From Bail Bonds to Foreclosures: The Financial Consequences of Pretrial Detention*

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Abstract

In the United States, a significant portion of inmates in local jails is detained awaiting trial. Given the direct and indirect costs associated with pretrial detention, detained individuals and members of their households may find it difficult to meet their financial obligations. Matching individual case-level data from the criminal justice system to household-level data on foreclosures, bankruptcies, and lien judgments, we examine how pretrial detention affects household solvency. Exploiting the quasi-random assignment of court commissioners to cases for identification, we find that pretrial detention results in higher rates of household insolvency. This effect is driven by an increase in the incidence of foreclosures during periods of decreasing house prices and by an increase in the incidence of Chapter 7 bankruptcies. Subsample analyses show that the overall insolvency effects are more pronounced among younger defendants living in houses they do not own—suggesting spillovers to older household members—and when households have less equity in the home. Lastly, we provide suggestive evidence that the insolvency effects are exacerbated when defendants borrow funds from commercial bail bondsmen.

Keywords: Pretrial detention, bankruptcy, solvency, foreclosures
JEL codes: D14, G51, K41, K42

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1 Introduction

In 2021, there were approximately 7 million admissions to jail in the United States.[1] Following decades of large numbers of incarceration, approximately 45 percent of Americans have an immediate family member that has ever been incarcerated (Enns, Yi, Comfort, Goldman, Lee, Muller, Wakefield, Wang, and Wildeman, 2019). These family members often share the monetary costs associated with pretrial detention, the system that requires defendants to post bail in exchange for their release before trial. In this paper, we study how the court's decision on whether to release a defendant or detain him on bail affects the financial solvency of defendants' households, and examine whether home equity can act as a buffer against this effect.

Being detained on bail can adversely affect a defendant's financial solvency in multiple ways. First, monetary bail can directly strain defendants' household finances by imposing a significant liquidity shock (if the defendant or his relatives provide the funds) or due to the monetary cost associated with the bail bond fees (if the defendant borrows the funds). Second, being detained can deprive defendants of labor income and reduce future earning potential, with prior research finding that detention leads to higher conviction rates and lower rates of formal-sector employment (Gupta, Hansman, and Frenchman, 2016; Dobbie, Goldin, and Yang, 2018). However, Becker (1968) argues that individuals with lower opportunity costs of being detained (i.e., earning lower income and possessing fewer assets) are more likely to engage in criminal activity, which suggests that criminal defendants may not face severe financial consequences from pretrial detention.

While criminal defendants themselves may face lower opportunity costs from being detained, their families and close relations are often the ones that bear the financial burden of their detention. In some cases, parents and significant others act as co-guarantors of commercial bail bonds when defendants pay bail through a bail agent.[2] In other cases,

[1] Mass Incarceration: The Whole Pie 2023, Prison Policy Initiative.

[2] A New York Times article lists several such examples, including i) a Santa Clara father putting up his house as collateral on his daughter's bail bond and facing foreclosure as a result, ii) a Des Moines woman having

defendants—especially younger ones—reside with parents or grandparents who rely on the defendant's labor income for financial support.[3] Thus, in this paper we focus on the financial consequences of pretrial detention on defendants' *households* to capture potential intra-family financial spillovers.

Estimating the effect of pretrial detention on financial outcomes presents two key challenges. The first is that defendants who are released on their own recognizance are different from those that are detained on bail, given that release decisions are based on factors such as a defendant's threat to his community and his flight risk. Notably, courts are generally mandated to consider a defendant's ability to pay bail when making detention decisions, which would introduce bias in a simple OLS regression by lowering the release rate of those with greater financial means. The second challenge is that there is scarce data linking criminal court cases with households' financial records.

To overcome the identification problem, we exploit the quasi-random assignment of court commissioners to criminal cases in the state of Maryland. Commissioners are responsible for the initial detention decision following an arrest, and we make use of the fact that some commissioners tend to be more lenient than others in granting pretrial release. Following Dahl, Kostøl, and Mogstad (2014) and Dobbie et al. (2018), we estimate a residualized leave-out measure of a commissioner's leniency using the *other* cases that the assigned commissioner handled during that same year, after controlling for observable defendant and case characteristics.

To overcome the data challenge, we assemble a novel dataset that merges information on the universe of over 1.2 million criminal cases in the state of Maryland over the 2000-2020 period with three sources of data on consumer bankruptcy, foreclosures, and judgment liens. We use a defendant's address to match these data at the household level, which

her car repossessed after putting it up as collateral for her child's father's bail bond, and iii) a New Orleans mother facing threats from men with guns and bulletproof vests who demanded she pays her son's bail bond fees. See `https://www.nytimes.com/2018/03/31/us/bail-bonds-extortion.html`.

[3] A recent PEW poll showed that 47% of young adults resided with one or both parents even before the Covid pandemic. See `https://www.pewresearch.org/fact-tank/2020/09/04/a-majority-of-young-adults-in-the-u-s-live-with-their-parents-for-the-first-time-since-the-great-depression/`.

allows us to study the impact of pretrial detention on not only the defendant but also on his entire household. This link is key in our study, given that the burden of monetary bail is frequently borne by cohabitating family members. We further use linkages between cases and housing transactions to explore the roles of mortgage debt and home equity in exacerbating or attenuating the effect of pretrial detention on household insolvency.

We start by examining the validity of our instrument. We show that the leniency of a commissioner is a strong predictor of whether a defendant is released at the initial hearing, but is unrelated to observable characteristics such as the defendant's age, race, sex, and the type of charge they face. We conduct additional two-stage least squares tests on other criminal cases outcomes. First, we verify that assignment to lenient commissioners is associated with shorter periods of detention and lower incidences of bail payments, either through cash, property, or commercial bail bonds. Next, we validate findings of prior studies in our setting by showing that instrumented pretrial release is negatively associated with rates of guilty convictions and recidivism. These tests suggest that pretrial detention can result in incidences of household insolvency via two potential channels, monetary bail and loss of future income.

We then examine the effect of pretrial detention on household insolvency using two-stage least-squares. To this end, we create indicator variables for various insolvency events (foreclosures, bankruptcies, and judgment liens) at various time horizons relative to the initial hearing date. We find that instrumented pretrial release negatively predicts future (but not past) rates of *overall* insolvency (i.e., bankruptcy, foreclosure, *or* judgment lien), with the effect being statistically noticeable at a horizon of two years or longer. At a three-year horizon, pretrial release reduces the probability of overall insolvency by 2.4 percentage points, which represents a 15.5% reduction relative to the sample mean.

Next, we analyze each type of insolvency separately. We find no statistically significant effect on foreclosures in the overall sample, but a strong effect during the period of decreasing house prices (2008-2012). In particular, we find that pretrial release reduces foreclosure rates

by 3 percentage points (21.7% relative to its mean) at a three-year horizon during the 2008-2012 subperiod. The null finding for the overall sample suggests that rising housing prices may cushion households from adverse shocks. For instance, households may find it easier to tap into their home equity as a source of emergency liquidity.

Turning our attention to bankruptcy, we find that pretrial release results in a 1.59 percentage point decrease (46.7% of its mean) in the probability of filing for Chapter 7 bankruptcy within the following three years, with the effect becoming noticeable within the first year following the arrest. In contrast, pretrial release has no detectable effect on filing for Chapter 13 bankruptcies. This is consistent with defendants with limited means generally relying on Chapter 7 liquidations, which involve lower legal fees and resolve more quickly than Chapter 13 restructurings. Given that our bankruptcy sample is limited to a period of increasing housing prices (2000-2008), home-owning households that would have otherwise opted for Chapter 13 filings to protect their property may have avoided bankruptcy by tapping into their home equity.

We also find that pretrial release decreases the probability of a judgment lien within the following three years by 0.98 percentage points, a 31% decline relative to its mean. Judgment liens give creditors a lawful claim on property held by debtors when the debtors fall behind on their payments, and are often used by bail bondsmen against delinquent clients. In fact, Maryland bail bond contracts have, until recently, allowed for an automatic financial judgment against clients if they miss a payment.[4] However, our findings may capture judgment liens from not just bail agents but also hospitals, tax authorities, and other types of creditors.

We explore the possibility that the financial consequences of pretrial detention are borne by other members of the defendant's household. We find that the benchmark effect on overall insolvency is more pronounced for defendants that live in properties that they are less likely

[4] Such provisions are usually forbidden in consumer contracts as they deprive borrowers of the chance to contest the debt.

to own–i.e., properties acquired when they were relatively young.[5] This suggests that the effect we document on household insolvency reflects spillovers to cohabitating older relatives, such as parents that pledge their homes to bail out their children. We also repeat our analysis using insolvency events linked to defendants not only through addresses but also through *names*, and find that the higher rates of insolvencies we document are largely concentrated among same-address individuals that do *not* match the defendant's name, further suggesting spillovers to other household members.

Next, we investigate whether housing debt (equity) can exacerbate (mitigate) the effect of pretrial detention on household finances. We find that our benchmark effect is stronger when the defendant lives in a property purchased with a mortgage and when the mortgage has a longer remaining maturity until it is paid off, suggesting that higher leverage and lower accumulated equity increases insolvency risk. We also find the overall insolvency effect to be concentrated mostly in the post-2008 time period, when abundant liquidity from the booming property market of the early 2000s was no longer available.

Lastly, we explore the role of bail financing. Using the subsample of cases where defendants posted bail, we examine the relationship between insolvency outcomes and various types of bail payment types (cash, property, or bail bondsman). We find that bail paid through property bonds (i.e., pledging property to the court) and surety bonds (i.e., borrowing from a bail bondsman) are associated with higher rates of future insolvency relative to bail paid in cash. While this finding is consistent with the notion that financing bail payments by pledging property as collateral or borrowing from a bail bondsman places an additional financial strain on defendants' households, it may also reflect differences in the ability of defendants to pay bail.

Our paper contributes to several strands of the literature. First, it contributes to the literature examining the social and economic effects of pretrial detention. Prior studies have found pretrial detention to result in higher rates of conviction and recidivism (Gupta

[5] Due to limited capital accumulation and borrowing constraints, younger individuals are unlikely to be homeowners (Halket and Vasudev, 2014).

et al., 2016; Leslie and Pope, 2017; Stevenson, 2018b) and in lower rates of formal sector employment and government benefit takeup (Dobbie et al., 2018). We contribute to this literature by showing that pretrial detention has adverse effects on a variety of financial outcomes and by showing that the effects likely spill over to other members of defendants' households.

Our paper further provides a potential link between prior studies that document racial gaps in criminal detention (Rehavi and Starr, 2014; Raphael and Rozo, 2019; Arnold, Dobbie, and Hull, 2022) and in housing wealth (Blau and Graham, 1990; Garriga, Ricketts, Schlagenhauf, et al., 2017; Rugh and Massey, 2010; Kermani and Wong, 2021). While black and white defendants experience similar effects of pretrial detention on insolvency, black defendants constitute a significant majority of detained individuals (over 75% in our sample). Our findings suggest that the racial gap in housing wealth may partially be attributed to differences in detention rates, especially through spillovers to defendants' household members.

Second, our paper also contributes to the emergent literature on how fines and fees in the criminal justice system can trap individuals in a cycle of poverty. For example, recent research has shown that legal fees and fines can result in higher rates of criminalization (Pager, Goldstein, Ho, and Western, 2022) and employment instability (Mello, 2021) for low-income individuals, and that pretrial detention results in substantial non-bail court fees (Stevenson, 2018b). However, there has been comparatively little work examining how legal costs affect financial solvency, despite Harris, Evans, and Beckett (2010) documenting that legal debt in the U.S. penal system is substantial relative to expected earnings.

Third, our paper contributes to the literature on the effect of home equity on different economic outcomes. Within this literature, a vast list of papers has focused on labor and savings outcomes, including Mulligan (2008); Adelino, Schoar, and Severino (2015); Schmalz, Sraer, and Thesmar (2017); Bernstein (2018); Brown and Matsa (2020); Bernstein and Koudijs (2021); Gopalan, Hamilton, Kalda, and Sovich (2021); He and le Maire (2021);

Cespedes, Liu, and Parra (2023). We contribute to this literature by showing that home equity allows impacted households to mitigate the effect of pretrial detention on financial insolvency.

Finally, our paper contributes to the literature linking crime and foreclosures. Prior papers have found that foreclosures are associated with an increase in neighborhood crime (Immergluck and Smith, 2006; Ellen, Lacoe, and Sharygin, 2013), suggesting foreclosures impose significant social costs in addition to the already sizable direct financial costs documented by Diamond, Guren, and Tan (2020). Our finding that criminal detention increases the rate of foreclosures provides evidence of an additional causal relationship in the opposite direction, raising the possibility that crimes and foreclosures reinforce one another in a pernicious feedback loop.

2 Institutional Setting

2.1 Pretrial detention and monetary bail

According to the U.S. Bureau of Justice Statistics, approximately 65% of the 734,500 inmates held in local jails across the United States in 2019 were awaiting court action on a current charge (Minton and Zeng, 2021). Many have pointed to defendants' inability to afford monetary bail as a primary reason for this high pretrial detention rate. Monetary bail in the present-day U.S. involves the accused defendant posting cash or personal property as collateral to secure his release from pretrial detention. This collateral—or bond—is returned to the defendant if he appears for his appointed court date, and forfeited if he fails to appear. Given limited financial resources of many defendants, bail requirements can often result in prolonged detention spells and other adverse outcomes.

The modern U.S. bail system in particular contains two features that have contributed to its recent criticisms. First, through the Comprehensive Crime Control Act of 1984, the U.S. criminal code allows courts broad discretion over whether or not to set bail as a condition

for pretrial release, as well as the dollar amount at which bail is set. In principle, the court's decision should take into account relevant factors such as the severity of the alleged offence, the defendant's threat to the community, his flight risk, and his ability to pay. In practice, these decisions are made quickly by individual court officials such as judges and court commissioners, who are susceptible to human errors and biases in their decision-making.[6]

Second, the U.S. bail system is distinct from those in other countries in the ubiquitous presence of commercial bail bond agents ("bail bondsmen") in intermediating bail payments. Bondsmen can step in if a defendant cannot provide sufficient cash or property to make bail, and act as a "surety" (i.e., guarantor) by pledging the needed funds to secure the defendant's release. In return, the bondsman receives a fee (typically 10-15% of the total bail amount) that is often paid in installments by the defendant or his family members. If the defendant fails to appear in court, the bondsman is permitted by law or contractual arrangement to seek recovery of the bond amount and related expenses. Even if the defendant appears in court, he does not recover the 10-15% fee, which usually represents a significant financial cost to those with limited means.[7] In recent years, bail bondsmen have received criticism over exploitative practices, with the New York Times deeming them the "payday lenders of the criminal justice world."[8]

2.2 Maryland's pretrial system

We focus on the Maryland court system as the setting for our empirical analysis, where criminal cases are handled by two types of courts. District Courts handle criminal misde-

[6] For example, Danziger, Levav, and Avnaim-Pesso (2011) document a "hungry judge effect" relating to harsher parole decisions before meals in Israeli courts, and Arnold et al. (2022) find evidence of racial discrimination in bail decision in New York City courts. In recent years, bail decisions have increasingly involved the use of predictive algorithms, but the use of algorithms in judicial decisions has also faced criticisms over bias and transparency problems. For example, Stevenson (2018a) documents a jump in racial disparity between black and white defendants in pre-trial release decisions following the implementation of a risk-assessment algorithm in Kentucky's state justice system.

[7] Liu, Nunn, and Shambaugh (2018) finds that a 10 percent fee on the median bail amount would exceed the median financial assets of defendants in the bottom income quintile.

[8] See `https://www.nytimes.com/2018/03/31/us/bail-bonds-extortion.html`

meanors and certain felonies, while Circuit Courts handle cases that involve more serious crimes. When an individual is arrested for a crime, he is taken to a Court commissioner who determines whether there is probable cause to charge the defendant. The commissioner further ensures that the detainee understands the charges and potential penalties, advises on the right and responsibility in obtaining an attorney, and decides whether the defendant qualifies to be represented by the Public Defender.

