Parole Supervision at the Margins

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Abstract:

Over 70% of U.S. prisoners are released under parole supervision. 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. Second, I separately identify the effects of the three major aspects of parole supervision – (1) supervision intensity, (2) special conditions such as curfew or placement in a halfway house, and (3) the assigned parole officer who manages supervision – by leveraging three separate quasi-random assignment mechanisms in Pennsylvania. Along most margins, I find that more intensive 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). Despite the scale of the U.S. prison system, we have little understanding of its long-term effects. While several studies have found negative effects of incarceration on post-release outcomes (e.g. Mueller-Smith 2015; Mueller-Smith & Schnepel 2021; DiTella & Schargrodsky 2013; Henneguelle et al. 2016) others have found null to positive effects of incarceration (e.g. Loeffler 2013; Bhuller et al. 2020). 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 when making policy decisions regarding who to incarcerate, how to incarcerate, and for how long.

Further, note that 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). 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. 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.

This work answers two sets of questions. First, what is the effect of additional time incarcerated, prior to release, on post-release outcomes such as recidivism and employment? Second, how does parole supervision impact post-release outcomes, and *why* does parole supervision have this affect? That is, what specific aspects of parole supervision drive re-entry

outcomes, to what extent, and for whom? I answer these questions using individual-level data on the Pennsylvania prison and parole populations between 2005 and 2020. I tease out the causal effects of additional time incarcerated and community supervision policies by leveraging three separate quasi-random assignment mechanisms in Pennsylvania.

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 (1) whether the individual should be released to serve the remainder of their sentence under community supervision, and, if released, (2) which special conditions to impose on the individual while under parole supervision such as curfew, drug and alcohol treatment, contact restrictions, or placement in a community corrections center (i.e., halfway house). As different parole board members and hearing examiners have different propensities to grant parole and assign certain conditions, I leverage the random assignment of these decision makers to parole board hearings to estimate the effects of (1) early release and (2) board-imposed special conditions.

Second, individuals released under parole in Pennsylvania are assigned one of several supervision intensity levels, which dictate how often and in what form (in person, over the phone, etc.) they must meet with their assigned parole agent (i.e., parole officer). In Pennsylvania, these levels are determined almost exclusively by an inmate's score on the Level of Service Inventory – Revised (LSI-R) recidivism risk test, with cutoff values separating supervision levels. I leverage these discontinuities, around two separate risk score cutoff values (minimum vs. medium supervision, medium vs. high supervision), to estimate the effects of different levels of supervision intensity. Third, I leverage the quasi-random assignment of parolees to parole agents to estimate the relative importance of an assigned parole agent, who oversees compliance with these other conditions, on parolee outcomes.

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 the negative effects of *any* time incarcerated, found in a subset of the previous literature, 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, among individuals at the margin of receiving certain conditions upon release, I find that additional special conditions assigned by the parole board (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. Moving from a low (meeting once every three months) to medium (meeting once every month) supervision level 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 of supervision. Finally, one's assigned parole agent 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 agent once every month a opposed to once every three months, among individuals at that margin. However, in most situations – such as additional special conditions and meeting with an agent more often than once a month – additional supervision conditions, among individuals at the margin of receiving them, appear to either have no effect, or a negative effect, on reentry.

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) finds criminogenic effects of incarceration in Texas using the random assignment of defendants to judges. Mueller-Smith & Schnepel (2021) also find criminogenic effects of incarceration in Texas. Specifically, they observe a decrease in recidivism and increase in

employment for individuals who are diverted from the criminal justice system when comparing individuals on either side of a discontinuity in program eligibility, as well as strong evidence that the results are driven by the stigma associated with a felony conviction. DiTella & Schargrodsky (2013) and Henneguelle et al (2016) both find that electronic monitoring diversion programs decrease recidivism relative to prison, using a randomized judge design in Argentia and difference-in-differences design in France, respectively. Loeffler (2013), however, finds no effects of incarceration on future recidivism in Illinois using a randomized judge design. In addition, Petrich et al. (2021) argue that incarceration in the United States has either no effect or a slight criminogenic effect on recidivism based on a meta-analysis of 116 studies that leverage research designs ranging from matching to randomized controlled trials. Finally, Bhuller et al (2020) find the opposite result in Norway - that incarceration decreases future recidivism and increases employment - using a similar randomized judges design. They find that this result 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 there.

However, less evidence exists on the effects of incarceration at the intensive margin – the effect of additional time incarcerated once already 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, while 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, 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.

The second set of research focuses on the effects of parole supervision. There is little causal evidence about its effects. Among existing studies, 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.

