Parole Supervision at the Margins

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Abstract:

Nearly three-fourths of incarcerated individuals are released under parole supervision in the United States. However, relatively little is known about the effects of supervised release. In this work, I first investigate the effects of early release from prison using the quasi-random assignment of interviewers to parole hearings in Pennsylvania. I find that, at the margin of release, individuals initially paroled experience higher rates of post-release recidivism than individuals released at a later date, but experience similar rates of post-release employment. Second, I separately identify and estimate the effects of the major components of parole supervision – both supervision intensity and the special conditions of parole such as curfew, placement in a halfway house, or treatment program enrollment. To do so, I leverage two separate quasi-random assignment mechanisms in Pennsylvania – recidivism risk test score discontinuities that determine supervision intensity and the quasi-random assignment of parole interviewers who select special conditions. Results differ across margins, but overall additional supervision leads to additional parole violations with little effect on new arrests or employment.

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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 (Visher et al. 2008). To improve these outcomes, policy makers need clear evidence about the effects of incarceration to motivate decisions regarding whether to incarcerate, how to incarcerate, and for how long.

Several studies have investigated the effects of incarceration on later-life outcomes, and have found mixed results. Along the extensive margin – any incarceration – some studies find criminogenic effects (e.g. Andersen & Andersen 2014; Mueller-Smith 2015; Aizer & Doyle 2015; Henneguelle et al. 2016; Mueller-Smith & Schnepel 2021), some find positive effects (Hjalmarsson 2009; Loeffler & Grunwald 2015; Estelle & Phillips 2018; Bhuller et al. 2020; Rose & Shem-Tov 2021), and some find no effects (Loeffler 2013; Michell et al. 2017, Eren & Mocan 2021; Loeffler & Nagin 2022). Together, these results motivate a conclusion that the effects of incarceration on post-release outcomes are context-specific (Doleac & LaForest 2022), and that the settings where prison reduces recidivism may be settings where rehabilitation is emphasized (Loeffler & Nagin 2022). However, little research exists on the effects of incarceration along the intensive margin – the effect of additional time incarcerated once already incarcerated. Among existing work, Kling (2006), Green & Winik (2010), and Aurora (2018) find no effects of longer prison sentences on post-release outcomes using differences in judge and prosecutor decisions, while Kuziemko (2013) finds a reduction in recidivism from additional prison time using discontinuities in parole guidelines.

Additionally, note that the poor reentry outcomes for previously incarcerated individuals largely occur while under community supervision. For example, in 2019 74% of released previously incarcerated individuals were released early under some form of parole (Carson 2020). Further, the rate of recidivism for parolees is notably higher than the overall rate of recidivism. For example, 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 these

poor outcomes almost no research exists on the effects of community correctional supervision. 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 and reentry programs.

This work answers two sets of open questions. First, what is the effect of additional time incarcerated, prior to release, on post-release outcomes such as recidivism and employment? That is, once an individual is incarcerated, what are the impacts of additional time incarcerated relative to early release? Second, how does parole supervision, and each individual aspect of parole supervision, impact post-release success? That is, what specific aspects of parole supervision impact re-entry success, to what extent, and for whom?

I answer these questions using individual-level data on the Pennsylvania prison and parole populations between 2005 and 2019. I tease out the causal effects of additional time incarcerated and community supervision policies by leveraging three separate quasi-random assignment mechanisms in Pennsylvania that jointly determine whether an individual is released early as well as the structure of their assigned community supervision program upon release.

First, prisoners in Pennsylvania are entitled to a parole hearing after they have served half of their sentence. At this hearing, a randomly assigned parole board member and hearing examiner decide whether the individual should be released to serve the remainder of their sentence under community supervision. As different parole board members and hearing examiners have different propensities to grant parole, I leverage the random assignment of these decision makers to parole board hearings to estimate the effects of early release.

Second, parole supervision in Pennsylvania is comprised of three components. A supervision intensity level, which dictates how often and in what form (in person, over the phone, etc.) a parolee must meet with a parole officer. Special conditions, which a parolee must abide by while under parole supervision, such as curfew, drug and alcohol treatment, contact restrictions, or placement in a community corrections center (i.e., halfway house). And finally, the assigned parole officer themself who oversees compliance with the first two components and has wide discretion over the tenor and structure of the parolee-officer relationship.

In Pennsylvania these three components are quasi-randomly assigned separately. Supervision intensity is determined by an individual's Level of Service Inventory – Revised (LSI-

R) recidivism risk test score, special conditions are assigned by the parole interviewers who grant parole, and a parole officer is assigned based on existing officer caseload sizes at the time or release. I leverage these three separate assignment mechanisms to separately estimate the effects of each individual component of parole supervision.

I find no evidence of deleterious effects of additional time incarcerated on post-release outcomes, among individuals already incarcerated. For these individuals, the "age-out" effect of release at a later age when recidivism is less likely (Ulmer & Steffensmeier 2014) appears to outweigh any criminogenic effects of additional time incarcerated. These results imply that potential criminogenic effects of *any* time incarcerated are likely fully accrued by an incarcerated individual prior to his minimum sentence date. In addition, I find suggestive evidence that individuals released early to parole recidivate more often than individuals denied parole even after the expiration of their full sentence. While the mechanisms underlying this result are unknown (and may be due to effects of additional prison programing completed by individuals denied parole, or more time in the community without adequate reentry support for individuals granted parole), it provides further evidence that there is little criminogenic effect of additional time incarcerated.

Regarding parole supervision I find that, along most margins, additional supervision leads to additional parole violations with little effect on new arrests or employment. One exception to this finding pertains to supervision intensity level – moving from a low (meeting once every three months) to medium (meeting once every month) supervision intensity decreases recidivism by around 10%, while moving from a medium to high (meeting twice every month) supervision level increases it by around 5%. For individuals at these margins, a medium level of supervision appears to minimize recidivism, and strike the best balance between the deterrent effects and burden effects of supervision. I next find that additional special conditions assigned by parole hearing interviewers (such as curfew) appear to have little effect on employment or new arrests. However, each leads to a slight increase in the probability of receiving a parole sanction in the form of a written warning, the assignment of new, additional special conditions that must be met, or reincarceration. Finally, one's assigned parole officer appears to have little effect on new arrests, but does explain 5% of the variation in documented parole violations and 7% of the variation in documented employment across parolees.

Taken together, these results imply that there are some situations in which additional parole supervision can be constructive – such as meeting with a parole officer once every month a

opposed to once every three months – among individuals at the margin of receiving different levels of supervision in Pennsylvania. However, in most situations – such as additional special conditions and meeting with an officer more often than once a month – more intensive supervision leads to additional parole violations with little effect on future arrests or employment.

Literature Review

This research adds to three sets of literature. The first is research on the effects of incarceration. Overall, prior evidence of the criminogenic effects of incarceration are mixed. Most prior research focuses on the extensive margin of incarceration (i.e., the effect of any time incarcerated) and finds different conclusions in different settings. For example, Mueller-Smith (2015) and Aizer & Doyle (2015) find criminogenic effects of incarceration in Houston and Chicago, respectively, relative to probation using the random assignment of defendants to judges. Mueller-Smith & Schnepel (2021) also find criminogenic effects of incarceration in Houston relative to a diversion program that allows defendants to avoid a criminal record, using a discontinuity in program eligibility, and suggestive evidence that the effects driven by the stigma associated with a felony conviction. Additionally, many studies in international settings find criminogenic effects of short prison stays when compared to electronic monitoring diversion programs (Andersen and Andersen 2014; Henneguelle et al. 2016; Larsen 2017; Williams and Weatherburn 2019).

However, several studies find no effect of incarceration on future outcomes (Loeffler 2013; Michell et al. 2017, Eren & Mocan 2021; Loeffler & Nagin 2022) or a positive effect (Hjalmarsson 2009; Loeffler & Grunwald 2015; Estelle & Phillips 2018; Bhuller et al. 2020; Rose & Shem-Tov 2021). Among these studies, Loeffler (2013) finds no effects of incarceration, relative to probation, on future recidivism in Illinois using a randomized judge design. Hjalmarsson (2009) instead finds a positive effect of incarceration, relative to probation, on future convictions using discontinuities in sentencing guidelines. In addition, Bhuller et al. (2020) also finds that incarceration decreases future recidivism and increases employment in Norway, using a randomized judges design. They specifically conclude that the effect is driven by individuals who were not working prior to incarceration, who gained future employment opportunities while incarcerated due to the reentry and job training programs provided in Norway. Overall, these mixed results suggest that the effects of probation relative to incarceration are context-specific (Doleac & LaForest, 2022), and

that the settings where prison reduces recidivism may be settings where rehabilitation is emphasized ((Loeffler & Nagin, 2022).

Less evidence exists on the effects of incarceration at the intensive margin – the effect of additional time incarcerated once incarcerated. Among existing studies, Kling (2006) finds no effects of longer prison sentences on future employment outcomes, using a randomized judge design in Florida and California. Green & Winik (2010) find no effects of longer sentences on recidivism for individuals incarcerated for drug-related offenses, albeit with a small sample, using a similar randomized judge design in Washington DC. More recent work by Aurora (2018) finds that longer prison sentences have no effect on county level crime, using differences in political affiliation of county prosecutorial offices after close elections across the United States. Finally, 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, both Kuziemko (2013) and Macdonald (2020) find that removing the opportunity for parole leads to worse post-release outcomes, using policy changes in Georgia and Arizona respectively. Both conclude that this is likely due to removing the incentive that the opportunity for parole provides to pursue good behavior and program participation while incarcerated.

