Racial Bias and the Effects of Parole Officers on Reentry

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Abstract:

Over 70% of U.S. prisoners are released to parole under the supervision of a parole officer, yet relatively little is known about the effects of this relationship. In this work, I use the randomized assignment of parole officers to parolees in Pennsylvania to evaluate the effects of assigned parole officers on post-release outcomes. First, I find that individual officers directly impact documented parole violations and employment but have little relative effect on new arrests. Leveraging the results of a survey of parole officers in Pennsylvania, I find that parolees assigned to officers that prioritize interpersonal support receive less documented parole violations than other parolees, but find little difference in effect between officers that prioritize structural supports and those that do not. Finally, I find that assignments of officers of the opposite race and sex both lead to increased documented parole violations and decreased documented employment after release. However, I find no evidence that this bias is driven by specific individual officers. Instead, this bias is detected at a relatively constant rate across all officers, suggesting that observed racial and gender bias in parolee-officer relationships 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). However, incarcerated individuals make up less than one-fourth of individuals under state correctional supervision in the United States. In 2019, for example, 74% of released previously incarcerated individuals were released under some form of parole supervision (Carson 2020), and, in total, an additional 1.4% and 0.3% of the U.S. adult population were living in the community under probation and parole supervision (Oudekerk & Kaeble 2021). Among previously incarcerated individuals, post-release recidivism and unemployment rates are exceedingly high– 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 (Visher et al. 2008). Further, the rate of recidivism for parolees is notably higher than the overall rate of recidivism – in 2008 in Pennsylvania 51% of individuals released under parole supervision 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).

Despite the massive scale of community correctional supervision in the United States, 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 the efficacy of community supervision programs.

One of the defining characteristics of parole is one's assigned parole agent (i.e., parole officer). Parole agents have a wide amount of discretion in how they build relationships with parolees and how they respond when parolees break parole conditions. For example, parole agents can choose whether to provide a parolee a written warning, assign additional conditions, or to re-incarcerate, a decision that can have a large and lasting impact on a parolee's future. In addition, the way in which a parole agent interacts with a parolee can, in theory, positively or negatively impact a parolee's future recidivism and process of re-integration into their community.

This paper investigates the relative effects that different parole agents have on parolee outcomes, and the underlying mechanisms that drives those effects. Specifically, the paper answers three questions. First, to what extent do different parole agents produce better or worse post-release outcomes for their assigned parolees, relative to other agents? Second, based on responses to a parole agent survey about their job perspectives, what type of parole agent mindsets appear to be most highly correlated with good parolee outcomes? And third, to what extent does a parole agent's age, sex, and race, relative to the parolee they supervise, impact parolee success? And are their signs that these relative demographic effects are driven by a small set of parole officers?

I answer these questions using individual-level data on the Pennsylvania parole population between 2005 and 2020. I tease out the causal effects of parole agents by leveraging the "as-good-as-random" assignment of parole agents to parolees in Pennsylvania, once assigned to a parole agent unit based on residence location and special needs.

First, I find that individual agents have little relative effect on a parolee's future arrests or parole violations that lead to reincarceration. However, parole agents do have a notable effect on a parolee's documented lesser parole violations, that result in written warnings or additional parole conditions, as well as documented employment. Specifically, a one standard deviation change in agent effect on lesser violations increases the chance a parolee receives a lesser violation in the first year after release by 5%, and a one standard deviation change in agent effect on documented 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 agent perspectives, I find that parolees assigned to agents that say they focus on interpersonal supports receive less lesser parole violations than other parolees, but parolees assigned to agents that say they focus on structural supports have similar outcomes to other parolees. Neither type of job perspective appears to be correlated with parolee recidivism outcomes.