Finally, 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 and 5 describe the empirical model, tests for instrument validity, and results for the early release and parole effects analyses, respectively. 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. If the hearing is successful, he is released into the community under parole supervision, pending good behavior while on parole, until the completion of his sentence. If the hearing is not successful, he remains incarcerated and is scheduled for a subsequent parole hearing at a later date.

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 a fixed 6 year term. 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 quasi-randomly assigned a hearing date at their facility of residence. Explicitly, there are four steps to the scheduling process each month. 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. Third, the parole board scheduler provides the list of selected dates 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 (b) whether the inmate was convicted of a violent or non-violent crime, as individuals convicted of violent and non-violent crimes are often scheduled on separate dates. 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 1A. 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." These individual interviewer choices are shown in Figure 2.

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 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 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 hearings.

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 (Figure 1B). 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 (Figure 1C). 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.

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 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. Second, the parolee must meet with their assigned parole agent a certain number of times each month, determined by their designated supervision intensity level. Third, the assigned parole agent has wide discretion over the tenor of the parolee / agent relationship, how to cultivate it, and whether and how to sanction the parolee when he breaks a condition or misses a meeting.

Special parole conditions, such as curfew, drug testing, and restrictions on social contacts are initially imposed by parole hearing voters. 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 agents have discretion to impose additional special conditions in response to parole violations.

Next, parole supervision level determines both the regularity with which a parolee must meet with his parole agent and the regularity with which the parole agent 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).[2]

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.[3]

[2] 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 agent 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).

[3] 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

Finally, parole agents 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. As discussed in LaForest (2021), 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 randomly assigned to parole agents based only on agent 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 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 Caucasian 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

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.

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 substantially less likely to be paroled. Additionally, individuals eligible for expedited interviews (1 vote needed) are paroled 81% of the time, while violent offenders who require majority board approval (5 votes needed) are only paroled 28% of the time. 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.

Table 3 provides data on parole conditions assigned at release. In Pennsylvania, initial parole conditions are selected by hearing examiners and parole board members, as part of the parole hearing process, as discussed above. These include conditions assigned to nearly every parolee in Pennsylvania, such as not consuming alcohol, mandatory drug testing, maintaining employment or an active job search, and paying one's own supervision fees.[4] 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.

Finally, Table 4 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 lesser 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 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

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

$Paroled_{it}^{*}$ 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 not provide an unbiased estimate of the effects of early release as less risky individuals are 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 two 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, violent offender, and hearing type 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) they are assigned to 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

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

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. This procedure produces an unbiased estimate of the effects of parole as long as the instruments meet three conditions – relevance, exogeneity, and monotonicity (Dobbie et al., 2018; Bhuller et al., 2020).

Instrument Relevance

Instrument relevance requires variation in parole leniency across board members and hearing examiners. 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 parole outcome on interviewer stringency. For both assigned hearing examiners and board members, the likelihood of parole monotonically increases in leave-one-out interviewer leniency (with the exception of hearing examiners in the upper tail of interviewer leniency) and is close to linear.

I also conduct an f-test for weak instruments to further assess the relevance of the leniency measures. As shown in Table 5, 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

Instrument exogeneity requires interviewer assignment to be orthogonal to all other characteristics that impact parole, within interview facility, year, violent offender and interview-type groups. In addition to qualitative discussions with Pennsylvania Parole Board personnel confirming the quasi-random assignment mechanism of interviewers to parole hearings (as discussed in Section 2), this independence is shown in Table 5, Panel B. Specifically, neither hearing examiner or board member stringency measures appear to be correlated with interviewee personal characteristics.

Instrument Monotonicity

In order to interpret the 2SLS estimates as the local average treatment effects of early release, among individuals near the margin of release, interviewers must have monotonic preferences regarding the release of those individuals. That is, a "lenient" interviewer must be more lenient to all types of individuals near the margin of release than a "stringent" interviewer, 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 subsets of interviewees (within-subsample monotonicity). I specifically investigate subsets of individuals convicted of different crime types, individuals at a minimum sentence parole hearing or review parole hearing, and individuals at a hearing with one, two, or five required votes to parole. 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 (across-subsample monotonicity).

Results

Table 6 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 affects 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 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 time. Overall, these results suggest that early release increases recidivism. They imply that the criminogenic effects of incarceration appear to be concentrated on the intensive margin (any incarceration), with little observable criminogenic effects of additional time incarcerated among individuals who have served at least half their sentence.

5. The Effects of Parole

Research Design – Special Conditions

Next, I turn to the effects of parole itself on post-release outcomes, by first investigating the effects or parole special conditions. The main specification to evaluate the effects of board-

imposed special parole conditions mirrors Equations 1-4 above, except (1) the explanatory variable of interest is either "total number of conditions" or "a single specific condition," instead of "parole approval / denial," and (2) 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 control for 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

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

I next 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 monotonous.

