The Effects of Misdemeanor Bail Reform

Paul Heaton
August 16, 2022

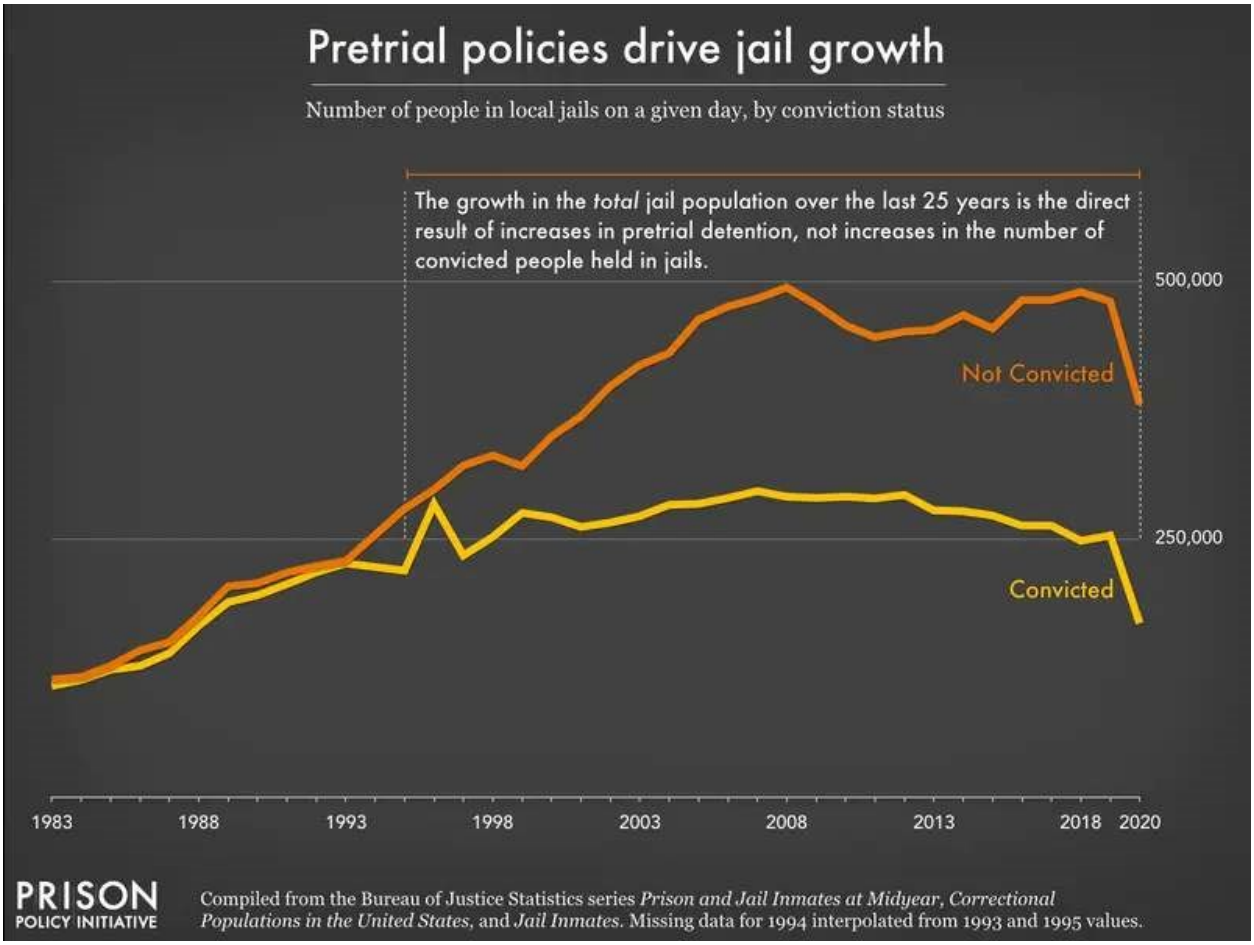
QUATTRONE CENTER
for the Fair Administration of Justice

Acknowledgements: This research was funded by Arnold Ventures. We express appreciation to Jim Bethke, AJ Roy, Laura Hogan, and Stephanie Armand from the Harris County Justice Administration Department who facilitated access to the underlying data. Claire Fondrie-Teitler provided outstanding research assistance. We acknowledge helpful comments from Ross Miller, Megan Stevenson, and seminar participants at the University of Pennsylvania. All statements and material in the document are solely the responsibility of the authors and do not represent the views of any of the aforementioned individuals or organizations.

1. Background

There is ongoing public debate over the necessity and proper scope of pretrial reform

Jurisdictions throughout the country are considering changes to pretrial policy. In recent years, a number of states and cities have enacted reforms designed to reduce the role of cash bail in the pretrial process and expand access to pretrial release. One factor fueling calls for reform is a growing recognition that pretrial detention--which is to say, jailing legally innocent people prior to the adjudication of their case as a means of ensuring court appearance and/or preventing criminal acts while in the community--is a major contributor to mass incarceration in the United States, as shown in the data below compiled by the Prison Policy Initiative.



Source: https://www.prisonpolicy.org/reports/pie2022.html

However, the uptick in certain categories of crime--most notably homicide--in the past few years across the U.S. has led some to argue that pretrial reform should be halted or rolled back. Critics claim that pretrial reform has fueled an increase in crime by releasing dangerous people who would have been incapacitated had they been held pretrial.

Harris County, Texas--the nation's third most populous county and the home of Houston--has been an epicenter of debate over bail reform. Beginning in 2017, the county implemented an ambitious set of reforms targeting people charged with misdemeanors, and some have also advocated for reform to pretrial policy for felony defendants. However, a mixed public narrative regarding these reforms has developed. Some community groups, advocates, and policymakers claim that the reforms have successfully reduced detention and associated costs. Others claim that recent increases in crime in the county can be attributed to reforms that began in 2017.

Source: https://www.khou.com/article/news/local/report-bail-system-harris-county-more-crime/285-40699193-260b-49f6-8fad-cf2266da23f5

Source: https://www.houstonpublicmedia.org/articles/news/criminal-justice/2022/03/03/420398/two-years-on-harris-countys-misdemeanor-bail-reforms-appear-to-be-working-as-intended/

Research Question

How does expanding pretrial release for people charged with misdemeanors affect case outcomes and future system contact?

This work summarizes ongoing research conducted by the Quattrone Center for the Fair Administration of Justice designed to inform current debates about bail reform. Using detailed data covering tens of thousands of cases in the county, we evaluate a key early bail reform provision in Harris County, a federal injunction that required release of individuals charged with misdemeanors who were assigned small amounts of cash bail under the then-existing system but who indicated they were unable to pay. Our independent assessment of the impacts of this reform on a range of outcomes, including public safety, provides important new evidence regarding the efficacy of reforms that increase release rates for those charged with low-level offenses, an approach that has recently been considered in numerous jurisdictions.

Existing Research Demonstrates Pretrial Detention Has Important Downstream Effects

Numerous high-quality studies of the effects of pretrial detention have been published in recent years. Many of these studies use judge random assignment or other quasi-experimental methods to establish the causal relationship between detention and case outcomes. Several relevant studies are catalogued below.

Study	Finding
Arpit Gupta, Christopher Hansman & Ethan Frenchman, The Heavy Costs of High Bail: Evidence from Judge Randomization, 45 J. Legal Stud. 471 (2016)	In Pittsburgh and Philadelphia, imposing cash bail increases conviction rate by 12% and increases future crime by 9%
Emily Leslie & Nolan G. Pope, The Unintended Impact of Pretrial Detention on Case Outcomes: Evidence from New York City Arraignments, 60 J.L. Econ. 529 (2017).	In New York, pretrial detention increases conviction rate by 20% for misdemeanors and doubles average incarceration length
Megan T. Stevenson, Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes, 34 J.L. Econ. & Org. 511 (2018).	In Philadelphia, pretrial detention increases conviction rate by 13%, sentence length by 42%, and court fees by 41%

Study	Finding
Will Dobbie, Jacob Goldin & Crystal S. Yang, The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges, 108 Am. Econ. Rev. 201 (2018).	In Philadelphia and Miami-Dade, pretrial detention increases guilty plea rate by 32%, does not reduce future offending, and decreases employment rates by 20%
Stephanie Didwania, The Immediate Consequences of Federal Pretrial Detention, 22 Am. L. & Econ. Rev. 24 (2020).	In the federal system, pretrial release reduces sentence length
Paul Heaton, Sandra Mayson & Megan Stevenson, The Downstream Consequences of Misdemeanor Pretrial Detention, 69 Stan. L. Rev. 711 (2017).	In Harris County, TX, misdemeanor pretrial detention increases guilty pleas by 25% and more than doubles sentence length.

Predictions Based on Prior Work

The reform we study was intended to increase pretrial release rates for individuals charged with misdemeanors. Based on the existing body of high-quality research, we would hypothesize that such a reform would have the following effects:

1. Reduce guilty pleas
2. Reduce conviction rates
3. Reduce likelihood of a jail sentence
4. Reduce average sentence length
5. Not increase, and possibly reduce, future contact with the criminal justice system

2. Data Sources

Pursuant to a MOU approved by the county commissioners in December 2020, we obtained administrative data from the county covering all misdemeanor and felony cases from 2015-present, a total of 517K cases as of May 2022. To provide us with data, the Harris County Justice Administration Department (JAD) coordinated a complex process to combine data from numerous county criminal justice agencies, each of which has independent data systems that record various data points related to the processing of criminal cases. The table below summarizes key data elements drawn from each agency.

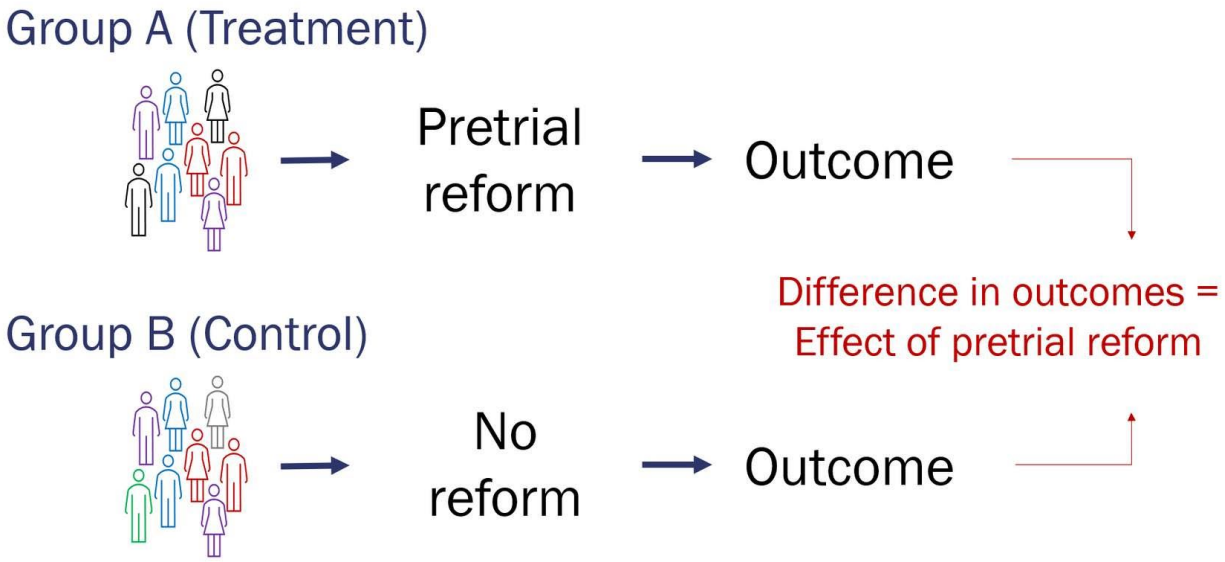
Data Source	Key Data Elements
Police	Arrest timing Defendant demographics
Sheriff	Booking and release times
Pretrial Services	Risk assessment scores
Courts	Bail requirements Filed charges Charge resolution Sentence Future system contact

Although the data were anonymized in our analytic file, we retained the ability to link cases involving the same individual across time, meaning that we could construct synthetic criminal histories within the county and also identify new cases involving the same individual that occurred after the initiation of a particular case.

3. Analytic Approach

There are many different possible reforms to the pretrial system. In addressing the impacts of "bail reform", it is important to be precise about the particular intervention one is evaluating. In this study, we focus on a reform which allows some individuals charged with misdemeanors who would have been detained under a cash bail system to instead be released on unsecured bail. This particular variety of reform is of considerable interest for several reasons: 1) many jurisdictions that have implemented pretrial reform have done something similar; i.e. expanded release for those charged with less serious crimes; 2) releasing people charged with misdemeanors is likely more politically feasible than reforms targeting people charged with felonies; and 3) pretrial detention in less serious cases is of particular concern because such cases are widespread, and because of the potential role pretrial detention could play in producing wrongful convictions.

To measure the effects of liberalizing pretrial release, we would ideally compare groups of defendants who were and were not exposed to reform but who are otherwise as similar to one another as possible.



A key challenge is accounting for the many other factors distinct from pretrial reform that affect how cases resolve in the criminal system. For example, we know that macro level factors such as demographic change, natural disasters, or the COVID epidemic are likely to impact crime rates and case adjudication patterns. Moreover, demographics are likely to play a role in how people charged with crimes interact with the system, and the composition of the

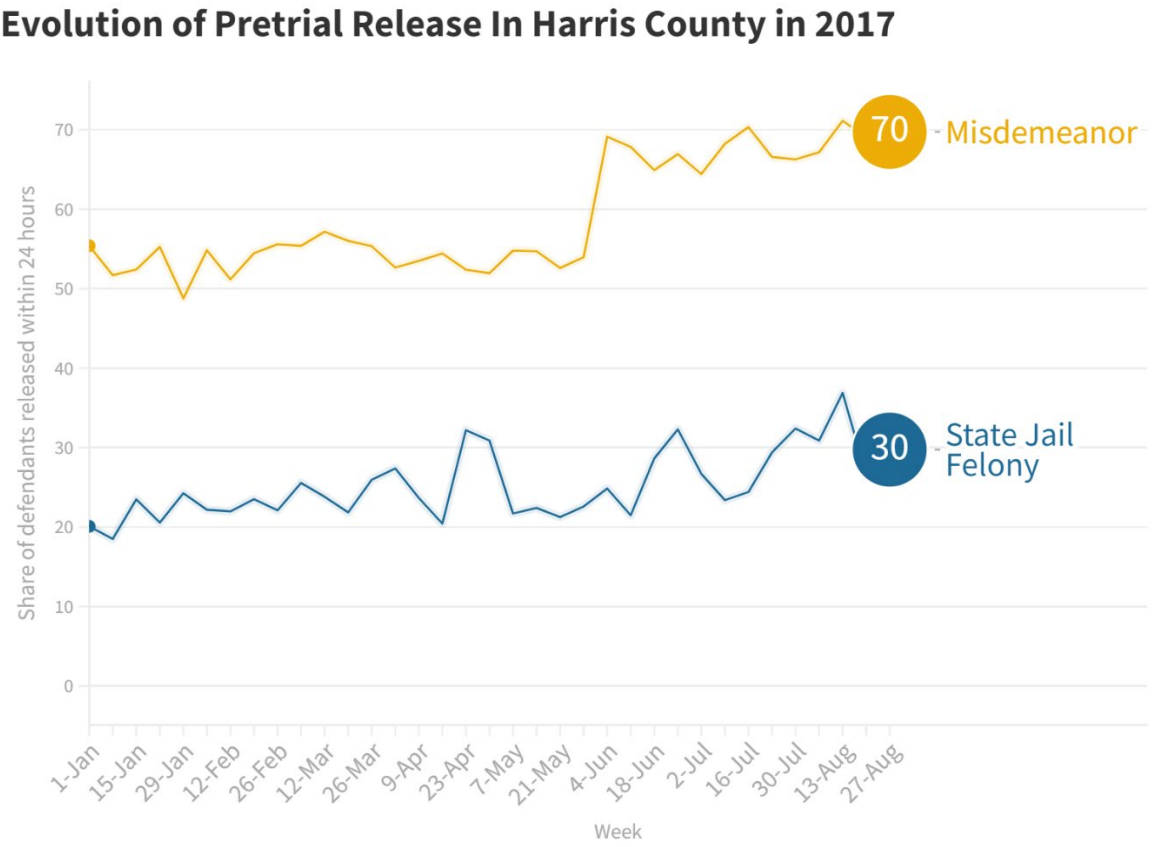
defendant pool is changing over time. A strong evaluation requires a credible way of disentangling the effects of pretrial reform from such other factors.

