

Racial Bias, Gender Bias, and the Effects of Parole Officers on Reentry

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Abstract:

Little is known about the effect of the parolee-officer relationship. In this work, I use the quasi-random assignment of paroled individuals to parole officers in Pennsylvania to evaluate the effects of individual parole officers on reentry success. First, I find that individual officers affect documented parole violations and employment but not future arrests. Second, leveraging the results of a survey of parole officers, I find that officers that prioritize interpersonal support reduce parole violations while officers that prioritize structural support have no effect on post-release success. Third, I find that parolees assigned to officers of other races or genders experience increased parole violations and decreased employment. However, I find no evidence that this bias is driven by specific officers. Instead, it is detected at a relatively constant rate across all parolee-officer pairings, suggesting it is due to systemic or societal bias as opposed to “a few bad apples.”

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1. Introduction

In 2019, 1.4 million individuals were incarcerated in State and Federal prisons in the United States, equivalent to 0.6% of the U.S. adult population (Carson 2020; U.S. Census Bureau 2019). In addition, post-release recidivism and unemployment rates are exceedingly high among previously incarcerated individuals – 71% are re-arrested within five years of release, 46% return to prison within five years of release (Durose & Antenangeli 2021), and 55% are unemployed eight months after release (Visser et al. 2008). Despite the size of the U.S. incarcerated population, we have little understanding of how to improve post-release success.

Most previously incarcerated individuals are released under parole supervision. For example, in 2019 74% of individuals released from State and Federal prisons were released early under some form of parole (Carson 2020). In addition, the rate of recidivism for paroled individuals is notably higher than the overall rate of post-release recidivism – in 2008 51% of individuals released under parole supervision in Pennsylvania were re-incarcerated within three years of release compared to 20% of individuals released at the expiration of their full sentence (Pennsylvania Department of Corrections 2013).

To what extent does parole supervision impact post-release success? Despite poor outcomes for individuals under community corrections supervision, almost no research exists on its effects (Doleac & LaForest 2022). Further, given the wide variation in community supervision policy across states and municipalities in the United States (Phelps & Curry 2017), a better understanding of which policies and procedures are most effective, and why, is needed to improve these programs and post-release outcomes for previously incarcerated individuals.

One of the main components of parole supervision is one's assigned parole officer (i.e., parole agent). Unlike other relationships in the criminal justice system, a parolee and parole officer develop a relationship over time and many repeated interactions (Chamberlain et al. 2017). Over time, parole officers provide interpersonal support to paroled individuals, build rapport with them, connect them with community re-entry programs, and convey a clear message of deterrence regarding future criminal behavior. Throughout the relationship, parole officers have a wide degree of discretion in how they build relationships with parolees, structure supervision, and how they respond when paroled individuals break parole conditions. For example, parole officers can choose whether to provide a written warning, assign additional conditions, or to re-incarcerate when they

observe a parole violation – decisions that on their own can have a large and lasting impact on a paroled individual's future.

This paper investigates the relative effects of individual parole officers on parolee outcomes, and the underlying mechanisms that drives those effects. Specifically, it answers three questions. First, to what extent do individual parole officers produce better or worse outcomes for paroled individuals relative to other officers? Second, what types of parole officer relationships have a positive effect on parolee outcomes? And third, to what extent does a parole officer's race and gender, relative to the paroled individuals they supervise, impact parolee success? And are their signs that these demographic-related effects are driven by individual or aggregate-level biases?

I answer these questions using individual-level data on the full Pennsylvania parole population between 2005 and 2019. I tease out the causal effects of parole officers by leveraging the quasi-random assignment of parole officers to parolees in Pennsylvania over this period. Upon release, a paroled individual is assigned to a parole officer unit based on his post-release residence location and special needs. However, within each unit, paroled individuals are assigned individual parole officers based exclusively on current officer caseload sizes.

I find that, first, individual officers have little relative effect on paroled individuals' future arrests or major parole violations that lead to reincarceration. However, individual parole officers do have a notable effect on paroled individuals' minor parole violations – that result in written warnings or additional parole conditions – and employment. Specifically, a one standard deviation increase in officer expected effect on minor parole violations increases the chance a parolee receives a minor violation in the first year after release by 5%. Additionally, a one standard deviation increase in officer expected effect on employment increases the chance a parolee is documented as employed, six months after release, by 8%.

Next, leveraging the results of a survey of parole officer perspectives, I find that officers that focus on interpersonal supports reduce minor parole violations. However, officers that focus on structural supports have no observable effect on minor parole violations relative to officers that do not. Neither type of focus appears to have an effect on future arrests or employment.

Finally, I find evidence of racial and gender bias in parolee-officer relationships that impacts recidivism, parole violations, and employment. Specifically, being assigned to an officer of a different racial background leads to slight increases in future recidivism (6%) and minor parole

violations (3%) and a slight decrease in employment (6%). Being assigned to an officer of a different gender has a similar but slightly stronger effect – an increase in recidivism (7%) and minor parole violations (12%) and a decrease in employment (8%). Further analysis finds that these differences in outcomes occur across nearly all parolee-officer relationships, at a fairly uniform rate, rejecting a hypothesis that this bias is driven by a subset of specific parole officers. Instead, the results support a hypothesis that this bias is driven by existing structural and societal bias that impact the relationships parolees and parole officers jointly construct.

Overall, the results imply that relative differences in parole officer approaches have little effect on future offending and reentry, regardless of whether officers focus on providing interpersonal support, structural support, or deterrence. Differences in parole officer approaches instead appear to affect minor parole violations and employment, of which agents that prioritize interpersonal supports seem to have most success.

The results also find evidence of racial and gender bias in parolee-officer relationships which impact parolee success. However, this bias appears to be driven by aggregate structural or societal bias which impacts the parolee-officer relationship, as opposed to bias driven by specific parole officers. As such, the results motivate a policy conclusion that these biases may not be able to be corrected at anything less than a societal level. However, if parole officers are made aware of these biases they can potentially work to reduce them when building relationships with assigned parolees.

Literature Review

This research adds to three sets of literature. The first is research on the effects of community supervision. Of the small set of existing studies, most conclude that additional community supervision has little corrective impact (Doleac & LaForest 2022). Regarding probation, several randomized controlled trials find no effect of specific intensive probation programs relative to standard programs (Petersilia & Turner 1993; Lane et al. 2005; Hennequelle et al. 2010; Hyatt & Barnes 2014; Barnes et al. 2012). Regarding parole, recent work by Banan (2022) compares the effects of release to parole supervision relative to release without supervision in North Carolina, using a regression discontinuity design. She finds that parole supervision leads to a short-term increase in parole violations that offsets a slight decrease in new crimes, with no long-term effects. Zapryanova (2020) investigates the effect of additional time spent on parole,

using the random assignment of judges in Georgia along with discontinuities in parole board guidelines, and finds no effect of additional time under parole supervision on returns to prison.