Results about the effects of incarceration at the intensive margin appear to differ across countries, providing further evidence that the effects of incarceration are context-specific. Landersø (2015) leverages a policy reform in Denmark that increased incarceration length by one-to-two months, and finds that increased incarceration improved employment outcomes, potentially due to additional participation in in-custody rehabilitation programs. Arbour & Marchand (2022) and Meier et al. (2020) find the opposite result. Arbour & Marchand (2022) use a randomized parole board member design in Quebec, for low-risk individuals with sentences of less than two years, and find that paroled individuals have lower recidivism rates. Meier et al. (2020) use randomized judges who approve parole in Israel and find that early release reduces returns to prison in that setting.

The second relevant set of literature focuses on the effects of community supervision. In the United States, the population of individuals in community corrections programs is incredibly large – for every person in a State or Federal prison another three are living under correctional community supervision. In 2019, for example, 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). Surprisingly, given the size and scope of the community corrections system in the United States, there is little causal evidence about its effects.

Several randomized controlled trials find no effect of specific intensive probation programs relative to standard probation programs (Petersilia & Turner 1993; Lane et al. 2005; Henneguelle et al. 2010; Hyatt & Barnes 2014; Barnes et al. 2012). However, regarding the effects of parole supervision, or particular aspects of community supervision, only a few studies exist. 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 a 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. Finally, 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 level at either margin.

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 Section 3 discusses the data. Section 4 describes the empirical model, tests for instrument validity, and results for the effects of early release from prison. Section 5 does the same for the effects of each of the three components of parole supervision. Section 6 concludes.

2. Background – The Parole System in Pennsylvania

In Pennsylvania, a state prisoner is entitled to a parole hearing after serving his minimum sentence, which is at least half of his full sentence. Figure 1 present the overall parole hearing process, along with the steps of the process that determine each component of parole supervision upon release. First, an individual takes the LSI-R recidivism risk test, which is included as one of many pieces of information provided to hearing interviewers at the parole hearing, as well as the single determinant of parole intensity level if released. Next, hearing interviewers are assigned, who conduct the parole hearing, determine whether to grant parole, and determine parole special conditions if the individual is released. If released, a specific parole officer is then assigned to the individual based on current officer caseload sizes in the individual's home community. Finally, the paroled individual is released to serve the remainder of his sentence under community supervision.

Parole Hearing

In Pennsylvania, the majority of parole hearings are conducted jointly by one parole board member and one hearing examiner. The state has nine parole board members, each selected by the governor and approved by the state legislature to serve fixed 6 year terms. One-to-three new parole board member terms begin each year and, from 2005-2019, board members were seldomly reappointed to more than one full term. The state has around 20 hearing examiners, who serve full-time (non-fixed-length) positions and are hired by the Pennsylvania parole board.

Between 2005 and 2019 each board member and hearing examiner was quasi-randomly assigned to conduct all parole hearings at a particular facility on a particular date. Each eligible incarcerated individual, meanwhile, was separately assigned a hearing date at their facility of residence based on their minimum sentence date (for initial parole hearings) or review date (for individuals previously denied parole). Explicitly, the scheduling process each month is broken down into four steps. First, each board member and hearing examiner provides a parole board scheduler the list of days they are available to conduct hearings that month. Second, the scheduler assigns each parole board member and hearing examiner specific days at specific facilities based on five criteria – (a) availability, (b) interviewer home location / region of the state, (c) interviewer total caseload, (d) to create variation for each interviewer in assigned facility from day to day, and (e) to create variation for each interviewer in hearing examiner / board member pairings from day to day. Given that there are only nine Parole Board members, on certain facility-days no board

members are available to attend parole hearings in person. On these days at these facilities, different board members are assigned to each individual hearing based on a rotating schedule, to either take part in the hearing remotely or review the hearing details and vote at a later date.

Third, the parole board scheduler provides the list of selected facility-days to the facility scheduler at each facility, without providing the names of the interviewers assigned to each day. Finally, facility schedulers schedule eligible inmates to available dates based on two criteria – (a) inmate parole eligibility date and, sometimes, (b) whether the inmate was convicted of a violent or non-violent crime, as individuals convicted of violent and non-violent crimes were sometimes scheduled on separate dates at certain prisons. No additional information about inmates is available to facility schedulers during this process.

Each day, at each facility, the assigned board member and hearing examiner jointly conduct all scheduled parole hearings. The process for each individual hearing is shown in Figure 2. The board member and hearing examiner individually review information about the individual, jointly interview him, and then vote independently on whether to grant parole. If an interviewer recommends parole he can also recommend imposing special parole conditions upon release such as curfew, restrictions on social contacts, or required residency in a community corrections center. If an interviewer recommends denial he can also recommend a date for the incarcerated individual's next parole hearing as well as areas for improvement for the individual such as "no future prison infractions" or "successful completion of specific prison programing."

If the board member and hearing examiner both recommend parole the individual is paroled. If the board member and hearing examiner both recommend denial the individual is denied parole. If one interviewer recommends parole while the other interviewer recommends denial, a second parole board member is randomly selected, from among the other eight parole board members, to review the case at a later date and cast the deciding vote.

If the individual is paroled, he is released under all special parole conditions recommended by either of the two interviewers who recommended parole. If the individual is denied parole, he is scheduled for a review hearing at the earlier of the two dates proposed by the two interviewers who recommended denial. The individual is also provided all recommendations for improvement suggested by either of the two interviewers who recommended denial, and these recommendations are provided to the interviewers later assigned to conduct the review hearing. Note that review hearings and violator hearings (i.e., hearings for individuals previously paroled but reincarcerated

for a parole violation) are scheduled alongside minimum sentence hearings on the same days, using the same assignment process described above, and thus new interviewers are randomly assigned to review and violator hearings.[2]

Parole Supervision

Paroled individuals are released into the community to serve the remainder of their sentence under community supervision. This supervision is, broadly speaking, defined by three factors. First, the parolee must meet with their assigned parole officer a certain number of times each month, determined by their designated supervision intensity level. Second, the parolee is subject to special parole conditions such as curfew, drug testing, and restrictions on social contacts that they must abide by at all times while on parole. Third, the assigned parole officer has wide discretion over the tenor of the parolee-officer relationship, how to cultivate it, and whether and how to sanction the parolee when he breaks a condition or misses a meeting.

Parole supervision level determines both the regularity with which a parolee must meet with his parole officer and the regularity with which the parole officer must check-in with close members of the parolee's community (i.e., "collateral contacts", such as meeting with a family member, roommate, or employer). Upon release, parolees are assigned to either minimum supervision (one face-to-face meeting every three months, one collateral contact every three months), medium supervision (one face-to-face meeting every one month, one collateral contact every three months), or maximum supervision (one face-to-face meeting every two weeks, one collateral contact every one month).[3]

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[2] The majority of parole hearings require two votes to parole or two votes to deny. However, incarcerated individuals who were convicted of non-violent offenses and exhibit good conduct while incarcerated are eligible for an abbreviated hearing with a single hearing examiner interviewer. Conversely, incarcerated individuals convicted of certain high-level violent offenses (i.e., homicide and sex crimes), while still interviewed by a single hearing examiner and board member, require majority parole board approval to parole. Specifically, at these hearings, if both interviewers recommend denial the individual is denied parole and the case is not shared with the other eight board members. If at least one of the two interviewers recommend parole, the case is shared with additional board members, iteratively, until five of the nine board members have recommend to approve or deny parole. In the primary analysis I use these alternative hearings when creating interviewer parole propensity instruments but only use regular (two-votes-needed) hearings in the analysis itself. Results are appreciably similar when these hearings are also included in the analysis.

[3] For all three levels of supervision, every other face-to-face contact must be at the parolee's approved residence. If the offender is in a treatment program, the officer must conduct double the required number of collateral contacts, such that every other contact is with the treatment provider. Collateral contacts may be face-to-face, over the phone, or through email. After one year on parole, parolees are eligible for one of three lowered levels of supervision based on good behavior (administrative supervision, special circumstance supervision, and monitored supervision).

Between 2005 and 2019 supervision intensity level was assigned almost-exclusively based on a parolee's Level of Service Inventory – Revised (LSI-R) recidivism risk test score. This test predicts re-offending risk upon release, and is administered prior to an individual's scheduled parole hearing (Andrews & Bonita, 1995). Paroled individuals scoring below a lower threshold score receive minimum supervision, above the lower threshold score but below a higher threshold score receive medium supervision, and above the higher threshold score receive maximum supervision.[4]

Second, special parole conditions – such as curfew, drug testing, and restrictions on social contacts – are initially imposed by parole hearing interviewers. In Pennsylvania, they include conditions applied to nearly all parolees (e.g. drug testing, work requirements, and supervision fees) and conditions only applied to some parolees (e.g., curfew, residence in a community corrections center, and required financial support for dependents). After release, parole officers have discretion to impose additional special conditions in response to parole violations.

Finally, parole officers have substantial discretion over the type of relationship they build with each parolee, and discretion over whether to provide a warning, additional restrictions, or re-incarcerate when a parolee violates a condition of parole. In Pennsylvania between 2005 and 2019 parolees were assigned to initial parole officer units based on only 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 randomly assigned to parole officers based only on officer caseload size at the time of release.