For this study, we exploit the June 4, 2017 onset of a federal injunction which arose in response to a lawsuit against the county. In 2016, a coalition of civil rights groups sued Harris County in federal court on behalf of Maranda ODonnell and a class of similarly-situated people. These plaintiffs asserted that the existing pretrial system violated constitutional guarantees of due process and equal protection in that proceedings were not sufficiently individualized and discriminated against poor defendants.

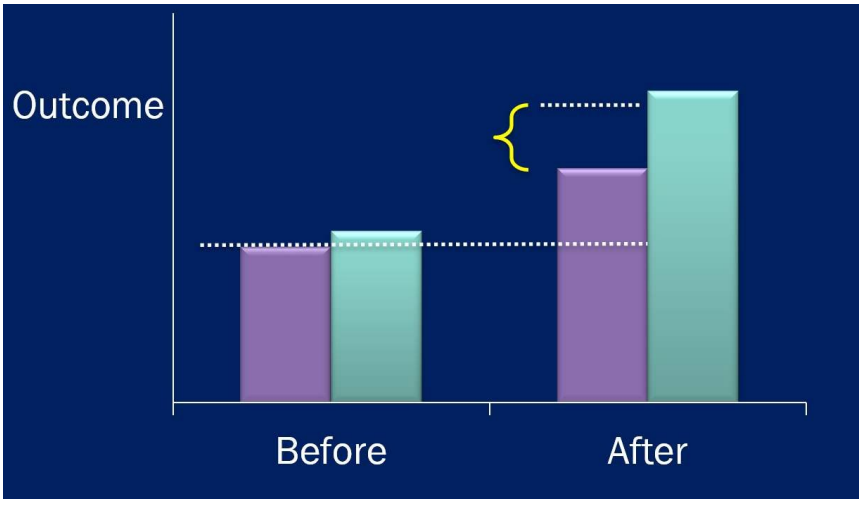
The county opposed the lawsuit, but in April 2017, following an extensive evidentiary hearing, a federal judge issued a preliminary injunction in the case that largely sided with the plaintiffs. Most defendants under the then-existing system could be released shortly after arrest if they could post 10% of a bond amount of between $500 and $5,000 that was largely determined by a bail schedule that set cash bail amounts according to a grid based on prior criminal history and seriousness of current charge. One of the key provisions of the injunction required that the county release any individuals charged with misdemeanors who would have been bailable under the then-existing system, but who signed an affidavit saying that they were unable to pay the required bail amount. Several categories of defendants, including those with holds (e.g., due to prior immigration violations) or those charged with family violence offenses were not releasable under the injunction.

The graphic below illustrates the intuition behind our empirical strategy. The figure plots the share of defendants released within 24 hours after being booked, with separate lines for those charged with misdemeanors and state jail felonies. State jail felonies are a category of felony in Texas punishable by 6 months to 2 years in custody; this less serious form of felony is the category of felony arguably most similar to a misdemeanor.

In the first half of 2017, the release rates across the two categories of offenses track each other fairly consistently, but there is a sharp increase in the release rate for people charged with misdemeanors that coincides with the beginning of the injunction. The release rate for those charged with state jail felonies remains fairly stable for the rest of the year. This abrupt shift furnishes a "natural experiment" useful for measuring the impacts of a reform that expands access to pretrial release. In particular, we can compare outcomes for people charged with misdemeanors to those charged with state jail felonies before and after the injunction. There is no strong reason to expect that misdemeanor cases filed shortly after the injunction should be dramatically



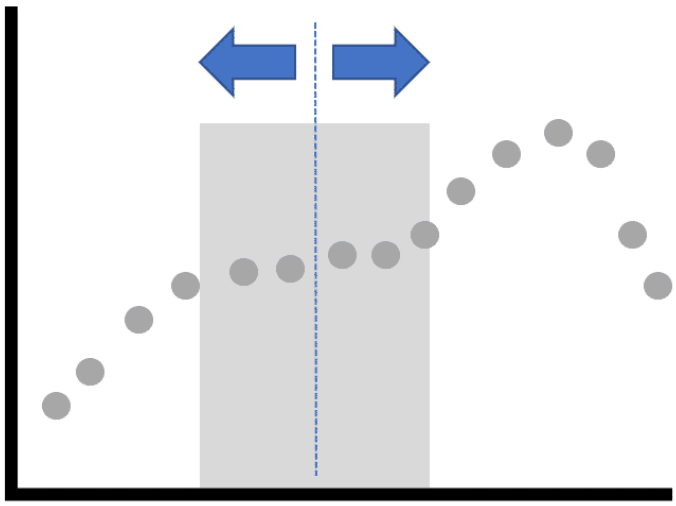
different from those filed before the injunction in terms of quality of evidence, underlying culpability of the defendant, etc., but the former set of cases did have much greater access to pretrial release. The inclusion of an unaffected control group of defendants charged with felonies allows us to net out the impacts of other factors (e.g. Hurricane Harvey) that impacted all criminal defendants in the system.



To operationalize the analysis, we estimate differences-in-differences regressions where the unit of observation is a case, and the primary explanatory variable is an indicator for a misdemeanor case in the post-injunction period. In our baseline

analysis, we focus on the 55,792 misdemeanor and state jail felony cases filed in calendar year 2017.

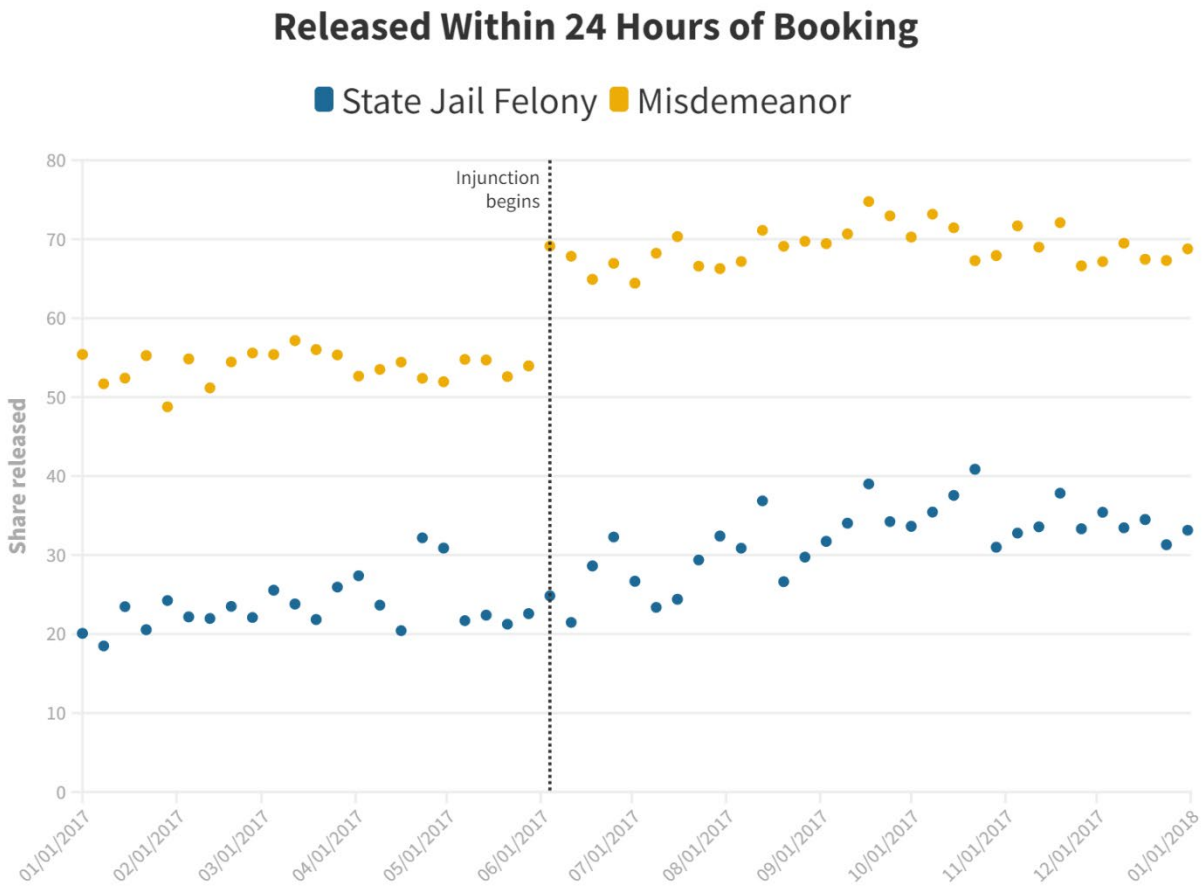
We focus on the time period immediately surrounding the injunction because it likely furnishes the best opportunity to isolate the effects of expanding pretrial release from other factors that affect who commits crime and how cases are adjudicated. In our robustness checks we consider a variety of alternative time periods and comparison groups.



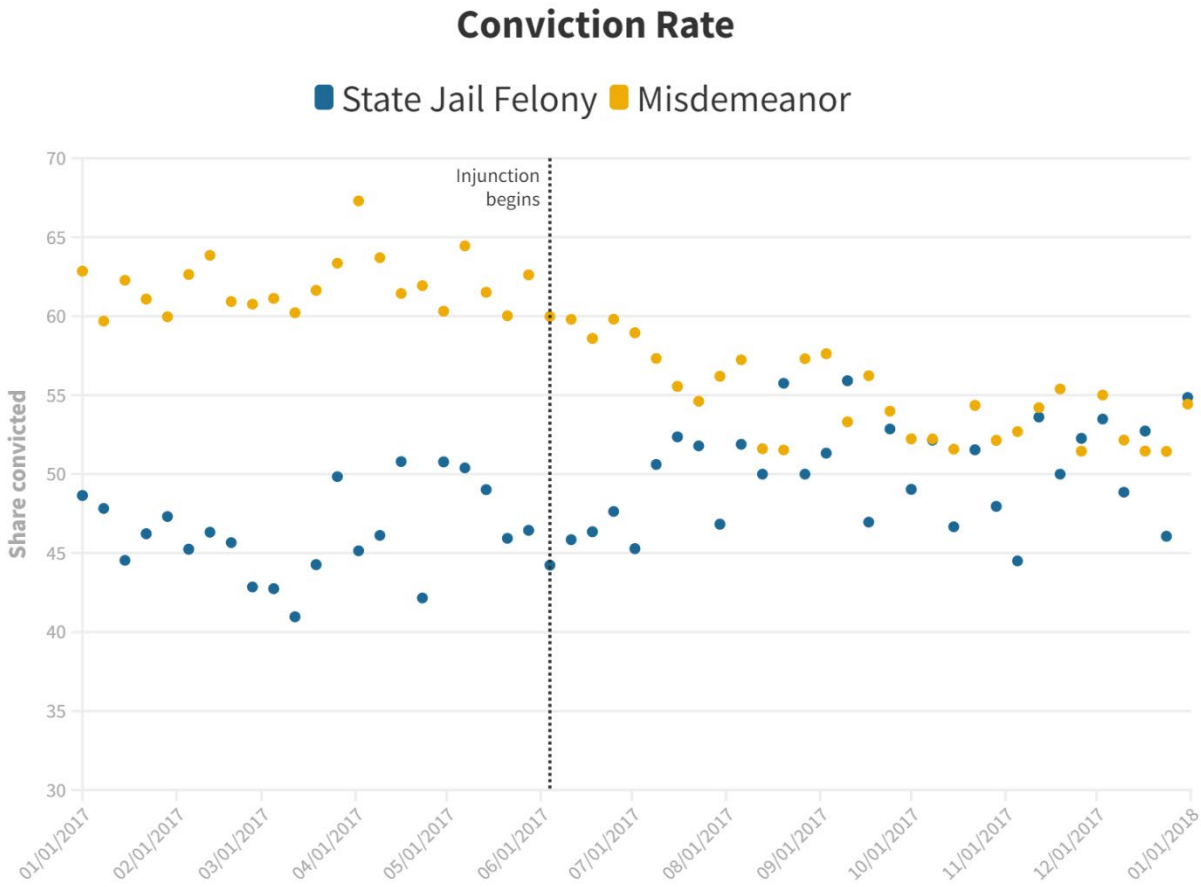
Our regressions control for hundreds of variables capturing contextual factors related to the case, including defendant demographics (age, sex, race, ethnicity, ZIP), prior criminal history, case characteristics (date, court, attorney type, arresting agency), and detailed charges. Conceptually, the regression analysis is designed to compare the difference in outcomes between two pools of defendants that have similar criminal histories, demographics, and charges, but who differ in their access to pretrial release due to the timing of when their cases arose vis-à-vis the injunction.

4.Raw Data

Before moving to the regression results, it is useful to just look at the raw data, as it provides suggestive evidence of the injunction's impacts. The figure below shows average release rates, where averages are taken across all cases of a given category (misdemeanor or state jail felony), filed in a particular week. The figure shows an abrupt jump of around 15 percentage points in the release rate for misdemeanor defendants as the injunction was implemented. This suggests the injunction did successfully reduce pretrial detention. It is also noteworthy that even after the injunction, a non-negligible fraction of people charged with misdemeanors were still detained for more than 24 hours, due to holds, charges involving carve out offenses like family violence, unwillingness to complete an indigency affidavit, or other reasons. The chart also suggests some increase over time in release rates for people charged with state jail felonies, suggesting that there may have been some upward drift in pretrial release rates even without the injunction.

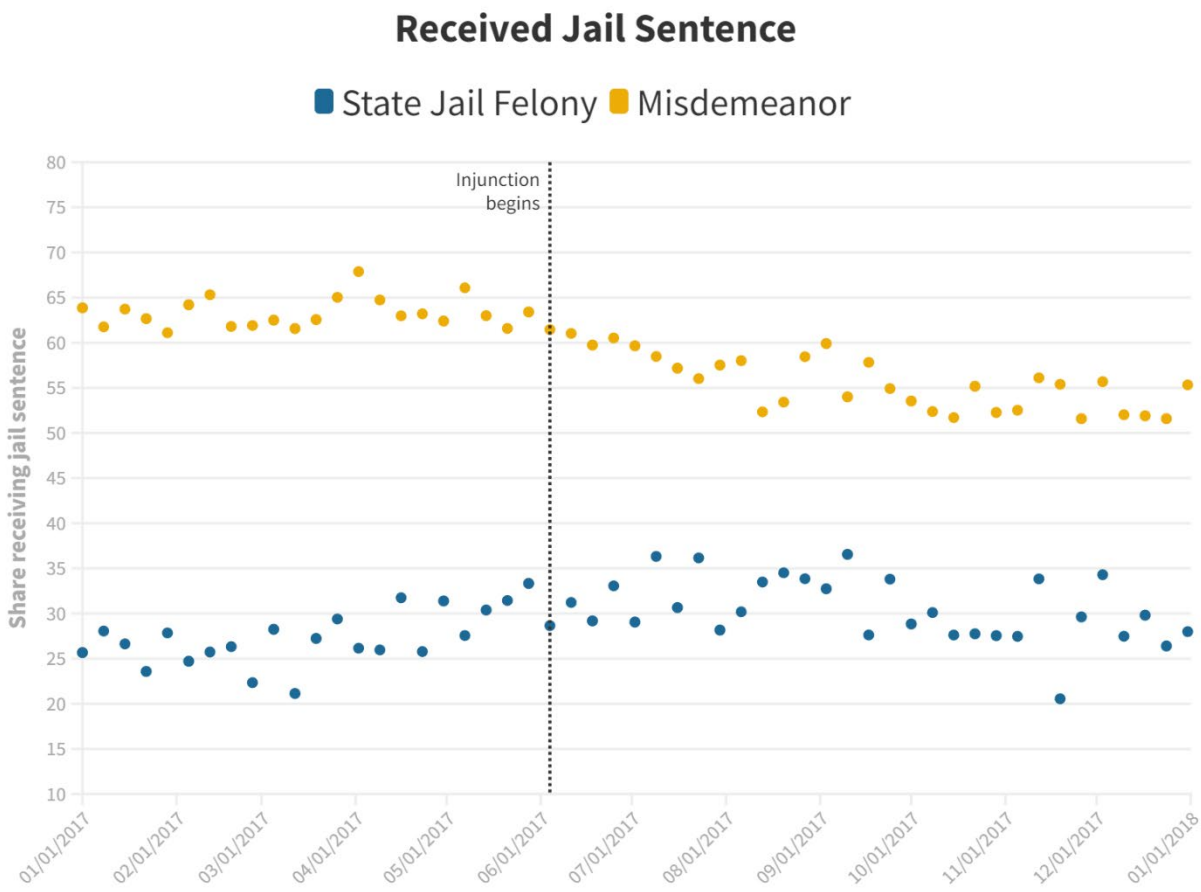


Existing evidence from high-quality studies indicates that pretrial release causally reduces the probability of conviction. Given that the injunction clearly increased release rates, if the injunction operates as expected, we might expect to see a corresponding change in conviction rates at the time of the injunction. The figure below, which plots conviction rates (where cases can result in either a conviction, diversion, or non-conviction) over time, shows exactly that.



