# Parole Supervision on the Margins

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Abstract:

Recidivism and unemployment rates are exceedingly high for previously incarcerated individuals in the United States. This work investigates whether, and how, post-release supervision can reduce these rates and improve post-release success. First, I investigate the effects of early release from prison to parole – 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, with similar rates of post-release employment. Second, I estimate the effects of each major component of parole supervision – supervision intensity and required special conditions such as curfew or placement in a halfway house – on post-release success, leveraging 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 more 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 and post-release supervision and support to motivate decisions about whether, how, and how long to incarcerate, and how best to facilitate reentry.

Nearly three-fourths of incarcerated individuals are released under parole supervision in the United States (Carson 2020). Upon release, the recidivism rate for individuals under parole supervision is even higher than for individuals released without supervision. E.g., in Pennsylvania in 2016, the three-year recidivism rates for individuals released under parole supervision was 66.2% relative to 56.8% for individuals released at the completion of their sentence (Pennsylvania Department of Corrections 2022). Not surprisingly – given these poor reentry outcomes – relatively little is known about the effects of supervised release. 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 explores two sets of questions. The first is how additional time incarcerated, prior to release, affects post-release outcomes such as recidivism and employment. That is, once an individual is incarcerated, is there a criminogenic impact of additional time incarcerated relative to early release? The second is how parole supervision, and each individual aspect of parole supervision, affects post-release success. That is, what specific aspects of post-release supervision impact re-entry success, to what extent, and for whom?

Existing studies have reached different conclusions about the effects of incarceration on later-life outcomes. 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). Two overarching conclusions from this work are that the effects of incarceration on post-release outcomes are context-specific (Doleac & LaForest 2022) and that the settings where a prison stay reduces recidivism are potentially those where rehabilitation is emphasized (Loeffler & Nagin 2022).

Less research exists on the effects of incarceration along the intensive margin – the effect of additional time incarcerated once incarcerated. Among existing studies, several find no effect. Kling (2006) finds no effect of longer prison sentences on future employment, Green & Winik (2010) find no effect on recidivism for individuals convicted of drug-related offenses, and Aurora (2018) finds no effect on county level crime (using a randomized judge designs in Florida and California, a randomized judge design in Washington DC, and differences in political affiliation of county prosecutorial offices after close elections across the U.S., respectively). However, Kuziemko (2013) finds that additional prison time decreases recidivism in Georgia (using discontinuities in parole board guidelines and the effect of a mass prisoner release in 1981). In addition, 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), likely because the opportunity for parole provides incentives to pursue good behavior and program participation while incarcerated.[2]

Regarding community supervision, the population of individuals in community corrections programs in the United States is extremely large – three people are living under correctional community supervision for every person currently incarcerated in the United States (Oudekerk & Kaeble 2021). Surprisingly – given the size and scope of the community corrections system in the United States – little causal evidence exists about the effects of community supervision. The most thorough research on the topic consists of several randomized controlled trials comparing specific intensive probation programs to standard probation programs, each of which finds no effect (Petersilia & Turner 1993; Lane et al. 2005; Henneguelle et al. 2010; Hyatt & Barnes 2014; Barnes et al. 2012).

[2] As further evidence that the effects of incarceration are context-specific, estimates of the effect of incarceration on the intensive margin differ widely across countries. Landersø (2015) finds that increased incarceration in Denmark improved employment outcomes (leveraging a policy reform in Denmark that increased incarceration length by one-to-two months), potentially due to additional participation in in-custody rehabilitation programs. Arbour & Marchand (2022) and Meier et al. (2020) instead find negative effects. Arbour & Marchand (2022) find that low-risk paroled individuals in Quebec have lower recidivism rates than individuals denied parole, while Meier et al. (2020) finds that early release in Israel reduces returns to prison (using a randomized parole board member and randomized judge design, respectively).

Few studies exist, however, on the effects of post-incarceration parole supervision or the individual components of community supervision. Recent work by Banan (2022) finds that parole supervision – relative to release without supervision – leads to a short-term increase in parole violations that offsets a slight decrease in new crimes, with no long-term effects (using a regression discontinuity design in North Carolina). Zapryanova (2020) finds no effect of additional time spent under parole supervision on returns to prison (using a randomized judge design along with discontinuities in parole board guidelines in Georgia). Georgiou (2014) finds no effect of increased supervision intensity (using a regression discontinuity design in Washington) and Lee (2023) finds a negative effect of halfway house residency upon release (using a randomized case worker design in Iowa). Finally, Rose (2021) finds that incarcerating individuals for probation violations has little deterrent effect but does increase racial disparities, with sanctions for failing to pay fines and fees having a particularly large disparate impact (using a policy change in North Carolina). Overall, existing evidence related to the effects of community supervision are not promising.

I contribute to the evidence base on the effects of early release and post-release supervision using individual-level data on the Pennsylvania prison and parole populations between 2004 and 2023. I tease out the causal effects of additional time incarcerated and community supervision policies by leveraging two separate quasi-random assignment mechanisms in Pennsylvania that, jointly, determine whether an individual is released early as well as the structure of their community supervision regime upon release.

In Pennsylvania, incarcerated individuals are entitled to a parole hearing after they have served half of their convicted sentence. At this hearing, a quasi-randomly assigned hearing examiner and parole board member jointly decide whether to release the individual to serve the remainder of their sentence under parole supervision. As hearing examiners and parole board members vary in their propensities to grant parole, similar individuals experience different parole outcomes based on which interviewers are assigned to their case.

Parole supervision itself is comprised of two main components: special conditions – which an individual 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 supervision intensity – which dictates how often an individual must meet with an assigned parole officer. In Pennsylvania these components are separately quasi-randomly assigned. Special conditions are assigned by the same parole interviewers who determine parole,

conditional upon parole approval. As hearing examiners and parole board members vary in their propensities to assign each condition, similar paroled individuals are assigned different conditions based on which interviewers are assigned to their case. Supervision intensity is determined by an individual's recidivism risk test score (Level of Service Inventory – Revised (LSI-R)) assessed prior to their parole hearing. As supervision intensity levels are determined by discrete cutoffs in the LSI-R test score range, similar individuals with test scores just above and just below each cutoff are assigned different supervision intensities.

Overall, I find that early release leads to an increase in recidivism with little effect on employment. When comparing paroled individuals to individuals released later, the "age-out" effect of release at an older age (Ulmer & Steffensmeier 2014) appears to outweigh any criminogenic effects of additional time incarcerated. These results imply that the potential criminogenic effects of incarceration are likely fully accrued by the time an individual is eligible for parole. In addition, I find suggestive evidence that early release continues to lead to increased recidivism in the medium-to-long term, after the completion of one's convicted sentence. This result may be due to positive effects of additional prison programing for individuals denied parole, or more time in the community without adequate reentry support for individuals granted parole.

Regarding parole supervision itself, I find that additional special conditions assigned by parole hearing interviewers (such as curfew) appear to have little effect on recidivism or employment. However, each leads to a slight increase in the probability of committing parole violation related to breaking parole conditions and drug test failures, violations which lead to the assignment of additional, new conditions or placement in non-secure community facilities. Overall, additional special conditions do not improve reentry success for individuals at the margin of receiving them.

Finally, I find little effect of moving from low supervision (once-every-three-month meetings with a parole officer) to medium supervision (once-every month meetings with a parole officer). However, moving from medium to high supervision (twice-a-month meetings with a parole officer) increases recidivism by 10-20%, driven by a combination of new arrests and technical parole violations. For individuals at the medium-to-high recidivism risk margin, a medium level of supervision appears to strike a better balance between the deterrence and burden effects of community supervision. Taken together, these results imply that more intensive

supervision – such as additional special conditions and more frequent meetings with one's parole officer – lead to worse reentry outcomes for individuals on the margin of receiving it.

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 the two major components of parole supervision. Section 6 concludes.

## 2. Background – The Parole System in Pennsylvania

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).

### *Parole Hearing*

In Pennsylvania, incarcerated individuals are entitled to a parole hearing after they have served their minimum sentence, which is (usually) half of their full convicted sentence. Figure 1 present the parole hearing process. First, Pennsylvania Department of Corrections (DOC) staff compile a wide set of information about an individual's incarcerated behavior, prior criminal history, likely recidivism risk, and reentry needs upon release. This information includes whether the individual has participated in and successfully completed required DOC programs, has received infractions or been documented for assaultive behavior or drug use while incarcerated, has been recommended for parole by the prison superintendent, and their predicted violence, recidivism, and sex offender risk levels.

An individual's recidivism risk level is derived from the results of the Level of Service Inventory – Revised (LSI-R) recidivism risk test, which scores an individual's recidivism risk on a scale of 0-54 (Andrews & Bonita, 1995). Individuals with a score below a lower threshold are considered "low" risk and will be assigned a low supervision intensity if released to parole, individuals with a score above a higher threshold are considered "high" risk and will be assigned a high supervision intensity if released to parole, and individuals with a score between these thresholds are considered "medium" risk and will be assigned a medium supervision intensity if

paroled.[3] This risk score, along with the wide variety of other information described above, is compiled into a parole decisional instrument information packet which is provided to the interviewers assigned to the individual's parole hearing.

Separately, parole interviewers – comprised of hearing examiners and parole board members – are assigned to interview dates at each prison. The majority of parole hearings are conducted jointly by one parole board member and one hearing examiner. Pennsylvania has nine parole board members at any given time, each selected by the governor and approved by the state legislature to serve fixed six-year terms. One-to-three new parole board member terms begin each year and, from 2004-2020, board members were seldomly reappointed to more than one full term. The state has around 20 hearing examiners at any given time, who serve full-time, non-fixed-length terms and are hired by the parole board.

Each board member and hearing examiner is quasi-randomly assigned to a particular facility on each interview date. Explicitly, the scheduling process each month is broken down into two steps. First, each board member and hearing examiner provides a parole board scheduler the list of days they are available to conduct hearings next 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) interviewer variation across facilities from day to day, and (e) hearing examiner / board member pairing variation from day to day.[4] Given that there are only nine parole board members, certain facility-days are not assigned a board member. On these facility-days board members are separately assigned, at the individual hearing level, to remotely review.

Next, each eligible incarcerated individual is assigned a hearing date at their facility of residence, via a two-step process. First, the parole board scheduler provides a list of selected facility-days for interviews to take place that month to the facility scheduler at each facility, without providing the names of the interviewers assigned to each day. Second, facility schedulers schedule eligible individuals to available dates on a first-come-first-served basis based on their

[3] 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.

[4] Interviewer variation across facilities (criteria (d)) is regional for hearing examiners and state-wide for board members and is included to make sure each interviewer gets experience at multiple facilities. Hearing examiner / board member pairing variation (criteria (e)) is included to make sure each hearing examiner gets to work with each board member throughout the year.

parole eligibility date – their "minimum sentence date" for initial parole hearings or "review date" for individuals previously denied parole. If certain days that month are flagged as not assigned a board member than individuals are often grouped together by violence risk level – such that individuals with high violence risk levels are scheduled on days when board members will be present as these individuals are more likely to require additional scrutiny prior to parole. No additional information about each incarcerated individual, other than parole eligibility date and violence risk, is available to facility schedulers during this process.

On each facility-day the assigned hearing examiner and board member jointly conduct all scheduled hearings. The process for each individual hearing is shown in Figure 2. The hearing examiner and board member each individually review the provided parole decisional instrument information packet, jointly interview the individual, and independently vote on whether to grant parole. If an interviewer votes to parole they can choose to impose special parole conditions on the individual upon release, such as curfew or a required residency in a community corrections center. If an interviewer votes to deny they must recommend a date for the individual's next parole hearing and may provide recommended areas for improvement for the individual to correct prior to their next hearing.

If both interviewers vote to parole the individual is paroled, and if both vote to deny the individual is denied parole. If the two interviewers disagree a second parole board member is assigned, from among the eight remaining parole board members, to remotely review the case and cast a deciding vote. The tiebreaking vote assignment process follows a 1-9 ordered list, with the next voter on the list being assigned to each subsequent required tiebreaking vote.

If paroled, an individual is released under all special parole conditions recommended by either of the two interviewers who voted to parole. If denied parole, an individual is scheduled for a review hearing at the earlier of the two dates recommend by the two interviewers who voted to deny, and provided all recommended areas for improvement suggested by either of the two interviewers who recommended to deny. In addition, these recommended areas for improvement are included in the parole decisional instrument information packet provided at the individual's next hearing. Review hearings and violator hearings (i.e., hearings for individuals previously paroled but reincarcerated for a parole violation) are scheduled on the same days, using the same assignment process, as minimum sentence hearings – new interviewers are quasi-randomly assigned to these hearings.

While most parole hearings require two concurring votes, there are two sets of exceptions. First, 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. Second, incarcerated individuals convicted of certain high-level violent offenses (e.g., homicide and sex crimes) require a majority of the parole board to approve parole. Hearings for these individuals are still conducted by one hearing examiner and one board member. If both interviewers vote to deny the individual is denied parole and the case is not sent to the other eight board members. If at least one interviewer votes to parole, the case is sent to each of the other board members, one at a time following a 1-9 ordered list, until a majority of the board have voted to parole or deny. These "single vote" and "majority vote" hearings are scheduled on the same days, using the same assignment process, as regular "two vote" hearings.

***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 two components. First, the individual must abide by all assigned special parole conditions – such as curfew, drug testing, and restrictions on social contacts – while on parole. Second, the individual must meet with their parole officer a certain number of times each month, based on their designated supervision intensity level.

Special parole conditions are initially assigned by parole interviewers, as described above. They include conditions applied to nearly all parolees – such as drug testing, work requirements, and supervision fees – and conditions applied with a great degree of variation across parolees – such as 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 or remove special conditions in response to good behavior.