Next, Georgiou (2014) investigates the effects of parole supervision intensity levels in Washington State, using a regression discontinuity design around two risk score cutoffs that determine supervision intensity level, and finds no effect of higher supervision levels at either margin. Finally, LaForest (2022) separately identifies the effects of parole supervision intensity levels and special conditions such as curfew, placement in a halfway house, or enrollment in a drug treatment program using two discontinuities in risk score cutoffs that determine supervision level and the random assignment of parole hearing voters that determine special conditions in Pennsylvania. He finds that, along most margins, increased supervision leads to additional parole violations with little effect on future arrests or employment.

Only one prior study looks at the causal effects of parole officers. Specifically, Anderson and Wildeman (2015) leverage the random assignment of parolees to parole officers in Copenhagen and find that officers have an individual effect on dependency on public benefits and recidivism, but not employment, in Denmark. All other existing studies on the effects of parole officers is correlational or qualitative. Bares & Mowen (2019) conduct a matching analysis using a panel data survey and find a correlation between officers that provide professional support and decreased recidivism, but no correlation between interpersonal support and recidivism. Morash et al. (2016) find no effect of individual officers on female parolees, using a matching analysis. Finally, Chamberlin et al. (2018) leverage a survey of parolees and find that those that mentioned having better relationships with their parole officers had better parole outcomes.

The second set of literature this research contributes to is methodological – the use of “the randomized assignment of decision-makers” to evaluate the marginal effect of policy decisions. To date this work focuses almost exclusively on the quasi-random assignment of judges in a criminal court setting (e.g., Bhuller et al., 2020; Dobbie et al., 2018; Bhuller et al., 2018; Mueller-Smith, 2015; Loeffler, 2013). However, recent work has begun to apply this technique in other settings, such as the quasi-random assignment of prosecutors in a criminal court (Agan et al., 2021), the quasi-random assignment of police officers to calls for service (Weisburst, 2018), and the quasi-random assignment of child welfare investigators to child maltreatment investigations (Gross & Baron, 2021).

Finally, the third set of literature this research contributes to focuses on the effects of racial bias in the criminal justice system. Existing research largely focuses on how racial bias impacts police officer and judge decision-making, in one-off interactions with potential offenders. For example, Arnold et al. (2018) find evidence of racial bias in judge bail decisions in Miami and Philadelphia. Weisburst (2018) finds variation on arrest propensity across officers in Dallas, and Gonclaves & Mello (2021) find that minorities are less likely to receive discounts on their speeding tickets, driven by a subset of officers that practice discrimination, in Florida. Anwar & Fang (2006) test for racial bias in police motor vehicle searches in Florida, and find no evidence of extensive racial prejudice but cannot reject a hypothesis that officers exhibit relative racial prejudice. Finally, Kleinberg et al (2018) show that risk score algorithms that include racial demographic details can produce more equitable and efficient recommendations than algorithms that omit them in an attempt to be race-blind.

Section 2 discusses the parole process in Pennsylvania and the data. Section 3 describes the empirical models and estimates for parole officer effects. Section 4 discusses the parole officer survey and the correlation between survey responses and parole officer effects. Section 5 investigates racial and gender bias within parole officer – parolee relationships, and what appears to be driving the observed bias. Section 6 concludes.

2. Background & Data

In Pennsylvania, a state prisoner is eligible for parole after serving his minimum sentence, which is at least half of his full sentence. If paroled, he is released into the community to serve the remainder of his sentence under community supervision, pending good behavior. This supervision is, broadly speaking, defined by three factors. First, the parolee is assigned a parole officer to oversee his supervision. Second, the parolee must meet with his assigned parole officer a certain number of times each month, determined by his designated supervision intensity level. Third, the parolee is subject to parole conditions such as drug testing, employment requirements, fee payments, alcohol abstention, curfew, residency restrictions, program participation requirements, and restrictions on social contacts, that they must abide by at all times while on parole.

Assigned parole officers have wide discretion over the tenor of the parolee-officer relationship and whether and how to sanction the parolee for breaking conditions of parole. For

example, parole officers can focus more on the case worker aspects of their job – providing interpersonal support to paroled individuals, connecting them with community re-entry programs and services, etc. – or more on the correctional officer aspects of their job – providing surveillance and highlighting the harsh sanctions associated with future crimes. In addition, parole officers can choose whether and how to be flexible regarding when and where a paroled individual must meet with them, and when and whether to assign or remove additional parole conditions. Finally, when they observe a parole violation, officers have substantial discretion over whether to provide a written warning, assign additional parole conditions, or to re-incarcerate.

In Pennsylvania between 2005 and 2019 parolees were assigned to regional parole officer units based on two criteria – (1) the census block of the parolee’s residency (Philadelphia and Pittsburg) or zip code of the parolee’s residency (all other regions of the state) and (2) any special needs of the parolee, such as alcohol and other drugs (AOD) needs, sexual offender (SO) needs, or mental health (MH) needs. Within each specific officer unit, parolees were then quasi-randomly assigned to parole officers based only on officer caseload size at the time of release. Specifically, at the time of release a paroled individual was assigned to the parole officer in the unit with the smallest caseload at that time.²

Pennsylvania has the sixth highest state parole population, with 45,000 individuals on parole in their community at any given time, 15,000 of which enter and exit parole supervision each year. Each parolee falls under the jurisdiction of one of the states’ 10 parole district offices, and is assigned to one of 500 community supervision officers in the state. Each officer is assigned a mix of paroled individuals and probationers, with an average active caseload of around 60-100 for each officer. Data on prisoners, parole board hearings, and parole-related outcomes comes from the Pennsylvania Department of Corrections (DOC). Data on pre- and post-incarceration arrests comes from the Pennsylvania State Police.

Table 1 provides information on paroled individuals in Pennsylvania at the time of release. The majority of paroled individuals are male, just under half are Caucasian and just under half are Black. Nearly half have not completed a high school degree. Convicted crime type is fairly evenly

² Note that parolees are often reassigned to new officers at a later date, due to parole officers changing units, taking vacations, or specific parole officer inner-office decisions and requests. In this analysis I focus on initial officer assignment and am evaluating intent-to-treat effects. As such, note that the observed officer effect will be a lower bound of the full officer effect if no reassignments took place. Secondly, after one year on parole without incident parolees are eligible for a lower level of “administrative parole” in Pennsylvania. As such I focus on parole officer effects during the first one-year period of parole.

split between violent crimes, drug crimes, and property crimes. Finally, the average number of years left on an individual's sentence at the time of release is 2.5 years, and half of the individuals have served prior sentences in DOC custody.