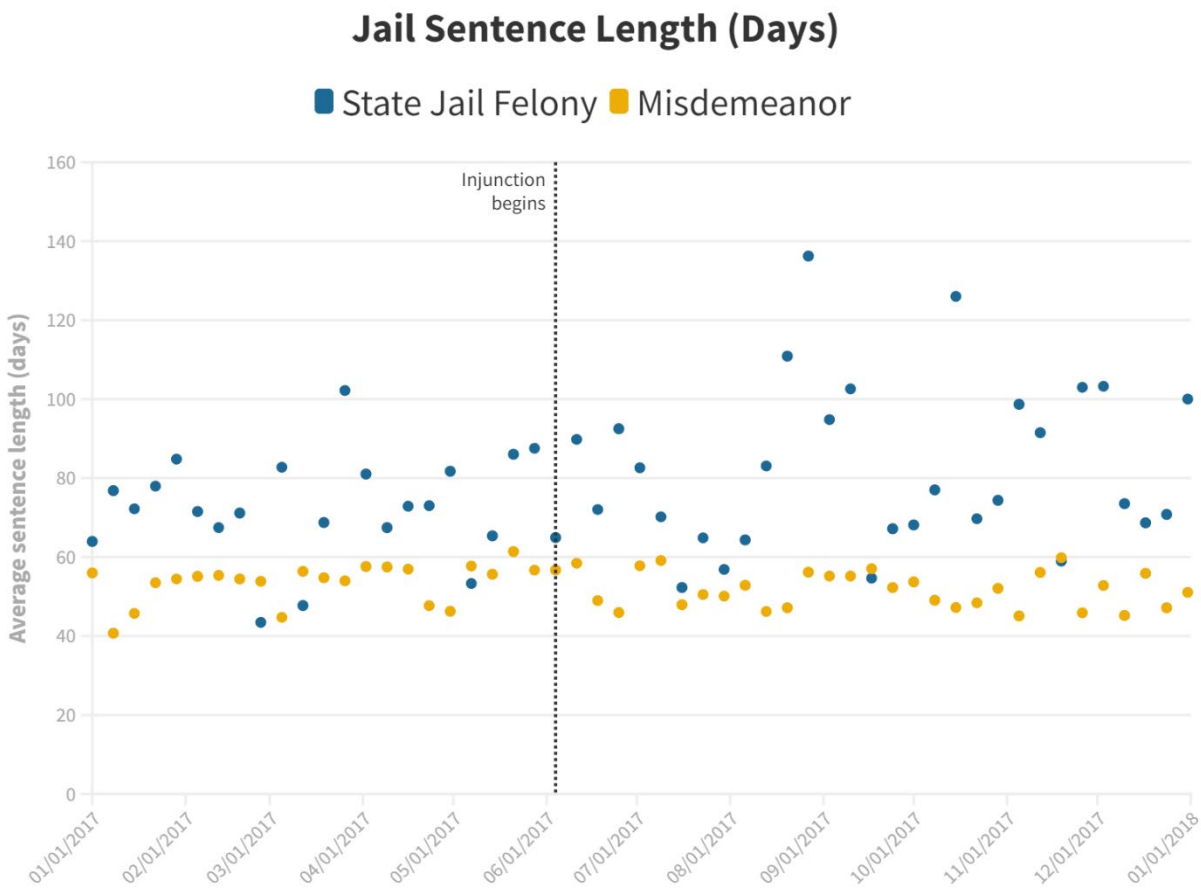
Whereas conviction rates evolve roughly in parallel across the two groups of defendants in the first half of 2017, beginning in the summer, the conviction rate for people charged with misdemeanors falls. The timing of the change appears consistent with the notion that it reflects the impacts of the injunction.

When we look at whether someone ultimately received a jail sentence in their case, we observe a similar pattern. The rates evolve in parallel during the beginning of the year, but then converge as the injunction goes into effect.



For some outcomes, such as custodial sentence length, the visual patterns are less obvious. In the figure below it appears that jail sentences for people charged with misdemeanors didn't change much following the injunction, although there is suggestive evidence from people charged with state jail felonies that sentences may have been rising over 2017.

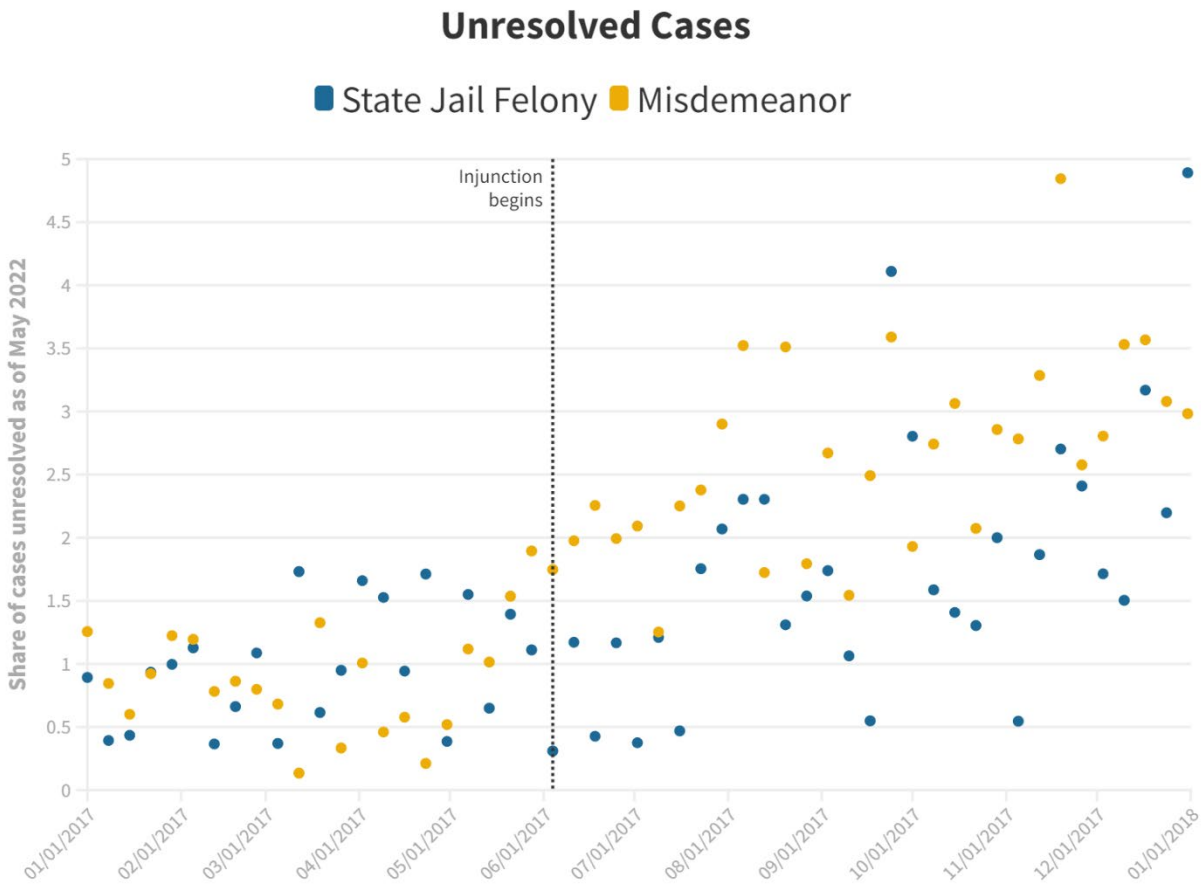
Of course, one potential concern with releasing more people pretrial is that they may not show up for court, meaning that if they did commit the alleged offense they may not be held accountable. Researchers have considered a variety of ways to measure non-appearance, and there continues to be a robust debate about which measures are most appropriate. For example, one drawback of relying on official measures such as bond revocation is that these result from discretionary decisions by judges and magistrates, and decisions to revoke bond may depend as much on the underlying policy environment as they do on the behavior of defendants.



In an effort to assess non-appearance in some way, we constructed a new measure of unresolved cases by coding whether each given case had a disposition recorded as of the time of our data pull (May 2022). This measure is different from failures to appear (because it is possible for someone to fail to show up for certain court dates but still ultimately resolve their case) and bond forfeitures (because it is possible for someone to consistently fail to appear without having their bond forfeited). However, conceptually, it seems well aligned with a primary argument often raised in support of pretrial detention, which is that without being held in custody, a defendant may escape accountability for a criminal act. Individuals who have an unresolved case by this metric have not had their cases fully vetted through the adjudication process; those whose cases have resolved have completed enough of the process that we are at least able to record a disposition for the case.

Visually, it appears in the figure below that there is an appreciable uptick in the rate of case non-resolution that coincides with the onset of the injunction. At the same time, case non-resolution is an infrequent problem, as the vast

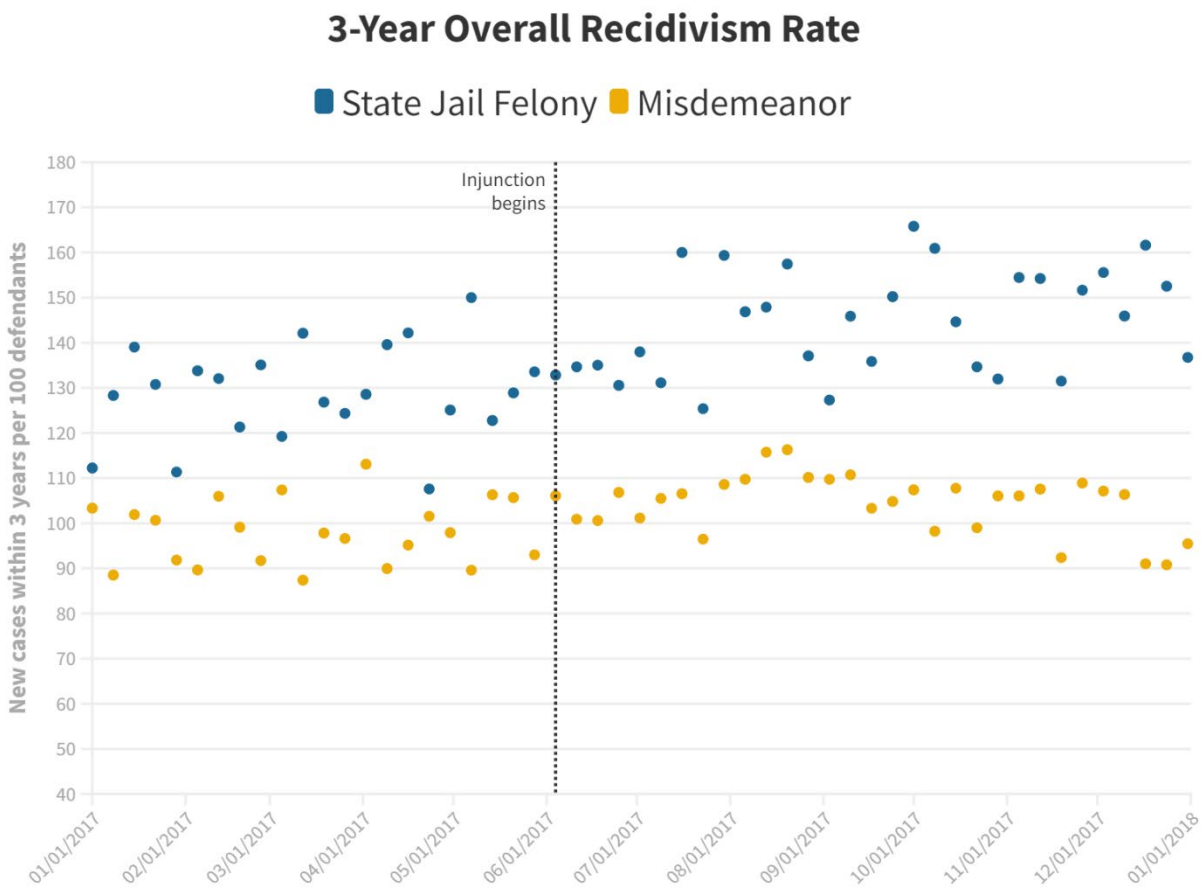
majority of cases (>95%) have resolved in some manner by mid-2022. There is also some indication of an uptick in case non-resolution for those charged with state jail felonies towards the later part of the year, possibly due to disruptions created by Hurricane Harvey.



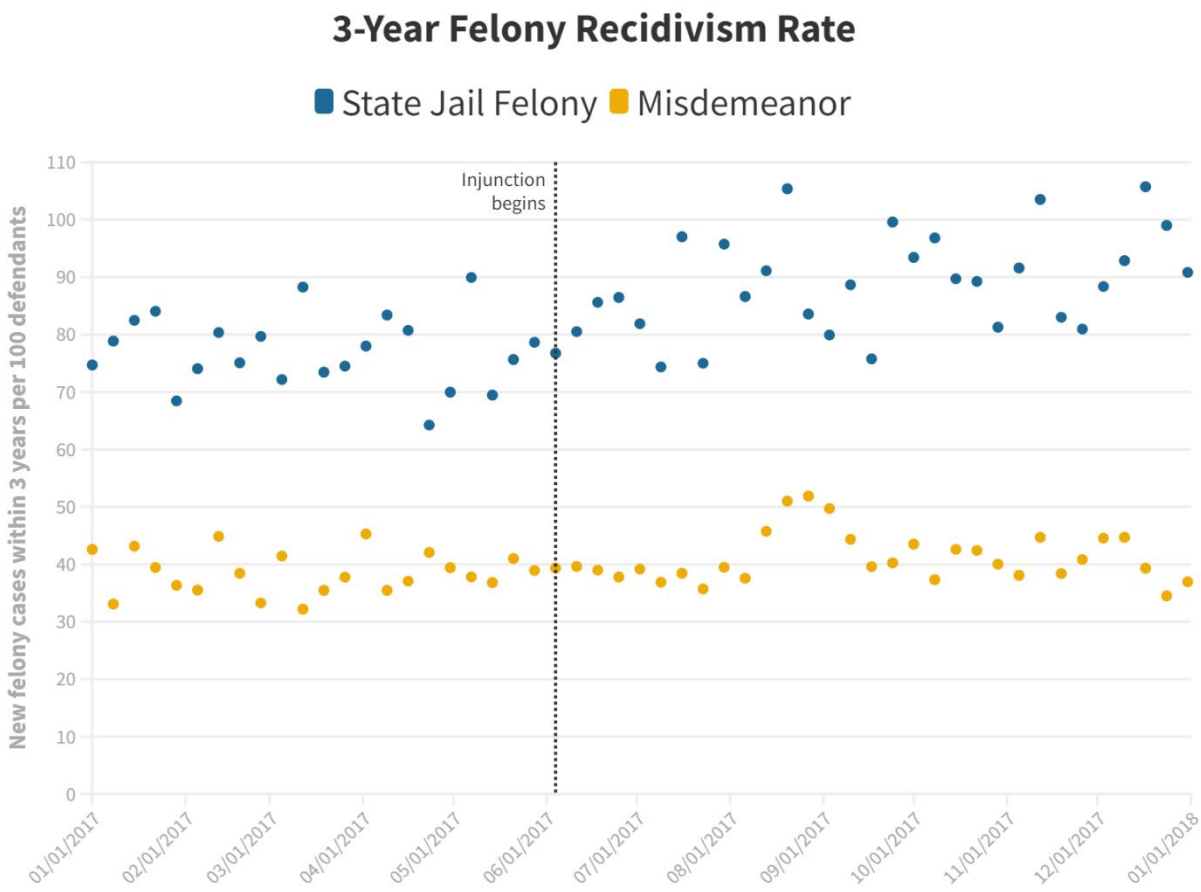
The other primary argument raised by proponents of pretrial detention is that it may prevent people from committing future offenses. To examine recidivism, we calculated the number of new cases filed for each defendant in a given number of years after their initial case filing. For example, the chart below plots three-year recidivism rates. Importantly, we measure recidivism from the time of the initial case filing, not the time of release or case resolution, which ensures that we have a comparable follow-up period for each defendant regardless of whether they are released or detained.

