>Demographics</b>																								
Age	18.463	1.966	18.578	2.086	17.669	2.048	17.818	1.831	18.722	2.229	18.949	2.359	17.724	2.153	18.260	2.048	18.696	2.233	19.261	2.296	18.174	2.077	18.825	1.939
Race																								
White	.171	.376	.281	.451	.275	.448	.439	.500	.168	.374	.297	.459	.251	.435	.438	.500	.168	.374	.310	.464	.247	.433	.444	.501
Black	.429	.495	.385	.488	.352	.479	.258	.441	.427	.495	.380	.487	.362	.482	.247	.434	.426	.495	.380	.487	.358	.481	.254	.439
Hispanic	.348	.477	.289	.455	.352	.479	.273	.449	.354	.479	.272	.446	.347	.477	.274	.449	.355	.479	.268	.444	.353	.479	.270	.447
Other	.052	.220	.044	.207	.021	.144	.030	.173	.051	.220	.051	.220	.040	.197	.041	.200	.051	.219	.042	.202	.042	.201	.032	.177
Male	.887	.317	.711	.455	.887	.317	.591	.495	.890	.313	.728	.446	.899	.301	.616	.490	.888	.316	.718	.451	.900	.301	.619	.490
Size (Phoenix=1)	.531	.499	.541	.500	.507	.502	.339	.477	.527	.500	.519	.501	.437	.497	.339	.477	.525	.500	.514	.502	.442	.498	.328	.473
Parent Socioeconomic Status	51.998	12.380	49.265	12.511	51.628	11.325	48.250	10.588	52.152	12.346	49.564	12.597	51.144	11.601	47.781	10.595	52.142	12.295	49.319	12.739	50.979	11.576	48.008	11.009
<b>Interview/Recall Information</b>																								
Number of Months in Recall	7.398	2.703	7.393	2.713	7.014	2.458	7.076	2.846	7.652	2.851	7.867	3.012	7.065	2.379	7.904	2.859	7.614	2.851	8.007	3.002	6.932	2.720	8.175	3.326
Interviewed in placement?	.371	.484	.200	.401	.387	.489	.167	.376	.368	.483	.146	.354	.181	.386	.137	.346	.368	.482	.148	.356	.263	.442	.175	.383
Percent Time in Community	61.427	43.324	78.983	36.463	49.612	43.038	75.292	35.204	62.082	43.149	81.752	30.106	72.931	31.979	85.040	27.184	62.221	43.191	83.028	32.922	75.419	36.953	79.930	34.428

Table 5: Random-Intercept Models Predicting Changes in Attitudes

Variable	Change in AWFL - Opportunities Scores				Change in AWFL - Expectations Scores				Change in FOI Scores				Change in MIs Scores			
	Coef.	St. Err.	MEF	St. Err.	Coef.	St. Err.	MEF	St. Err.	Coef.	St. Err.	MEF	St. Err.	Coef.	St. Err.	MEF	St. Err.
<b>Treatment Groups &amp; Time Interactions</b>																
Treatment=1 (Post Participation)	.0011	.0206	.0061	.0089	-.0098	.0306	.0054	.0133	-.0020	.0213	.0279**	.0092	-.0049	.0225	.0205*	.0098
Group																
Only Voluntary	-.0076	.0341	.0127	.0173	.0077	.0508	.0112	.0257	.0126	.0353	.0283	.0179	-.0209	.0374	.0152	.0189
Only Involuntary	-.0253	.0406	-.0024	.0161	.0134	.0604	.0150	.0239	-.0137	.0420	.0265	.0166	-.0057	.0445	.0240	.0176
Mixed	-.0094	.0658	.0065	.0297	.0417	.0977	.0237	.0442	.0045	.0680	.0435	.0307	-.0229	.0721	.0139	.0327
Group X Treatment																
Only Voluntary, Treated	.0104	.0385	.0168	.0224	-.0006	.0573	.0075	.0333	-.0225	.0398	.0196	.0232	-.0064	.0421	.0112	.0245
Only Involuntary, Treated	-.0054	.0412	-.0039	.0180	-.0147	.0612	.0063	.0268	.0083	.0426	.0287	.0187	-.0090	.0451	.0191	.0197
Mixed, Treated	.0089	.0551	.0100	.0364	-.0148	.0818	.0149	.0541	.0273	.0569	.0525	.0376	.0052	.0603	.0140	.0400
Participation Category X Treatment																
Control	-.0217	.0189	-.0052	.0134	-.0073	.0282	.0038	.0200	.0039	.0195	.0302*	.0139	-.0208	.0207	.0171	.0147
Only Voluntary	-.0057	.0339	.0100	.0261	-.0144	.0505	.0041	.0389	.0078	.0351	.0321	.0270	.0047	.0371	.0175	.0285
Only Involuntary	.0195	.0314	.0072	.0220	-.0002	.0468	.0149	.0328	.0167	.0325	.0347	.0228	-.0046	.0344	.0218	.0241
Mixed	-.0126	.0598	.0003	.0306	-.0385	.0889	.0047	.0454	-.0096	.0619	.0389	.0316	-.0089	.0657	.0095	.0334
<b>Criminal Attitudes</b>																
Social Rewards from Crime Factor Score	-.0025	.0082			-.0091	.0123			-.0029	.0085			-.0318***	.0090		
<b>Illegal Employment</b>																
Rate of Illegal Work	-.0018**	.0006			-.0020*	.0008			-.0019**	.0006			-.0007	.0006		
Variety of Illegal Work	.0799**	.0273			.0819*	.0405			.0433	.0282			.0137	.0300		
<b>Peer Influences</b>																
Resistance to Peer Infl. (RPI)	.0298*	.0148			.0644**	.0220			.0428**	.0153			.0070	.0162		
<b>Psychological Scales</b>																
BSI Factor	-.0005	.0086			-.0210	.0128			.0206*	.0089			-.0049	.0094		
Moral Disengagement	-.0016	.0020			-.0001	.0029			.0055**	.0020			-.0005	.0022		
Psychosocial Maturity (PSMI)	.0621**	.0229			.2071***	.0341			.0922***	.0237			.1209***	.0251		
PSMI - Cluster Average	-.0984**	.0324			-.2913***	.0483			-.1181***	.0335			-.2057***	.0355		
<b>Interview/Recall Information</b>																
Interviewed in placement?	.0736**	.0228			.0737*	.0339			.0192	.0235			.0249	.0249		
<b>Missing Value Indicators</b>																
BSI Missing?	.0072	.0166			.0665**	.0247			.0236	.0172			.0248	.0181		
<b>Demographics</b>																
Survey Site (1=Phoenix)	-.0341	.0199			-.0264	.0295			-.0490*	.0205			.0124	.0217		
<b>Auxiliary Model Statistics</b>																
Intercept		2.221				4.816				6.339				7.829		
N*T		8059				8025				8072				8054		
N		1023				1023				1023				1023		
Intraclass Correlation		3545				4105				3649				3608		
Reliability Estimate		.896				.896				.896				.896		
AIC		6548.801				10698.090				6629.449				7077.154		
BIC		6947.490				11096.530				7028.230				7475.808		
Hausman Specification Test		18.51 (p>.05)				42.68 (p>.05)				30.94 (p>.05)				57.36 (p>.05)		

Table 6: Predicting Offending Rates (Incl. Drugs) Over Employment Tertiles

Random-Intercept Models Predicting Offending Rates (Drugs Incl.) By Tertile of Post-Program Employment Rate												
Variable	Lower Tertile of Emp Rate				Middle Tertile of Emp Rate				Upper Tertile of Emp Rate			
	Coef.	St. Error	Marg. Eff.	St. Error	Coef.	St. Error	Marg. Eff.	St. Error	Coef.	St. Error	Marg. Eff.	St. Error
<b>Treatment Groups &amp; Time Interactions</b>												
<i>Treatment=1 (Post Participation)</i>	.460	1.482	.215	.671	-.130	.770	-.030	.245	.249	.331	-.030	.138
<i>Group</i>												
Only Voluntary	.746	2.354	.228	1.231	-1.044	1.265	-.325	.551	.171	.585	.052	.277
Only Involuntary	.979	2.889	-.064	1.328	-.636	1.334	.240	.459	.775	.698	-.090	.256
Mixed	2.308	14.093	1.059	6.934	-.908	2.441	-.363	1.029	.139	.976	-.038	.412
<i>Group X Treatment</i>												
Only Voluntary, Treated	-.199	2.590	.333	1.710	.997	1.341	-.136	.645	.182	.627	.192	.339
Only Involuntary, Treated	-.459	3.015	-.064	1.475	.613	1.355	.346	.506	-.289	.673	-.102	.278
Mixed, Treated	-.323	3.791	1.114	7.155	.342	2.191	-.317	1.148	-.334	.847	-.065	.501
<i>Participation Category X Treatment</i>												
Control	1.123	1.286	.531	.899	-.397	.578	-.313	.423	-.124	.296	-.199	.210
Only Voluntary	.384	2.410	.419	1.985	-.30	1.071	-.481	.821	-.315	.555	-.095	.407
Only Involuntary	-.349	2.723	-.238	2.175	.650	.933	.577	.682	-1.113*	.504	-.608	.321
Mixed	-.920	14.086	.602	2.118	.401	2.007	-.156	1.127	.232	.853	.070	.464
<b>Gang Involvement</b>												
<i>Gang Member? (Cluster Avg.)</i>	13.383	7.575			-.475	3.946			6.153**	2.274		
<i>Contact Frequency w/Gang Members</i>	1.727*	.846			-.993**	.381			-.351	.262		
<i>Contact Frequency (Cluster Avg.)</i>	-2.884	1.491			.759	.769			-1.164*	.488		
<b>Illegal Employment</b>												
<i>Variety of Illegal Work</i>	4.266**	1.601			1.564*	.757			1.370**	.504		
<i>Variety of Illegal Work (Cluster Avg.)</i>	-3.922	4.854			-.850	1.823			-10.584***	1.259		
<i>Rate of Illegal Work (Clust. Avg.)</i>	-.008	.095			-.027	.049			.157***	.026		
<b>Peer Influences</b>												
<i>% of Peers Ever Arrested</i>	.545	2.760			-.280	1.050			-1.460**	.535		
<i>% of Peers Ever Jailed</i>	-.783	2.787			-.056	1.066			1.662**	.575		
<b>Substance Abuse</b>												
<i>Frequency of Abuse (Cluster Avg.)</i>	.079	.239			.024	.103			.115*	.052		
<b>Auxiliary Statistics</b>												
<i>N*T</i>		1535				2375				3072		
<i>N</i>		223				323				401		
<i>Intraclass Correlation</i>		.236				.176				.346		
<i>Reliability Estimate</i>		.884				.893				.896		
<i>AIC</i>		14607.06				15942.68				13459.41		
<i>BIC</i>		14948.58				15862.13				13845.33		
<i>Hausman Specification Test</i>		9.91 (p>.05)				23.64 (p>.05)				50.51 (p>.05)		



Table 7: Predicting Offending Rates (Excl. Drugs) Over Employment Tertiles

<b>Random-Intercept Models Predicting Offending Rates (Drugs Excl.) By Tertile of Post-Program Employment Rate</b>												
<b>Variable</b>	<b>Lower Tertile of Emp Rate</b>				<b>Middle Tertile of Emp Rate</b>				<b>Upper Tertile of Emp Rate</b>			
	<i>Coef.</i>	<i>St. Error</i>	<i>Marg. Eff.</i>	<i>St. Error</i>	<i>Coef.</i>	<i>St. Error</i>	<i>Marg. Eff.</i>	<i>St. Error</i>	<i>Coef.</i>	<i>St. Error</i>	<i>Marg. Eff.</i>	<i>St. Error</i>
<b>Treatment Groups &amp; Time Interactions</b>												
<i>Treatment=1 (Post Participation)</i>	.067	.706	.097	.312	.092	.275	.018	.086	.064	.059	.000	.024
<i>Group</i>												
Only Voluntary	-.052	1.089	-.004	.561	-.648	.447	-.174	.191	.006	.105	.004	.049
Only Involuntary	-.037	1.389	.027	.620	-.197	.484	-.002	.161	-.096	.124	-.076	.045
Mixed	1.978	10.669	1.104	5.318	-.327	.847	-.120	.357	.048	.173	-.017	.073
<i>Group X Treatment</i>												
Only Voluntary, Treated	.202	1.198	.102	.783	.669	.470	-.013	.223	.043	.112	.038	.060
Only Involuntary, Treated	.007	1.435	.056	.675	.083	.488	.035	.175	.104	.120	-.022	.049
Mixed, Treated	.152	1.746	1.190	5.445	.116	.767	-.076	.400	-.088	.151	-.025	.088
<i>Participation Category X Treatment</i>												
Control	.386	.604	.244	.422	-.137	.204	-.066	.150	-.015	.053	-.028	.038
Only Voluntary	.133	1.101	.062	.906	-.260	.372	-.310	.286	-.033	.098	-.012	.072
Only Involuntary	.398	1.256	.223	.996	.124	.328	.063	.238	-.070	.089	-.109	.057
Mixed	-1.628	10.719	.301	.969	.095	.697	-.070	.401	.015	.151	-.010	.082
<b>Gang Involvement</b>												
<i>Current Gang Member?</i>	-5.634*	2.179			.648	.789			-.315	.259		
<i>Contact Frequency w/Gang Members</i>	1.262**	.390			.048	.132			.019	.048		
<b>Illegal Employment</b>												
<i>Rate of Illegal Work</i>	-.030	.017			-.003	.006			-.001	.002		
<i>Rate of Illegal Work (Cluster Avg.)</i>	.014	.046			.009	.017			.016**	.005		
<i>Variety of Illegal Work</i>	2.763***	.723			.356	.263			.240**	.089		
<i>Variety of Illegal Work (Cluster Avg.)</i>	-2.081	2.290			-.498	.638			-1.173***	.224		
<b>Psychological Inventories</b>												
<i>Moral Disengagement</i>	.029	.056			-.050*	.021			.004	.006		
<i>WAI Factor</i>	-.980*	.393			.067	.129			.021	.037		
<b>Social Support</b>												
<i>Number of Caring Adults (Logged)</i>	-1.453**	.491			-.102	.186			.014	.042		
<i>Domains of Social Support</i>	.401*	.189			-.006	.065			-.003	.016		
<b>Auxiliary Statistics</b>												
<i>N*T</i>		1331				2179				2951		
<i>N</i>		219				322				399		
<i>Intraclass Correlation</i>		-----				.3480				.4034		
<i>Reliability Estimate</i>		-----				.8855				.8906		
<i>AIC</i>		7888.611				9219.112				8873.285		
<i>BIC</i>		8221.006				9583.056				9256.638		
<i>Hausman Specification Test</i>		11.88 (p>.05)				29.17 (p>.05)				9.50 (p>.05)		

Table 8: Predicting Property Offending Variety Over Employment Tertiles

Random-Intercept Models Predicting Property Offending Variety Scores By Tertile of Post-Program Employment Rate												
Variable	Lower Tertile of Emp Rate				Middle Tertile of Emp Rate				Upper Tertile of Emp Rate			
	Coef.	St. Error	Marg. Eff.	St. Error	Coef.	St. Error	Marg. Eff.	St. Error	Coef.	St. Error	Marg. Eff.	St. Error
<b>Treatment Groups &amp; Time Interactions</b>												
Treatment=1 (Post Participation)	-.001	.148	-.039	.068	.007	.125	-.041	.041	-.009	.067	-.059*	.028
<b>Group</b>												
Only Voluntary	.083	.241	.046	.125	.105	.209	-.008	.091	-.089	.117	-.067	.056
Only Involuntary	.317	.294	-.006	.136	-.068	.213	-.061	.075	-.156	.139	-.106*	.051
Mixed	.291	1.468	.123	.725	-.474	.406	-.188	.172	-.055	.192	-.082	.082
<b>Group X Treatment</b>												
Only Voluntary, Treated	-.062	.264	.021	.173	-.041	.222	-.016	.108	.185	.127	-.009	.069
Only Involuntary, Treated	-.465	.308	-.191	.150	.062	.217	-.045	.084	.113	.135	-.071	.056
Mixed, Treated	-.046	.385	.104	.746	.266	.362	-.126	.193	.069	.169	-.062	.101
<b>Participation Category X Treatment</b>												
Control	-.012	.130	-.024	.091	.026	.095	-.035	.069	-.057	.060	-.097*	.042
Only Voluntary	.025	.246	.059	.201	-.042	.178	-.030	.137	-.116	.113	-.121	.083
Only Involuntary	-.060	.275	-.036	.222	.041	.154	-.040	.113	.026	.101	-.094	.065
Mixed	-.256	1.468	-.005	.215	.291	.337	-.037	.187	-.062	.169	-.111	.094
<b>Employment</b>												
Number of Interruptions in Legal Work	.084	.165			.257*	.116			.112*	.055		
<b>Illegal Employment</b>												
Rate of Illegal Work	.012**	.004			.000	.003			-.007**	.002		
Rate of Illegal Work (Cluster Avg.)	-.014	.009			-.001	.008			.016**	.005		
Variety of Illegal Work	.657***	.164			1.377***	.123			1.411***	.100		
Variety of Illegal Work (Cluster Avg.)	-.510	.472			-1.533***	.283			-1.975***	.243		
<b>Psychological Inventories</b>												
Moral Disengagement	.032*	.013			.009	.010			-.015*	.007		
<b>Substance Abuse</b>												
Frequency of Abuse	.044*	.019			.060***	.014			.033***	.009		
Frequency of Abuse (Cluster Avg.)	-.074**	.024			-.067***	.017			-.045***	.010		
<b>Auxiliary Statistics</b>												
N*T		1709				2567				3207		
N		225				324				401		
Intraclass Correlation		.0725				.0946				.1592		
Reliability Estimate		.8916				.8987				.8996		
AIC		4626.381				7288.018				7568.927		
BIC		4974.776				7662.449				7957.604		
Hausman Specification Test		13.03 (p>.05)				10.29 (p>.05)				17.59 (p>.05)		

Table 9: Predicting Violent Offending Variety Over Employment Tertiles

Random-Intercept Models Predicting Violent Offending Variety Scores By Tertile of Post-Program Employment Rate												
Variable	Lower Tertile of Emp Rate				Middle Tertile of Emp Rate				Upper Tertile of Emp Rate			
	Coef.	St. Error	Marg. Eff.	St. Error	Coef.	St. Error	Marg. Eff.	St. Error	Coef.	St. Error	Marg. Eff.	St. Error
<b>Treatment Groups &amp; Time Interactions</b>												
<i>Treatment=1 (Post Participation)</i>	.091	.107	-.060	.049	.092	.094	-.025	.030	.024	.047	-.044*	.019
<i>Group</i>												
Only Voluntary	.241	.173	-.052	.090	.116	.156	-.001	.068	-.033	.081	-.067	.039
Only Involuntary	.108	.211	-.032	.098	.012	.159	-.025	.056	-.024	.096	-.076*	.036
Mixed	.161	1.057	-.146	.522	.044	.304	-.056	.129	-.004	.133	-.022	.057
<i>Group X Treatment</i>												
Only Voluntary, Treated	-.281	.190	-.127	.125	-.046	.166	.010	.080	.015	.088	-.054	.048
Only Involuntary, Treated	-.158	.222	-.059	.108	-.039	.162	-.013	.063	-.009	.093	-.071	.039
Mixed, Treated	-.415	.277	-.275	.537	-.044	.270	-.045	.144	.040	.118	-.001	.070
<i>Participation Category X Treatment</i>												
Control	.023	.093	-.034	.066	-.032	.071	-.077	.052	-.018	.042	-.054	.029
Only Voluntary	-.130	.177	-.117	.144	-.075	.133	-.040	.103	-.018	.078	-.075	.058
Only Involuntary	.024	.198	-.020	.160	.077	.115	.015	.084	-.021	.070	-.086	.045
Mixed	.001	1.057	-.145	.154	-.045	.252	-.079	.139	-.018	.118	-.031	.065
<b>Employment</b>												
<i>Number of Interruptions in Legal Work</i>	-.012	.119			.176*	.086			.053	.038		
<b>Gang Involvement</b>												
<i>Current Gang Member?</i>	.096	.061			-.798**	.265			-.227	.194		
<i>Contact Frequency with Gang</i>	.096	.061			.154**	.044			.040	.036		
<i>Contact Frequency (Cluster Avg.)</i>	-.125	.109			-.191*	.089			-.005	.068		
<b>Illegal Employment</b>												
<i>Rate of Illegal Work</i>	.003	.003			-.002	.002			-.006***	.001		
<i>Rate of Illegal Work (Cluster Avg.)</i>	-.007	.007			.001	.006			.008*	.004		
<i>Variety of Illegal Work</i>	.299*	.118			.532***	.092			.535***	.070		
<i>Variety of Illegal Work (Cluster Avg.)</i>	-.238	.340			-.586**	.212			-.715***	.169		
<b>Interview/Recall Information</b>												
<i>Number of Months in Recall</i>	.041*	.017			.007	.013			.013	.008		
<b>Psychological Inventories</b>												
<i>PSMI</i>	.213*	.103			.132	.086			.049	.056		
<i>WAI Factor</i>	-.043	.062			-.071	.046			-.108***	.029		
<i>WAI Factor (Cluster Avg.)</i>	.088	.089			.103	.064			.162***	.038		
<b>Substance Abuse</b>												
<i>Frequency of Abuse</i>	.038**	.014			.031**	.010			.015*	.006		
<i>Frequency of Abuse (Cluster Avg.)</i>	-.032	.017			-.032*	.013			-.012	.007		
<b>Missing Value Indicators</b>												
<i>BSI Missing?</i>	-.015	.097			-.004	.072			-.116**	.043		
<i>BSI Missing (Cluster Avg.)</i>	.046	.201			.142	.149			.191*	.080		
<b>Auxiliary Statistics</b>												
<i>N*T</i>		1709				2567				3207		
<i>N</i>		225				324				401		
<i>Intraclass Correlation</i>		.1889				.1300				.1762		
<i>Reliability Estimate</i>		.8916				.8987				.8996		
<i>AIC</i>		3883.205				6389.656				5842.063		
<i>BIC</i>		4231.6				6764.088				6230.741		
<i>Hausman Specification Test</i>		7.65 (p>.05)				9.82 (p>.05)				8.87 (p>.05)		

**DISSERTATION FIGURES**

Figure 1: Age-Graded Informal Social Control Perspective

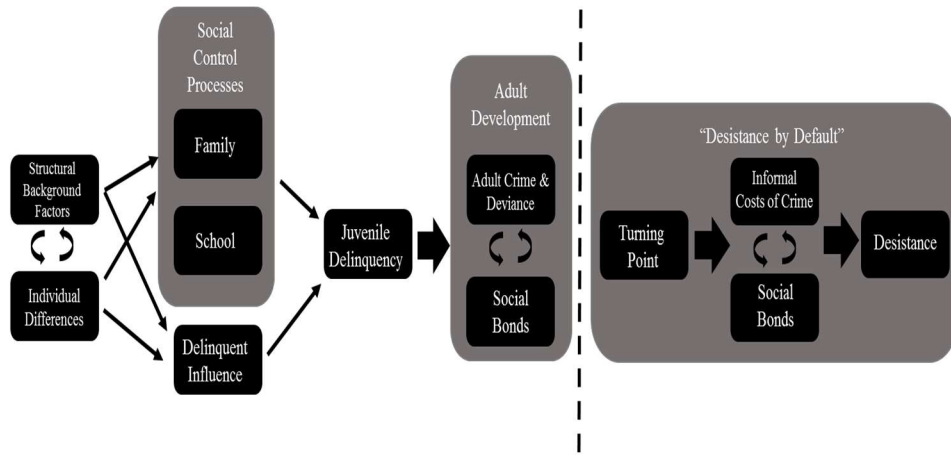


Figure 2: Cognitive Transformation Perspective

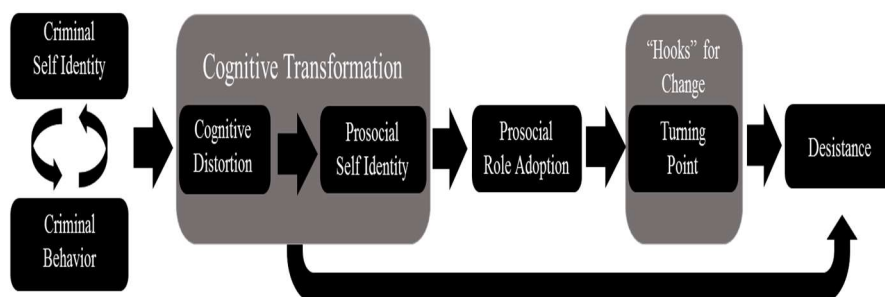


Figure 3: Plot of AWFL - Opportunity Scores Across Recall Periods Pre/Post Participation

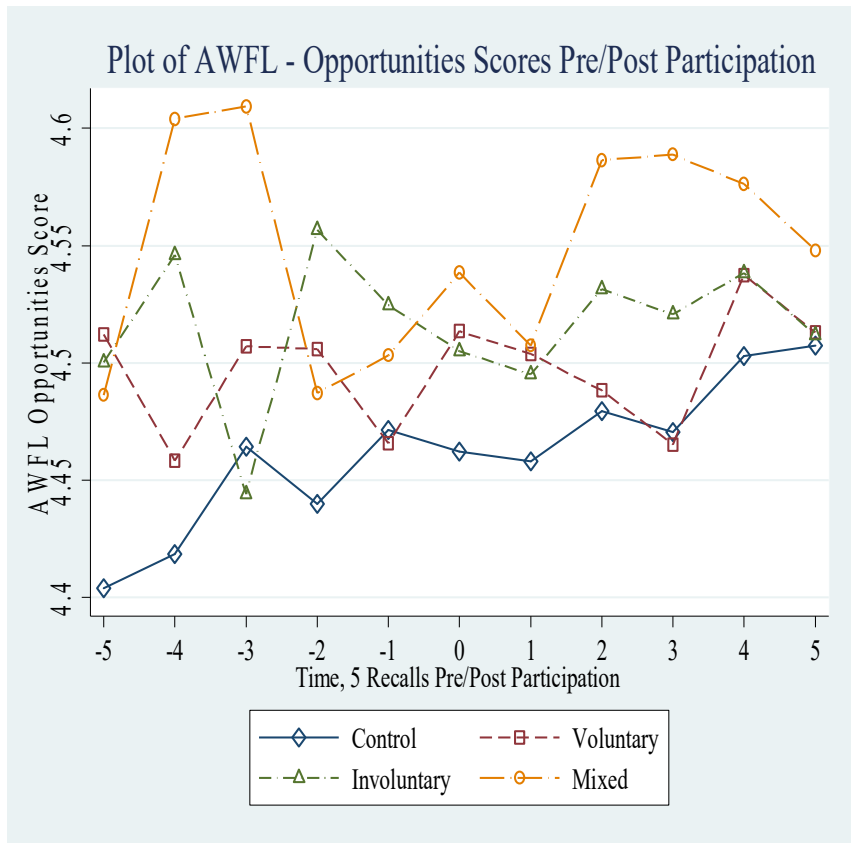


Figure 4: Plot of AWFL – Expectations Scores Across Recall Periods Pre/Post Participation