Parole supervision intensity level is determined by an individual's LSI-R score, as described above.[5] Supervision intensity determines both the regularity with which an individual must meet with their parole officer and the regularity with which the officer will check-in with

[5] 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.

close members of the individual's community (i.e., "collateral contacts", such as family members, roommates, or an employer). Specifically, upon release, individuals are assigned to one of three levels of supervision: minimum – one in-person meeting every three months, one collateral contact every three months; medium – one in-person meeting every month, one collateral contact every three months; or maximum – one in-person meeting every two weeks, one collateral contact every month.[6]

## 3. Data

This work uses data on the Pennsylvania prison and parole populations between 2004 and 2023. Data on convictions, incarcerated stays, parole hearings, parole violations, and parole-officer documented employment is provided by the Pennsylvania Department of Corrections (DOC). Data on arrests is provided by the Pennsylvania State Police (PSP), and data on tax-return documented employment is provided by the Pennsylvania Department of Labor & Industry (L&I).[7]

Table 1 provides information on incarcerated individuals in Pennsylvania at the time of their initial (minimum sentence) parole hearing. Between January 1st, 2004, and December 31st, 2019, 153,749 unique convicted stays took place with initial parole hearings. In Pennsylvania, the majority of incarcerated individuals are male, and the population is fairly evenly split between non-Hispanic white and Black individuals. Educational attainment is quite low – 40% of individuals have not completed a high school degree and only 2% have received a 2 or 4-year postsecondary degree. Convicted crime type is evenly split between violent crimes, drug crimes, and property crimes, and the average convicted sentence length is 4 years.

Table 2 provides information on each factor included in an individual's parole decisional instrument information packet as well as their interview type. The decisional packet includes both (1) a recommendation from the superintendent (i.e., warden) of the faculty and (2) a cumulative recommendation score that aggregates several factors about an individual related to their incarcerated behavior, predicted violence risk, and predicted recidivism risk. Individuals who are

[6] For all levels of supervision, every other in-person contact must be at the individual's approved residence. If the individual 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 in-person, over the phone, or over email. After one year on parole, based on good behavior individuals are eligible for a substantially lower level of supervision (e.g., administrative supervision, special circumstance supervision, and monitored supervision).

[7] Each data set is available between 3/06 and 12/19. Specific data set availability ranges are as follows: convictions and incarcerated stays (1/04-6/23); parole hearings (1/04-12/19); parole violations (3/06-6/23), parole-officer documented employment (3/06-3/21); arrests (1/04-5/22); and tax-return documented employment (1/05-12/19).

recommended for parole by the superintendent and decisional instrument are likely to be paroled (with success rates of 70% and 65%, respectively), while individuals who are not recommended for parole are substantially less likely to be paroled (with success rates of 12% and 15%, respectively). Turning to factors that comprise the decisional instrument, individuals with high violence risk, high recidivism risk, unwillingness to participate in DOC programming, and behavioral offenses while incarcerated are less likely to be paroled (with average success rates of 43%, 47%, 5%, and 11%, respectively). Overall, 56% of incarcerated individuals are granted parole at their minimum sentence hearing.

Table 3A provides information on one-year post-release outcomes. On average, individuals released to parole and individuals released at the end of their convicted sentence return to state custody at a similar rate – 33% – the former via a combination of arrests and technical parole violations (i.e., violations of parole conditions that trigger re-incarceration) and the later via arrests. In addition, 39% of paroled individuals receive at least one minor parole violation – i.e., violations of parole conditions that trigger a written warning, new conditions, or placement in a non-secure community facility – within one year of release. Next, state wage record data depicts low employment rates for paroled individuals (only 37% are employed during the second quarter after release and only 58% are employed at any time during the first year after release) and even lower rates for individuals released at the end of their convicted sentence (22% and 36%, respectively).

For paroled individuals, employment rates are similar, on average, when measured using state wage record data or parole-officer-documented employment data. However, these average rates mask notable differences in employment trends between the datasets, as shown in Table 3B. Theoretically, each employment dataset is potentially biased, with the biases pointing in opposite directions. For state wage records, individuals have incentives to underreport employment to reduce tax obligations. For parole-officer documented employment, individuals have incentives to overreport employment to comply with the parole condition (assigned to nearly all individuals) to be actively working (or searching for work) at all times. These opposing biases appear in the data via opposite employment trends during the first year after release – state wage record documented employment rates decrease each quarter (from 42% to 34%) while parole-officer-documented employment rates increase each quarter (from 27% to 40%). These trends fit a hypothesis that, over time, individuals are both better able to (1) receive their pay under the table

(by transitioning from formal employment arranged before release to informal employment) and (2) convince their parole officers that they are employed regardless of actual employment.

Table 3B also provides state wage data on industry of employment and wages during the first year after release. Upon release from prison, among employed individuals, 29% are employed in temporary help positions, 17% in manufacturing, 16% in food services, 8% in construction, and 8% in retail trade. Over the first year after release the percent employed in temporary help positions decreases (to 18%) while the percent employed in manufacturing and construction slightly increase (to 19% and 9%, respectively). Finally, total wages are exceptionally low for recently paroled individuals – among employed individuals, average wages are only $3,000 per quarter.

## 4. The Effects of Early Release

### *Research Design*

Let $Y_{it}$ represent a post-release outcome such as arrest, 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, sentence length, facility, LSI-R score, parole decisional instrument score, and parole hearing year.[8] Finally, let $\varepsilon_{it}$ be a stochastic error term. The relationship between early release and post-release outcome *Y* is defined by $\beta_1$ in the equation

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

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 low-risk individuals are more likely to be paroled than high-risk individuals. To avoid selection bias, I estimate a Two-Stage-Least-Squares (2SLS) regression using the

[8] $X_{it}$ is comprised of the following covariates: male, black, Hispanic, divorced, married, high school graduate, age (at time of interview), violent crime conviction, drug crime conviction, public order crime conviction, minimum sentence length, superintendent recommends parole, superintendent conditionally recommends parole, decisional instrument recommends parole, decisional instrument score, medium violence risk, high violence risk, medium recidivism risk, high recidivism risk, LSI-R score, participation in required DOC programming, waiting list for required DOC programming, unwilling to participate in required DOC programming, DOC behavioral offenses, single vote interview, majority vote interview, facility, interview year.

leniency of the interviewers quasi-randomly assigned to an individual's parole hearing as an instrument for whether the individual is paroled ($Paroled_{it}^{*}$).[9]

To construct the instrument, I first create residual measures of observed leniency for every parole hearing between 2004 and 2019, that net out fully interacted facility, year, and violent / non-violent offender fixed effects ($W_{it}$).[10] 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.[11] 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.[12]

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

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

[9] For other examples of "randomized decision-maker" designs see, for example, Bhuller et al. (2020), Dobbie et al. (2018), Bhuller et al. (2018), Mueller-Smith (2015), Loeffler (2013) (judges in criminal court settings), Agan et al. (2021) (prosecutors in a criminal court setting), Weisburst (2018) (police officers to calls for service), and Gross & Baron (2021) (child welfare investigators to child maltreatment investigations).

[10] The primary specification uses review and violator hearings, in addition to minimum sentence hearings, to create the residual measures of observed parole leniency. Results are appreciably similar when only minimum sentence hearings are used to create the instruments, as shown in Figure 4.

[11] The primary specification drops (1) votes that appear to be assigned at the interview level as opposed to the day-institution level, (2) voter-years with less than five assigned interview days or less than 100 total votes, and (3) voter-year-institutions where only one voter of that type was ever assigned. Results are appreciably similar for alternative specifications, as shown in Figure 4.

[12] Alternative instrument construction techniques, such as Cluster Jackknife Instrumental Variables Estimation (CJIVE) (Frandsen & Leslie, 2024) produce appreciably similar results, as shown in Figure 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.[13]

***Instrument Relevance & Validity***

This procedure produces an unbiased estimate of the effects of parole under exogeneity, relevance, and monotonicity assumptions (Dobbie et al., 2018; Bhuller et al., 2020). 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 assignment mechanism of interviewers to parole hearings (as discussed in Section 2), Table 4, Panel B, presents results from a formal joint f-test for independence between the instruments ($V_{jt(-i)}$) and other observable characteristics about the individual ($X_{it}$). Both sets of instruments appear to be independent, with p-values of 0.38 and 0.74.

Instrument relevance requires the variation in parole leniency across board members and hearing examiners to tangibly impact parole decisions. Figure 3 provides graphical evidence of this variation. The figures present histograms of the leave-one-out leniency measure for hearing examiners and board member initial votes in the sample, along with local linear regressions of parole outcomes on leave-one-out interviewer leniency. For both types of interviewers, the likelihood of parole appears to monotonically increase as leave-one-out interviewer leniency increases. Specifically, moving from the least to most lenient hearing examiner and initial board member increases the chance of parole by 10 and 6 percentage points, respectively.[14] To further assess the relevance of the leniency measures, I conduct f-tests for weak instruments. As shown in Table 4, Panel A, first stage f-values for the relationship between the leniency measures and parole, controlling for all other observable characteristics about incarcerated individuals ($X_{it}$), are 339 and 234 respectively.[15]

[13] The primary analysis uses both a hearing examiner and board member instrument, constructed separately, in the first stage equation. Alternative specifications using just a hearing examiner instrument or just a board member instrument would also be appropriate and produce appreciable similar results, as shown in Figure 4.

[14] The drop in parole rates for stringent board members, at the left side of the local linear regression, is driven by a single board member who, over a five year period, paroled a substantially lower number of individuals than their peers.

[15] As shown in Appendix Figure A1 and Table A2, board member tiebreaker votes pass relevance, exogeneity, and monotonicity tests. However, only 7,221 tiebreaker votes are observed in the data over the study period. Board member majority votes appear to have little relevance - the order in which board members vote on majority vote cases

In order to interpret the 2SLS estimates as the local average treatment effects of early release for individuals at the margin of release, interviewers must have monotonic preferences regarding parole leniency. That is, a "lenient" interviewer must be lenient for all types of individuals at the margin of release and a "strict" interviewer must be strict for all types of individuals at the margin of release. To test this monotonicity assumption I test for both within- and across-subsample monotonicity. Table 4, Panel C, shows that first stage estimates of the effect of interviewer leniency – constructed using the full sample – on parole, controlling for other observables, appear positive and statistically significant when restricting the regression sample to various subsets of interviewees (i.e., separated by convicted crime type, race, and interview type). Table 4, Panel D, shows that first stage estimates of the effect of interviewer leniency – constructed using a sample that omits a particular subset of interviewees – on parole, controlling for other observables, are still positive and statistically significant when restricting the regression sample to the omitted subset.

***Results***

Table 5 presents results for the effects of early release on post-release outcomes. As individuals who are denied paroled remain incarcerated and thus, mechanically, have short-term recidivism and employment rates of 0%, I present two different sets of specifications to convey the effects of early release. The first, presented in Panel A, compares the outcomes of initially paroled and initially denied, eventually paroled, individuals during the first year after each individual's release. These specifications hold one's reentry window constant, comparing individuals who are released early at a younger age to individuals who are released later at an older age. The second specification, presented in Panel B, compares the outcomes of initially paroled and initially denied individuals over the same medium- and long-term outcome periods starting on the date of one's parole hearing. These specifications hold one's age and post-hearing outcome-period constant, comparing individuals who are released early and spend more time in the community to individuals who are released late and spend less time in the community (and more time incarcerated). Column 1 presents the primary specifications while Columns 2 and 3 present illustrative alternative specifications.

does not appear to have a notable impact on the outcome of these cases in which a majority of the parole board must approve parole.

For illustrative purposes, Column A2 presents "one year after release" results for OLS regressions that do not control for selection or covariates. Among individuals eventually released to parole, individuals initially paroled are released on average 14 months sooner than individuals who are initially denied. Upon release, individuals who are initially paroled are on average 20% less likely to recidivate and 12% more likely to be employed during the first year after release. This effect, however, is driven by selection – initially paroled individuals are released specifically because they are lower risk.

Column A3 presents results from a similar OLS regression that controls for covariates. After controlling for observables, individuals who are initially paroled still experience better outcomes then those who are initially denied, but the magnitude of the difference is smaller – individuals initially paroled are 8% less likely to recidivate and 5% more likely to be employed during their first year after release. This effect, however, is still driven by selection on unobservables – initially paroled individuals are released specifically because they are lower risk on characteristics observable to hearing interviewers but unobservable in the data.

Column A1 presents the first primary specification (which controls for selection using assigned interviewer leniency instruments). Individuals at the margin of parole who are denied release (due to assigned interviewer stringency) but eventually paroled are, on average, incarcerated for an additional 11 months. The estimates imply that early release leads to a 19% increase in the chance of recidivism (largely driven by a 36% increase in arrests) in the first year after release, and suggestive evidence of a decrease in employment, relative to initial parole denial. These results compare individuals granted parole and released earlier, at a younger age, to individuals denied parole and released later, at an older age. As such, better outcomes for initially denied individuals could be caused by one of two mechanisms – (1) later release or (2) older age at release. Theoretically, individuals initially denied parole may recidivate more due to the potentially criminogenic effect of additional time incarcerated and the potential stigma and discouragement of parole denial (West-Smith et al. 2000). Alternatively, individuals denied parole may recidivate less due to the age-out effects of release at an older age (Ulmer & Steffensmeier 2014). Overall, these results imply that the age-out effects of later release dominate any criminogenic or stigma effects of additional time incarcerated. Further, they imply that additional time incarcerated itself may be beneficial for post-release outcomes.

Columns B2 and B3 present two additional illustrative specifications. These specifications compare outcomes over a two and five-year period starting from each individual's parole hearing (controlling for selection using assigned interviewer leniency instruments). Individuals granted parole recidivate at an 88% higher rate in the two-year period following the initial parole hearing than individuals denied parole. However, this result is largely mechanical – individuals denied parole remain incarcerated for a substantial portion of this outcome period, during which they are unable to recidivate. Over a five-year period, there is little difference in recidivism rates between initially paroled and denied individuals.

Column B1 presents the second primary specification (which controls for selection using assigned interviewer leniency instruments). This specification compares outcomes during a two-year follow-up period that starts five years after one's parole hearing, among the sample of individuals with less than five years left on their convicted sentence at the time of their hearing. At the start of this outcome window all individuals in the subsample will have completed their convicted sentence and be either living in the community or incarcerated for a new crime that occurred between release and the start of the outcome window. Over this period, the results provide suggestive evidence that initially paroled individuals recidivate at higher rates than initially denied individuals. There is little difference over this period in employment rates or the probability of a stay in DOC custody for these two groups.