3. Data

This work uses data on all Pennsylvania prisoners with parole hearings between 2005 and 2019. Pennsylvania has the sixth highest state prison population, with nearly 50,000 individuals incarcerated at any given time across the state's 23 state correctional institutions (Pennsylvania

[4] The Parole Board can assign parolees to an additional, highest level of supervision at their own discretion (enhanced supervision), but this level of supervision is rarely used in practice. Additionally, while most parole supervision levels are determined by LSI-R scores, sex offenders and individuals convicted of domestic violence are automatically assigned maximum supervision, and individuals released to community correction centers are assigned no less than medium supervision while in residence.

Department of Corrections, 2018). In the state, an additional 50,000 individuals are under parole supervision at any given time under the jurisdiction of one of the state's 9 parole district offices (Pennsylvania Board of Probation and Parole, 2018). 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 incarcerated individuals in Pennsylvania at the time of their minimum sentence parole hearing. The majority of incarcerated individuals are male, just under half are white (non-Hispanic) and just under half are 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, the average full sentence length is 5.6 years, and over one-third of individuals have served prior sentences in DOC custody.

Table 2 provides information on parole hearing outcomes. 60% of parolees are successfully granted parole at their initial parole hearing. At hearings, interviewers receive a parole recommendation from the facility superintendent and a parole recommendation from a decisional instrument that takes into account several factors about the individual and their in-custody behavior. At initial parole hearings, the superintendent and decisional instrument each recommend parole four-fifths of the time. Among individuals who receive these recommendations, around 70% are granted parole. Turning to the factors that comprise the decisional instrument, individuals with higher violence risk, recidivism risk, and in-custody behavioral issues are less likely to be paroled. Finally, while 60% of individuals are paroled during their initial parole hearing, individuals initially denied parole are paroled 55% of the time during review hearings.

Finally, Table 3 provides details on parolee post-release outcomes. Within one year of release, 17% of parolees are returned to prison for parole violations, 22% are arrested for new crimes while on parole, and 37% receive one or more minor parole violations that result in a written warning or an assignment of new, additional special conditions. In addition, only 48% of parolees have had at least one month of employment, as documented by their parole officer, during the first year after release.

4. The Effects of Early Release

Research Design

Let Y_{it} represent a post-release outcome such as reincarceration, parole violation, or employment, for individual i a certain number of years after his parole hearing at time t. Let $Paroled_{it}^{*}$ be a measure (defined below) of whether the individual is granted parole at the hearing. Let X_{it} be a set of personal characteristics about the individual such as convicted crime type, remaining sentence length, facility, LSI-R score, parole decisional score, and year of the parole hearing. Finally, let ε_{it} be a stochastic error term. The relationship between early release and post-release outcome *Y* is defined by β_1 in the equation

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

First, note that an Ordinary Least Squares (OLS) regression of outcome Y_{it} on whether the individual was paroled (i.e., $Paroled_{it}^{*} = Paroled_{it}$) will provide a biased estimate of the effects of early release as less risky individuals are also more likely to be paroled. To avoid this selection bias, I estimate a Two-Stage-Least-Squares (2SLS) regression using a measure of the leniency of the interviewers quasi-randomly assigned to an individual's parole hearing as an instrument for whether the individual is paroled ($Paroled_{it}^{*}$).

To construct the instrument, I first create residual measures of observed leniency for every parole hearing between 2005 and 2019, that net out fully interacted facility, year, and violent / non-violent offender fixed effects (W_{it}). These residual hearing-level observed leniency measures, $ResidParole$, are constructed as the residuals from an OLS regression of the equation

$$Paroled_{it} = \gamma_0 + \gamma_1 W_{it} + e_{it} \quad , \tag{2}$$

where e_{it} is a stochastic error term. The residuals are then used to construct leave-one-out hearing examiner and board member leniency measures for each hearing, defined as the average leniency residual for interviewer (j) across all hearings (h) to which they are assigned during the calendar year (n_j) except for the current hearing for inmate (i) and any other hearings pertaining to that inmate (n_{ji}):

$$V_{jt(-i)} = \left(\frac{1}{n_j - n_{ji}}\right)\left(\sum_{h=1}^{n_j} ResidParole_h - \sum_{c=1}^{n_{ji}} ResidParole_c\right) \quad . \tag{3}$$

Note that these hearing examiner and board member leniency measures are constructed separately for each interviewer each year to account for changes in specific hearing examiner and board member leniency over time.

Finally, these instruments are used in a first stage equation of parole on voter leniency to construct $Paroled_{it}^{*}$. Specifically, $Paroled_{it}^{*}$ is constructed as the fitted values from an OLS regression of the equation

$$Paroled_{it} = \alpha_0 + \alpha_1 V_{ht(-i)} + \alpha_2 V_{bt(-i)} + \alpha_3 X_{it} + \epsilon_{it} \quad , \qquad (4)$$

Where $V_{ht(-i)}$ is the leniency measure of the hearing examiner assigned to the hearing and $V_{bt(-i)}$ is the leniency measure of the board member assigned to the hearing.[5] This procedure produces an unbiased estimate of the effects of parole as long as the instruments meet relevance, exogeneity, and monotonicity assumptions (Dobbie et al., 2018; Bhuller et al., 2020).

Instrument Relevance & Validity

Instrument relevance requires variation in parole leniency across board members and hearing examiners to impact actual parole decisions. Figure 3 provides graphical evidence of this variation. First, the figure present histograms of the leave-one-out leniency measure for all hearing examiners and board members in the sample, and show wide variation in leniency. For example, lenient hearing examiners are 12 percentage points more likely to parole an incarcerated individual than stringent hearing examiners, and lenient board members are 10 percentage points more likely to parole an incarcerated individual than stringent board members. Second, the figures plot local linear regressions of an individual's parole outcome on leave-one-out interviewer leniency. For both assigned hearing examiners and board members, the likelihood of parole appears to monotonically increase as leave-one-out interviewer leniency increases. I also conduct an f-test for weak instruments to further assess the relevance of the leniency measures. As shown in Table 4, Panel A, first stage f-values are around 350 and 450 for hearing examiner and board member leniency measures, respectively, from a regression of parole outcome on leave-one-out interviewer leniency, personal characteristics, and hearing date variables.

Instrument exogeneity requires interviewers to be "as-good-as-randomly" assigned, within interview facility, year, and violent / non-violent offender interview groups. In addition to qualitative discussions with Pennsylvania Parole Board personnel confirming the quasi-random

[5] Note that the primary analysis uses both a hearing examiner and board member instrument, constructed separately, in the first stage equation. However, alternative specifications using just a hearing examiner instrument, just a board member instrument, or an instrument constructed as the specific leniency of each individual hearing examiner / board member pair, would also be appropriate. Results using these alternative specifications are appreciable similar to the primary results, as shown in the Online Appendix.

assignment mechanism of interviewers to parole hearings (as discussed in Section 2), Table 5, Panel B, presents results from a formal test for independence. As shown, neither hearing examiner or board member stringency measures appear to be correlated with observable interviewee personal characteristics.

In order to interpret the 2SLS estimates as the local average treatment effects of early release, among individuals at the margin of release, interviewers must have monotonic preferences regarding parole leniency. That is, a "lenient" interviewer must be lenient to all types of individuals at the margin of release, and vice-versa. I test this monotonicity assumption in two ways. First, Table 5, Panel C, shows that first stage estimates of parole on interviewer leniency measures – constructed using the full sample – are still positive and statistically significant when restricting the regression sample to various subsets of interviewees (i.e., testing for within-subsample monotonicity). I specifically investigate subsets of individuals convicted of different crime types, individuals at a minimum sentence parole hearing or a review parole hearing, and individuals at different types of interviews (regular parole hearings, "one-interviewer" expedited parole hearings, and "majority board approval" high-level violent offenses parole hearings). Second, Table 5, Panel D, shows that first stage estimates of parole on interview leniency measures – constructed using a sample that omits a particular subset of interviewees – are still positive and statistically significant when restricting the regression sample to the omitted subset (i.e., testing for across-subsample monotonicity).

Results

Table 5 presents results of the effects of early release on post-release outcomes. First, Columns A2 and A3 present results from OLS regressions that do not control for selection (Column A2 presents results from a regression that also omits covariates). Among individuals eventually released to parole, individuals who are granted parole at a given hearing are released 15.7 months sooner than individuals who are denied parole, and are 8% less likely to be reincarcerated during their first year after release (Column A2). However, once we include a wide variety of observable characteristic covariates (Column A3) these differences decrease substantially – individuals granted parole at a given hearing are only 2% less likely to be arrested during their first year after release, and no more likely to be reincarcerated for a parole violation, after controlling for observable differences between these populations.

Column A1 presents results that control for selection using assigned interviewer leniency instruments. Individuals at the margin of parole who are denied release are, on average, incarcerated for an additional 10 months. Perhaps surprisingly, here we see the opposite effect – early release leads to an increased chance of arrest in the first year after release, and suggestive evidence of a decrease in employment. Note that here we're comparing results for the first year after release between (1) individuals granted parole and released earlier at a younger age and (2) individuals denied parole and released to parole later at an older age. Theoretically, older individuals will recidivate less due to age-out effects (Ulmer & Steffensmeier 2014) but may recidivate more due to the criminogenic effects of additional time incarcerated and the potential stigma and discouragement of parole denial (West-Smith et al. 2000). Here, it appears the age-out effects of later release dominate the criminogenic & stigma effects of additional time incarcerated.