Finally, I find evidence of aggregate-level racial and gender bias in parolee-parole agent relationships that impacts recidivism, violations, and documented employment. Specifically, being assigned to an agent of a different racial background leads to slight increases in future recidivism (6%) and lesser parole violations (3%) and a slight decrease in documented employment (6%). Being assigned to an agent of a different gender has a similar but slightly stronger effect – an increase in recidivism (7%) and lesser parole violations (12%) and a decrease in documented employment (8%). Further analysis supports the hypothesis that there results are not driven by a small number of biased parole agents. Instead, these differences in outcomes appear fairly uniformly across all parolee-parole agent relationships. Given the two-way nature of the parolee-parole agent relationship, the results support a hypothesis that these effects are driven by structural and existing societal bias that impact the relationship a parolee and parole agent jointly create.

Literature Review

This research adds to three sets of literature. The first set of research focuses on the effects of parole supervision. There is little causal evidence about its effects. First, work by Kuziemko (2013) finds that additional prison time decreases recidivism by exploiting discontinuities in parole board guidelines in Georgia and the effect of a mass prisoner release in 1981. However, she also finds that a policy reform in 1998 that removed the option of parole for certain offenders, predominately those convicted of robbery and assault, led to increased disciplinary infractions in custody and *increased* recidivism rates after release for this group. She posits that the cause of this increased recidivism is that, without the opportunity for early release, incarcerated individuals do not invest in their own rehabilitation while incarcerated.

Next, Solomon et al. (2005) conduct a matching analysis and find no effects of parole supervision, relative to unsupervised release, in California. Grattet & Lin (2014) evaluate the effects of parole supervision intensity across the United States, using a matching analysis, and find that increased supervision intensity increases absconding violations relative to other types of violations. Several additional studies have investigated specific community supervision programs. Specifically, Petersilia & Turner (1993) conducted a randomized control trial of an intensive supervision program in California and found that the program had no effect on new arrests but led to an increase in re-incarceration for technical violations. Schaefer & Little (2019) conducted a matching analysis of a parole model in Australia that focuses on opportunity-reduction strategies and found that the program decreased recidivism.

Little literature exists on the specific impact of parole agents. Anderson and Wildeman (2015) leverage the random assignment of parolee to parole agents in Copenhagen and find that agent have an differential effect on dependency on public benefits and recidivism, but not employment, in that setting. The only other research that exists on the effect of parole agents is correlational. Bares & Mowen (2019) conduct a matching analysis using a panel data survey and find a correlation between agents that provide professional support and decreased recidivism, but no correlation between recidivism and interpersonal support. Morash et al. (2016) find no effect of individual agents 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 agents had better parole outcomes.

The second set of literature focuses on racial bias in the criminal justice system. Existing research largely focuses on how racial bias impacts police officer and judge decision-making. 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.

Lastly, the third set of research 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).

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

2. Background & Data

Pennsylvania has the sixth highest state prison and parole population, with 45,000 individuals on parole in their community, 15,000 of which enter and exit parole supervision each year. 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 agent with wide discretion over the tenor of the parolee / agent relationship and whether and how to sanction the parolee for breaking conditions of parole. Second, the parolee must meet with his assigned parole agent a certain number of times each month, determined by his designated supervision intensity level. Third, the parolee is subject to mandatory parole conditions such as drug testing, employment requirements, fee payments, and a requirement to abstain from alcohol, as well as discretionary parole conditions such as curfew, residency restrictions, program participation requirements, and restrictions on social contacts, that they must abide by at all times while on parole.

Each parolee is under the jurisdiction of one of the states' 10 parole district offices, and assigned to one of 500 community supervision agents in the state. Each agent is assigned a mix of parolees and probationers, with an average active caseload of around 100 for each agent. In Pennsylvania between 2005 and 2019 parolees were assigned to parole agent 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 unit, parolees were then "as-good-as-randomly" assigned to parole agents based only on agent caseload size at the time of release.

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. The main explanatory variable of interest is initial parole agent assignment, and the main outcome period of interest is the first year upon release to parole.[2]

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

[2] Note that, as some parolees are reassigned new agents at a later date, due to a parole agent leaving on vacation or other parole agent inner-office decisions, the observed agent effect in this analysis is ultimately be a lower bound of the full agent 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 effects during the first year of parole in this analysis.