Overall, these results imply that there is no criminogenic effect of additional time incarcerated, with suggestive evidence of a positive effect of additional time incarcerated, among individuals at the margin of early release. However, note that these results alone do not motivate a recommendation to increase incarceration lengths. While early release does not appear to reduce recidivism or increase employment, there are a wide variety of unobservable reentry outcomes – such as familial stability, housing stability, educational attainment, and health – that may be improved by early release. In addition, early release likely improves the general wellbeing of the released individual (relative to remaining incarcerated) upon release, and reduces incarceration costs for the state. Each of these potential impacts should be considered when setting early release policy. These results just provide evidence that early release does not reduce recidivism or improve employment outcomes, among individuals at the margin. To improve these outcomes, policy makers may be better served by focusing on policies related to diverting individuals from prison in the first place.

Figure 4 provides results for alternative specifications and population subgroups. Results are generally robust across alternative model specifications (Specifications 1-5). Results are also similar across population subgroups (Specifications 6-9), with suggestive evidence that the short-term recidivism effects of early release are larger for non-white individuals, and that the long-term recidivism effects of early release are smaller for individuals with drug-crime convictions.

***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 of early release for low risk incarcerated individuals – who would be granted parole by all hearing interviewers – and high risk incarcerated individuals – who would be denied parole by all hearing interviewers. The effects identified by this analysis pertain to individuals who would be released if assigned to lenient parole interviewers but not released if assigned to stringent parole interviewers.

Which incarcerated individuals are at this margin in Pennsylvania? Figure 4 presents a histogram of parole outcome predicted probabilities for each initial parole hearing in Pennsylvania, controlling for all observable information about an individual prior to parole interviewer assignment. For illustrative purposes, the grey bar shows a range of individuals likely to be at the margin of release – individuals with pre-interview predicted values between .42 (whose predicted value becomes .5 if assigned the most lenient interviewers) and .58 (whose predicted value becomes .5 if assigned the most stringent interviewers).[16] Around 13% of the sample falls within the grey bar, with 58% of individuals above it and 29% below it.

In the data, individuals with parole predicted probabilities in this range overwhelmingly fall into one of two subgroups. The first subgroup is individuals convicted of a violent crime who have demonstrated good behavior while incarcerated (i.e., completed all assigned programs and committed no misconducts, assaultive behavior, or drug / alcohol offenses). The second subgroup

[16] Note that a subset of individuals outside the grey bar are also at the margin of release (E.g., the average individual above the grey bar is still denied paroled 24% of the time while the average individual below the gray bar is still granted parole 17% of the time). This is due to factors – which make them riskier or less risky parole candidates – which are observable to interviewers but unobservable in the data. For example, while individuals above the bar are less risky on paper (they committed non-violent crimes and have had good in-custody conduct) a subset of them may actually be more risky in other ways (e.g., displaying worrisome signs during their parole interview) such that they are actually at the margin of parole.

is the opposite set of individuals – those convicted of a non-violent crime who have demonstrated poor behavior while incarcerated. Together, these two subgroups – individuals who are low-risk on one of the two primary dimensions considered at parole interviews and high-risk on the other – comprise a significant percentage of the incarcerated population at the margin of release in Pennsylvania, for whom this analysis has identified the average effect of early release.

## 5. The Effects of Parole

### *Research Design – Special Conditions*

I next investigate the effects of parole supervision itself on post-release success, by separately estimating the effect of each major component of parole supervision. I first estimate 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 paroled individual – such as not consuming alcohol, mandatory drug testing, maintaining employment or an active job search, and paying supervision fees.[17] These also include conditions assigned with a wide degree of variation – 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 completion of treatment programs. The average paroled individual is assigned 10.8 special conditions upon release.

I estimate the effects of both (1) the total number of special conditions assigned and (2) each individual condition type. To do so, I estimate a 2SLS regression using the condition assignment leniency of the interviewers quasi-randomly assigned to an individual's parole hearing as instruments for whether special conditions are assigned. The main specification to evaluate the effects of board-imposed special parole conditions follows Equations 1-4 above, with four differences. First, the explanatory variable of interest is either "total number of conditions" or "a single condition type." Second, I restrict the sample to parole hearings that result in parole, as individuals who are denied parole remain incarcerated and are assigned no conditions. Third, in Equation 2, I now include "interviewer parole leniency" ($V_{jt(-i)}$) along with fully interacted facility, year, and violent / non-violent offender fixed effects ($W_{it}$):

$$Paroled_{it} = \gamma_0 + \gamma_1 W_{it} + \gamma_2 V_{jt(-i)} + e_{it} \quad . \tag{2B}$$

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

This addition directly controls for interviewer parole leniency when creating residual measures of interviewer condition leniency. With its inclusion, the analysis now compares individuals in the same facility / year / violence group, assigned interviewers with similar parole leniencies, who happen to be assigned different conditions upon release due to differences in interviewer condition leniencies.

The final difference is that the covariate set $X_{it}$ in equations (1) and (4) now includes interviewer parole leniency ($V_{jt(-i)}$) and, for "specific condition type" regressions, the interviewer condition leniency measures for all other condition types $\left(\sum_{c=1}^{n_{(-c)}}\left(V_{jt(-i)}^{c}\right)\right)$. As hearing interviewers can choose to assign many different combinations of conditions, this inclusion controls for any correlation between interviewer condition leniency across conditions, allowing the estimation of causal, unbiased estimates of the effects of each specific condition (Muller-Smith, 2015).

As before, the 2SLS procedure produces an unbiased estimate of the effects of the "total number of conditions" and each "individual condition type" as long as the instruments are relevant, exogenous, and exhibit monotonicity. Figure 6 provides graphical evidence of the relevance of the "total number of conditions" leniency instrument measures. For both types of interviewers, the likelihood of being assigned additional conditions appears to monotonically increase as leave-one-out interviewer leniency increases. Specifically, lenient hearing examiners assign 2.2 less special conditions than stringent hearing examiners, and lenient board members assign 0.6 more special condition than stringent board members. Hearing examiner and board member "total number of conditions" instruments also pass several other relevance, exogeneity, and monotonicity tests, as shown in Appendix Table A2.

Relevance, exogeneity, and monotonicity test results for each individual special parole condition are presented in Appendix Figure A2 and Table A3. Each special condition included in the main results table (Table 8) passes relevance and monotonicity tests, though there are some signs that, among paroled individuals, special condition hearing examiner leniency for certain conditions is correlated with other observable characteristics about the individual.

***Results – Special Conditions***

Table 7 presents estimates for the effect of the total number of parole conditions on post-release success. For illustrative purposes, Column A3 presents results for OLS regressions that do

not control for selection. Each additional assigned condition is associated with a 6% higher rate of recidivism, a 10% higher rate of major parole violations that lead to reincarceration, and a 4% higher rate of minor parole violations – that leads to a written warning, new conditions, or placement in a non-secure community facility. As before, this effect is driven by selection on unobservables – individuals assigned more conditions are assigned them specifically because they are higher risk on characteristics observable to hearing interviewers but unobservable in the data.

Column A1 presents the primary specification (which controls for selection using assigned interviewer leniency instruments). Individuals who receive additional conditions (due to assigned interviewer stringency) have similar rates of recidivism and employment to individuals who do not. However, each additional condition increases the chance an individual receives a minor parole violation by 1%. Table 7, Panel B further unpacks this result. The increase in parole violations is driven by an increase in broken condition and drug test failure violations, which incur sanctions that impose new conditions or require stays in non-secure facilities.

Table 8 presents estimates for the effect of each individual condition (controlling for selection using assigned interviewer condition leniency instruments, while controlling for interviewer propensity to assign each other condition) on one year post-release outcomes. Columns 1, 2, and 5 show that individual conditions appear to have little effect on recidivism (for either arrests or technical parole violations) or employment. However, Columns 3 and 4 provide suggestive evidence that many conditions increase lesser parole violations, driven by an increase in broken condition violations. Specifically, treatment-related conditions appear to increase the likelihood of receiving a parole violation for breaking conditions, as does a condition restricting interactions with prior co-defendants or individuals flagged as belonging to gangs. Conversely, a condition restricting interaction with prior victims of one's convicted crimes appears to reduce lesser parole violations. Finally, while the effects of community corrections center residency and inpatient treatment residency are less well-identified, the estimates provide suggestive evidence that each residency type reduces recidivism in Pennsylvania.

Overall, individuals at the margin or receiving additional special conditions do not appear to benefit from them. The main effect of additional conditions is a slight increase in parole violations for broken conditions, which in turn leads to the assignment of even more conditions. Note, however, that these LATE estimates only apply to individuals at the margin of receiving each condition. For individuals away from the margin of receiving a specific condition – for whom

the condition would be applied regardless of assigned interviewer – the effect of the condition could be positive (or negative) as it's effect is unidentified in the data.

***Research Design - Supervision Level***

Second, I estimate the effect of parole supervision intensity level, using regression discontinuity designs around two separate recidivism risk test score cutoffs – the LSI-R score cutoff between minimum & medium supervision and the LSI-R score cutoff between medium & maximum supervision. 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 an 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 $\beta_1$ in the equation

$$Y_{it} = \beta_0 + \beta_1 MedSupScore_{it} + \beta_2 X_{it} + \tilde{\varepsilon}_{it} \quad (5)$$

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 $\beta_1$ in the equation

$$Y_{it} = \beta_0 + \beta_1 MaxSupScore_{it} + \beta_2 X_{it} + \tilde{\varepsilon}_{it} \quad (6)$$

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 7 plots the LSI-R scores for incarcerated individuals in Pennsylvania. The lower and upper cutoff values changed several times during this period, and ranged between 17-21 and 27-30, respectively, over this time.[18] As shown in Figure 7A, there appears to be no sign of bunching at either cutoff prior to an individual's parole hearing. Formal density checks around

[18] 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 cutoffs, following McCrary (2008), find no evidence of manipulation (p = 0.13 at the lower cutoff; p = 0.99 at the upper cutoff).

Note that LSI-R scores are included in the parole decisional instrument packets provided to interviewers at each parole hearing. As such, these scores may impact interviewers' decisions to parole and assign special conditions. As shown in Figure 8, if a slight change in LSI-R score near either cutoff impacts an interviewer's parole decision, then more, higher risk individuals just above each cutoff with be denied parole and omitted from the paroled sample, relative to individuals just below each cutoff. This potential differential attrition would lead to better average outcomes for paroled individuals just above each cutoff, biasing estimates towards better outcomes for increased supervision intensity.

Figure 7B graphically investigates this concern by plotting the LSI-R scores for the subset of individuals who are granted parole at their parole hearings. There is no sign of differential parole rates for individuals just above and below the lower cutoff, and only a modest sign of differential parole rates for individuals just above and below the higher cutoff. Specifically, this difference only appears between 2009 and 2014 – when the cutoff between medium and high recidivism risk was an LSI-R score of 26/27. In addition, formal density checks around each cutoff (following McCrary, 2008) – when restricting the sample to paroled individuals – find no evidence of manipulation (p = 0.28 at the lower cutoff; p = 0.34 at the upper cutoff).

Due to this concern about differential attrition (both theoretically and graphically) I estimate a secondary specification that restricts the sample to a subset of individuals away from the parole margin of release. Specifically, this secondary specification restricts the sample to individuals with expected probabilities of parole, prior to taking the LSI-R, greater than 80%. As these individuals are very likely to be paroled regardless of whether their LSI-R score falls above or below each cutoff, results for this sample will be substantially more insulated from this differential attrition concern.

***Results – Supervision Level***

Estimates for the effects of parole supervision intensity level are presented in Table 9. Column 1 presents the main results and Column 2 presents results when restricting the sample to individuals away from the margin of release. Overall, the estimates in Panel A imply that there is little effect from moving from a low to medium level of supervision intensity. However, the

estimates in Panel B, Column 1, imply that moving from a medium to high level of supervision intensity increases recidivism by 5-7%, driven by a combination of new arrests and parole violations.[19] Results are even stronger in Panel B, Column 2, when restricting the sample to individuals away from the margin or release – recidivism increases by 35% for individuals assigned to a high relative to a medium level or supervision. Note that these results point in the opposite direction of the potential differential attrition bias discussed above, and as such the underlying magnitude of the effect could be even larger than these estimates. Along both margins, supervision intensity level appears to have little effect on employment.

When determining supervision intensity, there is an inherent trade-off between deterrence and burden. The higher the level of supervision, the more potential deterrent is provided against future arrests and violations. However, higher levels of supervision also increase the burden of supervision for paroled individuals, and the number of hurdles they have to clear each month to meet parole requirements while navigating personal reentry challenges related to employment, housing, and community re-integration. Overall, these results suggest that twice-a-month meetings, relative to once-a-month meetings, increase the burden of supervision more than any potential benefit of increased deterrence, among individuals at the high recidivism risk margin.

## 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 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 after an individual's initial parole hearing. From a policy perspective, to reduce any criminogenic effects of incarceration these results recommend focusing on diversion programs – to keep individuals out of prison in the first place – instead of focusing on shortened sentence lengths.

[19] The increase in parole violations that lead to reincarceration are made up of additional drug test failure, absconding, and weapon possession violations.

Among incarcerated individuals at the margin of release, this work suggests that early release leads to slightly higher recidivism in both the short- and long-term. While this work provides evidence on the effects of parole on returns to prison and documented employment, it does not evaluate the effects of parole on other outcomes of post-release success such as housing stability, educational attainment, family stability, and personal wellbeing. When making policy decisions about whether and when to parole, the effects of early release on these other outcomes should be considered alongside these results.

Next, this work evaluates the effects of parole itself by separately evaluating the effect of its main components – special conditions and supervision intensity level. First, discretionary special conditions appear to have little corrective impact for individuals at the margin of receiving them, and only increase individuals' propensity to receive minor parole violations for breaking them. Second, increasing from a medium-to-high parole supervision intensity level increases both new arrests and technical parole violation reincarcerations, among individuals at the medium-to-high recidivism risk margin.