Columns B1-B3 present alternative results, which compare outcomes for paroled and denied individuals net of age effects. First, Columns B2-B3 compare outcomes over the two-year and five-year periods that follow a parole hearing. Individuals initially paroled recidivate substantially more often during the first two years after their parole hearing than individuals initially denied parole, but this result is largely mechanical – individuals initially denied parole remain incarcerated for a substantial portion of this period, during which time they are unable to recidivate. As shown in Column B3, when I extend the outcome period to five years after a parole hearing the results decrease substantially.

Finally, Column B1 compares outcomes during a two year follow-up period that starts five years after the parole hearing, among individuals with five or less years left on their sentence at the time of their hearing. For this analysis, by the start of the outcome period all individuals will have been released, and no longer be under parole supervision, related to the conviction for which they had been incarcerated. However, paroled individuals will have spent less time incarcerated and more time on parole in the community prior to the start of the outcome period. Perhaps surprisingly, I observe that individuals who were released sooner were more likely to be arrested during this follow-up period. Overall, these results suggest that early release increases recidivism relative to later release. They imply that any criminogenic effects of incarceration are likely concentrated on the extensive margin, with little observable criminogenic effects of additional time incarcerated among individuals who have served at least half their sentence.

Margin of Release

This analysis estimates the local average treatment effect (LATE) of early release for individuals at the margin of release. Note that it cannot identify the effects of early release for individuals away from the margin of release. Specifically, it cannot identify effects for very low risk incarcerated individuals with good behavior while incarcerated – who would be granted parole by all hearing interviewers – and very high risk incarcerated individuals with poor behavior while incarcerated – who would be denied parole by all hearing interviewers. The effects identified by this analysis only pertain to individuals who would be released if assigned certain more-lenient parole interviewers but not if assigned certain more-stringent parole interviewers.

Which incarcerated individuals are at this margin in Pennsylvania? Figure 4 provides some context about the proportion of incarcerated individuals at the margin of release, and the percentage ultimately paroled. First, it presents a histogram of the parole outcome predicted values for each parole hearing in Pennsylvania prior to parole interviewer assignment. Specifically, they are the predicted values from the first-stage regression of parole outcome on interviewer leniency and individual observed characteristics, subtracting parole interviewer leniency effects. The grey bar shows the range of predicted values that appear to be at the margin – for which we expect the parole outcome to change based on whether the individual is assigned stringent or lenient interviewers. Finally, the local linear regression line depicts the percent of individuals with each predicted value that are actually paroled.

First, note that 20% of the sample falls within the grey bar, and that individuals in this range are paroled between 30% and 70% of the time. Within the data, this group is largely comprised on individuals who committed violent crimes, with medium-to-high decisional instrument scores, who have shown good behavior while incarcerated and are recommended for parole by the superintendent of their facility or residence.

However, this group does not comprise the full set of individuals "at the margin of release." The majority of the sample (60%) has predicted probabilities above the grey bar, but is still denied paroled 20% of the time. As such, a subsample of these individuals is actually at the margin of release as well, due to factors which make them riskier parole candidates which are unobservable in the data. For example, while this subsample of individuals is less risky on paper (they committed lesser crimes, have low-to-medium decisional instrument scores, committed good behavior while incarcerated and are recommended for parole by the facility superintendent) they

may have displayed worrisome signs during their parole interview that made them riskier candidates for parole. The oppositive is true for a subsample of individuals below the grey bar, as these individuals are still granted parole 10% of the time. Together, these individuals – the population in the grey bar, along with a proportion of individuals just above and below it – constitute the population of individuals at the margin of release in Pennsylvania, for whom we have identified the average effect of early release. Overall, it comprises a set of individuals who are low-risk along some dimensions but high-risk along others.

5. The Effects of Parole

Research Design - Supervision Level

Next, I turn to the effects of parole supervision itself on post-release outcomes, by separately estimating the effect of each component of parole supervision. I first estimate the effect of parole supervision intensity level using regression discontinuity designs around two separate recidivism risk score cutoff values – the LSI-R score cutoff between minimum & medium supervision levels and the LSI-R score cutoff between medium & maximum supervision levels. As a small number of individuals are assigned to different supervision levels than specified by the LSI-R, I estimate intent-to-treat estimates. Specifically, let h_l be some bandwidth around the lower cutoff value and h_u be some bandwidth around the upper cutoff value, and let $MedSupScore$ and $MaxSupScore$ be indicator variables for whether the individual's LSI-R score assigns him to medium or maximum supervision, respectively. The effect of medium supervision, relative to minimum supervision, on post-release outcome *Y* is defined by β_1 in the equation

$$Y_{it} = \beta_0 + \beta_1 MedSupScore_{it} + \beta_2 X_{it} + \tilde{\varepsilon}_{it} \quad (7)$$

when restricting the sample to individuals with LSI-R scores h_l or less away from the lower cutoff value who are eligible for both minimum and medium supervision (i.e., omitting sex offenders, individuals convicted of domestic violence, and individuals released to Community Correction Centers). Similarly, the effect of maximum supervision, relative to medium supervision, on post-release outcome *Y* is defined by β_1 in the equation

$$Y_{it} = \beta_0 + \beta_1 MaxSupScore_{it} + \beta_2 X_{it} + \tilde{\varepsilon}_{it} \quad (8)$$

when restricting the sample to individuals with LSI-R scores h_u or less away from the upper cutoff value who are eligible for both medium and maximum supervision (i.e., omitting sex offenders and individuals convicted of domestic violence). Note that, in both equations, LSI-R score is an

element of X_{it}. I select cutoff values h_l and h_u following Imbens & Kalyanaraman (2012), but results are appreciably similar for alternative cutoff values.

Figure 5 plots the LSI-R scores for every paroled individual in Pennsylvania between 2005 and 2019. The lower and upper cutoff values changed several times during this period, and ranged between 17-21 and 27-30, respectively, over this time.[6] As shown in Figure 5, there appears to be no sign of bunching at either cutoff. Formal density checks around the cutoffs, following McCrary (2008), find no evidence of manipulation (p = 0.11 at the lower cutoff; p = 0.32 at the upper cutoff).

Note that LSI-R score information is available to interviewers at the time of an individual's parole hearing, and thus may impact interviewer decisions to grant parole and assign parole conditions. If a slight change in LSI-R score near either cutoff affects this decision, then the results of this analysis could be biased as a proportion of (higher risk) individuals above the cutoff will be denied parole and thus omitted from the paroled sample. To address this concern, I estimate several alternative specifications that restrict the sample to subsets of individuals away from the parole margin of release. Specifically, individuals who are low risk and would be paroled even if their LSI-R score varied slightly near the cutoff. Results are appreciably similar across these alternative specifications.

Results – Supervision Level

Estimates of the effects of parole supervision level are presented in Table 6. Column 1 presents the main results, while Column 2 presents results when restricting the sample to individuals away from the margin of release. Perhaps surprisingly, increasing supervision level from low to medium appears to decrease short-term recidivism, while increasing supervision level from medium to high appears to increase short-term recidivism, both on the range of 5-10%. However, neither supervision level appears to affect employment.

These results are consistent with a hypothesis that low levels of supervision (meeting with parolees once every three months) are too low to serve as a proper deterrent against future arrests and violations, while high levels of supervision (meeting with parolees twice every month) are too high of a hurdle for parolees, leading to more arrests and violations for non-compliance. Given

[6] Specifically, the lower threshold was 20 and below prior to 1/1/2009, 17 and below between 1/1/2009 and 12/3/2014, and 19 and below from 12/4/2014 to 12/31/2019. The upper threshold was 29 and above prior to 1/1/2009, 27 and above between 1/1/2009 and 12/3/2014, and 28 and above from 12/4/2014 to 12/31/2019.

the inherent trade off within community corrections between the deterrent effects and burden effects of supervision, the results suggest that one meeting per month may best balances these competing effects, at least among individuals at the low and high recidivism risk margins.

Research Design – Special Conditions

I next investigate the effects of special parole conditions. Table 7 provides data on the parole conditions assigned at release in Pennsylvania. These include conditions assigned to nearly every parolee, such as not consuming alcohol, mandatory drug testing, maintaining employment or an active job search, and paying one's own supervision fees.[7] They also include conditions that are assigned with a wide amount of discretion across parolees, such as curfew, community corrections center residency, restrictions on contact with codefendants, gangs, victims, and drug users and sellers, required financial support for dependents, and required successful completion of treatment programs. The average parolee is assigned 6-7 of these discretionary conditions upon release.

The main specification to evaluate the effects of board-imposed special parole conditions mirrors Equations 1-4 above, with several minor exceptions. First, the explanatory variable of interest is either "total number of conditions" or "a single specific condition," instead of "parole approval / denial." Second, I restrict the sample to parole hearings that resulted in parole. Here, I use the leniency of hearing interviewers to assign special conditions, conditional on parole, as an instrument for whether special conditions are assigned.