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

Black. Nearly half have not completed a high school degree. Convicted crime type is fairly evenly 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% of parolees are returned to prison for parole violations, 19% are arrested for new crimes while on parole, and 40% receive one or more lesser parole violations that result in a written warning or an 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 Agent Effects

Specification 1 – Agent Fixed Effects

There are various ways to evaluate the extent to which parole agent assignment impacts post-release outcomes. One approach is to compare to extent to which individual agent 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

Table 2 – Parolee Outcomes

	Mean
Recidivism	37%
Arrest	19%
Non-Arrest TPV Reincarceration	19%
Lesser Parole Violation	40%
Employed (Ever)	48%
Employed at 6 Months	37%

Notes:
N = 148,588 releases to parole

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 agent 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 agent effects, is given by vectors β_1 and β_2 in Equation 1:

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

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

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

Note that, to take into account changes in an agent's effect over time, I allow agent 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 agent. 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 agent. As such,

[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 or 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.).

$AR_2^2/AR_1^2 - 1$ represents the percent of total observed variation in outcomes attributable to assigned parole agents.

Specification 2 – Leave-One-Out Agent Effects

An alternative approach to evaluating the relative effect of individual parole agents 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 agents within parole agent units, I create "leave-one-out" effects for each agent, and then evaluate the "relevance" of these agent effects on post-release outcomes. Specifically, I first create residual measures of observed outcome Y_{it} for each parole stay, that net out fully interacted year and assigned parole agent 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

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

where e_{it} is a stochastic error term.

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

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

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

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

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

Exogeneity

One additional benefit of creating leave-one-out expected agent effect measures is that they can be used to test our exogeneity assumption of random assignment to parole agents. Specifically, that within-parole agent unit assignment is "as-good-as-random" and based only on parole agent 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, this appears to be the case – f-tests for joint significance from regressions of leave-one-out agent effects of each outcome on all observable characteristics produce p-values in the 0.10-0.70 range, providing evidence that agent assignment is indeed "as-good-as-random."

Results

Results for the "agent 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 agent fixed effects, respectively. As shown in Column 1, all known information about an individual on parole, other than assigned parole agent, can only explain about 10% of his post-release outcomes. Controlling for randomly assigned parole agent 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 agent fixed effects. Overall, agents appear to have little relative effect on post-release recidivism, only explaining 2% and 5% of the explainable variation in arrest and non-arrest TPV reincarceration outcomes, respectively. However, agents appear to have a large relative effect on lesser parole violations, that results in a written warning or new restrictions but do not result in reincarceration, and documented employment. Agents explain 16% and 15% of the explained variation in lesser parole violations and documented employment, respectively, in the first year after release.

Results using leave-one-out agent 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 agent 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 due to assigned parole agent. The LLR for parole violation reincarceration shows a slight relationship between actual and expected parole violations due to assigned parole agent, specifically for parole agents with expected impacts near the middle of the effect distribution, but

Table 3 – Leave-one-out Effect Exogeneity

	Joint f-test p-value
Recidivism	0.50
Arrest	0.70
Non-Arrest TPV Reincarceration	0.13
Lesser Parole Violation	0.10
Written Warning	0.13
New Restrictions	0.48
Ever Employed	0.10
Employed at 6 Months	0.20

Notes:
N = 148,588.

Table 4 – Agent Effects Specification #1 – Agent 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%
Arrest	0.08	0.08	2%	0%
Non-Arrest TPV Reincarceration	0.09	0.10	5%	1%
Lesser Parole Violation	0.09	0.11	16%	2%
Written Warning	0.10	0.12	15%	2%
New Restrictions	0.05	0.05	9%	0%
Ever Employed	0.14	0.16	15%	2%
Employed at 6 Months	0.13	0.16	17%	3%