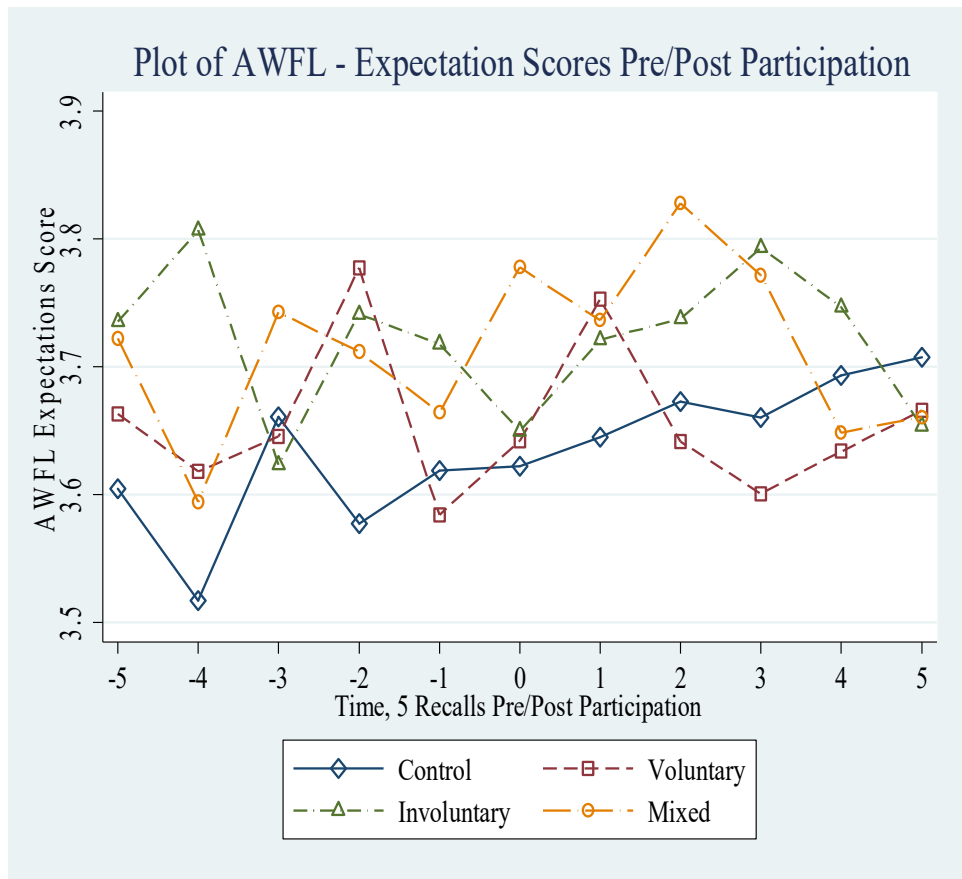


Figure 5: Plot of FOI Scores Across Recall Periods Pre/Post Participation

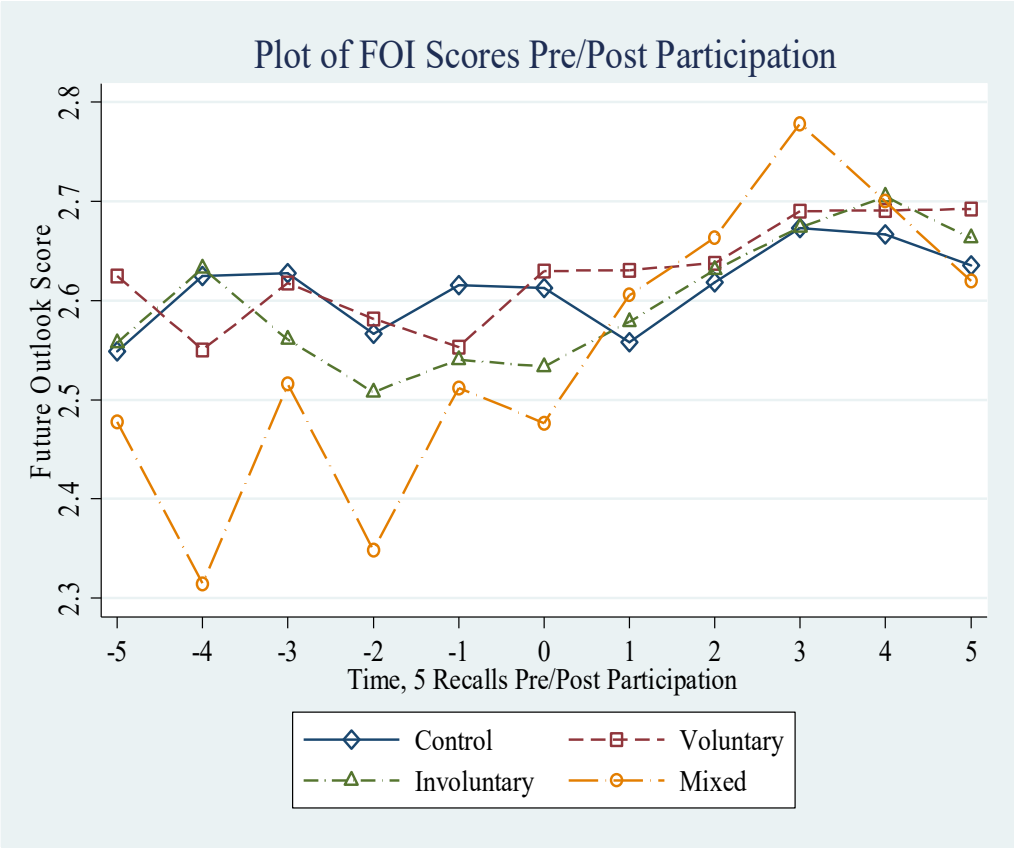




Figure 6: Plot of MtS Scores Across Recall Periods Pre/Post Participation

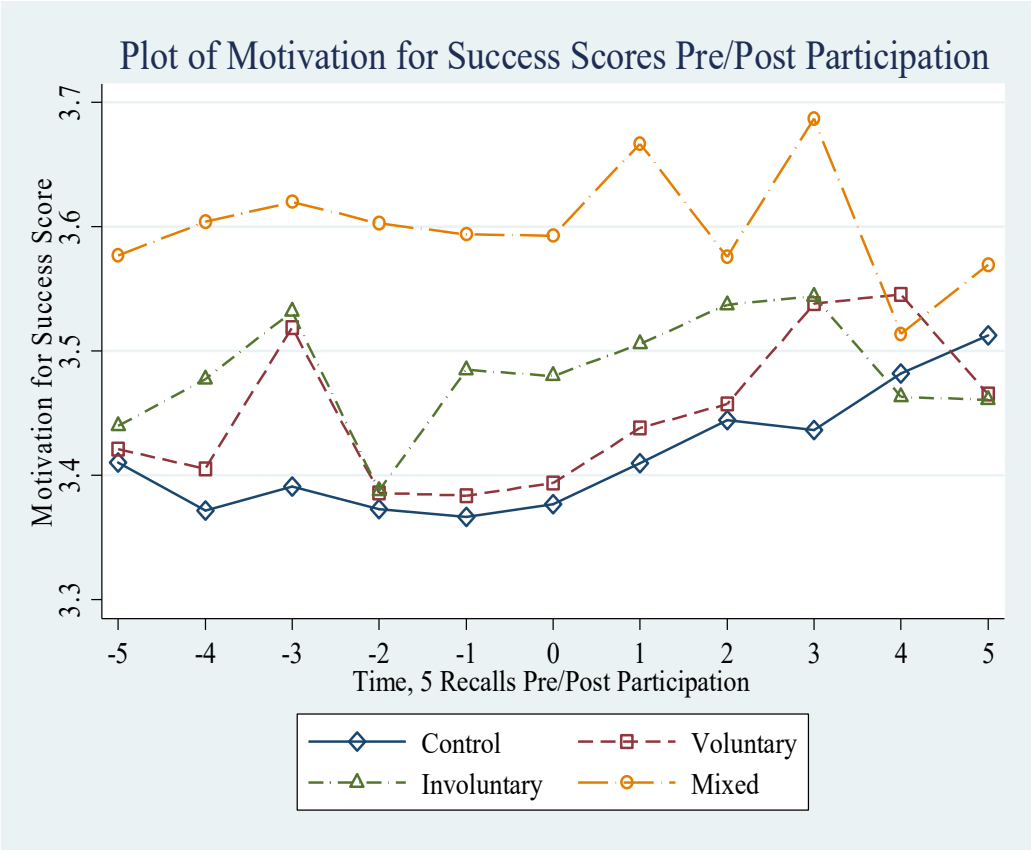


Figure 7: Plot of Offending Rates (Incl. Drugs) Across Recall Periods Pre/Post Participation

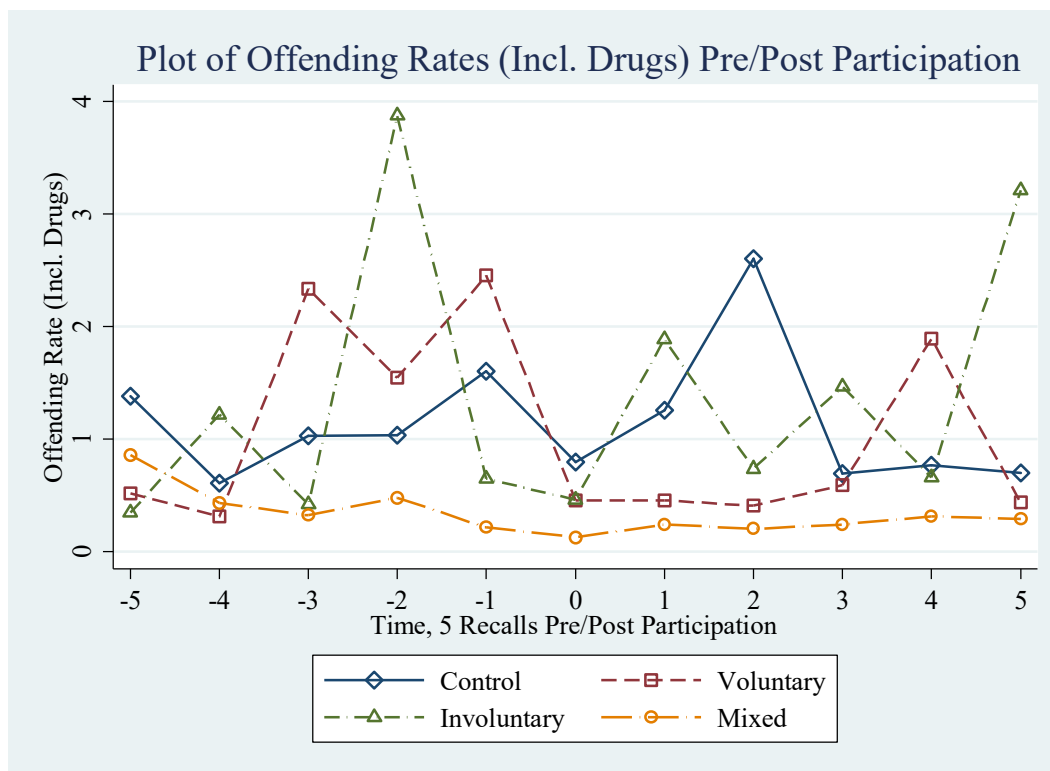


Figure 8: Plot of Offending Rates (Excl. Drugs) Across Recall Periods Pre/Post Participation

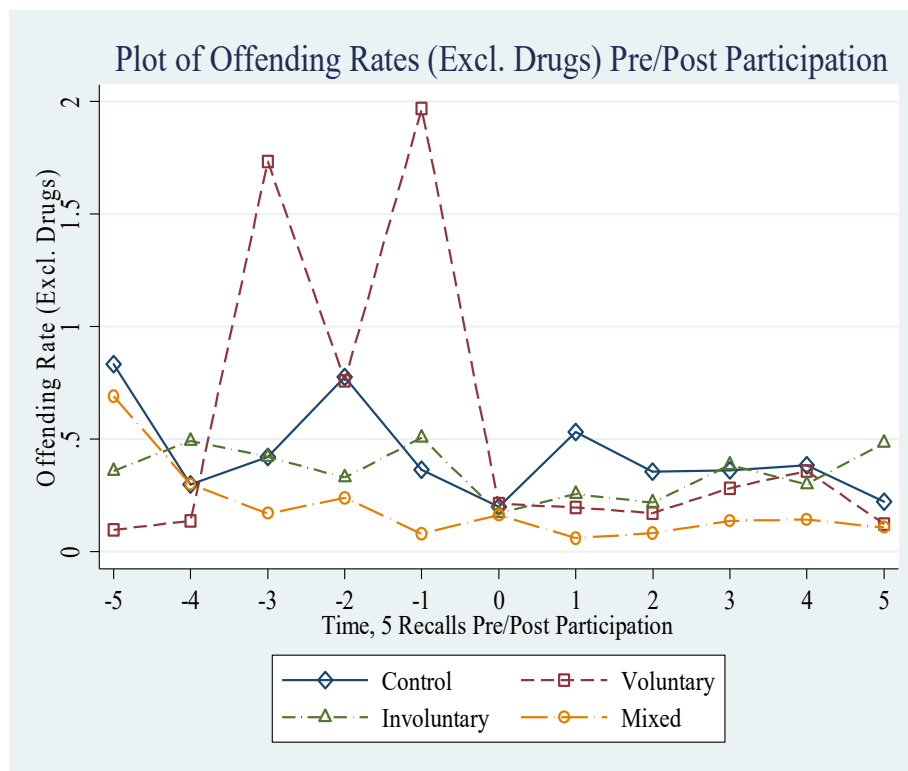


Figure 9: Plot of Property Offending Variety Scores Across Recall Periods Pre/Post Participation

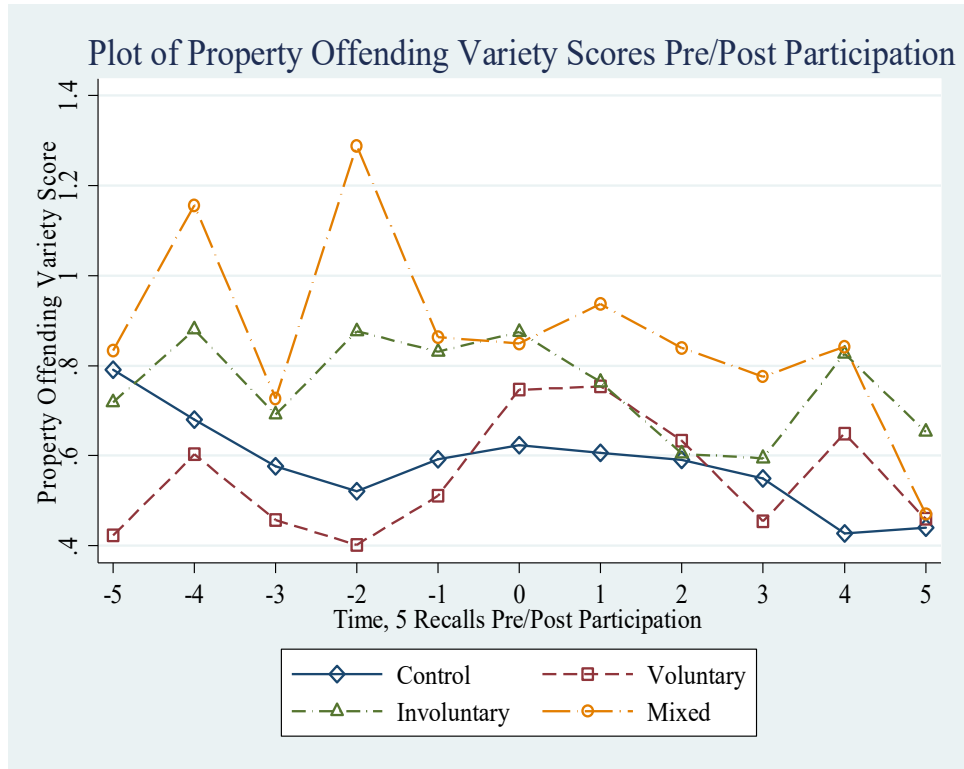
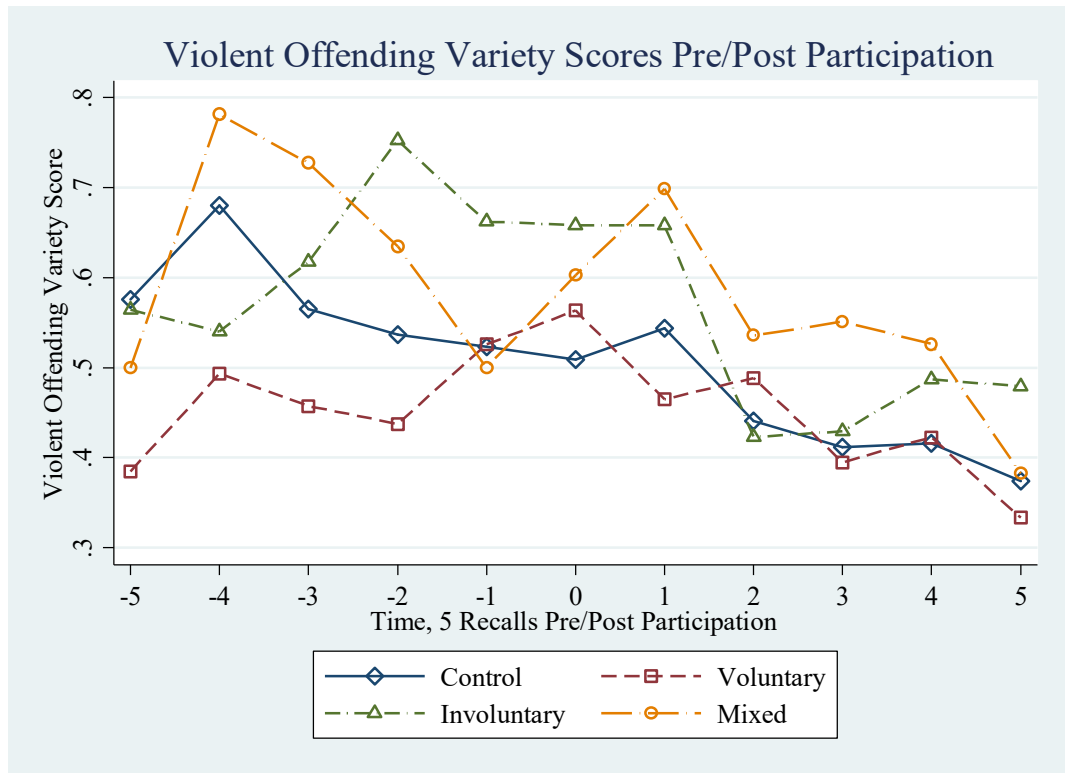


Figure 10: Plot of Violent Offending Variety Scores Across Recall Periods Pre/Post Participation



## **APPENDIX A: CONSTRUCTION OF SIMULATED CONTROL GROUPS**

### **I. DESCRIPTION OF RANDOMIZATION PROCEDURE AND SENSITIVITY TESTS**

As noted in the main text, the comparison of types of program participation – only voluntary (VOL), only involuntary (INVOL), and mixed (MIX) – lacked a relevant control group for comparison – namely, a group of individuals who do not participate in any program, voluntarily or involuntarily. In light of this, I chose to randomly select a control (CTRL) group from the PtD sample that did not report any type of program participation during any recall period to use as this comparison. A potential issue with a simple random selection (i.e., randomly selecting participants at some rate – 50%, 65%, etc...) design is that it would not account for timing – that is, randomly selected individuals could also be randomly selected for the recall periods which would be used for comparison. Due to this, a sampling design had to be implemented that both A) selected program non-participants at random and, B) randomly selected the recall periods for these non-participants to be used for comparison.

In order to accomplish this, I implemented a series of random selection criteria – first, all person-recalls were assigned a random value between 1 and 10000. I then condition that only those recalls assigned a random number divisible by 9 without a remainder are assigned a value of 1, and are thus eligible for selection in the next step. This number is chosen because it results in a .10 probability that a randomly selected recall period will be assigned a value of one and, combined with the number of trials (10 recall periods) results in the expected probability that any person will be selected of

approximately .65.<sup>30</sup> The first of these 1s is then taken as the recall period to be used as  $t=0$ , or the recall period in which program participation begins for actual participants, and I then assign time values around this recall (-5 for the fifth recall period pre-program, -4 for the fourth recall period pre-program, and so on).

A potential concern with this procedure is that any set of randomly-selected non-participants, and the time periods within them, might be systematically different from another set of randomly-selected non-participants (and time periods). In order to account for this, the procedure outlined above is repeated ten times, resulting in ten random samples of non-participants and the relevant time periods of within them. I then estimated a fixed-effect OLS model, one for each of these samples, using the following equation:

$$(Y_{it} - \bar{Y}_i) = \alpha_{it} + \beta(X_{it} - \bar{X}_i) + e_{it}$$

Where both  $Y$  and  $X$  have been within-transformed for each person, such that all independent and dependent variables are measured as departures from person-specific averages across all waves. This model is estimated for the full analysis sample ten times, each estimation including one of the randomly selected control groups. Results are then compiled using the “suest” (seemingly unrelated estimation) command in STATA to

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<sup>30</sup> This value is obtained using the equation to calculate probabilities from a series of Bernoulli trials -  $P(k) = \binom{n}{k} p^k q^{n-k}$  where  $p$  is the probability of success (.10),  $k$  is the number of successes (here, at least one),  $n$  is the number of trials (10), and  $q$  is the probability of failure (.90). Summing the probabilities of one or more successes leads to a total probability of approximately .65, meaning that, in any randomization applied as above, I would expect that 65% of non-participants would be selected as controls.

compare coefficient estimates across models using Wald tests. The results from the randomization procedure and coefficient comparisons are presented below.

## II. RANDOMIZATION RESULTS

Of primary concern is that the ten randomizations produce groups that are not so systematically different from one another that model estimates are influenced by the choice of randomly selected control group. First, then, it was necessary to assess whether these randomizations produced groups different in respect to the number of persons sampled or the timing of recalls – specifically, that no group has a relative overabundance of cases or pre/post treatment periods. Table #, below, provides the distribution control cases across pre/post treatment periods.

### **\*\* TABLE A-1 ABOUT HERE \*\***

Of first note is that sample sizes remain fairly stable across recalls – though there are some shifts from sample to sample, the overall average of approximately 623 well represents the actual number of cases in any given sample, to a certain extent (samples 1 and 7 have larger departures than is typical, comparatively). Perhaps of primary concern, however, is how recall periods have been sampled within these cases – if it is the case that some samples have an overabundance of later periods as post-treatment as compared to others, this may result in systematic differences in coefficient estimates. This is because the structure of pre/post periods will also inherently be related to unobservable maturational processes among the sample and I would expect these processes to exert greater influence at later recall periods as the sample has collectively grown older and all participants have entered their early-twenties. Conversely, if there is a relative dearth pre



periods in one sample, I might expect treatment estimates of program participation to be positively biased, as I would be comparing older treated cases to younger untreated cases more often.

In order to assess the distribution of pre/post recall periods across control samples, I conduct a chi-square test of independence. The results from this test (ChiSq=52.67, p=0.999) confirm that no systematic differences exist in the distribution of pre/post recall periods across randomly selected control samples and, therefore, the following sensitivity analyses may be conducted under the assumption that the choice between samples is arbitrary, at least as it regards the labeling of recall periods as pre or post treatment.

### **III. SENSITIVITY ANALYSIS RESULTS – PREDICTING CHANGES IN ATTITUDES**

*Individual Group Plots.* Prior to compiling analysis results using “suest” I compared scatterplots of the four dependent variables – AWFL, FOI (opportunities and expectations), and MtS – using each control group, resulting in forty total figures that are combined into four, one for each dependent variable. These figures are presented below.

#### **\*\* FIGURES A-1 TO A-4 ABOUT HERE \*\***

In each figure, the markers and connected line have been emboldened to better distinguish control group plots (thick solid line with diamond markers) from those of other groups (whose lines are constant across all graphs). For simplicity of plotting the figure, legends have been removed as well as axis titles, but if needed for reference,

Figures 3 (AWFL, opportunities), 4 (AWFL, expectations), 5 (FOI), and 6 (MtS) contain this information for the variables plotted here.

First examining Figure A-1 (FOI), we see that all control groups plots tend to exhibit an upward trend in FOI scores as we move forward through recall periods, but there are some notable departures in how the different lines conform to this overall trend. For example, Control Group 3 exhibits several peaks and valleys, creating a jagged line while Control Group 7 exhibits an almost entirely smooth linear trend upward. Different contrasts abound, but each plot tends to agree with respect to FOI scores measured at  $t=0$ , and even though values may appear somewhat different across the plots, the scale of the axis must also be taken into consideration, as each control group plot stays within a range of 2.5 to 2.7, thus minimizing the absolute differences between any particular pair of points.

In contrast, AWFL expectations scores (Figure A-2) exhibit more irregularities – in some control groups (1,3,5,6,7, and 8) there is an overall upward trend while in others the trend appears to be flat (4,9) or downward (2,10). This might suggest that the choice of control group is important with respect to AWFL expectations, but a closer inspection of the axis scale and trends for other groups diminishes this possibility. Namely, trends in expectations for the remaining groups appear to be somewhat unpredictable themselves, with large upward or downward shifts from period to period. This might suggest that this measure is prone to significant and unpredictable shifts across the entirety of the sample, and that differences between control groups are simply a result of randomization, and not related to underlying systematic differences between samples. Further, the scale of the y-axis makes these small differences more pronounced – in only five plot points (out of

110) do control group expectation scores exceed the typical upper and lower boundaries of 3.7 and 3.55, respectively. Therefore, though these differences may appear large and systematic, it is more likely that they represent small, random shifts in scores representative of the entirety of the sample.

Moving to Figure A-3 (AWFL Opportunity scores) we see two basic trends present – the first, an upward trend (groups 1,2,3,5,8,9) and the second a flat trend (4,6,7,10). As was true before, however, the scale of the figure makes these differences appear more pronounced – most control group plots lie between scores of 4.4 and 4.5, with few points exceeding these boundaries. Further, intermediary plot points (i.e., those between recalls of -3 and +3) tend to agree more often than those at the very beginning or end of the observation window – coincidentally, these are also the recall periods having fewer cases within them (see Table #) which would naturally result in predictably higher disparities in average values of AWFL opportunity scores at these time points since these averages are computed from fewer available cases.

Finally, Figure A-4 plots MtS scores for all control groups. In all but two plots are upward trends evident and almost all plot points lie between a range of 3.35 to 3.45. As was true before, intermediary plot points tend to agree more often than those at the beginning and end of the observation window, and this is due to the fact that group averages at these points are computed using fewer available cases and are, thus, more prone to sample idiosyncrasies.

***Control Group Plots – All Trends.*** Figures A-5 through A-8, below, isolate just the plots for control groups, and include best fit lines for each to further compare potential group differences. First, FOI scores (Figure A-5) tend to cluster together fairly well, and all best fit lines agree on trends (upward) and appear to differ only slightly in the magnitude of these trends.

**\*\* FIGURES A-5 TO A-8 ABOUT HERE \*\***

AWFL expectations scores (Figure A-6) exhibit some irregularity – three flat/downward trends, seven upward – but there appears to be strong agreement in the magnitude of upward trends, as most of these lines cluster together on or after  $t=0$ . AWFL opportunity scores (Figure A-7) cluster together comparatively more than do expectations scores as almost all best fit lines intersect at some point in the distribution and generally agree with respect to the direction and magnitude of the underlying trends. Finally, MtS scores (Figure A-8) again cluster together fairly well, and all but three trend lines agree with respect to the magnitude and direction of underlying trends.

***Comparing Model Coefficients.*** As aforementioned, ten fixed-effect OLS models are estimated using each control group, and these estimates are then compiled using the “suest” command in STATA so that coefficients may be compared across all models simultaneously. The coefficient estimates for all variables within each model are presented below, in Tables A-2 to A-5.

**\*\* TABLE A-2 TO A-5 ABOUT HERE \*\***

*FOI Scores.* As is demonstrated in Table A-2, most coefficients exhibit small differences across models, as the expected difference between minimum and maximum coefficient values tends to be lower than .05, with just a few variables crossing this threshold. Namely, the coefficient for having a college degree differs by as much as .384 points and reaches statistical significance in just one instance (Group 4). Given the very low rates of obtaining a college degree in the total sample, one might expect this coefficient to be more prone to sampling differences. Regardless, the variability of this coefficient across all models resulted in an insignificant Wald test, confirming that, although it is significant in one instance, this result does not indicate that the coefficients are significantly different across all models. Further, current gang membership exhibits some notable differences across control groups, with a maximum difference of approximately .08. However, in no instance is this coefficient significant, and sampling variability does not appear to result in a systematic difference in coefficients as Wald tests for this variable were insignificant.

*AWFL – Expectation Scores.* Though most coefficients remain relatively stable across models (Table 3-A), there are a few that differ considerably. Namely, the minimum and maximum estimated effects of gang involvement and obtaining a college degree values differ by more than a tenth of a point on the AWFL expectation score. However, in both cases estimated effects agree with respect to direction – i.e., that the relationship is negative – but disagree with respect to magnitude. Despite these seeming differences, Wald tests for the equality of coefficients across all ten models yielded no significant findings, indicating that these differences, though considerable at face value, are not different enough as to suggest a systematic difference between control samples.

*AWFL – Opportunity Scores.* As might have been expected some coefficients do differ across models (Table 4-A) – most particularly those that explicitly use the control group for comparison (i.e., estimates of the treatment effect of program participation). For example, the estimate of the treatment effect for the only voluntary group ranges from -0.008 to 0.019 – it is important to note, however, that none of these coefficients reach any standard level of significance, as their corresponding standard errors are quite high relative to the size of the estimated effects. In contrast, significant coefficients, like those for the property variety score, exhibit little change over control groups – values stabilize around -0.027, indicating that a positive change in property offending variety is associated with a negative shift in perceptions of AWFL opportunities.

Though this list could abound with similar examples of either trend, the primary message drawn from this analysis regards the number of coefficients deemed significantly different through post-estimation Wald tests. As was true for prior models, there is no significant difference between any of these coefficients, again indicating that the choice of control group for this analysis is essentially arbitrary.

*MtS Scores.* As was true for prior outcomes, maximum differences in coefficients for MtS scores rarely exceed a value of .05 (Table A-5). Departures from this include whether the individual is a current gang member, has a job license, or has a college degree. In regard to the former, all coefficients are in the same direction (negative) but disagree with respect to the magnitude of the effect of gang membership. This same

pattern is evident for having a job license and in both cases, Wald tests indicate no significant differences between these coefficients. In contrast, the effect of having a college degree appears more variable. While most coefficients are positive, some are much greater than others. However, in no instance are any of the estimated effects significant, and Wald tests indicated no significant differences between these estimated effects across all control groups.

#### **IV. SENSITIVITY ANALYSIS RESULTS – PREDICTING CHANGES IN OFFENDING**

*Individual Group Plots.* Prior to compiling results using the “suest” command I compared scatterplots of the four dependent variables – offending rates including drugs, offending rates excluding drugs, property offending variety scores, and violent offending variety scores – using each control group, resulting in forty total figures that are combined into four, one for each dependent variable. These figures are presented below.

##### **\*\* FIGURES A-9 TO A-12 ABOUT HERE \*\***

In each figure, the markers and connected line have been emboldened to better distinguish control group plots (thick solid line with diamond markers) from those of other groups (whose lines are constant across all graphs). For simplicity of plotting the figure, legends have been removed as well as axis titles, but if needed for reference, Figures 7 (offending rates including drugs), 8 (offending rates excluding drugs), 9 (property offending variety scores), and 10 (violent offending variety scores) contain this information for the variables plotted here.

Beginning with Figure A-9, we see that offending rates including drugs exhibit slight dissimilarities across control groups. Though all trends appear to be somewhat flat overall, jumps in offending rates appear at different time periods. For example, in the plot for Control Group 2, there is a large spike in offending rates leading into  $t=0$ , then a sharp decline afterward. This same pattern is present for groups 4, 7, and to a certain extent, groups 9 and 10. In contrast, groups 5, 6, and 8 exhibit large jumps in offending rates at different time periods, either well before  $t=0$  (group 8) or after it (groups 5 and 6). However, a common pattern across all plots is that these jumps tend to be followed by large declines, and offending rates including drugs typically have similar values at the ends of the observation windows presented here (i.e.,  $t-5$  &  $t+5$ ).

Moving to Figure A-10, there is considerably more regularity in offending rates excluding drugs across all control groups. Though there are clearly slight disturbances in regard to when upward/downward spikes occur, all trends appear fairly stable and certain group trends display remarkable similarity. Specifically, the trends for groups 3 and 7 and for groups 5 and 9 exhibit almost the very same patterns. For the first set there are drops and then rises in offending rates leading into  $t=0$  then relative stability afterward. In regard to the second set, we see a jump in pre-  $t=0$  offending rates, then a decline into  $t=0$ , then another jump in rates immediately afterward. As we saw with offending rates including drugs, however, rates excluding drugs typically begin and end at the same or very similar values and most trends are either flat, or appear to have a very slight downward slope.

Property offending variety scores, depicted in Figure A-11, generally follow a stable, declining trend for all control groups. Exceptions to this are illustrated in groups 5,



7, 9, and 10. Groups 5 and 10 display very similar trends, beginning at low property offense variety then inclining as time moves forward before declining to pre-rise trends as of  $t+5$ . In contrast, group 7 exhibits a flat, fairly stable trend over time while group 9 displays a small upward trend. Across all groups, however, there are very slight shifts in trends, especially as compared to the treated groups in the analysis, and the overall trend appears to be either flat, or slightly declining.