Estimation of the effect of "total number of special conditions" directly follows this estimation procedure. Estimation of the effects of each individual special condition, however, requires one further step. As hearing interviewers can choose to assign many different conditions at once, and interviewer leniency between conditions is likely correlated, I control for judge stringency across all other available conditions in order to estimate causal, unbiased estimates of the effects of each specific condition, following Muller-Smith (2015). That is, I include the leave-one out leniency measures for all other available conditions ($n_{(-c)}$) in the first stage equation for the condition of interest. Specifically, the instrument for condition c, $Cond_{it}^{c*}$, is constructed as the fitted values from an OLS regression of the equation

[7] The average supervision fee in Pennsylvania is $43 per month (Pennsylvania Board of Probation and Parole 2018).

$$Cond_{it}^{c} = \alpha_0 + \alpha_1 V_{ht(-i)}^{c} + \alpha_2 V_{bt(-i)}^{c} + \sum_{j=1}^{n_{(-c)}} \left(\alpha_{1j} V_{ht(-i)}^{j} + \alpha_{2j} V_{bt(-i)}^{j}\right) + \alpha_3 X_{it} + \epsilon_{it} \; . \quad (5)$$

I then also include the leave-one-out leniency measures for all other special conditions in the second stage equation for the condition of interest,

$$Y_{it} = \beta_0 + \beta_1 Cond_{it}^{c\,*} + \sum_{j=1}^{n_{(-c)}} \left(\alpha_{1j} V_{ht(-i)}^{j} + \alpha_{2j} V_{bt(-i)}^{j}\right) + \beta_2 X_{it} + \varepsilon_{it} \quad . \quad (6)$$

As before, the 2SLS procedure produces an unbiased estimate of the effects of "total number of special conditions" and "each individual special condition" as long as the instruments are relevant, exogenous, and exhibit monotonicity.

Figure 6 provides graphical evidence of the variation in "total number of special conditions" leniency across board members and hearing examiners. Similar to the parole decisions discussion above, the figures show a wide variation in leniency. For example, lenient hearing examiners assign two more special conditions on parolees than stringent hearing examiners, and lenient board members assign one more special condition on parolees than stringent board members. In addition, for both assigned hearing examiners and board members, the number of assigned conditions monotonically increases in interviewer leniency, at nearly all levels of leniency, and is close to linear. Finally, an f-test shows that the instruments are strongly predictive of number of assigned conditions – with first stage f-test values in the range of 400+ for both hearing examiners and board member leniency measures. Hearing examiner and board member instruments also pass several exogeneity and monotonicity tests, as shown in Appendix A.[8]

One additional concern relates to the joint interviewer decision to recommend parole and assign special conditions conditional on parole. As these decisions are not independent, it is possible that the decision to parole is influenced by the expected decision to assign conditions, and that restricting the sample to paroled individuals may bias the results. As such, I conduct two further robustness checks. First, I estimate the effect of "total number of conditions" and each individual condition using the full sample of parole hearings, including hearing that do not result in parole, while controlling for interviewer parole propensity (note that individuals that are not released on parole remain incarcerated and, by default, receive zero special parole conditions). These results, controlling for parole leniency, are appreciably similar to the main results.

[8] Relevance, exogeneity, and monotonicity test results for each individual discretionary special parole condition are similar to results for the "total number of special conditions" outcome, and are presented in the Online Appendix.

Second, I restrict the sample to paroled individuals away from the margin of release. These are individuals who have a low risk of recidivism and are expected be paroled no matter which interviewers are assigned to them. There is no risk of sample selection bias for these individuals as nearly all will be paroled regardless of interviewer, but there is still variation in interviewer propensity to assign special conditions across them. While the sample is smaller, estimates for these individuals will be causal and unbiased. These results are also appreciably similar to the main results, regardless of the criteria used to define this subpopulation (e.g,, individuals with positive warden and decisional instrument parole recommendations, individuals with the highest 25% of predicted parole probabilities, etc.).

Results – Special Conditions

Table 8 presents estimates of the effect of the total number of special conditions on post-release outcomes. Column 3 presents results from an OLS regressions that does not control for selection. Individuals who are assigned more conditions have a higher recidivism rate, driven by an increase in reincarceration for parole violations, and are also more likely to receive minor parole violations (ones that results in a written warning or new restrictions). Without controlling for selection, each additional assigned condition is associated with a 2% increase in recidivism and a 3% increase in the chances of receiving a minor parole violation.

Column 1 presents the main results. After controlling for selection, I estimate no effects of additional parole conditions on employment. However, I do observe a significant positive effect of additional parole conditions on both technical parole violation reincarcerations and minor parole violations. Specifically, each additional assigned condition increases the chance of a violation that leads to a technical parole violation reincarceration by 10%, a violation that leads to new restrictions by 7%, and a violation that leads to a written warning by 3%. Columns 2 present results from a specification that restricts the sample to parolees away from the margin of release. Estimates from this specifications are similar, thought less significant, to the main results in Column 1.

Based on these results, among individuals at the margin of receiving an additional condition, additional parole conditions have no discernable impact on new arrests or employment. Each additional condition only increases the chance that a parolee receives a parole violation and is, in response, reincarcerated, given additional conditions, or given a written warning. As each

condition adds restrictions on a parolee's day-to-day activities after release, these results recommend assigning less conditions to individuals at the margin of receiving them.

Table 8 presents estimates for each individual condition, controlling for interviewer propensity to assign all other conditions, on one year post-release outcomes. These results show suggestive evidence that most individual conditions increase the likelihood of receiving parole violations, as the majority of point estimates are positive, even though there is not enough statistical power to detect individual effects with a high degree of confidence. As such these results suggest that individual conditions have little corrective impact for individuals at the margin, with the exception that required attendance of an alcohol support group does appear to decrease future arrests. Finally, note that several conditions (such as residence in a community corrections center) do not have enough variation in assignment across parolees, after taking into account interviewer propensity to assign all other conditions, to estimate their effects.

Research Design – Parole Officers

Finally, I investigate the extent to which assigned parole officers impact parolee outcomes. There are several ways to evaluate the relative effect of individual parole officers. One such approach leverages the same "randomized decision makers" framework discussed above. Given the "as-good-as-random" assignment of parolees to officers within parole officer units, I create "leave-one-out" individual-officer effects separately for each outcome of interest, and then evaluate the "relevance" of these officer effects on the associated outcome. Specifically, I first create residual measures of observed outcome Y_{it} for each parole stay, that net out fully interacted year and assigned paroleofficer unit fixed effects (U_{it}). These parole stay level outcome 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 officer expected effects, defined as the average effect for officer (j) across all parolees (p) they are assigned 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}):

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

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 outcomes we can regress outcome Y_{it} on the leave-one-out expected effect of the assigned parole officer as well as all other observable information about the parolee:

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

If the leave-one-out expected effects are highly correlated with parolee outcomes it implies that individual officers have an observable impact on parolee outcomes.

An additional benefit of leave-one-out expected officer effect measures is that they can be used to test our exogeneity assumption of "as-good-as-random" assignment of parolees to parole officers within parole officer units. If this is the case, then the leave-one-out measures should not be correlated with any other observable characteristics about the parolee. As shown in Table 9, this appears to be the case – f-tests for joint significance from regressions of leave-one-out officer effects of each outcome on all observable characteristics produce p-values in the 0.10-0.70 range, providing evidence that officer assignment is indeed "as-good-as-random."

Results – Parole Officers

Results testing the relevance of leave-one-out officer effects to parolee outcomes are shown in Figure 7. 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 increases 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 job approaches have a large effect on keeping paroled 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 have an effect on minor parole violations and documented employment, it is unclear what is causing these effects. For example, certain officers may build stronger relationships with parolees that cause parolees to change their post-release behavior in a way that impacts their propensity to break minor parole conditions 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 these observed effects are driven by differences in officer reporting decisions. For example, certain officers may be more likely to write up and sanction parolees for minor violations, and certain officers may be more likely to follow up and vet whether a parolee's reported employment information is accurate. As such, it is not clear whether these observed parole officer effects are driven by differences in officer reporting behavior or officer impacts on parolee behavior.

6. Discussion

This work investigates the effects of parole supervision in two ways. First, it compares the effects of release to parole to the effects of additional time incarcerated. Among individuals at this margin of release, the results shows that the age-out effects of additional time incarcerated, along with the potential positive effects of incarcerated program participation, appear to dominate any potential criminogenic effects of additional time incarcerated and stigma effects of parole denial.

These results imply that any criminogenic effects of incarceration appear to accrue early on during an individual's incarcerated stay, with little criminogenic effects of incarceration during the second half of a stay. From a policy perspective, to reduce the criminogenic effects of incarceration these results recommend focusing on diversion programs, to keep individuals out of prison in the first place, as opposed to a focus on shortened sentence lengths.

Among individuals at the margin of release in Pennsylvania (those which are high risk is some dimensions but low risk in others), these results also suggest that early release leads to slightly higher recidivism in the short- and long-term. From a policy perspective, these results recommend rejecting parole slightly more often to minimize returns to prison. However, note that these results only evaluate the effects of parole on returns to prison and documented employment. They do not evaluate the effects of parole on the wide variety of other personal outcomes the parole decision affects, such as housing stability, educational attainment, family stability, and personal wellbeing. When making policy decisions about who to parole and when, the effects of parole on these other outcomes should also be taken into account.

Next, this work evaluates the effects of parole itself by separately evaluating the effect of its three main components – supervision intensity level, special conditions, and a parolee's assigned parole officer. First, parole supervision intensity level affects both new arrests and technical parole violation reincarceration, with once-a-month meeting requirements minimizing recidivism among individuals at both high and low risk level margins. Discretionary special conditions appear to have little corrective impact for individuals at the margin of receiving them, and only increase an individual's propensity to receive parole violations (though the effect of mandatory special conditions such as drug testing, work requirements, and payment of supervision fee cannot be identified in Pennsylvania). Finally, parole officer discretion appears to have little effect on new arrests, but does impact both documented parole violations and employment.