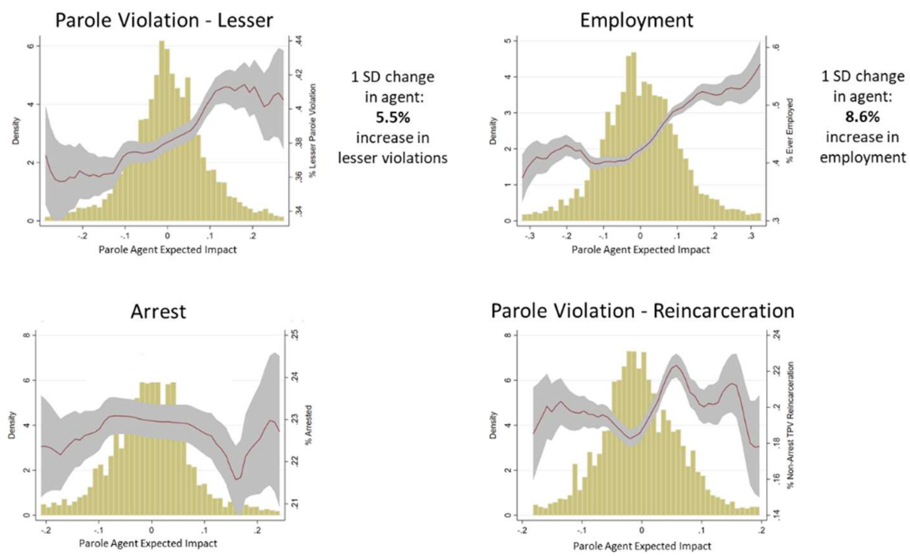
Notes:
N = 148,588.

displays wide variation in this relationship in each tail of the distribution. Based on these results, it does not appear that individual agents have a notable effect on recidivism.

The LLRs for lesser parole violations (Panel 1) and employment (Panel 2) look much better. For both outcomes, the LLRs are close to monotonically increasing – as leave-one-out expected outcomes related to assigned agent for these measures increase, so do actual outcomes. Specifically, a one standard deviation increase in agent expected effect on lesser parole violations leads to a 5.5% increase lesser violations, and a one standard deviation increase in agent expected effect on employment leads to a 8.6% increase in employment. Using either specification, the data suggest that individual agents impact lesser parole violations and documented employment.

Do agents that generate higher documented employment also generate lower lesser parole violations? To investigate this I compare leave-one-out measures for each individual agent for

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



these outcomes, and find a correlation between the employment measure and the lesser violation measure of .10. This correlation implies that assignment to an agent that maximizes documented employment also maximizes lesser violations, at least to an extent.

These results can be interpreted in several ways. First, it does not appear that individual parole agent 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 agents do have an effect on lesser parole violations and documented employment, it is unclear what is driving these effects. For example, certain agents may build relationships with parolees that cause parolees to change their post-release behavior, impacting their propensity to break lesser conditions of parole or to find and keep work once released to the community. However, it is also possible that these agents have no effect on parolee behavior. Instead, it is possible that the observed effects in the data are instead driven by differences in agent reporting decisions. For example, certain agents may be more likely to write up and sanction parolees for violations, and certain agents may be more likely to follow up and vet whether parolee reported employment is factual. As such, these results could be driven by differences in agent reporting behavior as opposed to effects on parolee behavior.

4. Mechanisms – Relationship between Agent Effects & Survey Results

To investigate why certain parole agents have a larger effect on post-release outcomes than others, I analyze the relationship between parolee outcomes and parole agents' self-reported approaches to parole supervision, as documented by a survey of parole agents. In Pennsylvania, the Department of Corrections surveyed all parole agents in three consecutive years – 2015, 2016, and 2017. The survey included 37 questions and statements about parole agents' 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 agents were not required to complete the survey, response rates were fairly high – on average 59% of parole agents responded to the survey each year.

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

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

Where U_{it} is the parolee's assigned parole agent 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 agents 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 agent 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 parole agents appear to have little differential effect on recidivism in Pennsylvania. However, as before I observe an effect of individual parole agents on documented lesser parole violations and employment. In terms of parole agent survey perspectives, the results suggest that parolees who are assigned to agents who focus on providing interpersonal support are less likely to receive lesser 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 lesser parole violations in the first year after release.

Table 5 – Relationship between Agent Effects and Survey Results

Survey Categories	Arrested % Change	Non-Arrest TPV Reincarceration % Change	Lesser 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.