Table 2 provides details on parolee post-release outcomes. Within one year of release, 19% are arrested for new crimes while on parole. 51% receive at least one parole violation. These violations are broken down into two types. First, 19% have a major violation – for which parole is revoked and the individual is sent back to prison. Second, 40% receive one or more minor parole violations – which result in either a written warning or the assignment of new, additional special parole conditions. In addition, only 37% of parolees are documented as employed six months after release, and only 48% have had at least one month of employment anytime during the first year after release.

3. Relative Parole Officer Effects

Specification 1 – Officer Fixed Effects

There are various ways to evaluate the extent to which parole officer assignment impacts post-release outcomes. One approach is to compare to extent to which individual officer fixed effects impact outcome estimates, relative to other observable characteristics about the parolee and parole. Specifically, let Y_{it} be the outcome for individual i after release at time t (reincarceration, parole violation, or employment), let X_{it} be a set of observable personal characteristics about the parolee and level of parole supervision,³ let U_{it} be the parolee's census block or zip code and special needs (which, together, uniquely identify a parolee's assigned parole officer unit), and let ε_{it} be a stochastic error term. The impact of observable personal characteristics on post-release outcomes, without taking into account individual parole officer effects, is given by vectors β_1 and β_2 in Equation 1:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 U_{it} + \varepsilon_{it} \quad . \quad (1)$$

³ Specifically, X_{it} includes parolee race, gender, educational attainment, marital status, convicted crime type, minimum sentence length (years), DOC facility of release, number of prior stays in DOC custody, year released to parole, month released to parole, age (years) at time of release, years left on sentence at time of release, and residency type at time of release (home, community corrections center, treatment facility). It also includes details regarding the individual's parole board hearing and stay on parole including Level of Service Inventory - Revised risk score, parole board hearing guidelines risk score, supervision level at time of release (minimum, medium, or maximum), and indicator variables for each assigned parole condition at time of release (e.g. curfew, must support dependents, can't contact prior co-defendants, etc.).

Next, let $Agent_i^j$ equal one if individual i is assigned to parole officer j at the time of release, and zero otherwise. The impact of observable personal outcomes when taking into account parole officer effects is given by vectors β_1 , β_2 , and β_j for each parole officer j in Equation 2:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 U_{it} + \sum_j (\beta_j Agent_i^j) + \varepsilon_{it} \quad (2)$$

Note that, to take into account changes in an officer's effect over time, I allow officer j fixed effects to vary across years. Additionally, note that the adjusted R^2 associated with an OLS regression of Equation 1 (AR_1^2) conveys the percent of variation in outcomes that can be ascribed to observable characteristics not including parole officer. Note, additionally, that the adjusted R^2 associated with an OLS regression of Equation 2 (AR_2^2) conveys the percent of variation in outcomes that can be ascribed to observable characteristics including parole officer. As such, $AR_2^2/AR_1^2 - 1$ represents the percent of total observed variation in outcomes attributable to assigned parole officers.

Specification 2 – Leave-One-Out Officer Effects

An alternative approach to evaluating the relative effect of individual parole officers leverages the literature on randomized decision makers (e.g., Bhuller et al., 2020; Dobbie et al., 2018; Agan et al., 2021; Weisburst, 2018). Given the “as-good-as-random” assignment of parolees to officers within parole officer units, I create “leave-one-out” expected effects for each officer, and then evaluate the “relevance” of these officer expected effects on each individual post-release outcome. Specifically, I first create residual measures of observed outcome Y_{it} for each parole stay, that net out fully interacted year and assigned parole officer unit fixed effects (U_{it}). These residual parole stay-level observed leniency measures, $ResidY$, are constructed as the residuals from an OLS regression of the equation

$$Y_{it} = \gamma_0 + \gamma_1 U_{it} + e_{it} \quad , \quad (3)$$

where e_{it} is a stochastic error term.

The residuals are then used to construct leave-one-out parole officer expected effects, defined as the average effect for officer (j) across all parolees (p) they are assigned to during the calendar year (n_j) except for the parolee of interest (i) and any other assignments of that parolee to that officer (n_{ji}):

$$V_{jt(-i)} = \left(\frac{1}{n_j - n_{ji}} \right) \left(\sum_{p=1}^{n_j} ResidY_p - \sum_{c=1}^{n_{ji}} ResidY_c \right) \quad . \quad (4)$$

Note that these leave-one-out expected effect measures are constructed separately for each parole officer each year to account for changes in specific officer practices and effects across time.

Finally, to estimate the effect of individual parole officers on each outcome we can regress outcome Y_{it} on the leave-one-out expected effect of assigned parole officer as well as all other observable information about the parolee:

$$Y_{it} = \alpha_0 + \alpha_1 V_{jt(-i)} + \alpha_2 X_{it} + \alpha_3 U_{it} + \epsilon_{it} \quad . \quad (5)$$

One additional benefit of creating leave-one-out expected officer effect measures is that they can be used to test our exogeneity assumption of random assignment to parole officers. Specifically, we can test the assumption that within-parole officer unit assignment is “as-good-as-random” and based only on parole officer caseloads. If this is the case, then in the aggregate data the leave-one-out measures should not be correlated with any other observable characteristics about the parolee. As shown in Table 3, officer assignments passes this exogeneity test. Specifically, f-tests for joint significance from regressions of leave-one-out officer outcome effects on all observable parolee characteristics produce p-values in the range of 0.10-0.70.

Results

Results for the “officer fixed effects” regressions are presented in Table 4. Columns 1 and 2 present the Adjusted R^2 ’s for regressions of several outcome variables, over the first year after release to parole, excluding and including officer fixed effects, respectively. As shown in Column 1, all known information about an individual on parole, other than assigned parole officer, can only explain about 10% of post-release outcomes. Controlling for randomly assigned parole officer increases R^2 by somewhere between zero and three percentage points, depending on the outcome, as shown in Column 4. Additionally, Column 3 presents the percent of adjusted R^2 attributable to the officer fixed effects. Overall, officers appear to have little relative effect on post-release recidivism, only explaining 2% and 5% of the explainable variation in arrest and parole violation reincarceration outcomes, respectively. However, officers appear to have a large relative effect on documented minor parole violations, that results in a written warning or new restrictions but do not result in reincarceration, and documented employment. Officers explain 16% and 15% of the explained variation in documented minor parole violations and employment, respectively, in the first year after release.