If liberalizing pretrial release leads to more crime, we would expect a level change or a shift in slope around the time of the injunction--as we observed above for convictions--as more people charged with misdemeanors are released. However, patterns of new cases seem quite stable across the period encompassing the onset of the injunction for misdemeanor defendants, while

if anything it appears that recidivism might be rising slightly for the comparison group. We see a similar pattern in the raw data when we look below at recidivism involving new felony cases.



Consistent with our discussion above, in our regression analysis, we do not condition on being released, as has been done in some past work on this topic, but instead compare different groups of defendants based upon the timing of their cases. Conditioning on release or presenting statistics specifically for the released population is inappropriate because the injunction changes the set of defendants who are included in this group. Thus, any patterns we see over time in outcomes specifically for people who are released confound any casual effects of the policy change with changes in the underlying composition of the released population.



Overall, the patterns above seem to suggest that the injunction may have increased release rates, diminished conviction rates and the incidence of custodial sentences, and increased rates of case non-resolution. Effects on sentence length appear somewhat ambiguous. The raw data do not suggest that the injunction increased crime.

5.Main Results

In the tables below, we report estimates from our differences-in-differences regressions designed to measure the effects of the injunction. Because these regressions control for a range of contextual variables, they are designed to measure the effects of the injunction after accounting for changes over time in the types of defendants and cases that show up in the system. To aid in interpreting the magnitudes of the estimates, in each table we also report, for a given outcome, the average of that outcome for people charged with misdemeanors in the months immediately preceding the onset of the injunction.

Pretrial Release

Impacts on Pretrial Release

Outcome	Pre-injunction Mean	Estimated Impact of Injunction
Released within 24 hours	.541	**.068***** (.008)
Released within 72 hours	.745	**.032***** (.007)
Hours of pretrial detention	272	**-45.5*** (25.9)

Statistically significant effect at * 10% level ** 5% level *** 1% level. In this and subsequent tables standard errors for the estimates clustered on offense type by case filing date are reported in parentheses. Sample size is 55,792.

The table above reports results for three different outcome measures capturing different aspects of pretrial release--the share of defendants released within 24 hours after booking, the share released within 72 hours, and the total number of hours of pretrial detention. Prior to the injunction, 54% of defendants charged with misdemeanors were released within 24 hours; the regression estimate indicates that the injunction increased the release rate by 6.8 percentage points (13%), a difference that is highly statistically significant. As a reference, this equates to the release of about 1,500 additional defendants just in the 6-month period following the injunction. We also observe

statistically significant, albeit smaller, increases in the share of defendants released within 72 hours.

Because we observe booking times, release times, and case disposition times, we can also calculate the length of the pretrial detention spell, which is the time between booking and either initial release or case disposition, whichever is earliest. On average, people charged with misdemeanors spent 272 hours (11 days) in pretrial detention prior to the injunction, but the injunction reduced this average by 45 hours (17%). Expanding pretrial release thus measurably and appreciably reduced utilization of the local jail, an important effect in light of ongoing concerns regarding jail overcrowding in Harris County (and many other jurisdictions). Overall, it is clear that the injunction increased release rates as intended, although it is also notable that a non-negligible share of people charged with misdemeanors continued to be detained for more than 72 hours even after the injunction went into effect.

Although informative, none of the pretrial measures above fully captures how patterns of pretrial detention are affected by the injunction. For example, from the perspective of a defendant, shifting from a week of pretrial detention down to 36 hours might have appreciable benefits in terms of one's ability to maintain employment, care for children, etc., but such a change would be unmeasured when using release within 24 hours as an outcome.

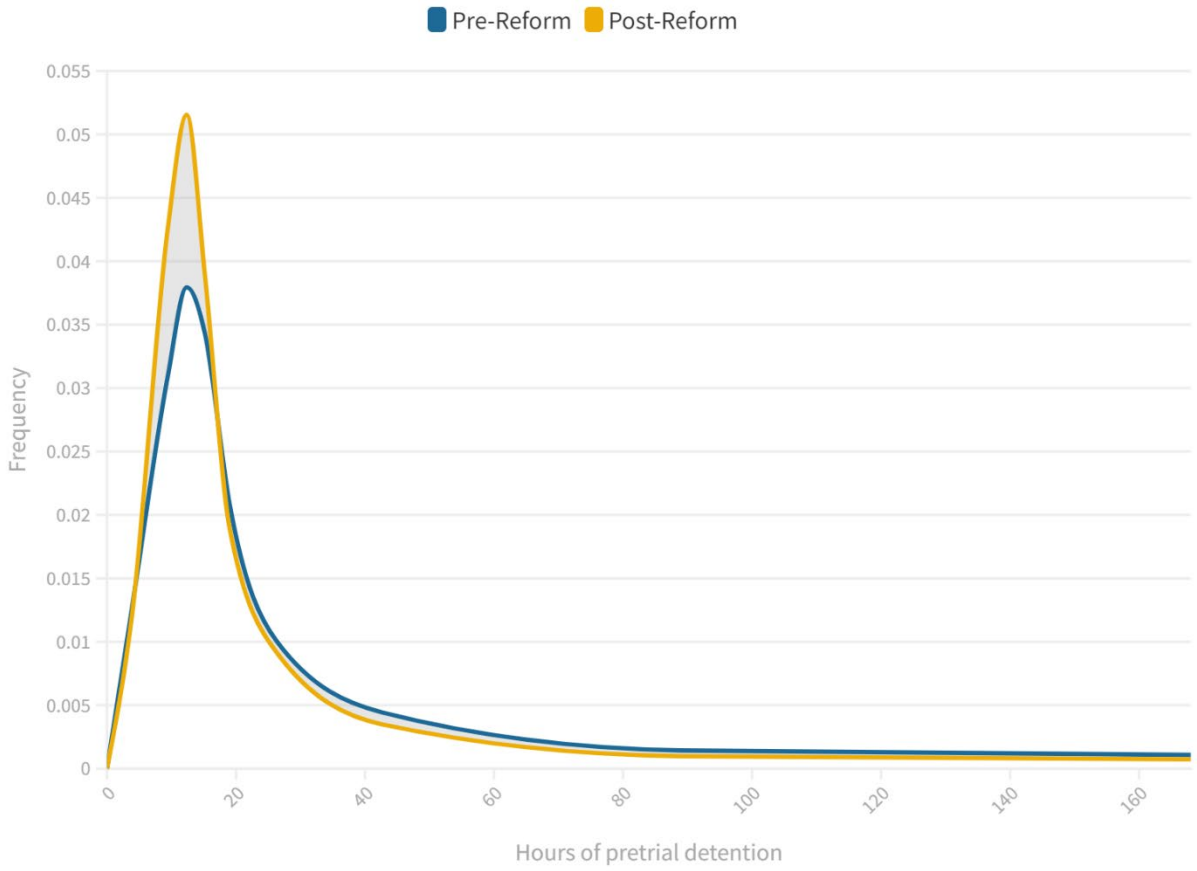
To provide a more comprehensive account of how the injunction impacted pretrial detention, we also considered how the injunction affected the distribution of pretrial detention time. In the figure below, the blue line plots the distribution that existed prior to the injunction; the horizontal axis shows hours in pretrial detention, so this chart tells us the share of the misdemeanor defendant population that spent various amounts of time in pretrial custody. The figure demonstrates that a substantial fraction of defendants were released within 24 hours, likely because they were assigned low bond amounts by the bail schedule and were able to quickly post cash bail. However, there is a long right tail on the distribution representing a smaller number of people held substantially longer than a few days.

How Did the Reform Affect Time Spent in Pretrial Detention?

To assess how the injunction impacted this distribution, we estimated a series of regressions similar to those described previously, but taking as outcome variables whether the individual was released within 5 hours, 10 hours, 15 hours, etc. The estimated coefficients in these regressions tell us how the distribution at a particular point shifted after controlling for underlying changes in defendant and case characteristics. The yellow distribution plots

the new distribution implied by these coefficient estimates, permitting us to see how the distribution shifts with the injunction.

Change in the Amount of Time Detained Pretrial



There is considerably more mass in the area of the distribution below 20 hours following the injunction, meaning that many individuals are shifted from spending longer than 20 hours in pretrial detention to fewer than 20 hours. Integrating across the two distributions offers one way to estimate the share of misdemeanor defendants shifted to earlier release as a result of the injunction; here by that metric roughly 12% of defendants benefit from the injunction by being released earlier.

Case Outcomes

In the table below we report impacts on case outcomes. We consider three mutually exclusive outcomes based on the initial disposition of the case--conviction, diversion, and acquittal or dismissal. Prior to the injunction, 62% of misdemeanor defendants were convicted of at least one charge in their case, but the injunction reduced conviction rates by 9.4 percentage points (15%). Nearly all of this change can be explained by a reduction in guilty pleas. We observe a statistically significant and practically large increase in the likelihood of diversion and acquittal/dismissal associated with exposure to the injunction.

One widely-recognized concern with pretrial detention for misdemeanor cases is that, when expected jail sentences are short, pretrial detention can result in a individual essentially "pre-serving" their entire jail sentence, creating strong incentives to plead guilty to a time served sentence even if innocent, as doing so actually shortens the amount of time spent in custody relative to going to trial. The large reductions in guilty pleas observed post-injunction suggest that liberalizing release can help to counteract such forces and reduce the likelihood that innocent people plead out to offenses they did not commit. Of course, it is almost certainly also the case that some of the increase in dismissals reflect individuals who were factually guilty; a key empirical question then becomes to what extent did these "lost" convictions frustrate public safety objectives by increasing future contact with the criminal system, an issue we turn to below.

Impacts on Case Resolution

Outcome	Pre-injunction Mean	Estimated Impact of Injunction
Convicted	.619	**-.094***** (.008)
Diverted	.070	**.025***** (.006)
Acquitted/dismissed	.310	**.068***** (.008)
Case unresolved	.009	**.008***** (.002)
Pled guilty	.613	**-.093***** (.007)

Statistically significant effect at * 10% level ** 5% level *** 1% level

Although diversion is a relatively infrequent outcome, we also observe appreciable increases in diversion following the injunction. Is it plausible to think that increasing release rates might impact rates of diversion, given that the diversion process is largely governed by prosecutor discretion? Even if prosecutors do not consider pretrial detention at all in assessing eligibility for diversion, there may be some defendants who, if faced with pretrial detention, choose to plead guilty immediately and accept jail time rather than explore alternative sanctions with their attorney. Relaxing eligibility for pretrial detention would permit such defendants to pursue alternatives to a quick guilty plea. Thus, one still might observe measurable changes in diversion stemming from defendant behavior even if diversion eligibility does not depend directly on detention status.

We note that some of the impacts shown here are large compared to those identified in prior causal studies--for example, Heaton, Mayson, and Stevenson (2017) find that pretrial detention increased the likelihood of conviction by 25%, which, if applied to the roughly 12% of the defendant population apparently shifted to a shorter pretrial stay, would imply a coefficient in the above regression of around -.02, much smaller than the actual coefficient. Although the explanation for this difference in magnitudes is uncertain, one possibility is that pretrial release may have had a larger impact in the time period we study here, perhaps because defendants now had representation at the initial hearing. Another possibility is that there may have been spillover effects of the injunction on defendants not directly impacted by the injunction.

We also observe an increase in the likelihood that a case has not been resolved as of 2022 associated with the injunction. The estimate, while positive and highly statistically significant, is small in magnitude, implying that the injunction prevents resolution of .8% of the cases in which people are charged with misdemeanors. To put this in perspective, we noted above in the distributional analysis that approximately 12% of the misdemeanor defendant population appears to have experienced an earlier release as a result of the injunction; if we use that as a measure of the population at risk, the implied failure rate of increasing pretrial release is around 7%. As a comparison, the Pretrial Services Agency for the District of Columbia, a federal agency that oversees what is widely viewed as a high-performing pretrial supervision system that couples high release rates with comparatively low rates of nonappearance and pretrial crime, reported a nonappearance rate of 12% in FY2017.

Sentencing

The table below reports the estimated impacts on sentencing outcomes. We measure sentence length based on the number of jail days recorded in our administrative data, and continue to include all relevant defendants in the analysis, so the sample includes individuals who were not sentenced to jail and therefore had sentences of zero.

Impacts on Sentencing

Outcome	Pre-injunction Mean	Estimated Impact of Injunction
Sentenced to jail	.633	**-.107***** (.007)
Sentenced to probation	.014	**.008***** (.002)
Length of jail sentence (days)	53.7	**-7.98**** (3.84)

Statistically significant effect at * 10% level ** 5% level *** 1% level

The injunction produced a statistically significant and practically large decrease in the likelihood of receiving a jail sentence in the case. The estimated decrease of 10.7 percentage points represents a 17% decline in the likelihood of a jail sentence. We also observe a statistically significant, albeit small, increase in the likelihood of a probation sentence. The injunction also reduced sentence lengths by an average of about 8 days, or 15%. As with conviction rates, these impacts are large compared to estimates in prior studies.

Recidivism

To evaluate the impact of the injunction on future criminal system involvement, we next estimate regressions where the outcome is the number of new criminal cases involving a particular defendant filed in Harris County within the next 1 or 3 years following the filing of the initial focal case. We separately consider felony cases and cases of any level of seriousness. We note that the fact that the calendar dates of the follow-up period differ across defendants does not compromise the analysis--because all of our analyses include a full set of case filing date fixed effects as controls, we are in essence

only comparing defendants with other defendants whose cases are filed on the same day, and who therefore have the same follow-up period.

We refer to outcomes based on these metrics as capturing "recidivism", recognizing that as with any potential recidivism measure, there are important limitations. By counting new case filings rather than new convictions, we are attributing to defendants some "crimes" for which they are legally innocent, and thus this metric is over-inclusive. It is also under-inclusive, in that it only counts cases filed within Harris County, and excludes criminal acts that do not result in criminal charges (e.g. some crimes that occur when individuals are in custody). However, one advantage of using this recidivism metric is that it measures future contact with the system, and minimizing defendants' future contact with the criminal system is a shared goal of both those who prioritize protecting accused people from harms imposed on them by the criminal legal system and those who prioritize protecting the general public from criminal actors. Moreover, the adjudication process itself imposes costs on taxpayers, the public, and those accused of crimes even when the individual is ultimately not convicted. Including such episodes within the ambit of our recidivism metric ensures that we don't ignore those costs.

Impacts on Future Criminal Justice System Contact

Outcome	Pre-injunction Mean	Estimated Impact of Injunction	Implied % Change
New cases within 1 year	.525	-.016 (.016)	-3.0%
New felony cases within 1 year	.181	-.003 (.009)	-1.6%
New cases within 3 years	1.14	**-.069**** (.027)	**-6.1%**
New felony cases within 3 years	.448	**-.038**** (.018)	**-8.5%**

Statistically significant effect at * 10% level ** 5% level *** 1% level

Looking at the first year after the case is filed, we observe negative point estimates for both all new cases and new felony cases. The estimates, while not statistically significant, are sufficiently precise so as to rule out small increases in crime due to the injunction. For example, we can statistically reject an

increase in new felony cases of more than 8%. When we look out to 3 years post-case filing, we observe modest, negative, and statistically significant decreases in overall crime associated with exposure to the injunction. This pattern is consistent with other research on pretrial detention that finds that detention might yield some short-run reduction in new contact with the system through incapacitation, but that such impacts attenuate or even reverse as one considers a longer time horizon.