Most importantly for our purposes, the commissioner determines the conditions for pretrial release based on considerations of the defendant's threat to the community, flight risk, and ability to pay bail. The commissioner can choose from the following options: i) release on recognizance (ROR), which releases the defendant to await trial without preconditions, ii) held in default of bond (HDOB), which requires bail as a precondition for pretrial release, and iii) held without bond (HWOB), which typically applies to high-risk defendants and keeps them detained pending trial without the option of bailing themselves out. If the defendant is held on bail, the commissioner also determines the dollar amount of bail. Typically, the bail must be paid upfront as a condition for release, but the commissioner can also allow for unsecured bonds in which the defendant signs an agreement to pay the bail amount only if he fails to appear in court ex-post (unsecured personal bonds), as well as bonds that require only partial upfront payments of the full bail amount (percent and partial bonds).

The Maryland system of rotating court commissioners is similar to those of other major U.S. metropolitan settings, as seen in the magistrate systems of Philadelphia and Pittsburgh (Gupta et al., 2016) and bail judge systems of Miami and Philadelphia (Dobbie et al., 2018). Following these studies, we exploit the quasi-random assignment of commissioners to cases in Maryland to identify variation in leniency in pretrial detention decisions.

Several factors make the Maryland court system a particularly suitable setting to implement this methodology. First, District Court commissioners preside over the *initial* detention hearing only, and not over subsequent hearings or trials. Second, commissioners work in assigned shifts, potentially across multiple locations, and are required to be available for day,

evening, night, weekend, and/or holiday shifts and subject to call-in during emergencies and staffing shortages. Third, with 279 commissioners working across the entire state,[9] the typical commissioner takes on a high caseload, which allows us to compute relatively precise estimates for commissioner leniency.

Defendants who are held on bail generally have three options for payment. First, *cash bonds* entail paying the full amount by cash, certified check, money order, or (subject to a court service fee) credit/debit cards. Second, *property bonds* entail the posting of personal property where the net equity in the property meets or exceeds the bail amount. This type of bond is usually only used for cases where bail is set in excess of $100,000. Finally, *surety bonds* entail the involvement of a commercial bail bondsman who pays the bail bond on the defendant's behalf in exchange for a fee, usually set as a fixed percentage of the bail amount (generally 10%).

Maryland's commercial surety bond industry is relatively similar to those in other U.S. states. First, there are relatively few restrictions placed on commercial bail bondsmen. For example, there are no special requirements to be licensed as a bondsman other than being over the age of 18, and there are no special prohibitions on how commercial bondsman can advertise their services. Second, the maximum allowable bail premium, at 10% of the total bail amount, is also similar to those of many other states.[10]

As in many other jurisdictions, the Maryland criminal bail system has faced criticisms in recent years. A report of the Commission to Reform Maryland's Pretrial System found that over 65% of the population of Maryland's jails were individuals that were held pre-trial, i.e., without having been convicted of any crime.[11] A large number of those detainees were in jail because they could not afford the bail set by the commissioner, imposing a monetary cost on society on top of the high costs defendants and their families face.[12] The report also

[9] See `https://www.mdcourts.gov/district/selfhelp/whodoeswhat#commissioners`.

[10] See `https://www.aboutbail.com/pages/bail-cost`. The effective annual interest rate on premiums can be much larger than 10% given the short window for repayment of fees.

[11] The Governor's Commission to Reform Maryland's Pretrial System

[12] The daily cost of incarceration is approximately $100 per inmate per day. Source: Annual Determination of Average Cost of Incarceration Fee, September 1, 2021.

indicates that defendants that managed to pay bail by relying on a bondsman paid significant amounts in fees. Specifically, during the 2011-2015 period, defendants across Maryland paid a total of over $250 million in non-refundable fees to bail bondsmen, most of them paid by residents of low-income areas in Baltimore City and Baltimore County.

Under mounting public pressure, the Maryland Court of Appeals reformed its bail system in 2017 with the passage of the Court Rule 4-216.1. This reform prohibits a judge from imposing financial conditions for release that are beyond the means of the individual. With this, Maryland joins a group of states that reformed the bail system, including Alaska, California, Illinois, Kentucky, Maine, Nebraska, New Jersey, Oregon, and Wisconsin, among other states. Following the reform, the proportion of defendants released on their own recognizance increased substantially as expected. However, an unintended consequence of the reform was that the proportion of defendants held *without* bail substantially increased as well, as commissioners were hesitant to release those they deemed high risk under the new bail prohibitions.[13] In our empirical analysis, we focus on the period prior to the reform to avoid contaminating our results with this dramatic shift in commissioners' decision-making process.

3 Empirical Design

In this section, we describe our empirical specification and the identification strategy we employ to overcome the endogeneity of the pretrial release decision.

3.1 Overview

To examine the effect of pretrial release, we regress defendant outcomes on a dummy variable indicating whether the defendant was released at his initial hearing using the following

[13] See `https://www.wsj.com/articles/marylands-bail-reform-is-a-warning-for-would-be-moralizers-1506119393`.

empirical specification:

$$Y_{ict} = \beta_0 + \delta Released_{ic} + X_{ict}\beta + \epsilon_{ict}, \tag{1}$$

where Y_{ict} is the outcome variable of interest for individual i in case c in year t, $Released_{ic}$ is an indicator variable for whether the defendant was released on own recognizance, X_{ict} is a vector of case- and defendant-level control variables, and ϵ_{ict} is the residual error term.

We examine various case and financial outcomes as the dependent variable Y_{ict}. For case outcomes, we examine indicators for timely release, eventual guilty disposition, failure to appear in court, and recidivism, in addition to measures of time spent in detention, to validate findings from prior research and to establish plausible channels through which pretrial detention might affect financial solvency. We also examine bail-related outcomes, including indicators for requests for a bail review hearing and bail payment method to verify that being denied release leads to a higher probability of posting bail. Most importantly for this paper, we examine financial solvency outcomes, including indicators for foreclosure, bankruptcy, and lien judgments. We include a detailed description of these variables in Section 4.

Given the effects on conviction rates, recidivism, and formal-sector employment documented by prior research (Gupta et al., 2016; Dobbie et al., 2018) and due to the financial burden of monetary bail, pretrial release is likely associated with lower rates of foreclosures, bankruptcies, and lien judgments. Moreover, these effects potentially affect not only a defendant but also his household members for at least two reasons. First, in many instances, parents, grandparents, or significant others also shoulder the financial burdens of monetary bail, either by providing the funds themselves or acting as co-signers on surety bonds. Second, the loss of employment and income associated with pretrial detention, as documented by Dobbie et al. (2018), may also have negative implications for those that depend financially on the defendant.

Thus, in our empirical analysis, we focus on financial outcomes for individuals in the

same *address* of the defendant to additionally capture adverse financial effects that spill over to *cohabitating* relations of the dependant. However, we also repeat our benchmark analysis after matching cases to insolvency events using *both* addresses and names to disentangle whether insolvencies resulting from pretrial detentions are associated with the defendant or other members of his household. To further provide suggestive evidence of the household spillover channel, we conduct additional analysis on the sample of young defendants who likely reside with parents and other relatives.

In our baseline analysis, we include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects in estimating Eq. 1. Since our financial outcomes are defined based on defendants' addresses, the inclusion of zipcode-by-year fixed effects serves to mitigate omitted variable concerns related to local conditions, such as economic shocks and housing market shocks.

3.2 Identification Strategy

To identify the causal effect of pretrial release on insolvency, we exploit variation in leniency across court commissioners over pretrial release decisions. While the shifting nature of commissioner rotation is helpful for identification, the assignment of cases may not be entirely random as commissioners may have some discretion over which shifts they accept. Therefore, selection may bias our estimates if we use simple averages or commissioner fixed effects as our leniency measure. For example, commissioners that tend to work weekend shifts face a different pool of defendants than commissioners that tend to work weekday shifts. Therefore, we construct a residualized, leave-out mean measure that accounts for selective matching between commissioners and cases following Dahl et al. (2014) and Dobbie et al. (2018).

To this end, we first compute residuals from regressing our indicator variable for the initial hearing decision, $Released_{ic}$, on a set of defendant and case characteristics using the

following specification:

$$Released^{*}_{ict} = Released_{ic} - \gamma X_{ict} = ReleasedRIV_{ctj} + \epsilon_{ict} \tag{2}$$

where X_{ict} represents a vector of characteristics associated with defendant i for case c in year t, including the logarithm of the defendant's age and a series of fixed effects, including sex, charge category, race, court-by-year-by-day of the week, and court-by-month-by day of the week. The residual release decision, $Released^{*}_{ict}$, comprises a commissioner's leniency measure $ReleasedRIV_{ctj}$ as well as an idiosyncratic component ϵ_{ict}.

We then average across all cases the commissioner sees in a given year, leaving out those involving defendant i:

$$ReleasedRIV_{ctj} = (\frac{1}{n_{tj} - n_{itj}})(\sum_{k=0}^{n_{tj}} Released^{*}_{ikt} - \sum_{c=0}^{n_{itj}} Released^{*}_{ict}) \tag{3}$$

where n_{tj} is the number of cases seen by commissioner j in year t and n_{itj} is the number of cases that commissioner j sees involving defendant i in year t.

By averaging out the idiosyncratic term ϵ_{ict}, we recover the commissioner leniency measure $ReleasedRIV_{ctj}$. This measure represents the average release rate of the assigned commissioner across all cases, *except* those involving defendant i that a commissioner sees in a given year after accounting for various case and defendant characteristics. The "leaving out" of cases involving defendant i is important, as otherwise regressing defendant outcomes on our commissioner leniency measure would produce biased estimates of the causal effect of pre-trial release by introducing the same estimation errors on both the left- and right-hand sides of the regression. In our two-stage least-squares analysis, we use $ReleasedRIV_{ctj}$ as an instrument for $Released_{ict}$ in estimating Eq. 1.

4 Data

4.1 Data Sources

We obtain case-level data for the universe of over 1.2 million criminal cases during the 2000-2020 period from the Maryland Judiciary public access database.[14] This data provides detailed information on the original charge of the alleged offence and warrants associated with any subsequent arrests, defendant characteristics (including name, age, race, sex and address), and importantly for our purposes, details about the initial commissioner hearing (including the hearing date, the unique ID of the assigned commissioner, and the commissioner's ROR/HDOB/HWOB decision).[15] For defendants held on bail, we also observe whether they are held on an unsecured or partial bond, and whether bail payment is made through cash, property, or a commercial bond agent.

In addition to information on criminal cases, the Maryland Judiciary database also provides information on civil cases, including those pertaining to foreclosures and judgment liens, which we use to construct two of our financial outcome variables. This dataset covers 386,938 judgment liens and 282,485 foreclosure filings over the 2000-2020 period, and contains information on the filing date and defendant's address, which we use to match to criminal cases.

We supplement our court-level data on foreclosures using real estate transaction information from the Zillow Transaction and Assessment Dataset (ZTRAX). ZTRAX's data for the State of Maryland covers 9 million transactions for the period 1993-2020, and includes both foreclosures and non-foreclosure home sales. We use included information on addresses and transaction dates to match ZTRAX records to criminal case records.

Lastly, we obtain data on consumer bankruptcy filings from Gross, Notowidigdo, and Wang (2014). This data originally comes from the Public Access to Court Electronic Records

[14] Case information is publicly available at `https://casesearch.courts.state.md.us/casesearch`.

[15] A subset of courts do not report the ID of the commissioner. Given that this information is crucial for our study, we exclude cases from those courts from our analysis.

system (PACER), and consists of 318,000 filings over the 2000-2011 period. Relevant information provided by this dataset includes the bankruptcy type (Chapter 7 or Chapter 13), as well as the filing date and filer address, which we use to match bankruptcies to criminal cases.

4.2 Sample Construction

We link criminal case addresses to civil cases, ZTRAX transactions, and bankruptcy filings using exact address matches based on the house number, street name, street type (avenue, road, street, drive, etc.), street direction (N, S, E, W, etc.), and zip code. We use addresses rather than names for two reason. First, addresses are relatively more standardized than names in the criminal case data, as defendant names often contain aliases and alternative spellings that result in poor matching. Second, and more importantly, linking through addresses allows us to capture the effects of pretrial detention not only on the defendant but also on his entire household. As previously discussed, cohabitating parents, grandparents, or significant others are often involved in bailing out defendants, either through providing the cash or property collateral themselves or acting as co-signers on commercial bail bonds, and therefore may also become financially affected if the defendant is held on bail.

We apply several filters to our sample. First, we limit our sample to the 2000-2016 period. As previously described, a 2017 court ruling in Maryland resulted in a dramatic change in commissioners' pretrial detention decisions. Figure I(a) illustrates this shift, with the proportion of ROR decisions dropping sharply and the proportion of HWOB decisions rising sharply following the reform. Therefore, applying this filter allows us to avoid issues stemming from the dramatic shift in overall release rates in 2017, as well as issues of interpreting whether the effects of being detained stems from being held on bail or held without bail. In the pre-2017 period, approximately 50% of defendants were held on bail, 44% of defendants were released on own recognizance, and only 6% of defendants were held without bail.

We also limit our sample to cases in which the commissioner was assigned to at least

50 hearings during the same calendar year. As Figure II(a) illustrates, the unconditional distribution of average release rates by commissioner-year spikes at 0%, 25%, 33%, 50%, and 100%. This is due to the large number of instances in which a commissioner takes very few cases in a given year, as seen in Figure II(c).[16] By requiring for an annual load of at least 50 cases, we can be more precise in averaging out idiosyncratic within-judge variation in ROR decisions using Eq. 3 to estimate our judge leniency instrument $ReleasedRIV_{ctj}$. After applying this restriction, we observe in Figure II(b) that the average annual release rate no longer exhibits spikes in the distribution, and in subfigure (d) we observe that the large spike in single-caseload years is removed from our sample. We further restrict our sample by dropping cases with addresses in properties with multiple units (i.e., addresses containing an apartment number). This serves to reduce noisy matching to datasets that do not consistently report apartment numbers (such as ZTRAX transactions).

Lastly, we limit the sample to cases where the address is linked with at least one ZTRAX transaction prior to the initial hearing date (extending back to 1990, the beginning of the ZTRAX data). This allows us to conduct important cross-sectional analysis by exploiting characteristics of defendants and their housing situation. More specifically, it allows us to calculate the age of the defendant at the time of the acquisition of the house and the time to maturity of the mortgage at the time of arrest. Defendants that were very young at the time of the acquisition likely do not own the property and live with relatives. Defendants living in properties with mortgages with shorter time to maturity likely own a larger fraction of equity in their homes.

Our final sample consists of 284,709 cases across three major District Courts in Maryland.[17] Out of those cases, 66.35% come from the Baltimore City District Court, 13.26% come from the Montgomery County District Court, and 20.39% come from the Prince George County District Court. As previously noted, we account for time-varying differences across

[16] These instances may stem from erroneous commissioner IDs, or from commissioners who quit shortly after being hired.

[17] Data is available for other jurisdictions but do not provide commissioner IDs, which we require for our identification strategy.

courts in our empirical analysis by including court-time fixed effects in both the residualization process of calculating our instruments and directly in our benchmark specification (Eq. 1) itself.

4.3 Variable Definitions and Descriptive Statistics

We provide summary statistics of case-level variables in Panel A of Table I. We observe that 44.4% of defendants in our sample are granted release on own recognizance. We also observe that, after controlling for case and defendant characteristics using our residualized leave-out mean procedure, the average commissioner leniency instrument *ReleasedRIV* exhibits a mean of 0.1%. Given the inclusion of court-by-time fixed effects in our residualization process, the average *ReleasedRIV* does not fluctuate over time (as illustrated in Figure I), meaning that the identifying variation in our tests is orthogonal to time trends. We also provide summary statistics for a non-residualized version of our instrument, *ReleasedIV*, which we calculate by averaging *Released* decisions instead of *Released** residuals in Eq. 3.

Panel A further shows that defendants in our sample are predominantly Black (76.4%),[18] male (83.9%), and have a median age of just under 30. We also provide summary statistics for the several relevant case-level variables: an indicator for whether the defendant is charged with a felony (*FelonyCharge*), the natural logarithm of the time elapsed before the defendant is released (*lnTimeDetained*), an indicator for whether the defendant is released within one day of the initial hearing (*TimelyRelease*), an indicator if the final disposition of the case was a guilty verdict (*Guilty*), an indicator for whether the defendant failed to appear at trial (*FailToAppear*), and an indicator for whether the defendant committed another crime following the initial hearing (*Rescidivist).*[19] We also illustrate time trends for *TimelyRelease*, *Guilty*, *FailToAppear*, and *Recidivist* in Figure I(b).