Results using “leave-one-out officer effects” reach the same conclusion, as shown in Figure 1. Each panel presents a histogram of leave-one-out effects for a different outcome, along with a local linear regression (LLR) of parolee outcome on assigned parole officer leave-one-out effects for that outcome. While all four histograms appear relatively normal, the local linear regressions for arrest (Panel 3) and parole violation reincarceration (Panel 4) are not monotonically increasing. The LLR for arrest shows no relationship between actual arrests and leave-one-out expected arrests. The LLR for parole violation reincarceration shows a slight relationship between actual and expected parole violations due to assigned parole officer, specifically for parole officers with expected impacts near the middle of the effect distribution, but displays wide oscillation in this relationship in each tail of the distribution. Based on these relationships, it appears that individual officers do not have a notable effect on returns to prison.

The LLRs for minor parole violations (Panel 1) and employment (Panel 2) show much stronger relationships. For both outcomes, the LLRs are close to monotonically increasing – as leave-one-out expected outcomes related to assigned officer for each measures increase, so do actual parolee outcomes. Specifically, a one standard deviation increase in officer expected effect on minor parole violations leads to a 5.5% increase in minor violations, and a one standard deviation increase in officer expected effect on employment leads to a 8.6% increase in employment. Based on these relationships, it appears that individual officers have a notable effect on minor parole violations and documented employment.

Do officers that increase documented employment also decrease minor parole violations? To investigate this I compare the leave-one-out measures for these outcomes, and find a correlation between the documented employment effect and documented minor violation effect of .10. This correlation implies that assignment to an officer that increases documented employment also appears to increase documented minor violations, at least to an extent.

These results can be interpreted in several ways. First, it does not appear that individual parole officer choices or relationships have a large effect on keeping released individuals out of prison, either by decreasing future arrests or parole violations that are serious enough to warrant re-incarceration (e.g., failing a drug test). Second, while it does appear that individual officers do have an effect on minor parole violations and documented employment, it is unclear what is driving these effects. For example, certain officers may build relationships with parolees that cause parolees to change their post-release behavior, impacting their propensity to break minor

conditions of parole or to find and keep work once released to the community. However, it is also possible that these officers have no effect on parolee behavior. Instead, it is possible that the observed effects in the data are instead driven by differences in officer reporting decisions. For example, certain officers may be more likely to write up and sanction parolees for violations, and certain officers may be more likely to follow up and vet whether parolee reported employment is factual, even if the officers have no direct impact on parolee behavior. As such, these results could be driven by differences in officer effects on parolee behavior, differences in officer reporting behavior, or a combination of both.

4. Mechanisms – Relationship between Officer Effects & Survey Results

To investigate why certain parole officers have a larger effect on post-release outcomes than others, I analyze the relationship between parolee outcomes and parole officers' self-reported approaches to parole supervision, as documented by a survey of parole officers. In Pennsylvania, the Department of Corrections surveyed all parole officers in three consecutive years – 2015, 2016, and 2017. The survey included 37 questions and statements about parole officers' job perspectives, such as "Reentry is part of supervision", "I build rapport with the parolees on my caseload", "I want to help parolees", and "I consider myself more of a case worker than a police officer" (the full set of questions is provided in Appendix A), scored on a scale of 1 (strongly disagree) to 5 (strongly agree). While parole officers were not required to complete the survey, response rates were fairly high – on average 59% of parole officers responded to the survey each year.

Letting S_i represent the vector of survey responses for the parole officer assigned to parolee i , the relationship between parole officer perspectives S_i and assigned parolee outcome Y_{it} is represented by β_s in Equation 6,

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 U_{it} + \beta_s S_i + \varepsilon_{it} \quad , \quad (6)$$

Where U_{it} is the parolee's assigned parole officer unit and X_{it} is a vector of other observable characteristics about the parolee. Given that the survey was only conducted between 2015 and 2017, I do not allow the effect of survey response to vary over time. Further, for parole officers who filled out the survey in multiple years only the first completed survey is used – results are appreciably similar if I instead use the most recent completed survey.

The impact of aggregate parole officer survey responses, by question type, on parolee outcomes is presented in Table 5. Generally, I find little effect of survey responses on recidivism, supporting the conclusion above that assigned parole officer appears to have little relative effect on recidivism in Pennsylvania. As before, I do observe an effect of parole officers on minor parole violations and employment. In terms of parole officer survey perspectives, the results suggest that parolees who are assigned to officers who focus on providing interpersonal support are less likely to receive minor documented parole violations in their first year post-release. Specifically, a one standard deviation increase in survey response to questions about whether having a good relationship with a parolee is important, or whether they help a parolee after the parole makes a mistake are each associated with a 2% decrease in minor parole violations in the first year after release.

However, the results suggest that parolees who are assigned to officers who focus on providing structural support have no differences in outcomes to those who are assigned to officers who do not focus on providing structural support. For example, a one standard deviation increase in survey response to questions about whether social work is part of a parole officer's job has no observable effect on minor parole violations or documented employment in the first year after release. Finally, parolees assigned to officers who believe current PBPP policies are reasonable have a 5% higher documented employment rate, and parolees assigned to officers who never responded to the optional survey have a 2% lower documented employment rate but also a 2% lower rate of receiving minor parole violations.

These results show that parolees assigned to parole officers with certain job perspectives have better post-release outcomes related to minor parole violations and documented employment. However, it is not clear whether the effect is driven by differences in job perspectives themselves, or something about certain officers that lead them to hold certain job perspectives. For example, these effects could be caused by officers holding certain job perspectives. If this is the case, then these results lead to a policy recommendation centered on parole officer training - that more officers should be taught these particular perspectives. Alternatively, however, these effects could be driven by unobserved characteristics of certain parole officers, which they bring to the job with them when they are hired and which happen to be correlated with their job perspectives. If this is the case, then these results lead to an policy recommendation centered on parole officer hiring - that more officers that exhibit these perspectives should be hired.

5. Racial & Gender Bias

Next, I look at the relationship between demographic characteristics of parole officers and parole officer effects, as well as the effect of relative differences in demographic characteristics between parole officers and parolees. First, as described in Equation 7, the relationship between parole officer observable characteristics, P_{it} , and parolee outcomes is defined by vector β_p ,

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 U_{it} + \beta_p P_{it} + \varepsilon_{it} \quad , \quad (7)$$

where P_{it} is comprised of parole officer race, gender, age, and years of service variables.