Instrument Relevance, Exogeneity, and Monotonicity

Figure 4 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.

Board member instruments also pass several exogeneity and monotonicity tests, and hearing examiner instruments pass several monotonicity tests, as shown in Appendix Table A1. However, note that hearing examiner instruments show some evidence of correlation with decisional instrument characteristics, such as violence risk score and DOC behavioral offenses. Specifically, a regression of the hearing examiner stringency measure on interviewee personal characteristics yields a joint f-test p-value of .02. As such, to present conservative estimates I omit hearing examiner instruments from the primary "special conditions" regressions discussed below. Note, however, that results are appreciably similar when these instruments are included.[5]

Exclusion Restrictions

One final 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.

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. (specifically, these are individuals who have a very high predicted probability of release prior to interviewer assignment). 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

[5] 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.

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 top 25% predicted parole probabilities from a regression of parole likelihood on parolee characteristics prior to interviewer assignment, etc.).

Results – Special Conditions

Table 7 presents results of the effects of "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 lesser parole violations (ones that results in a written warning or new restrictions) – each additional assigned condition is associated with a 2% increase in recidivism and a 3% increase in the chances of receiving a lesser 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 lesser 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 slightly less significant, to the main results in Column 1.

Based on these results, among individuals at the margin of receiving an additional condition, each additional parole condition have no discernable impact on new arrests or employment. Each additional condition only increases, slightly, 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 committing 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 appears to decrease new 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 definitive estimates of their effects.

Research Design - Supervision Level

I estimate the effect of parole supervision intensity level using regression discontinuity designs around two separate 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 the same for alternative cutoff value selections.

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).

Exclusion Restriction

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 these decisions, then the results of this analysis could be biased. To address this concern, I estimate several alternative specifications that restrict the sample to subsets of individuals away from the parole margin of release. These are 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 9. 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 effect 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 to consistently meet, leading to more arrests and violations for non-compliance. Given the inherent trade off with community supervision between deterrence and

[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.

burden, these results suggest that one meeting per month best balances these competing effects among individuals at both the low and high margins of recidivism risk.

Effects of Assigned Parole Agent

In Pennsylvania, parolees are assigned to parole agents randomly within parole agent units. I use this variation to estimate the extent to which assigned parole agents impact parolee outcomes. As discussed extensively in LaForest (2021), assigned parole agents have a large effect on parolee documented employment and parole violations, but appear to have little effect on new arrests. Specifically, using leave-one-out "outcome" propensity measures, I find a one standard deviation decrease in parole agent "lesser violation" propensity leads to a 5.5% decrease in lesser violations, and a one standard deviation increase in parole agent "employment" propensity leads to a 6.6% increase in documented employment. While the parolee/agent relationship does not appear to decrease future parolee crimes, agent discretion does appear to impact documented parolee employment and future sanctions. However, it is unclear whether this impact is driven primarily by changes in parolee behavior or by variation in agent response to parolee behavior, as discussed in LaForest (2021).

6. Discussion

This work shows that the age-out effects of additional time incarcerated appear to dominate the combination of criminogenic effects of additional time incarcerated and stigma effects of parole denial. Results imply that the criminogenic effects of incarceration appear to accrue early on during an individual's incarcerated stay, with little to any criminogenic effects of incarceration during the second half of the stay. From a policy perspective, in order 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.

Regarding the effects of parole supervision, the overall effects of parole are driven by three separate mechanisms – special conditions (both discretionary and mandatory), supervision intensity level, and a parolee's relationship with his assigned parole officer. This work shows how the overall effects of parole are driven by these individual mechanisms. 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. 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. Finally, parole agent discretion appears to have little affect on new arrests, but does impact both documented parole violations and employment. The fourth aspect of parole, mandatory special conditions such as drug testing, work requirements, and payment of supervision fees, is the only mechanism whose effects cannot be separately identified within the parole system in Pennsylvania.

From a policy perspective, these results recommend assigning less special conditions to individuals at the margin of receiving each condition, and setting once-a-month meeting requirements for individuals at both the high and low recidivism risk level margins. In addition, they recommend further evaluation about exactly which types of parolee-parole agent relationships foster employment and minimize parole violations (see LaForest 2021 for a detailed discussion of these relationships). Overall, these results recommend that policy makers looking to improve parole outcomes should focus on setting parole conditions only when necessary, setting appropriate supervision levels for each individual parolee, and investigating ways to better foster constructive parolee-parole agent relationships in order to improve re-entry success.