Lastly, we provide summary statistics for several bail-related variables. *BailReview* is an

[18] The remaining defendants are categorized into White (21.25%), Asian (0.54%), 0.1% (American Indian), and Unknown (1.49%). The data does not include information on whether the defendant is Hispanic.

[19] To be more specific, we define *Rescidivist* based on whether there was a subsquent warrant for the defendant's arrest following his initial hearing.

indicator for the occurrence of a bail review hearing. A defendant not released on recognizance at the initial hearing is entitled to a bail review before a Judge (not a commissioner) where he has a chance to seek less restrictive conditions for pretrial release (e.g., lowering the bail amount or being granted release on own recognizance). *BondCash*, *BondCorp*, and *BondProp* denote indicator variables for bail paid via cash bond, commercial surety bond, and property bond, respectively. *BondOther* denotes an indicator for bonds that do not require full upfront payment (e.g., unsecured personal bonds or partial bonds). We also show time trends across different bail payment methods in Figure I, with subfigure (c) illustrating trends in proportion of cases and subfigure (d) showing trends in average bail amounts.

In Panel B of Table I, we provide summary statistics for four sets of indicator variables that we examine as dependent outcomes Y_{ict} in Eq. 1. These consist of: 1) a foreclosure indicator (*Foreclosures*), 2) a bankruptcy indicator (*Bankruptcies*), 3) a judgment lien indicator (*Judgment Lien*), and 4) an indicator for any of the aforementioned personal insolvency events (*Insolvency*). As previously discussed, all financial variables are linked to cases via defendants' addresses.[20]

We define each financial outcome at different horizons with respect to the initial hearing date, ranging from 3 years prior to the initial hearing to 3 years after the initial hearing. For example, the last row of panel B indicates that in 15.3% of cases in our sample, there is either a foreclosure, bankruptcy, or judgment lien associated with the defendant's listed address within three years of the initial commissioner hearing.[21] We examine different horizons *before* the initial hearing date to ensure there are no significant differences or trends in insolvency events leading up to the case. Note that, since we examine outcomes up to three years following the initial hearing date, extending the sample beyond 2017 would also result in the

[20] We note that the mean for *Foreclosures*, *Bankruptcies*, and *Judgment Liens* do not add up to the mean for *Insolvency*. There are two potential reasons for this. First, a case may have multiple insolvency events at a given horizon. Second, the *Bankruptcies* figures are based on the 2000-2008 sample period only, when bankruptcy data is available, but the *Insolvency* figures are based on the entire 2000-2016 sample period.

[21] We are careful to avoid counting insolvency events when there has been an intervening house transaction that likely resulted in a change in ownership. Specifically, using ZTRAX transactions data, we set foreclosures, bankruptcies, and judgment liens at post-hearing (pre-hearing) horizon X to zero if the earliest (latest) occurrence of the event took place after (before) a housing transaction at the same address.

censoring of events that occur after 2020 (beyond our data coverage).

Given that we have two foreclosure datasets, we define our foreclosure indicator to take on a value of one if there is a linked foreclosure from *either* the ZTRAX or Maryland Civil Court datasets. As Figure I(e) illustrates, the raw foreclosure frequency counts from the two sources exhibit correlated time trends. However, ZTRAX data lacks coverage prior to 2007, but has better cross-sectional coverage over the later parts of the sample. In our empirical tests on foreclosures, we also examine a subsample for which coverage of the two datasets overlap.

We also face data limitations in our analysis of bankruptcies. As the raw plot of bankruptcy and judgment lien counts in Figure I(f) illustrates, our coverage of bankruptcies ends in 2011. Therefore, our analysis focusing on bankruptcy includes cases only up to 2008, three years before the end of the bankruptcy sample. However, we also examine the two major categories of consumer bankruptcy separately. As illustrated in Figure I(f), Chapter 7 bankruptcy (liquidation) exhibits more variation over time while Chapter 13 bankruptcy (restructuring) is generally more stable but significantly less frequent.

4.4 Instrument Validity

We conduct tests to check the validity of *ReleasedRIV* as an instrument in identifying the local average treatment effect (LATE) of pretrial detention on subsequent household insolvency. First, the exclusion restriction requires that commissioner assignment should affect defendant outcomes only through influencing the probability of pretrial release. While the exclusion restriction cannot be tested directly, we provide evidence of the relationship between our instrument and a host of covariates that suggests that harsh and lenient commissioners are not assigned to different types of defendants and cases.

The coefficients in Table II, column (1) show that the actual pretrial release decision, *Released*, is positively correlated with age and negatively correlated with the defendant being male, black, and facing a felony charge. The non-residualized leave-out mean measure

ReleasedIV is also negatively correlated with felony charges. However, the coefficients in column (3) show that the covariates are not systematically related to our residualized leave-out mean measure *ReleasedRIV*, with a joint p-value of 0.506. This provides suggestive evidence that commissioners with different tendencies are assigned to ex-ante similar defendants and cases.

Next, we check for the relevance of our instrument in the first stage of the two-stage least squares process. Figure III provides bin scatterplots that illustrate the relationship between our instruments and the endogenous variable *Released*. Specifically, subfigure (a) shows the relationship between the residualized *ReleasedRIV* and *Released*, and subfigure (b) shows the relationship between the non-residualized *ReleasedIV* and *Released*. In both cases, we see evidence of a strong linear relationship.

We provide the estimate from the first stage linear probability regression in column (1) of Table III. We see that the positive relationship between *ReleasedRIV* and *Released* is very significant, with an associated F-statistic of 6,305.53 that is well above the threshold for instrument relevance (Stock and Yogo, 2005; Olea and Pflueger, 2013). This specification includes a host of fixed effects, including court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects. Unless otherwise indicate, all regressions for the remainder of this paper include this set of fixed effects.

Last, we evaluate whether our instrument satisfies the monotonicity assumption—i.e., that the relationship between the instrument and the endogenous variable is monotonic across individuals. If this assumption is violated, then our two-stage least squares estimates will not reflect a well-defined local average treatment effect (Angrist, Imbens, and Rubin, 1996; Heckman and Vytlacil, 2005). While we cannot directly test this assumption, we can conduct tests based on one of its key implications: that the first-stage estimates should be non-negative for all subsamples. In the remaining columns of Table III, we observe that the relationship between *ReleasedRIV* and *Released* is persistently positive and sizeable across different subsamples categorized by race, sex, age, and geographic location. Moreover, the

point estimates are similar across subsamples, ranging from 0.888 to 1.07.

5 Results

In this section, we examine the effect of pretrial release on financial outcomes using the instrumental variable approach delineated above. We first describe the results on case outcomes and then on multiple measures of household insolvency.

5.1 Pretrial Outcomes

We present the OLS results on case outcomes in Panel A of Table IV. We find that defendants that are released have, in general, significantly better outcomes than those that are detained while awaiting trial. The OLS estimates suggest that released defendants are about 50 percentage points more likely to be released within a day (column 1), spend significantly less time detained (column 2), and are 0.6 percentage points less likely to plead guilty (column 3). These defendants are more likely to fail to appear (column 4), a finding that is consistent with the notion that defendants that are detained cannot flee. We also find a 1.2 percentage point decrease in the probability of recidivism, which represents a 31.6% decline over the mean level (column 5).

We find consistent results in our two-stage least squares (2SLS) analysis in Panel B, where we exploit the quasi-random assignment of cases to commissioners that generates plausibly exogenous variation in the tendency to release defendants. These results confirm that defendants that are released on their own recognizance before trial experience significantly better outcomes. The marginally released defendant experiences an increased probability of timely release, shorter period of detention, lower likelihood of pleading guilty, higher likelihood of failing to appear in court, and lower likelihood of recidivism.[22] These results are in line with findings in prior studies such as Dobbie et al. (2018).

[22] Timely release is defined as a detention period shorter than one day.

We then turn our attention to the effect of pretrial release on bail setting. In Panel A of Table V we present the OLS results, which suggest that released defendants are less likely to have a bail hearing (column 1) and less likely to pledge cash bonds (column 2), corporate bonds (column 3), property bonds (column 4), and bonds that do not require full upfront payment (column 5). In Panel B we present the results of the 2SLS analysis and find consistent results. This evidence is in line with the idea that pretrial detention has significant negative consequences on defendants in terms of detention, case outcomes, and bail terms. In the next subsections, we explore the financial consequences of pretrial detention.

5.2 Household Insolvency

A defendant that is not released on his own recognizance pretrial remains in jail or pays bail to be released, a financial burden that often falls on his family members. In either case, the outcome likely imposes a significant financial strain given a deterioration in the household's income or the high costs associated with bail. Thus, we shift our attention to the effects of pretrial detention on household insolvency. To this end, we define household insolvency as an event in which the defendant or his household members experience a foreclosure, a personal bankruptcy, or a judgment lien.

We present the OLS results of the impact of pre-trial detention on household insolvency in Panel A of Table VI. We find that the release of a defendant leads to a decline in the frequency of insolvency events. This effect is not noticeable during the first three or six months following the initial hearing (columns 1 and 2) but becomes noticeable and significant within the first year (column 3), with a reduction in the probability of insolvency of approximately 5% over the baseline level. We find that the effect remains significant for three years (columns 4 and 5) and also when we use a measure of insolvency occurring at an indefinite horizon (column 6). To control for housing-level factors that may affect the probability of foreclosure, we include control variables including indicators for whether an insolvency event occurred within the three years prior to the hearing and for whether the house was purchased within the three

years prior to the hearing. We see that both indicators are strong predictors of subsequent insolvency events, and we include them as controls throughout the remainder of the paper.[23]

In Panel B we present the results of the two-stages least squares analysis, where we find consistent results. During the first year, there is a reduction of 0.7 percentage points in the probability of insolvency, or 10.2% of the baseline level, although the coefficient is statistically insignificant in columns (1) to (3). However, within the first two years, the effect becomes significant and larger in magnitude (a 13.5% reduction), as shown in Column (4). This effect remains large and significant for the first three years, with an associated decline of 15.6% of the mean as reflected by the coefficient in column (5). The effect persists when we use the unrestricted horizon (column 6).

We note that our 2SLS estimates are consistently larger than our OLS ones by a factor of 3-4. We posit that potential downward bias in the OLS estimates may stem from commissioners having to factor in a defendant's ability to pay when making their pretrial release decision. In particular, if commissioners are less likely to hold individuals on the verge of financial distress on bail, then we should expect our OLS estimates to be biased downwards. To the extent that our instrumental variable is unrelated to the focal defendant's economic means, we are able to correct for this bias.

These results show that the financial effects of pretrial detention do not materialize immediately, a finding that is in line with the idea that insolvency is a lengthy process. For instance, while there is variation across jurisdictions, the foreclosure process typically takes no less than 120 days. Importantly, the plots in Figure IV, which illustrate estimates for regressions examining *backward-looking* insolvency outcomes at different horizons, show no differential effects in the months and years prior to the arrest. More specifically, the estimates prior to the treatment are small and statistically insignificant.

[23] Our results are robust to the exclusion of these control variables.

5.3 Home Foreclosures

We then examine the effect of pre-trial release on each component of insolvency: home foreclosure, personal bankruptcy, and judgment liens. Table VII presents the 2SLS estimates of the impact on home foreclosures. Panel A presents the results for the full sample period, where we find that the release of a defendant has a positive effect on foreclosures, i.e., it reduces the probability of a foreclosure. Within the first three years, the probability of a foreclosure declines by approximately 10%, although the coefficients are statistically indistinguishable from zero. A potential explanation for this finding is that, during a significant part of this period, there was a large run-up in house prices, which might have allowed defendants to mitigate the impact of pretrial detention on mortgage delinquency by borrowing against home equity or renegotiating the terms of the mortgage.

To test whether this explanation is likely, we repeat the analysis in the sample period in which house prices were declining in the State of Maryland (2008-2012).[24] We present the results for this subperiod in Panel B, where we find a large and significant effect of pretrial release on foreclosures. This effect becomes noticeable within one year of the initial hearing and remains noticeable for three years and over. More specifically, the release of a defendant reduces the likelihood of foreclosure by 3 percentage points, or 21.7% of the mean foreclosure rate during the first three years. We plot the estimates of our 2SLS analysis in Figure V, where we include the estimates presented in Table VII and those for the periods prior to the arrest. Both in Panel (A), where we focus on the full sample, and in Panel (B), where we focus on the subperiod with declining house prices (2008-2012), we find no differential effects prior to the arrest.

We note that the 2008-2012 period, which contains the Great Recession that followed the financial crisis of 2007-2008, also marked a period of high unemployment rates and income stagnation, especially among low-income individuals (Meyer and Sullivan, 2013). Therefore, it is a period when the opportunity cost of foregone income due to pretrial detention should

[24] Data from https://fred.stlouisfed.org/series/MDSTHPI

have been lower, as individuals were unlikely to earn high incomes even if they were not detained. This suggests that the foreclosure effect we detect during this period is likely driven by a lack of home equity buffer rather than from lost income due to prolonged detention.

5.4 Personal Bankruptcy and Judgment Liens

We also examine the effect of pretrial release on personal bankruptcy and judgment liens, a court ruling that gives creditors the right to take possession of a debtor's property (home) or possessions (such as a car, appliances, furniture, etc.). A lien can be placed, among others, by a bail bondsman when a defendant or his guarantor do not pay the money owed, or by the court when the defendant pledged a property bond and failed to appear in court.

We present the results of the 2SLS analysis in Table VIII. The estimates in Panel A suggest an economically large but mostly statistically insignificant effect on bankruptcy rates. Specifically, only the coefficient in column (4), corresponding to the 2-years horizon, is statistically significant, suggesting a reduction in the probability of bankruptcy of 27%. The coefficients in columns (1) to (3) and (5) are statistically indistinguishable from zero, but economically large. In Panel B we present the estimates on judgment liens, where we find an economically large effect but statistically insignificant effect, except for the coefficient in column (5), which represents a reduction of 29.4% in the likelihood of having a lien placed within the first three years.

A potential explanation for the lack of significance is that our bankruptcy indicator covers both Chapter 7 liquidations and Chapter 13 restructurings, potentially masking heterogeneity across the two types of consumer bankruptcy. Chapter 7 bankruptcy allows individuals to immediately discharge all debts and makes up approximately three-quarters of all the bankruptcy cases in our sample. Chapter 13 bankruptcy generally involves filers restructuring their debt and agreeing to a payment plan while keeping their assets. Therefore, detained individuals may file for Chapter 7 bankruptcy if they seek a quick resolution, or for Chapter 13 if they wish to maintain possession of their assets.

We examine Chapter 7 and Chapter 13 bankruptcies separately and present the analysis in Table IX. The estimates in Panel A suggest a large and statistically significant effect of pretrial release on Chapter 7 bankruptcy that becomes noticeable within a year of the hearing and remains noticeable for the first three years. In particular, the coefficient in column (3) suggests a reduction of 65% in the probability of filing for Chapter 7 bankruptcy within a year of the detention, 68% within two years (column 4), and 46% within three years (column 5). In contrast, all the coefficients in Panel B are statistically indistinguishable from zero, suggesting no effect on Chapter 13 bankruptcy.

These findings suggest that the financial strain of pretrial detention leads defendants to seek the speedy resolution of Chapter 7 liquidation rather than protecting their assets through Chapter 13 restructuring, consistent with the notion that liquidation bankruptcy is typically a better fit for individuals with low incomes and low asset levels (Littwin, 2020). We note that our bankruptcy analysis is limited to the 2000-2008 subperiod since our bankruptcy data extends only to 2011. As previously mentioned, this was a period of rising house prices that potentially buffered households against adverse financial shocks. Therefore, our null findings on Chapter 13 liquidations may be explained by the fact that defendants with high levels of home equity that may have *otherwise* wished to protect their assets through Chapter 13 bankruptcy were instead able to take advantage of booming house prices to avoid insolvency altogether.

