Public versus Private Law Enforcement: Evidence from Bail Jumping

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Abstract

After being arrested and booked, most felony defendants are released to await trial. On the day of the trial, a substantial percentage fail to appear. If the failure to appear is not quickly explained, warrants are issued and two quite different systems of pursuit and rearrest are put into action. Public police have the primary responsibility for pursuing and rearresting defendants who were released on their own recognizance or on cash or government bail. Defendants who made bail by borrowing from a bond dealer, however, must worry about an entirely different pursuer. When a defendant who has borrowed money skips trial, the bond dealer forfeits the bond unless the fugitive is soon returned. As a result, bond dealers have an incentive to monitor their charges and ensure that they do not skip. When a defendant does skip, bond dealers hire bail enforcement agents, more colloquially known as bounty hunters, to pursue and return the defendants to custody. We compare the effectiveness of these two different systems by examining failure to appear rates, fugitive rates and capture rates of felony defendants who fall under the respective systems. We apply propensity score and matching techniques.

Keywords: bail, surety bond, pretrial release, bounty hunter, propensity score, matching method

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The authors wish to thank Jonathan Guryan, Steve Levitt, Lance Lochner, Bruce Meyer, Jeff Milyo, Christopher Taber, Sam Peltzman and seminar participants at Claremont McKenna College, the American Economic Association annual meetings (2002), George Mason University, Northwestern University and the University of Chicago.

1. Introduction

Approximately one quarter of all released felony defendants fail to appear at trial. Some of these failures to appear (FTA) are due to sickness or forgetfulness and are quickly corrected, but many represent planned abscondments. After one year, some thirty percent of the felony defendants who initially fail to appear remain fugitives from the law. In absolute numbers, some 200,000 felony defendants fail to appear every year and of these, approximately 60,000 will remain fugitives for at least one year.[1]

Defendants who fail to appear impose significant costs on others. Direct costs include the costs of rearranging and rescheduling court dates, the wasted time of judges, lawyers and other court personnel and the costs necessary to find and apprehend or rearrest fugitives. Other costs include the additional crimes that are committed by fugitives. In 1996, for example, 16 percent of released defendants were rearrested *before* their initial case came to trial (Bureau of Justice Statistics 1999). We can be sure that the percentage of felony defendants who commit additional crimes is considerably higher than their rearrest rate. We might also expect that the felony defendants who fail to appear are the ones most likely to commit additional crimes. Indirect costs include the increased crime that results when high failure to appear and fugitive rates reduce expected punishments.[2]

The dominant forms of release are by surety bond, i.e. release on bail that is lent to the accused by a bond dealer, and non-financial release. Just over one-quarter of all

[1] All the figures are from the State Court Processing Statistics (SCPS) program of the Bureau of Justice Statistics and can be found in the reports of various years on *Felony Defendants in Large Urban Counties*. We describe the data at greater length below. The SCPS program creates a sample representative of one month of cases from the 75 most populous counties (which account for about half of all reported crimes). In 1996 the sample represented 55,000 cases, which in turn represent some 660,000 filings in a year and 1,320,000 filings in the nation. The absolute figures are calculated using this total and the release, FTA, and fugitive (defined as FTA for one year or more) rates from the random sample.

released defendants are released on surety bond, a very small percentage pay cash bail or put up their own property with the court (less than 5 percent combined); most of the rest are released on their own recognizance or on some form of public bail (called deposit bond) in which the defendant posts a small fraction, typically 10 percent or less, of the bail amount with the court.

Estimating the effectiveness of the pretrial release system in the US can be characterized as a problem of treatment evaluation. Treatment evaluation problems can be difficult because treatment is rarely assigned randomly. Release assignment, for example, is based on a judge's assessment of the likelihood that a defendant will appear in court as well as on considerations of public safety. Correctly measuring treatment effects requires that we control for treatment assignment. In this paper we control for selection by matching on the propensity score (Rubin 1974, 1977, Rosenbaum and Rubin 1983, 1984 Dehejia and Wahba 1999, Heckman, Ichimura and Todd 1999).

We begin with a brief history of pretrial release followed in section 3 by a further explanation of the different release forms and their incentive effects. Section 4 discusses the matching method. Section 5 presents the results of the matching and our estimates of the treatment effect. We estimate the treatment effect for three outcomes - the probability that a defendant fails to appear at least once; the probability that a defendant remains at large for one year or more conditional on having failed to appear (what we call the fugitive rate); and the probability that a defendant who failed to appear is recaptured as a function of time.