However, the results suggest that parolees who are assigned to agents who focus on providing structural support have no differences in outcomes to those who are assigned to agents 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 agent's job has no observable effect on lesser parole violations or documented employment in the first year after release.

Finally, parolees assigned to agents who believe current PBPP policies are reasonable have a 5% higher documented employment rate, and parolees assigned to agents who never responded to the optional survey have a 2% lower documented employment rate but also a 2% lower rate of receiving lesser parole violations. Note, however, that while these results show that parolees assigned to parole agents with certain job perspectives have better post-release outcomes related to lesser parole violations and documented employment, they do not explain *why* this is the case. For example, these effects could be driven by an agent holding certain job perspectives, leading to a policy recommendation centered on parole agent training and policies – that more agents should be taught these perspectives. Alternatively, however, these effects could be driven by other unobserved characteristics of parole agents, which certain parole agents bring to the job with them when they are hired and which happen to be correlated with those agent's job perspectives. If this is the case, then these results lead to a recommendation centered on parole agent hiring – that more agents that exhibit these perspectives should (be attempted to) be hired.

5. Racial & Gender Bias

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

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

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

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

Finally, I explore the impact of differences in relative parole agent–parolee race and gender. To do so, I first create a binary variable for race, equal to one if a parole agent or parole is (non-Hispanic) white, and equal zero if they are not. Then, I create variables for the quadrant of parolee – parole agent race, and the quadrant of parolee – parole agent gender. For example, for gender this variable can take the form of “male-male”, “male-female”, “female-female”, and “female-male”, with similar quadrants for race. Given these variables, I can estimate the effect of relative parole agent – parolee 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-parole agent relationship for parolee i:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 U_{it} + \beta_p Q_{it} + \varepsilon_{it} \quad , \tag{8}$$

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

Table 6 – Aggregate Effects of Agent Demographic Characteristics

	Arrested		**Non-Arrest TPV Reincarceration**		**Lesser Parole Violation**		**Employed at 6 Months**	
	% Change		% Change		% Change		% 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	0%	
	Non-White Parolee	7%	5%		Female Parolee	**-19%**	**-26%**	***
Arrest								
	White Parolee	0	3%		Male Parolee	0	0%	
	Non-White Parolee	16%	15%		Female Parolee	**-25%**	**-42%**	***
Non-Arrest TPV Reincarceration								
	White Parolee	0	3%		Male Parolee	0	1%	
	Non-White Parolee	-8%	-7%		Female Parolee	-10%	-7%	
Lesser Parole Violation								
	White Parolee	0	-2%		Male Parolee	0	-2%	
	Non-White Parolee	**8%**	**3%**	***	Female Parolee	**-7%**	**3%**	***
Employed at 6 Months								
	White Parolee	**0**	**-14%**	***	Male Parolee	**0**	**8%**	***
	Non-White Parolee	**-18%**	**-28%**	***	Female Parolee	**-7%**	**9%**	***

Notes:
N = 148,588 releases to parole

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

These results suggest aggregate-level evidence of racial bias in parolee – parole agent relationships. Note, however, that these effects could be driven by a variety of factors. If the bias is driven by parole agent actions, for example, some non-white agents could be treating white parolees more harshly than they should be, or black parolees more leniently than they should be, or some white agents 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 agents of a different race. Finally, these effects could be caused by perceived differences in race impacting the strength and quality of the supervisory relationship parole agents and parolees are able to, jointly, construct.

I observe similar effects related to relative parolee-agent gender. Male parolees appear to have similar levels of recidivism regardless of whether they are assigned to male of female parole agents, but female parolees have a 7% lower recidivism rate when assigned to female parole agents, leading to a net difference in recidivism of being assigned an agent of the opposite gender of 7%. Similar results appear for lesser parole violations and documented employment – being assigned a parole agent of the opposite gender leads to an increase in lesser 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 agents, bias in parolee behavioral responses themselves, or combined structural bias that impacts the relationships that parolees and parole officers are able to build together.

Are Effects Driven by Certain Individuals or Systemic?