As before, we plot the dynamic coefficients in Figure VI for all bankruptcies (Panel A), judgment liens (Panel B), Chapter 7 bankruptcies (Panel C), and Chapter 13 bankruptcies (Panel D). In all cases, we find evidence that supports the absence of differential effects prior to the arrest.

5.5 Robustness Tests

As a robustness test, we repeat our analysis of the effect of pretrial detention on household insolvency using an alternative specification for our instrumental variable. In particular, we

use a non-residualized instrumental variable, i.e., a measure that does not account for the potential selective matching between commissioners and cases. We present the results in the Internet Appendix, Table A.I. We find results that are largely consistent with those of the residualized instrumental variable. More specifically, we find a significant reduction in the likelihood of insolvency (Panel A), foreclosures (Panel B), bankruptcy (Panel C), and judgment liens (Panel D) for defendants that are released pre-trial.

In our main analysis sample, we drop cases with addresses that list an apartment number to reduce noisy matching to datasets that do not consistently report apartment numbers (such as ZTRAX transactions). To show that our findings are robust to the inclusion of such cases, we repeat our benchmark 2SLS analyses using a sample that does *not* exclude cases linked to apartments and present the results in Table A.II of the Internet Appendix. Again, we find that our findings are qualitatively unchanged, with pretrial detention associated with higher rates of subsequent insolvency across various insolvency categories.

6 Mechanism

In this section, we examine potential mechanisms for our findings, such as spillovers to other household members. First, we present the results of a subsamples test that exploits cross-sectional characteristics of the defendants and their housing situation. Second, we study insolvency events and look at whether the name of the person affected matches that of the defendant in the criminal case. Third, we explore whether our results are likely driven by a reduction in household income or by the financial burden imposed by the monetary bail. Fourth, we explore heterogeneous effects across bail payment methods.

6.1 Subsamples Tests

We examine heterogeneous effects of pretrial detention on household insolvency by exploiting cross-sectional characteristics of defendants and their housing situation to shed light on

potential mechanisms. We present the results of this analysis in Table X, where we first focus on characteristics of the defendants (Panel A) and then on characteristics of their housing situation (Panel B). The coefficients in the first two columns of Panel A suggest that the effect on insolvency is, on absolute terms, larger for younger defendants—those under 30 years old—than for older defendants. These coefficients are, however, not statistically different. We find similar results when we compare black versus non-black defendants (columns 3 and 4). We observe that the estimated effect of pretrial detention on household insolvency is stronger for male defendants than for female defendants (columns 5 and 6). However, there are relatively few cases of female defendants, resulting in limited power for these tests. Overall, these results suggest that our benchmark findings are not driven by a specific subgroup.

We then explore whether our results can be driven by a spillover from criminal defendants to older household members, who likely depend on the defendant's income or face the burden of bail. To this end, we analyze the age of the defendant at the time of the purchase of the home. Our key assumption is that defendants that were young—below the age of 25—when the property they live in was last purchased are not the owners of the property and that they likely live with relatives. In columns 7 and 8, we present the estimates obtained by splitting the sample into defendants that were below/above 25 years old when the property was purchased.[25] We find that the effect is larger in magnitude and statistically significant for defendants that were younger at the time of the acquisition, suggesting a spillover effect to older members of their households.

In Panel B, we examine heterogeneity in housing characteristics of the defendant's listed home. We find that the effect is concentrated in defendants that live in properties with prices above the median price (columns 1 and 2), which are likely associated with larger mortgage payments. We also find that the effect of pretrial release on insolvency is stronger and statistically significant for defendants living in properties that were purchased with a mortgage (columns 3 and 4), which is consistent with the idea that pretrial detention

[25] As with the age of the defendant in columns 1 and 2, we split the sample across the median age of the defendant at the time of the purchase of the property.

imposes a financial burden that affects the ability to make mortgage payments and results in foreclosures. We also find that the effect is concentrated on defendants living in properties with mortgages with longer maturity, measured from the time of the initial hearing. This is consistent with the idea that defendants that have less equity in their houses—or a longer period until the mortgage is fully paid off—cannot tap into their home equity to deal with the liquidity shock. Last, we find that the effect on household insolvency is concentrated in the latter half of our sample period (2008-2016). As previously discussed, the 2000-2007 portion of our sample was characterized by rapidly rising house prices which may have buffered households from adverse shocks.

Overall, these findings point to a spillover from younger to older house members and suggest that home equity and increasing home prices allow defendants to avoid insolvency.

6.2 Within-Household Spillovers

Next, we further explore potential spillover effects on cohabitating relations by exploiting the availability of individuals' names in our criminal case and insolvency datasets. To this end, we repeat our analysis by redefining our outcomes of interest based on whether the criminal defendant's name matches the name linked with the insolvency event. Conditional on matching addresses between the case data and insolvency data, we define an insolvency event to be a "full-name match" if the full name of the defendant matches the full name in the insolvency data, "last-name match" if the last name of the defendant matches the last name of in the insolvency data, and "different name" if the full name of the defendant does not match the full name of in the insolvency data.

We present the results in Table XI, where we explore the insolvency outcomes in which we previously found significant effects (overall insolvency, foreclosures during the 2008-2012 period, Chapter 7 bankruptcy during the 2000-2008, and judgement liens). In Panel A, we report the results on full-name-match insolvencies. If our benchmark findings are driven by defaults specific to the defendant, we should expect to find significant effects of pretrial

detention on these insolvency measures.[26] However, we see that other than for judgment liens, we cannot detect a statistically significant effect across various insolvency categories.

In Panel B, we report the results on last-name-match insolvencies. These estimates capture the effect of pretrial detention on the insolvency rates of other members of the defendant's household who share the defendant's last name. We see that there is a weakly significant effect on overall insolvencies, and the magnitudes of the effect on foreclosures, Chapter 7 bankruptcies, and judgment liens are larger, although the estimates are still insignificant for foreclosures and bankruptcies.

Finally, in Panel C, we report the results of pretrial detention on different-name insolvencies, which captures household spillovers to individuals *other* than the focal defendant. This includes family members that share the defendant's last name (as captured by the cases in Panel B), as well as relations who do not share a last name (which is not captured by the cases in Panel B). This is key to our analysis, given that a large number of households in these areas are single-parent households where the last name of the defendant would not match that of the relative.[27]

We see larger effects, both in economic and statistical magnitude, across all types of insolvencies, with the exception of judgment liens. Taken together, these results suggest that our baseline findings on foreclosures and Chapter 7 bankruptcies are driven by within-household spillovers.

6.3 Loss of Income vs. Burden of Bail

As previously discussed, pretrial detention can adversely affect a defendant's household finances through an income loss channel as well as a bail burden channel. Conditional on being detained on bail, the defendant faces a trade-off between paying bail, which allows

[26] One factor to consider is that in many instances the names listed in civil and criminal cases refer to aliases or include typos, making this name matching imperfect.

[27] The percentage of single-parent households as a percentage of total households with children in 2011 was 62.87%, 43.5%, and 23.23% in Baltimore City, Prince George County, and Montgomery County, respectively, with a large majority corresponding to households that were headed by single mothers. Source: Federal Reserve of St. Louis Economic Data.

him to return to work, and delaying or not paying bail, which may result in a greater loss of income and even the loss of his job. This choice is endogenous to many factors, including the defendant's employment status, ability to pay bail, and future earning potential, making it difficult to empirically disentangle these channels in explaining our main findings.

We exploit the two-stage structure of our IV regressions to check whether we can *rule out* one of these channels. To this end, we repeat our benchmark 2SLS tests using *ReleasedRIV* as the instrument, but replacing the endogenous instrumented variable *Released* with other variables specific to the income loss channel and bail burden channel, respectively. First, if our benchmark insolvency effects are driven by defendants quickly posting bail to secure their release, then we should *not* detect any second-stage effects on insolvency if we first project our instrument onto *lnTimeDetained*, our measure of detention length. Conversely, if our benchmark effects are driven by defendants that stay in detention after declining to post bail, then we should *not* detect any second-stage effects if we first project our instrument onto *BailBond*, an indicator of bail payment (via cash bond, property bond, or commercial bond).

We present the results of these falsification tests in Table XII. We see from Panel A that the instrumented *lnTimeDetained* has a positive effect on subsequent household insolvency at longer horizons, consistent with the channel of income loss stemming from extended detention. We also see from Panel B that the instrumented *BailBond* has a positive effect on subsequent household insolvency at longer horizons, consistent with the bail burden channel. In both sets of tests, we do not claim the exclusion principle holds, but rather that we cannot rule out either channel in explaining the effect of pretrial detention on household solvency. In fact, both channels may apply for defendants who are unable to post bail immediately and lose income during their detention, but still face the burden of bail once they pay.

Given that we cannot rule out the bail burden channel, we explore whether the *method* of bail payment is related to subsequent household insolvency. To this end, we estimate a simple OLS regression, in which we restrict the sample to HDOB cases where the defendant

posted the full upfront bail amount, and regress financial outcomes on dummy variables for different payment methods. As described in Section 4, upfront payment of bail can be made through cash, property, or commercial surety bond. Therefore, we include indicator variables for property bond (*BondProp*) and commercial surety bond (*BondCorp*) in our regression, with cash payment as the omitted category.

We present the results of these regressions in Table XIII, which show the effect of the bail payment method on overall insolvency (in Panel A) and foreclosures (in Panel B) and Table XIV, which show the effect of the bail payment method on bankruptcies and judgment liens. Since property bonds and surety bonds are typically used for larger bail payments, as seen in Figure I(d), we include *lnBailAmt*, the log bail amount, as one of our control variables in these regressions, to avoid confounding effects related to the intensive margin of bail. We see that the estimates for *BondProp* and *BondCorp* are consistently positive and significant at longer horizons across all tables, with the point estimates for *BondCorp* slightly larger in most cases. This suggests that paying bail through a property or commercial surety bond is associated with higher rates of subsequent insolvency, foreclosures, bankruptcies, and judgment liens. In Figure VII, we further show that the bond type indicators are not associated with higher rates of insolvency events at various horizon *prior* to the initial hearing date, which mitigates concerns that payment by property or surety bond is correlated with pre-existing financial distress.

These findings suggest that having a lien placed on one's property by the court (in the case of a property bond) or shouldering the financial burden of commercial bond fees (in the case of a surety bond) places greater financial strain on defendants' household finances relative to the liquidity shock of a cash bond. However, we cannot conclusively claim a causal relationship between payment type and insolvency rates from these OLS tests, given that choice of bond type is endogenously determined and likely reflects the defendants' liquidity and ability to pay.

7 Conclusion

In this paper, we examine the effect of pretrial release on household financial solvency. We exploit the quasi-random assignment of commissioners to cases and construct a residualized leave-out mean measure of commissioner leniency to instrument for detention decisions. We find causal evidence that pretrial release is associated with lower rates of subsequent household insolvency events. Specifically, we find that pretrial release reduces the rate of foreclosures in periods with declining house prices (when defendants are less able to tap into their home equity), lowers rates of Chapter 7 bankruptcies, and lowers rates of judgment liens.

In addition, we find suggestive evidence that financial costs spill over to members of a defendant's household, consistent with anecdotal evidence of parents facing financial strains after acting as co-guarantors on their children's bail bonds. Our evidence also suggests that home equity acts as a buffer against insolvency, but housing debt makes defendants' households more vulnerable to default. Last, we find that insolvency rates are higher when defendants pay by pledging property or borrowing from commercial bail bondsmen than when paying with cash. Overall, our findings uncover a significant financial cost associated with pretrial detention, adding to the growing literature that tries to shed light on the unintended consequences imposed by the bail system.

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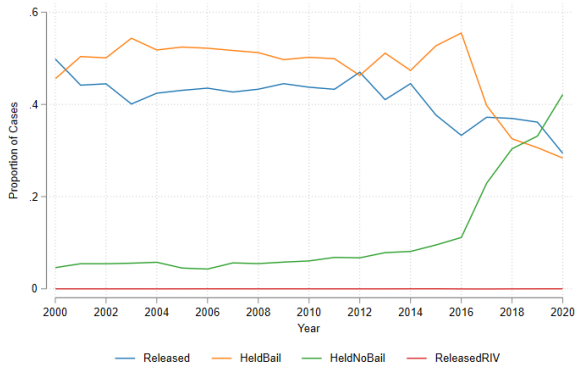
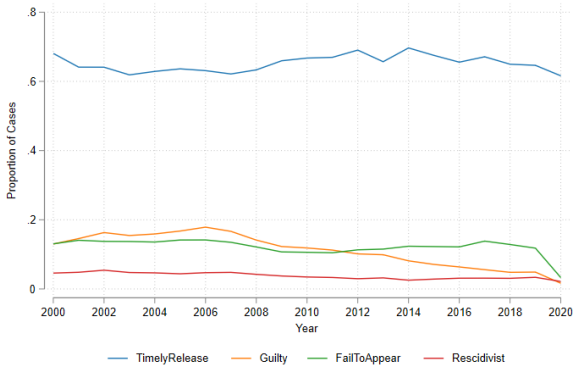
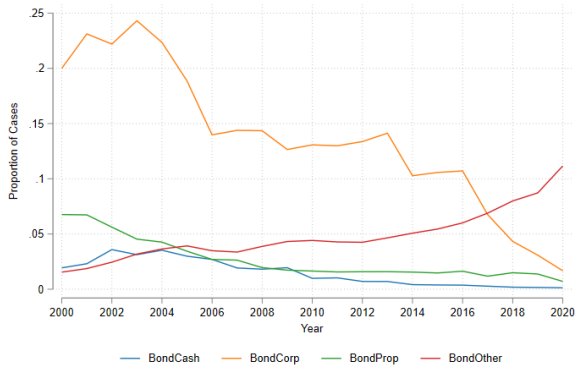
Figure I: Evolution of Selected Variables