Figure A-12 depicts violent offending variety scores and most control group trends clearly display a decline in violent offense variety over time. However, there are slight departures in the magnitude of this trend across groups – namely, groups 1, 2, 3, 6, and 7 display a strong downward trend, while groups 4, 5, and 8 appear to have a downward trend of lesser magnitude. In contrast groups 9 and 10 appear to follow somewhat dissimilar patterns. In regard to the former, the trend is relatively flat until  $t+1$ , then declines sharply before rising again. Absent time point  $t+5$ , however, this group would be quite similar to the others in respect to an overall downward trend. Therefore, the only group that appears to be an outlier is group 10, but this is mainly due to the rise in violent offense variety between  $t-4$  and  $t=0$  – absent this upward trend, the overall pattern for group 10 would be a declining one.

***Control Group Plots – All Trends.*** Figures A-13 through A-16, below, isolate just the plots for control groups, and include best-fit lines for each to further compare potential group differences. First, offending rates including drugs (Figure A-13) tend to cluster together fairly well, and most best-fit lines depict a slight downward trend. However,

there are three lines trending upward, and one trend line that is relatively flat. Overall, though, the aggregate picture here is that of a flat trend over time.

**\*\* FIGURES A-13 TO A-16 ABOUT HERE \*\***

We see comparatively more disagreement between trend lines for offending rates excluding drugs, depicted in Figure A-14. Trend lines do not cluster very well, though they do tend to meet at or around  $t=0$ . About half of the trend lines indicate a clear downward trend, while the remaining lines depict upward trends to varying degrees. Whatever the reason for these disparities, it appears that offending rates excluding drugs exhibit considerable variation across control groups, potentially implying that the choice of a group might matter for this analysis. It remains to be seen, however, if these differences in trends result in significantly different coefficient estimates.

In regard to property offense variety, the picture is considerably clearer. The majority of trend lines in Figure A-15 illustrate declines in property offending variety of similar magnitudes. Two trends do disagree with this overall picture – one of these trend lines is flat, while the other depicts an upward trend over time. Fortunately, these are the only apparent outliers across the 10 control groups, and an overall downward trend best reflects the pattern of property offense variety over time across all groups in the aggregate.

Similar to offending rates including drugs, trend lines for violent offending variety cluster tightly together, each illustrating a clear downward trend (Figure A-16). There are slight differences in the magnitude of a few trends and in their starting points at

$t$ -5, but these minute differences do not dramatically depart from the aggregate, downward trend that is apparent for all control groups.

***Comparing Model Coefficients.*** As aforementioned, ten fixed-effect OLS models are estimated using each control group, and these estimates are then compiled using the “suest” command in STATA so that coefficients may be compared across all models simultaneously. The coefficient estimates for all variables within each model are presented below, in Tables A-6 to A-9.

**\*\* TABLE A-6 TO A-9 ABOUT HERE \*\***

*Offending Rates (Incl. Drugs).* Coefficients for all variables predicting offending rates including drug sales are depicted in Table A-6. In contrast to prior coefficient comparisons, the differences between estimates are somewhat larger – the largest expected difference between a pair of coefficients in this table is .61 points (current gang membership) and there exist some sizable differences between estimates of treatment and group interaction coefficients. However, as it regards treatment and group interactions, no results are significant, and these disparities in estimates are not significant according to Wald tests. The same is true for current gang membership; though coefficients may vary widely, Wald tests suggest these differences are not systematic, nor are they for any variable in this analysis.

*Offending Rates (Excl. Drugs).* In contrast to coefficient differences for offending rates including drugs, differences for the rate excluding drugs are considerably smaller.

Depicted in Table A-7, the largest expected difference between two pairs of coefficients

is .38 points, and is associated with the current gang member variable. Though the differences in this coefficient may be large, Wald tests suggest these differences are not systematic. The next highest expected difference is between coefficients for the college degree variable, but all coefficients at least agree in direction and, again, Wald tests return as insignificant. For that matter, all Wald tests conducted on these pairs of coefficients came back as insignificant, suggesting that no systematic differences in estimates exist between any control groups.

*Property Offending Variety Scores.* Congruent to prior results, differences between coefficients predicting property offense variety are fairly small (Table A-8), the largest expected difference again being associated with the current gang membership variable (.17). However, all coefficients are in the same direction (negative) and are merely in disagreement with respect to the magnitude of the effect. Regardless, Wald tests for this and all other pairs of coefficients returned as insignificant, demonstrating that no systematic differences exist between the coefficients in models using any of the control groups.

*Violent Offending Variety Score.* Coefficient differences for violent offense variety are somewhat similar to those evidenced in the property offense variety models. The largest expected difference between two pairs of coefficients stems from the college degree variable (.23 points). Again, however, all coefficients agree with respect to direction, just not in their magnitude, while Wald tests suggest these differences are not systematic.

Also consistent with prior analyses, the current gang membership variable displays relatively large differences across models using different control group, with the maximum expected difference being .17 points. However, Wald tests for this and all other pairs of coefficients came back insignificant, suggesting that these differences, even though they are somewhat large, are not systematic.

Table A-1: Distribution of Pre/Post Recall Observations Across Control Groups

Pre/Post Recall	<u>Control Groups</u>										Total
	1	2	3	4	5	6	7	8	9	10	
-9	23	16	23	17	19	12	22	20	21	18	191
-8	47	51	59	51	48	44	57	55	59	40	511
-7	76	92	88	90	78	78	99	88	87	78	854
-6	127	124	128	137	116	123	142	120	123	117	1257
-5	174	166	172	193	157	166	179	159	167	158	1691
-4	221	215	219	238	219	227	233	213	217	199	2201
-3	267	277	262	291	282	292	298	277	281	264	2791
-2	342	343	330	343	352	361	363	357	338	345	3474
-1	408	418	409	430	441	427	454	440	411	426	4264
0	496	508	509	519	525	537	545	535	506	516	5196
1	569	571	576	586	587	611	616	576	577	576	5845
2	527	523	538	538	546	568	562	535	523	542	5402
3	487	479	491	493	504	515	508	497	489	499	4962
4	430	432	447	433	456	461	464	450	433	452	4458
5	370	385	393	375	411	394	399	397	374	401	3899
6	326	317	346	330	343	332	343	342	319	339	3337
7	270	255	284	274	286	258	277	278	263	275	2720
8	205	192	211	207	205	195	220	201	210	196	2042
<b>Total</b>	5365	5364	5485	5545	5575	5601	5781	5540	5398	5441	55095
<b>Sample Size</b>	605	610	622	622	628	635	650	628	616	612	6228

Table A-2: Coefficients for All Variables Across All Control Groups – FOI Scores

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
part_type#treat																				
CONTROL#1	-.009	.013	.014	.013	.010	.012	.009	.013	.000	.013	.022	.013	.003	.012	.005	.014	.004	.013	.022	.014
ONLY VOL#0	-.004	.017	.009	.016	.004	.016	.010	.016	.000	.017	.015	.017	-.001	.016	.005	.017	.002	.017	.015	.017
ONLY VOL#1	.013	.017	.029	.017	.024	.017	.025	.017	.017	.017	.027	.017	.020	.017	.019	.017	.019	.017	.034	.017
ONLY INVOL#0	-.038	.021	-.024	.021	-.029	.021	-.024	.021	-.032	.021	-.019	.021	-.033	.021	-.029	.022	-.030	.021	-.020	.021
ONLY INVOL#1	.011	.015	.027	.015	.023	.015	.025	.015	.018	.015	.028	.015	.017	.015	.018	.015	.018	.015	.032	.015
MIXED#0	-.035	.020	-.021	.020	-.027	.020	-.023	.020	-.031	.020	-.016	.020	-.029	.020	-.028	.020	-.027	.020	-.018	.020
MIXED#1	.072	.025	.087	.025	.084	.025	.087	.025	.077	.025	.088	.025	.083	.025	.079	.025	.080	.025	.093	.025
<i>Social Rewards from Crime</i>	-.033	.009	-.036	.009	-.032	.009	-.032	.008	-.038	.008	-.027	.008	-.035	.008	-.027	.009	-.033	.009	-.025	.009
<i>Personal Rewards from Crime</i>	-.015	.004	-.015	.004	-.016	.005	-.016	.005	-.014	.004	-.016	.005	-.014	.004	-.016	.005	-.015	.004	-.017	.005
<i>Logged Offending Rate (Excl. Drugs)</i>	.027	.036	.036	.035	.039	.033	.019	.030	.050	.034	.018	.030	.006	.030	.047	.034	.028	.037	.061	.035
<i>Property Offending Var. Score</i>	-.014	.008	-.008	.008	-.011	.008	-.004	.007	-.010	.008	-.009	.007	-.009	.007	-.006	.008	-.004	.008	-.010	.008
<i>Violent Offending Var. Score</i>	.013	.010	.006	.010	.005	.010	-.001	.010	.007	.010	.001	.009	.007	.010	.001	.010	.004	.010	.002	.009
<i>Age</i>	.012	.006	.009	.006	.008	.006	.010	.006	.007	.006	.009	.006	.008	.006	.012	.006	.007	.006	.006	.006
<i>Employment Rate</i>	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000
<i>Illegal Employment Rate</i>	-.001	.001	-.001	.000	-.001	.000	.000	.000	-.001	.000	.000	.000	-.001	.000	-.001	.001	-.001	.001	-.001	.001
<i>Illegal Employment Variety</i>	.036	.023	.027	.023	.029	.023	.011	.023	.043	.022	.012	.022	.025	.021	.042	.024	.032	.024	.023	.023
<i>Number of Months in Recall</i>	-.001	.003	-.002	.003	-.002	.003	.001	.003	.001	.003	.001	.003	-.001	.003	.000	.003	-.001	.003	.001	.003
<i>Interviewed in Facility?</i>	.014	.020	.021	.021	.009	.020	.014	.020	.024	.021	.005	.021	.029	.020	.010	.020	.026	.020	.021	.021
<i>Resistance to Peer Influence</i>	.076	.016	.082	.017	.079	.017	.073	.017	.064	.017	.077	.016	.071	.016	.063	.017	.062	.016	.082	.017
<i>Prop. Friends Ever Arrested</i>	.005	.030	-.001	.031	-.009	.030	-.003	.030	.008	.031	.001	.031	.003	.031	.012	.031	-.021	.029	.015	.031
<i>Prop. Friends Ever Jailed</i>	-.018	.033	.008	.033	.012	.033	.006	.033	.007	.034	.002	.034	-.008	.033	-.012	.034	.018	.032	-.006	.034
<i>Member of a Gang?</i>	-.023	.069	-.038	.067	-.057	.074	.023	.057	-.037	.067	-.008	.062	.030	.069	.005	.077	-.011	.067	-.024	.064
<i>Contact with Gang Members</i>	.000	.013	.002	.012	.006	.014	-.015	.011	.006	.013	.001	.012	-.007	.012	-.007	.013	-.004	.012	-.002	.012
<i>BSI Factor Score</i>	.018	.009	.028	.009	.027	.009	.025	.009	.021	.009	.022	.009	.026	.009	.022	.009	.012	.009	.022	.009
<i>YPI Factor Score</i>	-.015	.012	-.015	.012	-.017	.012	-.019	.012	-.024	.012	-.028	.011	-.019	.011	-.019	.012	-.015	.013	-.019	.012
<i>PSMI</i>	.141	.023	.152	.023	.149	.025	.136	.024	.134	.023	.132	.024	.157	.023	.128	.024	.139	.023	.143	.025
<i>Logged # of Caring Adults</i>	-.018	.013	-.034	.014	-.028	.013	-.023	.014	-.022	.014	-.023	.014	-.025	.013	-.019	.014	-.023	.014	-.031	.014
<i>Domains of Social Support</i>	.016	.005	.019	.005	.017	.005	.013	.005	.014	.005	.018	.005	.013	.005	.014	.005	.014	.005	.017	.005
<i>Diversity of Caring Adults</i>	-.016	.007	-.015	.007	-.011	.007	-.009	.007	-.018	.007	-.021	.007	-.009	.006	-.013	.007	-.020	.007	-.008	.006
<i>Substance Abuse - Total</i>	-.005	.002	-.003	.002	-.004	.002	-.003	.002	-.004	.002	-.003	.002	-.004	.002	-.003	.002	-.004	.002	-.004	.002
<i>Substance Abuse Variety</i>	.002	.010	-.007	.010	-.002	.010	-.005	.010	-.005	.010	-.003	.010	.000	.010	-.003	.011	-.004	.010	.000	.010
<i>Has a Job License?</i>	.045	.039	.041	.036	.080	.039	.062	.035	.076	.038	.042	.035	.048	.035	.040	.038	.078	.038	.042	.034
<i>Has a GED?</i>	.071	.032	.079	.032	.075	.036	.059	.034	.082	.033	.075	.034	.097	.031	.066	.035	.091	.032	.072	.035
<i>Has a High School Degree?</i>	-.001	.031	.013	.030	.033	.029	-.012	.029	.009	.030	.010	.028	.018	.029	.007	.030	.038	.029	.032	.030
<i>Has a College Degree?</i>	.125	.157	-.056	.215	.124	.147	.328	.141	.048	.188	.105	.148	.097	.169	.266	.161	.029	.176	.124	.177
<i>Logged Average Legal Wage Rate</i>	.019	.011	.021	.011	.025	.011	.020	.012	.033	.011	.029	.011	.027	.011	.032	.011	.032	.011	.030	.011
<i>Number of Work Interruptions</i>	-.004	.008	-.007	.008	-.003	.008	-.002	.008	-.010	.008	-.004	.008	-.003	.008	-.010	.008	-.009	.008	-.010	.008
<i>Moral Disengagement</i>	.001	.003	.002	.002	.004	.003	.002	.003	.003	.003	.003	.003	.000	.002	.002	.003	.000	.002	.002	.003
<i>Age at First Offense</i>	.000	.001	.001	.001	.002	.001	.002	.001	.002	.001	.001	.001	.002	.001	.001	.001	.001	.001	.003	.001
<i>White</i>	.003	.011	.000	.009	-.002	.010	-.004	.009	-.003	.009	.002	.009	-.004	.010	-.005	.010	-.003	.010	-.009	.010
<i>Black</i>	.003	.011	.001	.010	-.001	.010	-.002	.010	-.007	.010	.002	.010	-.004	.011	-.006	.011	-.003	.011	-.007	.011
<i>Hispanic</i>	.004	.011	-.001	.010	-.003	.010	-.008	.009	-.001	.009	-.001	.009	-.004	.011	-.006	.010	-.003	.010	-.008	.010
<i>Male</i>	.006	.006	.005	.006	.005	.005	.007	.005	.004	.005	.007	.005	.011	.005	.006	.006	.007	.006	.008	.005
<i>Site (Phoenix=1)</i>	.011	.006	.010	.006	.004	.006	.006	.006	.003	.006	.007	.006	.003	.006	.007	.006	.004	.006	.009	.006
<i>Intercept</i>	-.005	.026	-.031	.024	-.015	.024	-.025	.023	-.002	.024	-.038	.024	-.012	.024	-.009	.025	-.002	.025	-.038	.026

Table A-3: Coefficients for All Variables Across All Control Groups – AWFL Scores  
(Opportunities)

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
part_type#treat																				
CONTROL#1	.008	.014	.007	.014	.017	.014	.013	.014	-.009	.014	.027	.013	.004	.014	.009	.015	.023	.015	-.002	.015
ONLY VOL#0	.003	.017	.007	.017	.006	.017	.006	.017	-.004	.017	.013	.017	.004	.017	.011	.017	.011	.017	.003	.017
ONLY VOL#1	.003	.019	.004	.019	.008	.019	.004	.018	-.008	.019	.014	.018	.000	.018	.004	.019	.019	.019	-.005	.019
ONLY INVOL#0	-.005	.020	-.001	.020	-.005	.020	-.004	.020	-.011	.020	.002	.019	-.003	.020	-.001	.020	.002	.020	-.008	.020
ONLY INVOL#1	-.004	.014	-.006	.014	-.001	.014	-.005	.014	-.016	.014	.005	.013	-.009	.013	-.003	.014	.007	.014	-.010	.014
MIXED#0	.023	.024	.028	.024	.026	.024	.026	.024	.018	.024	.031	.024	.027	.024	.028	.024	.034	.024	.021	.025
MIXED#1	-.005	.024	-.008	.023	-.002	.023	-.004	.024	-.018	.023	.007	.023	-.007	.023	-.005	.024	.007	.023	-.012	.024
<i>Social Rewards from Crime</i>	-.026	.009	-.030	.009	-.020	.008	-.017	.008	-.032	.009	-.023	.009	-.022	.008	-.025	.008	-.031	.009	-.031	.009
<i>Personal Rewards from Crime</i>	-.014	.004	-.010	.004	-.014	.005	-.014	.005	-.014	.004	-.013	.005	-.007	.004	-.013	.005	-.011	.004	-.018	.006
<i>Logged Offending Rate (Excl. Drugs)</i>	-.006	.038	.015	.039	.033	.035	.024	.039	.024	.038	.029	.035	.016	.039	.033	.039	-.012	.039	-.004	.036
<i>Property Offending Var. Score</i>	-.027	.009	-.029	.009	-.029	.010	-.027	.009	-.027	.010	-.025	.009	-.031	.010	-.027	.009	-.029	.010	-.031	.009
<i>Violent Offending Var. Score</i>	-.003	.010	-.008	.010	.003	.010	-.011	.010	-.002	.011	-.012	.010	-.003	.010	-.009	.010	.002	.011	-.005	.010
<i>Age</i>	.002	.006	.001	.006	-.001	.006	.000	.007	-.001	.006	-.003	.006	.003	.007	-.002	.006	-.003	.006	.001	.006
<i>Employment Rate</i>	.000	.000	.001	.000	.000	.000	.001	.000	.001	.000	.000	.000	.001	.000	.001	.000	.000	.000	.001	.000
<i>Illegal Employment Rate</i>	-.002	.001	-.001	.001	-.001	.000	-.001	.000	-.001	.001	-.002	.000	-.001	.000	-.001	.001	-.002	.000	-.002	.001
<i>Illegal Employment Variety</i>	.078	.024	.060	.026	.056	.024	.054	.025	.051	.024	.063	.024	.054	.023	.073	.027	.084	.023	.070	.025
<i>Number of Months in Recall</i>	-.005	.003	-.006	.003	-.003	.003	-.004	.003	-.002	.003	-.002	.003	-.005	.003	-.001	.003	-.005	.003	-.002	.003
<i>Interviewed in Facility?</i>	.113	.021	.108	.020	.090	.021	.091	.021	.099	.021	.084	.021	.105	.020	.102	.020	.090	.021	.103	.021
<i>Resistance to Peer Influence</i>	.032	.016	.041	.017	.031	.017	.036	.016	.027	.016	.030	.016	.018	.016	.034	.016	.028	.016	.022	.016
<i>Prop. Friends Ever Arrested</i>	.052	.036	.032	.036	.009	.035	.003	.033	.027	.034	.057	.035	.031	.034	.049	.033	.021	.034	.075	.033
<i>Prop. Friends Ever Jailed</i>	-.077	.036	-.049	.036	-.034	.037	-.010	.035	-.039	.035	-.050	.036	-.039	.034	-.067	.034	-.046	.035	-.074	.034
<i>Member of a Gang?</i>	-.085	.081	-.098	.078	-.094	.087	-.035	.066	-.070	.069	-.019	.065	-.014	.060	-.085	.081	-.126	.081	-.119	.077
<i>Contact with Gang Members</i>	.017	.014	.020	.014	.018	.014	.001	.011	.010	.012	.005	.011	.011	.011	.010	.014	.018	.013	.017	.013
<i>BSI Factor Score</i>	.019	.010	.018	.010	.013	.010	.011	.010	.010	.010	.005	.010	.009	.010	.013	.010	.008	.010	.013	.011
<i>YPI Factor Score</i>	.007	.011	.019	.011	.005	.011	.006	.010	.007	.011	.000	.011	.005	.010	-.008	.010	.006	.011	.003	.011
<i>PSMI</i>	.117	.022	.117	.021	.102	.024	.100	.024	.107	.023	.088	.023	.123	.022	.082	.023	.107	.022	.094	.025
<i>Logged # of Caring Adults</i>	.005	.013	-.005	.013	-.011	.013	.006	.013	-.002	.013	.001	.013	-.002	.013	.012	.014	-.003	.013	.008	.013
<i>Domains of Social Support</i>	.009	.005	.010	.005	.011	.005	.006	.005	.008	.005	.009	.005	.010	.005	.006	.005	.009	.005	.010	.005
<i>Diversity of Caring Adults</i>	.006	.007	.004	.007	.010	.007	.007	.007	.007	.007	.004	.007	.003	.006	.004	.007	.007	.007	.002	.007
<i>Substance Abuse - Total</i>	.001	.002	.000	.002	.000	.002	.001	.002	.003	.002	.001	.002	.002	.002	.002	.002	.002	.002	.002	.002
<i>Substance Abuse Variety</i>	-.002	.011	-.003	.011	-.001	.011	-.006	.011	-.013	.011	-.010	.011	-.005	.011	-.010	.011	-.008	.011	-.007	.011
<i>Has a Job License?</i>	.026	.038	-.021	.038	.006	.038	.029	.035	-.017	.040	-.002	.036	-.023	.035	-.005	.037	.016	.037	.016	.039
<i>Has a GED?</i>	.080	.034	.113	.036	.083	.039	.055	.039	.107	.036	.099	.041	.117	.034	.073	.040	.104	.035	.072	.039
<i>Has a High School Degree?</i>	-.031	.031	-.003	.027	-.021	.030	-.043	.027	-.035	.030	-.029	.029	-.018	.029	-.010	.026	-.031	.030	-.022	.030
<i>Has a College Degree?</i>	.000	.073	-.062	.114	.023	.053	.021	.063	.041	.074	.011	.055	.025	.060	.011	.055	-.005	.074	-.007	.079
<i>Logged Average Legal Wage Rate</i>	-.011	.012	-.006	.012	-.006	.011	-.006	.012	-.011	.011	-.001	.010	-.014	.012	-.015	.012	-.007	.012	-.018	.012
<i>Number of Work Interruptions</i>	.010	.008	.009	.008	.006	.007	.014	.008	.013	.008	.015	.008	.014	.008	.012	.008	.011	.008	.017	.008
<i>Moral Disengagement</i>	-.005	.003	-.007	.003	-.004	.003	-.006	.003	-.005	.003	-.005	.002	-.007	.002	-.004	.003	-.005	.003	-.003	.003
<i>Age at First Offense</i>	.001	.001	.002	.001	.001	.001	.001	.001	.000	.001	.001	.001	.001	.001	.002	.001	.000	.001	.001	.001
<i>White</i>	-.029	.016	-.019	.011	-.023	.011	-.036	.013	-.035	.013	-.035	.013	-.035	.014	-.043	.014	-.020	.013	-.029	.013
<i>Black</i>	-.027	.016	-.013	.012	-.014	.012	-.028	.013	-.031	.013	-.027	.013	-.025	.014	-.038	.014	-.018	.013	-.031	.013
<i>Hispanic</i>	-.026	.016	-.014	.011	-.013	.012	-.033	.013	-.026	.013	-.029	.012	-.027	.013	-.036	.014	-.016	.013	-.028	.012
<i>Male</i>	.002	.005	.001	.005	.001	.004	.001	.005	-.001	.004	.004	.005	.003	.005	.003	.005	.002	.004	.005	.005
<i>Site (Phoenix=1)</i>	-.004	.007	.001	.006	.002	.006	-.002	.006	-.008	.006	.000	.006	-.002	.006	.000	.006	-.003	.006	-.006	.006
<i>Intercept</i>	.050	.028	.011	.025	.017	.025	.032	.025	.071	.026	.033	.025	.033	.026	.033	.027	.020	.026	.032	.026



Table A-4: Coefficients for All Variables Across All Control Groups – AWFL Scores  
(Expectations)

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
part_type#treat																				
CONTROL#1	.006	.018	-.005	.020	.023	.017	.001	.018	.002	.019	.043	.018	.020	.016	.015	.020	.021	.019	-.004	.020
ONLY VOL#0	-.020	.024	-.016	.025	-.004	.024	-.020	.024	-.011	.025	.007	.025	-.002	.024	-.008	.025	-.008	.025	-.016	.025
ONLY VOL#1	-.012	.025	-.012	.025	.006	.025	-.014	.025	-.009	.026	.019	.025	.002	.025	-.005	.026	.007	.026	-.019	.026
ONLY INVOL#0	-.021	.027	-.016	.028	-.007	.027	-.026	.027	-.015	.027	.004	.027	-.003	.027	-.014	.028	-.011	.027	-.021	.028
ONLY INVOL#1	-.020	.020	-.023	.021	-.004	.020	-.023	.020	-.017	.021	.008	.021	-.005	.020	-.013	.022	-.005	.021	-.026	.021
MIXED#0	.000	.031	.008	.032	.020	.031	.001	.032	.010	.031	.029	.032	.021	.031	.008	.032	.017	.032	.004	.032
MIXED#1	-.012	.036	-.020	.036	.002	.036	-.018	.036	-.012	.036	.016	.036	.003	.035	-.007	.037	.002	.036	-.022	.036
	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000
<i>Social Rewards from Crime</i>	-.062	.013	-.064	.014	-.055	.013	-.063	.013	-.070	.013	-.065	.013	-.072	.013	-.073	.013	-.074	.013	-.060	.013
<i>Personal Rewards from Crime</i>	-.015	.006	-.012	.005	-.013	.006	-.016	.006	-.016	.006	-.011	.006	-.011	.005	-.012	.006	-.012	.005	-.013	.007
<i>Logged Offending Rate (Excl. Drugs)</i>	.017	.045	.014	.047	.054	.043	.015	.045	.003	.044	.024	.042	-.007	.046	.001	.045	.020	.045	.002	.043
<i>Property Offending Var. Score</i>	-.041	.011	-.038	.011	-.040	.011	-.031	.011	-.032	.011	-.036	.010	-.037	.011	-.035	.011	-.040	.011	-.035	.010
<i>Violent Offending Var. Score</i>	-.010	.014	-.015	.015	-.005	.014	-.018	.014	-.004	.014	-.018	.014	-.020	.013	-.020	.015	-.001	.014	-.015	.014
<i>Age</i>	-.026	.009	-.022	.009	-.031	.009	-.031	.009	-.029	.009	-.032	.009	-.028	.009	-.033	.009	-.030	.009	-.025	.009
<i>Employment Rate</i>	.001	.000	.000	.000	.000	.000	.001	.000	.000	.000	.000	.000	.001	.000	.001	.000	.000	.000	.000	.000
<i>Illegal Employment Rate</i>	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001	.001	.001	.000	.001	.000	.001	.000	.001
<i>Illegal Employment Variety</i>	.043	.031	.039	.032	.028	.031	.019	.034	.008	.031	.013	.031	.015	.031	.029	.034	.025	.034	.031	.032
<i>Number of Months in Recall</i>	.009	.005	.005	.005	.011	.004	.011	.004	.012	.005	.010	.004	.009	.005	.014	.005	.009	.005	.009	.005
<i>Interviewed in Facility?</i>	.114	.030	.106	.029	.112	.028	.070	.029	.087	.028	.089	.029	.105	.029	.091	.029	.075	.029	.099	.029
<i>Resistance to Peer Influence</i>	.089	.023	.081	.024	.077	.023	.104	.024	.075	.023	.103	.023	.071	.023	.088	.023	.072	.023	.093	.023
<i>Prop. Friends Ever Arrested</i>	.035	.051	-.009	.053	-.011	.049	.018	.049	.022	.049	.021	.050	.042	.047	.069	.050	.001	.049	.066	.050
<i>Prop. Friends Ever Jailed</i>	-.080	.056	-.023	.057	-.007	.054	-.022	.053	-.031	.054	-.050	.054	-.055	.051	-.077	.054	-.040	.054	-.071	.054
<i>Memmer of a Gang?</i>	-.104	.098	-.130	.107	-.175	.124	-.191	.105	-.202	.106	-.197	.111	-.118	.108	-.239	.119	-.204	.109	-.221	.101
<i>Contact with Gang Members</i>	.012	.016	.020	.017	.020	.020	.020	.018	.021	.019	.023	.018	.019	.017	.029	.018	.026	.018	.026	.017
<i>BSI Factor Score</i>	-.015	.014	-.015	.014	-.017	.014	-.017	.014	-.019	.015	-.026	.014	-.020	.014	-.019	.014	-.027	.014	-.011	.014
<i>YPI Factor Score</i>	.005	.015	.017	.016	.000	.015	.007	.015	.015	.016	.000	.015	-.002	.015	.006	.015	.006	.015	.003	.015
<i>PSMI</i>	.298	.034	.336	.034	.300	.033	.315	.034	.322	.034	.307	.034	.345	.032	.279	.034	.312	.035	.298	.035
<i>Logged # of Caring Adults</i>	.011	.019	-.017	.019	-.005	.019	-.006	.019	-.010	.019	-.010	.019	-.009	.019	.002	.020	-.009	.019	-.007	.019
<i>Domains of Social Support</i>	.010	.007	.016	.008	.010	.008	.014	.007	.011	.007	.016	.007	.014	.007	.009	.008	.015	.007	.014	.008
<i>Diversity of Caring Adults</i>	-.012	.010	-.014	.010	-.011	.010	-.018	.010	-.010	.010	-.016	.010	-.013	.010	-.013	.010	-.018	.010	-.009	.010
<i>Substance Abuse - Total</i>	-.008	.003	-.010	.004	-.009	.003	-.007	.003	-.005	.003	-.008	.003	-.007	.003	-.008	.003	-.009	.003	-.008	.003
<i>Substance Abuse Variety</i>	.009	.020	.003	.020	-.005	.020	-.008	.020	-.017	.020	-.005	.020	-.004	.020	-.004	.020	.007	.020	-.001	.020
<i>Has a Job License?</i>	.015	.061	-.044	.060	-.023	.060	.019	.056	.009	.058	-.022	.056	-.025	.056	.003	.060	-.016	.058	-.010	.059
<i>Has a GED?</i>	.055	.051	.054	.053	.083	.052	.023	.051	.075	.051	.054	.055	.048	.051	.037	.054	.050	.051	.047	.054
<i>Has a High School Degree?</i>	-.010	.045	.012	.045	.024	.043	-.008	.041	-.004	.042	.007	.040	.015	.043	.018	.043	.002	.042	.024	.044
<i>Has a College Degree?</i>	-.262	.114	-.516	.077	-.133	.123	-.101	.156	-.171	.162	-.154	.135	-.101	.181	-.140	.136	-.281	.108	-.291	.105
<i>Logged Average Legal Wage Rate</i>	.021	.018	.035	.017	.043	.016	.027	.016	.020	.017	.029	.017	.016	.017	.014	.017	.033	.017	.036	.016
<i>Number of Work Interruptions</i>	-.001	.011	-.007	.011	-.013	.011	-.015	.010	-.013	.011	-.001	.011	-.007	.011	-.007	.011	-.011	.011	-.009	.011
<i>Moral Disengagement</i>	-.002	.003	-.005	.003	-.002	.003	-.003	.003	-.004	.003	-.003	.003	-.004	.003	-.003	.003	-.004	.003	-.004	.003
<i>Age at First Offense</i>	-.001	.002	.000	.002	.000	.002	.000	.002	.000	.002	-.001	.002	.000	.002	-.002	.002	-.002	.002	.000	.002
<i>White</i>	-.024	.023	-.013	.018	-.022	.019	-.024	.018	-.030	.018	-.028	.018	-.030	.020	-.043	.019	-.021	.020	-.017	.019
<i>Black</i>	-.021	.024	-.004	.019	-.004	.020	-.018	.019	-.019	.019	-.017	.019	-.023	.021	-.032	.020	-.014	.021	-.012	.019
<i>Hispanic</i>	-.012	.023	-.001	.018	-.004	.019	-.018	.018	-.015	.018	-.016	.018	-.012	.020	-.025	.019	-.009	.020	-.006	.019
<i>Male</i>	.003	.007	.001	.007	.008	.007	.007	.007	.003	.007	.007	.007	.006	.007	-.001	.008	.004	.007	.007	.008
<i>Site (Phoenix=1)</i>	-.001	.009	.013	.009	.008	.009	.008	.009	.002	.009	.007	.009	-.002	.009	.007	.009	.001	.009	.003	.009
<i>Intercept</i>	.069	.040	.018	.038	.006	.038	.028	.037	.036	.037	.025	.037	.043	.037	.069	.038	.041	.038	.026	.039