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

To investigate monotonicity of individual agent effects across racial and gender subgroups, I create leave-one-out measures of agent 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 agent effects on lesser violations and documented employment. Further I find correlation between these subgroup specific effects. For example, the correlation between agent expected effects on white parolees and non-white parolees on lesser parole violations is r = .12, and correlation between agent expected effects on white parolees and non-white parolees on employment six months after release is r = .15. In addition, the correlation between expected agent effects on male parolees and female parolees on lesser parole violations is r = .10 and correlation between expected agent effects on male parolees and female parolees on employment six months after release is r = .13. Generally, these results show that individual agents do not appear to have relatively 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 agent effects on each subsample of parolees – white parolees, non-white parolees, male parolees, and female parolees – using either Equation 2 of Equations 3-5. I then define the individual-level "white/non-white relative effect" of each parole agent 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 agent as the difference in that parole agent'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 agent (top histogram) relative to each non-white parole agent (bottom histogram) on recidivism, lesser parole violations, and documented employment. These figures show no evidence of outlier agents 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 agents have a mean slightly below non-white agents, signifying that white parolees assigned to them have a slightly lower propensity to incur lesser parole violations, in the aggregate, than non-white parolees assigned to them, relative to outcomes for individuals assigned to non-white agents. The same is true, in the reverse direction, for documented employment – white parole agents have slightly

Table 8 – Leniency Measure Monotonicity

	Arrested		Non-Arrest TPV Reincarceration		Lesser Parole Violation		Employed at 6 Months	
A. Within-Subsample Monotonicity								
Whilte Parolees	-0.09	***	0.10	***	0.24	***	0.31	***
Non-Whilte 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								
Whilte Parolees	0.02		0.05	***	0.12	***	0.16	***
Non-Whilte 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, controling 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.

higher average documented employment numbers for white parolees than non-white parolees, relative to non-white parole agents.

The same results are apparent for agents 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.

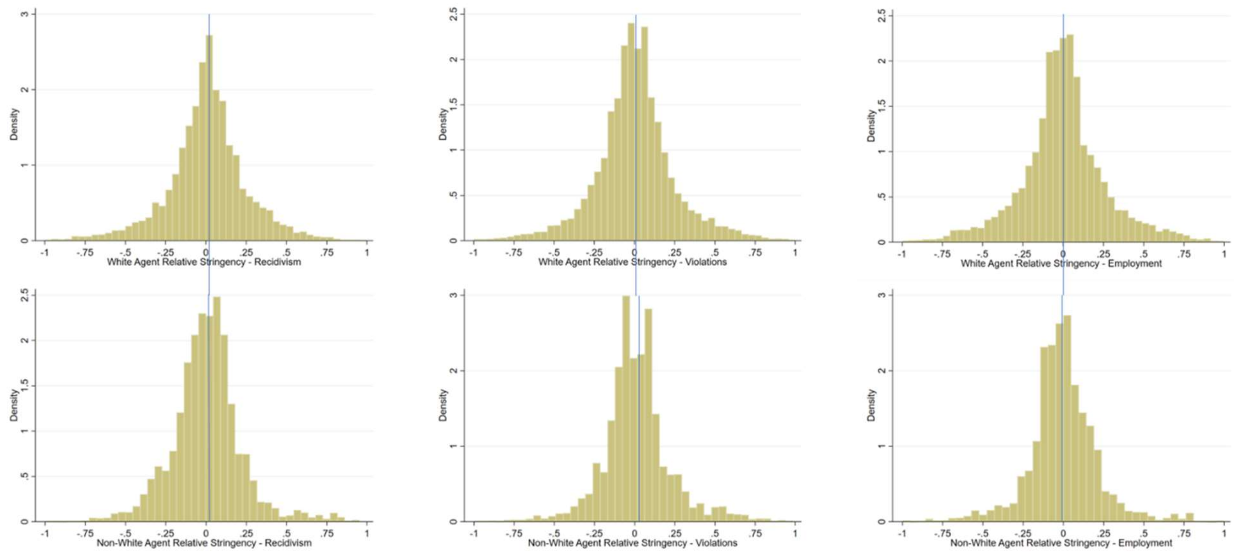
Overall, these results are consistent with the hypothesis that observed racial and gender bias in parolee – parole officer relationships is not caused by "a few bad apples." The bias observed in the aggregate data appears to be fairly evenly distributed across officers, consistent with a hypothesis that 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