From a policy perspective, these results recommend assigning less special conditions to individuals at the margin of receiving them, and setting once-a-month meeting requirements for individuals at both the high and low recidivism risk margins. In addition, they recommend further investigation into exactly which types of parolee-parole officer relationships foster post-release success, increase employment and minimize parole violations (see LaForest 2022 for a detailed discussion of these relationships). Overall, the results recommend that policy makers looking to improve parole outcomes should focus on assigning additional supervisory conditions and

intensity only when necessary, with the exception of setting meeting requirements at more than once every three months among individuals with at least some predicted recidivism risk. The results also recommend setting appropriate supervision levels for each individual parolee, and investigating ways to better foster constructive parolee-parole officer relationships in order to improve re-entry success.

Tables and Figures

Figure 1 – Parole Process

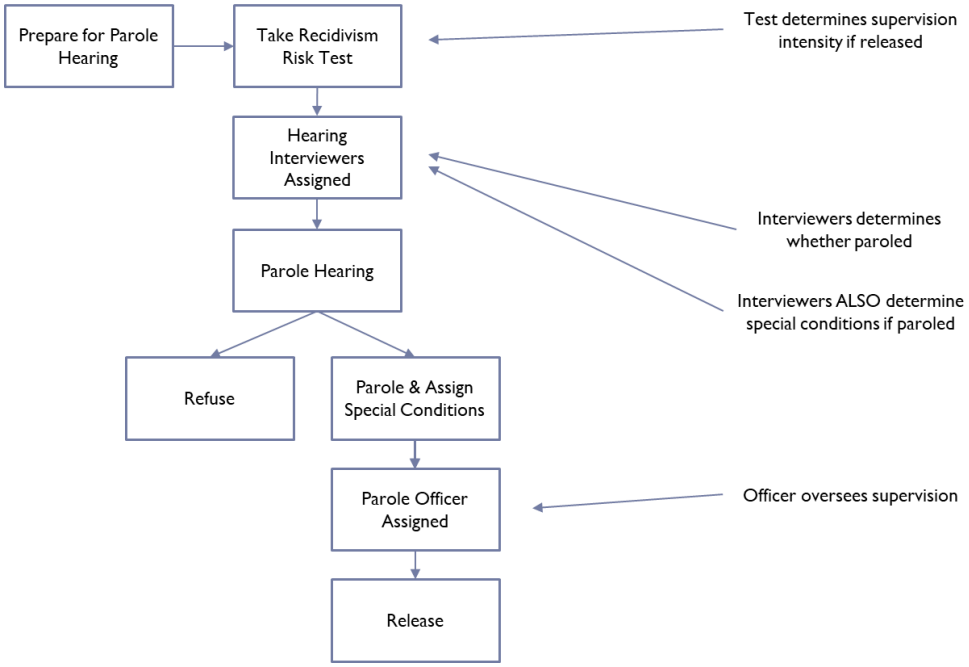


Figure 2 – Hearing Structure

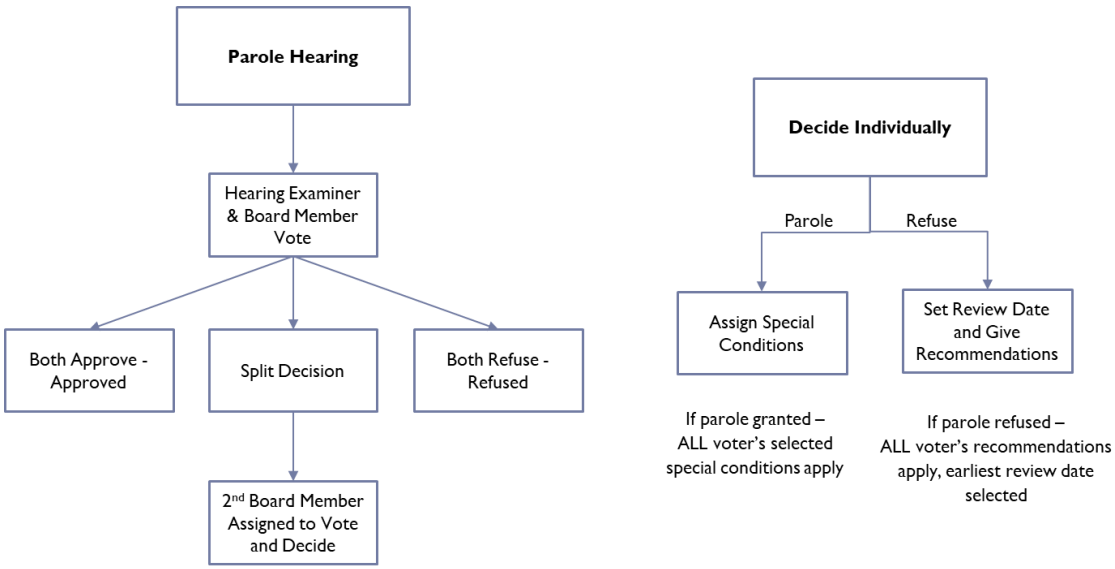


Table 1 – Incarcerated Population at Time of Minimum Sentence Hearing

Demographic Characteristics	Mean
Male	91%
Black	42%
Hispanic	11%
Age	35.1
Education - Less Than HS Degree	40%
Married	14%
Violent Crime Conviction	31%
Drug Crime Conviction	29%
Minimum Sentence Length (Years)	2.8
Any Prior Stays	0.4

Notes:

N = 133,789 minimum sentence hearings

Table 2 – Parole Hearing Statistics

A. Minimum Sentence Hearing		
Summary Statistics	**Mean**	**%Paroled**
Superintendent Recommendes Parole	80%	72%
Decisional Instrument Recommends Parole	84%	68%
Violence Risk - Low	68%	64%
Violence Risk - Medium	14%	55%
Violence Risk - High	18%	45%
LSIR Recidivism Risk - Low	19%	71%
LSIR Recidivism Risk - Medium	39%	65%
LSIR Recidivism Risk - High	42%	52%
DOC Programing - Unwilling to Participate	4%	5%
DOC Behavioral Offenses	10%	12%
Regular Interview (2 votes needed)	76%	57%
Expedited Interview (1 vote needed)	18%	81%
Majority Vote Interview (5 votes needed)	6%	28%
B. Hearings Types by Percentage		
Minimum	53%	60%
Review	37%	55%
Violator	11%	55%

Notes:

N = 133,789 minimum sentence hearings, 93,644 review hearings, and 27,286 violator hearings.

Table 3 – Post-Release Outcomes

Parolee Outcomes - One Year After Release	**Mean**
Recidivism	36%
Arrest	22%
PV Reincarceration	17%
Minor Parole Violation	37%
Employed (Ever)	48%
Employed at 6 Months	37%

Notes:

N = 127,770 releases to parole

Figure 3 – Variation in Parole Leniency

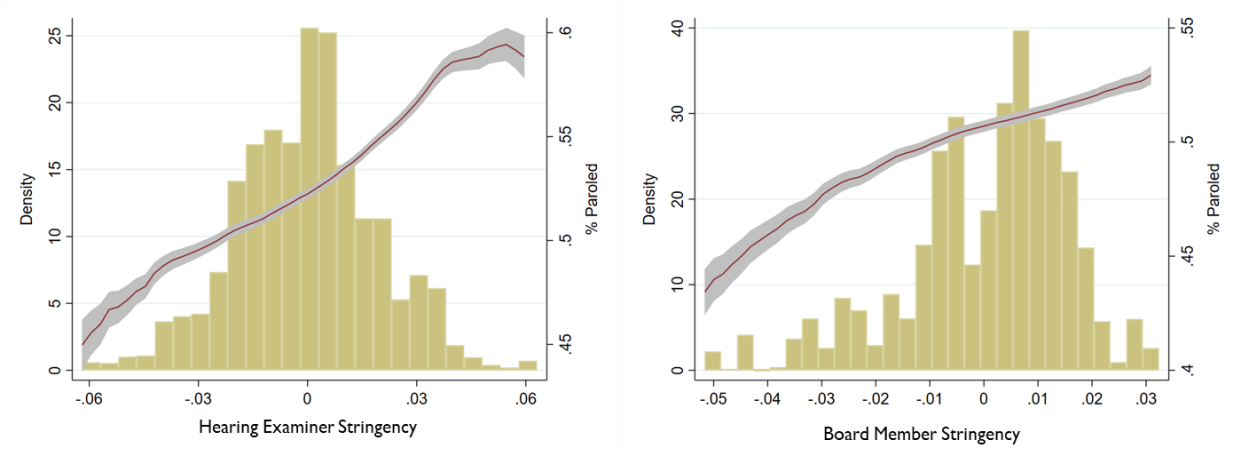


Table 4 – Instrument Validity

Instrument Relevance, Exogeneity, and Monotonocity

	Hearing Examiner Leniency	Board Member Leniency
A. Relevance		
First stage f-value	361	446
B. Exogeneity		
Joint f-test p-value	0.33	0.33
C. Within-Subsample Monotonicity		
Violent Crime	0.71***	1.24***
Drug Crime	1.04***	0.37***
Property Crime	0.95***	0.79***
Minimum Parole Hearing	1.00***	0.85***
Review Parole Hearing	0.65***	0.86***
Regular Interview (2 votes needed)	0.89***	0.83***
Expedited Interview (1 vote needed)	1.13***	na
Majority Vote Interview (5 votes needed)	0.57***	1.27***
D. Across-Subsample Monotonicity		
Violent Crime	0.66***	0.99***
Drug Crime	1.02***	0.31***
Property Crime	0.99***	0.74***
Minimum Parole Hearing	0.52***	0.61***
Review Parole Hearing	0.41***	0.69***
Regular Interview (2 votes needed)	0.39***	0.19***
Expedited Interview (1 vote needed)	0.79***	na
Majority Vote Interview (5 votes needed)	0.58***	1.23***

Notes:

N = 205,476 parole hearings with an assigned board member.