Tables and Figures

Figure 1 – Hearing Structure

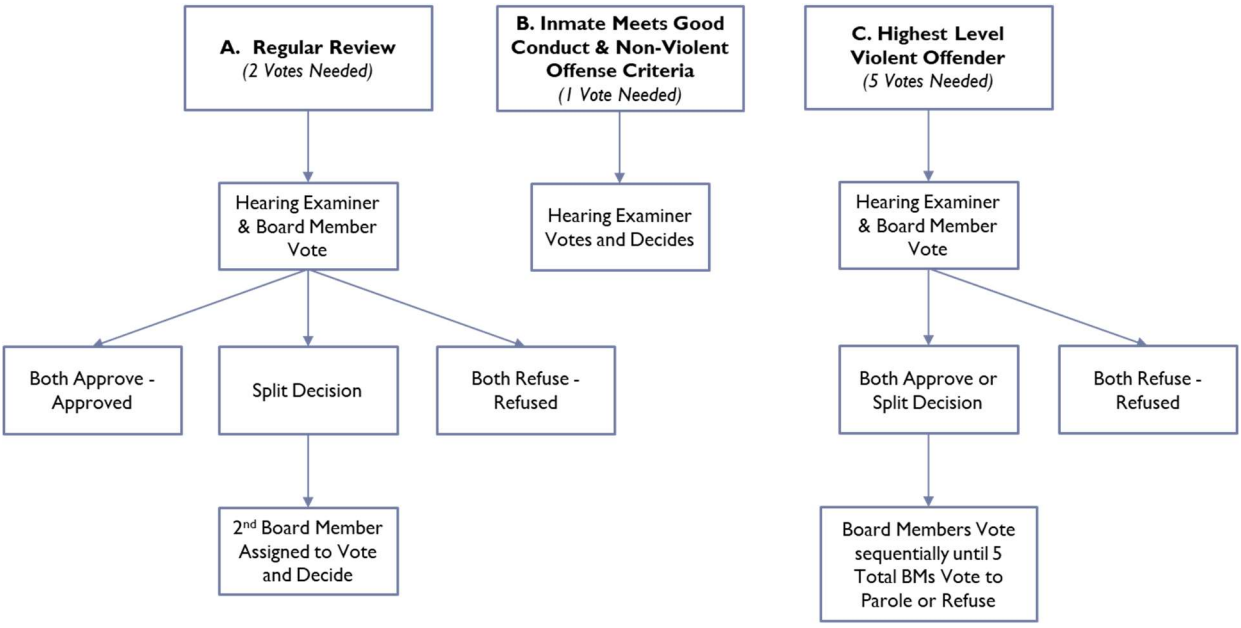

Figure 2 – Individual Voter Decisions

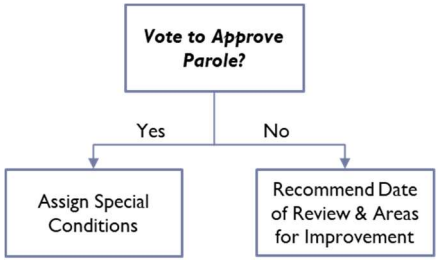

Table 1 – Incarcerated Population at Time of Minimum Sentence Hearing

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 – 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.