The correlations between parole officer demographic characteristics and parolee outcomes are presented in Table 6. Being assigned a male or Black officer has no effect on recidivism, but each lead to a decrease in minor parole violations as well as a decrease in documented employment, relative to being assigned a female or white officer. Additionally, parolees assigned officers with more years of service are less likely to have technical parole violations of each type during the first year post-release. Specifically, parolees assigned officers with one additional year of service are 0.7% less likely to have a parole violation that results in reimprisonment, and 0.6% less likely to have a minor parole violation that leads to a written warning or additional parole conditions. However, officer years of service appears to have little effect on parolee documented employment

Next, I explore the impact of differences in relative parolee-officer race and gender. To do so, I first create a binary variable for race, equal to one if a parole officer or parolee is (non-Hispanic) white, and equal to zero if they are not. Then, I create variables for the quadrant of parolee-officer race, and the quadrant of parolee-officer gender. For example, for gender this variable can be “male-male”, “male-female”, “female-female”, and “female-male”, with similar quadrants for race. Given these variables, I then estimate the effect of relative parolee-officer race and gender as β_q in Equation 8, where Q_{it} is a vector of the relative race and gender quadrant indicators of the parolee-officer relationship for parolee i :

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 U_{it} + \beta_p P_{it} + \beta_q Q_{it} + \varepsilon_{it} \quad , \quad (8)$$

Results on the aggregate effect of relative parolee-officer race, and relative parolee-officer gender are presented in Table 7. First, being assigned a parole officer of the opposite race (white vs. Black or Hispanic) appears to increase recidivism. Specifically, white parolees assigned to non-white officers have a 4% higher recidivism rate, on average, than white parolees assigned to white officers, after taking into account parole officer unit and all other observable characteristics

about each parolee. Additionally, non-white parolees assigned to white officers have around a 2% higher recidivism rate than non-white parolees assigned to non-white officers, leading to a net difference in recidivism of being assigned to an officer of the opposite race of 6%. Similar results appear for minor parole violations and documented employment – being assigned a parole officer of the opposite race leads to an increase in minor parole violations of 9% and a decrease in documented employment of 6%.

These results suggest aggregate-level evidence of racial bias in parolee-officer relationships. Note, however, that these effects could be driven by a variety of factors. If the bias is driven by parole officer actions, for example, some non-white officers could be treating white parolees more harshly than they should be, or black parolees more leniently than they should be. Instead, some white officers could be treating white parolees more leniently or black parolees more harshly than they should be. Any of these four hypothesis are consistent with the observed outcomes. Alternatively, this aggregate bias could be driven by how parolees themselves *respond* to being assigned parole officers of a different race. That is, parolees might respond differently to officers of different races, even if the officers themselves treat all parolees the same way. Finally, these effects could be caused by a joint mix of the two, if perceived differences in race impact the strength and quality of the supervisory relationship parole officers and parolees are able to, jointly, construct.

I observe similar aggregate effects of relative parolee-officer gender. Male parolees appear to have similar levels of recidivism regardless of whether they are assigned to male or female parole officers, but female parolees have a 7% lower recidivism rate when assigned to female parole officers, leading to a net difference in recidivism of being assigned an officer of the opposite gender of 7%. Similar results appear for minor parole violations and documented employment – being assigned a parole officer of the opposite gender leads to an increase in minor parole violations of 12% and a decrease in documented employment of 8%. Once again, it is unclear from these aggregate estimates whether the effects are driven by bias among certain parole officers, bias in parolee behavioral responses themselves, or combined structural bias that impacts the relationships that parolees and parole officers are able to construct.

Are Effects Driven by Certain Individuals, or Systemic?

I next explore the extent to which aggregate-level racial and gender biases in parolee-officer pairings are observable at the individual-level. To begin, I first look at whether there is evidence of a lack of monotonicity across parolee race and gender subgroups, in parole officer leave-one-out outcome effects. Second, I create and analyze explicit measures of individual-level differences in parole-officer effect across parolee racial and gender subgroups.

To investigate monotonicity of individual officer effects across racial and gender subgroups, I create leave-one-out measures of officer effect following Equations 3-5 separately for white parolees, non-white parolees, male parolees, and female parolees. Following (Bhuller et al., 2020) I then test for monotonicity in two ways. First, I test for within-subsample monotonicity by taking leave-one-out instruments constructed using the full sample, in regressions (Equation 5) restricted to the omitted subset. Second, I test for across-sample monotonicity by taking leave-one-out instruments constructed omitting the relevant subsample, in regressions restricted to the omitted subset. Results for both tests are provided in Table 8. Perhaps surprisingly, there is no evidence of an overall break in monotonicity within or across subsamples, when looking at officer effects on minor violations and documented employment. Further I find correlation between these subgroup specific effects. For example, the correlation between officer expected effects on white parolees and non-white parolees for minor parole violations is $r = .12$, and correlation between officer expected effects on white parolees and non-white parolees for employment six months after release is $r = .15$. In addition, the correlation between expected officer effects on male parolees and female parolees for minor parole violations is $r = .10$ and correlation between expected officer effects on male parolees and female parolees for employment six months after release is $r = .13$. Generally, these results show that individual officers do not appear to have notably different effects for different types of parolees (i.e., white vs. non-white, male vs. female).

Second, I construct measures of individual level differential effects across parolee race and gender subgroups. To do so, I first estimate parole officer effects on each subsample of parolees – white parolees, non-white parolees, male parolees, and female parolees – using Equations 3-5 (estimates using Equation 2 are appreciably similar). I then define the individual-level “white/non-white relative effect” of each parole officer as the difference in that parole officer’s effect on white parolees relative to their effect on non-white parolees. Similarly, I define the individual level “male/female relative effect” of each parole officer as the difference in that parole officer’s effect on male parolees relative to their effect on female parolees.

Distributions of these effects are displayed in Figure 2. Figure 2, Panel A presents the distribution in the white parolee/non-white parolee relative effect of each white parole officer (top histogram) relative to each non-white parole officer (bottom histogram) on recidivism, minor parole violations, and documented employment. These figures show no evidence of outlier officers with very large white parolee / non-white parole relative effects - the distribution of these effects for both white officers and non-white officers appear to be close to normally distributed. However, the means of these distributions are slightly different. Specifically, white officers have a mean slightly below non-white officers, signifying that white parolees assigned to them have a slightly lower propensity to incur minor parole violations, in the aggregate, than non-white parolees assigned to them, relative to outcomes for individuals assigned to non-white officers. The same is true, in the reverse direction, for documented employment – white parole officers have slightly higher average documented employment numbers for white parolees than non-white parolees, relative to non-white parole officers.

The same results are apparent for officers and parolees of different genders, as shown in Figure 2, Panel B. Once again, these figures show no evidence of outliers, the distributions appear to be close to normally distributed, and the only difference between the distributions is a slight shifting of the mean.