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 the medium-to-high recidivism risk margin. Overall, this work recommends that policy makers looking to improve parole outcomes should focus on assigning additional supervisory conditions and intensity only when necessary. In addition, this work concludes that no aspect of current parole supervision is a panacea – and new, alternative approaches may be needed to improve post-incarceration reentry success.

## Tables and Figures

*Figure 1 – Parole Process*

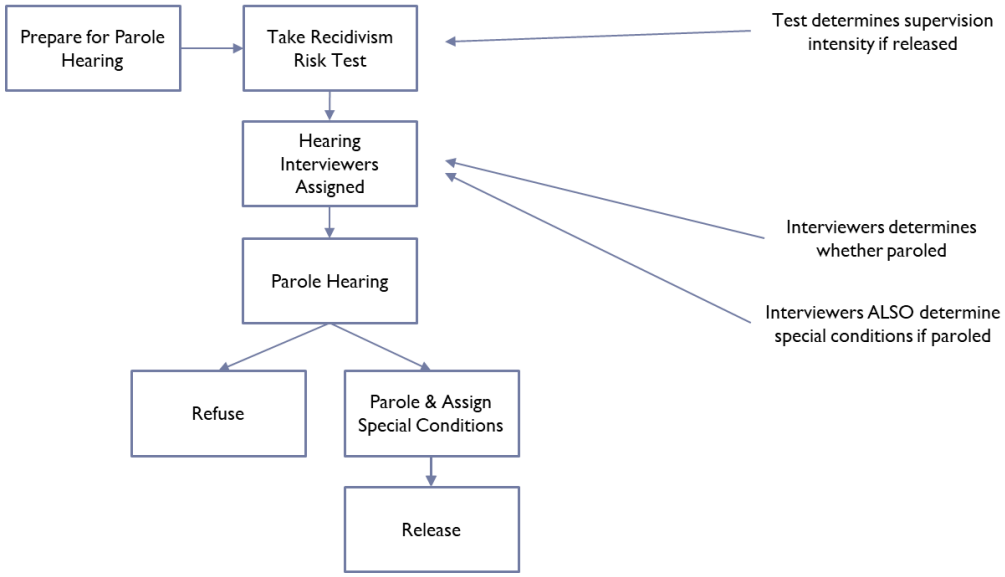



*Figure 2 – Hearing Structure*

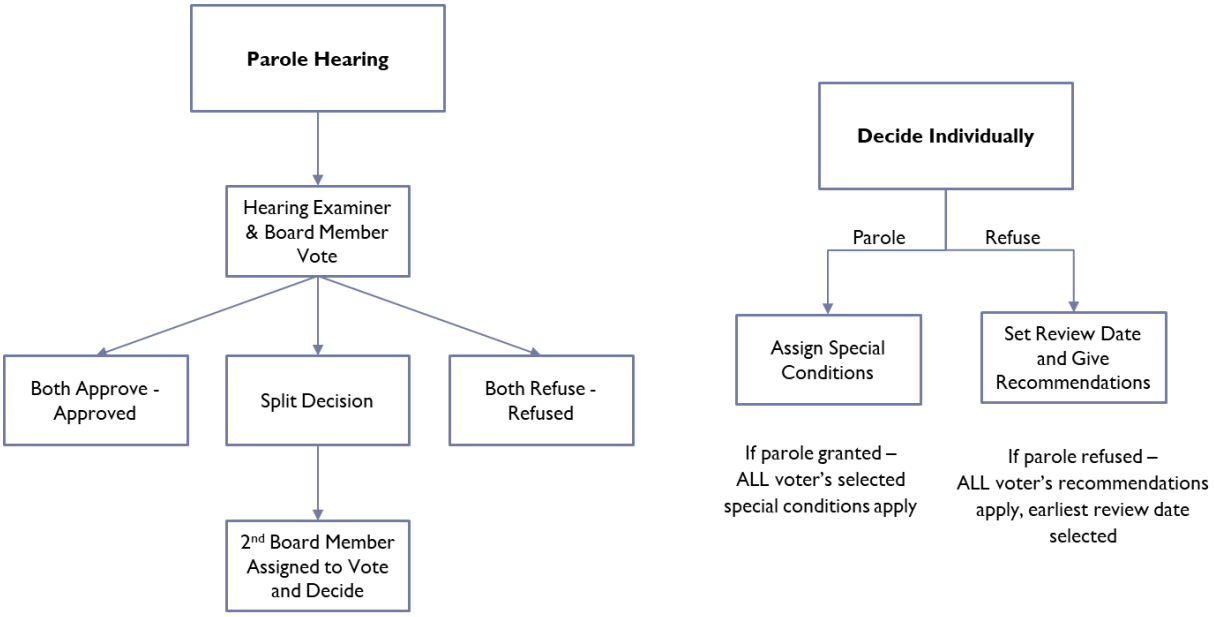


*Table 1 – Incarcerated Population at Time of Minimum Sentence Hearing*

| Demographic Characteristics | Mean |
|---|---|
| Male | 91% |
| Black | 42% |
| Hispanic | 8% |
| Married | 15% |
| Education - Less Than HS Degree | 40% |
| Age (at Start of Convicted Stay) | 33.0 |
| Violent Crime Conviction | 31% |
| Drug Crime Conviction | 29% |
| Minimum Sentence Length (Years) | 1.8 |

Notes:
N = 153,749 convicted stays with minimum sentence hearings.

*Table 2 – Parole Hearing Statistics*

| A. Minimum Sentence Hearings | Mean | %Paroled |
|---|---|---|
| Superintendent Rec. - Parole | 29% | 72% |
| Superintendent Rec. - Conditional | 50% | 68% |
| Superintendent Rec. - Denial | 21% | 12% |
| Decisional Instrument Rec. - Parole | 84% | 65% |
| Decisional Instrument Rec. - Denial | 16% | 15% |
| Violence Risk - Low | 68% | 64% |
| Violence Risk - Medium | 14% | 53% |
| Violence Risk - High | 18% | 43% |
| LSIR Recidivism Risk - Low | 20% | 68% |
| LSIR Recidivism Risk - Medium | 39% | 60% |
| LSIR Recidivism Risk - High | 41% | 47% |
| DOC Programing - Unwilling to Participate | 4% | 5% |
| DOC Behavioral Offenses | 10% | 11% |
| Expedited Interview - 1 vote needed | 16% | 79% |
| Regular Interview - 2 votes needed | 79% | 53% |
| Majority Vote Interview - 5 votes needed | 5% | 29% |
| **B. All Hearings** | | |
| Minimum | 57% | 56% |
| Review | 31% | 47% |
| Violator | 12% | 52% |

Notes:
N = 153,749 minimum sentence hearings, 83,813 review hearings, and 31,969 violator hearings.

*Table 3 – Post-Release Outcomes*

| A. Outcomes - One Year After Release | Paroled Mean | Sentence Served Mean |
|---|---|---|
| Recidivism - Any | 32% | - |
| *Arrest* | 19% | 34% |
| *PV Reincarceration* | 16% | - |
| Minor Parole Violation | 37% | - |
| Employed - Q2 (Parole Data) | 37% | - |
| Employed - Q2 (L&I Data) | 39% | 22% |
| Employed - Any (Parole Data) | 51% | - |
| Employed - Any (L&I Data) | 58% | 36% |

| B. Employment Details by Quarter | Q1 | Q2 | Q3 | Q4 |
|---|---|---|---|---|
| % Employed (Parole) | 27% | 37% | 39% | 40% |
| % Employed (L&I) | 42% | 39% | 37% | 35% |
| NAISC Employed Industry - Among Employed (L&I) | | | | |
| *561320 - Temporary Help Services* | 29% | 23% | 20% | 17% |
| *722 - Food Services* | 16% | 16% | 16% | 16% |
| *31-33 - Manufacturing* | 15% | 17% | 18% | 19% |
| *23 - Construction* | 8% | 8% | 9% | 9% |
| *44-45 - Retail Trade* | 7% | 8% | 8% | 8% |
| Median Wages - Among Employed (L&I) | $2,345 | $3,071 | $3,372 | $3,624 |

Notes:
N = 131,238 releases to parole and 21,166 sentence served releases. Panel B presents employment details separately for the first, second, third, and fourth quarters after release among releases to parole.

*Figure 3 – Variation in Parole Leniency*

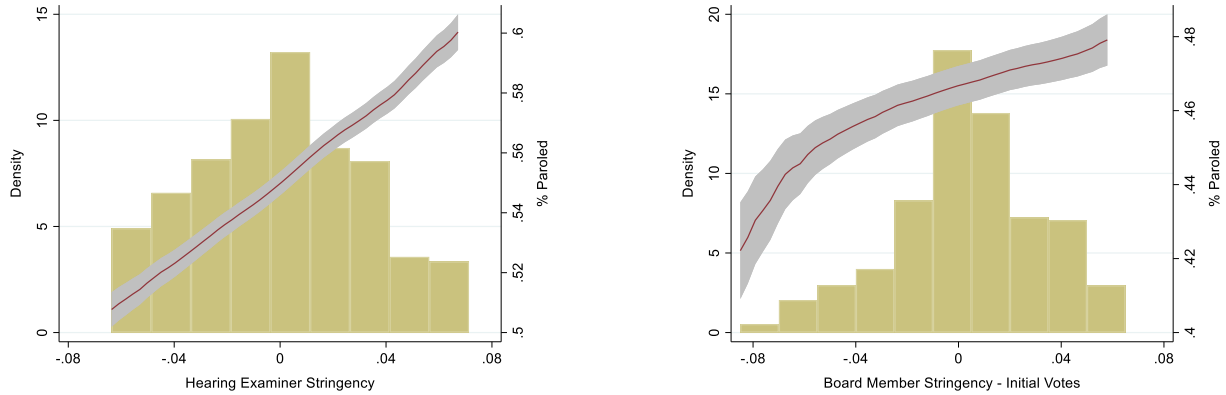


*Table 4 – Instrument Validity*

| | Hearing Examiner Leniency | Board Member Leniency (Initial Votes) |
|---|---|---|
| **A. Relevance** | | |
| First stage f-value | 339 | 234 |
| **B. Exogeneity** | | |
| Joint f-test p-value | 0.38 | 0.74 |
| **C. Within-Subsample Monotonicity** | | |
| Violent Crime | 0.44*** | .90*** |
| Drug Crime | 0.77*** | 0.29^ |
| Property Crime | 0.86*** | 0.92*** |
| White | 0.74*** | 0.86*** |
| Non-White | 0.69*** | 0.81*** |
| Regular Interview (2 votes needed) | 0.66*** | 0.83*** |
| Expedited Interview (1 vote needed) | 0.99*** | na |
| Majority Vote Interview (5 votes needed) | 0.44* | 0.84*** |
| **D. Across-Subsample Monotonicity** | | |
| Violent Crime | 0.42*** | 0.69*** |
| Drug Crime | 0.79*** | 0.25^ |
| Property Crime | 0.87*** | 0.93*** |
| White | 0.65*** | 0.73*** |
| Non-White | 0.61*** | 0.65*** |
| Regular Interview (2 votes needed) | 0.16*** | 0.56*** |
| Expedited Interview (1 vote needed) | 0.66*** | na |
| Majority Vote Interview (5 votes needed) | 0.51** | 0.90*** |

Notes:

N = 75,634 hearing examiner votes and 40,170 initial board member votes.

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.

^Board members rarely cast initial votes for drug crime conviction parole interviews - only 1,664 such instances occur. Associated P-values are .26 for Panel C and .30 for Panel D, respectively.

## *Table 5 – Effects of Early Release*

Table 5 - Effects of Early Release

| | A1. 1 Year Window After Eventual Release to Parole | | | | A2. OLS w/o Covariates | | | A3. OLS w Covariates | | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Outcomes** | Ctrl Mean | % Effect | Raw Est | *SE* | % Effect | Raw Est | *SE* | % Effect | Raw Est | *SE* |
| Recidivism | *37%* | 19% | 0.072 * | 0.04 | -20% | -0.073 *** | 0.004 | -8% | -0.031 *** | 0.004 |
| *Arrest* | *22%* | 36% | 0.080 ** | 0.04 | -20% | -0.043 *** | 0.003 | -11% | -0.023 *** | 0.003 |
| *PV Reincarceration* | *18%* | 8% | 0.015 | 0.03 | -22% | -0.041 *** | 0.003 | -7% | -0.014 *** | 0.003 |
| Minor Parole Violation | *39%* | 10% | 0.039 | 0.05 | -3% | -0.011 *** | 0.004 | -1% | -0.002 | 0.004 |
| Employed - Q2 (Parole Data) | *33%* | -16% | -0.051 | 0.05 | 15% | 0.049 *** | 0.004 | 7% | 0.022 *** | 0.004 |
| Employed - Q2 (L&I Data) | *37%* | -4% | -0.014 | 0.06 | 9% | 0.034 *** | 0.004 | 3% | 0.012 *** | 0.005 |
| Months Until Release | *18.2* | -60% | -10.95 *** | 0.97 | -76% | -13.92 *** | 0.093 | -63% | -11.51 *** | 0.080 |