Table 4 – Post-Release Outcomes

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

Notes:
N = 127,770 releases to parole

Figure 3 – Variation in Parole Leniency

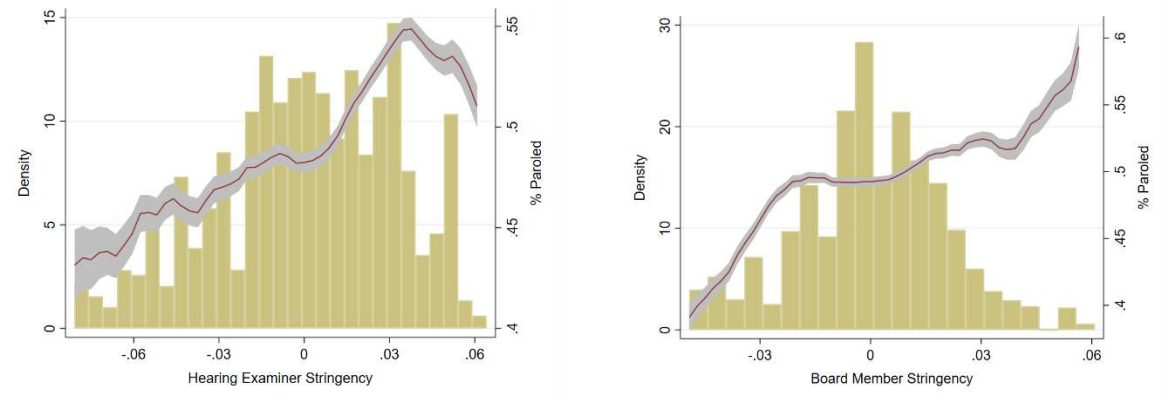

Table 5 – Instrument Validity

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 6 – 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)*
Non-Arrest TPV Reincarceration	36%	**0.07** **	*(.04)*	-12%	**-0.02** ***	*(.002)*	1%	0.00	*(.002)*
Lesser 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)*
Non-Arrest TPV Reincarceration	85%	0.03	*(.03)*	83%	**0.16** ***	*(.03)*	94%	**0.09** *	*(.05)*
Lesser 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 – Variation in Discretionary Condition Leniency

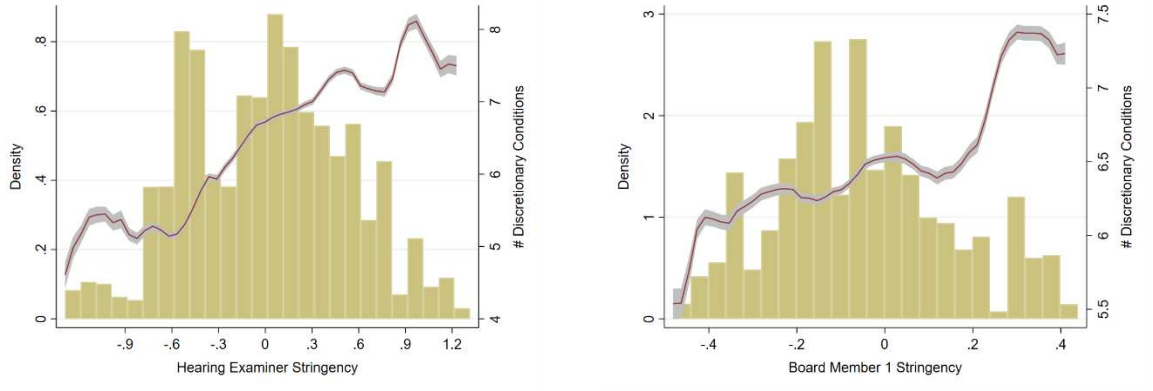

Table 7 – Effect of Total Number of Discretionary Conditions

	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)*
Non-Arrest TPV Reincarceration	10%	**0.016** ***	*(.004)*	6%	**0.009** *	*(.005)*	5%	**0.009** ***	*(.001)*
Lesser 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 8 – Effects of Individual Discretionary Conditions

	Arrested			Non-Arrest TPV Reincarceration			Lesser 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.

Figure 5 – Variation in LSI-R Scores

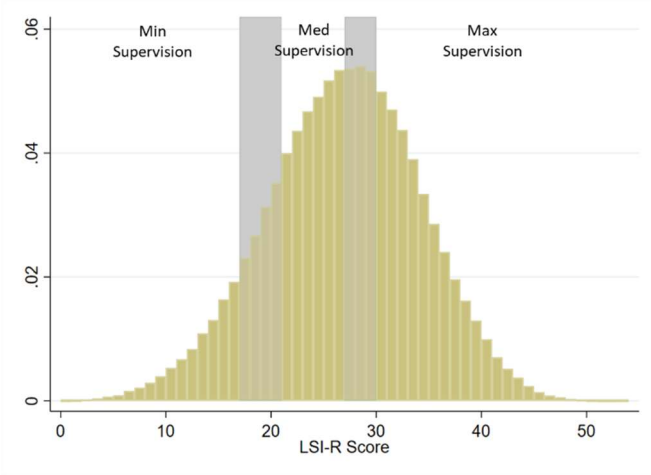

Table 9 – Effects of Supervision Level

	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)*
Non-Arrest TPV Reincarceration	*-9%*	**-0.018** *	*(.01)*	*-6%*	-0.013	*(.01)*
Lesser 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)*
Non-Arrest TPV Reincarceration	*4%*	0.014	*(.01)*	*5%*	0.016	*(.01)*
Lesser 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, 30,522 for Column A2, 48,685 for Column B1, and 43,284 for Column B2
Column 2 restricts the sample to individuals for whom parole is recommended by both the prison superintendent and decisional instrument.

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Appendix

A1. Total Special Conditions Instrument Relevance, Exogeneity, and Monotonocity

	Hearing Examiner Leniency	Board Member Leniency
A. Relevance		
First stage f-value	860	924
B. Exogeneity		
Joint f-test p-value	0.02	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.