Overall, these results are consistent with the hypothesis that observed racial and gender bias in parolee – parole officer relationships is not caused by “a few bad apples.” The bias observed in the aggregate data appears to be fairly evenly distributed across officers, consistent with a hypothesis that these effects are driven by larger, systemic or structural biases that impact the way parolees and parole officers of different races and genders are able to trust and build relationships together.

6. Discussion

This work shows that individual parole officers do not appear to have a notable relative effect on parolee recidivism – whether through future arrests or parole violations that lead the reincarceration. However, individual officers do appear to have an effect on minor parole violations – that lead to written warnings or additional parole conditions – as well as documented employment. These effects are also important, as minor parole violations often lead to additional conditions which a paroled individual must abide by, and post-release employment rates are

particularly low for previously incarcerated individuals. Based on parole officer survey responses, officers that prioritize interpersonal supports are by-and-large the same officers that reduce violations, providing suggestive evidence that parolee-officer relationships that focus on interpersonal supports can be beneficial for paroled individuals. However, officers that prioritize structural supports do not appear to have a notable effect on parolee outcomes, providing suggestive evidence that these types of existing support services are not effective in facilitating reentry.

This work also finds evidence of racial and gender bias in parolee-officer relationships. Specifically, being assigned a parole officer of a different race or gender leads to slightly higher rates of recidivism, higher rates of minor parole violation, and lower employment. However, there is no evidence this bias is driven by individual officers. Instead, the evidence supports a hypothesis that these relative effects are driven by structural or societal biases that negatively impact the ability of a parolee and parole officer to build a strong and supportive relationship. As such, the results motivate a conclusion that these biases must be tackled at a more aggregate societal scale. However, parole officers should be aware of these potential biases so that they can actively work to reduce them when building parolee-officer relationships.

Overall, these results support a conclusion that the parolee-officer relationship. While changing the behavior of certain officers could reduce minor parole violations and improve documented employment, more work is needed to determine potential alternative reentry policies and programs that can improve a paroled individual's post-release outcomes and best facilitate their transition back into the community.

Tables & Figures

Table 1 – Parolee Summary Statistics

	Mean
Male	91%
Black	44%
Hispanic	11%
Age	35.7
Education - Less Than HS Degree	40%
Married	14%
Violent Crime Conviction	29%
Drug Crime Conviction	31%
Any Prior Stays	0.5
Medium Supervision Level	41%
High Supervision Level	40%
# Discretionary Parole Conditions	6.5
Years Left on Sentence at Release	2.5

Notes:

N = 148,588 releases to parole

Table 2 – Parolee Outcomes – 1 Year after Release

	Mean
Recidivism	37%
<i>Arrest</i>	19%
<i>PV Reincarceration</i>	19%
Minor Parole Violation	40%
Employed (Ever)	48%
Employed at 6 Months	37%

Notes:

N = 148,588 releases to parole

Table 3 – Leave-one-out Effect Exogeneity

	Joint f-test p-value
Recidivism	0.50
<i>Arrest</i>	0.70
<i>PV Reincarceration</i>	0.13
Minor Parole Violation	0.10
<i>Written Warning</i>	0.13
<i>New Restrictions</i>	0.48
Ever Employed	0.10
<i>Employed at 6 Months</i>	0.20
Notes:	
N = 148,588.	

Table 4 – Officer Effects Specification #1 – Officer Fixed Effects

	Adjusted R ² w/o Agent Fixed Effects	Adjusted R ² with Agent Fixed Effects	% Obs. Variation Due to Agent	% Total Variation Due to Agent
Recidivism	0.10	0.11	2%	0%
<i>Arrest</i>	0.08	0.08	2%	0%
<i>PV Reincarceration</i>	0.09	0.10	5%	1%
Minor Parole Violation	0.09	0.11	16%	2%
<i>Written Warning</i>	0.10	0.12	15%	2%
<i>New Restrictions</i>	0.05	0.05	9%	0%
Ever Employed	0.14	0.16	15%	2%
<i>Employed at 6 Months</i>	0.13	0.16	17%	3%

Notes:

N = 148,588.

Figure 1 – Officer Effects Specification #2 – Leave-one-out Effects

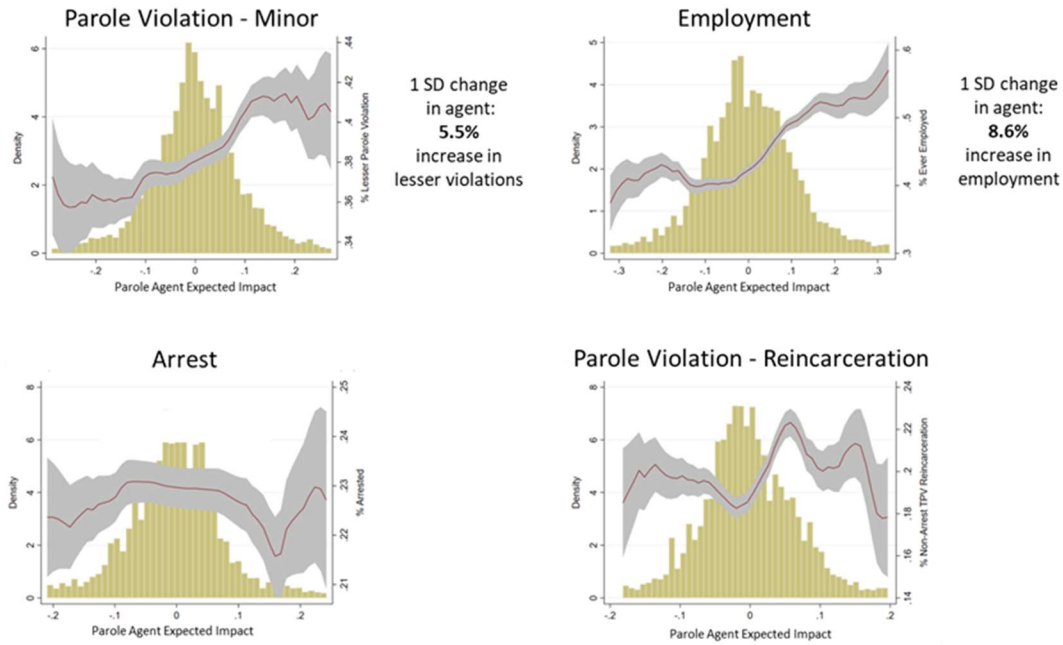


Table 5 – Relationship between Officer Effects and Survey Results

Survey Categories	Arrested % Change	PV Reincarceration % Change	Minor Parole Violation % Change	Employed at 6 Months % Change
Parolees Can Change Behavior	-2%	1%	1%	1%
Learn about Parolees	-1%	-1%	2% **	-2% **
Good Relationship	2%	0%	-2% **	-1%
Care about Parolees / Optimism	0%	1%	-2% **	0%
Social Work is Part of Job	0%	0%	0%	0%
Help Parolees After Mistakes	1%	-3% *	-2% ***	1%
Favorable Opinion of Parole Policies	0%	-1%	-1%	5% ***
Never Responded	0%	-2% **	-3%	-2% *

Notes:

N = 148,588 releases to parole

Estimates are the % change in outcome from a 1 SD increase in survey responses.