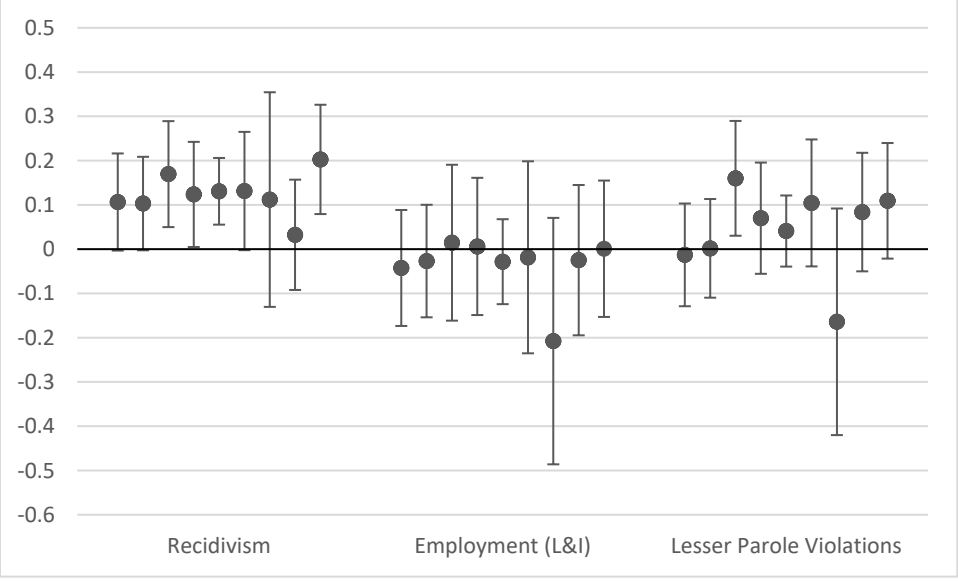
| | B1. 2 Year Window 5 Years After Interview | | | | B2. 2 Years After Interview | | | B3. 5 Years After Interview | | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Outcomes** | Ctrl Mean | % Effect | Raw Est | *SE* | % Effect | Raw Est | *SE* | % Effect | Raw Est | *SE* |
| Recidivism | *35%* | 47% | 0.167 * | 0.09 | 88% | 0.214 *** | 0.04 | -15% | -0.089 | 0.06 |
| *Arrest* | *33%* | 23% | 0.076 | 0.10 | 99% | 0.154 *** | 0.04 | -3% | -0.015 | 0.07 |
| *PV Reincarceration* | *3%* | 121% | 0.039 | 0.03 | 111% | 0.111 *** | 0.03 | 44% | 0.094 ** | 0.05 |
| Minor Parole Violation | *7%* | 65% | 0.044 | 0.04 | 79% | 0.174 *** | 0.04 | 33% | 0.128 ** | 0.05 |
| Employed - Final Qtr (L&I Data) | *17%* | 55% | 0.091 | 0.09 | -27% | -0.049 | 0.05 | 55% | 0.095 | 0.07 |
| DOC Custody | *24%* | 1% | 0.003 | 0.06 | | | | | | |

Notes:

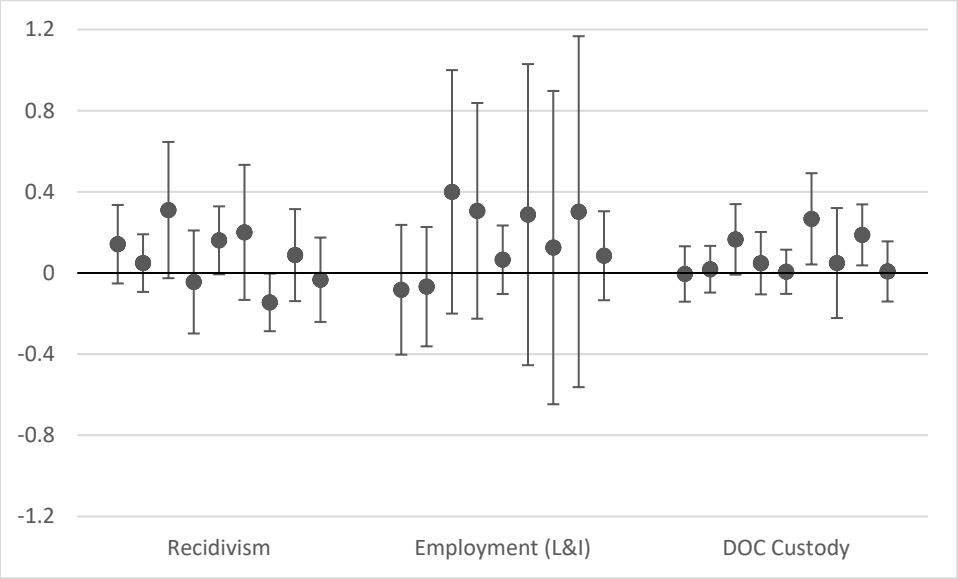
Outcomes are measured as whether the event took place anytime during the noted period. 2SLS regression with interviewer instruments is conducted in panels A1, B1, B2, and B3; simple OLS estimation is conducted in panels A2 and A3. Data in Columns A1, A2, and A3 is restricted to releases and eventually 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 at the time of their parole interview.N = 78,546 for Columns A1, A2, and A3, 54,300 for Column B1, 94,643 for Column B2, and 82,661 for Column B3.

## *Figure 4 – Robustness Checks & Heterogeneity Analysis*

### *A – 1 Year Window After Eventual Release to Parole*

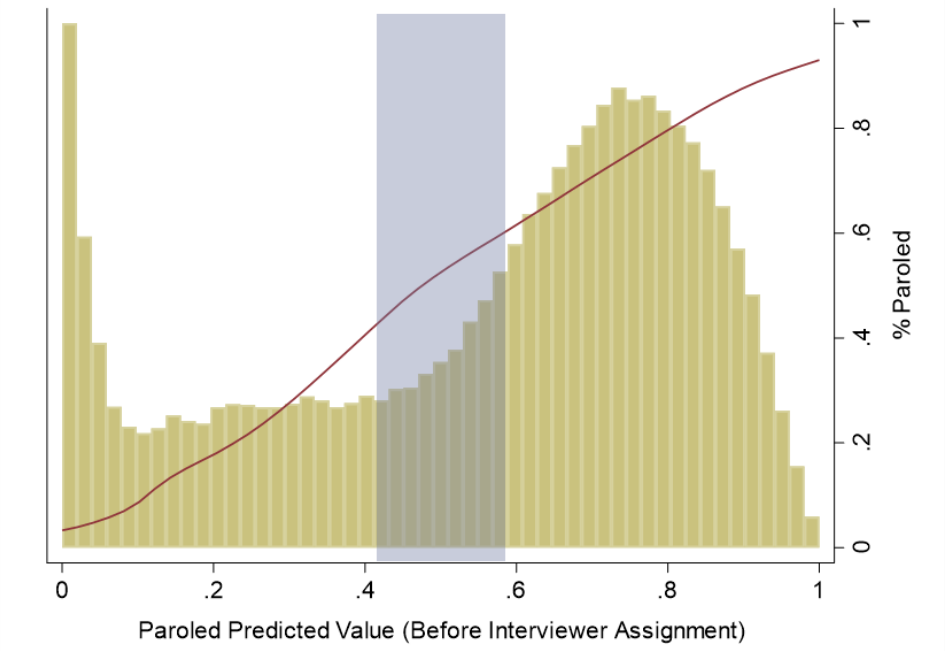


*B – 1 Year Window 5 Years After Interview*




Notes:
Point estimates with 95% confidence intervals are shown.
Robustness checks are as follows: (1) Only hearing examiner instruments (Sample size A: 66,556; Sample size B: 46,745); (2) Only hearing examiner instruments - including interviews assigned individually (75,936; 54,285); (3) Only board member initial vote instruments (28,251; 15,729);(4) Only board member initial vote instruments - including interviews assigned individually (99,060; 74,057); (5) Instruments created using CJIVE procedure (81,824; 56,538); (6) violent crime conviction hearings (Sample size A: 26,533; Sample size B: 14,777); (7) drug crime conviction hearings (18,987; 13,760); (8) non-Hispanic white incarcerated individuals (36,948; 26,613); (9) nonwhite incarcerated individuals (44,082; 29284).

*Figure 5 – Incarcerated Individuals Near the Margin of Release in Pennsylvania*



## *Table 6 – Board-Imposed Parole Conditions*

| | Mean |
|---|---|
| Total Conditions | 10.8 |
| *General Restrictions* | |
| Community Corrections Center Residency | 26% |
| Curfew | 47% |
| Require Permission to Drive | 69% |
| Cannot Enter Alcohol Establishments | 87% |
| Cannot Possess Ammo | 57% |
| Payment Restitution via Wage Attachment | 17% |
| *Contact-Related Conditions* | |
| Cannot Contact Codefendants or Gangs | 29% |
| Cannot Contact Drug Users or Sellers | 72% |
| Cannot Contact Victims | 47% |
| Must Support Dependents | 42% |
| Domestic Violence Protocol | 15% |
| *Treatment-Related Conditions* | |
| Attend Alcohol Support Group | 42% |
| Complete Outpatient Treatment | 50% |
| Treatment Evaluation Required | 29% |
| Community Treatment Program | 12% |
| Take Psychiatric Medicine if Prescribed | 18% |

| | Mean |
|---|---|
| *Non-Discretionary Conditions* | |
| Supervision Fee | 100% |
| Drug Testing Required | 97% |
| Cannot Consume or Possess Alcohol | 94% |
| Must Maintain Employment or Active Job Searc | 94% |
| Enroll in GED Program | 4% |
| Sex Offender Protocol | 1% |
| Electronic Monitoring | 5% |
| Travel Restrictions | 1% |
| Enhanced Supervision | 0% |

Notes:
N = 125,474 releases to parole with interview and condition data available.

## *Figure 6 – Variation in Discretionary Condition Leniency*

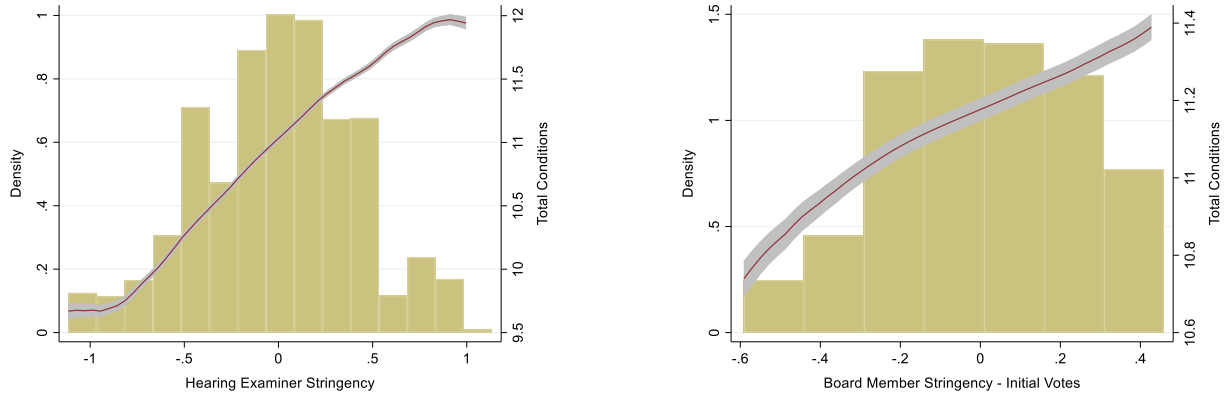


*Table 7 – Effect of Total Number of Discretionary Conditions*

| | All Releases, 2SLS | | | All Releases, OLS | | |
|---|---|---|---|---|---|---|
| **A. Recidivism & Employment** | % Change | Estimate | *SE* | % Change | Estimate | *SE* |
| Recidivism | 0% | 0.001 | *(.003)* | 6% | 0.019 *** | *(.001)* |
| *Arrest* | 0% | -0.001 | *(.002)* | 4% | 0.007 *** | *(.001)* |
| *PV Reincarceration* | 1% | 0.002 | *(.002)* | 10% | 0.015 *** | *(.001)* |
| Minor Parole Violation | 1% | 0.004 * | *(.002)* | 4% | 0.015 *** | *(.001)* |
| Employed - Q2 (Parole Data) | 0% | 0.000 | *(.003)* | -1% | -0.003 *** | *(.001)* |
| Employed - Q2 (L&I Data) | 1% | 0.005 | *(.003)* | 2% | 0.007 *** | *(.001)* |

| | | All Releases, 2SLS | | |
|---|---|---|---|---|
| **B. Violation Details** | Ctrl Mean | % Change | Estimate | *SE* |
| Violation Type | | | | |
| *Broke Condition* | *31%* | 1% | 0.004 * | *(.002)* |
| *Failed Drug Test* | *29%* | 2% | 0.004 * | *(.002)* |
| *Absconded* | *11%* | 2% | 0.002 | *(.002)* |
| *Arrested* | *15%* | -1% | -0.001 | *(.003)* |
| *Assaultive Behavior* | *1%* | -1% | 0.000 | *(.001)* |
| *Weapon Possession* | *1%* | 7% | 0.000 | *(.001)* |
| Sanction Type | | | | |
| *Written Warning* | *22%* | 2% | 0.004 | *(.003)* |
| *New Condition* | *23%* | 2% | 0.005 * | *(.003)* |
| *Non-Secure Facility Stay* | *11%* | 4% | 0.004 * | *(.002)* |
| *Reincarceration* | *26%* | 0% | 0.000 | *(.003)* |

Notes:

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

N = 71,716.