Table A-5: Coefficients for All Variables Across All Control Groups – MtS Scores

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
part_type#treat																				
CONTROL#1	.012	.013	.006	.014	.016	.013	.002	.013	.005	.014	.005	.015	.024	.014	.018	.015	.033	.013	.024	.014
ONLY VOL#0	-.004	.016	-.015	.017	-.006	.017	-.016	.016	-.012	.017	-.014	.017	.001	.016	-.006	.017	.006	.017	-.003	.017
ONLY VOL#1	.007	.018	.008	.018	.014	.018	.001	.018	.006	.019	.008	.019	.024	.019	.016	.019	.028	.018	.019	.018
ONLY INVOL#0	-.038	.020	-.050	.021	-.038	.021	-.054	.020	-.045	.021	-.051	.021	-.033	.020	-.041	.021	-.031	.020	-.038	.020
ONLY INVOL#1	.021	.015	.021	.015	.026	.015	.013	.015	.017	.015	.017	.016	.036	.015	.027	.016	.041	.015	.030	.015
MIXED#0	.020	.022	.009	.022	.019	.022	.009	.022	.014	.022	.010	.023	.027	.022	.020	.022	.030	.022	.023	.022
MIXED#1	-.010	.026	-.008	.026	-.007	.026	-.021	.026	-.015	.026	-.013	.026	.003	.026	-.005	.026	.009	.026	-.001	.026
<i>Social Rewards from Crime</i>	-.061	.009	-.059	.009	-.058	.009	-.061	.009	-.065	.009	-.069	.009	-.059	.009	-.062	.009	-.065	.009	-.064	.009
<i>Personal Rewards from Crime</i>	-.008	.004	-.007	.004	-.005	.004	-.007	.004	-.007	.004	-.005	.004	-.004	.004	-.006	.004	-.004	.004	-.006	.004
<i>Logged Offending Rate (Excl. Drugs)</i>	.023	.036	.022	.037	.001	.036	.001	.033	-.006	.036	.009	.033	.006	.036	.009	.035	-.017	.036	.008	.033
<i>Property Offending Var. Score</i>	-.019	.008	-.010	.009	-.018	.009	-.019	.008	-.022	.009	-.018	.008	-.021	.009	-.017	.009	-.011	.008	-.021	.008
<i>Violent Offending Var. Score</i>	-.002	.011	-.008	.011	-.006	.010	-.004	.010	-.006	.011	-.003	.010	-.012	.011	-.010	.011	-.002	.011	-.005	.010
<i>Age</i>	.015	.006	.013	.006	.009	.006	.004	.006	.010	.006	.005	.006	.004	.006	.004	.006	.010	.006	.007	.006
<i>Employment Rate</i>	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000
<i>Illegal Employment Rate</i>	.000	.000	.000	.000	.000	.000	.000	.001	.000	.001	.000	.001	.000	.000	.000	.001	.000	.001	.000	.000
<i>Illegal Employment Variety</i>	-.009	.024	.005	.025	.006	.025	.018	.026	.022	.025	.027	.025	.015	.025	-.012	.027	.016	.027	-.007	.024
<i>Number of Months in Recall</i>	-.006	.003	-.007	.003	-.005	.003	-.003	.003	-.007	.003	-.005	.003	-.004	.003	-.005	.003	-.007	.003	-.006	.003
<i>Interviewed in Facility?</i>	-.006	.021	-.005	.022	.001	.021	-.029	.020	-.012	.021	-.028	.020	-.012	.020	.006	.022	-.019	.020	-.015	.020
<i>Resistance to Peer Influence</i>	.037	.017	.028	.017	.024	.017	.040	.017	.035	.017	.029	.016	.031	.016	.040	.016	.026	.017	.041	.017
<i>Prop. Friends Ever Arrested</i>	.053	.037	.064	.037	.017	.038	.034	.037	.021	.037	.053	.036	.035	.036	.029	.037	.032	.037	.018	.037
<i>Prop. Friends Ever Jailed</i>	-.073	.039	-.081	.040	-.044	.040	-.041	.040	-.042	.039	-.057	.039	-.050	.039	-.041	.039	-.022	.040	-.015	.040
<i>Member of a Gang?</i>	-.047	.071	-.054	.071	-.071	.069	-.068	.074	-.045	.071	-.090	.065	-.082	.074	-.113	.068	-.105	.067	-.047	.071
<i>Contact with Gang Members</i>	-.006	.012	-.001	.012	-.005	.012	-.001	.013	-.002	.012	.003	.012	.003	.012	.006	.012	.009	.012	-.001	.012
<i>BSI Factor Score</i>	-.029	.011	-.036	.011	-.025	.011	-.030	.011	-.027	.011	-.028	.011	-.029	.011	-.031	.011	-.032	.011	-.025	.011
<i>YPI Factor Score</i>	-.035	.011	-.037	.011	-.031	.011	-.032	.010	-.024	.011	-.029	.010	-.038	.011	-.035	.011	-.023	.011	-.026	.010
<i>PSMI</i>	.130	.024	.107	.023	.139	.022	.127	.022	.131	.023	.117	.022	.121	.023	.130	.023	.129	.024	.112	.023
<i>Logged # of Caring Adults</i>	.007	.014	.005	.014	.005	.015	.008	.014	-.006	.014	.006	.014	-.003	.014	.006	.015	-.006	.014	.007	.014
<i>Domains of Social Support</i>	.012	.006	.016	.006	.012	.006	.014	.006	.018	.006	.014	.006	.018	.006	.013	.006	.015	.006	.015	.006
<i>Diversity of Caring Adults</i>	.009	.007	.006	.007	.003	.007	-.004	.007	.004	.007	.005	.007	.000	.007	.005	.007	.006	.007	.006	.007
<i>Substance Abuse - Total</i>	-.005	.002	-.006	.002	-.005	.002	-.004	.002	-.005	.002	-.007	.002	-.005	.002	-.005	.002	-.007	.002	-.003	.002
<i>Substance Abuse Variety</i>	.001	.012	-.002	.012	.000	.012	-.004	.012	.007	.011	.011	.011	.001	.012	.004	.012	.010	.011	-.001	.011
<i>Has a Job License?</i>	-.005	.036	-.065	.040	-.052	.041	-.033	.040	-.039	.043	-.071	.039	-.072	.038	-.024	.040	-.023	.040	-.041	.040
<i>Has a GED?</i>	.035	.034	.038	.035	.045	.036	.050	.034	.048	.036	.062	.035	.049	.034	.071	.035	.034	.035	.073	.035
<i>Has a High School Degree?</i>	-.018	.029	.028	.031	.026	.029	.034	.029	.054	.030	.049	.027	.037	.029	.031	.028	.025	.028	.012	.030
<i>Has a College Degree?</i>	.004	.161	.006	.251	.074	.120	.186	.100	.049	.160	.092	.119	.100	.134	.066	.134	-.020	.153	.160	.111
<i>Logged Average Legal Wage Rate</i>	.028	.012	.036	.012	.025	.012	.021	.012	.024	.012	.022	.012	.027	.012	.030	.012	.017	.012	.026	.012
<i>Number of Work Interruptions</i>	-.005	.008	-.003	.008	-.001	.008	-.003	.008	-.001	.008	.005	.008	-.004	.008	-.002	.008	-.001	.008	-.006	.008
<i>Moral Disengagement</i>	-.004	.002	-.003	.002	-.004	.002	-.002	.002	-.003	.002	-.003	.002	-.003	.002	-.005	.002	-.002	.002	-.005	.002
<i>Age at First Offense</i>	-.001	.001	-.001	.001	-.002	.001	.000	.001	-.001	.001	-.001	.001	-.001	.001	-.001	.001	-.001	.001	-.001	.001
<i>White</i>	-.003	.013	-.008	.013	-.001	.013	.001	.013	.000	.013	-.008	.013	.000	.012	-.006	.014	.012	.014	.011	.013
<i>Black</i>	-.002	.013	-.005	.014	.006	.013	.013	.013	.004	.014	-.001	.013	.010	.013	.006	.015	.020	.014	.018	.014
<i>Hispanic</i>	.008	.013	.002	.014	.015	.013	.020	.013	.014	.013	.000	.013	.016	.012	.016	.014	.022	.013	.027	.013
<i>Male</i>	.004	.005	.002	.005	.002	.005	.003	.005	.002	.005	.003	.005	.002	.005	.002	.005	.002	.005	-.001	.005
<i>Site (Phoenix=1)</i>	-.015	.006	-.014	.006	-.004	.007	-.004	.006	-.008	.007	-.006	.006	-.008	.006	-.006	.006	-.007	.006	-.007	.007
<i>Intercept</i>	.035	.026	.045	.027	.040	.026	.013	.026	.033	.027	.031	.026	.025	.026	.023	.027	.001	.029	.001	.028

Table A-6: Coefficients for All Variables Across All Control Groups – Offending Rates  
(Incl. Drugs)

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
1.treat	-.112	.225	-.213	.365	-.034	.179	.368	.321	.033	.169	.045	.156	-.016	.165	-.097	.099	.075	.085	.105	.217
part_type#treat																				
ONLY VOL#0	-.461	.413	-.304	.448	-.385	.436	-.032	.511	-.194	.443	-.187	.465	-.243	.443	-.488	.417	-.393	.407	-.311	.432
ONLY VOL#1	-.595	.369	-.391	.445	-.603	.369	-.603	.387	-.494	.394	-.447	.379	-.457	.389	-.591	.374	-.643	.371	-.663	.386
ONLY INVOL#0	-.486	.373	-.293	.381	-.426	.368	-.069	.460	-.205	.379	-.205	.401	-.290	.385	-.533	.352	-.463	.354	-.359	.378
ONLY INVOL#1	-.298	.397	-.070	.453	-.306	.388	-.319	.392	-.196	.411	-.147	.392	-.175	.412	-.304	.389	-.379	.399	-.361	.397
MIXED#0	.036	.153	.218	.186	.098	.133	.471	.325	.317	.174	.332	.225	.259	.186	.005	.083	.079	.088	.163	.144
MIXED#1	.096	.094	.309	.274	.050	.095	.086	.122	.173	.170	.210	.135	.186	.173	.070	.076	-.002	.069	-.001	.098
part_ct_2cat#part_type																				
0#ONLY VOL	.430	.377	.448	.388	.420	.385	.418	.388	.418	.385	.442	.386	.412	.381	.450	.380	.411	.378	.412	.385
0#ONLY INVOL	.441	.384	.512	.386	.454	.381	.482	.385	.475	.383	.471	.384	.454	.383	.413	.383	.401	.383	.459	.384
0#MIXED	-.100	.119	-.059	.137	-.097	.117	-.087	.124	-.080	.127	-.076	.119	-.095	.115	-.101	.101	-.128	.100	-.111	.124
1#CONTROL	-.221	.170	-.139	.179	-.179	.117	-.094	.153	-.077	.146	.026	.087	-.040	.091	.023	.058	-.063	.057	-.221	.156
Has Job License?	.582	.620	.473	.595	.417	.495	.447	.572	.361	.623	.357	.553	.341	.528	.654	.612	.547	.588	.553	.568
Has GED?	.593	.334	.793	.359	.663	.292	.809	.356	.959	.327	.893	.323	.801	.331	.755	.338	.568	.319	.588	.354
Has High School Degree?	.119	.173	.072	.178	.094	.143	.031	.175	.135	.156	.212	.141	.121	.157	.202	.166	.184	.155	.089	.173
Has College Degree?	-.309	.272	-.367	.374	-.452	.266	-.796	.429	-.515	.344	-.458	.283	-.343	.278	-.383	.312	-.233	.243	-.486	.262
Social Rewards from Crime	-.015	.038	-.020	.042	.027	.034	.027	.036	.021	.039	.058	.038	.031	.034	.017	.037	.007	.036	.041	.041
Personal Rewards from Crime	-.015	.023	-.003	.028	-.012	.023	.020	.027	.016	.025	-.009	.024	-.005	.024	-.023	.016	-.035	.018	-.007	.025
AWFL Opportunities	-.093	.090	.054	.128	-.008	.083	.088	.116	.043	.103	.053	.112	.066	.115	.002	.083	-.071	.087	.022	.097
AWFL Expectations	.085	.079	.083	.090	.109	.070	.132	.101	.056	.078	.061	.077	.061	.075	.018	.062	.056	.065	.097	.088
FOI	.053	.067	.179	.106	.130	.088	.182	.119	.062	.084	.132	.102	.097	.103	.082	.059	.049	.055	.230	.102
MtS	-.124	.075	-.209	.090	-.154	.071	-.212	.102	-.215	.088	-.183	.087	-.190	.088	-.102	.066	-.119	.072	-.222	.090
Age	.103	.053	.159	.082	.089	.041	.106	.042	.147	.061	.099	.046	.116	.054	.057	.032	.056	.034	.084	.040
Employment Rate	.001	.002	-.001	.002	.001	.002	-.001	.002	-.001	.002	-.001	.002	.000	.002	.000	.002	.000	.002	.001	.002
Illegal Work Rate	.016	.008	.017	.008	.017	.007	.017	.008	.016	.008	.014	.007	.015	.007	.014	.008	.016	.008	.018	.008
Illegal Work Variety	.386	.355	.220	.389	.316	.321	.223	.413	.344	.349	.326	.367	.310	.356	.521	.378	.326	.380	.268	.376
Logged Average Leg. Wage Rate	-.121	.078	-.178	.082	-.124	.065	-.142	.079	-.153	.079	-.124	.074	-.155	.073	-.100	.068	-.116	.068	-.145	.077
Number of Work Interruptions	-.052	.051	-.039	.054	-.028	.046	-.070	.055	-.055	.052	-.018	.043	-.003	.042	-.019	.037	-.017	.037	-.030	.057
Number of Months in Recall	-.045	.023	-.042	.029	-.030	.021	-.035	.028	-.056	.027	-.049	.025	-.054	.027	-.052	.022	-.051	.022	-.025	.025
Interviewed in Secure Facility?	1.223	.272	1.187	.304	1.202	.232	1.112	.321	1.176	.312	1.064	.287	.944	.284	1.078	.236	.997	.239	1.201	.271
RPI	-.029	.106	-.021	.123	-.026	.102	-.044	.125	-.049	.116	.022	.112	-.014	.114	.044	.098	.067	.094	-.061	.121
Prop. Friends Ever Arrested	-.142	.209	-.558	.343	-.318	.195	-.351	.299	-.498	.286	-.509	.304	-.470	.293	-.066	.189	-.188	.203	-.288	.221
Prop. Friends Ever Jailed	.037	.286	.479	.357	.349	.262	.332	.329	.501	.309	.630	.305	.607	.298	.085	.220	.244	.243	.321	.301
Current Gang Member?	.089	.620	-.279	.834	-.483	.618	.134	.782	-.016	.737	-.099	.819	-.275	.851	-.297	.646	-.158	.643	-.370	.726
Contact with Gang?	.100	.142	.188	.162	.143	.129	.151	.172	.154	.160	.045	.117	.052	.117	.050	.107	-.017	.113	.170	.164
BSI Factor	.043	.072	.054	.097	-.001	.076	.049	.096	.062	.094	.062	.093	.055	.091	.098	.055	.092	.061	.016	.085
Moral Disengagement	-.005	.014	.018	.019	.006	.014	.019	.019	.014	.017	.021	.019	.011	.018	-.003	.015	-.006	.014	.010	.017
PSMI	-.243	.141	-.204	.149	-.194	.122	-.262	.157	-.156	.145	-.180	.146	-.144	.149	-.195	.133	-.165	.133	-.219	.144
WAI Factor	-.013	.059	.020	.073	.057	.064	.078	.084	.037	.065	.066	.072	.046	.073	.001	.053	-.028	.053	.084	.076
Consideration of Others	-.021	.068	-.057	.097	-.076	.066	.002	.116	.053	.092	-.001	.108	-.018	.102	-.073	.066	-.085	.066	-.080	.083
YPI Factor	.084	.059	.102	.096	.138	.068	.104	.083	.088	.081	.076	.074	.075	.078	.014	.039	.034	.042	.132	.076
Logged # of Caring Adults	.143	.112	.062	.116	.080	.098	.086	.111	.112	.103	.078	.099	.051	.092	.073	.101	.075	.100	.064	.110
Domains of Social Support	-.054	.048	-.029	.053	-.023	.042	-.037	.048	-.042	.046	-.018	.038	-.004	.036	-.017	.035	-.015	.034	-.019	.047
Diversity of Social Support	-.013	.033	.010	.043	.005	.036	.014	.046	-.023	.036	.006	.041	.013	.041	.003	.034	.006	.036	.002	.044
Total Substance Use	.008	.015	-.037	.024	-.014	.018	-.034	.026	-.015	.019	-.034	.024	-.031	.022	.000	.016	-.002	.015	-.020	.021
Substance Use Variety	-.039	.084	-.036	.132	.041	.084	-.017	.127	-.073	.101	.017	.113	.004	.113	-.014	.075	.002	.074	.050	.098
Age at First Offense	.059	.028	.109	.042	.061	.026	.087	.039	.083	.036	.077	.031	.071	.033	.022	.014	.034	.016	.070	.028
White	.469	.761	.009	.658	.233	.506	.086	.596	.058	.609	.075	.605	.168	.671	.393	.639	.374	.621	.312	.598
Black	.409	.843	.104	.698	.144	.586	.209	.656	.171	.668	.186	.661	.314	.726	.373	.727	.364	.717	.206	.687
Hispanic	.181	.784	-.092	.634	-.092	.533	.008	.593	.016	.598	.037	.584	.170	.661	.314	.656	.204	.636	-.011	.626
Male	-.161	.103	-.227	.117	-.172	.100	-.174	.117	-.201	.113	-.228	.113	-.178	.106	-.180	.106	-.143	.107	-.164	.112
Site (Phoenix=1)	-.200	.156	-.380	.207	-.225	.153	-.289	.186	-.303	.188	-.261	.189	-.221	.175	-.099	.150	-.062	.163	-.236	.175
Intercept	-.535	.991	-.125	.942	-.215	.721	-.438	.838	-.034	.851	-.221	.851	-.197	.865	-.268	.868	-.449	.907	-.409	.913

Table A-7: Coefficients for All Variables Across All Control Groups – Offending Rates  
(Excl. Drugs)

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
1.treat	.006	.046	.042	.063	.026	.041	-.033	.050	.004	.047	-.049	.056	-.016	.061	.032	.036	.056	.036	-.015	.049
part_type#treat																				
ONLY VOL#0	-.297	.274	-.261	.285	-.284	.289	-.325	.284	-.284	.285	-.303	.291	-.293	.284	-.320	.286	-.281	.279	-.312	.286
ONLY VOL#1	-.402	.289	-.411	.294	-.417	.288	-.420	.295	-.406	.290	-.380	.291	-.382	.288	-.448	.292	-.416	.291	-.423	.294
ONLY INVOL#0	.025	.068	.072	.074	.038	.063	-.009	.057	.042	.059	.037	.060	.029	.060	.010	.063	.032	.064	.004	.065
ONLY INVOL#1	.028	.059	.030	.057	.015	.051	.017	.053	.030	.056	.069	.066	.055	.066	-.010	.052	.008	.056	.013	.055
MIXED#0	.070	.060	.107	.065	.078	.052	.028	.044	.082	.048	.065	.051	.073	.051	.049	.052	.075	.053	.043	.055
MIXED#1	.039	.051	.035	.058	.015	.047	.021	.051	.028	.053	.061	.064	.051	.060	-.004	.045	.020	.049	.016	.050
part_ct_2cat#part_type																				
0#ONLY VOL	.334	.271	.348	.274	.342	.275	.338	.276	.339	.274	.353	.275	.340	.272	.356	.273	.331	.272	.336	.275
0#ONLY INVOL	.011	.058	.026	.058	.019	.055	.014	.056	.017	.057	.023	.058	.014	.057	.007	.057	.003	.057	.021	.057
0#MIXED	-.065	.062	-.056	.064	-.055	.059	-.057	.059	-.054	.060	-.056	.064	-.060	.061	-.057	.060	-.070	.059	-.053	.061
1#CONTROL	-.020	.029	-.021	.033	-.010	.026	.003	.027	.011	.031	.024	.029	-.010	.022	.027	.028	.014	.022	-.006	.036
Has Job License?	.038	.079	.059	.076	.025	.074	-.007	.071	.019	.089	.019	.083	-.004	.073	.024	.090	.016	.081	.020	.084
Has GED?	.225	.159	.327	.167	.286	.142	.307	.160	.307	.160	.305	.166	.313	.161	.324	.168	.221	.157	.255	.169
Has High School Degree?	.100	.041	.088	.043	.083	.035	.085	.041	.091	.041	.102	.042	.104	.040	.105	.046	.092	.038	.085	.042
Has College Degree?	-.200	.195	-.055	.075	-.172	.117	-.091	.066	-.083	.060	-.179	.126	.023	.055	-.182	.136	-.171	.165	-.237	.181
Social Rewards from Crime	.027	.015	.028	.016	.037	.015	.045	.017	.034	.016	.047	.019	.027	.015	.035	.016	.027	.014	.050	.018
Personal Rewards from Crime	-.014	.014	-.017	.015	-.014	.012	-.007	.009	-.001	.009	-.020	.015	-.014	.015	-.006	.008	-.016	.014	-.006	.010
AWFL Opportunities	-.093	.051	-.067	.053	-.051	.044	-.023	.041	-.031	.039	-.067	.061	-.073	.059	-.024	.042	-.108	.052	-.049	.041
AWFL Expectations	.004	.026	.017	.030	.023	.025	.030	.033	.000	.025	.019	.030	.022	.028	-.008	.025	.015	.025	.017	.030
FOI	.041	.041	.088	.051	.065	.042	.085	.051	.062	.041	.053	.053	.049	.051	.078	.040	.052	.042	.119	.047
MtS	-.027	.036	-.069	.043	-.058	.036	-.058	.037	-.073	.036	-.046	.043	-.063	.046	-.044	.032	-.042	.037	-.063	.037
Age	.018	.012	.020	.014	.019	.011	.026	.010	.032	.012	.030	.013	.014	.012	.012	.010	.009	.011	.030	.011
Employment Rate	.000	.001	.000	.001	.000	.000	-.001	.001	.000	.001	-.001	.001	.000	.001	.000	.001	.000	.001	.000	.001
Illegal Work Rate	.001	.002	.000	.002	.001	.002	.001	.002	.001	.002	.001	.002	.001	.002	.000	.002	.001	.002	.002	.002
Illegal Work Variety	.294	.132	.298	.145	.243	.122	.210	.152	.274	.129	.240	.135	.262	.133	.344	.146	.270	.135	.232	.138
Logged Average Leg. Wage Rate	-.020	.027	-.045	.023	-.027	.022	-.027	.026	-.032	.025	-.025	.026	-.047	.021	-.021	.026	-.040	.022	-.030	.026
Number of Work Interruptions	-.015	.020	-.005	.020	-.004	.018	-.014	.020	-.013	.020	.002	.021	.002	.019	-.016	.021	-.007	.019	-.014	.022
Number of Months in Recall	-.006	.008	-.003	.009	-.005	.008	.000	.009	-.009	.008	-.008	.009	-.002	.009	-.005	.009	-.005	.008	-.002	.009
Interviewed in Secure Facility?	.342	.093	.385	.098	.356	.081	.375	.098	.394	.093	.378	.095	.358	.094	.360	.091	.303	.092	.401	.094
RPI	-.018	.034	.010	.037	-.003	.030	-.002	.036	-.010	.034	.011	.033	.003	.034	.001	.033	.010	.033	-.020	.036
Prop. Friends Ever Arrested	-.056	.078	-.140	.104	-.119	.086	.031	.049	-.107	.075	-.153	.101	-.120	.110	.000	.054	-.034	.077	.030	.057
Prop. Friends Ever Jailed	.011	.061	.099	.092	.099	.079	-.009	.056	.101	.076	.127	.086	.135	.098	-.028	.043	-.003	.059	.000	.056
Current Gang Member?	.029	.358	-.231	.429	-.228	.359	.034	.384	-.153	.395	-.200	.445	-.309	.490	.040	.390	.009	.378	.069	.406
Contact with Gang?	.057	.043	.108	.051	.084	.041	.061	.052	.084	.047	.076	.050	.066	.049	.063	.048	.051	.044	.072	.052
BSI Factor	.012	.037	.017	.038	.009	.033	.022	.031	.030	.031	.003	.040	.009	.039	.037	.030	.019	.037	.024	.032
Moral Disengagement	-.004	.005	-.001	.006	-.001	.005	.003	.006	-.002	.005	.000	.006	-.002	.005	-.003	.006	-.003	.005	.000	.006
PSMI	-.058	.047	-.050	.052	-.047	.044	-.072	.053	-.037	.049	-.059	.053	-.043	.054	-.071	.049	-.034	.049	-.061	.052
WAI Factor	-.041	.030	-.025	.030	-.012	.028	.004	.033	-.016	.026	-.010	.032	-.014	.032	-.030	.025	-.046	.028	-.004	.029
Consideration of Others	-.014	.041	-.046	.044	-.035	.037	-.021	.046	-.010	.037	-.034	.044	-.036	.042	-.026	.042	-.036	.041	-.029	.045
YPI Factor	-.004	.018	.010	.022	.014	.019	.008	.021	-.002	.020	.010	.023	.013	.023	-.025	.018	-.006	.019	.008	.022
Logged # of Caring Adults	.037	.035	.020	.037	.021	.032	.026	.034	.025	.035	.018	.038	.035	.032	.025	.038	.028	.034	.019	.037
Domains of Social Support	-.009	.010	.001	.011	-.004	.010	-.004	.010	-.002	.011	.002	.011	-.002	.009	-.001	.010	-.006	.009	.001	.011
Diversity of Social Support	.007	.012	.016	.015	.019	.013	.014	.017	.012	.013	.011	.015	.014	.014	.007	.014	.020	.013	.019	.016
Total Substance Use	-.006	.009	-.020	.011	-.014	.009	-.015	.012	-.010	.009	-.019	.011	-.016	.010	-.009	.010	-.007	.009	-.016	.011
Substance Use Variety	.022	.036	.050	.045	.053	.038	.050	.048	.031	.038	.068	.044	.048	.042	.028	.039	.019	.038	.056	.046
Age at First Offense	.027	.010	.031	.011	.026	.009	.022	.009	.024	.009	.029	.010	.027	.011	.011	.007	.022	.009	.025	.009
White	.406	.492	.282	.384	.242	.327	.270	.361	.273	.367	.283	.363	.321	.412	.338	.416	.336	.403	.311	.386
Black	.426	.543	.278	.441	.228	.377	.313	.419	.289	.427	.282	.423	.346	.467	.362	.471	.359	.463	.293	.441
Hispanic	.343	.496	.189	.387	.157	.331	.220	.366	.227	.371	.189	.369	.251	.421	.334	.420	.269	.407	.237	.389
Male	-.091	.067	-.111	.072	-.098	.065	-.090	.075	-.097	.070	-.132	.071	-.097	.068	-.117	.069	-.091	.071	-.084	.073
Site (Phoenix=1)	-.064	.082	-.086	.096	-.075	.081	-.039	.089	-.071	.094	-.078	.099	-.060	.086	-.032	.086	-.025	.095	-.072	.092
Intercept	-.487	.625	-.404	.542	-.319	.447	-.307	.502	-.299	.515	-.356	.515	-.399	.532	-.344	.547	-.452	.569	-.356	.534

Table A-8: Coefficients for All Variables Across All Control Groups – Property  
Offending Variety Scores