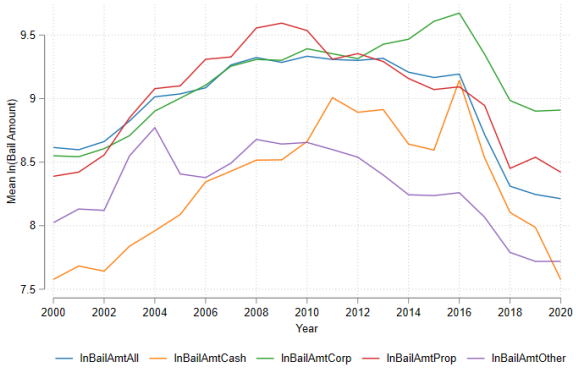
(a) Commissioner Decisions



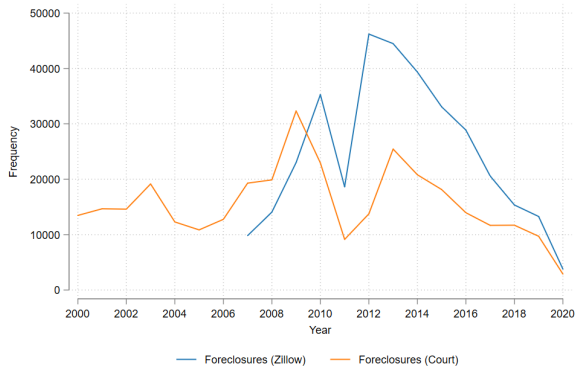
(b) Case Outcomes



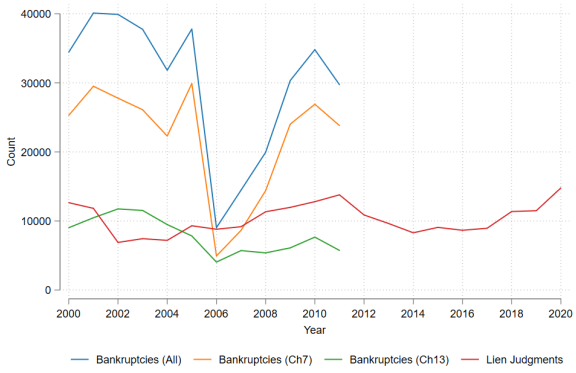
(c) Bail Payment Types



(d) Bail Amount by Payment Type



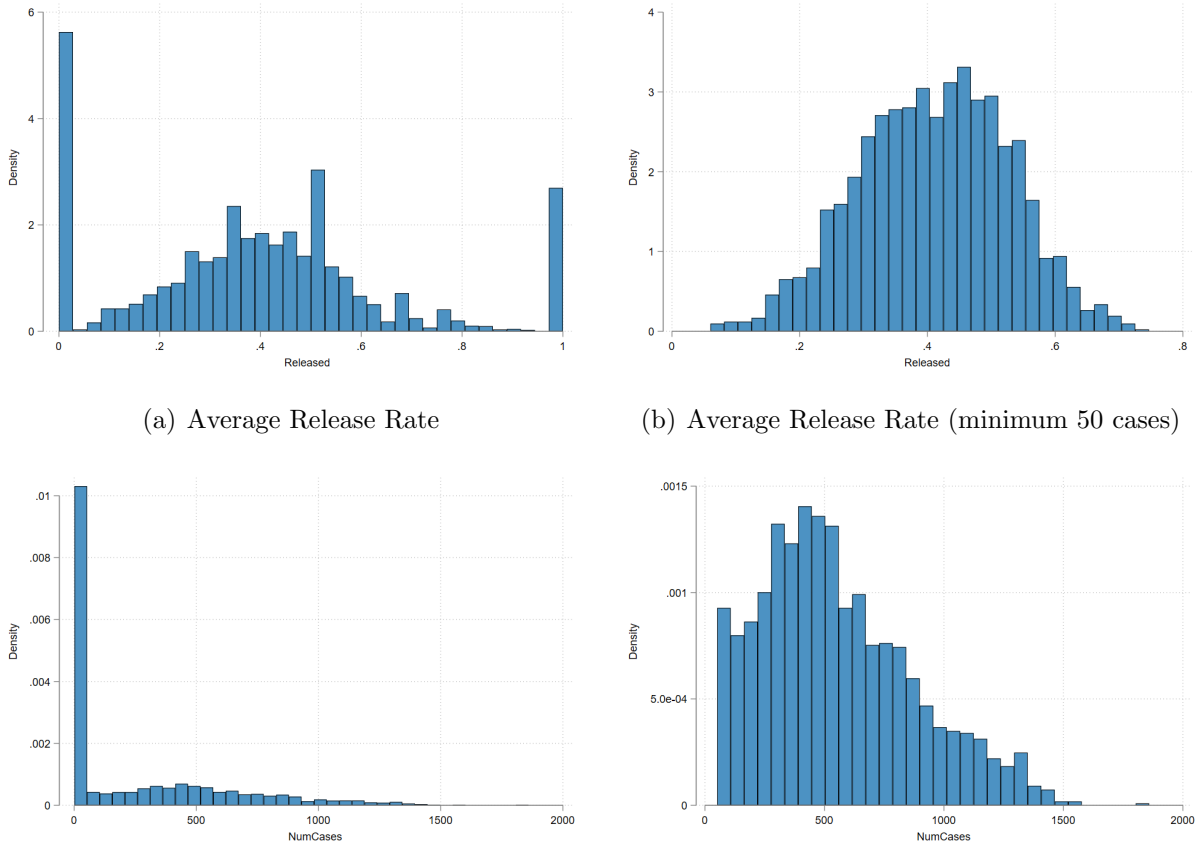
(e) Foreclosures



(f) Bankruptcy/Judgment Liens

This Figure plots the evolution of several variables of interest in our study. Panel (a) plots the evolution of the distribution of hearing outcomes over time (released on own recognizance, held without bond, or held in default of bond) and our instrumental variable. Panel (b) plots the evolution of case outcomes. Panel (c) plots the type of bail payments. Panel (d) plots the mean bail amount by type of bail posted. Panel (e) plots the number of foreclosures in two different sources of data, Zillow ZTRAX and court records. Panel (f) plots the number of bankruptcies and judgement liens

Figure II: Distribution of Commissioners' Release Rates and Case Counts



(a) Average Release Rate

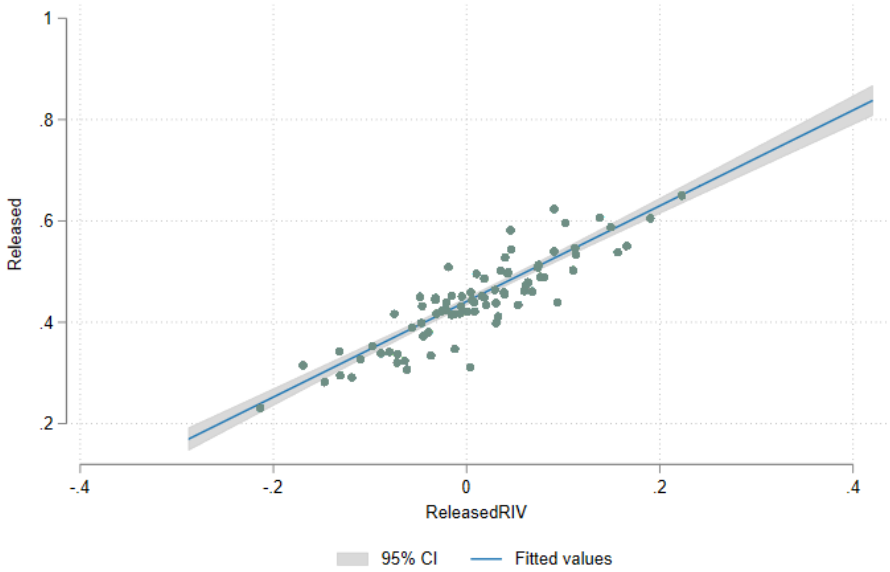
(b) Average Release Rate (minimum 50 cases)

(c) Number of Cases

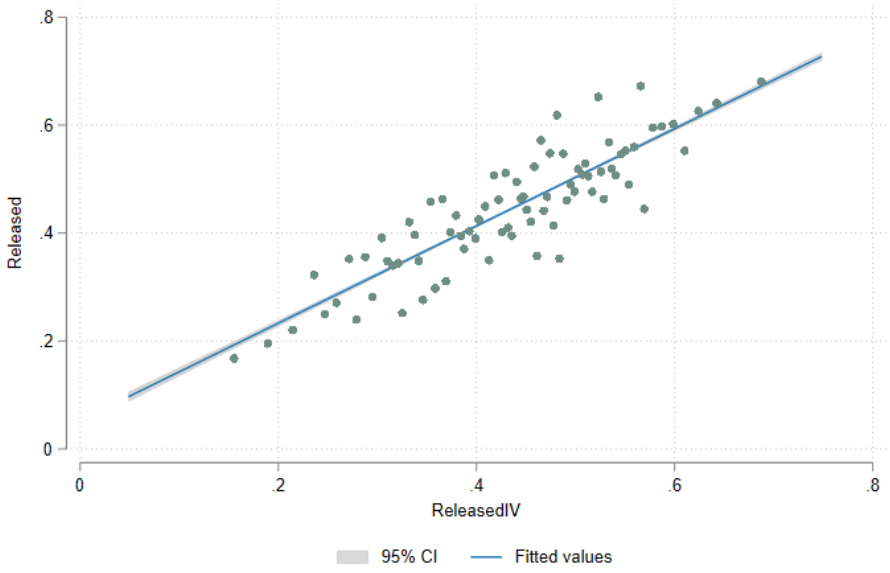
(d) Number of Cases (minimum 50 cases)

This Figure plots average release rates and number of cases for Commissioners in the State of Maryland. Panel (a) reports the distribution of commissioners' leniency rates calculated annually. In Panel (b) we restrict the sample to commissioner-years with a minimum of 50 cases. In Panel (c) we report the distribution of number of cases assigned to each commissioner in a given year. In Panel (d) the sample is restricted to commissioner-years with at least 50 cases.

Figure III: Relevance of Instrumental Variable



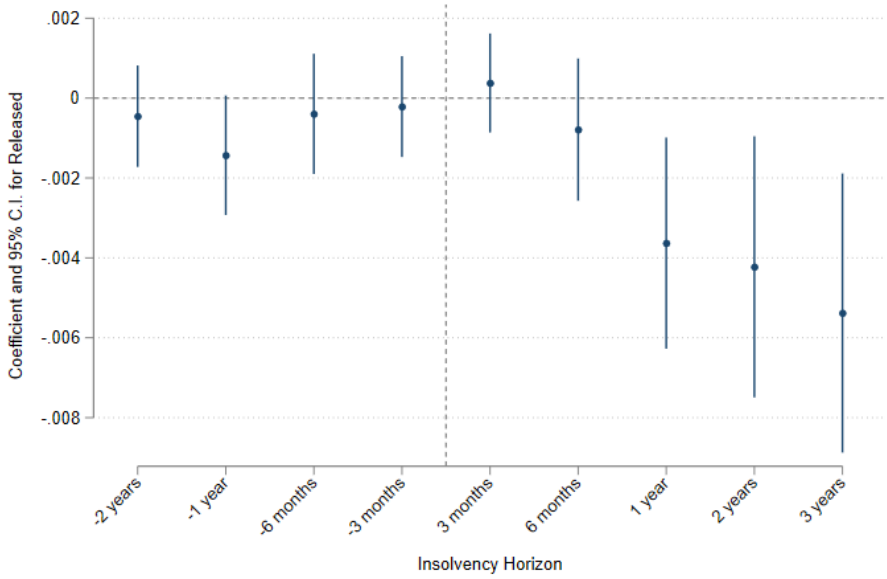
(a) ReleasedRIV



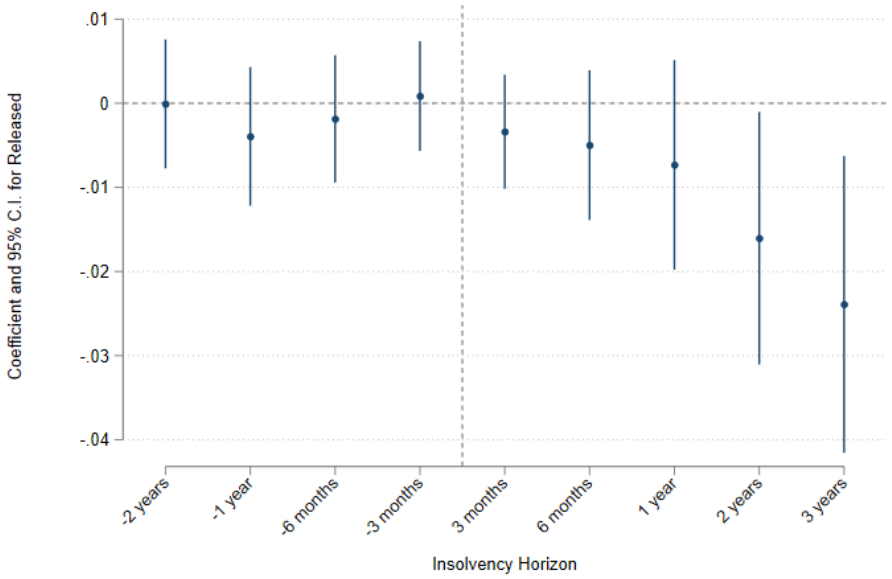
(b) ReleasedIV

This Figure shows the correlation between our instrumental variable and rates of release. Panel (A) plots this correlation for our residualized instrumental variable, *ReleasedRIV*, and release rates. Panel (B) plots this correlation for the non-residualized instrumental variable, *ReleasedIV* and release rates

Figure IV: Effect of Pretrial Release on Personal Insolvency



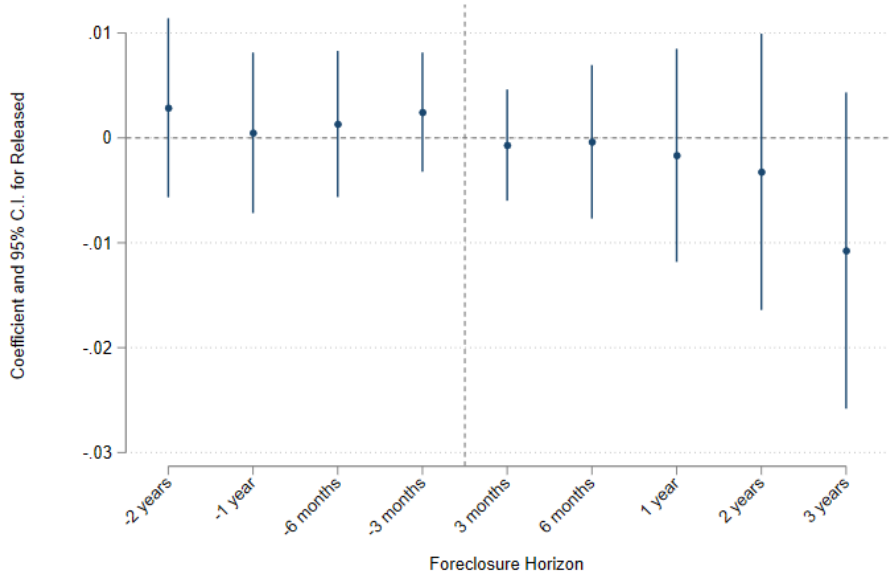
(a) OLS



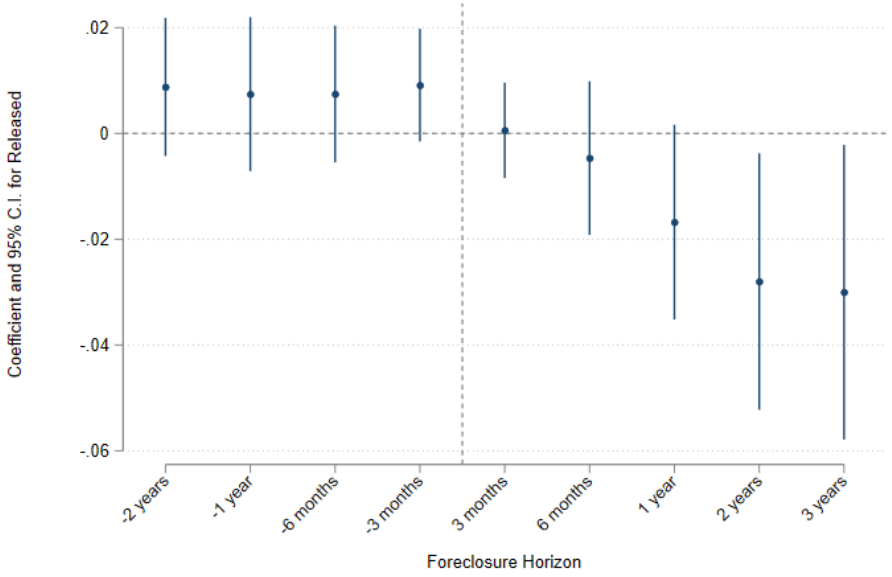
(b) 2SLS

This Figure plots the coefficients from our analysis of the effect of pretrial release on insolvency, as defined by the occurrence of a foreclosure, a bankrupcty, and/or a lien judgement at different horizons relative to the initial hearing date. Panel (A) plots the coefficients obtained from the Ordinary Least Squares analysis. Panel (B) plots the coefficients obtained from the Two-Stage Least Squares analysis.

Figure V: Effect of Pretrial Release on Foreclosures



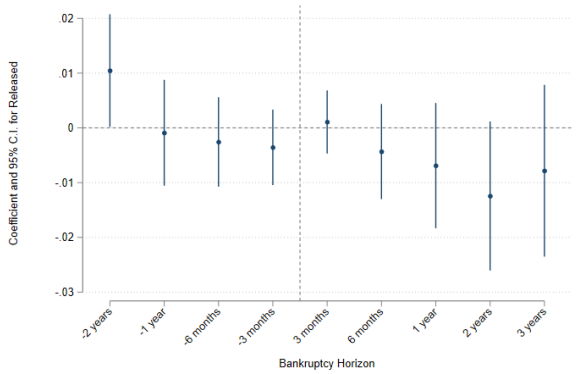
(a) Full Sample



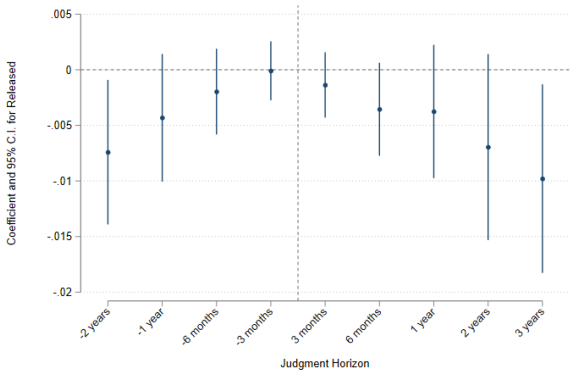
(b) Subperiod with Declining House Prices (2008-2012)

This Figure plots the coefficients from our two-stage least squares analysis of the effect of pretrial release on foreclosures at different horizons relative to the initial hearing date. Panel (A) plots the coefficients obtained from the analysis of the full sample period. Panel (B) plots the coefficients obtained from analysis of the subperiod with declining house prices (2008-2012).