*Table 8 – Effects of Individual Discretionary Conditions*

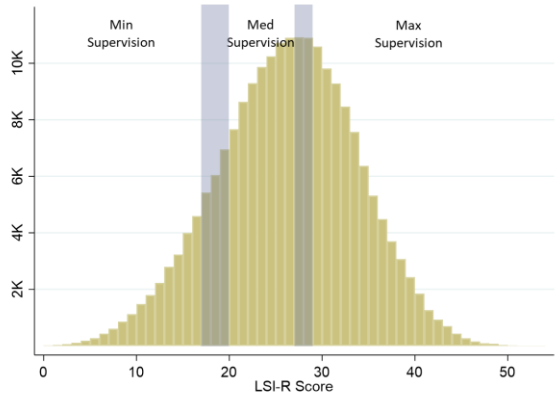
| | Arrested | | | TPV Reincarceration | | | Lesser Parole Violation | | | Violation Type - Broke Condition | | | Employed Qtr 2 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | % Change | Estimate | *SE* | % Change | Estimate | *SE* | % Change | Estimate | *SE* | % Change | Estimate | *SE* | % Change | Estimate | *SE* |
| *General Conditions* | | | | | | | | | | | | | | | |
| Curfew | -2% | 0.00 | *(.011)* | -2% | 0.00 | *(.011)* | -1% | 0.00 | *(.014)* | -5% | -0.01 | *(.017)* | -2% | -0.01 | *(.015)* |
| Cannot Enter Alcohol Establishments | -1% | 0.00 | *(.018)* | 2% | 0.00 | *(.016)* | -3% | -0.01 | *(.023)* | -1% | 0.00 | *(.027)* | 20% | 0.08 *** | *(.024)* |
| Cannot Possess Ammo | 7% | 0.01 | *(.019)* | -2% | 0.00 | *(.018)* | 3% | 0.01 | *(.024)* | 11% | 0.03 | *(.029)* | 11% | 0.04 * | *(.025)* |
| Require Permission to Drive | 15% | 0.03 * | *(.014)* | 9% | 0.01 | *(.014)* | 1% | 0.00 | *(.019)* | -13% | -0.04 * | *(.022)* | 9% | 0.03 * | *(.020)* |
| *Contact-Related Conditions* | | | | | | | | | | | | | | | |
| Cannot Contact Codefendants or Gangs | 25% | 0.05 | *(.037)* | -8% | -0.01 | *(.036)* | 24% | 0.09 * | *(.048)* | 30% | 0.09 * | *(.059)* | -8% | -0.03 | *(.050)* |
| Cannot Contact Drug Users or Sellers | -1% | 0.00 | *(.020)* | -2% | 0.00 | *(.018)* | 6% | 0.02 | *(.024)* | 6% | 0.02 | *(.028)* | 1% | 0.00 | *(.026)* |
| Cannot Contact Victims | -6% | -0.01 | *(.039)* | 13% | 0.02 | *(.036)* | -25% | -0.10 ** | *(.049)* | -42% | -0.13 ** | *(.058)* | 2% | 0.01 | *(.051)* |
| Must Support Dependents | 0% | 0.00 | *(.022)* | 7% | 0.01 | *(.022)* | 5% | 0.02 | *(.029)* | -1% | 0.00 | *(.035)* | 0% | 0.00 | *(.030)* |
| *Treatment-Related Conditions* | | | | | | | | | | | | | | | |
| Complete Outpatient Treatment | 0% | 0.00 | *(.026)* | 0% | 0.00 | *(.025)* | 8% | 0.03 | *(.033)* | 37% | 0.11 *** | *(.040)* | 1% | 0.00 | *(.035)* |
| Attend Alcohol Support Group | 7% | 0.01 | *(.035)* | -29% | -0.04 | *(.033)* | -8% | -0.03 | *(.044)* | -59% | -0.17 *** | *(.053)* | -22% | -0.08 * | *(.047)* |
| Treatment Evaluation Required | 0% | 0.00 | *(.032)* | -11% | -0.02 | *(.031)* | 8% | 0.03 | *(.040)* | 29% | 0.09 * | *(.049)* | -2% | -0.01 | *(.041)* |
| Community Treatment Program | -2% | 0.00 | *(.034)* | 40% | 0.06 * | *(.032)* | 12% | 0.04 | *(.042)* | 48% | 0.14 *** | *(.051)* | -3% | -0.01 | *(.041)* |
| Take Psychiatric Medicine if Prescribed | -89% | -0.17 * | *(.088)* | 33% | 0.05 | *(.079)* | 22% | 0.08 | *(.110)* | 88% | 0.26 ** | *(.129)* | 14% | 0.05 | *(.113)* |
| *Residence-Related Conditions* | | | | | | | | | | | | | | | |
| Community Corrections Center Residency | -86% | -0.16 ** | *(.077)* | -24% | -0.03 | *(.085)* | -70% | -0.27 ** | *(.111)* | -55% | -0.16 | *(.136)* | 14% | 0.05 | *(.110)* |
| Inpatient Treatment Center Residency | -43% | -0.08 | *(.224)* | -441% | -0.69 ** | *(.307)* | 45% | 0.17 | *(.395)* | -230% | -0.71 | *(.488)* | -262% | -0.95 ** | *(.426)* |

Notes:

N = 71,716. Outcomes are measured as whether the event took place anytime during the first year post-release. Employment outcomes come from parole data.

*Figure 7 – Variation in LSI-R Scores*

*Before Interview*




*Paroled Individuals*

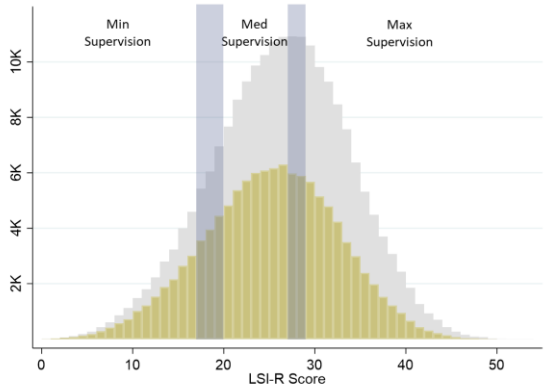



*Figure 8 – Potential for Differential Parole Along the Risk-Level Margin*

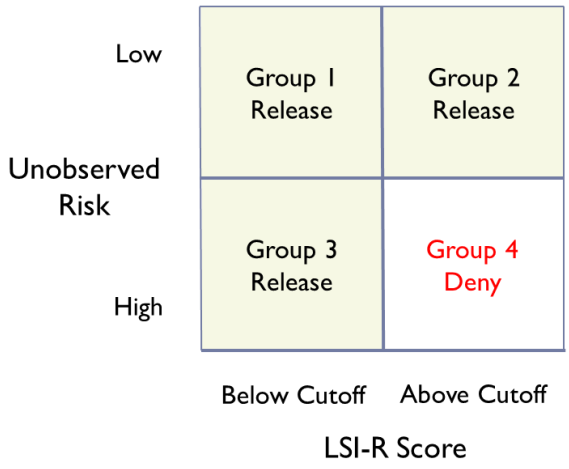


*Table 9 – Effects of Supervision Level*

| | All Releases | | | Releases Away from Margin of Release | | |
|---|---|---|---|---|---|---|
| **1 Year Post-Release Outcomes** | % Change | Estimate | *SE* | % Change | Estimate | *SE* |
| ***Low to Medium*** | | | | | | |
| Recidivism | *-1%* | -0.003 | *(.01)* | *17%* | 0.029 | *(.02)* |
| *Arrest* | *3%* | 0.004 | *(.01)* | *0%* | 0.000 | *(.02)* |
| *PV Reincarceration* | *-9%* | -0.007 | *(.01)* | *21%* | 0.012 | *(.01)* |
| Minor Parole Violation | *-1%* | -0.002 | *(.01)* | *6%* | 0.017 | *(.02)* |
| Employed - Q2 (Parole Data) | *1%* | 0.006 | *(.01)* | *-7%* | -0.034 | *(.03)* |
| Employed - Q2 (L&I Data) | *-3%* | -0.013 | *(.01)* | *-4%* | -0.019 | *(.02)* |
| ***Medium to High*** | | | | | | |
| Recidivism | *5%* | **0.017** * | *(.01)* | *35%* | **0.095** *** | *(.03)* |
| *Arrest* | *7%* | **0.014** ** | *(.01)* | *7%* | 0.012 | *(.02)* |
| *PV Reincarceration* | *7%* | 0.010 | *(.01)* | *50%* | **0.063** *** | *(.02)* |
| Minor Parole Violation | *-3%* | -0.014 | *(.01)* | *6%* | 0.023 | *(.03)* |
| Employed - Q2 (Parole Data) | *-1%* | -0.004 | *(.01)* | *2%* | 0.006 | *(.03)* |
| Employed - Q2 (L&I Data) | *-2%* | -0.007 | *(.01)* | *12%* | **0.050** * | *(.03)* |

Notes:

N = 47,082 for Column 1A, 62,597 for Column 1B, 12,070 for Column 2A, and 9,925 for Column 2B.

Column 2 restricts the sample to individuals away from the margin or release, i.e., in the top 20% of predicted parole probabilities prior to taking the LSI-R.

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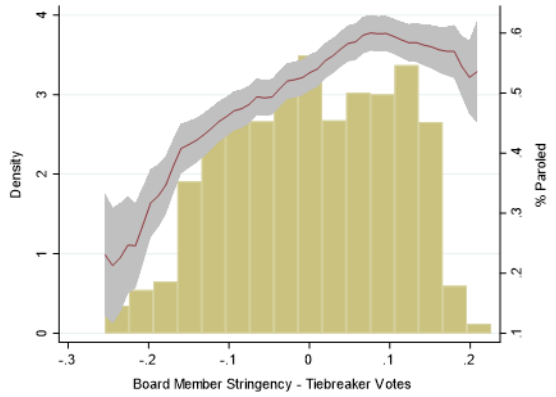
## Appendix

*Figure A1 – Variation in Parole Leniency for Board Member Tiebreaker & Majority Votes*



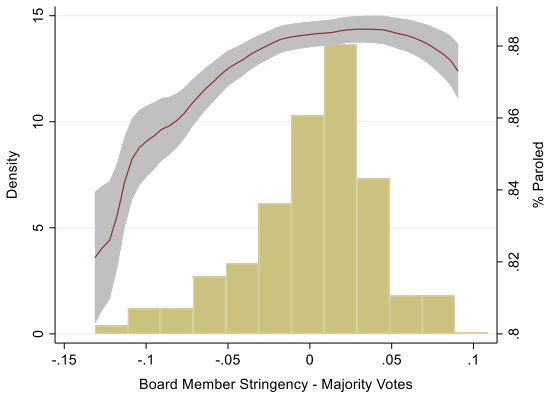



*Table A1 – Instrument Validity for Board Member Tiebreaker & Majority Votes Instruments*

| | Board Member Leniency (Tiebreaker Votes) | Board Member Leniency (Majority Votes) |
|---|---|---|
| **A. Relevance** | | |
| First stage f-value | 139 | 36 |
| **B. Exogeneity** | | |
| Joint f-test p-value | 0.79 | 0.83 |
| **C. Within-Subsample Monotonicity** | | |
| Violent Crime | 0.68*** | 0.18*** |
| Drug Crime | 0.99*** | -0.10 |
| Property Crime | 0.93*** | 0.11 |
| White | 0.86*** | .10** |
| Non-White | 1.01*** | 0.19*** |
| Regular Interview (2 votes needed) | na | na |
| Expedited Interview (1 vote needed) | na | na |
| Majority Vote Interview (5 votes needed) | na | na |
| **D. Across-Subsample Monotonicity** | | |
| Violent Crime | 0.65*** | 0.16*** |
| Drug Crime | 0.82*** | -0.09 |
| Property Crime | 1.03*** | 0.11 |
| White | 0.62*** | .09** |
| Non-White | 0.63*** | 0.19*** |
| Regular Interview (2 votes needed) | na | na |
| Expedited Interview (1 vote needed) | na | na |
| Majority Vote Interview (5 votes needed) | na | na |

Notes:

N = 3,379 tiebreaker board member votes and 39,873 board member majority votes.

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 A2 – Instrument Validity for Total Special Conditions Instruments*

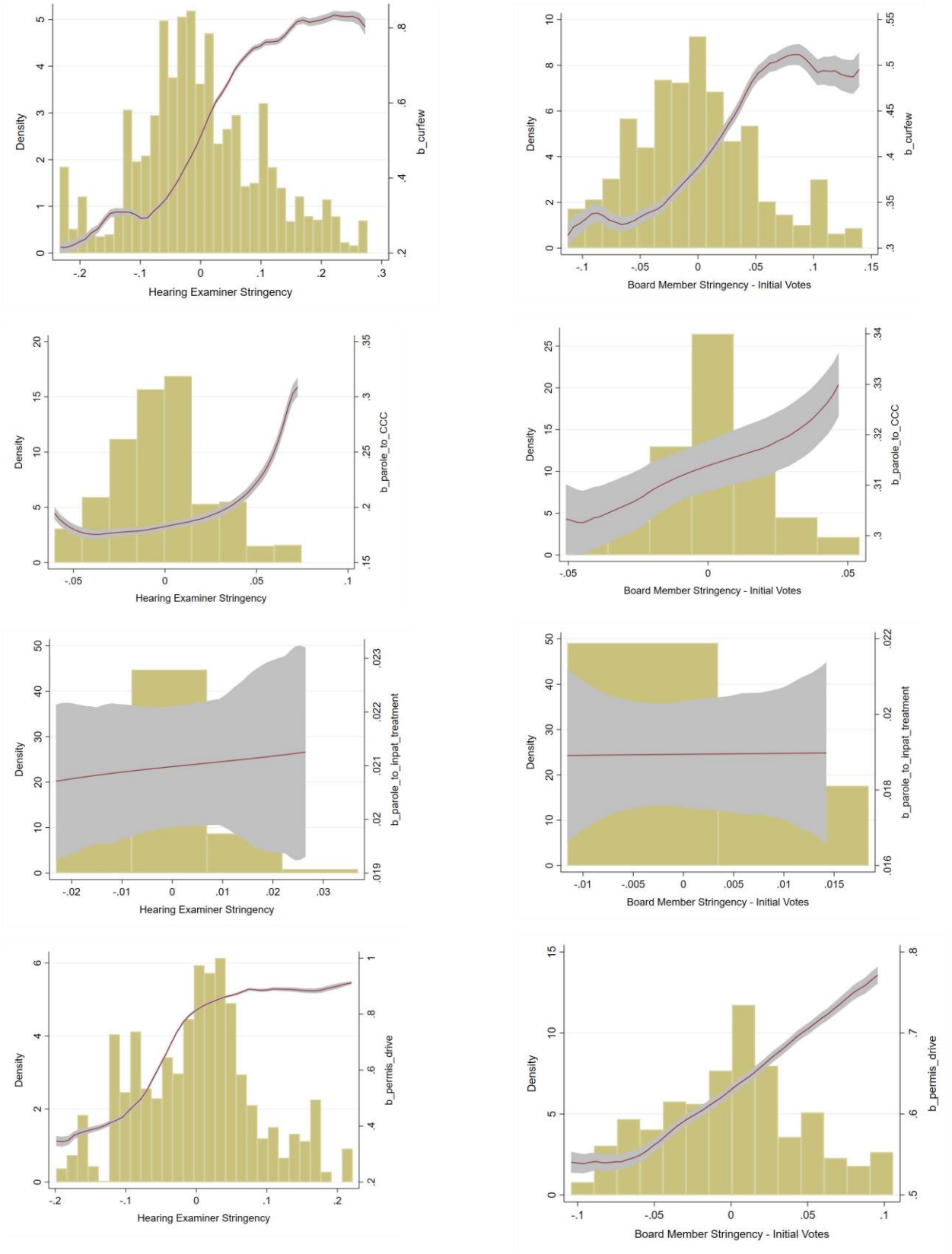
| | Hearing Examiner Leniency | Board Member Leniency |
|---|---|---|
| **A. Relevance** | | |
| First stage f-value | 6953 | 603 |
| **B. Exogeneity** | | |
| Joint f-test p-value | 0.12 | 0.81 |
| **C. Within-Subsample Monotonicity** | | |
| Violent Crime (paroled on leniency - estimate) | .91*** | 1.06*** |
| Drug Crime | 1.17*** | .82*** |
| Property Crime | 1.28*** | 1.10*** |
| Minimum Parole Hearing | 1.20*** | 0.99*** |
| Review Parole Hearing | 1.10*** | 1.09*** |
| Regular Interview (2 votes needed) | 1.09*** | 1.08*** |
| Expedited Interview (1 vote needed) | 1.37*** | na |
| Majority Vote Interview (5 votes needed) | 0.77*** | 0.96*** |
| **D. Across-Subsample Monotonicity** | | |
| Violent Crime (paroled on leniency - estimate) | 0.83*** | 0.90*** |
| Drug Crime | 1.20*** | 0.79*** |
| Property Crime | 1.36*** | 1.15*** |
| Minimum Parole Hearing | 1.25*** | 0.93*** |
| Review Parole Hearing | 1.03*** | 0.96*** |
| Regular Interview (2 votes needed) | 0.98*** | 0.42*** |
| Expedited Interview (1 vote needed) | 1.40*** | na |
| Majority Vote Interview (5 votes needed) | 0.76*** | 0.81*** |