In an analysis not reported in the table, we also replicated the recidivism analysis using all new cases up through May 2022, the current vintage of our data. This allows for a follow-up period of nearly 4½ years for individuals with cases filed near the end of our sample period (December 2017), and more than 5 years for those from the earlier parts of the sample. We obtain very similar results to those shown above for the 3-year follow-up period. Thus, the finding here that the injunction decreased crime over the longer run replicates through the most current available data.

How Widespread Were the Impacts of the Injunction?

Were the impacts documented above concentrated among one particular segment of the defendant population, or did the injunction widely affect defendants from a range of backgrounds? To examine this question, we re-estimated our main specifications for subsamples of our baseline sample categorized by sex, race/ethnicity, age, poverty status, and prior criminal history. Because we don't observe any poverty status information at the individual level, we construct a rough proxy by grouping defendants based upon whether they reside in a ZIP code with a poverty rate above 16%, a cutoff that represents 150% of the U.S. national average poverty rate. As noted previously, our criminal history variable is also limited by the fact that we only observe criminal history within Harris County across a finite lookback period, meaning that there may be some individuals labeled as having no history here who would in fact have a criminal history based on more comprehensive records.

Comparison of Impacts Across Different Population Segments

Specification	Released Within 24 Hours	Released Within 72 Hours	Convicted	Diverted
Overall (N=55792)	.068***	.031***	-.097***	.025***
	(.008)	(.007)	(.008)	(.006)
Male (N=44051)	.076***	.038***	-.101***	.030***
	(.009)	(.008)	(.008)	(.006)
Female (N=11575)	.047***	.013	-.085***	.015
	(.016)	(.015)	(.016)	(.014)
White (N=7323)	.075***	.031*	-.093***	.027*
	(.019)	(.017)	(.019)	(.014)
Black (N=15755)	.112***	.049***	-.127***	.031***
	(.013)	(.011)	(.012)	(.009)
Hispanic (N=10490)	.021	-.010	-.061***	.032**
	(.020)	(.020)	(.020)	(.015)
Age 25 and under (N=17294)	.086***	.073***	-.082***	.026*
	(.017)	(.016)	(.017)	(.015)
Age 26-40 (N=25298)	.054***	.014	-.097***	.032***
	(.011)	(.011)	(.010)	(.007)
Over age 40 (N=12908)	.098***	.045***	-.106***	.008
	(.013)	(.014)	(.014)	(.011)
Lower poverty ZIP (N=21159)	.030**	.007	-.090***	.042***
	(.013)	(.012)	(.012)	(.010)
Higher poverty ZIP (N=28211)	.079***	.034***	-.094***	.013
	(.010)	(.010)	(.010)	(.009)
No priors (N=30513)	.013	-.035***	-.069***	.033***
	(.013)	(.012)	(.011)	(.011)
Has priors (N=25157)	.126***	.096***	-.131***	.019***
	(.010)	(.009)	(.011)	(.006)

Statistically significant effect at * 10% level ** 5% level *** 1% level

Specification	Dismissed/ Acquittal	Unresolved	Pled Guilty	Sentenced to Jail
Overall	.072***	.008***	-.095***	-.111***
	(.008)	(.002)	(.007)	(.007)
Male	.071***	.008***	-.098***	-.115***
	(.009)	(.002)	(.008)	(.008)
Female	.071***	.007	-.086***	-.092***
	(.017)	(.004)	(.016)	(.015)
White	.065***	.001	-.090***	-.126***
	(.018)	(.004)	(.019)	(.019)
Black	.096***	.002	-.127***	-.134***
	(.013)	(.002)	(.012)	(.012)
Hispanic	.027	.014**	-.055***	-.059***
	(.021)	(.006)	(.021)	(.021)
Age 25 and under	.056***	-.001	-.079***	-.114***
	(.017)	(.005)	(.017)	(.015)
Age 26-40	.064***	.013***	-.095***	-.099***
	(.011)	(.003)	(.010)	(.010)
Over age 40	.098***	.004	-.106***	-.124***
	(.014)	(.004)	(.014)	(.013)
Lower poverty ZIP	.047***	.005	-.088***	-.097***
	(.013)	(.003)	(.012)	(.012)
Higher poverty ZIP	.081***	.010***	-.092***	-.107***
	(.011)	(.003)	(.010)	(.010)
No priors	.035***	.010**	-.068***	-.086***
	(.011)	(.004)	(.011)	(.011)
Has priors	.111***	.003*	-.128***	-.144***
	(.010)	(.002)	(.010)	(.010)

Statistically significant effect at * 10% level ** 5% level *** 1% level

Specification	Sentenced to Probation	Jail Sentence Length (Days)	New Cases Within 1 Year	New Felony Cases Within 1 Year
Overall	.008**	-6.90*	-.017	-.003
	(.002)	(3.41)	(.015)	(.010)
Male	.005*	-7.63	-.024	-.001
	(.002)	(4.24)	(.019)	(.012)
Female	.017**	-2.01	.004	-.008
	(.006)	(5.26)	(.026)	(.017)
White	.001	-14.73	-.024	.007
	(.006)	(7.58)	(.040)	(.027)
Black	.009*	-0.18	-.025	.013
	.004	(5.83)	.032	.016
Hispanic	-.005	1.42	-.047	-.005
	(.007)	(8.55)	(.039)	(.025)
Age 25 and under	.001	-13.87**	.015	.006
	(.006)	(4.58)	(.035)	(.023)
Age 26-40	.002	-11.87*	-.008	-.005
	(.003)	(5.64)	(.023)	(.014)
Over age 40	.019***	4.54	-.056	.001
	(.005)	(6.40)	(.029)	(.017)
Lower poverty ZIP	.014**	-3.03	-.011	-.003
	(.005)	(5.28)	(.025)	(.017)
Higher poverty ZIP	.005	-5.92	.004	.004
	(.003)	(5.10)	(.023)	(.013)
No priors	.013**	-4.14	-.009	-.005
	(.005)	(4.99)	(.018)	(.012)
Has priors	.002	-7.17	-.033	-.001
	(.002)	(4.48)	(.025)	(.014)

Statistically significant effect at * 10% level ** 5% level *** 1% level

Specification	New Cases Within 3 Years	New Felony Cases Within 3 Years
Overall	-.069*	-.038*
	(.028)	(.018)
Male	-.074*	-.036
	(.034)	(.022)
Female	-.035	-.019
	(.049)	(.033)
White	-.040	.051
	(.070)	(.044)
Black	-.131*	-.023
	(.056)	(.033)
Hispanic	-.084	-.055
	(.076)	(.047)
Age 25 and under	.071	.021
	(.061)	(.039)
Age 26-40	-.114*	-.073**
	(.045)	(.028)
Over age 40	-.128**	-.022
	(.049)	(.032)
Lower poverty ZIP	.019	-.001
	(.046)	(.031)
Higher poverty ZIP	-.048	-.038
	(.042)	(.024)
No priors	.005	-.003
	(.033)	(.021)
Has priors	-.154***	-.063*
	(.045)	(.027)

Statistically significant effect at * 10% level ** 5% level *** 1% level

We do not observe large differences in the effects of the injunction across groups defined by sex, age, and ZIP code poverty status. For race/ethnicity, there is suggestive evidence that Black defendants may have benefited more from the injunction than White defendants, while Hispanic defendants benefited less. For example, after adjusting for other contextual factors, an additional 11% of Black defendants were released within 24 hours under the injunction, versus 8% of White defendants and 2% of Hispanic defendants. When we look at conviction rates, we see these decreased by 13 percentage

points for Black defendants, versus 9 percentage points for White defendants and 6 percentage points for Hispanic defendants. Although the injunction ultimately led to more lenient case outcomes for all groups, the precise reasons the injunction apparently had less of a beneficial impact on Hispanic defendants remain unclear and merit further investigation.

Similarly, when we compare those with and without prior criminal records, we see that those with prior criminal records appear to have benefited most from the injunction, experiencing a 13 percentage point (versus 1 percentage point) increase in the likelihood of release within 24 hours, a 13 percentage point (versus 7 percentage point) decline in the likelihood of conviction, and a 14 percentage point (versus 9 percentage point) decrease in the likelihood of receiving a jail sentence. One plausible explanation for these patterns is that those with no prior history received the lowest bail amounts under the schedule that existed prior to the injunction, and thus may have been less well-positioned to benefit from the injunction. Apparently the injunction was particularly effective at releasing those who would have been assigned higher cash bail amounts due to their more extensive criminal histories.

We do not observe statistically significant increases in recidivism for any subgroup considered in this analysis across any of the four recidivism metrics we consider. There are statistically significant decreases in crime after 3 years for several subgroups, including those ages 26-40 and those with prior criminal records.

6.Robustness Checks

In this section, we consider the robustness of our main findings to alternative approaches to the statistical analysis. Overall, we find our primary conclusions that liberalizing pretrial release reduced plea and conviction rates, modesty increased case non-resolution, and did not increase future crime to be robust.

Comparison Group

Our statistical analysis uses defendants charged with state jail felonies as a comparison group to establish what would have happened to outcomes absent a change in rules governing pretrial release. Of course, there are differences in case handling between felony and misdemeanor cases, and although the differences-in-differences methodology accounts for differences that are stable over time, it seems at least possible that there may have been changes in case handling for certain types of felonies in 2017. If there were unaccounted-for changes to how felonies were adjudicated, this could affect the accuracy of our estimates.

In the table below, we report results for regressions similar to the baseline, but considering a range of alternative comparison groups. The bars depict 95% confidence intervals for each estimate. The comparison groups are as follows:

1. Baseline (for reference)
2. We estimate a specification with no comparison group, so we limit the analysis to individuals charged with misdemeanors and compare cases filed after the injunction with cases filed before. This approach avoids contrasts between misdemeanors and felonies, categories of offenses that some may view as non-comparable. However, as with any before-after analysis, it has a considerable drawback in that any uncontrolled changes over time in how cases are handled (e.g. due to Hurricane Harvey) will be improperly attributed to bail reform.
3. We limit the analysis to defendants charged with misdemeanors, and contrast people charged with offenses less likely to be impacted to the injunction with other misdemeanor offenses. Relative to the baseline, this approach also has the advantage of eschewing misdemeanor/felony comparisons. A drawback of this approach is that the injunction could have theoretically impacted all categories of misdemeanors, including the "control" offenses, meaning that we might understate the impacts of the injunction when we focus only on people charged with

misdemeanors. (See Appendix A for further information about this analysis.)

4. We extend the analysis to include all felonies--not just state jail felonies--which increases the sample size, but with the potential downside of drawing comparisons between offenses of fairly different levels of severity.
5. We identify categories of felonies with higher than average pretrial release rates prior to the injunction, and use those as a comparison group. Defining the control group in this manner may create greater comparability in pretrial handling of cases, because we are essentially comparing felonies for which a sizeable fraction of defendants are released pretrial to misdemeanors that also have higher release rates.
6. We drop DWI and controlled substances cases from the analysis, as these categories of cases can sometimes go through special procedures that have changed over time, and may therefore be less appropriate as a treatment or control group.
7. As a final check, we conduct a placebo analysis wherein we replicate our baseline, but using only data for 2015 and assuming a placebo reform date of June 7, 2015. Since there was no injunction in 2015, if state jail felonies properly capture external, unobserved factors that shift outcomes over time, we should observe statistically insignificant "impacts" in these specifications. The placebo analysis thus offers a sort of diagnostic check to see whether state jail felonies appear to furnish a reasonable comparison group for misdemeanors.

Although the magnitudes of estimated impacts vary somewhat across samples, the conclusions from the baseline analysis are largely supported in these robustness checks. For example, all of the non-placebo samples yield a statistically significant and practically important decrease in convictions and likelihood of a jail sentence from the injunction, and none of the specifications yield evidence of statistically significant increases in crime. Conclusions regarding diversion are somewhat sensitive to the choice of control group, and the estimates for sentence length become fairly imprecise when including more serious felonies--which tend to exhibit much more variation in sentencing--in the comparison group.

Estimated Impacts Using Different Comparison Groups

Specification	Released Within 24 Hours	Released Within 72 Hours	Convicted	Diverted
Baseline (N=55792)	.068***	.031***	-.097***	.025***
	(.008)	(.007)	(.008)	(.006)
No comparison group (N=42872)	.161***	.109***	-.080***	-.002
	(.007)	(.005)	(.005)	(.003)
Misdemeanors only (N=42872)	.088***	.056***	-.028***	-.006
	(.009)	(.007)	(.009)	(.004)
All felonies as control (N=70018)	.098***	.048***	-.078***	.015***
	(.006)	(.005)	(.005)	(.004)
High release felonies as control (N=48441)	.095***	.044***	-.073***	.032***
	(.012)	(.011)	(.011)	(.011)
Drop DWI/drug cases (N=38177)	.103***	.059***	-.092***	.022***
	(.011)	(.010)	(.009)	(.008)
Placebo (N=54154)	-.013	-.009	.005	-.003
	(.009)	(.008)	(.008)	(.006)

Specification	Dismissed/ Acquittal	Unresolved	Pled Guilty	Sentenced to Jail
Baseline	.072***	.008***	-.095***	-.111***
	(.008)	(.002)	(.007)	(.007)
No comparison group	.082***	.016***	-.080***	-.084***
	(.005)	(.001)	(.005)	(.005)
Misdemeanors only	.035***	-.010***	-.028***	-.032***
	(.009)	(.003)	(.009)	(.009)
All felonies as control	.063***	.006***	-.078***	-.080***
	(.005)	(.002)	(.005)	(.005)
High release felonies as control	.040***	.006	-.071***	-.068***
	(.012)	(.004)	(.010)	(.011)
Drop DWI/drug cases	.070***	.008***	-.091***	-.130***
	(.009)	(.003)	(.009)	(.010)
Placebo	-.002	.011***	.002	-.049***
	(.007)	(.001)	(.008)	(.008)

Statistically significant effect at * 10% level ** 5% level *** 1% level

Specification	Sentenced to Probation	Jail Sentence Length (Days)	New Cases Within 1 Year	New Felony Cases Within 1 Year
Baseline	.008**	-6.90*	-.017	-.003
	(.002)	(3.41)	(.015)	(.010)
No comparison group	.002	-4.65***	.013	.005
	(.001)	(0.97)	(.010)	(.005)
Misdemeanors only	.002	0.74	.015	-.001
	(.002)	(2.80)	(.017)	(.009)
All felonies as control	.003	15.29	.010	.002
	(.002)	(10.25)	(.010)	(.006)
High release felonies as control	-.002	19.17	.016	.000
	.006	(17.13)	.020	.012
Drop DWI/drug cases	.003	-10.48	-.066**	-.019
	(.003)	(5.47)	(.023)	(.015)
Placebo	-.005	-0.78	-.033*	-.004
	(.003)	(4.10)	(.015)	(.010)

Specification	New Cases Within 3 Years	New Felony Cases Within 3 Years
Baseline	-.069*	-.038*
	(.028)	(.018)
No comparison group	-.044*	-.007
	(.017)	(.009)
Misdemeanors only	-.002	.005
	(.033)	(.017)
All felonies as control	-.024	-.010
	(.018)	(.011)
High release felonies as control	.002	-.002
	(.035)	(.022)
Drop DWI/drug cases	-.164***	-.091**
	(.043)	(.028)
Placebo	-.045	.002
	(.032)	(.019)

Statistically significant effect at * 10% level ** 5% level *** 1% level

Sample and Specification

We also test the sensitivity of our findings to other logical changes to the sample or regression specification. In the table below, we consider the following alternative specifications:

1. Baseline (for reference)
2. We limit the analysis to cases filed between April and August 2017. This approach presumably helps to ensure that the treated and comparison groups are as similar to one another as possible, because all the cases occur within a few months of each other. It also limits the sample to cases commenced prior to Hurricane Harvey. However, the smaller sample size reduces the statistical power for detecting impacts of the injunction, and this approach only detects immediate impacts of the injunction, which may be less informative if some effects developed over time.
3. We consider a wider time window, from 2016 through 2018. This approach gives us a larger sample size. However, as we expand the time window for the analysis further away from the injunction, it becomes more likely that other external factors (e.g. a new DA taking office in 2017) that differentially impact felony and misdemeanor cases may have been operational, which might bias these estimates.
4. We use a very simple statistical specification that excludes all demographic and other control variables, and simply controls for case type (i.e. felony or misdemeanor) and date of case filing. This allows us to gauge the sensitivity of our conclusions to the choice of control variables.
5. We allow for the possibility that outcomes might be trending differently over time for misdemeanor and felony cases.
6. We omit cases involving defendants with ICE holds. ICE holds clearly affect eligibility for release, but in theory, the choice to request holds by ICE could be responsive to local conditions and policy developments, so it is unclear whether it is appropriate to control for holds in the analysis. This specification examines whether these defendants are essential to our main findings.
7. We limit the analysis to cases that were filed within one day of when the booking occurred. This focuses attention on "new" cases, many of which likely resulted from on-view arrests.