Table 6 – Aggregate Effects of Officer Demographic Characteristics

	Arrested % Change	PV Reincarceration % Change	Minor Parole Violation % Change	Employed at 6 Months % Change
Male Agent	1%	1%	-5% ***	-10% ***
Black Agent	0%	2%	-4% ***	-13% ***
Hispanic Agent	2%	9% ***	3%	-10% ***
Agent Years of Service	0.1%	-0.7% ***	-0.6% ***	0.1%

Notes:

N = 148,588 releases to parole

Table 7 – Signs of Aggregate Racial & Gender Bias

		Signs of Racial Bias		Signs of Gender Bias	
		White Agent	Non-White Agent	Male Agent	Female Agent
Recidivism					
	White Parolee	0	4% **	Male Parolee	0
	Non-White Parolee	7%	5%	Female Parolee	-19%
Arrest					-26% ***
	White Parolee	0	3%	Male Parolee	0
	Non-White Parolee	16%	15%	Female Parolee	-25%
PV Reincarceration					-42% ***
	White Parolee	0	3%	Male Parolee	0
	Non-White Parolee	-8%	-7%	Female Parolee	-10%
Minor Parole Violation					-7%
	White Parolee	0	-2%	Male Parolee	0
	Non-White Parolee	8%	3% ***	Female Parolee	-7%
Employed at 6 Months					3% ***
	White Parolee	0	-14% ***	Male Parolee	0
	Non-White Parolee	-18%	-28% ***	Female Parolee	-7%
					9% ***

Notes:

N = 148,588 releases to parole

Table 8 – Leniency Measure Monotonicity

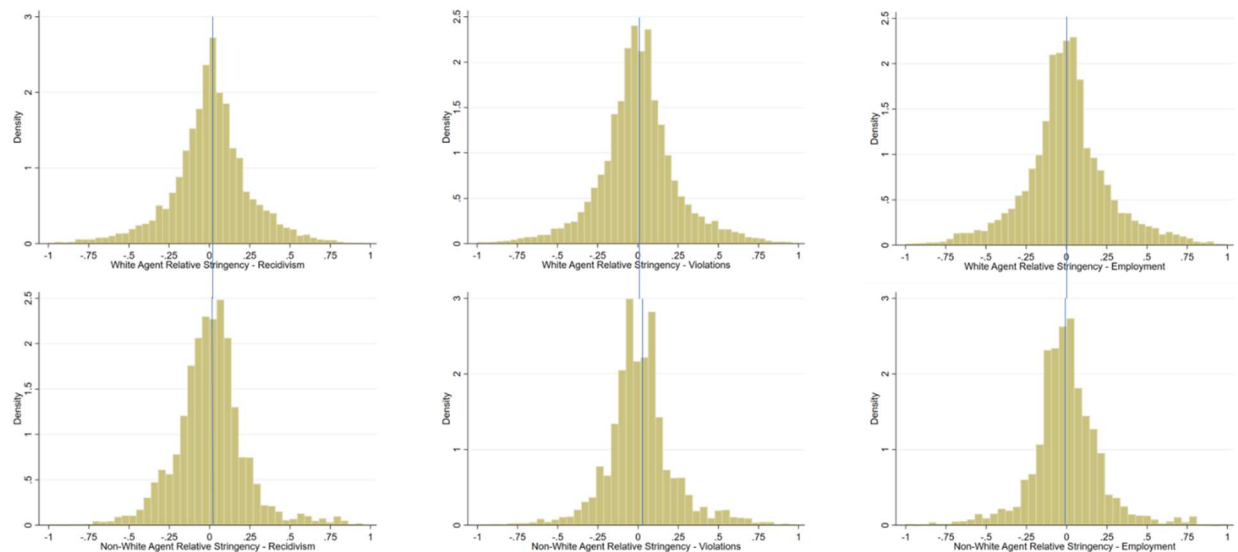
	Arrested	PV Reincarceration	Minor Parole Violation	Employed at 6 Months
A. Within-Subsample Monotonicity				
White Parolees	-0.09 ***	0.10 ***	0.24 ***	0.31 ***
Non-White Parolees	-0.11 ***	0.01	0.24 ***	0.38 ***
Male Parolees	-0.10 ***	0.07 ***	0.26 ***	0.36 ***
Female Parolees	-0.13 ***	0.04	0.20 ***	0.29 ***
B. Across-Subsample Monotonicity				
White Parolees	0.02	0.05 ***	0.12 ***	0.16 ***
Non-White Parolees	0.05 ***	0.06 ***	0.11 ***	0.15 ***
Male Parolees	0.00	0.03 ***	0.06 ***	0.09 ***
Female Parolees	-0.01	0.10 ***	0.24 ***	0.32 ***

Notes:

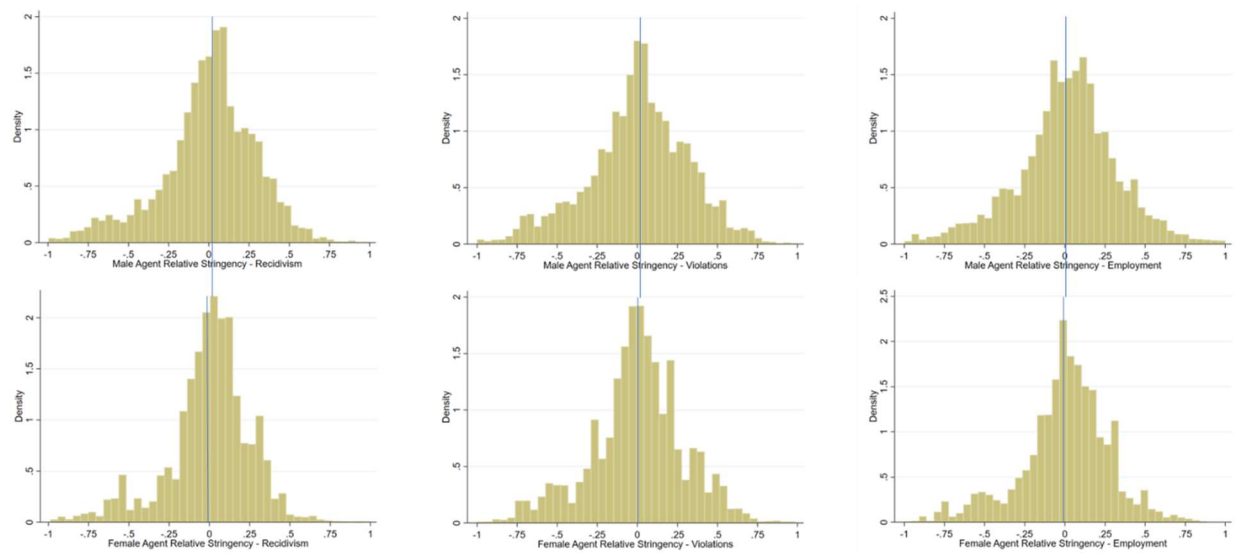
N = 148,588 releases to parole. Panels C and D present estimates and statistical significance (* <0.10, ** <0.05, *** <0.01) from regressions of several parole outcomes on leniency instruments for each outcome, controlling for observable characteristics about the individual, for different samples. Panel C presents estimates when leniency measures are created using the full set of data, but regressions are run using individual subsets of interest. Panel D presents estimates when leniency measures are created using the full set of data except for the subsample of interest, and regressions are run using only the subset of interest.

Figure 2 – Signs Racial Bias is Aggregate

Panel A – Difference in Officer Effect for White vs. Non-White Parolees



Panel B – Difference in Officer Effect for Male vs. Female Parolees



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Appendix A - Parole Officer Survey Questions

In my opinion...

Possible responses - Strongly Agree, Agree, No Opinion, Disagree, Strongly Disagree

1. Promoting pro-social behavior is an important part of my job as a parole officer/parole supervisor.
2. Building rapport with parolees is important to their success under supervision.
3. Knowing each parolee's current situation makes my job easier.
4. Having a good relationship with a parolee makes my job easier.
5. Current Parole Board policies are supportive of offender rehabilitation.
6. One of my roles as a parole officer/parole supervisor is to help parolees become successful members of society.
7. One of my roles as a parole officer/parole supervisor is to mentor parolees.
8. Parolees are unable to change their negative behaviors.
9. Parolees are capable of learning prosocial ways of thinking.
10. The workload demands set forth by Central Office are achievable.
11. Select "Strongly agree" here.
12. I consider myself more of a case worker than a police officer.
13. Supervision should be balanced between social work and law enforcement.
14. Reentry is part of supervision.

Consider your interactions with parolees and choose the most representative response:

Possible responses - Almost Always, Frequently, Sometimes, Occasionally, Hardly Ever

15. I care about parolees as people.
16. I am optimistic with parolees.
17. Parolees seem to feel comfortable enough to be open and honest with me.
18. I praise parolees for the good things they do.
19. I want to help parolees.
20. I consider parolees' views.
21. Parolees seem to keep important issues to themselves and won't tell me about them.
22. I think about the underlying reasons for parolee behavior.
23. When a parolee makes a mistake, I work with him to find out what brought him to that point.
24. I use a nonjudgmental approach in my interactions with parolees.
25. I match programming to a parolee's risk level.
26. I help parolees identify triggers and develop plans to effectively address those triggers.
27. I have frequent contact with parolees' support systems.
28. I demonstrate prosocial skills before asking a parolee to do so.
29. I am able to use risk level to apply appropriate interventions to each parolee.
30. I build rapport with the parolees on my caseload.
31. When parolees are going in a bad direction, I talk with them before taking serious action.
32. If a parolee breaks the rules, I calmly explain the consequences of his actions.

- 33. At the first contact, right after release from prison, parolees seem motivated to change.
- 34. Prior to maxing out of supervision, parolees seem hopeful about their future.
- 35. I include skill building discussions in each meeting with parolees.
- 36. I work with parolees to set goals for their personal improvement.
- 37. I can identify a parolee's criminogenic needs.

Appendix B – Auxiliary Tables and Figures

Table B1 - Relationship between Officer Effects and Individual Survey Question Responses

Survey Categories	Arrested % Change	PV Reincarceration % Change	Minor Parole Violation % Change	Employed at 6 Months % Change
Promote Prosocial	0%	3% **	0%	3% ***
Rapport Important	1%	4% ***	2% ***	0%
Know Parolee Situation	-3% **	-1%	-2% ***	-3% ***
Good Relationship	1%	-1%	2% **	2% *
Current Policies Supportive	0%	-1%	-2% **	3% ***
Help Success Society	2%	0%	1%	1%
Mentor Parolees	-1%	1%	1%	-2% *
Parolees Can Learn	-1%	-2%	0%	-2% *
Workload Achievable	-1%	-1%	2% **	4% ***
Case Worker	1%	-1%	-1%	0%
Supervision Balance	-1%	0%	1% *	-1%
Reentry Important	0%	3% *	0%	3% ***
Care Parolees	1%	2%	0%	0%
Optimistic with Parolees	-1%	-3% *	-1%	-1%
Parolees Comfortable	-1%	1%	-3% ***	1%
Treat Fairly	0%	0%	-1%	2% **
Praise for Success	-2%	2%	1%	0%
Want to Help	-1%	1%	-1%	0%
Consider Parolee Views	3%	0%	0%	1%
Think about Reason Behavior	0%	1%	3% ***	-1%
Work with Parolees	3%	-5% ***	0%	1%
Nonjudgemental	-1%	1%	0%	0%
Programming Match Risk	3%	0%	2% **	1%
Identify Triggers	-2%	-1%	-2% **	-2% *
Contact Support System	-1%	2%	-1%	2% *
Demonstrate Prosocial Skills	-1%	0%	2% ***	0%
Interventions Match Risk	-2%	1%	-1%	0%
I Build Rapport	3% **	-2%	-2% ***	-1%
Provide Warning	-2%	1%	0%	0%
Explain Consequences	1%	-1%	-2% ***	0%
Parolees Initially Motivated	1%	1%	-2% **	-1%
Parolees Seem Hopeful	-1%	1%	1%	-2% **
Skill Building Discussions	0%	-1%	-1%	-1%
Set Parolee Goals	1%	1%	0%	3% **
Identify Criminogenic Needs	0%	-3% **	0%	-4% ***
Parolees Can Change	-1%	-1%	0%	1%
Parolees Talk Issues	1%	-1%	0%	-1%

Notes:

N = 148,588 releases to parole

Estimates are the % change in outcome from a 1 SD increase in survey responses.