[2] Justice delayed can mean justice denied in practice as well as in theory. Thousands of cases are dismissed on constitutional grounds every year because police fail to serve warrants in a timely manner (Howe and Hallissy 1999).

2. History of Pretrial Release

Bail began in medieval England as a progressive measure to help accused defendants get out of jail while they waited, sometimes for many months, for a roving judge to show up to conduct a trial. If the local sheriff knew the defendant he might release him on the defendant's promise to return for trial, sometimes backed up by some sort of bond – but more often the sheriff would release the accused to the custody of a surety, usually a family member or friend. Under the common law, custody over the accused was never *relinquished* but instead was *transferred,* which explains the origin of the extraordinary rights that sureties have to pursue and capture escaped defendants. Initially, if the accused failed to appear, the surety literally took their place and was judged accordingly. Over time, the penalty became less severe until the system of money forfeiture became common.[3] The English system was adopted by the United States in most particulars with the exception that personal surety was slowly replaced by a commercial system. By the end of the 19th century commercial sureties were the norm.

Although money bail is still the most common form of release, money bail and especially the commercial surety industry have come under increasing and often virulent attack since the 1960s.[4] As noted above, bail began as a progressive measure to help defendants get *out* of jail when the default option was that all defendants would be held until trial. In the twentieth century, however, the default option was more often thought

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[3] Freed and Wald (1964) describe the history of bail at greater length and provide references.
[4] Floyd Feeney (1976, xi), for example, writes that "the present system of commercial surety bail should be simply and totally abolished….It is not so much that bondsmen are evil – although they sometimes are – but rather that they serve no useful purpose." The American Bar Association (1985, 114-115) refers to the commercial bond business as "tawdry" and discusses "the central evil of the compensated surety system." When Oregon considered reintroducing commercial bail, Judge William Snouffer testified "Bail bondsmen are a cancer on the body of criminal justice…" quoted in Kennedy and Henry (1996). Supreme Court Harry Blackmun called the commercial bail system "offensive" and "odorous" (see SCHILB v. KUEBEL 404 U.S. 357 (1971), available on the web at http://laws.findlaw.com/us/404/357.html.)

of as release and thus money bail was reconceived as a factor that kept people *in* jail. In addition, the greater burden of money bail on the poor elicited growing concern.[5] As a result significant efforts were made, beginning in the 1960s, to develop alternatives to money bail.

In the early 1960s, the Vera Institute's Manhattan Bail Project gathered information on a defendant's community ties and residential and employment stability and summarized this information in a point score. Defendants with high point scores were recommended for release on their own recognizance. Felony defendants who were recommended for release by the Manhattan Bail Project had failure to appear rates that were no higher than those released on money bail. Largely on the basis of these results, in 1966 President Lyndon Johnson signed into law the first reform of the federal bail system since 1789. The Federal Bail Reform Act of 1966 created a presumption in favor of releasing defendants on their own recognizance.

Although the Bail Reform Act of 1966 applied only to the federal courts these reforms have been widely emulated by the states (where the reform process began). Every state now has some pretrial services program and four states, Illinois, Kentucky, Oregon and Wisconsin, have outlawed commercial bail altogether.[6] In place of commercial bail, Illinois introduced the "Illinois Ten Percent Cash Bail" or "deposit bond" system. In a deposit bond system the defendant is required to post with the court an amount up to 10 percent of the face value of the bond. If the defendant fails to appear,

[5] In order to provide appropriate incentives, money bail is typically higher for the rich than the poor. Thus, it is not a priori necessary that money bail should discriminate against the poor although in practice this does occur due to non-linearities and fixed costs in the bail process. Assume that money bail is set so as to create equal failure to appear (FTA) rates across income classes. In such a case, there is no discrimination against the poor in the *setting* of bail. But if the bail amounts necessary to ensure equal FTA rates are not linear in wealth then such rates can generate unequal rates of release across income classes.
[6] In the Pretrial Services Act of 1982 pretrial service agencies were established in all 94 Federal district courts.

the deposit may be lost, and the defendant held liable for the full value of the bond. If the defendant appears for trial, the deposit is returned to the defendant, less a small service fee in some cases (National Association of Pretrial Service Agencies 1998). Some counties will also release defendants on unsecured bonds. Unsecured bonds are equivalent to zero percent deposit bonds. That is, defendants released on an unsecured bond are liable for the full bail amount if they fail to appear but they need not post anything to be released.