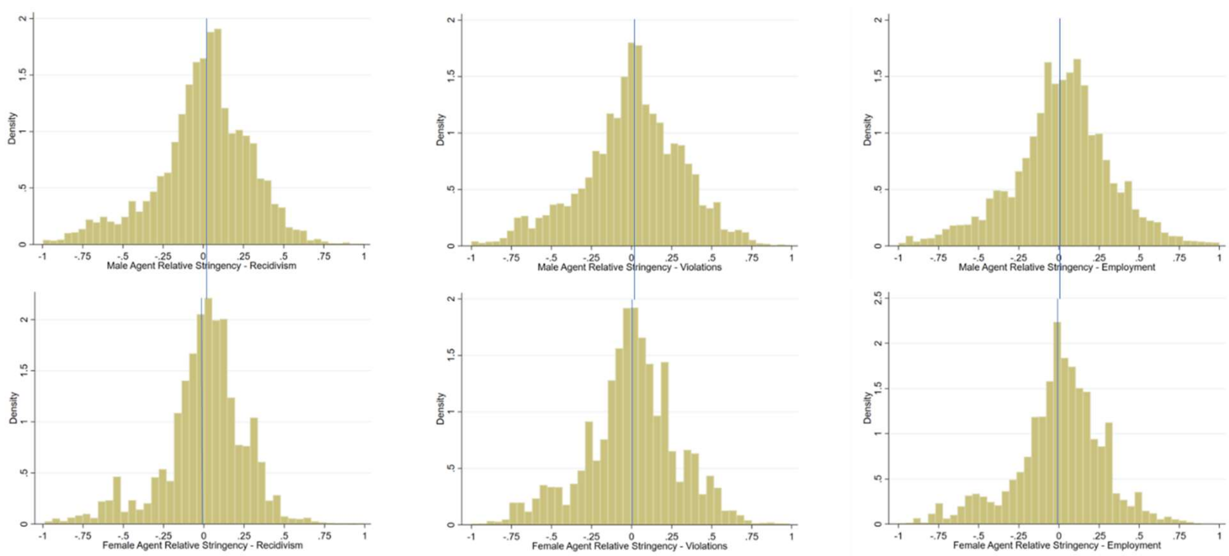
This work shows that individual parole agents do not appear to have a relative effect on parolee recidivism – whether through future arrests or parole violations that lead the reincarceration. However, individual agents do appear to have an effect on lesser parole violations, that lead to written warnings or additional parole conditions, as well as documented employment.

Figure 2 – Signs Racial Bias is Aggregate

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



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



Based on parole agent survey responses, it appears that assignment of agents that prioritize interpersonal supports lead to less lesser parole violations, while assignment of agents that prioritize structural supports appear to have no notable effect on post-release outcomes relative to other agents. However, it is unclear exactly what is causing these effects, and, specifically, whether parole agents with certain perspectives change parolee behavior or just respond differently to parolee behavior.

Regarding racial and gender bias, there is evidence of aggregate level racial and gender bias in parolee-parole agent relationships – being assigned a parole agent of a different race or gender leads to slightly higher rates of recidivism, higher rates of documented lesser parole violation, and slightly lower documented employment. However, there is no evidence this bias is driven by individual agents. Instead, the evidence supports a conclusion that these relative effects are driven by structural or societal biases that negatively impact the ability of a parolee and parole agent to build a strong and supportive relationship.

In terms of policy implications, these results support a conclusion that poor parolee outcomes can not be improved by solely changing the behavior of certain parole agents. While changing the behavior of certain agents could reduce lesser 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 their community.

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Appendix A - Parole Agent 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 agent/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 agent/parole supervisor is to help parolees become successful members of society.
7. One of my roles as a parole agent/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 Agent Effects and Individual Survey Question Responses

Survey Categories	**Arrested** % Change	**Non-Arrest TPV Reincarceration** % Change	**Lesser 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%
Optomisitic 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.