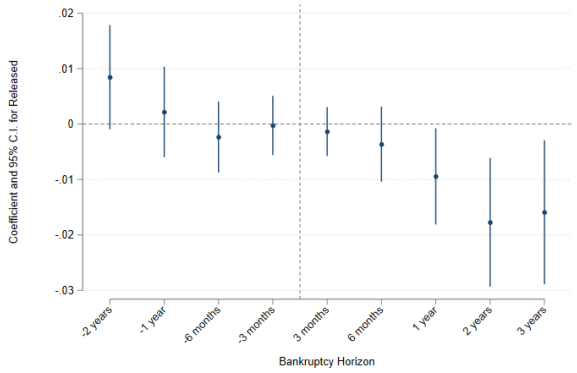
Figure VI: Effect of Pretrial Release on Bankruptcies and Judgment Liens



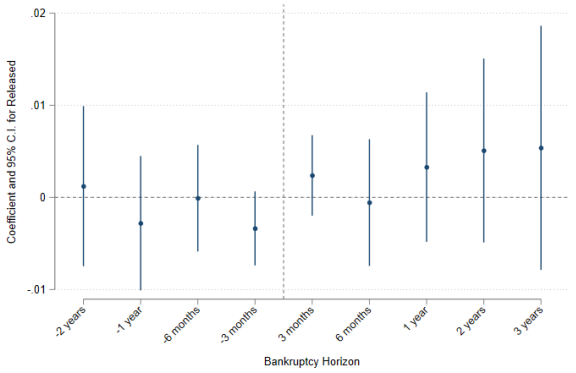
(a) All Bankruptcies



(b) Lien Judgments



(c) Ch. 7 Bankruptcies

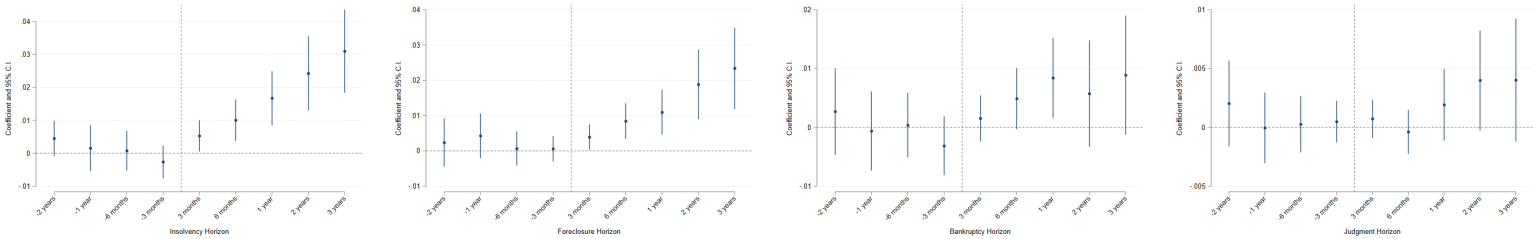


(d) Ch. 13 Bankruptcies

This Figure plots the coefficients from our two-stage least squares analysis of the effect of pretrial release on bankruptcy and lien judgements at different horizons relative to the initial hearing date. Panel (A) plots the coefficients for all bankruptcies. Panel (B) plots the coefficients for lien judgements. Panel (C) plots the coefficients for Chapter 7 bankruptcy. Panel (D) plots the coefficients for Chapter 13 bankruptcy.

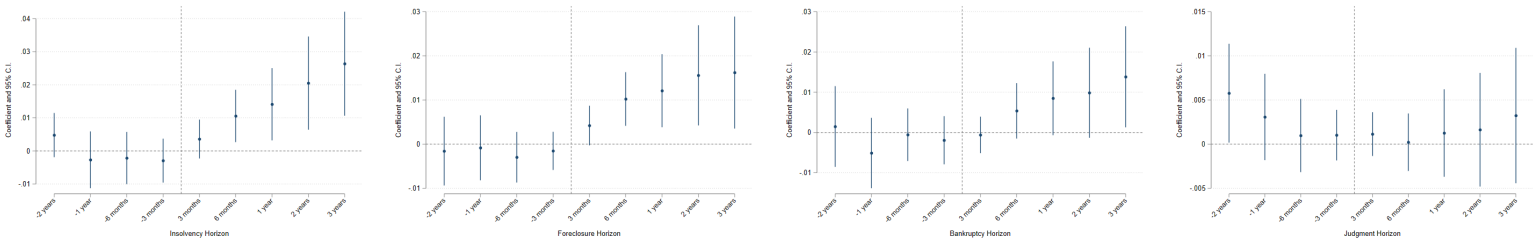
Figure VII: Bond Types and Insolvency

Panel A: Corporate Bonds



(a) Personal Insolvency (b) Foreclosures (c) Bankruptcies (d) Judgment Liens

Panel B: Property Bonds



(a) Personal Insolvency (b) Foreclosures (c) Bankruptcies (d) Judgment Liens

This Figure plots the coefficients from our analysis of the relationship between the type of bail posted and measures of personal insolvency at different horizons relative to the initial hearing date. Panel (A) plots the relationship between corporate bonds and personal insolvency. Panel (B) plots the relationship between corporate bonds and foreclosures. Panel (C) plots the relationship between corporate bonds and bankruptcies. Panel (D) plots the relationship between corporate bonds and judgement liens. Panel (E) plots the relationship between property bonds and personal insolvency. Panel (F) plots the relationship between property bonds and foreclosures. Panel (G) plots the relationship between property bonds and bankruptcies. Panel (H) plots the relationship between property bonds and judgement liens.

Table I: Descriptive Statistics

This table reports descriptive statistics for criminal cases in the State of Maryland (Panel A) and financial outcomes for defendants and their household members (Panel B). Criminal cases data are from the Maryland Judiciary System from 2000 to 2020. Data on Foreclosures, Bankruptcies, and Lien Judgements are from ZTRAX, the Public Access to Court Electronic Records system (PACER), and the State of Maryland's Judiciary system, respectively.

Panel A: Case Variables

	Observations	Mean	Std Dev	P25	Median	P75
Released	284,709	0.444	0.497	0.000	0.000	1.000
ReleasedRIV	276,365	0.001	0.081	-0.050	-0.000	0.050
ReleasedIV	284,709	0.434	0.111	0.356	0.445	0.514
Black	284,709	0.764	0.425	1.000	1.000	1.000
Male	284,709	0.839	0.367	1.000	1.000	1.000
lnAge	284,178	3.416	0.338	3.135	3.401	3.689
FelonyCharge	284,709	0.208	0.406	0.000	0.000	0.000
lnTimeDetained	253,042	0.671	1.198	0.000	0.000	0.693
TimelyRelease	284,709	0.690	0.463	0.000	1.000	1.000
Guilty	284,709	0.122	0.327	0.000	0.000	0.000
FailToAppear	284,709	0.108	0.311	0.000	0.000	0.000
Rescidivist	284,709	0.038	0.192	0.000	0.000	0.000
BailReview	284,709	0.353	0.478	0.000	0.000	1.000
BondCash	284,709	0.019	0.135	0.000	0.000	0.000
BondCorp	284,709	0.162	0.369	0.000	0.000	0.000
BondProp	284,709	0.028	0.166	0.000	0.000	0.000
BondOther	284,709	0.038	0.191	0.000	0.000	0.000

Panel B: Financial Variables

	Insolvency		Foreclosures		Bankruptcies		Judgment Liens	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
-3 Years	0.109	0.312	0.055	0.228	0.063	0.243	0.022	0.148
-2 Years	0.089	0.284	0.046	0.211	0.049	0.216	0.018	0.132
-1 Year	0.057	0.232	0.032	0.175	0.029	0.168	0.011	0.105
-6 Months	0.034	0.182	0.02	0.14	0.016	0.125	0.006	0.079
-3 Months	0.019	0.138	0.012	0.107	0.008	0.091	0.003	0.057
3 Months	0.02	0.139	0.012	0.109	0.007	0.086	0.004	0.06
6 Months	0.038	0.192	0.023	0.15	0.015	0.121	0.008	0.089
1 Year	0.071	0.258	0.043	0.203	0.028	0.164	0.015	0.123
2 Years	0.119	0.324	0.075	0.264	0.046	0.21	0.025	0.155
3 Years	0.153	0.36	0.1	0.3	0.059	0.235	0.032	0.175

Table II: Test of Randomization

This table reports results of the test of the random assignment of cases to commissioners. In Column 1 we report estimates from an OLS regression of pre-trial release on defendant's characteristics, the charge, and financial outcomes of the defendant, in addition to court-by-year and zipcode-by-year fixed effects. Column 2 reports estimates from an OLS regression on our non-residualized measure of commissioner's leniency on the same variables. Column 3 reports estimates from an OLS regression on our residualized measure of commissioner's leniency on the same variables.

	(1) Released	(2) ReleasedIV	(3) ReleasedRIV
lnAge	0.0158**	-0.0001	-0.0000
	(0.0079)	(0.0007)	(0.0007)
Black	-0.0224***	0.0006	-0.0002
	(0.0035)	(0.0006)	(0.0006)
Male	-0.1085***	-0.0027	-0.0011
	(0.0059)	(0.0026)	(0.0016)
FelonyCharge	-0.4220***	-0.0032***	-0.0003
	(0.0080)	(0.0007)	(0.0006)
InsolventPrev3yr	-0.0184*	-0.0012	-0.0014
	(0.0096)	(0.0019)	(0.0019)
ForeclosePrev3yr	0.0105	0.0009	0.0011
	(0.0090)	(0.0017)	(0.0017)
BankruptPrev3yr	0.0164*	0.0015	0.0012
	(0.0087)	(0.0017)	(0.0018)
LienPrev3yr	-0.0041	-0.0019	-0.0018
	(0.0102)	(0.0021)	(0.0023)
SellHousePrev3yr	-0.0036*	-0.0001	0.0001
	(0.0019)	(0.0004)	(0.0003)
Joint F-test	0.0000	0.0001	0.5059
Court-Year FE	X	X	X
Zip-Year FE	X	X	X
Observations	283,402	283,402	275,591
Adjusted R-squared	0.153	0.414	0.005

Table III: Instrument Relevance

This table reports results of the test of the relevance of our instrument. On each column, we present the coefficient of the first stage regression of our instrument on the decision of the commissioner to release the defendant on own recognizance. Column (1) reports the result for the whole sample. The following columns present the result for the subsamples of black defendants (Column (2)), non-black defendants (Column (3)), male defendants (Column (4)), female defendants (Column (5)), young defendants (Column (6)), and older defendants (Column (7)). In columns (8) to (10), we report the results for subsamples based on the location of the court. All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

	(1) All	(2) Black	(3) NonBlack	(4) Male	(5) Female	(6) Young	(7) Older	(8) Baltimore	(9) Montgomery	(10) PrinceGeorge
ReleasedRIV	0.9336***	0.9182***	0.9804***	0.9084***	1.0700***	0.8883***	0.9771***	0.9334***	0.9719***	0.9381***
	(0.0118)	(0.0144)	(0.0278)	(0.0148)	(0.0354)	(0.0180)	(0.0190)	(0.0148)	(0.0301)	(0.0239)
F-statistic	6305.53	4078.64	1242.06	3774.72	914.08	2427.38	2654.46	3974.92	1043.32	1534.11
Court-Year FE	X	X	X	X	X	X	X	X	X	X
Zip-Year FE	X	X	X	X	X	X	X	X	X	X
Month FE	X	X	X	X	X	X	X	X	X	X
DOW FE	X	X	X	X	X	X	X	X	X	X
Sex FE	X	X	X	X	X	X	X	X	X	X
Race FE	X	X	X	X	X	X	X	X	X	X
Charge FE	X	X	X	X	X	X	X	X	X	X
Observations	275,325	210,908	63,569	231,031	43,393	136,021	138,445	182,289	35,258	55,893
Adjusted R-squared	0.330	0.336	0.318	0.341	0.257	0.354	0.318	0.375	0.275	0.249

Table IV: Effect of Pretrial Release on Pre-trial Outcomes

This table reports OLS (Panel A) and two-stage least squares (Panel B) results of the effect of pre-trial release on pre-trial outcomes. In Panel B, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. The dependent variables are timely release (column 1), the logarithm of the number of days a defendant is detained (column 2), the likelihood of pleading guilty (column 3), the likelihood of failing to appear in court for the trial (column 4), and the likelihood of recidivism (column 5). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: OLS

	(1) TimelyRelease	(2) lnTimeDetained	(3) Guilty	(4) FailToAppear	(5) Rescidivist
Released	0.4960***	-1.2783***	-0.0055***	0.0719***	-0.0124***
	(0.0055)	(0.0144)	(0.0021)	(0.0023)	(0.0013)
Mean(dep. var.)	0.6908	0.6653	0.1214	0.1080	0.0389
SD(dep. var.)	0.4621	1.1921	0.3266	0.3104	0.1934
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	275,820	244,907	275,820	275,820	275,820
Adjusted R-squared	0.412	0.351	0.094	0.072	0.024

Panel B: 2SLS

	(1) TimelyRelease	(2) lnTimeDetained	(3) Guilty	(4) FailToAppear	(5) Rescidivist
Released	0.4878***	-1.2008***	-0.0291***	0.1186***	-0.0206***
	(0.0194)	(0.0595)	(0.0107)	(0.0100)	(0.0046)
Mean(dep. var.)	0.6908	0.6649	0.1215	0.1081	0.0389
SD(dep. var.)	0.4622	1.1917	0.3267	0.3105	0.1934
1st Stage F-Stat	6305.53	5438.16	6305.53	6305.53	6305.53
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	275,325	244,438	275,325	275,325	275,325

Table V: Effect of Pretrial Release on Bail Setting

This table reports OLS (Panel A) and two-stage least squares (Panel B) results of the effect of pre-trial release on bail setting. In Panel B, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. The dependent variables are the likelihood of a request for a bail review (column 1), the likelihood that a cash bond is pledged (column 2), the likelihood that a corporate bond is pledged (column 3), the likelihood that a property bond is pledged (column 4), and the likelihood that another type of bond is pledged (column 5). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: OLS

	(1) BailReview	(2) BondCash	(3) BondCorp	(4) BondProp	(5) BondOther
Released	-0.3997***	-0.0093***	-0.2580***	-0.0391***	-0.0391***
	(0.0117)	(0.0009)	(0.0086)	(0.0035)	(0.0037)
Mean(dep. var.)	0.3536	0.0185	0.1652	0.0288	0.0379
SD(dep. var.)	0.4781	0.1346	0.3714	0.1672	0.1910
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	275,820	275,820	275,820	275,820	275,820
Adjusted R-squared	0.254	0.019	0.162	0.097	0.137

Panel B: 2SLS

	(1) BailReview	(2) BondCash	(3) BondCorp	(4) BondProp	(5) BondOther
Released	-0.3317***	-0.0152***	-0.2089***	-0.0380***	-0.0306***
	(0.0677)	(0.0038)	(0.0300)	(0.0077)	(0.0075)
Mean(dep. var.)	0.3536	0.0185	0.1652	0.0288	0.0379
SD(dep. var.)	0.4781	0.1346	0.3713	0.1672	0.1910
1st Stage F-Stat	6305.53	6305.53	6305.53	6305.53	6305.53
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	275,325	275,325	275,325	275,325	275,325

Table VI: Effect of Pretrial Release on Personal Insolvency

This table reports OLS (Panel A) and two-stage least squares (Panel B) results of the effect of pre-trial release on personal insolvency at the household level. In Panel B, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. The dependent variables are the probability of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: OLS