The Manhattan Bail Project showed that the failure to appear rates of *carefully selected* felony defendants released on their own recognizance were no higher than those released on money bail. But the Manhattan Bail Project released relatively few defendants and so could easily "cream-skim" the defendants who were most likely to appear at trial. As pretrial release programs greatly expanded across the states in the late 1960s and early 1970s, selection became more difficult and was made even more difficult as prisons became overcrowded. Using data from the 1960s and 1970s from some 15 cities, Thomas (1976) suggested that as the percent of defendants released on their own recognizance increased so did the failure to appear rate – a conclusion also reached by many police chiefs and other observers of the bail process (Romano 1991).

Economic studies of the bail system include Landes (1973, 1974), Clarke et al (1976) and Myers (1981). These studies examine the role of the bail amount in the decision to FTA, generally finding that higher bail reduces FTA rates. These earlier studies did not focus on the central issue of this paper - the different incentive effects of the various release types.[7]

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[7] Ayres and Waldfogel (1994) demonstrate the subtlety of the distinctions made by bond dealers in setting bail bond rates. Although the courts (in New Haven, Connecticut in 1990) set higher bail amounts for minority defendants than

3. Incentive Effects of Different Release Types

The pretrial release system is designed to ensure that defendants appear in court. It's often asserted that the commercial bail system *discourages* appearance because those released on surety bond are given few incentives to show up for trial. In a key Supreme Court case, for example, Justice Douglas argued that:

> …the commercial bail system failed to provide an incentive to the defendant to comply with the terms of the bond. Whether or not he appeared at trial, the defendant was unable to recover the fee he had paid to the bondsman. No refund is or was made by the professional surety to a defendant for his routine compliance with the conditions of his bond. *Schilb v. Kuebel,* ((1971), 404 U.S. at 373-374).[8]

Similarly, Drimmer (1996, 742), says "hiring a commercial bondsman removes the incentive for the defendant to appear at trial." Goldkamp and Gottfredson (1985, 19) suggest that "use of the bondsman defeated the rationale that defendants released on cash bail would have an incentive to return" and in their influential set of performance standards for pretrial release the National Association of Pretrial Service Agencies (1998) says under commercial bail "the defendant has no financial incentive to return to court."[9]

In light of the persistent criticism that surety bail encourages FTA it is perhaps surprising that the data consistently indicate that defendants released via surety bond have lower FTA rates than defendants released under other methods. Part of this might be explained by selection – FTA rates, for example, may be higher for those defendants charged with minor crimes - perhaps these defendants reason that police will not pursue a failure to appear when the underlying crime is minor - and defendants charged with

for whites, Ayres and Waldfogel find that bond dealers acted in precisely the opposite manner. What this pattern suggests is that judges set higher bail for minority defendants *compared to white defendants with the same probability of flight.* Bond dealers are then induced by competition to charge minorities relatively lower bail bond rates.

[8] The case can be found on the web at http://laws.findlaw.com/us/404/357.html.

[9] See also Thomas (1976, 13) who because of this issue calls the surety system "irrational."

minor crimes are more likely to be released on their own recognizance than on surety release. A second reason, however, is that bond dealers, just like other lenders, have numerous ways of creating appropriate incentives for borrowers.

Most obviously, a defendant who skips town will owe the bond dealer the entire amount of the bond just as with the deposit bond system. Defendants are often judgment proof, however, so bond dealers often ask defendants for collateral and family cosigners to the bond (which is not done under the deposit bond system). If hardened criminals do not fear the law, they may yet fear their mother's wrath should the bond dealer take possession of their mother's home because they fail to show up for trial. In order to make flight less likely, bond dealers will also sometimes monitor their charges and require them to check in periodically. In addition, bond dealers often remind defendants of their court dates and, perhaps more importantly, remind the defendant's mother of the son's court date when the mother is a cosigner on the bond (Toborg 1983).[10]

If a defendant does fail to appear the bond dealer is granted some time to recapture him before the bond dealer's bond is forfeited. Thus, bond dealers have a credible threat to pursue and rearrest any defendant who flees. Bond dealers report that just to break even, 95 percent of their clients must show up in court (Drimmer 1996, Reynolds 2002). Thus, significant incentives exist to pursue and return skips to justice.

Bond dealers and their agents have powerful legal rights over any defendant who fails to appear, rights that exceed those of the public police. Bail enforcement agents, for example, have the right to break into a defendant's home without a warrant, make arrests using all necessary force including deadly force if needed, temporarily imprison defendants, and pursue and return a defendant across state lines without necessity of entering into an extradition process (Drimmer 1996). In Taylor vs. Taintor (16. Wall. U.S. 366, 1873), which remains good law, the Supreme Court noted (371-372):

> When bail is given, the principal is regarded as delivered to the custody of his sureties. Their dominion is a continuance of the original imprisonment. Whenever they choose to do so, they may seize him and deliver him up in their discharge, and if that cannot be done at once, they may imprison him until it can be done. They may exercise their rights in person or by agent. They may pursue him into another state; may arrest him on the Sabbath; and if necessary, may break and enter his house for that purpose. The seizure is not made by virtue of new process. None is needed. It is likened to the rearrest, by the sheriff, of an escaping prisoner.