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
1.treat	-.029	.030	-.034	.032	.008	.024	-.027	.033	-.031	.030	.017	.032	-.058	.032	-.010	.030	.018	.028	.011	.029
part_type#treat																				
ONLY VOL#0	-.034	.040	-.059	.041	-.034	.036	-.055	.041	-.047	.040	-.043	.039	-.061	.041	-.038	.040	.009	.038	-.038	.041
ONLY VOL#1	.016	.037	-.013	.037	-.020	.035	-.001	.037	.011	.037	-.027	.037	-.006	.037	.008	.036	.011	.036	-.018	.037
ONLY INVOL#0	.104	.051	.074	.052	.105	.048	.087	.051	.084	.051	.092	.050	.074	.052	.106	.052	.146	.049	.097	.052
ONLY INVOL#1	-.057	.037	-.084	.037	-.090	.035	-.077	.037	-.059	.037	-.100	.038	-.072	.038	-.064	.035	-.059	.036	-.091	.036
MIXED#0	.077	.069	.051	.068	.081	.065	.060	.069	.064	.067	.069	.067	.048	.069	.081	.068	.121	.066	.076	.069
MIXED#1	-.051	.092	-.089	.091	-.086	.092	-.068	.092	-.051	.091	-.091	.092	-.075	.092	-.055	.091	-.052	.091	-.082	.092
part_ct_2cat#part_type																				
0#ONLY VOL	.016	.028	.013	.028	.018	.028	.016	.027	.016	.028	.013	.028	.015	.028	.022	.027	.020	.028	.016	.028
0#ONLY INVOL	.027	.028	.024	.028	.026	.028	.029	.028	.025	.028	.026	.028	.026	.028	.028	.028	.030	.028	.027	.028
0#MIXED	.082	.113	.090	.114	.083	.115	.084	.114	.079	.113	.076	.113	.084	.114	.079	.115	.092	.115	.084	.114
1#CONTROL	.022	.034	-.023	.034	-.007	.027	.005	.031	.018	.033	-.021	.032	-.008	.034	.018	.033	.039	.030	.003	.034
Has Job License?	-.058	.085	.012	.086	-.001	.077	-.056	.085	-.038	.085	-.033	.079	-.019	.080	-.035	.091	-.059	.091	-.014	.087
Has GED?	-.113	.082	-.116	.081	-.152	.070	-.094	.077	-.174	.081	-.164	.078	-.140	.081	-.132	.081	-.123	.082	-.193	.084
Has High School Degree?	.019	.056	-.029	.055	.012	.051	.022	.057	.009	.058	-.017	.054	-.011	.053	-.014	.052	-.021	.055	-.020	.056
Has College Degree?	.037	.123	.130	.163	.100	.097	.269	.111	.222	.099	.104	.107	.161	.091	.152	.117	.054	.123	.077	.144
Social Rewards from Crime	.022	.017	.019	.017	.015	.016	.022	.017	.020	.017	.014	.016	.024	.018	.016	.017	.022	.018	.025	.017
Personal Rewards from Crime	.022	.010	.026	.010	.025	.010	.020	.010	.024	.011	.024	.011	.027	.011	.015	.010	.023	.011	.026	.011
AWFL Opportunities	-.059	.045	-.075	.045	-.072	.041	-.087	.049	-.060	.045	-.051	.046	-.082	.049	-.071	.044	-.088	.048	-.098	.047
AWFL Expectations	-.072	.023	-.072	.024	-.061	.021	-.054	.024	-.051	.022	-.070	.023	-.079	.023	-.060	.022	-.072	.023	-.059	.024
FOI	.004	.037	.015	.037	-.002	.034	.009	.035	-.004	.037	-.004	.036	.008	.037	.025	.036	.032	.037	.018	.038
MIS	-.059	.037	-.046	.038	-.063	.032	-.077	.035	-.088	.037	-.058	.036	-.075	.037	-.066	.035	-.028	.032	-.071	.036
Age	-.042	.012	-.042	.012	-.042	.011	-.045	.013	-.048	.013	-.047	.013	-.033	.013	-.051	.012	-.046	.013	-.044	.013
Employment Rate	.000	.001	.000	.001	.000	.000	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001
Illegal Work Rate	.002	.002	.001	.002	.002	.002	.001	.002	.002	.002	.003	.002	.003	.002	.001	.002	.002	.002	.001	.002
Illegal Work Variety	1.214	.098	1.230	.096	1.183	.087	1.281	.088	1.182	.097	1.158	.095	1.141	.093	1.283	.085	1.182	.092	1.265	.093
Logged Average Leg. Wage Rate	.006	.022	.007	.022	.006	.019	.014	.022	.001	.022	.018	.021	.000	.021	-.001	.022	-.017	.021	.002	.022
Number of Work Interruptions	.016	.019	.003	.019	.013	.017	.018	.019	.024	.019	.014	.019	.026	.020	.026	.019	.035	.019	.024	.020
Number of Months in Recal	.018	.007	.022	.007	.018	.006	.018	.007	.023	.007	.021	.007	.019	.007	.020	.006	.025	.007	.022	.007
Interviewed in Secure Facility?	.109	.063	.064	.066	.101	.056	.126	.064	.064	.063	.125	.062	.093	.064	.099	.062	.096	.062	.105	.063
RPI	-.039	.036	-.044	.035	-.046	.031	-.078	.037	-.048	.036	-.079	.035	-.050	.035	-.052	.035	-.040	.036	-.076	.037
Prop. Friends Ever Arrested	-.014	.074	.006	.073	-.036	.065	.008	.072	.022	.071	-.026	.073	-.020	.070	.034	.070	.007	.072	.011	.072
Prop. Friends Ever Jailed	.056	.077	.084	.075	.100	.068	.014	.077	.062	.073	.080	.076	.075	.075	.054	.074	.067	.076	.037	.077
Current Gang Member?	-.337	.211	-.302	.204	-.246	.186	-.224	.187	-.261	.201	-.294	.218	-.325	.201	-.399	.203	-.240	.208	-.258	.215
Contact with Gang?	.101	.041	.074	.041	.066	.036	.068	.039	.089	.040	.096	.042	.079	.040	.089	.042	.079	.040	.079	.043
BSI Factor	.034	.026	.026	.026	.040	.024	.033	.026	.028	.026	.025	.026	.019	.025	.021	.025	.032	.026	.026	.026
Moral Disengagement	.012	.007	.013	.006	.011	.006	.005	.007	.014	.006	.011	.007	.007	.007	.010	.006	.014	.007	.011	.007
PSMI	.027	.045	.085	.045	.065	.041	.054	.046	.068	.045	.082	.045	.074	.048	.032	.046	.042	.046	.078	.048
WAI Factor	-.051	.023	-.056	.022	-.045	.020	-.065	.023	-.054	.022	-.070	.022	-.053	.023	-.053	.022	-.053	.023	-.062	.023
Consideration of Others	-.061	.022	-.032	.023	-.038	.020	-.042	.022	-.051	.022	-.034	.022	-.039	.023	-.044	.021	-.052	.023	-.041	.023
YPI Factor	.031	.022	.045	.023	.041	.020	.038	.023	.039	.022	.036	.022	.049	.023	.027	.022	.049	.023	.037	.024
Logged # of Caring Adults	-.045	.028	-.044	.029	-.040	.026	-.056	.029	-.025	.029	-.046	.029	-.054	.028	-.080	.029	-.068	.029	-.032	.029
Domains of Social Support	.013	.012	.019	.011	.014	.010	.017	.012	.010	.012	.018	.012	.024	.011	.030	.011	.022	.011	.009	.012
Diversity of Social Support	.022	.015	.016	.015	.023	.014	.023	.016	.012	.015	.012	.015	.018	.016	.021	.016	.031	.017	.029	.016
Total Substance Use	.032	.008	.033	.008	.033	.007	.034	.008	.033	.008	.034	.008	.032	.008	.034	.008	.029	.007	.035	.008
Substance Use Variety	.127	.041	.159	.041	.131	.037	.121	.040	.128	.040	.122	.039	.133	.040	.122	.040	.155	.040	.117	.041
Age at First Offense	-.009	.003	-.011	.003	-.012	.003	-.011	.003	-.011	.003	-.011	.003	-.011	.003	-.011	.003	-.007	.003	-.011	.003
White	-.025	.035	-.031	.029	-.027	.026	-.020	.028	-.014	.027	-.023	.025	-.037	.029	-.054	.029	-.035	.031	-.024	.031
Black	-.028	.033	-.023	.029	-.021	.026	-.025	.026	-.006	.028	-.018	.026	-.031	.028	-.039	.030	-.021	.031	-.013	.031
Hispanic	-.019	.035	-.016	.029	-.011	.026	-.009	.027	.000	.027	-.009	.025	-.026	.029	-.033	.030	-.015	.031	-.014	.031
Male	.022	.013	.023	.012	.020	.011	.021	.013	.021	.012	.025	.012	.018	.012	.015	.012	.019	.013	.026	.013
Site (Phoenix=1)	.031	.016	.033	.017	.029	.015	.023	.015	.032	.018	.034	.018	.035	.015	.024	.017	.023	.016	.034	.018
Intercept	.076	.066	.127	.064	.106	.054	.124	.061	.085	.063	.102	.061	.141	.064	.134	.060	.015	.064	.085	.064

Table A-9: Coefficients for All Variables Across All Control Groups – Violent Offending

## Variety Scores

Variable	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6		Group 7		Group 8		Group 9		Group 10	
	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error	Coef.	St. Error
1.treat	-.029	.030	-.034	.032	.008	.024	-.027	.033	-.031	.030	.017	.032	-.058	.032	-.010	.030	.018	.028	.011	.029
part_type#treat																				
ONLY VOL#0	-.034	.040	-.059	.041	-.034	.036	-.055	.041	-.047	.040	-.043	.039	-.061	.041	-.038	.040	.009	.038	-.038	.041
ONLY VOL#1	.016	.037	-.013	.037	-.020	.035	-.001	.037	.011	.037	-.027	.037	-.006	.037	.008	.036	.011	.036	-.018	.037
ONLY INVOL#0	.104	.051	.074	.052	.105	.048	.087	.051	.084	.051	.092	.050	.074	.052	.106	.052	.146	.049	.097	.052
ONLY INVOL#1	-.057	.037	-.084	.037	-.090	.035	-.077	.037	-.059	.037	-.100	.038	-.072	.038	-.064	.035	-.059	.036	-.091	.036
MIXED#0	.077	.069	.051	.068	.081	.065	.060	.069	.064	.067	.069	.067	.048	.069	.081	.068	.121	.066	.076	.069
MIXED#1	-.051	.092	-.089	.091	-.086	.092	-.068	.092	-.051	.091	-.091	.092	-.075	.092	-.055	.091	-.052	.091	-.082	.092
part_ct_2cat#part_type																				
0#ONLY VOL	.016	.028	.013	.028	.018	.028	.016	.027	.016	.028	.013	.028	.015	.028	.022	.027	.020	.028	.016	.028
0#ONLY INVOL	.027	.028	.024	.028	.026	.028	.029	.028	.025	.028	.026	.028	.026	.028	.028	.028	.030	.028	.027	.028
0#MIXED	.082	.113	.090	.114	.083	.115	.084	.113	.079	.113	.076	.113	.084	.114	.079	.115	.092	.115	.084	.114
1#CONTROL	.022	.034	-.023	.034	-.007	.027	.005	.031	.018	.033	-.021	.032	-.008	.034	.018	.033	.039	.030	.003	.034
Has Job License?	-.058	.085	.012	.086	-.001	.077	-.056	.085	-.038	.085	-.033	.079	-.019	.080	-.035	.091	-.059	.091	-.014	.087
Has GED?	-.113	.082	-.116	.081	-.152	.070	-.094	.077	-.174	.081	-.164	.078	-.140	.081	-.132	.081	-.123	.082	-.193	.084
Has High School Degree?	.019	.056	-.029	.055	.012	.051	.022	.057	.009	.058	-.017	.054	-.011	.053	-.014	.052	-.021	.055	-.020	.056
Has College Degree?	.037	.123	.130	.163	.100	.097	.269	.111	.222	.099	.104	.107	.161	.091	.152	.117	.054	.123	.077	.144
Social Rewards from Crime	.022	.017	.019	.017	.015	.016	.022	.017	.020	.017	.014	.016	.024	.018	.016	.017	.022	.018	.025	.017
Personal Rewards from Crime	.022	.010	.026	.010	.025	.010	.020	.010	.024	.011	.024	.011	.027	.011	.015	.010	.023	.011	.026	.011
AWFL Opportunities	-.059	.045	-.075	.045	-.072	.041	-.087	.049	-.060	.045	-.051	.046	-.082	.049	-.071	.044	-.088	.048	-.098	.047
AWFL Expectations	-.072	.023	-.072	.024	-.061	.021	-.054	.024	-.051	.022	-.070	.023	-.079	.023	-.060	.022	-.072	.023	-.059	.024
FOI	.004	.037	.015	.037	-.002	.034	.009	.035	-.004	.037	-.004	.036	.008	.037	.025	.036	.032	.037	.018	.038
MtS	-.059	.037	-.046	.038	-.063	.032	-.077	.035	-.088	.037	-.058	.036	-.075	.037	-.066	.035	-.028	.032	-.071	.036
Age	-.042	.012	-.042	.012	-.042	.011	-.045	.013	-.048	.013	-.047	.013	-.033	.013	-.051	.012	-.046	.013	-.044	.013
Employment Rate	.000	.001	.000	.001	.000	.000	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001	.000	.001
Illegal Work Rate	.002	.002	.001	.002	.002	.002	.001	.002	.002	.002	.003	.002	.003	.002	.001	.002	.002	.002	.001	.002
Illegal Work Variety	1.214	.098	1.230	.096	1.183	.087	1.281	.088	1.182	.097	1.158	.095	1.141	.093	1.283	.085	1.182	.092	1.265	.093
Logged Average Leg. Wage Rate	.006	.022	.007	.022	.006	.019	.014	.022	.001	.022	.018	.021	.000	.021	-.001	.022	-.017	.021	.002	.022
Number of Work Interruptions	.016	.019	.003	.019	.013	.017	.018	.019	.024	.019	.014	.019	.026	.020	.026	.020	.035	.019	.024	.020
Number of Months in Recall	.018	.007	.022	.007	.018	.006	.018	.007	.023	.007	.021	.007	.019	.007	.020	.006	.025	.007	.022	.007
Interviewed in Secure Facility?	.109	.063	.064	.066	.101	.056	.126	.064	.064	.063	.125	.062	.093	.064	.099	.062	.096	.062	.105	.063
RPI	-.039	.036	-.044	.035	-.046	.031	-.078	.037	-.048	.036	-.079	.035	-.050	.035	-.052	.035	-.040	.036	-.076	.037
Prop. Friends Ever Arrested	-.014	.074	.006	.073	-.036	.065	.008	.072	.022	.071	-.026	.073	-.020	.070	.034	.070	.007	.072	.011	.072
Prop. Friends Ever Jailed	.056	.077	.084	.075	.100	.068	.014	.077	.062	.073	.080	.076	.075	.075	.054	.074	.067	.076	.037	.077
Current Gang Member?	-.337	.211	-.302	.204	-.246	.186	-.224	.187	-.261	.201	-.294	.218	-.325	.201	-.399	.203	-.240	.208	-.258	.215
Contact with Gang?	.101	.041	.074	.041	.066	.036	.068	.039	.089	.040	.096	.042	.079	.040	.089	.042	.079	.040	.079	.043
BSI Factor	.034	.026	.026	.026	.040	.024	.033	.026	.028	.026	.025	.026	.019	.025	.021	.025	.032	.026	.026	.026
Moral Disengagement	.012	.007	.013	.006	.011	.006	.005	.007	.014	.006	.011	.007	.007	.007	.010	.006	.014	.007	.011	.007
PSMI	.027	.045	.085	.045	.065	.041	.054	.046	.068	.045	.082	.045	.074	.048	.032	.046	.042	.046	.078	.048
WAI Factor	-.051	.023	-.056	.022	-.045	.020	-.065	.023	-.054	.022	-.070	.022	-.053	.023	-.053	.022	-.053	.023	-.062	.023
Consideration of Others	-.061	.022	-.032	.023	-.038	.020	-.042	.022	-.051	.022	-.034	.022	-.039	.023	-.044	.021	-.052	.023	-.041	.023
YPI Factor	.031	.022	.045	.023	.041	.020	.038	.023	.039	.022	.036	.022	.049	.023	.027	.022	.049	.023	.037	.024
Number of Caring Adults	-.045	.028	-.044	.029	-.040	.026	-.056	.029	-.025	.029	-.046	.029	-.054	.028	-.080	.029	-.068	.029	-.032	.029
Domains of Social Support	.013	.012	.019	.011	.014	.010	.017	.012	.010	.012	.018	.012	.024	.011	.030	.011	.022	.011	.009	.012
Diversity of Social Support	.022	.015	.016	.015	.023	.014	.023	.016	.012	.015	.012	.015	.018	.016	.021	.016	.031	.017	.029	.016
Total Substance Use	.032	.008	.033	.008	.033	.007	.034	.008	.033	.008	.034	.008	.032	.008	.034	.008	.029	.007	.035	.008
Substance Use Variety	.127	.041	.159	.041	.131	.037	.121	.040	.128	.040	.122	.039	.133	.040	.122	.040	.155	.040	.117	.041
Age at First Offense	-.009	.003	-.011	.003	-.012	.003	-.011	.003	-.011	.003	-.011	.003	-.011	.003	-.011	.003	-.007	.003	-.011	.003
White	-.025	.035	-.031	.029	-.027	.026	-.020	.028	-.014	.027	-.023	.025	-.037	.029	-.054	.029	-.035	.031	-.024	.031
Black	-.028	.033	-.023	.029	-.021	.026	-.025	.026	-.006	.028	-.018	.026	-.031	.028	-.039	.030	-.021	.031	-.013	.031
Hispanic	-.019	.035	-.016	.029	-.011	.026	-.009	.027	.000	.027	-.009	.025	-.026	.029	-.033	.030	-.015	.031	-.014	.031
Male	.022	.013	.023	.012	.020	.011	.021	.013	.021	.012	.025	.012	.018	.012	.015	.012	.019	.013	.026	.013
Site (Phoenix=1)	.031	.016	.033	.017	.029	.015	.023	.015	.032	.018	.034	.018	.035	.015	.024	.017	.023	.016	.034	.018
Intercept	.076	.066	.127	.064	.106	.054	.124	.061	.085	.063	.102	.061	.141	.064	.134	.060	.015	.064	.085	.064

Figure A-1: Future Outlook Inventory (FOI) Scores Across Control Groups

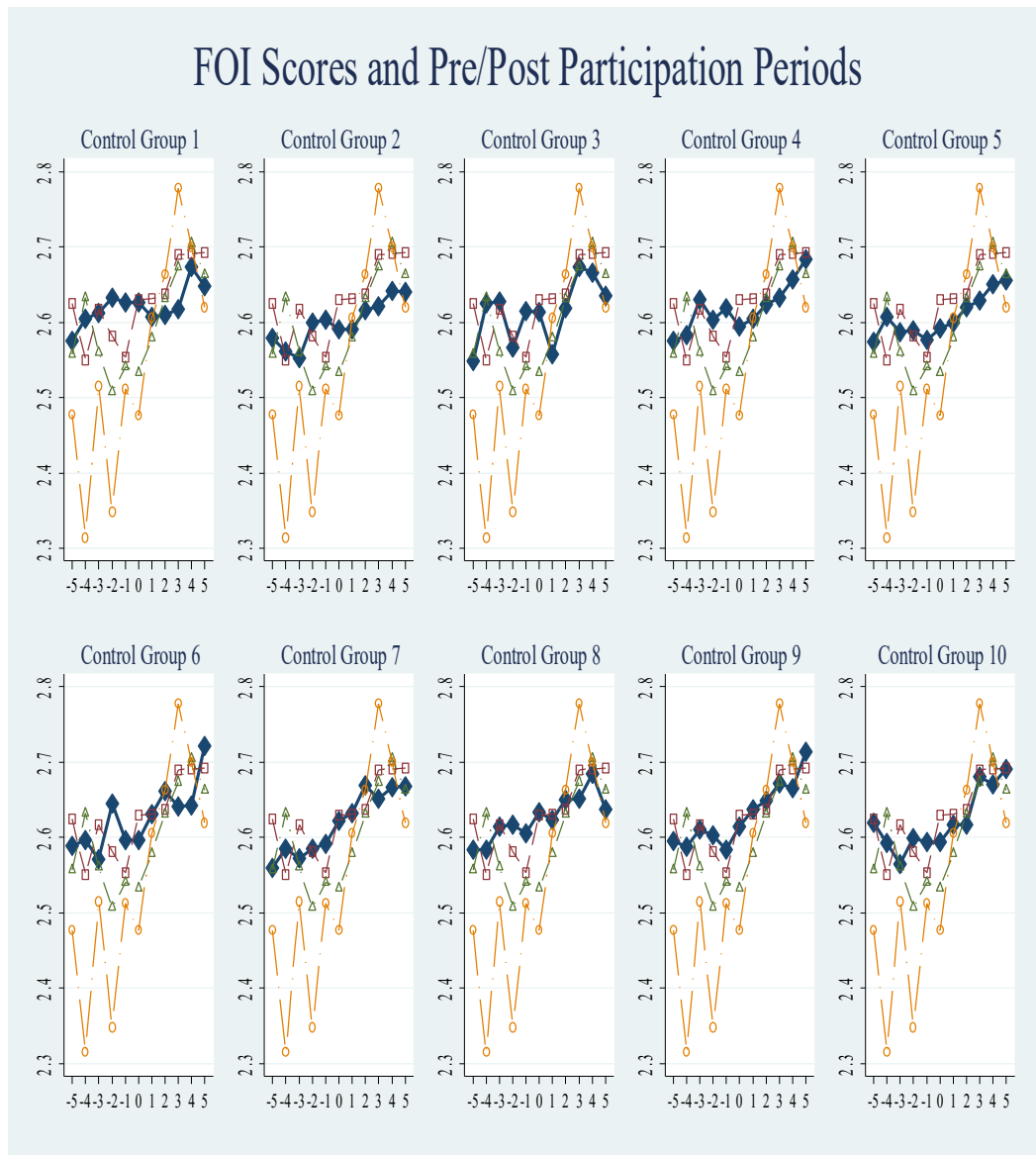


Figure A-2: Aspirations for Work, Family, and Law (AWFL) Expectation  
Scores Across Control Groups