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	0.0004	-0.0008	-0.0036***	-0.0042**	-0.0054***	-0.0066***
	(0.0006)	(0.0009)	(0.0013)	(0.0017)	(0.0018)	(0.0020)
InsolventPrev3yr	0.0448***	0.0734***	0.1095***	0.1385***	0.1497***	0.1496***
	(0.0014)	(0.0022)	(0.0027)	(0.0033)	(0.0035)	(0.0040)
SellHousePrev3yr	0.0031***	0.0053***	0.0111***	0.0179***	0.0209***	0.0123***
	(0.0006)	(0.0008)	(0.0011)	(0.0015)	(0.0018)	(0.0023)
Mean(dep. var.)	0.0196	0.0383	0.0712	0.1189	0.1533	0.2470
SD(dep. var.)	0.1387	0.1919	0.2572	0.3237	0.3603	0.4313
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	275,820	275,820	275,820	275,820	275,820	275,820
Adjusted R-squared	0.021	0.030	0.045	0.057	0.063	0.066

Panel B: 2SLS

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0034	-0.0050	-0.0073	-0.0160**	-0.0239***	-0.0191*
	(0.0034)	(0.0045)	(0.0063)	(0.0076)	(0.0090)	(0.0107)
InsolventPrev3yr	0.0447***	0.0733***	0.1094***	0.1384***	0.1497***	0.1496***
	(0.0014)	(0.0022)	(0.0027)	(0.0034)	(0.0035)	(0.0041)
SellHousePrev3yr	0.0031***	0.0053***	0.0111***	0.0179***	0.0209***	0.0123***
	(0.0006)	(0.0008)	(0.0011)	(0.0015)	(0.0018)	(0.0023)
Mean(dep. var.)	0.0196	0.0383	0.0712	0.1189	0.1533	0.2470
SD(dep. var.)	0.1387	0.1919	0.2572	0.3236	0.3603	0.4313
1st Stage F-Stat	6301.78	6301.78	6301.78	6301.78	6301.78	6301.78
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	275,325	275,325	275,325	275,325	275,325	275,325

Table VII: Effect of Pretrial Release on Foreclosures

This table reports two-stage least squares results of the effect of pre-trial release on foreclosures. Panel A reports the results for the entire sample period. In each Panel, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. Panel B reports the results for the subperiod with declining house prices (2008-2012). The dependent variables are the probability of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Full Sample

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0007	-0.0004	-0.0017	-0.0032	-0.0107	-0.0110
	(0.0027)	(0.0037)	(0.0052)	(0.0067)	(0.0077)	(0.0097)
SellHousePrev3yr	0.0023***	0.0038***	0.0082***	0.0155***	0.0191***	0.0190***
	(0.0005)	(0.0007)	(0.0009)	(0.0013)	(0.0016)	(0.0021)
InsolventPrev3yr	0.0301***	0.0501***	0.0773***	0.1036***	0.1150***	0.1260***
	(0.0014)	(0.0019)	(0.0024)	(0.0030)	(0.0030)	(0.0032)
Mean(dep. var.)	0.0121	0.0231	0.0433	0.0755	0.1002	0.1757
SD(dep. var.)	0.1094	0.1503	0.2034	0.2642	0.3002	0.3805
1st Stage F-Stat	6301.78	6301.78	6301.78	6301.78	6301.78	6301.78
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	275,325	275,325	275,325	275,325	275,325	275,325

Panel B: Housing Crisis Period (2008-2012)

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	0.0006	-0.0047	-0.0168*	-0.0280**	-0.0300**	-0.0392**
	(0.0046)	(0.0074)	(0.0093)	(0.0123)	(0.0141)	(0.0193)
SellHousePrev3yr	0.0041***	0.0071***	0.0152***	0.0275***	0.0306***	0.0391***
	(0.0008)	(0.0012)	(0.0014)	(0.0020)	(0.0022)	(0.0030)
InsolventPrev3yr	0.0370***	0.0569***	0.0893***	0.1238***	0.1379***	0.1646***
	(0.0023)	(0.0031)	(0.0037)	(0.0042)	(0.0046)	(0.0051)
Mean(dep. var.)	0.0150	0.0282	0.0557	0.1020	0.1380	0.2277
SD(dep. var.)	0.1217	0.1655	0.2294	0.3026	0.3449	0.4194
1st Stage F-Stat	4291.73	4291.73	4291.73	4291.73	4291.73	4291.73
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103,900	103,900	103,900	103,900	103,900	103,900

Table VIII: Effect of Pretrial Release on Bankruptcies and Judgment Liens

This table reports two-stage least squares results of the effect of pre-trial release on personal bankruptcy (Panel A) and lien judgements (Panel B). In each Panel, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. The dependent variables are the probability of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Bankruptcies

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	0.0011	-0.0043	-0.0069	-0.0124*	-0.0078	-0.0007
	(0.0029)	(0.0044)	(0.0058)	(0.0069)	(0.0080)	(0.0098)
InsolventPrev3yr	0.0166***	0.0255***	0.0403***	0.0491***	0.0506***	0.0459***
	(0.0015)	(0.0020)	(0.0029)	(0.0036)	(0.0037)	(0.0039)
SellHousePrev3yr	0.0017***	0.0029***	0.0046***	0.0063***	0.0072***	0.0046***
	(0.0005)	(0.0007)	(0.0010)	(0.0013)	(0.0015)	(0.0018)
Mean(dep. var.)	0.0074	0.0148	0.0279	0.0464	0.0586	0.0751
SD(dep. var.)	0.0859	0.1208	0.1646	0.2104	0.2350	0.2636
1st Stage F-Stat	2537.85	2537.85	2537.85	2537.85	2537.85	2537.85
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	132,466	132,466	132,466	132,466	132,466	132,466

Panel B: Judgment Liens

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0014	-0.0036*	-0.0038	-0.0070	-0.0098**	-0.0075
	(0.0015)	(0.0021)	(0.0030)	(0.0043)	(0.0043)	(0.0050)
InsolventPrev3yr	0.0068***	0.0129***	0.0193***	0.0246***	0.0279***	0.0266***
	(0.0010)	(0.0018)	(0.0027)	(0.0034)	(0.0038)	(0.0040)
SellHousePrev3yr	0.0002	0.0004	0.0014***	0.0016**	0.0011	-0.0039***
	(0.0003)	(0.0004)	(0.0005)	(0.0006)	(0.0007)	(0.0009)
Mean(dep. var.)	0.0035	0.0078	0.0152	0.0244	0.0312	0.0524
SD(dep. var.)	0.0593	0.0878	0.1225	0.1544	0.1739	0.2227
1st Stage F-Stat	6301.78	6301.78	6301.78	6301.78	6301.78	6301.78
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	275,325	275,325	275,325	275,325	275,325	275,325

Table IX: Effect of Pretrial Release on Types of Bankruptcies

This table reports two-stage least squares results of the effect of pre-trial release on Chapter 7 bankruptcy (Panel A) and Chapter 13 Bankruptcy (Panel B). In each Panel, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. The dependent variables are the probability of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Ch. 7 Bankruptcies

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0014	-0.0036	-0.0094**	-0.0177***	-0.0159**	-0.0094
	(0.0022)	(0.0034)	(0.0044)	(0.0059)	(0.0066)	(0.0073)
InsolventPrev3yr	0.0024***	0.0027**	0.0043***	0.0046**	0.0047**	0.0020
	(0.0008)	(0.0012)	(0.0014)	(0.0018)	(0.0021)	(0.0027)
SellHousePrev3yr	0.0002	0.0006	0.0008	0.0015*	0.0019*	0.0000
	(0.0003)	(0.0005)	(0.0007)	(0.0009)	(0.0011)	(0.0015)
Mean(dep. var.)	0.0035	0.0074	0.0144	0.0257	0.0340	0.0478
SD(dep. var.)	0.0592	0.0857	0.1190	0.1583	0.1812	0.2133
1st Stage F-Stat	2537.85	2537.85	2537.85	2537.85	2537.85	2537.85
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	132,466	132,466	132,466	132,466	132,466	132,466

Panel B: Ch. 13 Bankruptcies

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	0.0024	-0.0006	0.0033	0.0051	0.0054	0.0065
	(0.0022)	(0.0035)	(0.0041)	(0.0051)	(0.0067)	(0.0086)
InsolventPrev3yr	0.0142***	0.0231***	0.0369***	0.0484***	0.0513***	0.0533***
	(0.0012)	(0.0016)	(0.0025)	(0.0030)	(0.0031)	(0.0033)
SellHousePrev3yr	0.0015***	0.0022***	0.0039***	0.0055***	0.0065***	0.0068***
	(0.0003)	(0.0005)	(0.0007)	(0.0009)	(0.0010)	(0.0012)
Mean(dep. var.)	0.0039	0.0075	0.0138	0.0221	0.0270	0.0328
SD(dep. var.)	0.0624	0.0862	0.1168	0.1468	0.1622	0.1782
1st Stage F-Stat	2537.85	2537.85	2537.85	2537.85	2537.85	2537.85
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	132,466	132,466	132,466	132,466	132,466	132,466

Table X: Effect of Pretrial Release on Personal Insolvency Across Subsamples

These tables report two-stage least squares results of the effect of pre-trial release on personal insolvency at a three-year horizon from the initial hearing date for different subsamples. In each Panel, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. Panel A reports the result for defendants under 30 years of age (column 1), defendants over 30 years of age (column 2), black defendants (column 3), non-black defendants (column 4), male defendants (column 5), female defendants (column 6), defendants under the age of 25 when their current home was purchased (column 7), and defendants over the age of 25 when their current home was purchased (column 8). Panel B reports the results for defendants from homes with above-median purchase prices (column 1), defendants from homes with below-median purchase prices (column 2), defendants from homes purchased with a listed mortgage (column 3), defendants from homes purchased without a listed mortgage (column 4), defendants from homes with above-median mortgage maturity (column 5), defendants from homes with below-median mortgage maturity (column 5) cases prior to 2008 (column 7), and cases after 2008 (column 8) All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Defendant Characteristics

	(1) Young	(2) Old	(3) Black	(4) NonBlack	(5) Male	(6) Female	(7) YoungPurchase	(8) OldPurchase
Released	-0.0283*	-0.0195*	-0.0213*	-0.0384**	-0.0244**	-0.0132	-0.0295**	-0.0179
	(0.0144)	(0.0114)	(0.0111)	(0.0178)	(0.0109)	(0.0181)	(0.0136)	(0.0119)
Mean(dep. var.)	0.1546	0.1521	0.1484	0.1694	0.1538	0.1509	0.1502	0.1564
SD(dep. var.)	0.3615	0.3592	0.3555	0.3751	0.3607	0.3579	0.3573	0.3633
1st Stage F-Stat	2426.80	2656.88	4079.19	1235.99	3774.00	914.02	2638.48	2657.39
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136,021	138,445	210,908	63,569	231,031	43,393	137,536	136,914

Panel B: Housing Characteristics

	(1) HighPrice	(2) LowPrice	(3) Mortgage	(4) NoMortgage	(5) LongMaturity	(6) ShortMaturity	(7) Pre2008	(8) Post2008
Released	-0.0531***	-0.0013	-0.0364**	-0.0162	-0.0503*	-0.0238	-0.0148	-0.0322**
	(0.0184)	(0.0160)	(0.0153)	(0.0117)	(0.0276)	(0.0201)	(0.0123)	(0.0130)
Mean(dep. var.)	0.1780	0.1163	0.1713	0.1307	0.2085	0.1508	0.1482	0.1579
SD(dep. var.)	0.3825	0.3206	0.3767	0.3370	0.4063	0.3579	0.3553	0.3647
1st Stage F-Stat	1691.54	1526.29	3314.97	2794.44	1083.92	1926.38	2537.85	4327.58
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	68,166	67,845	118,708	137,997	46,046	45,869	132,466	142,717

Table XI: Insolvency Events with Name Matching

This table reports two-stage least squares results of the effect of pre-trial release on personal insolvency, foreclosures, Chapter 7 Bankruptcies, and Lien Judgements (indicated by column headings) at a three-year horizon from the initial hearing date. In each Panel, *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency, instruments for *Released*, an indicator for whether the defendant is released on own recognizance. In Panel A, all insolvency events are defined based on matching by address and by first and last names. In Panel B, all insolvency events are defined based on matching by address and by last name only. In Panel B, all insolvency events are defined based on matching by address and by different first or last names. The sample includes addresses with multiple units (i.e., including an apartment number). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Full Name Match

	(1) Insolvent	(2) Foreclosure	(3) Bankrupt7	(4) JudgmentLien
Released	-0.0023	-0.0020	0.0023	-0.0036**
	(0.0031)	(0.0034)	(0.0021)	(0.0018)
Mean(dep. var.)	0.0159	0.0051	0.0043	0.0069
SD(dep. var.)	0.1252	0.0716	0.0652	0.0825
1st Stage F-Stat	6301.78	4291.73	2537.85	6301.78
Fixed Effects	Yes	Yes	Yes	Yes
Observations	275,325	103,900	132,466	275,325

Panel B: Last Name Match

	(1) Insolvent	(2) Foreclosure	(3) Bankrupt7	(4) JudgmentLien
Released	-0.0096*	-0.0060	-0.0040	-0.0066***
	(0.0051)	(0.0067)	(0.0039)	(0.0025)
Mean(dep. var.)	0.0159	0.0051	0.0043	0.0069
SD(dep. var.)	0.1252	0.0716	0.0652	0.0825
1st Stage F-Stat	6301.78	4291.73	2537.85	6301.78
Fixed Effects	Yes	Yes	Yes	Yes
Observations	275,325	103,900	132,466	275,325

Panel C: Different Names

	(1) Insolvent	(2) Foreclosure	(3) Bankrupt7	(4) JudgmentLien
Released	-0.0143*	-0.0240*	-0.0119**	-0.0032
	(0.0084)	(0.0133)	(0.0054)	(0.0035)
Mean(dep. var.)	0.0159	0.0051	0.0043	0.0069
SD(dep. var.)	0.1252	0.0716	0.0652	0.0825
1st Stage F-Stat	6301.78	4291.73	2537.85	6301.78
Fixed Effects	Yes	Yes	Yes	Yes
Observations	275,325	103,900	132,466	275,325

Table XII: Detention Length vs. Monetary Bail Channel

These tables report two-stage least squares results of the effect of detention length (Panel A) and upfront bond payment (Panel B) on personal insolvency at various horizons from the initial hearing date (indicated by column headings). In each Panel, the instrument is *ReleasedRIV*, the residualized leave-out mean measure of commissioner leniency. The instrumented variable in Panel A is *lnTimeDetained*, the natural log of time detained, and the instrumented variable in Panel B is *BailBond*, an indicator variable for whether the defendant paid the full upfront bail. All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Detention Length Channel

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
lnTimeDetained	0.0025	0.0048	0.0060	0.0141**	0.0220***	0.0190**
	(0.0029)	(0.0039)	(0.0053)	(0.0064)	(0.0075)	(0.0086)
InsolventPrev3yr	0.0448***	0.0737***	0.1098***	0.1405***	0.1525***	0.1521***
	(0.0015)	(0.0023)	(0.0028)	(0.0034)	(0.0037)	(0.0042)
SellHousePrev3yr	0.0031***	0.0052***	0.0106***	0.0174***	0.0206***	0.0123***
	(0.0006)	(0.0009)	(0.0012)	(0.0016)	(0.0019)	(0.0024)
Mean(dep. var.)	0.0197	0.0383	0.0714	0.1196	0.1541	0.2472
SD(dep. var.)	0.1389	0.1920	0.2575	0.3245	0.3610	0.4314
1st Stage F-Stat	340.75	340.75	340.75	340.75	340.75	340.75
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	244,438	244,438	244,438	244,438	244,438	244,438

Panel B: Bail Bond Channel

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
BailBond	0.0129	0.0190	0.0279	0.0612**	0.0912**	0.0730*
	(0.0135)	(0.0176)	(0.0245)	(0.0301)	(0.0357)	(0.0407)
InsolventPrev3yr	0.0448***	0.0734***	0.1094***	0.1385***	0.1498***	0.1497***
	(0.0014)	(0.0022)	(0.0027)	(0.0034)	(0.0035)	(0.0041)
SellHousePrev3yr	0.0031***	0.0053***	0.0111***	0.0178***	0.0208***	0.0122***
	(0.0006)	(0.0008)	(0.0011)	(0.0015)	(0.0018)	(0.0023)
Mean(dep. var.)	0.0196	0.0383	0.0712	0.1189	0.1533	0.2470
SD(dep. var.)	0.1387	0.1919	0.2572	0.3236	0.3603	0.4313
1st Stage F-Stat	62.78	62.78	62.78	62.78	62.78	62.78
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	275,325	275,325	275,325	275,325	275,325	275,325

Table XIII: Bail Payment Method and Insolvency/Foreclosures

This table reports OLS results of the analysis of the relationship between the type of bail posted, personal insolvency (Panel A), and Foreclosures (Panel B). The dependent variables are the probability of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects. The omitted category is cash bail.