Bond dealers prepare for the possibility of flight by collecting information at the time they write the bond that may later prove useful. A typical application for bond, for example, will contain information on the defendant's residence, employer, former employer, spouse, children (names and schools), spouse's employer, mother, father, automobile (description, tags, financing), union membership, previous arrests etc.[11] In addition, bond dealers have access to all kinds of public and private databases. Bob Burton (1990), a bounty hunter of some fame, for example, says that a major asset of any bounty hunter is a list of friends who work at the telephone, gas, or electric utility, the post office, welfare agencies or in law enforcement.[12]

Bond dealers, however, recognize that what makes their pursuit of skips most effective is the time they devote to the task. In contrast, public police bureaus are often strained for resources and the rearrest of defendants who fail to show up at trial is usually given low precedence. The flow of arrest warrants for failure to appear has overwhelmed

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[10] Bail jumping is itself a crime which may result in additional penalties.

[11] We thank Bryan Frank of Lexington National Insurance Corporation for discussion and sending us a typical application form.

[12] Good bond dealers master the tricks of their trade. One bond dealer pointed out to us, for example, that the first three digits in a social security number indicate in what state the number was issued. This information can suggest that an

many police departments so that today many counties are faced with a massive stock of unserved arrest warrants. Baltimore alone had 54,000 unserved arrest warrants as of 1999 (Clines 2001). In recent years Cincinnati has had over 100,000 outstanding arrest warrants stemming from failures to appear in court. One Cincinnati defendant had 33 pending arrest warrants against him (Lecky 1997). In response to the overwhelming number of arrest warrants, most of which will never be served because of lack of manpower, some counties have turned to extreme measures such as offering amnesty periods. Santa Clara County in California, for example, has a backlog of 45,000 unserved criminal arrest warrants and in response has advertised a hotline that defendants can use to schedule their own arrests (Lee and Howe 2000).[13]

Although national figures are not available it is clear that the problem of outstanding arrest warrants is widespread. Texas, for example, is relatively clean with only 132,000 outstanding felony and serious misdemeanor warrants but Florida has 323,000 and Massachusetts, as of 1997, had around 275,000 (Howe and Hallissy 1999). California has the largest backlog of arrest warrants in the nation. The California Department of Corrections estimated that as of December 1998 there were more than *two and a half million* unserved arrest warrants (California Board of Corrections 1998, Howe, Hallissy 1999). Many of these arrest-warrants are for minor offenses but tens of thousands are for people wanted for violent crimes including more than 2,600 outstanding homicide warrants (Howe and Hallissy 1999). Howe and Hallissy (1999) report that "local, state and federal law enforcement agencies have largely abandoned

applicant might be lying if he claims to have been born in another state (many SSNs are issued at birth or shortly thereafter) and it may provide a lead for where a skipped defendant may have family or friends.

[13] See Prendergast (1999) for description of a similar program in Kenton County, Kentucky.

their job of serving warrants in all but the most serious cases." Explaining how this situation came about, they write:

> As arrests increased, jails became overcrowded. To cope, judges, instead of locking up suspects, often released them without bail with a promise to return for their next court date. For their part, police, rather than arrest minor offenders, issued citations and then released the suspects with the same expectation.
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> When suspects failed to appear for their court dates, judges issued bench warrants instructing police to take the suspects into custody. But this caused the number of warrants to balloon, and the police did not have the time or staff to serve them all.

4. The Matching Model with Multiple Treatments

Ideally in a treatment evaluation we would like to identify two outcomes: one if the individual is treated, Y_T , and one if no treatment is administered, Y_{NT} . The effect of the treatment is then $Y_T - Y_{NT}$. But we cannot observe an individual in both states of the world making a direct computation of $Y_T - Y_{NT}$ impossible (Rubin 1974). All methods of evaluation, therefore, must make some assumptions about "comparable" individuals. An intuitive method is to match each treated individual with a statistically similar untreated individual and compare differences in outcomes across a series of matches. Thus two statistical doppelgängers would function as the same individual in different treatments.