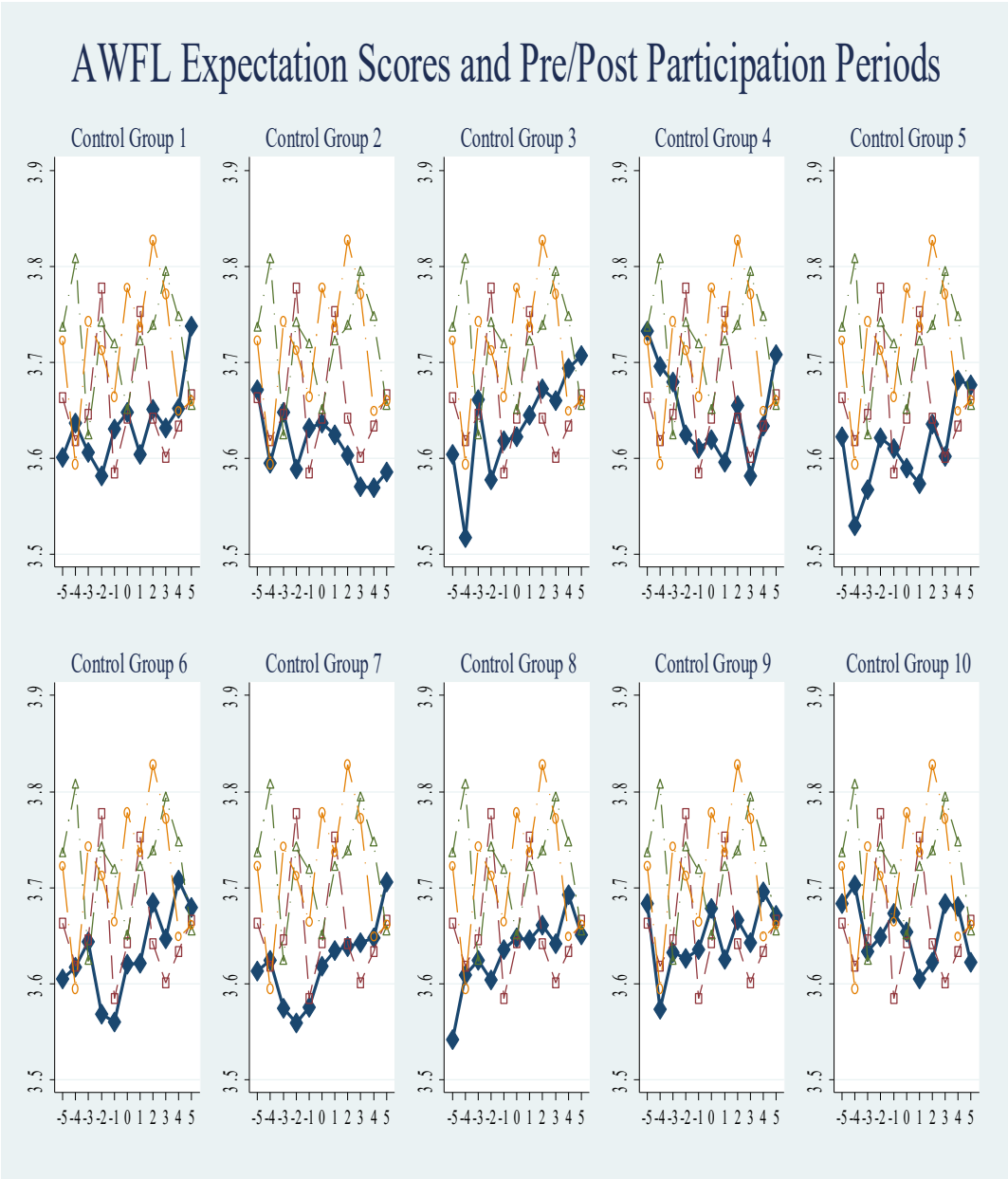




Figure A-3: Aspirations for Work, Family, and Law (AWFL) Opportunity

### Scores Across All Control Groups

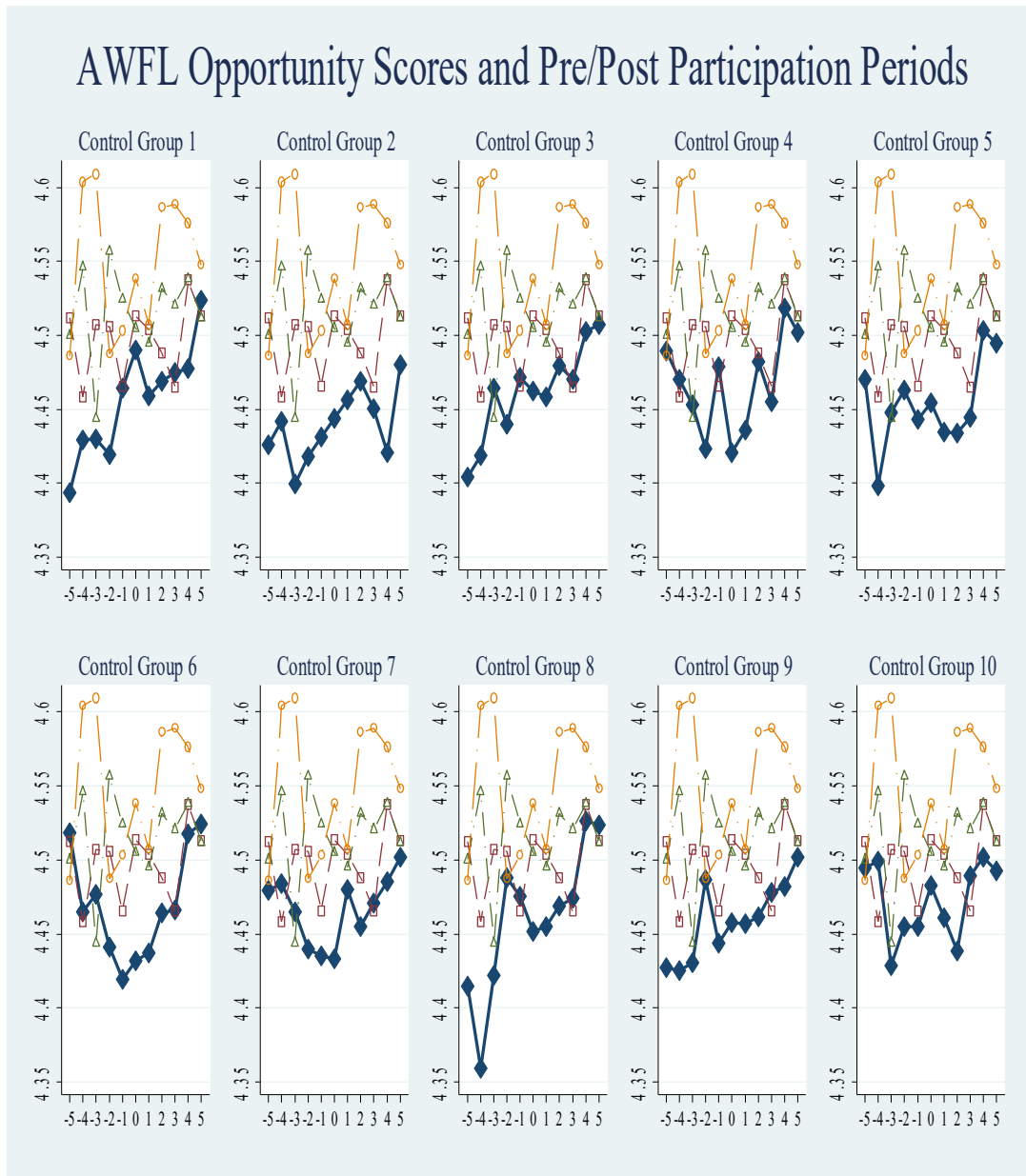


Figure A-4: Motivation for Success (MtS) Scores Across All Control Groups

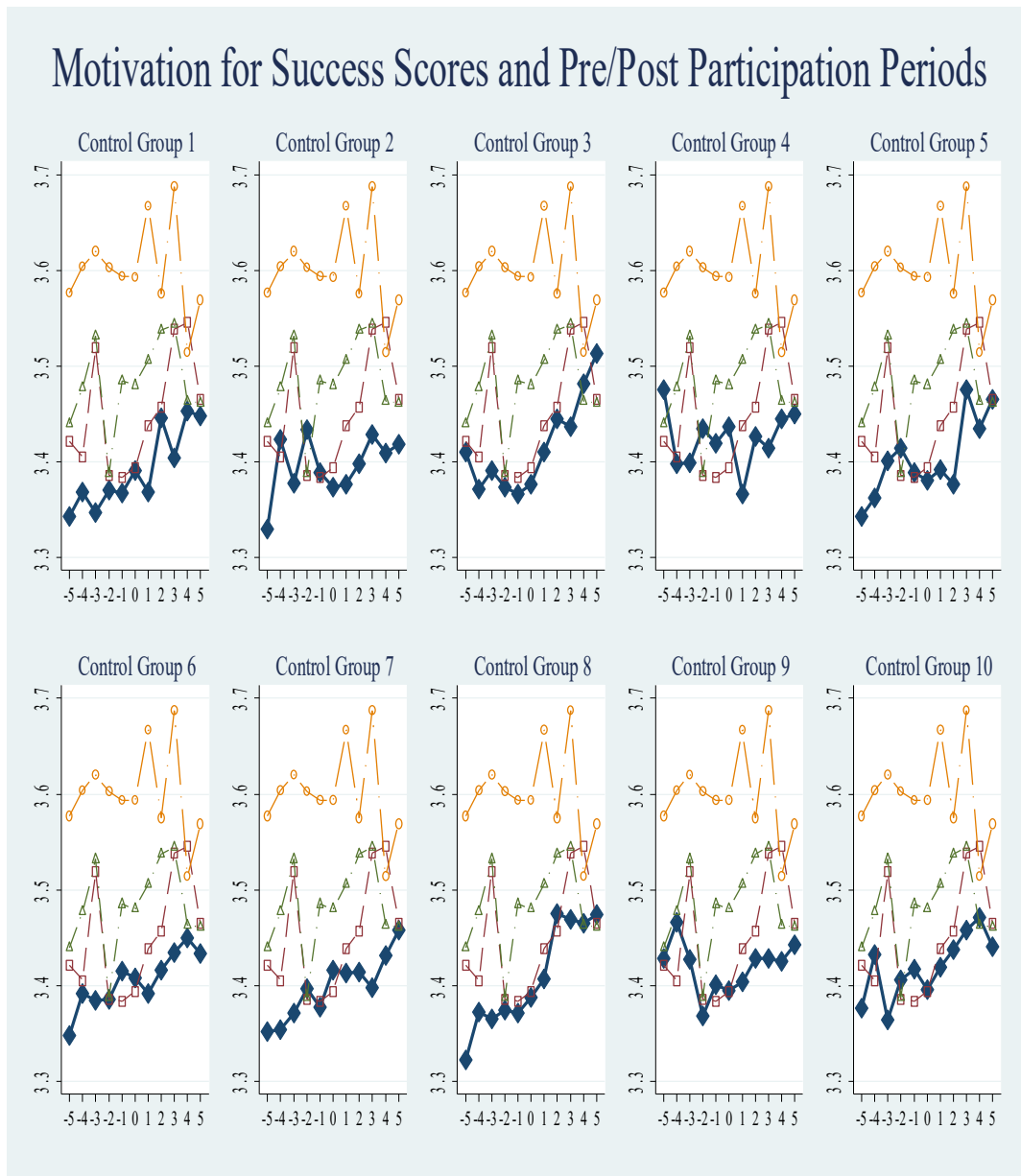


Figure A-5: Future Outlook Inventory (FOI) Scores – Just Control Group

Trends

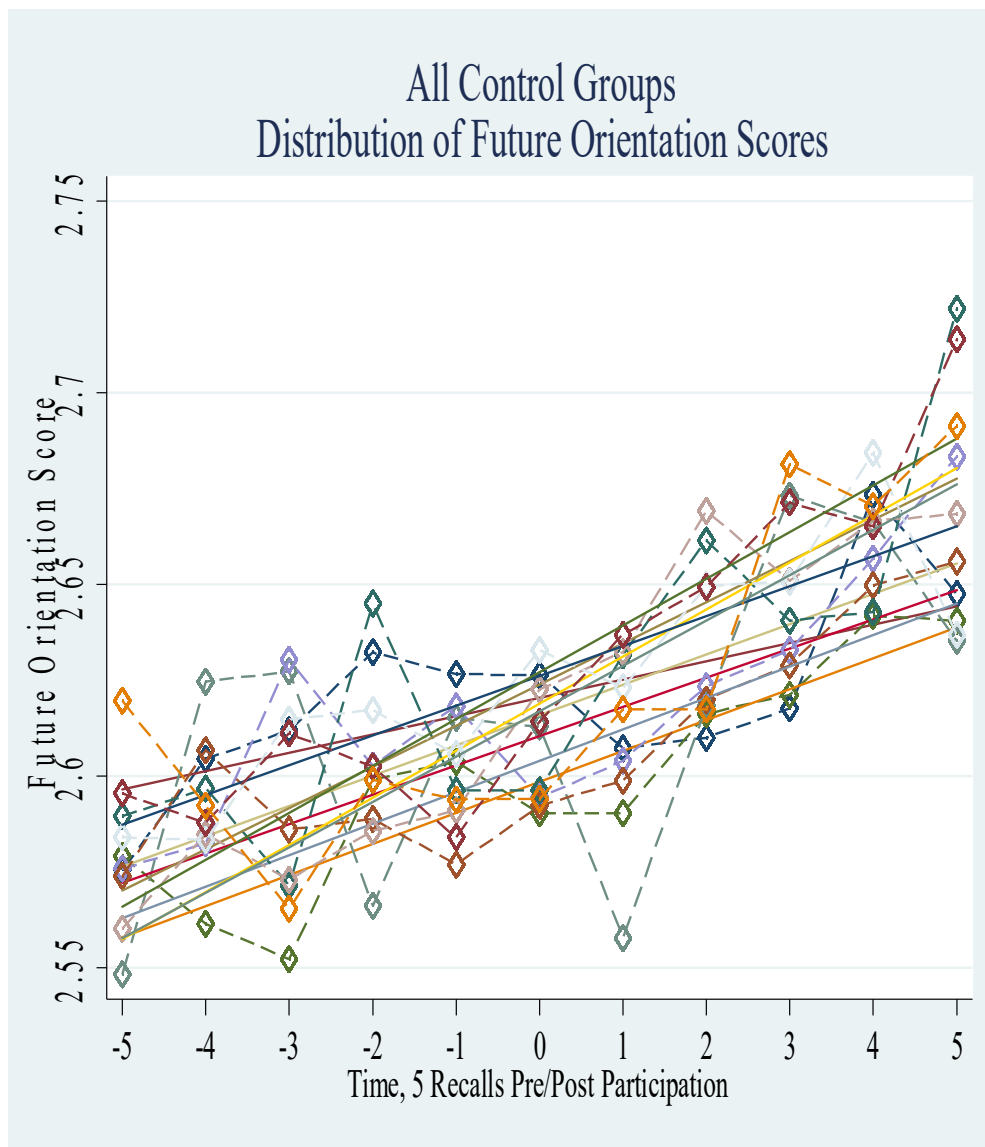


Figure A-6: AWFL Expectations Scores – Just Control Group Trends

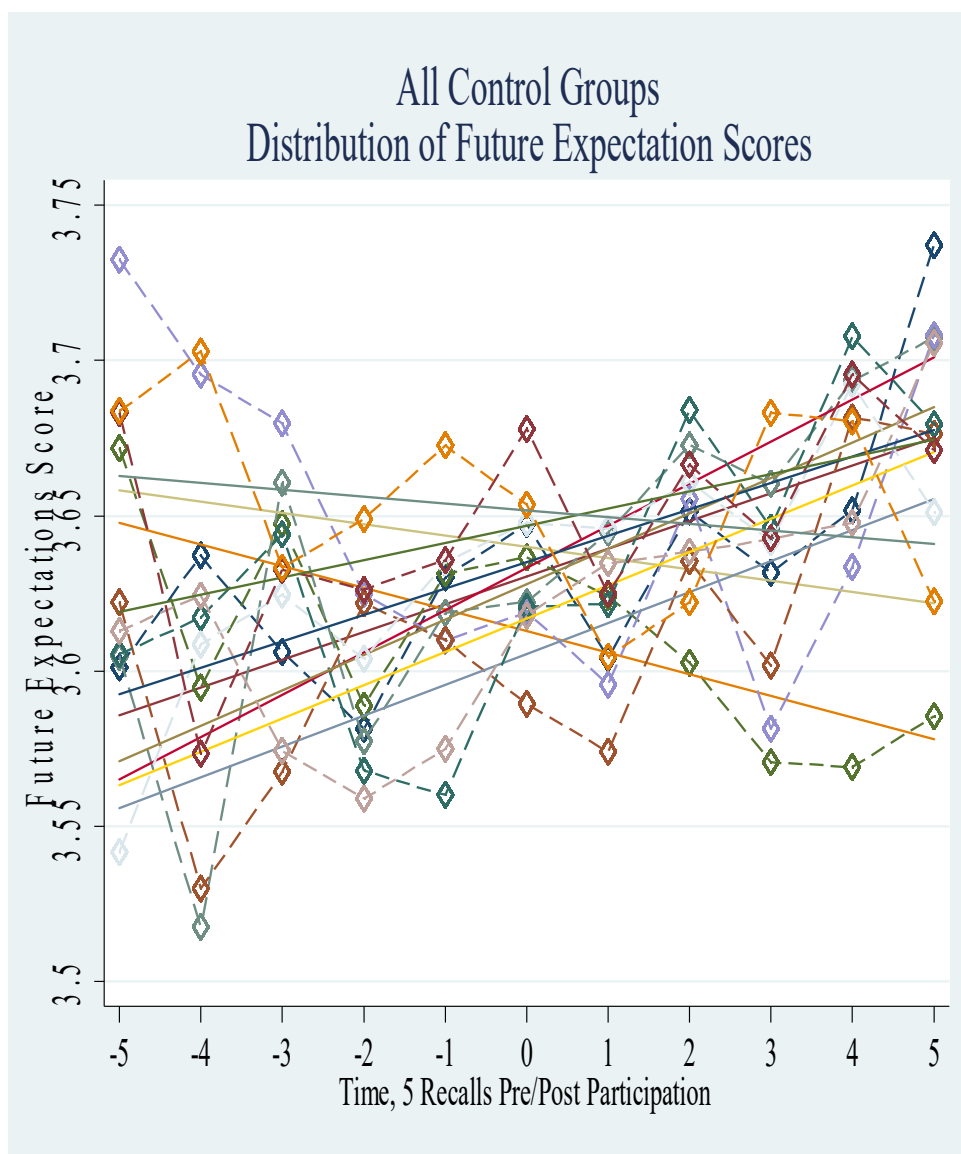


Figure A-7: AWFL Opportunities Scores – Just Control Group Trends

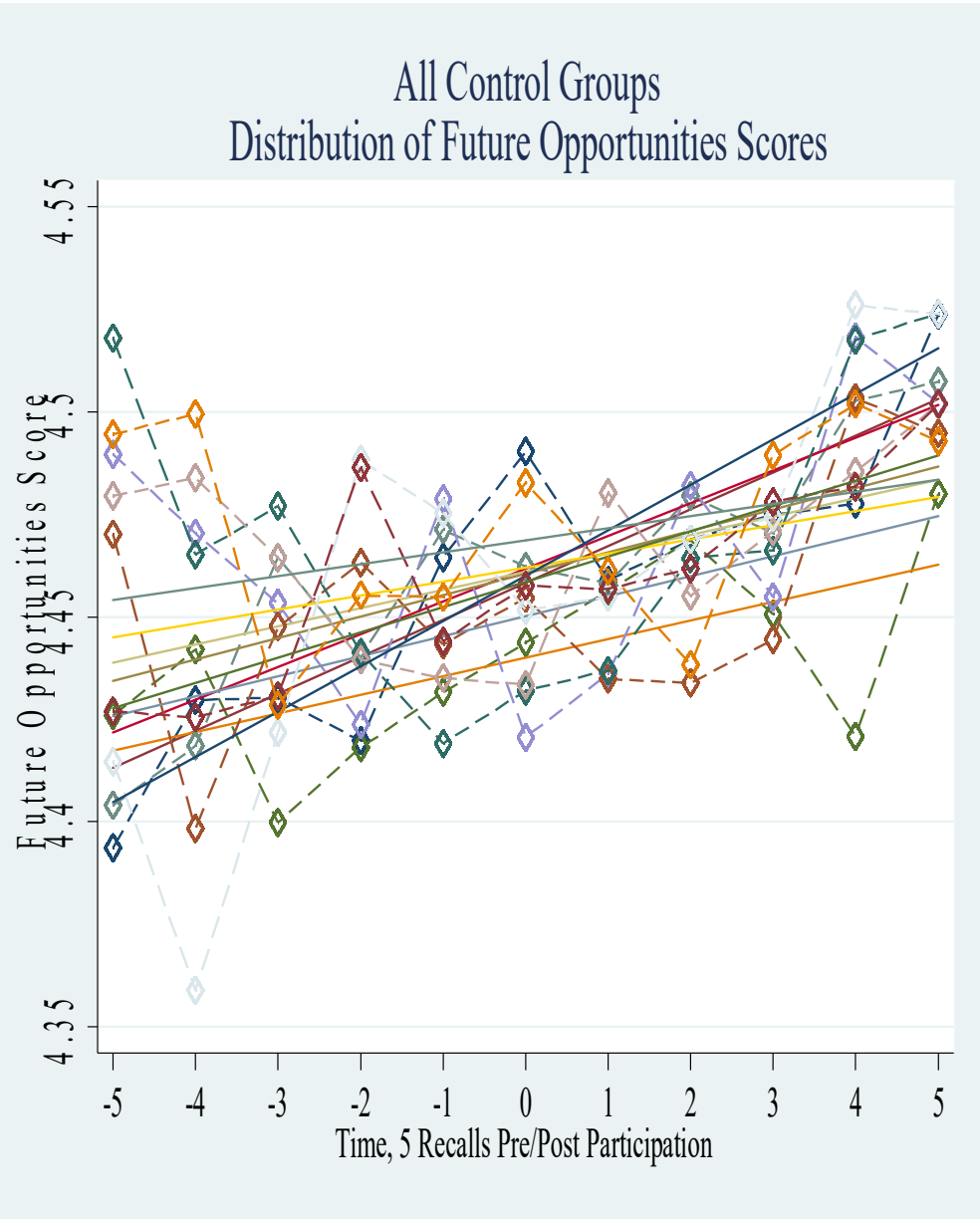


Figure A-8: MtS Scores – Just Control Group Trends

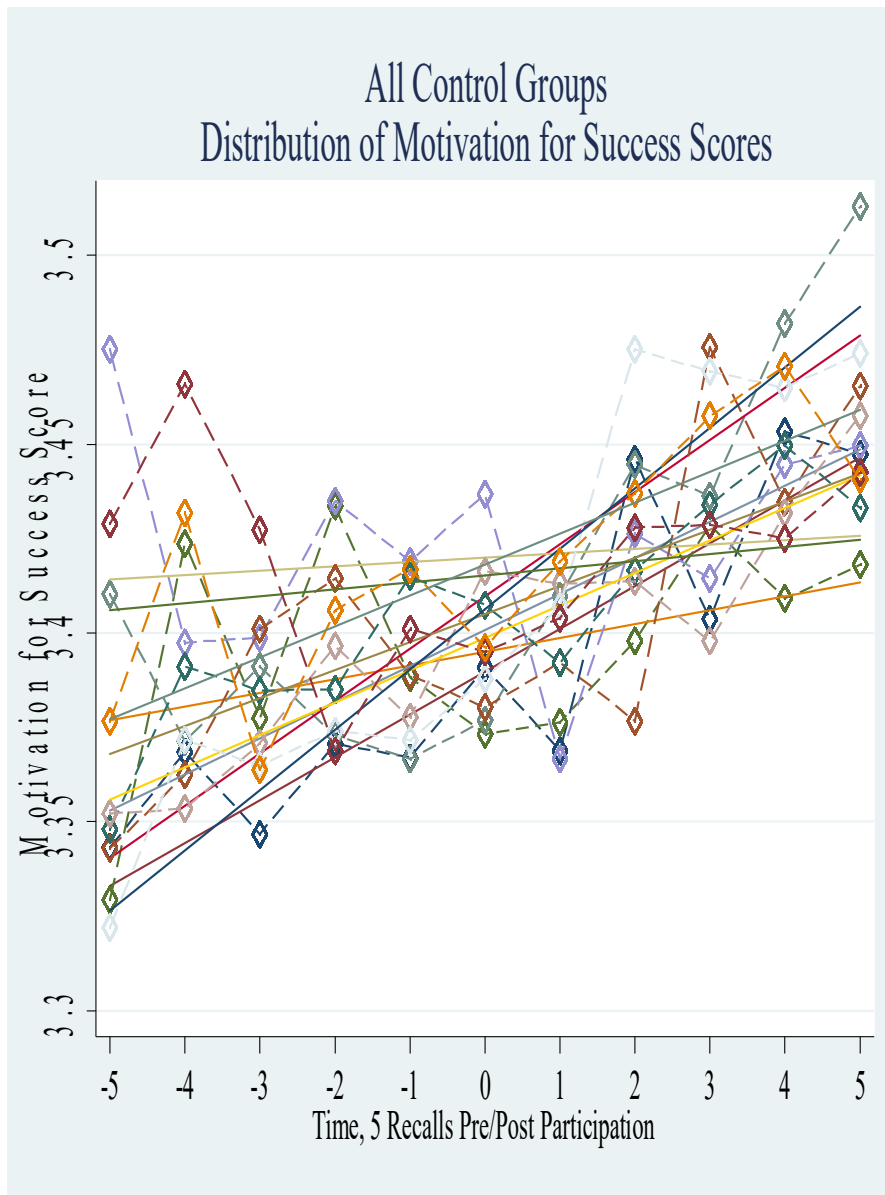


Figure A-9: Offending Rates (Incl. Drugs) Across Control Groups

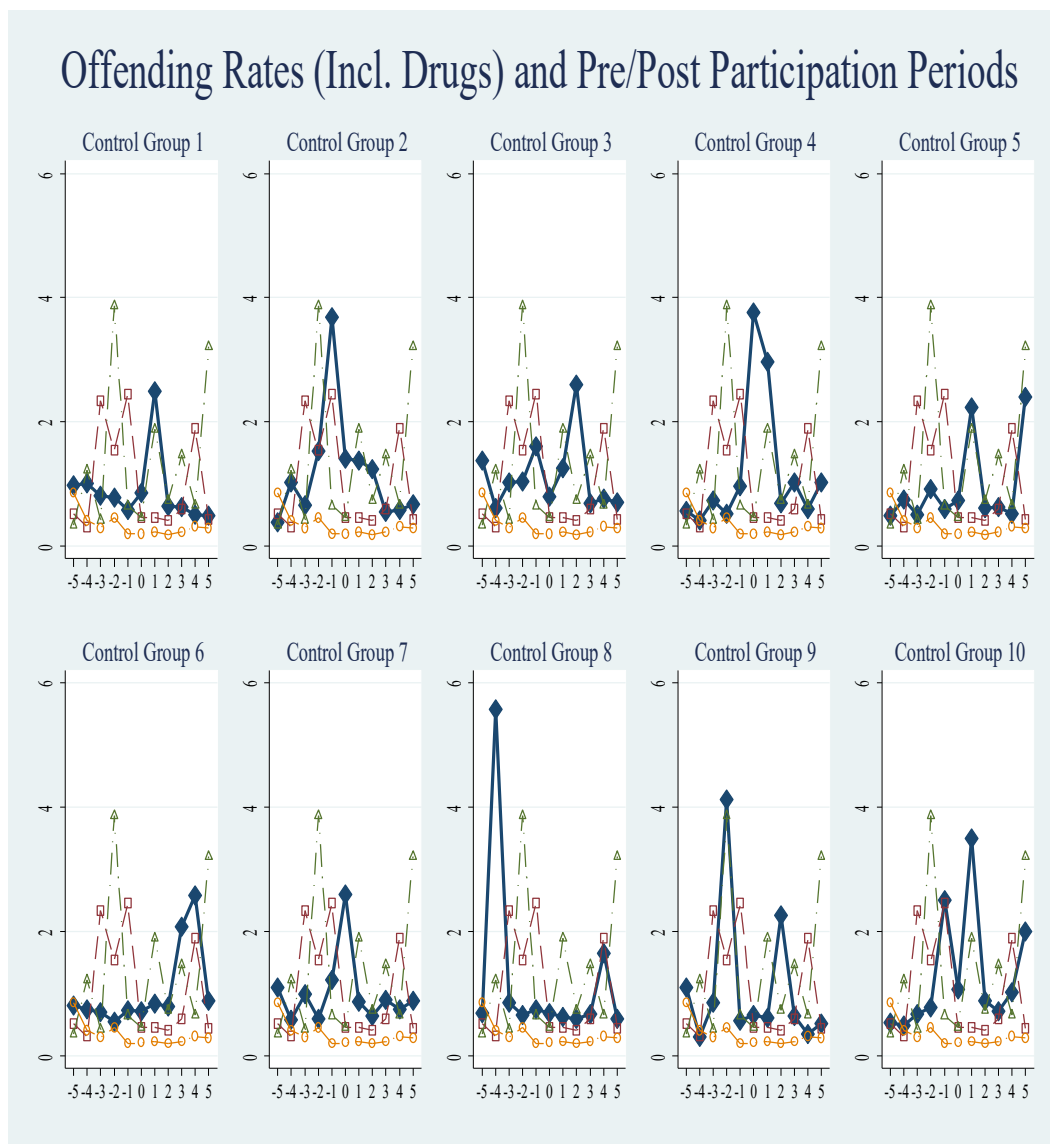


Figure A-10: Offending Rates (Excl. Drugs) Across Control Groups

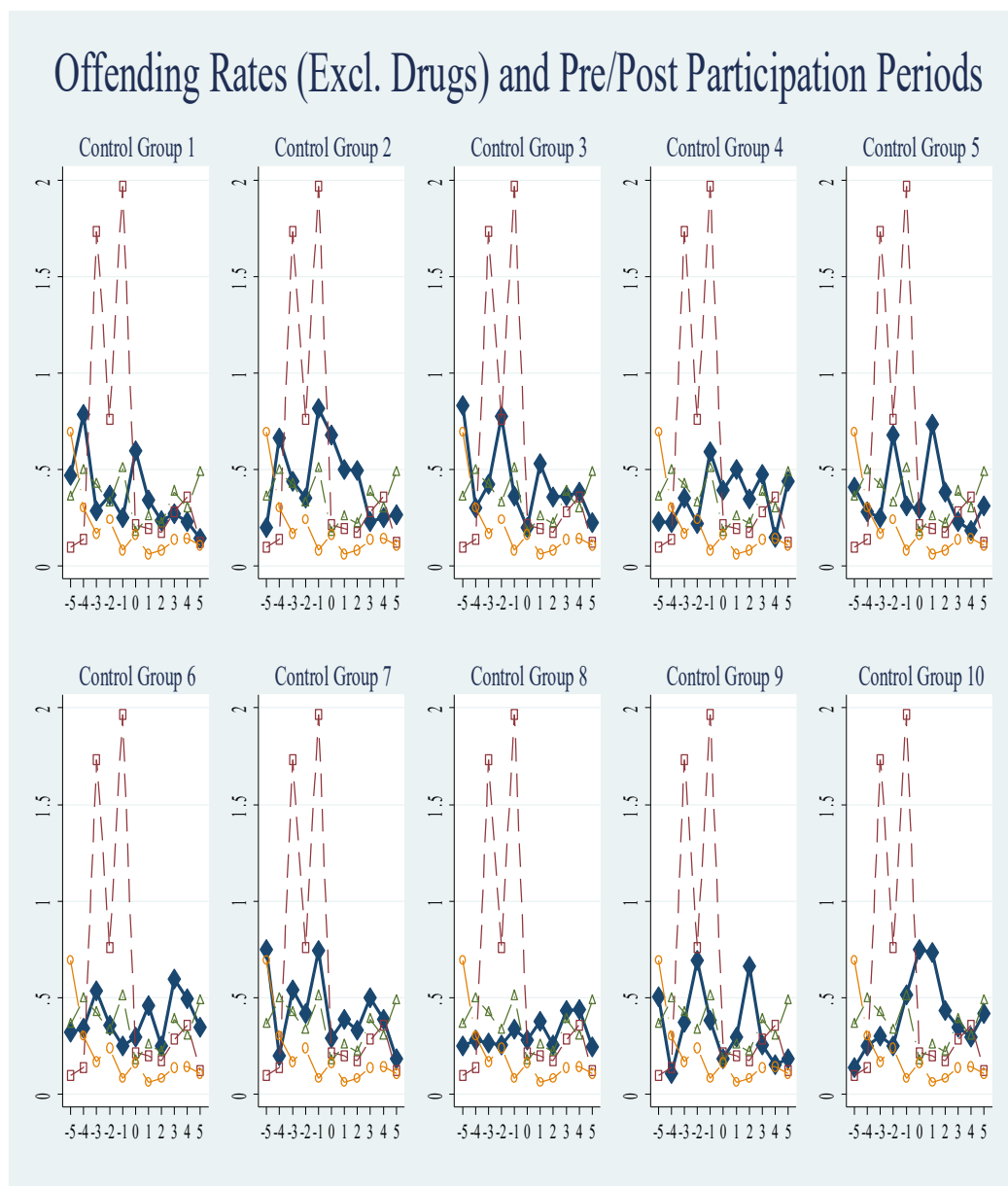




Figure A-11: Property Offending Variety Scores Across Control Groups

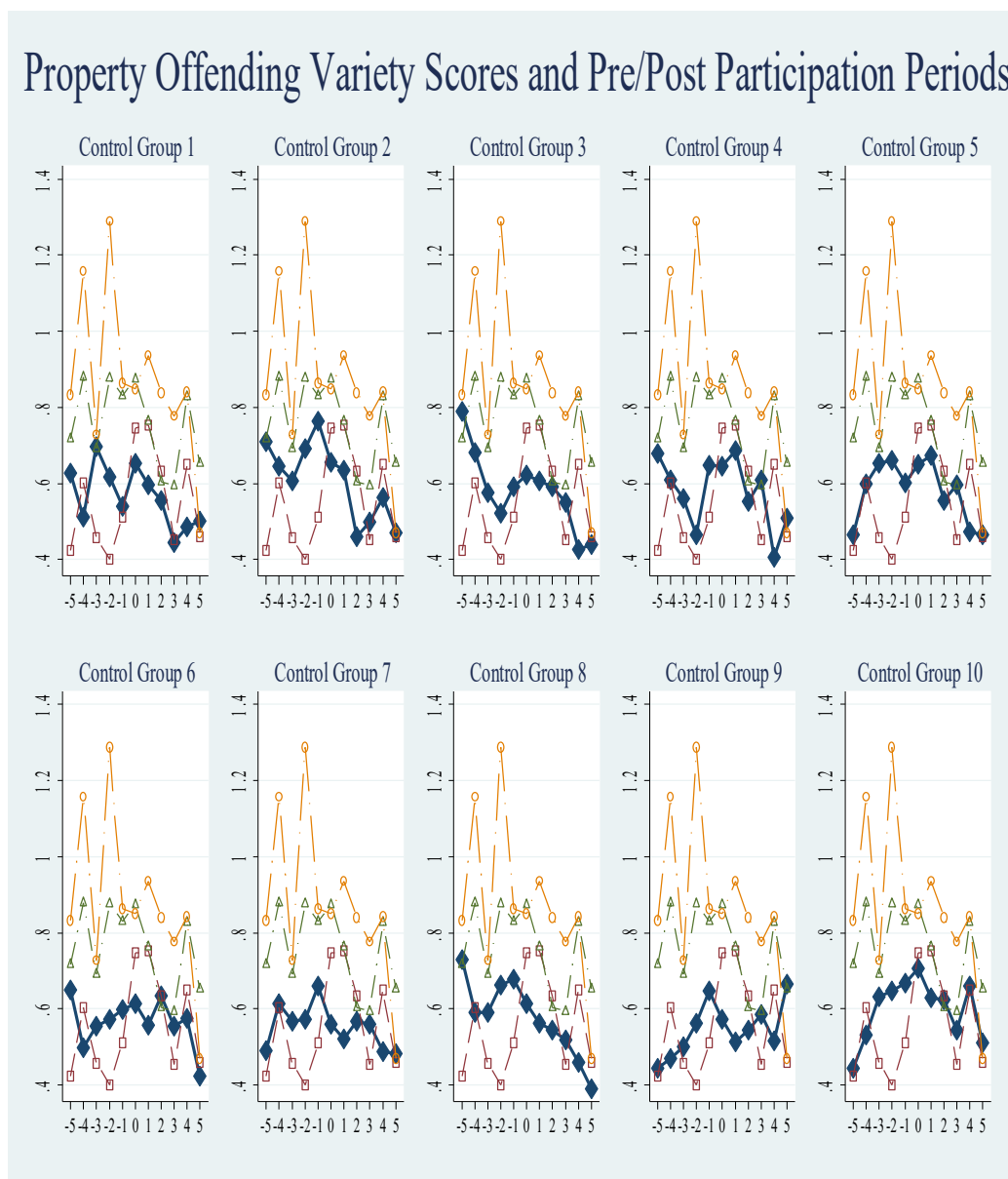


Figure A-12: Violent Offending Variety Scores Across Control Groups

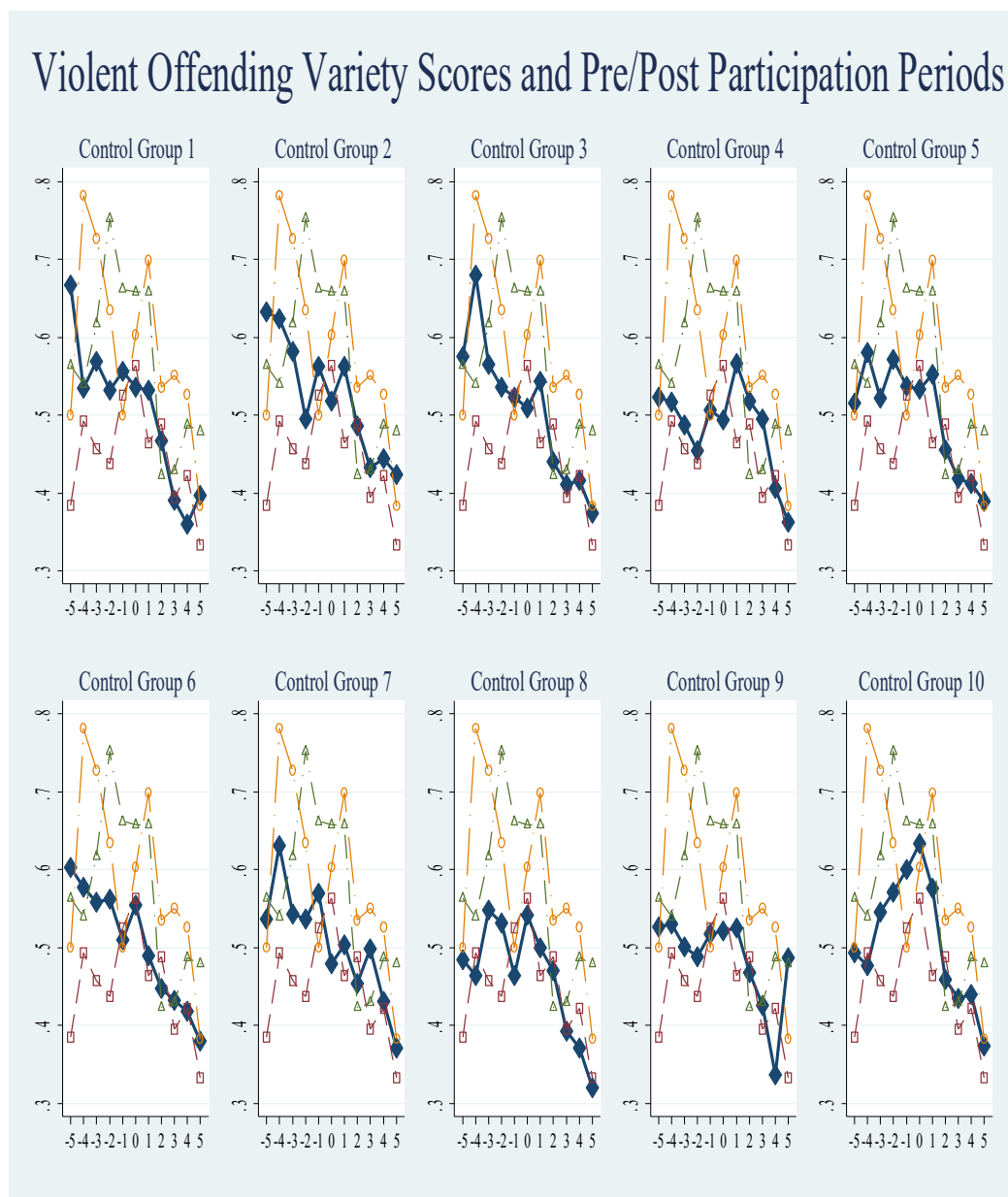


Figure A-13 Offending Rates (Incl. Drugs) – Just Control Group Trends

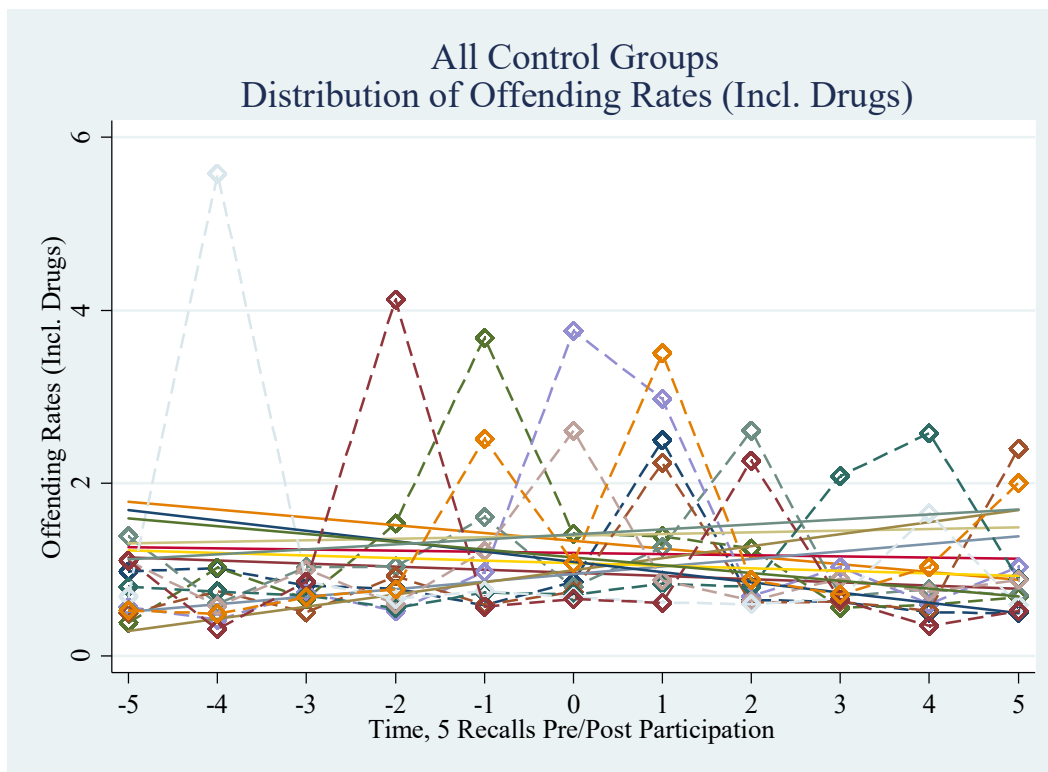


Figure A-14 Offending Rates (Excl. Drugs) – Just Control Group Trends

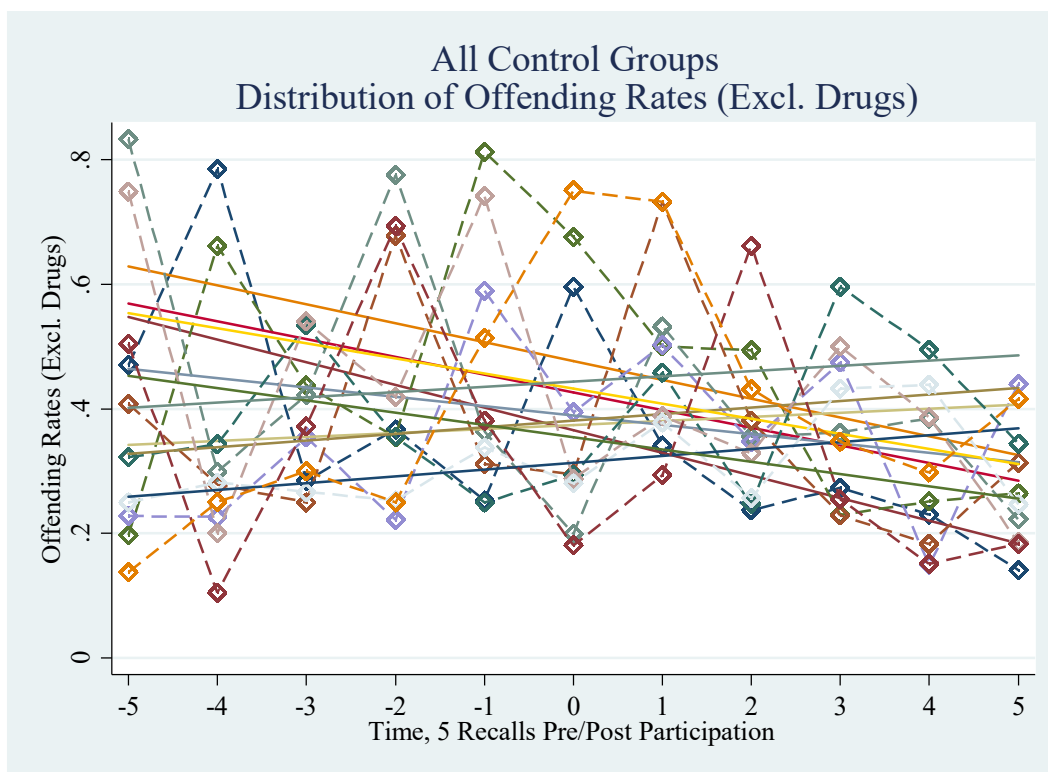


Figure A-15 Property Offending Variety Scores – Just Control Group Trends

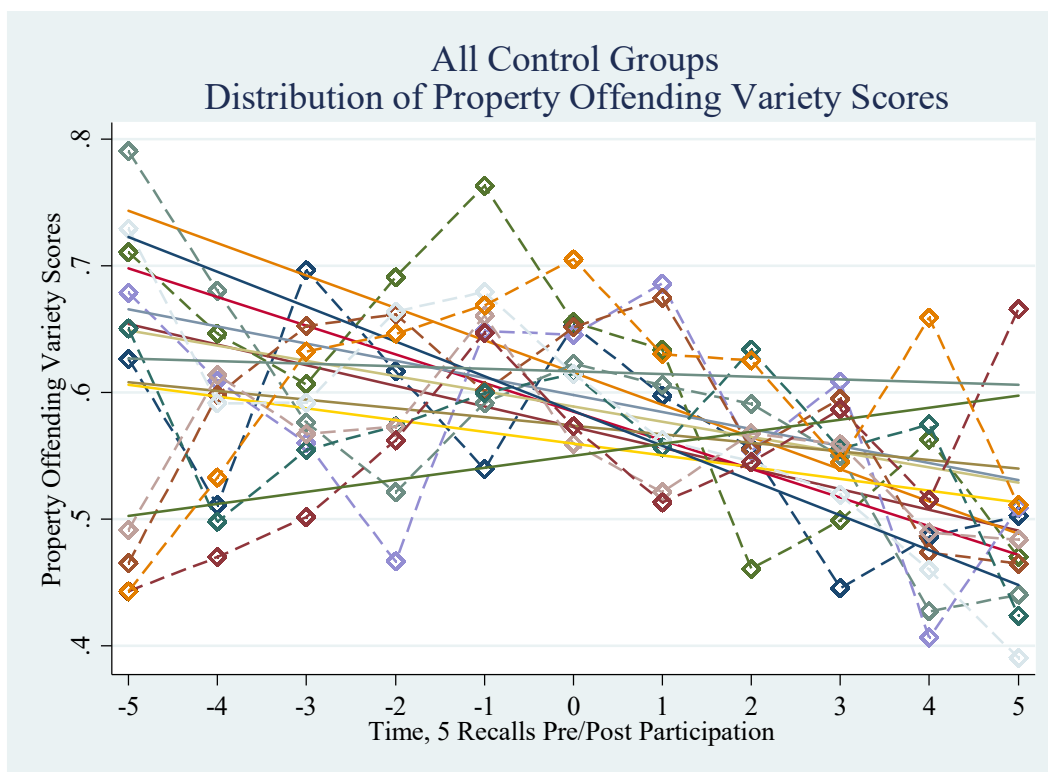
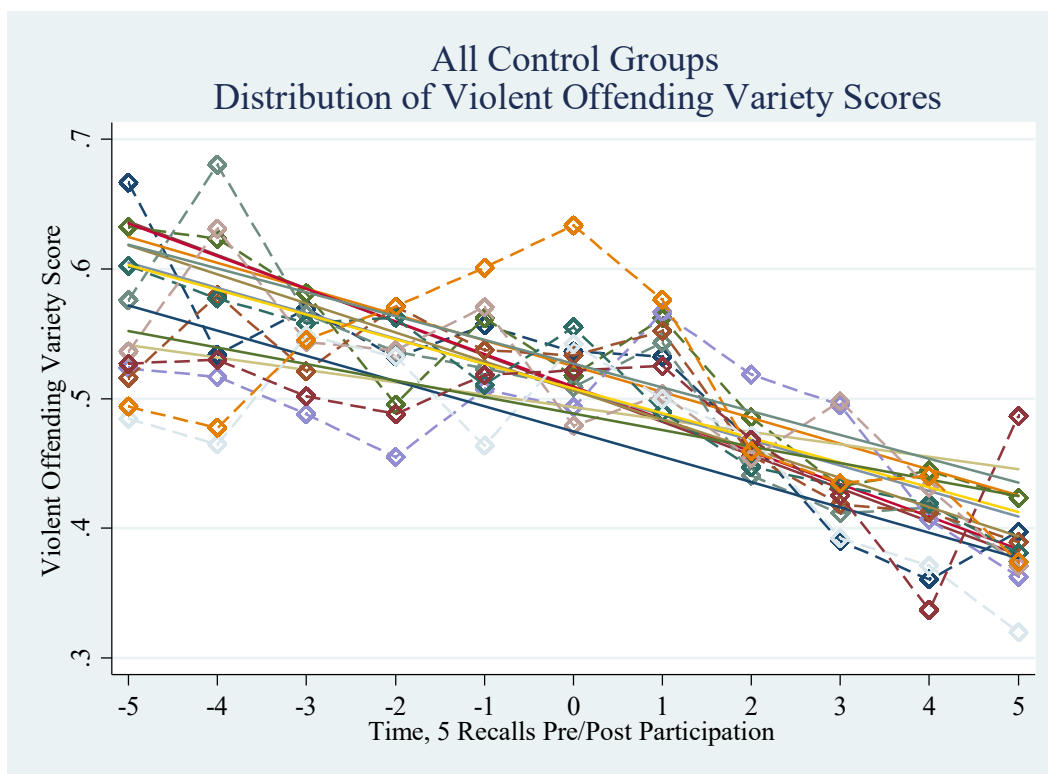


Figure A-16 Violent Offending Variety Scores – Just Control Group Trends



## APPENDIX B – FULL TABLES FROM DISSERTATION

Table B-1: Descriptive Statistics for  $t=3$  to  $t=0$

Variable	3 Recall Periods Pre-Participation (t=3)				2 Recall Periods Pre-Participation (t=2)				1 Recall Period Pre-Participation (t=1)				Recall Period of Participation (t=0)			
	Control (n=279)	Only Vol. (n=94)	Only Invol. (n=68)	Mixed (n=44)	Control (n=347)	Only Vol. (n=112)	Only Invol. (n=97)	Mixed (n=52)	Control (n=426)	Only Vol. (n=135)	Only Invol. (n=142)	Mixed (n=46)	Control (n=520)	Only Vol. (n=158)	Only Invol. (n=199)	Mixed (n=73)
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
<b>Aspirations for Work, Family, &amp; Law (AWFL)</b>																
Perceived Opportunities	4.442	.602	4.507	.484	4.444	.569	4.609	.512	4.448	.574	4.506	.519	4.557	.457	4.487	.552
Perceived Expectations	3.627	.881	3.646	.878	3.623	.857	3.743	.866	3.600	.884	3.777	.903	3.741	.874	3.712	.763
Future Outlook Inventory (FOI)	2.594	.588	2.618	.517	2.561	.554	2.516	.513	2.604	.579	2.582	.544	2.508	.539	2.348	.457
Motivation to Succeed (MIS)	3.383	.615	3.519	.690	3.532	.671	3.620	.596	3.396	.624	3.386	.581	3.388	.617	3.603	.634
<b>Criminal Attitudes</b>																
Social Rewards from Crime Factor Score	-.009	1.020	-.191	.963	.099	.832	.003	.939	.007	1.017	-.113	1.057	.196	1.016	-.086	.994
Personal Rewards from Crime	1.862	2.304	1.690	2.269	2.278	2.464	2.405	2.794	1.752	2.234	1.616	2.273	2.414	2.516	2.826	2.890
<b>Employment</b>																
Employment Rate (% Months in Recall Empl)	24.519	37.887	20.351	34.921	18.897	33.342	12.234	30.430	24.629	37.913	28.662	39.310	20.046	34.147	15.228	28.966
Employment Rate Percentile Category																
Lower Tenth (=1 if Yes)	.649	.477	.681	.469	.706	.459	.841	.370	.625	.484	.556	.499	.690	.464	.636	.485
Middle Tenth (=1 if Yes)	.125	.330	.149	.358	.118	.325	.068	.255	.130	.335	.188	.332	.124	.331	.096	.298
Upper Tenth (=1 if Yes)	.226	.419	.170	.378	.176	.384	.091	.291	.225	.417	.250	.435	.175	.382	.154	.364
Average Legal Wage Rate (By Recall)	2.225	4.675	1.795	2.854	2.209	3.986	1.162	2.526	2.299	4.823	2.074	3.294	1.678	3.124	1.420	2.374
Number of Interruptions in Legal Work	.449	.659	.372	.604	.382	.599	.364	.532	.467	.698	.429	.640	.546	.750	.442	.752
<b>Offending</b>																
Offending Rate (Drugs Incl.)	.702	3.723	2.336	18.781	.423	1.276	.300	.640	1.142	10.439	1.544	9.726	3.873	32.167	.461	1.508
Offending Rate (Drugs Excl.)	.376	2.059	1.734	12.953	.422	1.299	.170	.374	.433	3.448	.760	5.420	.329	.982	.240	.932
Property Variety Score	.593	1.323	.457	1.215	.691	1.458	.727	1.515	.598	1.380	.402	.885	.876	1.691	1.288	2.404
Violent Variety Score	.542	.915	.457	.812	.618	.915	.727	1.086	.528	.910	.438	.780	.753	1.011	.635	1.221
Age at First Offense	10.458	1.826	10.340	1.829	10.388	1.740	10.409	1.484	10.475	1.839	10.384	1.851	10.323	1.750	10.365	1.509
<b>Peer Influences</b>																
Member of Gang?	.109	.311	.074	.264	.074	.263	.136	.347	.104	.305	.054	.226	.144	.353	.135	.345
Contact with Gang	3.923	2.554	4.429	2.573	4.600	2.881	4.500	2.646	4.085	2.628	5.000	2.449	3.214	2.547	3.606	2.074
Resistance to Peer Infl. (RPI)	3.200	.570	3.257	.568	3.172	.542	3.134	.597	3.238	.562	3.264	.521	3.232	.557	3.073	.626
% of Friends Ever Arrested	.440	.411	.338	.379	.493	.399	.345	.357	.440	.413	.321	.388	.529	.414	.308	.358
% of Friends Ever Juiled	.354	.398	.257	.335	.410	.410	.278	.344	.354	.401	.237	.333	.423	.417	.223	.268
<b>Psychological Scales</b>																
Brief Symptom Inventory (BSI)	-.091	.721	.233	1.036	.006	.768	.219	.990	-.101	.709	.171	1.004	-.112	.853	.198	1.003
Psychosocial Maturity (PSMI)	3.157	.471	3.093	.570	3.120	.485	3.025	.509	3.183	.467	3.131	.478	3.093	.458	3.079	.499
Psychopathic Traits (PTI)	.065	1.007	.088	.838	.080	.873	.180	1.001	.036	1.018	-.024	.947	.093	.878	.134	1.101
<b>Social Support</b>																
Number of Caring Adults (Logged)	2.289	1.052	2.403	1.027	2.258	1.180	2.381	.909	2.273	1.051	2.322	1.136	2.584	.784	2.442	.919
Diversity of Caring Adults	1.960	1.262	1.979	1.227	1.868	1.292	2.295	1.440	1.874	1.304	2.063	1.429	2.124	1.277	2.115	1.215
Domains of Social Support	5.607	2.567	5.936	2.449	5.441	2.825	6.341	2.101	5.591	2.590	5.696	2.789	6.155	2.033	6.038	2.275
<b>Substance Abuse</b>																
Frequency of Abuse	7.496	5.812	10.478	7.531	7.174	7.487	7.556	5.544	7.634	5.619	8.425	4.987	10.250	6.667	6.913	5.518
Variety of Substances Used	1.319	1.493	.532	1.198	.529	1.165	.841	1.160	1.409	1.554	.679	1.323	.598	1.133	.731	.931
<b>Demographics</b>																
Age	17.900	1.530	18.021	1.659	17.721	1.582	17.114	1.573	18.169	1.739	18.330	1.914	17.619	1.933	17.596	1.612
Race																
White	.170	.375	.277	.450	.265	.444	.386	.495	.168	.374	.286	.454	.247	.434	.423	.499
Black	.427	.495	.394	.491	.338	.477	.250	.438	.430	.496	.393	.491	.351	.480	.231	.425
Hispanic	.351	.478	.277	.450	.368	.486	.295	.462	.350	.477	.277	.449	.381	.488	.308	.466
Other	.052	.220	.053	.226	.029	.170	.068	.255	.052	.220	.045	.207	.021	.143	.038	.194
Male	.887	.316	.723	.450	.926	.263	.614	.495	.887	.316	.705	.458	.897	.306	.577	.499
Site (Phoenix=1)	.520	.500	.574	.497	.426	.498	.325	.474	.528	.500	.571	.497	.454	.500	.292	.459
Parent Socioeconomic Status	52.063	12.418	50.516	12.006	49.642	11.381	46.761	10.945	52.018	12.395	50.032	12.641	51.078	11.314	47.327	10.646