Panel A: Personal Insolvency

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
BondProp	0.0036	0.0106***	0.0141**	0.0205***	0.0264***	0.0382***
	(0.0030)	(0.0040)	(0.0056)	(0.0072)	(0.0080)	(0.0100)
BondCorp	0.0053**	0.0100***	0.0167***	0.0242***	0.0310***	0.0446***
	(0.0024)	(0.0032)	(0.0042)	(0.0057)	(0.0064)	(0.0086)
InsolventPrev3yr	0.0409***	0.0682***	0.1017***	0.1281***	0.1375***	0.1274***
	(0.0035)	(0.0046)	(0.0056)	(0.0069)	(0.0066)	(0.0067)
SellHousePrev3yr	0.0031**	0.0058***	0.0127***	0.0190***	0.0242***	0.0170***
	(0.0014)	(0.0021)	(0.0027)	(0.0035)	(0.0039)	(0.0042)
lnBailAmt	-0.0001	-0.0005	-0.0003	0.0000	0.0005	0.0001
	(0.0006)	(0.0008)	(0.0011)	(0.0014)	(0.0015)	(0.0018)
Mean(dep. var.)	0.0200	0.0383	0.0736	0.1230	0.1562	0.2532
SD(dep. var.)	0.1399	0.1918	0.2611	0.3284	0.3631	0.4348
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,646	48,646	48,646	48,646	48,646	48,646
Adjusted R-squared	0.023	0.031	0.048	0.056	0.064	0.062

Panel B: Foreclosures

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
BondProp	0.0042*	0.0102***	0.0121***	0.0156***	0.0162**	0.0246***
	(0.0023)	(0.0031)	(0.0042)	(0.0058)	(0.0064)	(0.0086)
BondCorp	0.0039**	0.0084***	0.0109***	0.0188***	0.0234***	0.0304***
	(0.0018)	(0.0026)	(0.0032)	(0.0050)	(0.0058)	(0.0079)
InsolventPrev3yr	0.0252***	0.0396***	0.0640***	0.0897***	0.1009***	0.1056***
	(0.0029)	(0.0038)	(0.0043)	(0.0059)	(0.0057)	(0.0064)
SellHousePrev3yr	0.0019*	0.0023	0.0085***	0.0150***	0.0191***	0.0202***
	(0.0011)	(0.0015)	(0.0020)	(0.0028)	(0.0031)	(0.0037)
lnBailAmt	-0.0002	-0.0007	-0.0009	-0.0012	-0.0005	-0.0000
	(0.0005)	(0.0006)	(0.0009)	(0.0011)	(0.0012)	(0.0016)
Mean(dep. var.)	0.0120	0.0225	0.0432	0.0767	0.1003	0.1824
SD(dep. var.)	0.1087	0.1482	0.2033	0.2660	0.3005	0.3862
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,646	48,646	48,646	48,646	48,646	48,646
Adjusted R-squared	0.007	0.014	0.025	0.036	0.044	0.053

Table XIV: Bail Payment Method and Bankruptcy/Judgments

This table reports OLS results of the analysis of the relationship between the type of bail posted, bankruptcy (Panel A), and lien judgements (Panel B). The dependent variables are the probability of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects. The omitted category is cash bail.

Panel A: Bankruptcies

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
BondProp	-0.0006	0.0054	0.0085*	0.0099*	0.0138**	0.0147**
	(0.0023)	(0.0035)	(0.0047)	(0.0057)	(0.0064)	(0.0075)
BondCorp	0.0015	0.0049*	0.0084**	0.0057	0.0089*	0.0124**
	(0.0020)	(0.0026)	(0.0035)	(0.0046)	(0.0051)	(0.0058)
InsolventPrev3yr	0.0190***	0.0323***	0.0490***	0.0567***	0.0583***	0.0520***
	(0.0030)	(0.0038)	(0.0056)	(0.0066)	(0.0069)	(0.0070)
SellHousePrev3yr	0.0027**	0.0042***	0.0062***	0.0069***	0.0086***	0.0041
	(0.0011)	(0.0016)	(0.0020)	(0.0023)	(0.0027)	(0.0029)
lnBailAmt	0.0000	0.0001	0.0008	0.0012	0.0016	0.0017
	(0.0004)	(0.0006)	(0.0009)	(0.0012)	(0.0012)	(0.0013)
Mean(dep. var.)	0.0087	0.0177	0.0336	0.0541	0.0664	0.0821
SD(dep. var.)	0.0929	0.1318	0.1803	0.2263	0.2490	0.2746
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,632	35,632	35,632	35,632	35,632	35,632
Adjusted R-squared	0.008	0.019	0.036	0.045	0.053	0.063

Panel B: Judgment Liens

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
BondProp	0.0011	0.0002	0.0013	0.0016	0.0032	0.0110**
	(0.0013)	(0.0017)	(0.0025)	(0.0033)	(0.0039)	(0.0046)
BondCorp	0.0007	-0.0004	0.0019	0.0040*	0.0040	0.0109***
	(0.0008)	(0.0010)	(0.0015)	(0.0022)	(0.0027)	(0.0036)
InsolventPrev3yr	0.0036***	0.0089***	0.0130***	0.0152***	0.0170***	0.0146***
	(0.0012)	(0.0021)	(0.0029)	(0.0034)	(0.0037)	(0.0038)
SellHousePrev3yr	-0.0006	0.0005	0.0010	0.0015	0.0011	-0.0025
	(0.0005)	(0.0007)	(0.0010)	(0.0011)	(0.0012)	(0.0017)
lnBailAmt	-0.0001	-0.0001	0.0001	0.0001	-0.0002	-0.0005
	(0.0002)	(0.0003)	(0.0004)	(0.0005)	(0.0005)	(0.0007)
Mean(dep. var.)	0.0025	0.0051	0.0112	0.0176	0.0221	0.0374
SD(dep. var.)	0.0498	0.0712	0.1051	0.1316	0.1471	0.1896
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,646	48,646	48,646	48,646	48,646	48,646
Adjusted R-squared	0.126	0.128	0.146	0.178	0.205	0.232

Internet Appendix A

Table A.I: Non-Residualized Instrumental Variable

This table reports results of the effect of pre-trial release on personal insolvency (Panel A), Foreclosures (Panel B), Chapter 7 Bankruptcies (Panel C), and Lien Judgements (Panel D) using the non-residualized instrumental variable. The dependent variables are the likelihood of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Insolvency

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0035	-0.0063	-0.0131*	-0.0250***	-0.0314***	-0.0262**
	(0.0039)	(0.0050)	(0.0069)	(0.0080)	(0.0095)	(0.0118)
InsolventPrev3yr	0.0448***	0.0734***	0.1094***	0.1384***	0.1496***	0.1495***
	(0.0014)	(0.0022)	(0.0027)	(0.0033)	(0.0035)	(0.0040)
SellHousePrev3yr	0.0031***	0.0053***	0.0111***	0.0179***	0.0209***	0.0123***
	(0.0006)	(0.0008)	(0.0011)	(0.0015)	(0.0018)	(0.0023)
Mean(dep. var.)	0.0196	0.0383	0.0712	0.1189	0.1533	0.2470
SD(dep. var.)	0.1387	0.1919	0.2572	0.3237	0.3603	0.4313
1st Stage F-Stat	1363.33	1363.33	1363.33	1363.33	1363.33	1363.33
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	275,820	275,820	275,820	275,820	275,820	275,820

Panel B: Foreclosures

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0009	-0.0060	-0.0255**	-0.0393***	-0.0387***	-0.0490**
	(0.0047)	(0.0083)	(0.0105)	(0.0133)	(0.0148)	(0.0206)
SellHousePrev3yr	0.0041***	0.0072***	0.0153***	0.0276***	0.0306***	0.0390***
	(0.0008)	(0.0012)	(0.0014)	(0.0020)	(0.0022)	(0.0030)
InsolventPrev3yr	0.0371***	0.0570***	0.0896***	0.1242***	0.1382***	0.1647***
	(0.0022)	(0.0031)	(0.0037)	(0.0042)	(0.0046)	(0.0051)
Mean(dep. var.)	0.0150	0.0282	0.0558	0.1021	0.1381	0.2279
SD(dep. var.)	0.1217	0.1655	0.2296	0.3027	0.3450	0.4195
1st Stage F-Stat	534.58	534.58	534.58	534.58	534.58	534.58
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104,090	104,090	104,090	104,090	104,090	104,090

Panel C: Ch. 7 Bankruptcies

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0009	-0.0030	-0.0099**	-0.0176***	-0.0140*	-0.0070
	(0.0024)	(0.0039)	(0.0048)	(0.0064)	(0.0072)	(0.0077)
InsolventPrev3yr	0.0024***	0.0027**	0.0043***	0.0046**	0.0046**	0.0019
	(0.0008)	(0.0012)	(0.0014)	(0.0019)	(0.0021)	(0.0027)
SellHousePrev3yr	0.0002	0.0006	0.0007	0.0015*	0.0019*	0.0000
	(0.0003)	(0.0005)	(0.0007)	(0.0009)	(0.0011)	(0.0015)
Mean(dep. var.)	0.0035	0.0074	0.0144	0.0257	0.0340	0.0478
SD(dep. var.)	0.0592	0.0856	0.1190	0.1583	0.1811	0.2133
1st Stage F-Stat	1094.27	1094.27	1094.27	1094.27	1094.27	1094.27
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	132,744	132,744	132,744	132,744	132,744	132,744

Panel D: Lien Judgments

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0017	-0.0041*	-0.0049	-0.0097**	-0.0121***	-0.0108**
	(0.0016)	(0.0023)	(0.0032)	(0.0043)	(0.0045)	(0.0051)
InsolventPrev3yr	0.0068***	0.0128***	0.0192***	0.0246***	0.0279***	0.0266***
	(0.0010)	(0.0018)	(0.0027)	(0.0034)	(0.0038)	(0.0039)
SellHousePrev3yr	0.0002	0.0004	0.0014***	0.0016**	0.0011	-0.0039***
	(0.0003)	(0.0004)	(0.0005)	(0.0006)	(0.0007)	(0.0009)
Mean(dep. var.)	0.0035	0.0077	0.0152	0.0244	0.0312	0.0523
SD(dep. var.)	0.0593	0.0877	0.1224	0.1543	0.1738	0.2227
1st Stage F-Stat	1363.33	1363.33	1363.33	1363.33	1363.33	1363.33
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	275,820	275,820	275,820	275,820	275,820	275,820

Table A.II: Including Apartments

This table reports results of the effect of pre-trial release on personal insolvency (Panel A), Foreclosures (Panel B), Chapter 7 Bankruptcies (Panel C), and Lien Judgements (Panel D) using the residualized instrumental variable. The dependent variables are the likelihood of such an event within the following three months (column 1), six months (column 2), one year (column 3), two years (column 4), three years (column 5), and anytime before the end of our sample period (column 6). The sample includes addresses with multiple units (i.e., including an apartment number). All specifications include court-by-year, zipcode-by-year, month, day-of-week, sex, race, and charge fixed effects.

Panel A: Insolvency

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0027	-0.0024	-0.0063	-0.0154**	-0.0210**	-0.0140
	(0.0031)	(0.0042)	(0.0061)	(0.0074)	(0.0085)	(0.0102)
InsolventPrev3yr	0.0441***	0.0724***	0.1088***	0.1394***	0.1523***	0.1551***
	(0.0014)	(0.0022)	(0.0026)	(0.0033)	(0.0035)	(0.0041)
SellHousePrev3yr	0.0032***	0.0056***	0.0116***	0.0189***	0.0229***	0.0167***
	(0.0005)	(0.0008)	(0.0010)	(0.0014)	(0.0017)	(0.0022)
Mean(dep. var.)	0.0188	0.0367	0.0683	0.1143	0.1478	0.2402
SD(dep. var.)	0.1359	0.1879	0.2522	0.3182	0.3549	0.4272
1st Stage F-Stat	7174.35	7174.35	7174.35	7174.35	7174.35	7174.35
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	307,959	307,959	307,959	307,959	307,959	307,959

Panel B: Foreclosures

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	0.0018	-0.0017	-0.0108	-0.0222*	-0.0230*	-0.0420**
	(0.0038)	(0.0060)	(0.0084)	(0.0116)	(0.0129)	(0.0167)
SellHousePrev3yr	0.0037***	0.0063***	0.0138***	0.0246***	0.0282***	0.0402***
	(0.0008)	(0.0011)	(0.0013)	(0.0019)	(0.0022)	(0.0030)
InsolventPrev3yr	0.0356***	0.0547***	0.0850***	0.1165***	0.1292***	0.1548***
	(0.0022)	(0.0030)	(0.0035)	(0.0039)	(0.0042)	(0.0047)
Mean(dep. var.)	0.0149	0.0280	0.0553	0.1010	0.1372	0.2244
SD(dep. var.)	0.1212	0.1649	0.2285	0.3014	0.3441	0.4172
1st Stage F-Stat	5228.97	5228.97	5228.97	5228.97	5228.97	5228.97
Observations	116,366	116,366	116,366	116,366	116,366	116,366

Panel C: Ch. 7 Bankruptcies

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0013	-0.0015	-0.0075*	-0.0152***	-0.0119**	-0.0050
	(0.0019)	(0.0031)	(0.0042)	(0.0051)	(0.0060)	(0.0065)
InsolventPrev3yr	0.0030***	0.0044***	0.0071***	0.0096***	0.0107***	0.0089***
	(0.0008)	(0.0011)	(0.0014)	(0.0017)	(0.0020)	(0.0025)
SellHousePrev3yr	0.0002	0.0005	0.0008	0.0014*	0.0019*	-0.0001
	(0.0003)	(0.0004)	(0.0007)	(0.0009)	(0.0010)	(0.0014)
Mean(dep. var.)	0.0033	0.0071	0.0138	0.0247	0.0326	0.0460
SD(dep. var.)	0.0578	0.0840	0.1165	0.1551	0.1775	0.2094
1st Stage F-Stat	2798.01	2798.01	2798.01	2798.01	2798.01	2798.01
Observations	148,381	148,381	148,381	148,381	148,381	148,381

Panel D: Lien Judgments

	(1) 3 Months	(2) 6 Months	(3) 1 Year	(4) 2 Years	(5) 3 Years	(6) Post
Released	-0.0004	-0.0022	-0.0015	-0.0039	-0.0056	-0.0009
	(0.0014)	(0.0019)	(0.0027)	(0.0039)	(0.0042)	(0.0050)
InsolventPrev3yr	0.0081***	0.0152***	0.0235***	0.0315***	0.0373***	0.0421***
	(0.0011)	(0.0019)	(0.0029)	(0.0039)	(0.0044)	(0.0049)
SellHousePrev3yr	0.0003	0.0009***	0.0021***	0.0028***	0.0027***	-0.0021**
	(0.0002)	(0.0003)	(0.0005)	(0.0006)	(0.0007)	(0.0009)
Mean(dep. var.)	0.0034	0.0074	0.0144	0.0232	0.0297	0.0506
SD(dep. var.)	0.0579	0.0855	0.1190	0.1504	0.1699	0.2191
1st Stage F-Stat	7299.36	7299.36	7299.36	7299.36	7299.36	7299.36
Observations	308,712	308,712	308,712	308,712	308,712	308,712