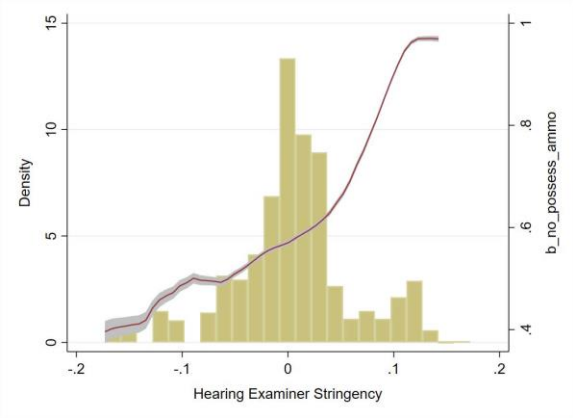
Notes:

N = 56,950 hearing examiner votes and 32,314 initial board member votes.

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.

*Figure A2 – Variation in Individual Condition Leniency*

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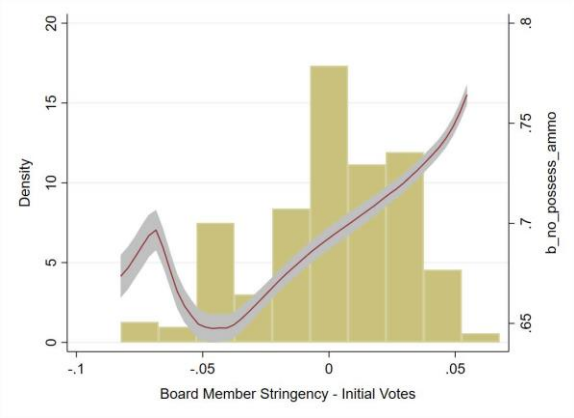
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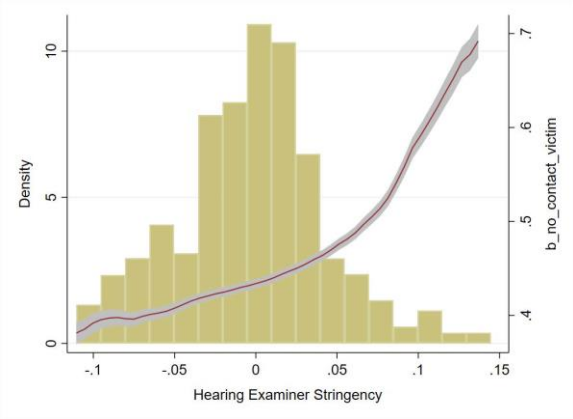
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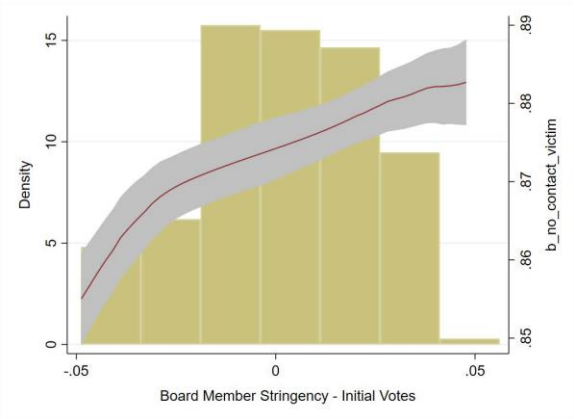
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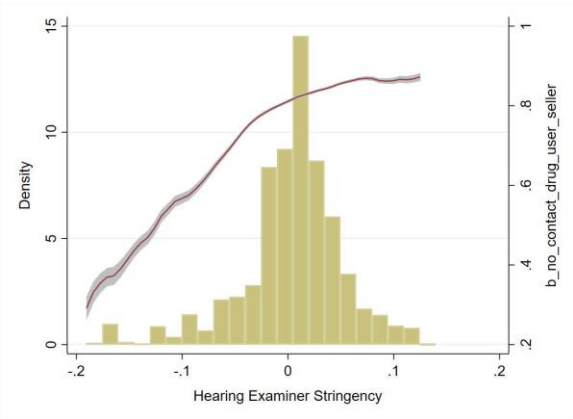
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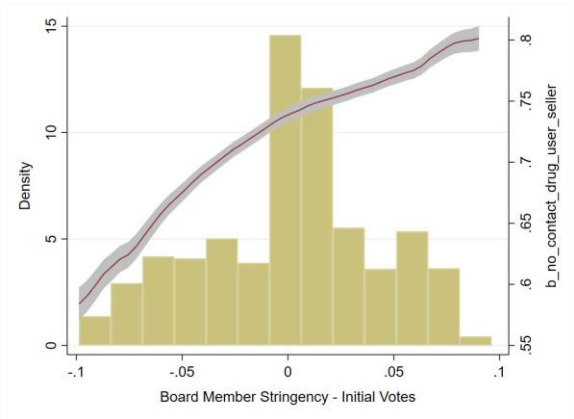
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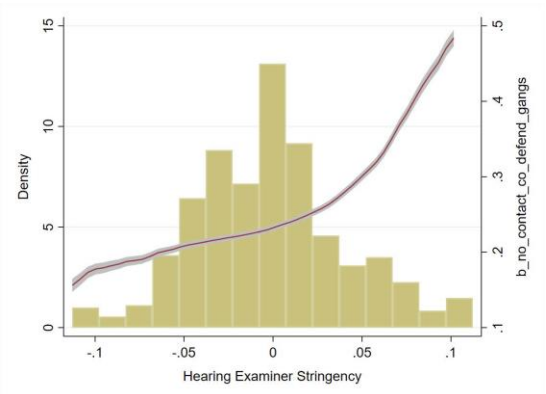
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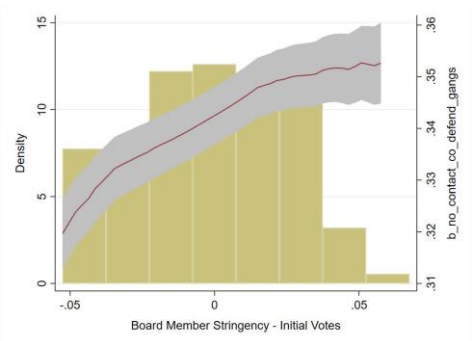
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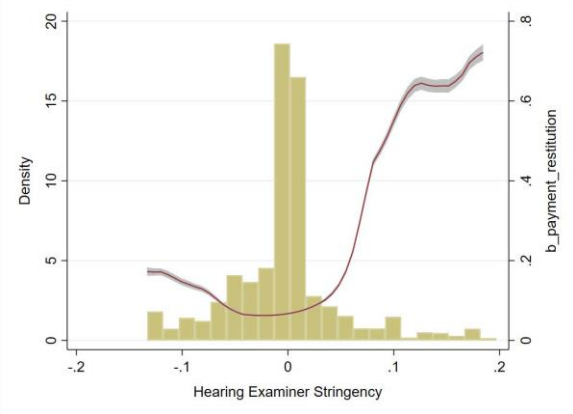
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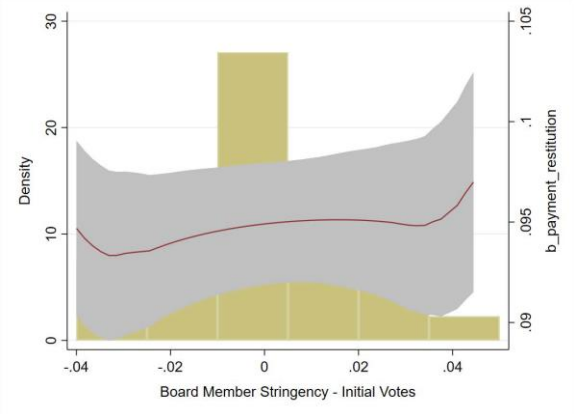
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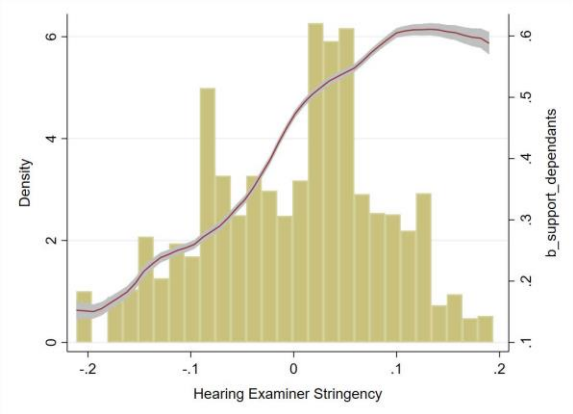
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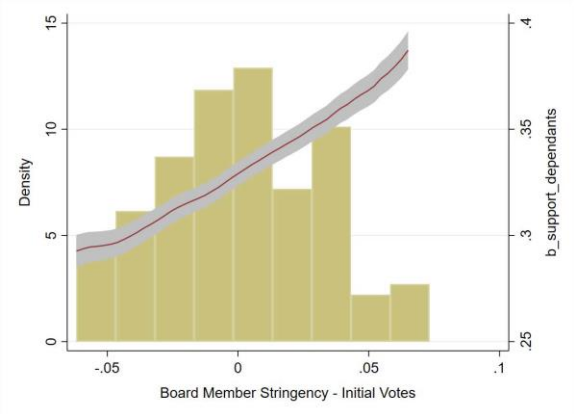
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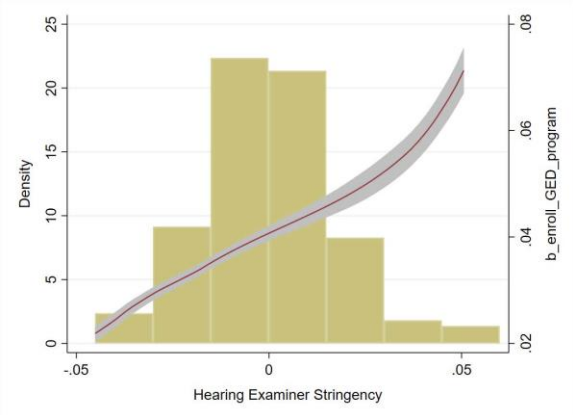
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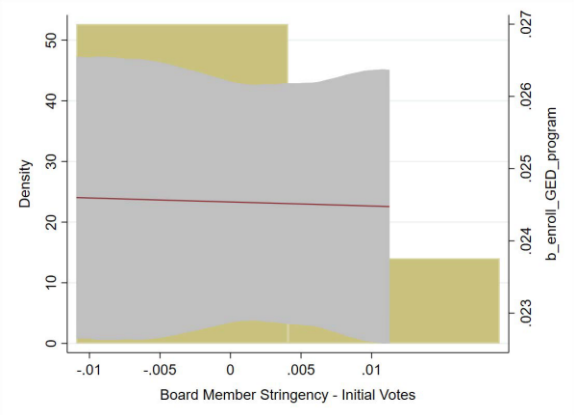
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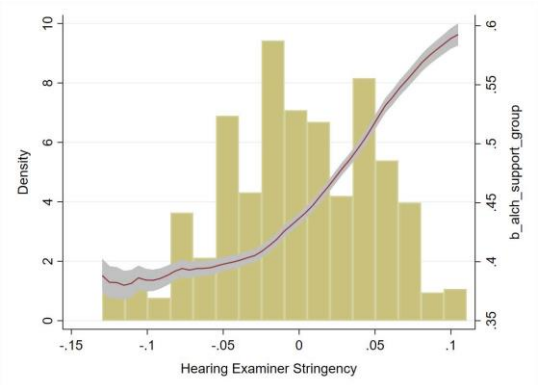
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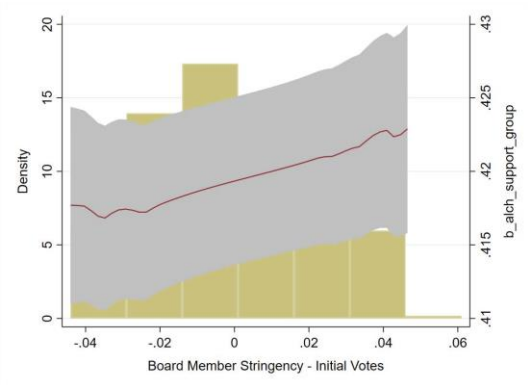
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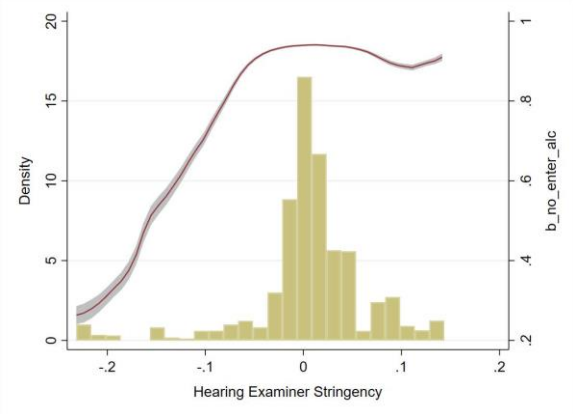
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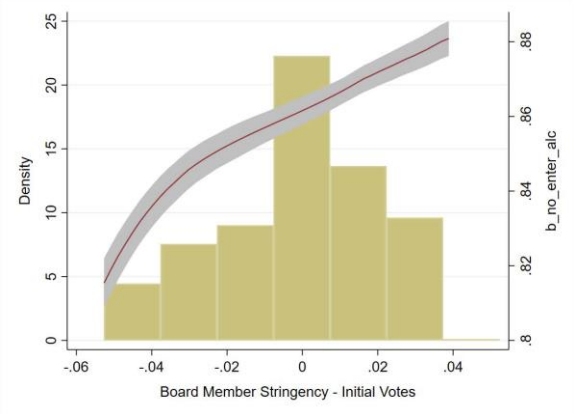
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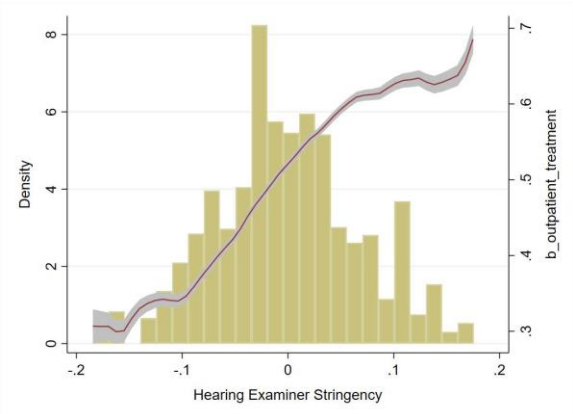
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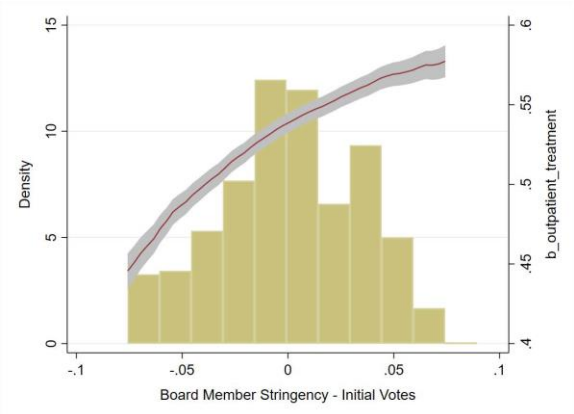
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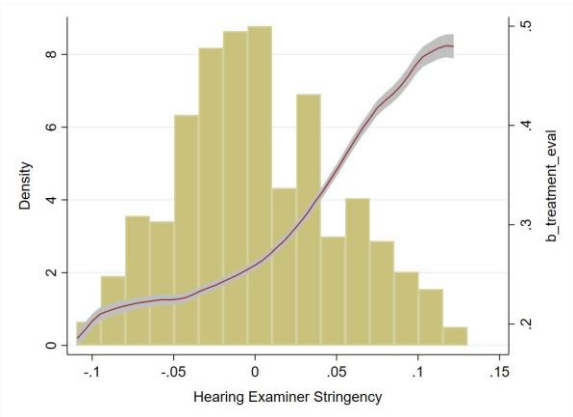
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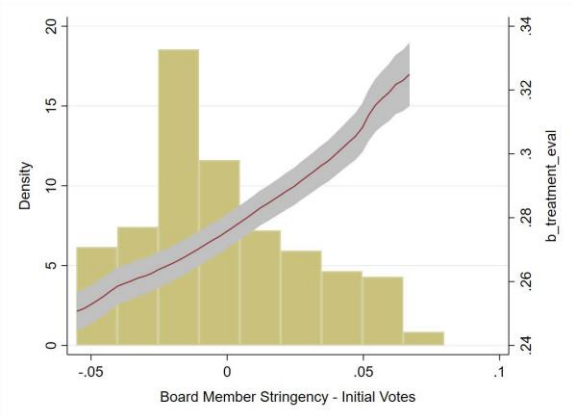
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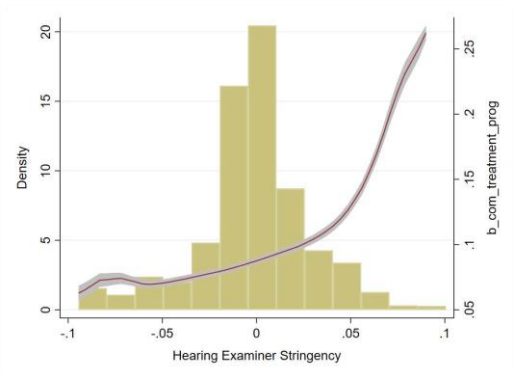
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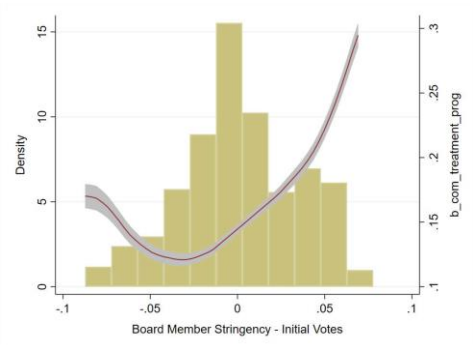
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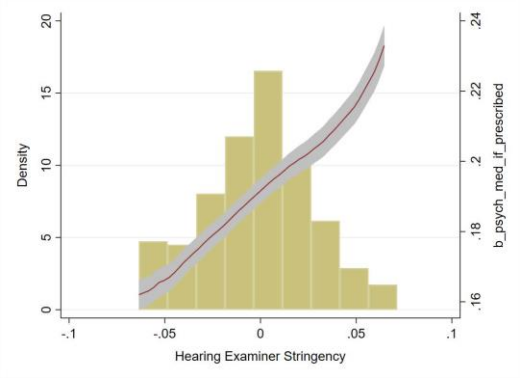
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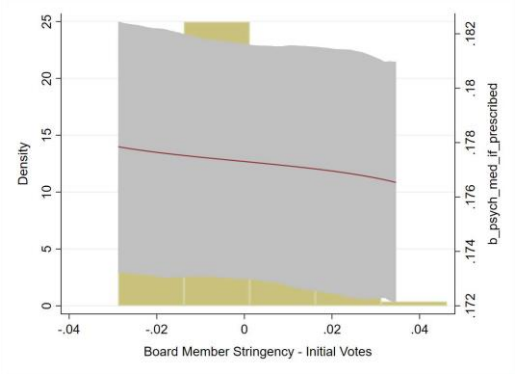
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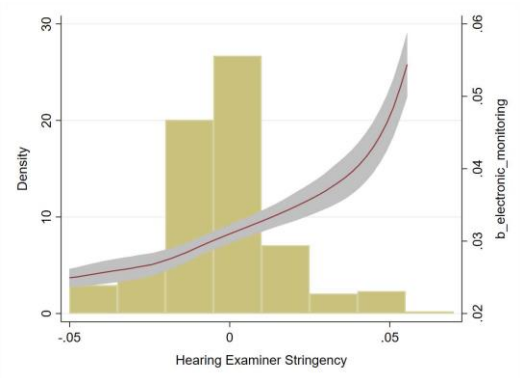