Estimated Impacts Using Different Samples and Specifications

Specification	Released Within 24 Hours	Released Within 72 Hours	Convicted	Diverted
Baseline (N=55792)	.068***	.031***	-.097***	.025***
	(.008)	(.007)	(.008)	(.006)
Shorten sample to Apr.-Jul 2017 (N=23982)	.089***	.049***	-.066***	.020**
	(.013)	(.012)	(.013)	(.009)
Lengthen sample to 2016-2018	.008	-.030***	-.092***	-.008**
(N=172,458)	(.005)	(.005)	(.005)	(.004)
No controls (N=55839)	.064***	.029***	-.107***	.027***
	(.008)	(.008)	(.009)	(.006)
Include time trends (N=55792)	.106***	.057***	-.047***	.028***
	(.017)	(.014)	(.016)	(.011)
Drop ICE holds (N=52692)	.074***	.028***	-.099***	.024***
	(.008)	(.007)	(.008)	(.006)
Cases filed close to arrest (N=44625)	.090***	.042***	-.111***	.022***
	(.009)	(.008)	(.009)	(.007)

Specification	Dismissed/ Acquittal	Unresolved	Pled Guilty	Sentenced to Jail
Baseline	.072***	.008***	-.095***	-.111***
	(.008)	(.002)	(.007)	(.007)
Shorten sample to Apr.-Jul 2017	.045***	.008***	-.064***	-.083***
	(.013)	(.003)	(.012)	(.012)
Lengthen sample to 2016-2018	.100***	.001	-.089***	-.158***
	(.005)	(.001)	(.005)	(.005)
No controls	.079***	.009***	-.106***	-.108***
	(.009)	(.002)	(.009)	(.008)
Include time trends	.019	.007**	-.045***	-.073***
	(.016)	(.003)	(.015)	(.014)
Drop ICE holds	.075***	.004**	-.097***	-.114***
	(.008)	(.002)	(.008)	(.007)
Cases filed close to arrest	.088***	.009***	-.109***	-.128***
	(.009)	(.002)	(.009)	(.009)

Statistically significant effect at * 10% level ** 5% level *** 1% level

Specification	Sentenced to Probation	Jail Sentence Length (Days)	New Cases Within 1 Year	New Felony Cases Within 1 Year
Baseline	.008**	-6.90*	-.017	-.003
	(.002)	(3.41)	(.015)	(.010)
Shorten sample to Apr.-Jul 2017	.013***	-2.08	-.017	-.006
	(.003)	(5.08)	(.025)	(.015)
Lengthen sample to 2016-2018	.000	-12.95***	-.014	.006
	(.002)	(2.06)	(.009)	(.006)
No controls	.008***	-9.03*	-.023	-.028**
	(.002)	(3.94)	(.017)	(.010)
Include time trends	.019***	0.89	-.036	.000
	.004	(6.02)	.030	.019
Drop ICE holds	.008**	-7.45*	-.018	-.002
	(.002)	(3.50)	(.016)	(.010)
Cases filed close to arrest	.007*	-7.87*	-.024	-.002
	(.003)	(3.61)	(.018)	(.011)

Specification	New Cases Within 3 Years	New Felony Cases Within 3 Years
Baseline	-.069*	-.038*
	(.028)	(.018)
Shorten sample to Apr.-Jul 2017	-.021	-.035
	(.043)	(.027)
Lengthen sample to 2016-2018	-.011	.029**
	(.017)	(.011)
No controls	-.102**	-.095***
	(.033)	(.020)
Include time trends	-.030	-.037
	(.053)	(.034)
Drop ICE holds	-.074*	-.039*
	(.029)	(.019)
Cases filed close to arrest	-.092**	-.043*
	(.034)	(.021)

Statistically significant effect at * 10% level ** 5% level *** 1% level

These different robustness checks largely yield evidence consistent with the baseline. For example, all specifications show a statistically significant decrease in the conviction rate and likelihood of a jail sentence, and none of the specifications shows patterns consistent with an increase in total charges within 1 or 3 years post-booking. Thus, the main results are robust across numerous logical specification changes.

Event Study Analysis

In Appendix B, we also plot coefficients from event-study analyses of key outcomes of interest. For the event study analysis, we estimate flexible versions of our main specification that show the evolution of outcomes both before and after the onset of the injunction. Examining patterns in the outcomes prior to the onset of the injunction provides evidence regarding the potential existence of uncontrolled factors that might bias our estimates of the effects of the injunction. Similarly, the event study allows us to see how well the timing of the change in a particular outcome corresponds to the onset of the injunction; to the extent that the timings of these events match well, this provides stronger evidence that we are capturing the causal effects of the injunction.

The event study evidence appears strongest for pretrial release, conviction, pleas, and probation sentences--for each of these outcomes, there is little evidence of any pre-injunction trend, and the outcome changes beginning at the onset of the injunction. For a few outcomes, the event study charts are corroborative but not completely unequivocal--for receiving a jail sentence and case non-resolution, for example, there is slight indication of a possible pre-injunction time trend, while for acquittals/dismissals the timing of the impacts appears slightly delayed relative to the onset of the injunction. For diversion and sentence length, the event study results suggest some caution in interpreting the baseline results, as there is no obvious abrupt change in these outcomes at the time of the injunction.

For new charges in the three years post-arrest, the event study figures contradict the hypothesis that the injunction increased crime--if that were the case, we would expect to see an increase in the effect estimate beginning at the time of the injunction, and no such increase is apparent. The figure appears somewhat equivocal as to whether the injunction reduces crime.

Taken as a whole, the results from these robustness tests suggest that many of the core findings from the main analysis are remarkably robust. The strongest and most robust findings are that injunction increased release rates, decreased conviction rates, and did not increase future crime. These conclusions do not rely on a particular choice of control group or sampling frame, and the timing of these impacts strongly suggest that they reflect the impacts of the injunction as opposed to other factors. Overall, there is also fairly good evidence that the injunction decreased the likelihood of a jail sentence, increased the likelihood of probation, and increased case non-resolution. There is some evidence the injunction reduced sentence length and increased diversion, but these findings are more sensitive to the sample and statistical specification.

7. Key Findings and Conclusions

Key Findings

1 Misdemeanor pretrial reform produced more lenient outcomes and reduced the system's imprint without adversely impacting public safety.

Our analysis largely confirms the findings of prior causal studies that reducing pretrial detention should reduce conviction rates and sentence severity without increasing future contact with the criminal justice system, at least in the medium to longer run. In the chart below, we compare the hypotheses presented previously based on existing high-quality research with key quantitative results from our study.

Predicted Effect		Actual Measured Effect
1. Reduce guilty pleas	→	Plea rates fell by 15%
2. Reduce conviction rates	→	9% of defendants avoided conviction
3. Reduce likelihood of a jail sentence	→	Likelihood of jail sentence fell by 17%
4. Reduce average sentence length	→	Average sentence length fell by 15%
5. Not increase or reduce future criminal justice system contact	→	No measurable increase in new cases within 1 year 6% decline in new cases over 3 years

Each of the primary hypotheses generated from prior work was confirmed in these data. The injunction served to increase pretrial release rates, lower conviction rates (plausibly including wrongful convictions), and generate

more lenient sentences. Our preferred estimates suggest that the reforms may have modestly decreased crime over a three-year lookback period, although the conclusion that the reform decreased crime is somewhat dependent on the sample and specification.

These findings should be of value and interest to people with a range of ideological views regarding the criminal system. For more right-leaning stakeholders who worry about the fiscal costs of corrections or overcriminalization, these results should be encouraging, as they identify a reform that reduced both front and back-end use of incarceration, and also reduced convictions that apparently were not contributing to future reductions in criminal activity. For more left-leaning stakeholders, this research demonstrates a feasible approach to reducing the imprint of the system and associated human costs, and the intervention seems to have had particularly large benefits for Black defendants and those coming from particularly poor neighborhoods.

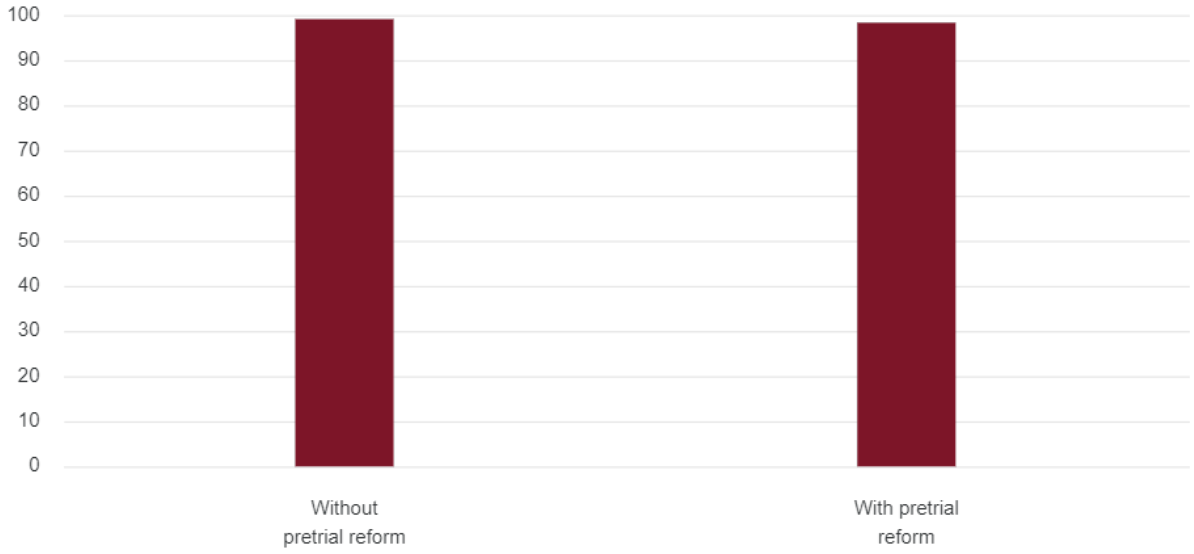
One aspect in which these results do differ somewhat from the prior literature is in the magnitude of the effects--for some outcomes (e.g. conviction rate), we observe larger effects than might have been expected based on past research. Understanding how the effects of pretrial release differ across different populations and community contexts remains an important opportunity for future research.

2 Increasing pretrial release rates did not substantially impede the resolution of cases, although there were some modest impacts.

A primary justification given for pretrial detention is that it prevents defendants from escaping accountability for alleged criminal acts by failing to show up at court. Of course, many defendants would show up for court even if released, and so an important parameter for calibrating pretrial policy is the share of defendants who would fail to appear if released. For example, a pretrial policy that incurs the costs of detaining 100 people in order to prevent 1 person from not showing up in court would seem less favorable than a policy that detains 10 people, all of whom would have failed to appear had they been released. The natural experiment created by the injunction furnishes an opportunity to evaluate this quantity.

We do observe a statistically significant increase in case non-resolution of a result of releasing more people charged with misdemeanors, but the overall impact appears small. As shown in the figure below, our estimates indicate that the injunction reduced case resolution rates from 99% to 98%.

Fraction of Cases Resolved Within ~5 Years



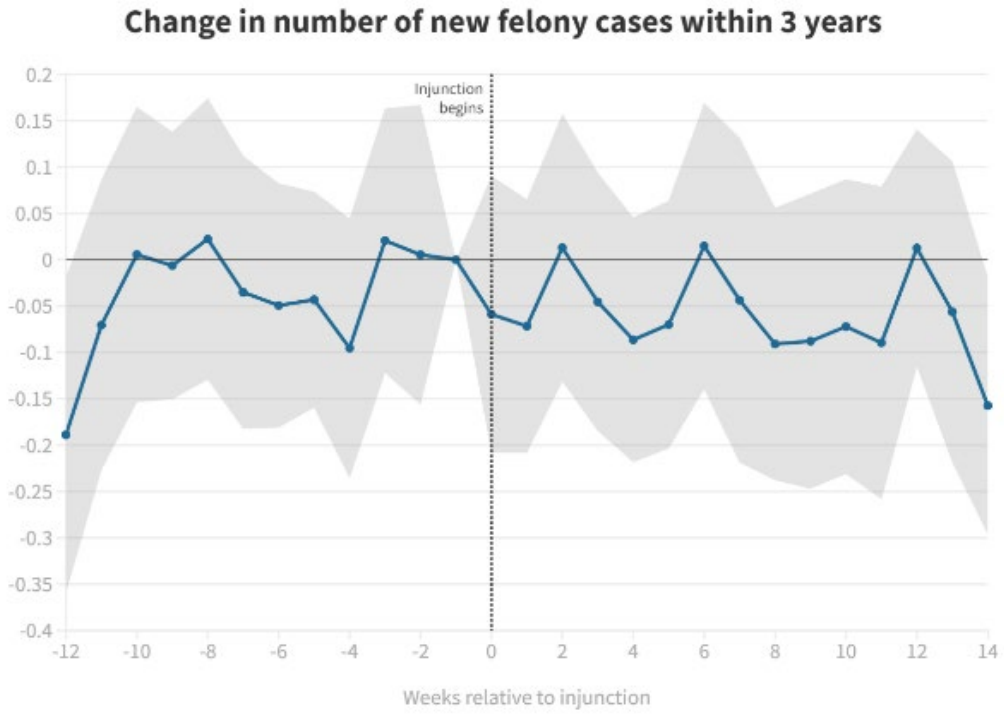
Another way of understanding these numbers is to ask: what fraction of people released earlier under the injunction subsequently went on to "escape justice" in that their cases were never fully adjudicated? Our results suggest this fraction is on the order of 10%, a non-negligible fraction but in line with other pretrial systems widely viewed as high-performing.

Of course, this analysis considers only one metric of interest--whether courts are able to record an official case disposition--and there are other relevant performance dimensions not considered here (e.g. total time to case disposition or number of court settings). As more nuanced data regarding court appearances and other steps in the process become available, we'll likely be able to gain further insights about the additional impacts of expanding pretrial release.

3 Expanding pretrial release under the ODonnell injunction did not fuel a spike in crime, as some have claimed.

Another concern raised by critics of liberalizing pretrial release is that released individuals might commit additional crimes in the community either during or after their period of pretrial release. Under this view, pretrial detention is necessary to incapacitate dangerous people, and expanding release will thus lead to more crime. Indeed, some commentators have attempted to tie misdemeanor bail reform in Harris County to recent upticks in certain categories of crime in the county.

Measuring the impact of pretrial reform on recidivism is difficult, because it requires comparing two pools of defendants otherwise similar in their latent propensity to commit future crime but differing in their exposure to pretrial reform. Prior analyses of misdemeanor bail reform in the county have largely failed to ensure equivalence across different pools of defendants being compared, rendering their conclusions potentially misleading. When we make a more careful effort to ensure we are contrasting similarly situated pools of defendants, we obtain unambiguous results that clearly show that the increase in release rates under the injunction was not associated with an increase in future crime.