<b>Interview/Recall Information</b>																
Number of Months in Recall	6.545	1.995	6.628	2.205	6.426	1.879	6.273	1.933	7.043	2.478	7.080	2.519	6.969	2.038	6.692	2.236
Interviewed in placement?	.379	.485	.213	.411	.471	.503	.318	.471	.379	.485	.179	.385	.598	.493	.308	.466
Percent Time in Community	62.120	43.315	74.504	39.177	51.762	44.404	66.181	42.493	60.914	43.636	78.786	36.140	50.735	44.023	71.104	42.695
<b>Missing Value Indicators</b>																
BSI Missing?	.274	.446	.213	.411	.176	.384	.159	.370	.268	.443	.196	.399	.134	.342	.269	.448
Caring Adults Missing?	.104	.304	.085	.281	.147	.357	.068	.255	.107	.308	.116	.322	.051	.174	.077	.269
Friends Missing?	.095	.292	.096	.296	.103	.306	.023	.151	.104	.304	.134	.342	.113	.319	.058	.235
Offense Rate (Drugs Incl.) Missing?	.059	.236	.021	.145	.059	.237	.091	.291	.061	.237	.027	.162	.093	.292	.096	.298
Offense Rate (Drugs Excl.) Missing?	.132	.337	.074	.264	.132	.341	.136	.347	.138	.344	.054	.226	.134	.342	.135	.345

Table B-1: Descriptive Statistics for  $t=0$  to  $t+3$ [illegible]



Table B-2: Full Attitudes/Orientations Results

Variables	AWFL Opportunites		AWFL Expectations		FOI Scores		MtS Scores	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
<i>Social Rewards from Crime</i>	-.0025	.008	-.0091	.012	-.0029	.009	-.0318***	.009
<i>Personal Rewards from Crime</i>	-.0004	.003	.0039	.005	-.0048	.004	-.0012	.004
<i>Logged Offending Rate (Excl. Drugs)</i>	-.0191	.030	-.0100	.045	.0329	.031	-.0311	.033
<i>Property Offending Variety Score</i>	-.0067	.015	-.0055	.022	-.0011	.015	-.0032	.016
<i>Violent Offending Variety Score</i>	.0111	.011	-.0082	.016	-.0010	.011	-.0034	.012
<i>Age at First Offense</i>	-.0027	.004	-.0037	.006	-.0028	.004	-.0079	.004
<i>Age</i>	-.0099	.050	-.0248	.074	-.0607	.052	-.0471	.055
<i>White</i>	-.0129	.036	-.0323	.053	.0019	.037	-.0008	.039
<i>Black</i>	-.0023	.037	-.0357	.056	.0147	.039	-.0001	.041
<i>Hispanic</i>	-.0004	.036	-.0143	.053	.0068	.037	.0177	.039
<i>Male</i>	.0079	.019	-.0189	.028	.0212	.020	.0028	.021
<i>Site (Phoenix=1)</i>	-.0341	.020	-.0264	.030	-.0490*	.021	.0124	.022
<i>Parents' Social Capital</i>	-.0004	.001	-.0008	.001	-.0005	.001	.0003	.001
<i>Employment Rate</i>	-.0001	.000	-.0003	.000	-.0001	.000	.0002	.000
<i>Illegal Employment Rate</i>	-.0018**	.001	-.0020*	.001	-.0019**	.001	-.0007	.001
<i>Variety of Illegal Work</i>	.0799	.027	.0819	.041	.0433	.028	.0137	.030
<i>Logged Average Legal Wage Rate</i>	.0058	.016	.0321	.024	.0091	.017	.0109	.018
<i>Number of Work Interruptions</i>	-.0031	.019	-.0122	.028	-.0194	.020	-.0036	.021
<i>Number of Months in Recall</i>	-.0031	.003	.0022	.005	.0002	.003	-.0050	.004
<i>Interviewed in Secure Facility?</i>	.0736	.023	.0737	.034	.0192	.024	.0249	.025
<i>RPI Score</i>	.0298*	.015	.0644**	.022	.0428**	.015	.0070	.016
<i>Prop. Friends Ever Arrested</i>	.0065	.035	.0593	.052	.0109	.036	.0471	.038
<i>Prop. Friends Ever Jailed</i>	-.0049	.037	-.0410	.055	-.0209	.038	-.0381	.040
<i>Member of a Gang?</i>	.0534	.059	.0330	.088	.0476	.061	.0473	.065
<i>Contact Frequency with Gang</i>	-.0076	.012	-.0095	.017	-.0140	.012	-.0052	.013
<i>BSI Factor Score</i>	-.0005	.009	-.0210	.013	.0206*	.009	-.0049	.009
<i>YPI Factor Score</i>	.0063	.009	.0217	.014	-.0127	.010	.0000	.010
<i>PSMI</i>	.0621**	.023	.2071***	.034	.0922***	.024	.1209***	.025
<i>Moral Disengagement</i>	-.0016	.002	-.0001	.003	.0055**	.002	-.0005	.002
<i>Logged Number of Caring Adults</i>	.0017	.015	.0084	.023	.0025	.016	.0151	.017
<i>Domains of Social Support</i>	.0013	.006	.0069	.009	.0100	.006	.0072	.006
<i>Diversity of Caring Adults</i>	.0039	.008	-.0068	.012	-.0099	.008	.0073	.009
<i>Frequency of Substance Abuse</i>	-.0010	.003	-.0068	.004	.0002	.003	-.0040	.003
<i>Variety of Substance Abuse</i>	.0027	.013	.0139	.019	-.0020	.013	.0122	.014
<i>Treatment</i>	.0011	.021	-.0098	.031	-.0020	.021	-.0049	.023
<i>Group</i>								
<i>ONLY VOL</i>	-.0076	.034	.0077	.051	.0126	.035	-.0209	.037
<i>ONLY INVOL</i>	-.0253	.041	.0134	.060	-.0137	.042	-.0057	.044
<i>MIXED</i>	-.0094	.066	.0417	.098	.0045	.068	-.0229	.072
<i>Group X Treatment</i>								
<i>I#ONLY VOL</i>	.0104	.038	-.0006	.057	-.0225	.040	-.0064	.042
<i>I#ONLY INVOL</i>	-.0054	.041	-.0147	.061	.0083	.043	-.0090	.045
<i>I#MIXED</i>	.0089	.055	-.0148	.082	.0273	.057	.0052	.060
<i>Age Squared</i>	.0002	.001	.0003	.002	.0013	.001	.0011	.001
<i>Property Var. Score Squared</i>	-.0010	.002	-.0026	.003	-.0013	.002	.0006	.002
<i>Number of Work Inter. Sqrd</i>	-.0016	.007	-.0021	.010	.0073	.007	-.0022	.008
<i>Missing Offending Rate?</i>	-.0428	.041	-.0116	.061	.0273	.043	-.0310	.045
<i>Missing BSI?</i>	.0072	.017	.0665**	.025	.0236	.017	.0248	.018
<i>Missing Caring Adults?</i>	-.0356	.036	.0169	.054	.0170	.038	.0352	.040
<i>Missing Friends?</i>	-.0021	.023	-.0118	.034	-.0247	.024	-.0027	.025
<i>Average PSMI</i>	-.0984**	.032	-.2913***	.048	-.1181***	.034	-.2057***	.035
<i>Participation Category X Treatment</i>								
<i>I#CONTROL</i>	-.0217	.019	-.0073	.028	.0039	.020	-.0208	.021
<i>I#ONLY VOL</i>	-.0057	.034	-.0144	.050	.0078	.035	.0047	.037
<i>I#ONLY INVOL</i>	.0195	.031	-.0002	.047	.0167	.032	-.0046	.034
<i>I#MIXED</i>	-.0126	.060	-.0385	.089	-.0096	.062	-.0089	.066
<i>Intercept</i>	.2221	.504	.4816	.751	.6339	.521	.7829	.552

Table B-3: Sensitivity Analysis for Attitudes/Orientations – Simple Model

Variables	AWFL Opportunities		AWFL Expectations		FOI Scores		MtS Scores	
	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
<i>Age</i>	.0036	.003	.0085	.005	.0253***	.003	.0129***	.003
<i>White</i>	-.0265	.073	.1744	.124	-.0627	.075	.1048	.082
<i>Black</i>	-.0664	.075	.1244	.128	.0160	.077	.0033	.084
<i>Hispanic</i>	-.0898	.071	.0152	.122	-.0585	.073	-.1262	.080
<i>Male</i>	-.0738	.036	-.2250	.062	-.0685	.037	-.0767	.041
<i>Site (Phoenix=1)</i>	-.0170	.037	-.1714**	.064	.0779*	.038	-.1785***	.042
<i>Parents' Social Capital</i>	-.0035**	.001	-.0084***	.002	-.0032*	.001	-.0071***	.001
<i>Treatment</i>	.0360	.020	.0601*	.031	.0103	.021	.0751**	.022
<i>Group</i>								
<i>ONLY VOL</i>	-.0122	.052	-.1108	.088	-.0363	.053	-.0315	.058
<i>ONLY INVOL</i>	-.0215	.055	-.0426	.093	-.0797	.057	-.0747	.062
<i>MIXED</i>	.1020	.114	-.0441	.194	-.2180	.117	-.0156	.128
<i>Group X Treatment</i>								
<i>1#ONLY VOL</i>	-.0171	.034	-.0470	.050	.0367	.034	-.0316	.036
<i>1#ONLY INVOL</i>	-.0258	.033	-.0002	.050	.0769*	.034	.0070	.036
<i>1#MIXED</i>	-.0317	.047	-.0365	.071	.1579**	.048	-.0817	.051
<i>Participation Category X Treatment</i>								
<i>1#CONTROL</i>	-.0144	.015	.0054	.022	.0039	.015	.0074	.016
<i>1#ONLY VOL</i>	.0523	.069	.1280	.119	.0458	.071	-.0210	.078
<i>1#ONLY INVOL</i>	.0934	.064	.0974	.110	.0344	.066	.1590	.072
<i>1#MIXED</i>	-.0779	.124	-.0563	.212	.1351	.127	.0835	.139
<i>Intercept</i>	4.7219***	.114	4.1508***	.190	2.3531***	.117	3.7563***	.127

Table B-4: Full Random-Intercept Results for Offending Rates (Incl. Drugs)