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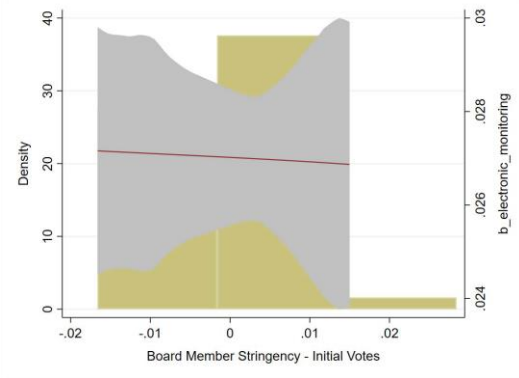
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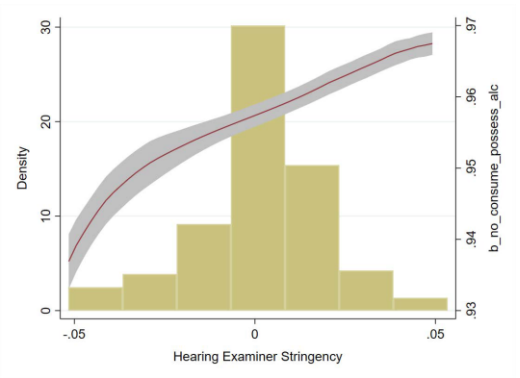
Board Member Stringency - Initial Votes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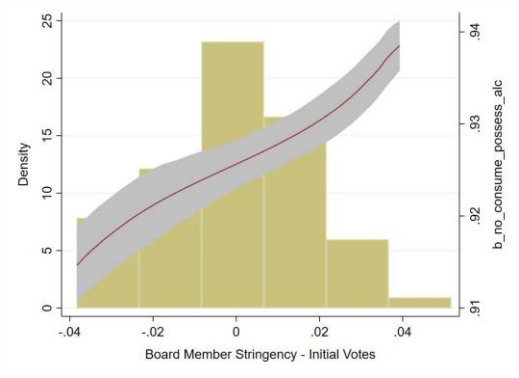
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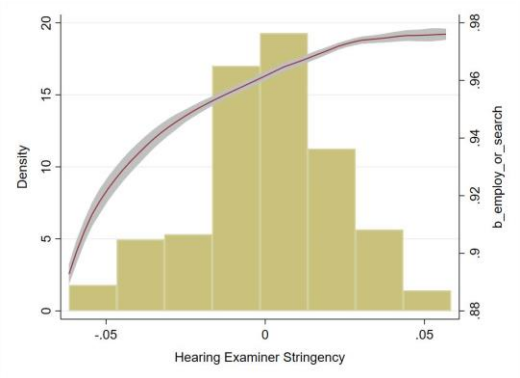
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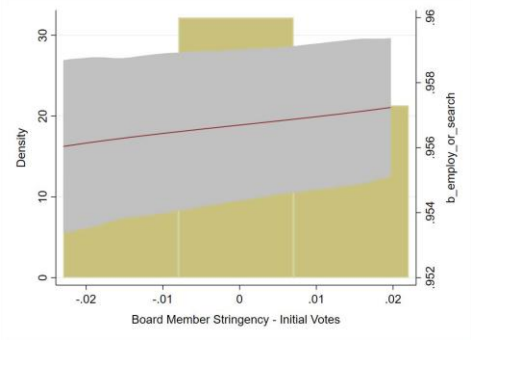
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Board Member Stringency - Initial Votes

Table A3 - *Instrument Relevance and Exogeneity for Total Special Conditions Instruments*

| | Hearing Examiner Relevance | Board Member Relevance | Hearing Examiner Exogeneity | Board Member Exogeneity |
|---|---|---|---|---|
| *General Restrictions* | | | | |
| Community Corrections Center Residency | 505 | 34 | 0.0010 | 0.02 |
| Curfew | 12775 | 1221 | 0.0008 | 0.59 |
| Require Permission to Drive | 8790 | 769 | 0.0008 | 0.62 |
| Cannot Enter Alcohol Establishments | 12594 | 245 | 0.1068 | 0.50 |
| Cannot Possess Ammo | 9600 | 522 | 0.0108 | 0.77 |
| Payment Restitution via Wage Attachment | 9751 | 985 | 0.5001 | 0.05 |
| *Contact-Related Conditions* | | | | |
| Cannot Contact Codefendants or Gangs | 1799 | 45 | 0.0675 | 0.60 |
| Cannot Contact Drug Users or Sellers | 8368 | 631 | 0.1005 | 0.09 |
| Cannot Contact Victims | 2809 | 148 | 0.4708 | 0.49 |
| Must Support Dependents | 4476 | 153 | 0.0000 | 0.89 |
| *Treatment-Related Conditions* | | | | |
| Inpatient Treatment Residency | 349 | 1 | 0.0023 | 0.04 |
| Attend Alcohol Support Group | 2682 | 28 | 0.0140 | 0.03 |
| Complete Outpatient Treatment | 5102 | 278 | 0.0018 | 0.01 |
| Treatment Evaluation Required | 2018 | 196 | 0.0000 | 0.17 |
| Community Treatment Program | 1823 | 472 | 0.0052 | 0.80 |
| Take Psychiatric Medicine if Prescribed | 844 | 1 | 0.0000 | 0.56 |
| *Non-Discretionary Conditions* | | | | |
| Supervision Fee | 49 | 73 | 0.0004 | 0.12 |
| Drug Testing Required | 344 | 219 | 0.0000 | 0.74 |
| Cannot Consume or Possess Alcohol | 1640 | 176 | 0.0094 | 0.52 |
| Must Maintain Employment or Active Job Se | 3219 | 25 | 0.0000 | 0.14 |
| Enroll in GED Program | 1273 | 0 | 0.0007 | 0.67 |
| Electronic Monitoring | 6727 | 152 | 0.0006 | 0.00 |
| Travel Restrictions | 262 | 4 | 0.0005 | 0.63 |

Notes:

N = 56,950 hearing examiner votes and 32,314 initial board member votes.

Columns 1 and 2 present 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. Columns 3 and 4 present joint f-test p-values from regressions of hearing examiner and board member leniency measures on observable characteristics about the individual.