Although we demonstrate this result a variety of ways and using a variety of recidivism metrics above, the event study figure above--which shows how rates of future felony cases evolve around the time of the injunction--provides one particularly clear illustration of this result. Despite a sizeable increase in the pretrial release rate, there is simply no evidence of an elevated rate of future felony offending post-injunction.

Conclusion

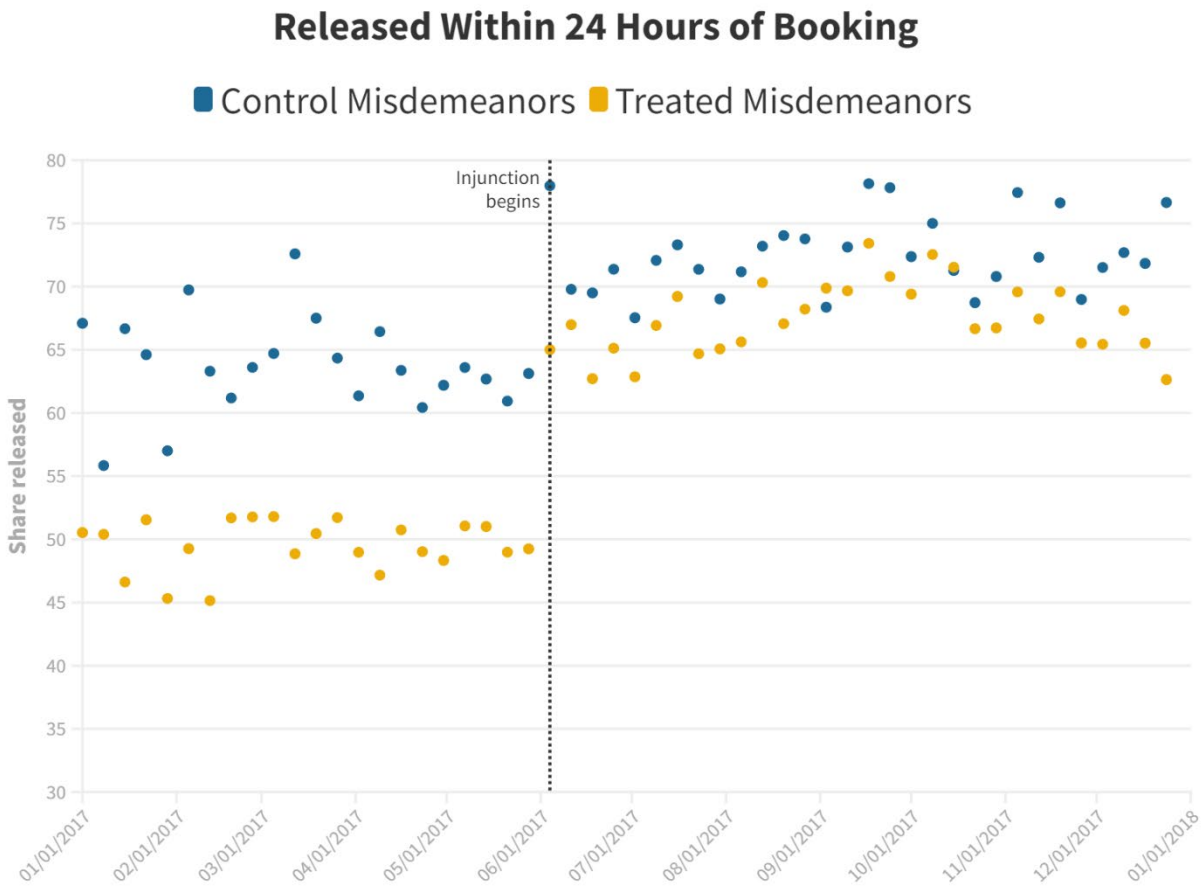
This analysis demonstrates that a core component of misdemeanor bail reform in Harris County--releasing people charged with misdemeanors on unsecured bail who might have otherwise been detained due to failure to post small amounts of cash bail--was largely a success. It also provides some of the first evidence that the results from existing causal studies on pretrial detention generalize to situations where a community implements a reform specifically designed to increase pretrial release rates. It should provide some assurance to other jurisdictions considering liberalizing pretrial release for people charged with low-level offenses that it is possible to do so in a manner that reduces the costs and imprint of the criminal justice system while not adversely impacting public safety.

Of course, this analysis considers only one aspect of a larger pretrial system. Stakeholders in Harris County and elsewhere continue to consider the merits of other changes to pretrial policy. For example, there are ongoing debates about current practices governing pretrial release of people charged with felonies and improving court backlogs (which increase the costs of pretrial detention) that are not addressed by this research. Although this study does not speak directly to such issues, it does demonstrate the importance of data collection and careful, rigorous analysis to demonstrate which pretrial reforms are working and in what ways.

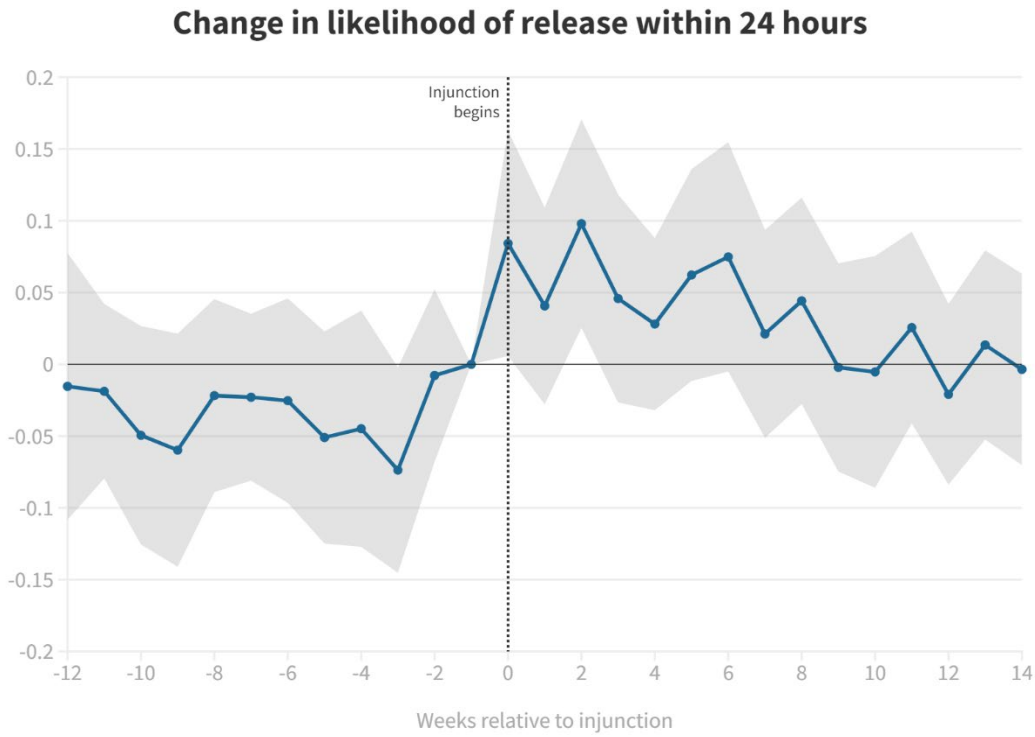
Appendix A

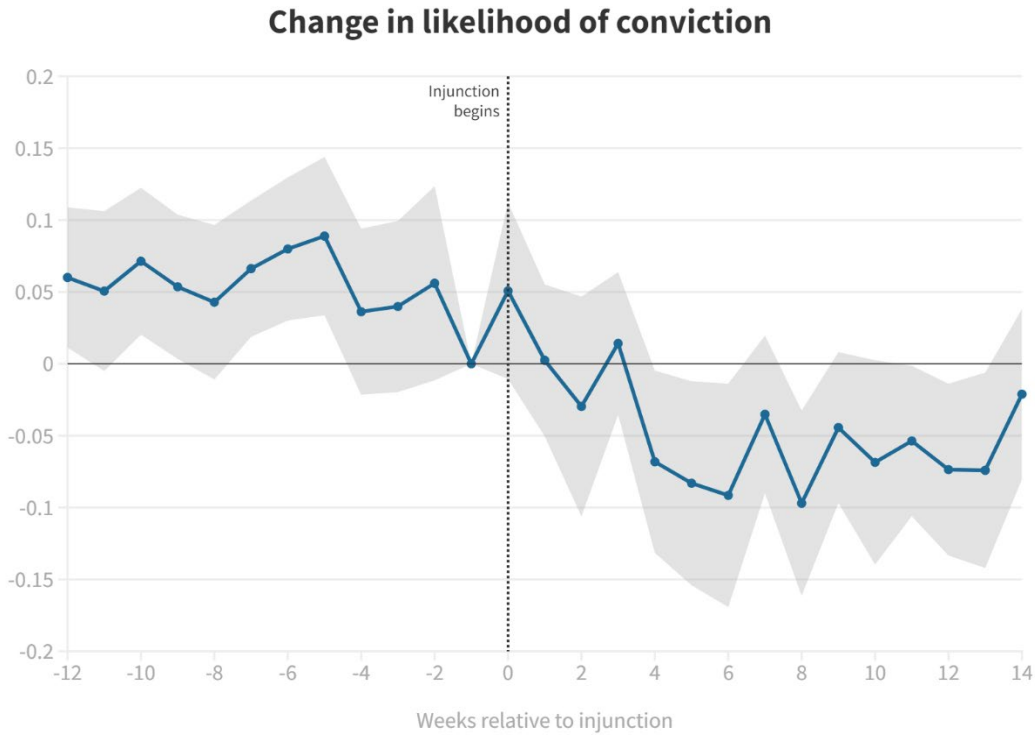
For the misdemeanor only analysis, we define as the control group people charged with violations of TX Penal Code 49.04 (DWI, which carries a minimum term of confinement); 38.02 (failure to identify) or 521.45 (driving with a false license) (because concealing one's identity precludes timely completion of the affidavit); and 22.05A (reckless endangerment), and 25.07 (violating an order of protection) (because these crimes often involve family violence which is a specific carve out in the injunction).

The figure below depicts the release rates over time for the treatment and control groups of misdemeanors based on the above definitions.

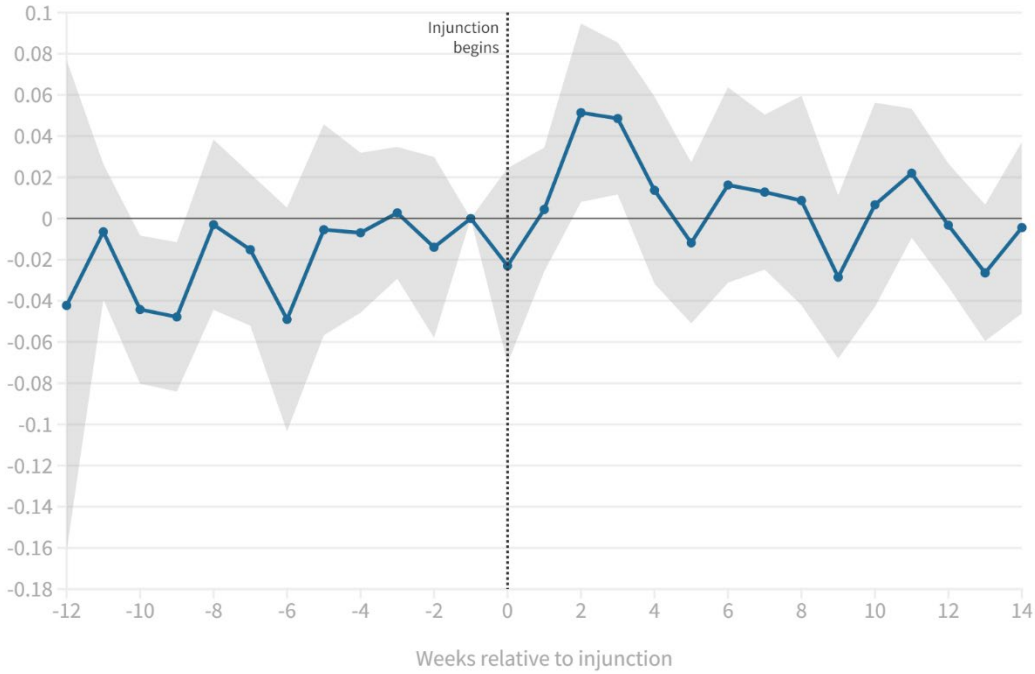


Appendix B

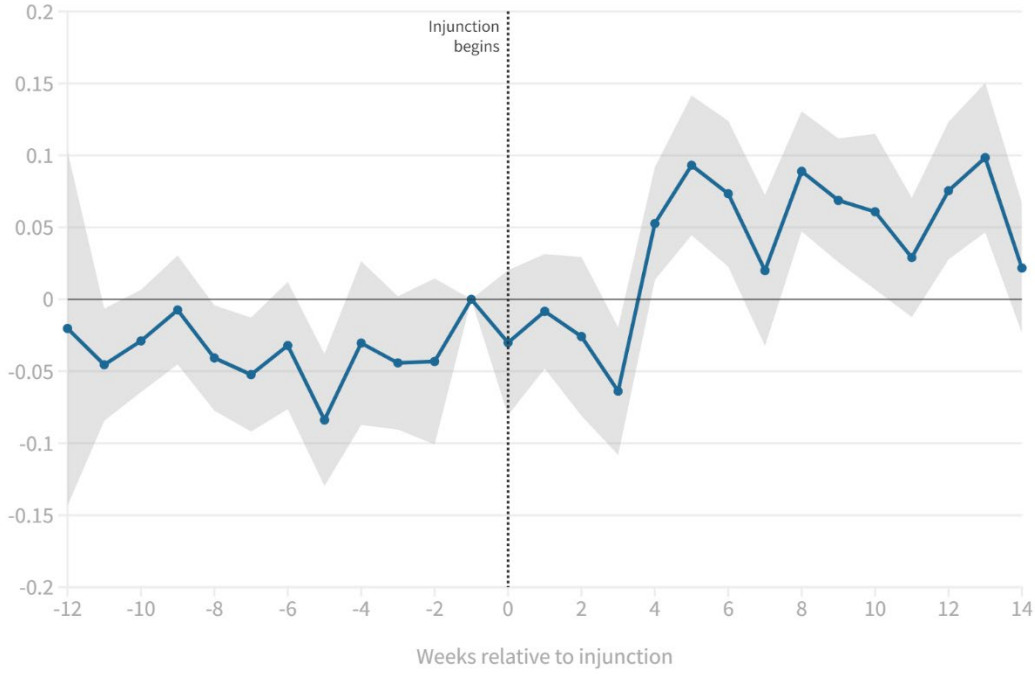

Change in likelihood of release within 24 hours
Injunction begins
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
-12
-10
-8
-6
-4
-2
0
2
4
6
8
10
12
14
Weeks relative to injunction


Change in likelihood of conviction
Injunction begins
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
-12
-10
-8
-6
-4
-2
0
2
4
6
8
10
12
14
Weeks relative to injunction