Panel A presents first stage f-values from regressions of parole outcome on hearing examiner and board member leniency measures, controling for observable characteristics about the individual. Panel B presents joint f-test p-values from regressions of hearing examiner and board member leniecy measures on observable characteristics about the individual. Panels C and D present estimates and statistical significance (* <0.10, **<0.05, ***<0.01) from regressions of parole outcome on hearing examiner and board member leniency measures, 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.

Table 5 – Effects of Early Release

	A1. 1 Year After Eventual Release to Parole			A2. OLS w/o Covariates			A3. OLS w Covariates		
Outcomes	% Change	Estimate	*SE*	% Change	Estimate	*SE*	% Change	Estimate	*SE*
Recidivism	16%	0.06	*(.05)*	-8%	**-0.03** ***	*(.002)*	0%	0.00	*(.002)*
Arrest	3%	0.01	*(.04)*	-8%	**-0.02** ***	*(.002)*	-2%	**-0.01** ***	*(.002)*
PV Reincarceration	36%	**0.07** **	*(.04)*	-12%	**-0.02** ***	*(.002)*	1%	0.00	*(.002)*
Minor Parole Violation	25%	**0.09** **	*(.04)*	-2%	**-0.01** ***	*(.002)*	-2%	**-0.01** **	*(.002)*
Employed (Ever)	-13%	-0.06	*(.05)*	8%	**0.04** ***	*(.003)*	1%	0.00	*(.003)*
Employed at 6 Months	-18%	-0.06	*(.05)*	8%	**0.03** ***	*(.002)*	0%	0.00	*(.003)*
Months Until Release		**-10.2** ***	*(.97)*		**-15.7** ***	*(.106)*		**-13.1** ***	*(.074)*

	B1. 2 Year Window 5 Years After Interview			B2. 2 Years After Interview			B3. 5 Years After Interview		
Outcomes	% Change	Estimate	*SE*	% Change	Estimate	*SE*	% Change	Estimate	*SE*
Recidivism	38%	**0.14** *	*(.08)*	38%	**0.23** ***	*(.04)*	11%	0.03	*(.06)*
Arrest	39%	0.13	*(.08)*	25%	**0.13** ***	*(.04)*	0%	0.00	*(.06)*
PV Reincarceration	85%	0.03	*(.03)*	83%	**0.16** ***	*(.03)*	94%	**0.09** *	*(.05)*
Minor Parole Violation	-52%	-0.04	*(.04)*	87%	**0.271** ***	*(.04)*	48%	0.09	*(.06)*

Notes:

Outcomes are measured as whether the event took place anytime during the noted period. N = 209,054 for Columns A1, A2, and A3, 120,943 for Column B1, 253,910 for Column B2, and 199,809 for Column B3. Data in Columns A1, A2, and A3 is restricted to releases and eventually paroled refusal interviews. Data in Column B1 is restricted to interviews that took place for prisoners with 5 or less years left on their sentence.

Figure 4 – Incarcerated Individuals Near the Margin of Release in Pennsylvania

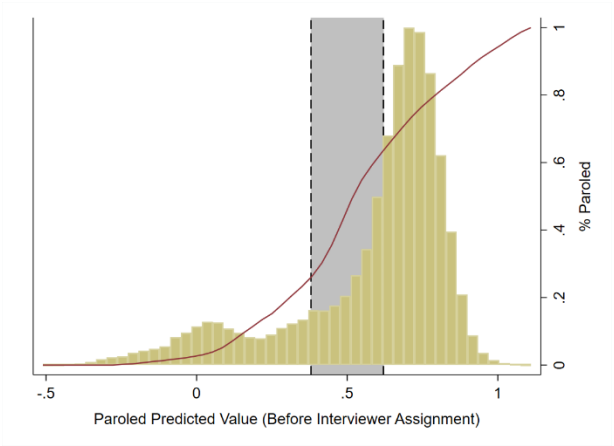


Figure 5 – Variation in LSI-R Scores

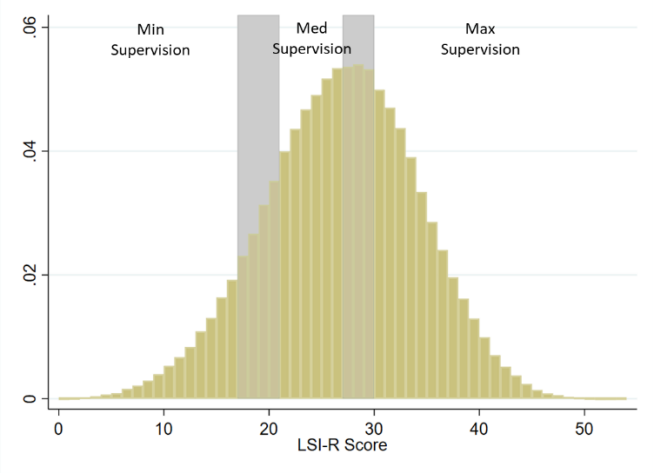


Table 6 – Effects of Supervision Level

	All Releases			Releases Away from Margin of Release		
1 Year Post-Release Outcomes	% Change	Estimate	*SE*	% Change	Estimate	*SE*
Low to Medium						
Recidivism	*-10%*	**-0.026** **	*(.01)*	*-7%*	-0.017	*(.01)*
Arrest	*-4%*	-0.006	*(.01)*	*1%*	0.002	*(.01)*
PV Reincarceration	*-9%*	**-0.018** *	*(.01)*	*-6%*	-0.013	*(.01)*
Minor Parole Violation	*-3%*	-0.011	*(.01)*	*-4%*	-0.013	*(.01)*
Ever Employed	*0%*	0.000	*(.01)*	*1%*	0.005	*(.01)*
Employed at 6 Months	*-3%*	-0.013	*(.01)*	*-1%*	-0.003	*(.01)*
Medium to High						
Recidivism	*5%*	**0.020** *	*(.01)*	*6%*	**0.024** **	*(.01)*
Arrest	*9%*	**0.021** **	*(.01)*	*11%*	**0.025** ***	*(.01)*
PV Reincarceration	*4%*	0.014	*(.01)*	*5%*	0.016	*(.01)*
Minor Parole Violation	*-3%*	-0.011	*(.01)*	*-4%*	-0.016	*(.01)*
Ever Employed	*-2%*	-0.007	*(.01)*	*0%*	0.000	*(.01)*
Employed at 6 Months	*0%*	-0.001	*(.01)*	*1%*	0.003	*(.01)*

Notes:
N = 33,310 for Column A1, 25,522 for Column A2, 48,685 for Column B1, and 38,284 for Column B2.
Column 2 restricts the sample to individuals for whom parole is recommended by both the prison superintendent and decisional instrument.

Table 7 – Parole Conditions

Discretionary Conditions	Mean
Total	6.6
Restriction Conditions	
Curfew	46%
Community Corrections Center Residency	26%
Cannot Enter Alcohol Establishments	88%
Cannot Possess Ammo	59%
Require Permission to Drive	70%
Companion Conditions	
Cannot Contact Codefendants or Gangs	29%
Cannot Contact Drug Users or Sellers	76%
Cannot Contact Victims	49%
Must Support Dependents	42%
DV Protocol	16%
Payment Restitution via Wage Attachment	18%
Drug Support Conditions	
Attend Alcohol Support Group	45%
Complete Outpatient Treatment	49%
Treatment Evaluation Required	29%
Take Psychiatric Medicine if Prescribed	18%

Non-Discretionary Conditions	Mean
Drug Testing Required	97%
Must Maintain Employment or Active Job Search	94%
Cannot Consume or Possess Alcohol	95%
Supervision Fee	100%
Electronic Monitoring	4%
Travel Restrictions	1%
Sex Offender Protocol	1%

Notes:
N = 127,770 releases to parole.