Full Random-Intercept Results - Offending Rate (Incl. Drugs) By Tertile of Post-Program Employment Rate						
Variables	Lower Tertile of Emp Rate		Middle Tertile of Emp Rate		Upper Tertile of Emp Rate	
	Coef	St. Err.	Coef	St. Err.	Coef	St. Err.
<i>Social Rewards from Crime</i>	-.035	.538	-.072	.256	.110	.136
<i>Personal Rewards from Crime</i>	-.184	.263	.062	.104	.054	.056
<i>AWFL Opportunities Score</i>	.592	.971	-.189	.463	.119	.287
<i>AWFL Expectations Score</i>	.032	.699	.003	.309	.102	.182
<i>FOI Score</i>	.256	1.003	.043	.469	.234	.251
<i>MtS Score</i>	-.681	.987	.252	.416	-.331	.219
<i>Age at First Offense</i>	-.035	.283	.078	.142	.008	.063
<i>Age</i>	-.899	3.273	-1.218	1.472	-.535	.841
<i>White</i>	.234	2.658	.177	1.381	.049	.551
<i>Black</i>	.317	2.491	.130	1.365	-.172	.615
<i>Hispanic</i>	.289	2.391	-.023	1.338	-.219	.560
<i>Other</i>						
<i>Male</i>	.308	1.353	-.168	.581	.144	.312
<i>Site (Phoenix=1)</i>	-.628	1.419	.692	.655	.040	.343
<i>Parents' Social Capital</i>	.032	.049	-.005	.020	.000	.011
<i>Employment Rate</i>	-.003	.031	.000	.010	.004	.005
<i>Illegal Employment Rate</i>	.007	.036	-.019	.017	.001	.011
<i>Variety of Illegal Work</i>	4.266**	1.601	1.564*	.757	1.370**	.504
<i>Logged Average Legal Wage Rate</i>	.004	.963	.072	.375	-.090	.201
<i>Number of Work Interruptions</i>	2.050	1.612	.401	.699	.111	.270
<i>Number of Months in Recall</i>	.088	.231	-.058	.101	-.038	.054
<i>Interviewed in Secure Setting?</i>	-.012	1.170	.385	.572	.483	.433
<i>RPI Score</i>	.780	.992	-.173	.455	-.037	.242
<i>Prop. Friends Ever Arrested</i>	.545	2.760	-.280	1.050	-1.460	.535
<i>Prop. Friends Ever Jailed</i>	-.783	2.787	-.056	1.066	1.662**	.575
<i>Member of a Gang?</i>	-7.247	4.717	2.231	2.274	-1.563	1.417
<i>Contact with Gang Members?</i>	1.727*	.846	-.993**	.381	-.351	.262
<i>BSI Factor</i>	.116	.540	-.160	.247	.059	.153
<i>Moral Disengagement</i>	.078	.123	-.031	.061	.015	.035
<i>PSMI Score</i>	.261	1.396	-.375	.693	-.604	.401
<i>WAI Factor Score</i>	-1.141	.853	-.046	.371	.127	.206
<i>Consideration for Others</i>	-.529	.641	.008	.296	-.090	.166
<i>YPI Factor Score</i>	-.357	.639	.099	.294	.081	.175
<i>Logged Number of Caring Adults</i>	-1.887	1.051	-.099	.520	.044	.236
<i>Domains of Social Support</i>	.496	.406	.024	.183	.030	.092
<i>Diversity of Caring Adults</i>	.036	.547	.085	.248	-.002	.121
<i>Frequency of Substance Abuse</i>	-.088	.184	.029	.084	-.049	.043
<i>Variety of Substance Abuse</i>	-.027	.935	.287	.396	.047	.194
<i>Treated</i>	.460	1.482	-.130	.770	.249	.331
<i>Participation Group</i>						
<i>ONLY VOL</i>	.746	2.354	-1.044	1.265	.171	.585
<i>ONLY INVOL</i>	.979	2.889	-.636	1.334	.775	.698
<i>MIXED</i>	2.308	14.093	-.908	2.441	.139	.976
<i>Group X Treatment</i>						
<i>I#ONLY VOL</i>	-.199	2.590	.997	1.341	.182	.627
<i>I#ONLY INVOL</i>	-.459	3.015	.613	1.355	-.289	.673
<i>I#MIXED</i>	-.323	3.791	.342	2.191	-.334	.847
<i>Age Squared</i>	.020	.082	.032	.037	.015	.021
<i>Number of Work Interruptions Sqrd.</i>	-.377	.688	-.157	.327	-.082	.092
<i>Missing BSI?</i>	-.799	1.323	-.174	.576	.057	.304
<i>Missing Caring Adults?</i>	-1.944	2.492	.491	1.098	-.567	.598
<i>Missing Friends?</i>	.418	1.430	.009	.687	.281	.392
<i>Participation Category X Treatment</i>						
<i>I#CONTROL</i>	1.123	1.286	-.397	.578	-.124	.296
<i>I#ONLY VOL</i>	.384	2.410	-.300	1.071	-.315	.555
<i>I#ONLY INVOL</i>	-.349	2.723	.650	.933	-1.113*	.504
<i>I#MIXED</i>	-.920	14.086	.401	2.007	.232	.853
<i>Average Illegal Emp Rate</i>	-.008	.095	-.027	.049	.157***	.026
<i>Average Variety of Illegal Work</i>	-3.922	4.854	-.850	1.823	-10.584***	1.259
<i>Average Gang Membership</i>	13.383	7.575	-.475	3.946	6.153**	2.274
<i>Average Contact with Gang</i>	-2.884	1.491	.759	.769	-1.164*	.488
<i>Average PSMI</i>	-.520	2.294	-.126	1.084	.467	.612
<i>Average Frequency of Subst. Abuse</i>	.079	.239	.024	.103	.115*	.052
<i>Average BSI Missing</i>	.256	2.755	1.094	1.210	-.086	.570
<i>Average WAI Factor</i>	1.402	1.224	-.172	.516	.227	.274
<i>Intercept</i>	7.090	33.286	12.447	15.054	5.062	8.579

Table B-5: Full Random-Intercept Results for Offending Rates (Excl. Drugs)

<b>Full Random-Intercept Results - Offending Rate (Excl. Drugs) By Tertile of Post-Program Employment Rate</b>						
	<b>Lower Tertile of Emp Rate</b>		<b>Middle Tertile of Emp Rate</b>		<b>Upper Tertile of Emp Rate</b>	
<b>Variables</b>	<b>Coef</b>	<b>St. Err.</b>	<b>Coef</b>	<b>St. Err.</b>	<b>Coef</b>	<b>St. Err.</b>
<i>Social Rewards from Crime</i>	.031	.250	-.014	.090	.005	.024
<i>Personal Rewards from Crime</i>	-.053	.121	.024	.036	.011	.010
<i>AWFL Opportunities Score</i>	.132	.450	-.091	.163	.026	.051
<i>AWFL Expectations Score</i>	-.008	.326	.056	.108	.005	.032
<i>FOI Score</i>	.004	.465	-.003	.165	.048	.045
<i>MtS Score</i>	-.252	.458	.029	.146	-.028	.039
<i>Age at First Offense</i>	.007	.131	.006	.050	.006	.011
<i>Age</i>	-.273	1.508	.051	.518	.014	.150
<i>White</i>	.083	1.243	-.018	.484	-.041	.098
<i>Black</i>	.229	1.172	-.110	.479	-.057	.109
<i>Hispanic</i>	.225	1.117	-.006	.469	-.050	.100
<i>Other</i>						
<i>Male</i>	.318	.619	-.102	.203	.021	.055
<i>Site (Phoenix=1)</i>	-.349	.655	.342	.230	.017	.061
<i>Parents' Social Capital</i>	.006	.023	-.003	.007	.001	.002
<i>Employment Rate</i>	-.004	.014	-.001	.003	-.001	.001
<i>Illegal Employment Rate</i>	-.030	.017	-.003	.006	-.001	.002
<i>Variety of Illegal Work</i>	2.763***	.723	.356	.263	.240**	.089
<i>Logged Average Legal Wage Rate</i>	.044	.436	.081	.131	.024	.036
<i>Number of Work Interruptions</i>	.857	.746	-.033	.245	.000	.048
<i>Number of Months in Recall</i>	.036	.107	-.009	.035	-.002	.010
<i>Interviewed in Secure Setting?</i>	-.034	.557	.395	.209	.094	.081
<i>RPI Score</i>	.295	.461	-.107	.160	-.011	.043
<i>Prop. Friends Ever Arrested</i>	.308	1.293	.035	.364	-.204	.095
<i>Prop. Friends Ever Jailed</i>	-.203	1.302	-.232	.371	.212*	.102
<i>Member of a Gang?</i>	-5.634*	2.179	.648	.789	-.315	.259
<i>Contact with Gang Members?</i>	1.262**	.390	.048	.132	.019	.048
<i>BSI Factor</i>	.175	.251	-.087	.085	-.002	.027
<i>Moral Disengagement</i>	.029	.056	-.050*	.021	.004	.006
<i>PSMI Score</i>	.225	.644	-.073	.241	-.064	.071
<i>WAI Factor Score</i>	-.980*	.393	.067	.129	.021	.037
<i>Consideration for Others</i>	-.473	.299	.109	.104	-.016	.030
<i>YPI Factor Score</i>	-.470	.293	.064	.103	.002	.031
<i>Logged Number of Caring Adults</i>	-1.453**	.491	-.102	.186	.014	.042
<i>Domains of Social Support</i>	.401*	.189	-.006	.065	-.003	.016
<i>Diversity of Caring Adults</i>	.002	.256	.074	.087	.021	.022
<i>Frequency of Substance Abuse</i>	.041	.084	-.001	.029	-.006	.008
<i>Variety of Substance Abuse</i>	-.612	.424	.143	.136	.026	.034
<i>Treated</i>	.067	.706	.092	.275	.064	.059
<i>Participation Group</i>						
<i>ONLY VOL</i>	-.052	1.089	-.648	.447	.006	.105
<i>ONLY INVOL</i>	-.037	1.389	-.197	.484	-.096	.124
<i>MIXED</i>	1.978	10.669	-.327	.847	.048	.173
<i>Group X Treatment</i>						
<i>1#ONLY VOL</i>	.202	1.198	.669	.470	.043	.112
<i>1#ONLY INVOL</i>	.007	1.435	.083	.488	.104	.120
<i>1#MIXED</i>	.152	1.746	.116	.767	-.088	.151
<i>Age Squared</i>	.006	.038	-.002	.013	.000	.004
<i>Number of Work Interruptions Sqrd.</i>	-.175	.314	-.009	.114	-.005	.016
<i>Missing BSI?</i>	-.281	.610	-.060	.202	.020	.054
<i>Missing Caring Adults?</i>	-1.216	1.146	-.088	.384	-.084	.107
<i>Missing Friends?</i>	.123	.661	.049	.241	.047	.070
<i>Participation Category X Treatment</i>						
<i>1#CONTROL</i>	.386	.604	-.137	.204	-.015	.053
<i>1#ONLY VOL</i>	.133	1.101	-.260	.372	-.033	.098
<i>1#ONLY INVOL</i>	.398	1.256	.124	.328	-.070	.089
<i>1#MIXED</i>	-1.628	10.719	.095	.697	.015	.151
<i>Average Illegal Emp Rate</i>	.014	.046	.009	.017	.016**	.005
<i>Average Variety of Illegal Work</i>	-2.081	2.290	-.498	.638	-1.173***	.224
<i>Average Gang Membership</i>	6.044	3.573	-.543	1.378	.751	.408
<i>Average Contact with Gang</i>	-1.273	.703	-.066	.270	-.163	.087
<i>Average PSMI</i>	-.071	1.075	-.159	.379	.032	.109
<i>Average Frequency of Subst. Abuse</i>	.040	.110	-.015	.036	.013	.009
<i>Average BSI Missing</i>	-.005	1.281	.209	.428	.017	.102
<i>Average WAI Factor</i>	.787	.570	-.111	.180	.007	.049
<i>Intercept</i>	3.411	15.358	.735	5.303	-.334	1.531

Table B-6: Full Random-Intercept Results for Property Offending Variety Scores

Full Random-Intercept Results - Property Offending Variety Score By Tertile of Post-Program Employment Rate						
Variables	Lower Tertile of Emp Rate		Middle Tertile of Emp Rate		Upper Tertile of Emp Rate	
	Coef	St. Err.	Coef	St. Err.	Coef	St. Err.
<i>Social Rewards from Crime</i>	-.028	.055	.005	.042	.017	.028
<i>Personal Rewards from Crime</i>	-.020	.027	.031	.017	.013	.011
<i>AWFL Opportunities Score</i>	-.040	.099	-.052	.077	.048	.058
<i>AWFL Expectations Score</i>	-.008	.071	-.029	.051	-.055	.037
<i>FOI Score</i>	-.052	.102	.082	.077	.020	.051
<i>MtS Score</i>	.003	.100	.022	.069	.026	.044
<i>Age at First Offense</i>	.007	.029	-.006	.024	.006	.013
<i>Age</i>	.032	.335	-.081	.242	.000	.169
<i>White</i>	-.069	.270	.050	.229	.013	.110
<i>Black</i>	-.127	.253	.055	.226	.014	.123
<i>Hispanic</i>	-.150	.244	.022	.222	.025	.112
<i>Other</i>						
<i>Male</i>	-.004	.139	-.069	.097	.005	.064
<i>Site (Phoenix=1)</i>	-.005	.144	.088	.108	.080	.069
<i>Parents' Social Capital</i>	.001	.005	.001	.003	-.001	.002
<i>Employment Rate</i>	-.001	.003	-.001	.002	.001	.001
<i>Illegal Employment Rate</i>	.012	.004	.000	.003	-.007	.002
<i>Variety of Illegal Work</i>	.657***	.164	1.377***	.123	1.411***	.100
<i>Logged Average Legal Wage Rate</i>	.115	.100	.007	.063	.007	.041
<i>Number of Work Interruptions</i>	.084	.165	.257*	.116	.112*	.055
<i>Number of Months in Recall</i>	.032	.023	-.009	.017	-.003	.011
<i>Interviewed in Secure Setting?</i>	-.167	.118	.060	.092	-.090	.083
<i>RPI Score</i>	-.089	.101	.088	.074	.022	.049
<i>Prop. Friends Ever Arrested</i>	.045	.284	-.009	.174	-.122	.108
<i>Prop. Friends Ever Jailed</i>	.019	.285	-.072	.176	.066	.116
<i>Member of a Gang?</i>	-.754	.482	-.796*	.355	.182	.280
<i>Contact with Gang Members?</i>	.178*	.085	.174**	.059	-.107*	.051
<i>BSI Factor</i>	-.005	.055	-.030	.041	.058	.031
<i>Moral Disengagement</i>	.032*	.013	.009	.010	-.015*	.007
<i>PSMI Score</i>	.223	.143	-.005	.115	.047	.081
<i>WAI Factor Score</i>	-.028	.087	-.080	.061	.020	.042
<i>Consideration for Others</i>	-.015	.065	.028	.049	.005	.034
<i>YPI Factor Score</i>	-.030	.065	-.001	.049	.053	.035
<i>Logged Number of Caring Adults</i>	-.001	.107	-.051	.084	-.017	.048
<i>Domains of Social Support</i>	.010	.041	.021	.030	-.011	.019
<i>Diversity of Caring Adults</i>	-.003	.055	-.011	.041	.038	.024
<i>Frequency of Substance Abuse</i>	.044*	.019	.060***	.014	.033***	.009
<i>Variety of Substance Abuse</i>	.176	.096	.024	.065	.055	.039
<i>Treated</i>	-.001	.148	.007	.125	-.009	.067
<i>Participation Group</i>						
<i>ONLY VOL</i>	.083	.241	.105	.209	-.089	.117
<i>ONLY INVOL</i>	.317	.294	-.068	.213	-.156	.139
<i>MIXED</i>	.291	1.468	-.474	.406	-.055	.192
<i>Group X Treatment</i>						
<i>1#ONLY VOL</i>	-.062	.264	-.041	.222	.185	.127
<i>1#ONLY INVOL</i>	-.465	.308	.062	.217	.113	.135
<i>1#MIXED</i>	-.046	.385	.266	.362	.069	.169
<i>Age Squared</i>	-.002	.008	.002	.006	.000	.004
<i>Number of Work Interruptions Sqrd.</i>	-.045	.071	-.084	.054	-.025	.019
<i>Missing BSI?</i>	-.034	.135	.018	.096	-.028	.062
<i>Missing Caring Adults?</i>	.055	.255	-.143	.181	-.027	.121
<i>Missing Friends?</i>	.030	.147	-.101	.113	-.009	.080
<i>Participation Category X Treatment</i>						
<i>1#CONTROL</i>	-.012	.130	.026	.095	-.057	.060
<i>1#ONLY VOL</i>	.025	.246	-.042	.178	-.116	.113
<i>1#ONLY INVOL</i>	-.060	.275	.041	.154	.026	.101
<i>1#MIXED</i>	-.256	1.468	.291	.337	-.062	.169
<i>Average Illegal Emp Rate</i>	-.014	.009	-.001	.008	.016**	.005
<i>Average Variety of Illegal Work</i>	-.510	.472	-1.533***	.283	-1.975***	.243
<i>Average Gang Membership</i>	.823	.774	1.162	.606	.039	.453
<i>Average Contact with Gang</i>	-.243	.152	-.230	.118	.041	.097
<i>Average PSMI</i>	-.191	.234	-.010	.179	-.036	.124
<i>Average Frequency of Subst. Abuse</i>	-.074**	.024	-.067***	.017	-.045***	.010
<i>Average BSI Missing</i>	.136	.279	.134	.200	.135	.116
<i>Average WAI Factor</i>	.079	.124	.084	.085	.031	.055
<i>Intercept</i>	.248	3.394	.404	2.475	-.360	1.725

Table B-7: Full Random-Intercept Results for Violent Offending Variety Scores

Full Random-Intercept Results - Violent Offending Variety Score By Tertile of Post-Program Employment Rate						
Variables	Lower Tertile of Emp Rate		Middle Tertile of Emp Rate		Upper Tertile of Emp Rate	
	Coef	St. Err.	Coef	St. Err.	Coef	St. Err.
<i>Social Rewards from Crime</i>	.006	.039	.024	.032	.003	.019
<i>Personal Rewards from Crime</i>	-.015	.019	.011	.013	.004	.008
<i>AWFL Opportunities Score</i>	-.037	.071	-.049	.057	.004	.040
<i>AWFL Expectations Score</i>	.050	.051	.004	.038	-.017	.026
<i>FOI Score</i>	-.022	.073	.069	.058	-.003	.035
<i>MtS Score</i>	-.018	.072	-.071	.051	-.013	.031
<i>Age at First Offense</i>	.015	.021	.013	.018	-.003	.009
<i>Age</i>	.137	.241	-.140	.181	.044	.117
<i>White</i>	-.072	.194	.058	.171	-.015	.077
<i>Black</i>	-.073	.182	.027	.169	-.034	.086
<i>Hispanic</i>	-.082	.175	.023	.166	-.060	.078
<i>Male</i>	.034	.100	-.068	.072	-.017	.044
<i>Site (Phoenix=1)</i>	.018	.104	.009	.081	-.001	.048
<i>Parents' Social Capital</i>	.000	.004	-.001	.002	.000	.002
<i>Employment Rate</i>	.001	.002	.000	.001	.000	.001
<i>Illegal Employment Rate</i>	.003	.003	-.002	.002	-.006	.001
<i>Variety of Illegal Work</i>	.299*	.118	.532***	.092	.535***	.070
<i>Logged Average Legal Wage Rate</i>	.056	.072	-.013	.047	.009	.028
<i>Number of Work Interruptions</i>	-.012	.119	.176*	.086	.053	.038
<i>Number of Months in Recall</i>	.041*	.017	.007	.013	.013	.008
<i>Interviewed in Secure Setting?</i>	.006	.085	.092	.069	-.025	.058
<i>RPI Score</i>	-.060	.073	.004	.056	-.011	.034
<i>Prop. Friends Ever Arrested</i>	.264	.204	-.010	.130	-.045	.075
<i>Prop. Friends Ever Jailed</i>	-.284	.205	-.001	.132	.096	.081
<i>Member of a Gang?</i>	-.573	.347	-.798**	.265	-.227	.194
<i>Contact with Gang Members?</i>	.096	.061	.154**	.044	.040	.036
<i>BSI Factor</i>	.065	.040	-.047	.031	.039	.022
<i>Moral Disengagement</i>	.004	.009	.008	.007	-.003	.005
<i>PSMI Score</i>	.213*	.103	.132	.086	.049	.056
<i>WAI Factor Score</i>	-.043	.062	-.071	.046	-.108***	.029
<i>Consideration for Others</i>	-.016	.047	.008	.037	-.007	.023
<i>YPI Factor Score</i>	.014	.047	.028	.036	.009	.025
<i>Logged Number of Caring Adults</i>	-.048	.077	-.087	.063	-.018	.033
<i>Domains of Social Support</i>	.026	.030	.017	.023	.006	.013
<i>Diversity of Caring Adults</i>	-.008	.040	.007	.030	.013	.017
<i>Frequency of Substance Abuse</i>	.038**	.014	.031**	.010	.015*	.006
<i>Variety of Substance Abuse</i>	-.021	.069	.029	.049	-.023	.027
<i>Treated</i>	.091	.107	.092	.094	.024	.047
<i>Participation Group</i>						
<i>ONLY VOL</i>	.241	.173	.116	.156	-.033	.081
<i>ONLY INVOL</i>	.108	.211	.012	.159	-.024	.096
<i>MIXED</i>	.161	1.057	.044	.304	-.004	.133
<i>Group X Treatment</i>						
<i>I#ONLY VOL</i>	-.281	.190	-.046	.166	.015	.088
<i>I#ONLY INVOL</i>	-.158	.222	-.039	.162	-.009	.093
<i>I#MIXED</i>	-.415	.277	-.044	.270	.040	.118
<i>Age Squared</i>						
<i>Number of Work Interruptions Sqrd.</i>	.015	.051	-.085*	.040	-.018	.013
<i>Missing BSI?</i>	-.015	.097	-.004	.072	-.116**	.043
<i>Missing Caring Adults?</i>	.040	.183	-.204	.135	-.065	.084
<i>Missing Friends?</i>	-.113	.106	-.100	.085	.083	.055
<i>Participation Category X Treatment</i>						
<i>I#CONTROL</i>	.023	.093	-.032	.071	-.018	.042
<i>I#ONLY VOL</i>	-.130	.177	-.075	.133	-.018	.078
<i>I#ONLY INVOL</i>	.024	.198	.077	.115	-.021	.070
<i>I#MIXED</i>	.001	1.057	-.045	.252	-.018	.118
<i>Average Illegal Emp Rate</i>						
<i>Average Variety of Illegal Work</i>	-.007	.007	.001	.006	.008*	.004
<i>Average Gang Membership</i>	-.238	.340	-.586**	.212	-.715***	.169
<i>Average Contact with Gang</i>	.576	.557	.857	.453	.071	.314
<i>Average PSMI</i>	-.125	.109	-.191*	.089	-.005	.068
<i>Average Frequency of Subst. Abuse</i>	-.225	.168	-.129	.134	-.093	.086
<i>Average BSI Missing</i>	-.032	.017	-.032*	.013	-.012	.007
<i>Average WAI Factor</i>	.046	.201	.142	.149	.191*	.080
<i>Intercept</i>	.088	.089	.103	.064	.162***	.038
	-1.223	2.443	1.418	1.850	-.246	1.199

Table B-8: Sensitivity Analysis for Offending Rates (Incl. Drugs) – Simple Model

<b>Sensitivity Analysis for Offending Rates (Incl. Drugs) - Only Demographic Variables and Non-Differenced Outcome</b>						
<b>Variables</b>	<b>Lower Tertile of Emp Rate</b>		<b>Middle Tertile of Emp Rate</b>		<b>Upper Tertile of Emp Rate</b>	
	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>
<b>Demographics</b>						
Age	-.347	.330	.090	.123	-.066	.062
White	-1.386	4.557	-.097	2.077	.407	.693
Black	.126	4.151	.314	2.049	.389	.751
Hispanic	.815	4.074	1.175	2.007	.488	.697
Male	2.500	2.548	1.287	.919	.397	.397
Site (Phoenix=1)	.131	2.098	-1.310	.869	-.050	.390
Parents' Social Capital	-.100	.077	-.032	.029	.010	.013
Treatment	1.761	1.902	.217	.771	.026	.369
	.000	.000	.000	.000	.000	.000
<b>Group</b>						
ONLY VOL	.219	3.911	1.530	1.585	.833	.656
ONLY INVOL	.381	3.871	-.664	1.386	-.222	.715
MIXED	-.727	16.088	.680	3.145	.225	1.103
<b>Group X Treatment</b>						
1#ONLY VOL	-.972	3.946	-2.351	1.436	-.432	.685
1#ONLY INVOL	-2.724	3.917	.485	1.267	.680	.652
1#MIXED	-1.307	6.114	-.716	2.337	.030	.898
<b>Participation Category X Treatment</b>						
1#CONTROL	2.224	1.564	.531	.599	-.133	.317
1#ONLY VOL	2.054	4.949	1.585	1.773	-.634	.730
1#ONLY INVOL	4.979	5.081	1.901	1.386	.749	.634
1#MIXED	1.097	16.351	-.349	3.181	-.317	1.087
<b>Intercept</b>	10.223	8.823	-.175	3.407	.351	1.520

Table B-9: Sensitivity Analysis for Offending Rates (Excl. Drugs) – Simple Model

<b>Sensitivity Analysis for Offending Rates (Excl. Drugs) - Only Demographic Variables and Non-Differenced Outcome</b>						
<b>Variables</b>	<b>Lower Tertile of Emp Rate</b>		<b>Middle Tertile of Emp Rate</b>		<b>Upper Tertile of Emp Rate</b>	
	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>
<b>Demographics</b>						
Age	-.061	.066	-.035	.044	-.025*	.012
White	-.451	.863	.128	.648	.167	.195
Black	-.569	.790	.283	.642	.138	.211
Hispanic	-.064	.774	.413	.627	.088	.196
Male	.839	.473	.526	.281	.130	.112
Site (Phoenix=1)	-.633	.401	-.551*	.271	-.092	.109
Parents' Social Capital	-.024	.015	-.003	.009	.003	.004
Treatment	-.345	.386	.163	.286	.021	.075
<b>Group</b>						
ONLY VOL	-.677	.748	1.155*	.531	.511**	.164
ONLY INVOL	-.651	.758	-.249	.480	.217	.174
MIXED	-.564	3.370	.215	1.018	.209	.281
<b>Group X Treatment</b>						
1#ONLY VOL	.012	.787	-1.817	.535	-.306	.128
1#ONLY INVOL	.286	.795	-.100	.478	-.107	.122
1#MIXED	.189	1.219	-.236	.870	-.061	.165
<b>Participation Category X Treatment</b>						
1#CONTROL	.417	.328	-.086	.230	.003	.058
1#ONLY VOL	1.804	.919	1.356*	.541	-.312	.205
1#ONLY INVOL	.197	.958	.217	.426	.060	.178
1#MIXED	-.095	3.426	-.178	.966	-.141	.303
<b>Intercept</b>	3.397	1.743	.745	1.143	.224	.354



Table B-10: Sensitivity Analysis for Property Offending Variety Scores – Simple Model

<b>Sensitivity Analysis for Property Offending Variety Scores - Only Demographic Variables and Non-Differenced Outcome</b>						
<b>Variables</b>	<b>Lower Tertile of Emp Rate</b>		<b>Middle Tertile of Emp Rate</b>		<b>Upper Tertile of Emp Rate</b>	
	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>
<b>Demographics</b>						
Age	-.036*	.017	-.024	.015	-.048***	.011
White	.453	.298	.042	.337	.117	.192
Black	-.060	.272	-.242	.332	.118	.209
Hispanic	.210	.266	-.259	.325	.215	.193
Male	.214	.169	.471	.152	.334	.113
Site (Phoenix=1)	-.199	.137	-.471**	.142	-.145	.108
Parents' Social Capital	-.004	.005	-.004	.005	-.004	.004
Treatment	.000	.096	-.069	.090	.026	.064
<b>Group</b>						
ONLY VOL	-.221	.235	.153	.233	.132	.156
ONLY INVOL	.151	.227	.147	.196	.465**	.167
MIXED	-.383	.960	1.180*	.480	.883**	.274
<b>Group X Treatment</b>						
1#ONLY VOL	.385	.189	-.073	.161	-.092	.108
1#ONLY INVOL	.092	.191	.014	.142	-.339**	.103
1#MIXED	.319	.294	-.388	.260	.012	.138
<b>Participation Category X Treatment</b>						
1#CONTROL	-.055	.073	-.052	.066	-.093	.048
1#ONLY VOL	-.113	.323	-.091	.293	-.073	.205
1#ONLY INVOL	-.692	.333	-.166	.228	-.257	.178
1#MIXED	.037	.990	-.864	.529	-.643*	.302
<b>Intercept</b>	1.392**	.501	1.486**	.495	1.195***	.330

Table B-11: Sensitivity Analysis for Violent Offending Variety Scores – Simple Model

<b>Sensitivity Analysis for Violent Offending Variety Scores - Only Demographic Variables and Non-Differenced Outcome</b>						
<b>Variables</b>	<b>Lower Tertile of Emp Rate</b>		<b>Middle Tertile of Emp Rate</b>		<b>Upper Tertile of Emp Rate</b>	
	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>	<i>Coef</i>	<i>St. Err.</i>
<b>Demographics</b>						
Age	-.048***	.011	-.021*	.010	-.040***	.007
White	.264	.207	-.395	.219	-.076	.112
Black	.030	.188	-.444*	.216	-.055	.122
Hispanic	.262	.185	-.403	.212	-.065	.113
Male	.253	.117	.354	.099	.299	.066
Site (Phoenix=1)	.048	.095	-.216*	.092	-.085	.063
Parents' Social Capital	-.003	.003	-.008**	.003	-.001	.002
Treatment	-.089	.065	-.119*	.060	-.057	.040
<b>Group</b>						
ONLY VOL	-.308	.162	-.020	.153	.087	.092
ONLY INVOL	-.076	.156	-.005	.129	.283**	.099
MIXED	-.353	.661	.384	.314	.099	.161
<b>Group X Treatment</b>						
1#ONLY VOL	.313	.127	-.071	.108	-.062	.068
1#ONLY INVOL	.140	.129	.057	.096	-.188**	.065
1#MIXED	.074	.198	-.221	.175	.303***	.087
<b>Participation Category X Treatment</b>						
1#CONTROL	-.035	.049	.020	.044	-.047	.030
1#ONLY VOL	.239	.224	.171	.190	.090	.120
1#ONLY INVOL	-.193	.231	-.085	.148	-.102	.104
1#MIXED	.259	.683	-.031	.344	-.107	.176
<b>Intercept</b>	1.349***	.343	1.729***	.325	1.083***	.198

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