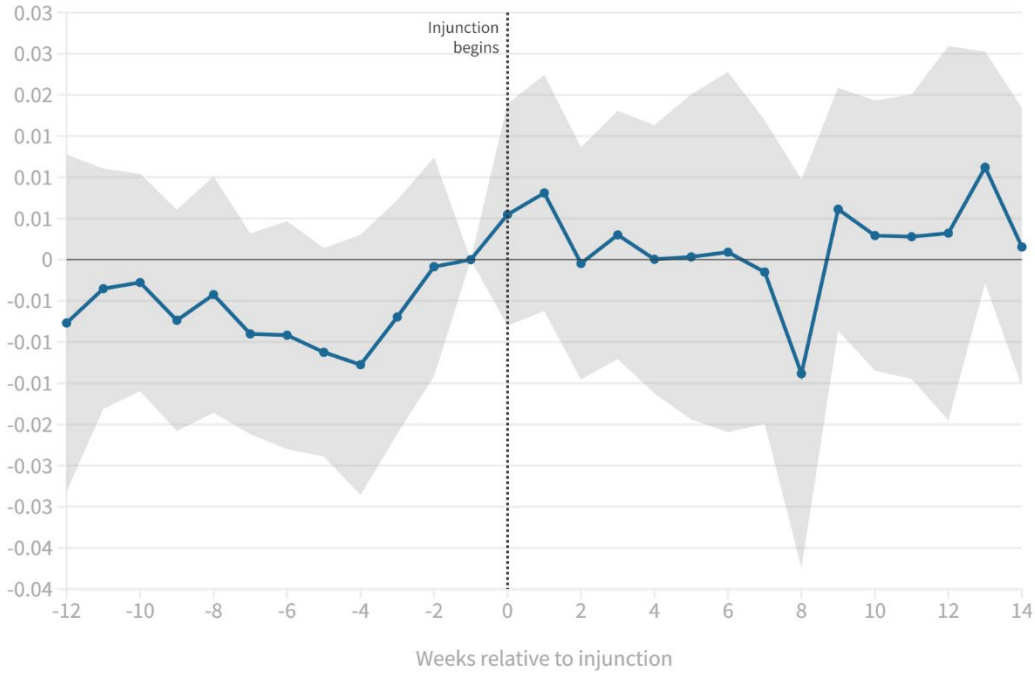
Change in share of cases diverted



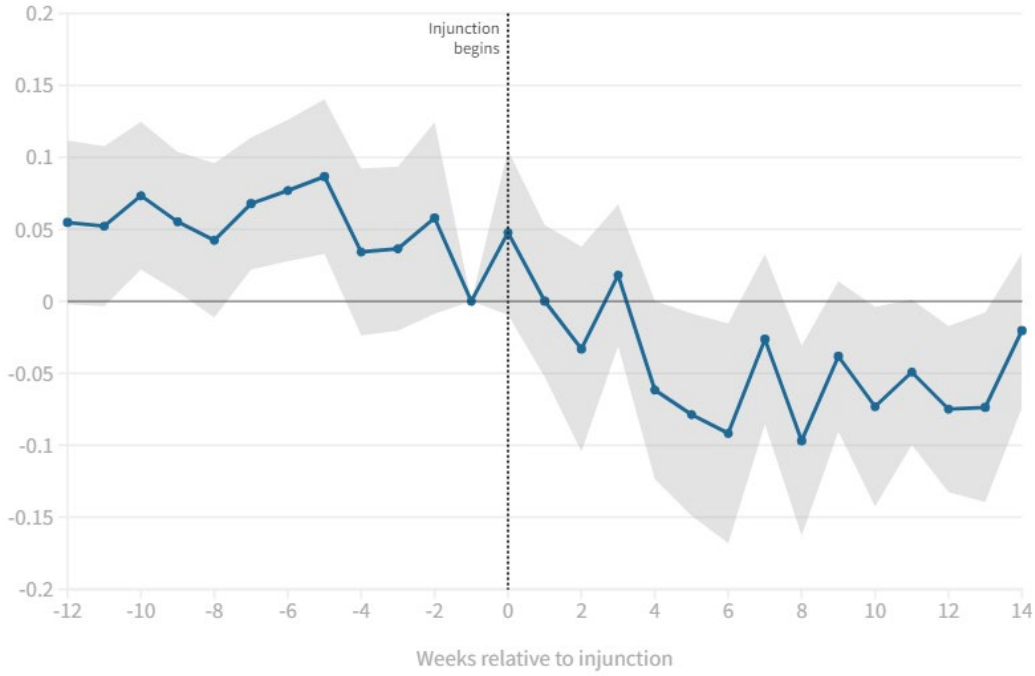
Change in share of cases acquitted or dismissed



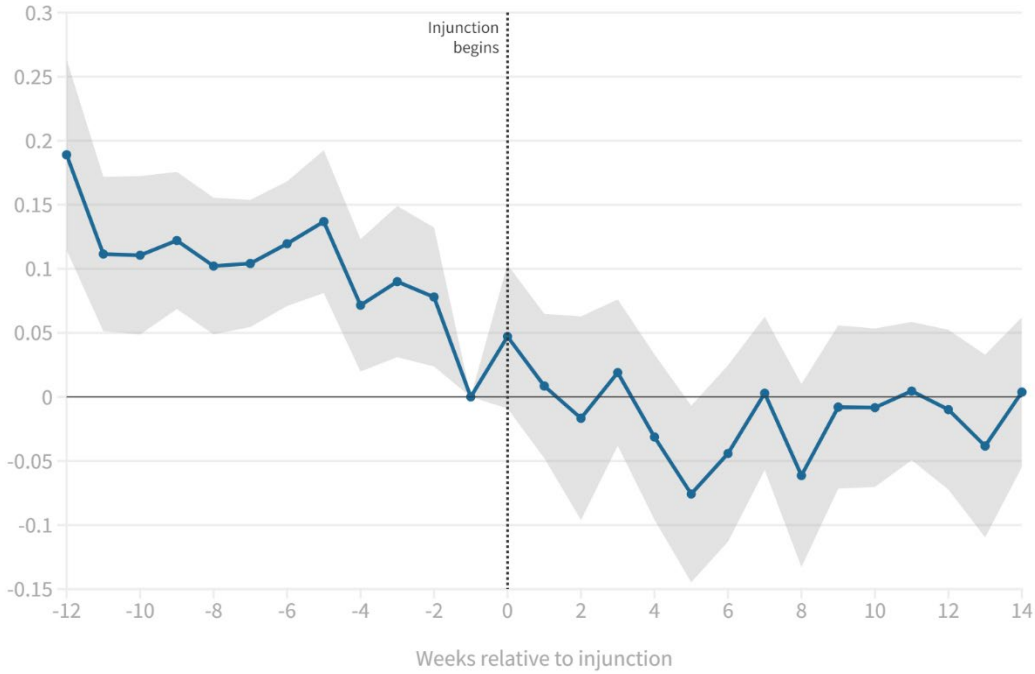
Change in share of cases never resolved



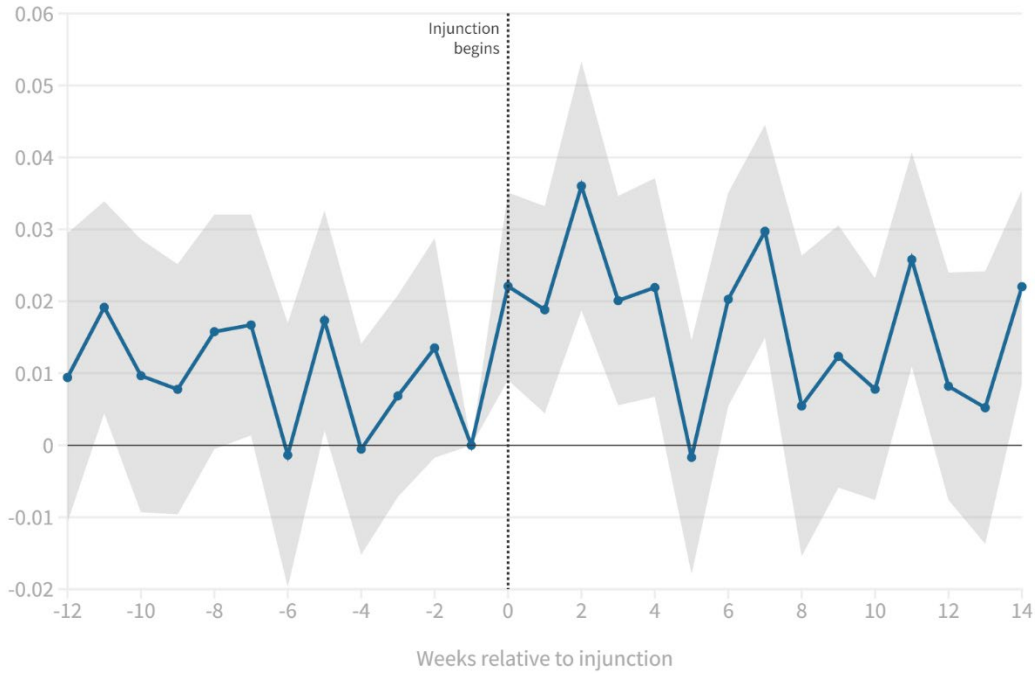
Change in likelihood of guilty plea

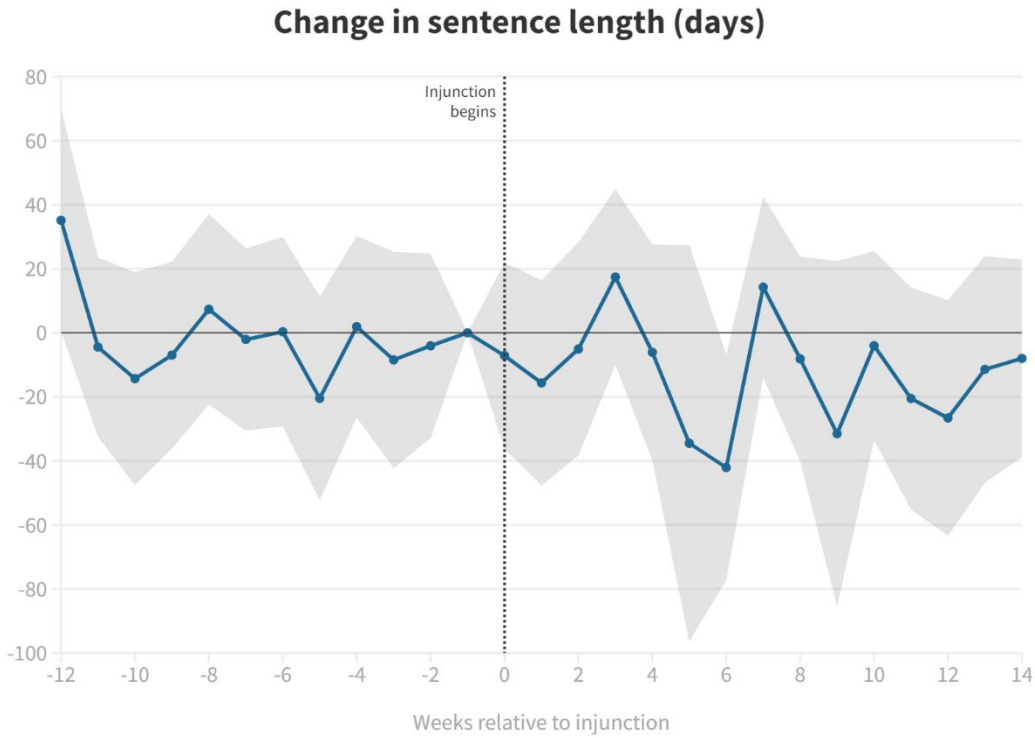


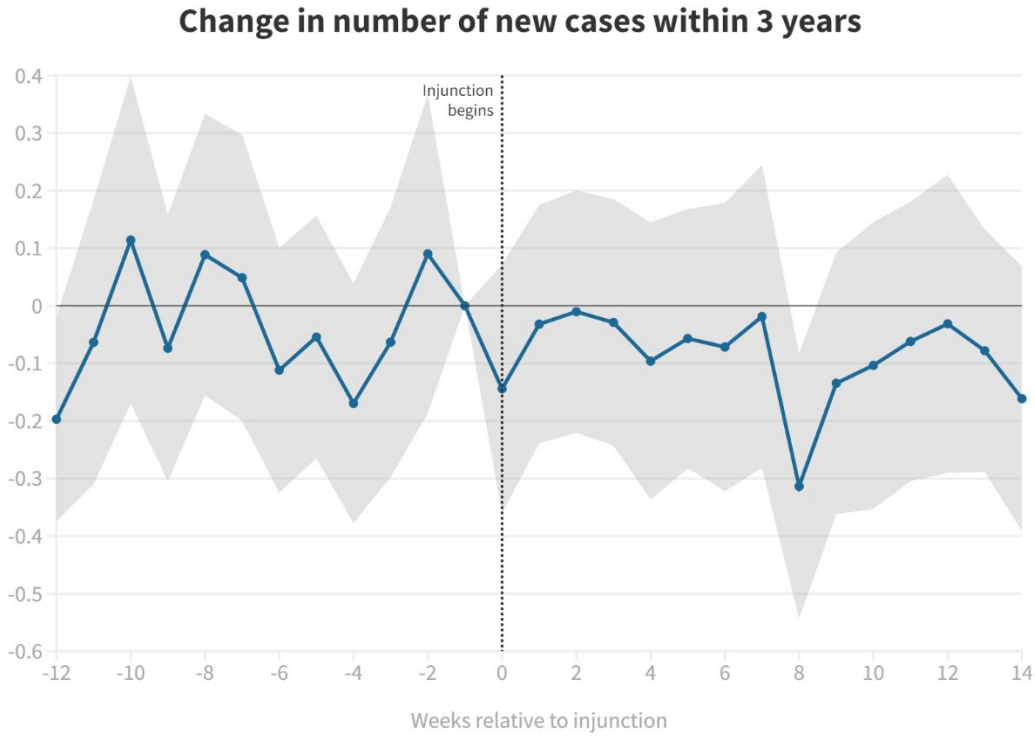
Change in likelihood of jail sentence

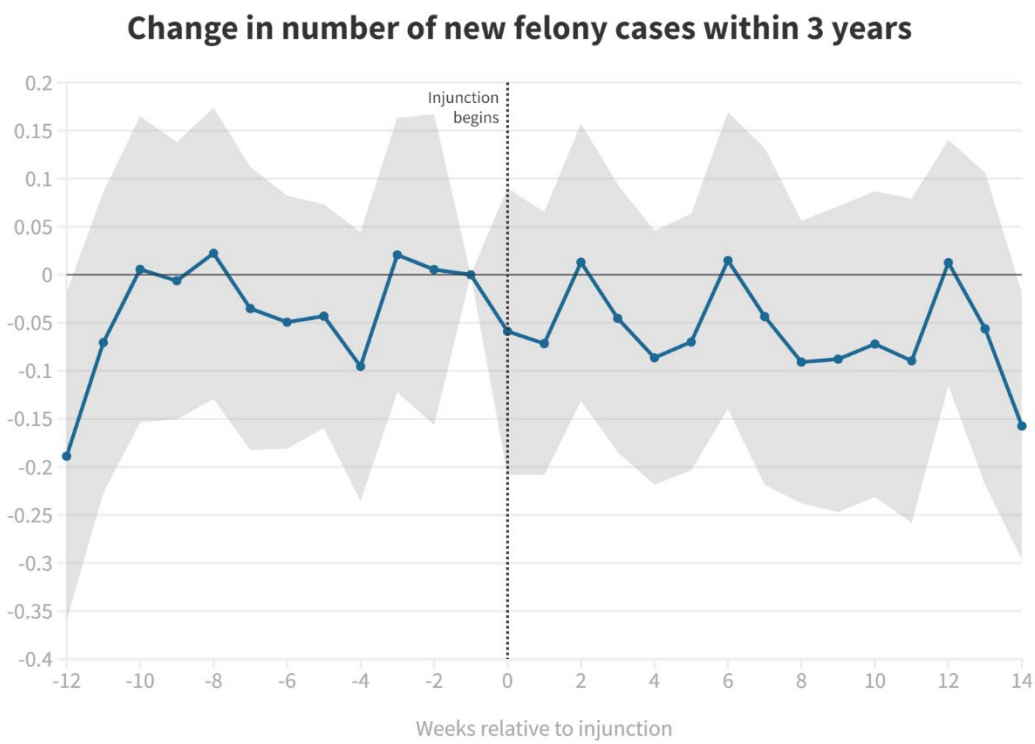


Change in likelihood of probation sentence




Change in sentence length (days)
Injunction begins
80
60
40
20
0
-20
-40
-60
-80
-100
-12
-10
-8
-6
-4
-2
0
2
4
6
8
10
12
14
Weeks relative to injunction


Change in number of new cases within 3 years
Injunction begins
0.4
0.3
0.2
0.1
0
-0.1
-0.2
-0.3
-0.4
-0.5
-0.6
-12
-10
-8
-6
-4
-2
0
2
4
6
8
10
12
14
Weeks relative to injunction


Change in number of new felony cases within 3 years
Injunction begins
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
-0.25
-0.3
-0.35
-0.4
-12
-10
-8
-6
-4
-2
0
2
4
6
8
10
12
14
Weeks relative to injunction