Figure 6 – Variation in Discretionary Condition Leniency

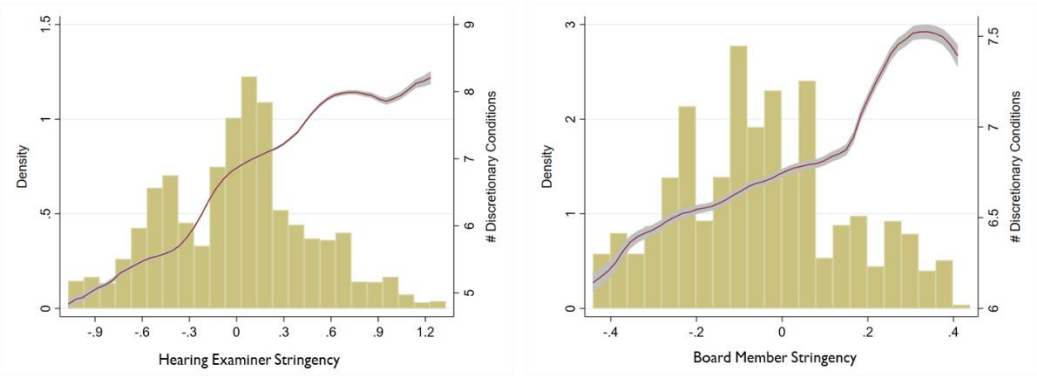


Table 8 – Effect of Total Number of Discretionary Conditions

	All Releases			Releases Away From Margin of Release			OLS, All Releases		
1 Year Post-Release Outcomes	% Change	Estimate	*SE*	% Change	Estimate	*SE*	% Change	Estimate	*SE*
Recidivism	3%	0.009	*(.006)*	1%	0.002	*(.006)*	2%	**0.007** ***	*(.001)*
Arrest	0%	0.000	*(.005)*	-1%	-0.003	*(.005)*	0%	0.000	*(.001)*
PV Reincarceration	10%	**0.016** ***	*(.004)*	6%	**0.009** *	*(.005)*	5%	**0.009** ***	*(.001)*
Minor Parole Violation	3%	**0.011** **	*(.006)*	1%	0.004	*(.006)*	3%	**0.010** ***	*(.001)*
New Restrictions	7%	**0.008** **	*(.004)*	6%	0.006	*(.004)*	7%	**0.007** ***	*(.000)*
Written Warning	3%	0.007	*(.005)*	1%	0.003	*(.005)*	2%	**0.004** ***	*(.001)*
Ever Employed	2%	0.011	*(.007)*	2%	0.010	*(.007)*	0%	0.000	*(.001)*
Employed at 6 Months	1%	0.004	*(.007)*	1%	0.004	*(.007)*	0%	0.000	*(.001)*

Notes:

Outcomes are measured as whether the event took place anytime during the first year post-release.

N = 127,030 for Columns 1 and 3 and 95,620 for Column 2.

Column 2 restricts the sample to individuals for whom parole is recommended by both the prison superintendent and decisional instrument.

Table 9 – Effects of Individual Discretionary Conditions

	Arrested			PV Reincarceration			Minor Parole Violation			Ever Employed		
	% Change	Estimate	*SE*	% Change	Estimate	*SE*	% Change	Estimate	*SE*	% Change	Estimate	*SE*
Restriction Conditions												
Curfew	7%	0.01	*(.03)*	-22%	-0.04	*(.02)*	13%	0.05	*(.03)*	7%	0.03	*(.04)*
Cannot Enter Alcohol Establishments	-12%	-0.02	*(.07)*	66%	0.09	*(.06)*	-1%	0.00	*(.08)*	-35%	**-0.17** *	*(.10)*
Cannot Possess Ammo	30%	**0.06** **	*(.03)*	9%	0.01	*(.02)*	5%	0.02	*(.03)*	-7%	-0.04	*(.04)*
Require Permission to Drive	-2%	0.00	*(.02)*	5%	0.01	*(.02)*	5%	0.02	*(.02)*	8%	0.04	*(.03)*
Companion Conditions												
Cannot Contact Drug Users or Sellers	33%	0.07	*(.05)*	57%	**0.08** *	*(.04)*	61%	**0.16** ***	*(.05)*	10%	0.05	*(.07)*
Must Support Dependents	12%	0.02	*(.06)*	9%	0.02	*(.05)*	-20%	-0.07	*(.07)*	14%	0.07	*(.08)*
Payment Restitution via Wage Attachment	-7%	-0.01	*(.02)*	35%	**0.06** ***	*(.02)*	-2%	-0.01	*(.03)*	-5%	-0.03	*(.04)*
Drug Support Conditions												
Attend Alcohol Support Group	-77%	**-0.17** **	*(.07)*	29%	0.04	*(.06)*	-6%	-0.02	*(.08)*	9%	0.04	*(.13)*
Complete Outpatient Treatment	-18%	-0.04	*(.06)*	-5%	-0.01	*(.06)*	8%	0.03	*(.07)*	27%	0.13	*(.08)*
Treatment Evaluation Required	-24%	-0.05	*(.07)*	45%	0.07	*(.06)*	43%	**0.15** *	*(.08)*	0%	0.00	*(.10)*
Conditions With Little Variation												
Community Corrections Center Residency	133%	0.28	*(.45)*	-2%	0.00	*(.35)*	164%	0.61	*(.43)*	94%	0.48	*(.52)*
DV Protocol	288%	0.63	*(1.4)*	539%	0.88	*(1.1)*	-238%	-0.86	*(1.4)*	359%	1.73	*(1.2)*
Cannot Contact Codefendants or Gangs	-89%	-0.19	*(.14)*	19%	0.03	*(.13)*	0%	0.00	*(.17)*	64%	0.31	*(.19)*
Cannot Contact Victims	27%	0.06	*(.20)*	-133%	-0.20	*(.16)*	-85%	-0.31	*(.20)*	-47%	-0.23	*(.32)*
Take Psychiatric Medicine if Prescribed	-432%	-0.94	*(2.2)*	-955%	-1.49	*(2.4)*	835%	2.98	*(3.1)*	438%	2.22	*(4.5)*

Notes:

Outcomes are measured as whether the event took place anytime during the first year post-release.

N = 127,030.

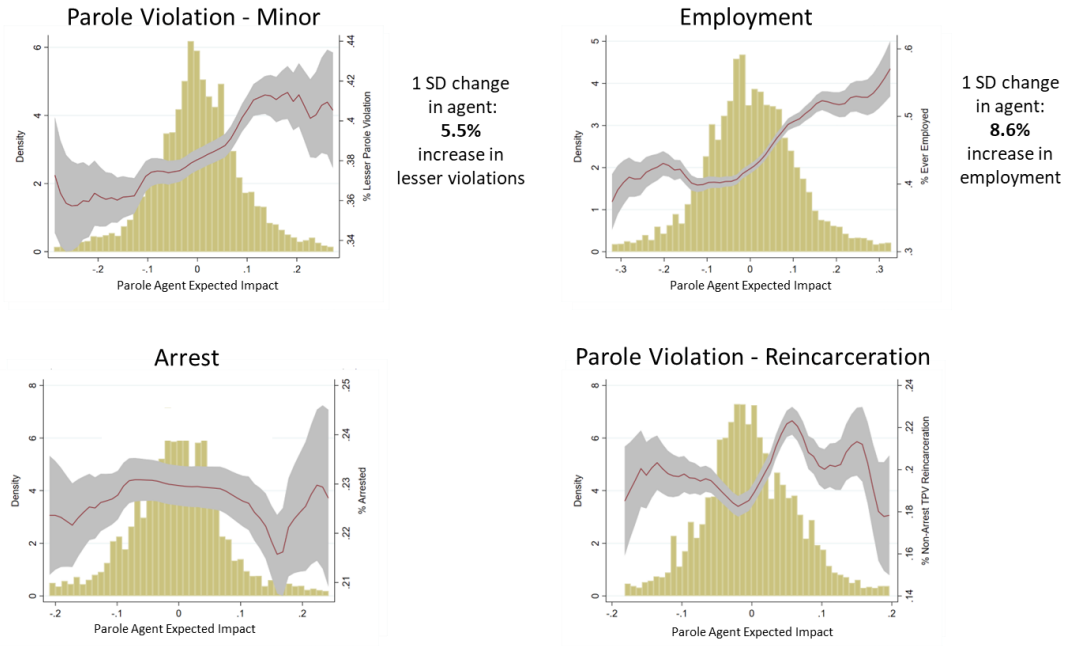
Table 10 – Leave-one-out Officer Effect Exogeneity

Instrument Exogeneity Table

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

Notes:
N = 127,112.

Figure 7 – Officer Leave-one-out Effects



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Appendix A

	Hearing Examiner Leniency	Board Member Leniency
A. Relevance		
First stage f-value	860	924
B. Exogeneity		
Joint f-test p-value	0.15	0.45
C. Within-Subsample Monotonicity		
Violent Crime (paroled on leniency - estimate)	1.07***	0.96***
Drug Crime	1.39***	1.28***
Property Crime	1.52***	1.13***
Minimum Parole Hearing	1.39***	1.32***
Review Parole Hearing	1.36***	1.07***
Regular Interview (2 votes needed)	1.30***	1.17***
Expedited Interview (1 vote needed)	1.48***	na
Majority Vote Interview (5 votes needed)	1.46*	0.12
D. Across-Subsample Monotonicity		
Violent Crime (paroled on leniency - estimate)	1.20***	0.90***
Drug Crime	1.50***	1.36***
Property Crime	1.86***	1.19***
Minimum Parole Hearing	1.36***	1.42***
Review Parole Hearing	1.67***	1.10***
Regular Interview (2 votes needed)	0.78***	0.08***
Expedited Interview (1 vote needed)	1.75***	na
Majority Vote Interview (5 votes needed)	1.94***	0.33

Notes:

N = 127,030 parole hearings that resulted in parole.

Panel A presents first stage f-values from regressions of total number of parole special conditions on hearing examiner and board member leniency measures, controling for observable characteristics about the individual. Panel B presents joint f-test p-values from regressions of hearing examiner and board member leniency measures on observable characteristics about the individual. Panels C and D present estimates and statistical significance (* <0.10, **<0.05, ***<0.01) from regressions of total number of parole special conditions on hearing examiner and board member leniency measures, 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.