An important advantage of matching methods is that they do not require assumptions about functional form. When the research question is about a mean treatment effect, as it is here, matching methods also allow for an economy of presentation because they focus attention on the question of interest rather than on a long series of variables that are used only for control purposes. Unfortunately, matching methods typically founder between a rock and a hard place. The technique works best when individuals are matched across many variables but as the number of variables increases, the number of distinct "types" increases exponentially so the ability to find an exact match falls dramatically.

In an important paper, Rosenbaum and Rubin (1983) go a long way to surmounting this problem. Rosenbaum and Rubin show that if matching on *X* is valid then so is matching on the probability of selection into a treatment conditional on *X*. The multi-dimensional problem of matching on X is thus transformed into a single dimension problem of matching on $Pr(T=1 \mid X)$ where $T=1$ denotes treatment.[14] $Pr(T=1 \mid X)$ is often called the propensity score or p-score.

The matching technique extends naturally to applications with multiple treatments through the use of a multi-valued propensity score with matching on conditional probabilities (Lechner 1999, Imbrens 1999). Assume that there are *M* mutually exclusive treatments and let the outcome in each state be denoted Y_1, Y_2, etc. As before, we observe only a specific outcome but are interested in the counterfactual; what would the outcome have been if this person had been assigned to a different treatment? Rather than a single comparison, we are now interested in a series of pairwise comparisons between treatments *m* and *l*. The treatment effect on the treated is written:

$$\theta_0^{m,l} = E(Y^m - Y^l \mid T = m) = E(Y^m \mid T = m) - E(Y^l \mid T = m), \qquad (1)$$

where $\theta_0^{m,l}$ denotes the effect of treatment *m* rather than *l*.

Identification of (1) can occur under appropriate conditions the most important being that treatment outcomes are independent of treatment selection after conditioning on a vector of attributes, *X* (the conditional independence assumption). Formally,

[14] Matching methods are common among applied statisticians and natural scientists but have only recently been analyzed and applied by econometricians and economists. Papers on the econometric theory of matching include Heckman, Ichimura, Todd (1998) and Imbrens (1999). More applied work includes Heckman, Ichimura and Todd

$$Y^1 \ldots Y^M \perp T \mid X = x \tag{2}$$

If this assumption is valid we can use the conditional propensity score to identify the treatment effect (see Lechner 1999),

$$\theta^{m,l} = E(Y^M \mid T = m) - \underset{p^{m|ml}}{E}\left[E(Y^l \mid p^{m|ml}(X), T = l) \mid T = m\right], \tag{3}$$

In practice, the conditional propensity score, $p^{m|ml}(x)$, is computed indirectly from the marginal probabilities $p^l(x)$ and $p^m(x)$ estimated from a discrete choice model. In this case:

$$E[p^{m|ml}(x) \mid p^l(x), p^m(x)] = E[\frac{p^m(x)}{p^l(x) + p^m(x)} \mid p^l(x), p^m(x)] = p^{m|ml}(x). \tag{4}$$

The matching estimator in our case is created by an ordered probit model for reasons that will be discussed below. An outline of the basic procedure is given in Table 1.

It's important to emphasize that the propensity scores are not of direct interest but rather are the metric by which members of the treated group are matched to members of the "untreated" group ("differently" treated in our context). After matching, and given the conditional independence assumption, the treated and untreated group can be analyzed *as if* treatment had been assigned randomly. Thus, differences in mean FTA rates across *matched* samples are estimates of the effect of treatment.

Less formally, matching on propensity scores can be understood as a pragmatic method for balancing the covariates of the sample across the different treatments (Dehejia and Wahba 1998). Note that the covariates that we care most about balancing are those that affect the treatment outcome. Assume, for example, that X influences treatment

(1997), Dehejia and Wahba (1998) and Lechner (2000). Our multi-treatment application is closest to that of Lechner (1999).

selection but does not independently influence treatment outcome. If the goal of the selection model were to consistently estimate the causes of treatment selection we would want to include *X* in the model but it is not necessarily desirable to include it when the purpose is to create a metric for use in matching (Augurzky and Schmidt 2000). A simple example occurs when *X* predicts treatment exactly. Inclusion of *X* would defeat the goal of matching because all propensity scores would be either zero or one. Similarly, we will include in the propensity score model variables that may affect the treatment outcome even if they do not casually affect treatment selection.

5. Data and Descriptive Statistics

We use a data set compiled by the U.S. Department of Justice's Bureau of Justice Statistics called State Court Processing Statistics (SCPS), 1990, 1992, 1994, 1996 (ICPSR 2038). We supplement with an earlier version of the same collection, the National Pretrial Reporting Program (NPRP), 1988-1989 (ICPSR 9508). The data are a random sample of one month of felony filings from approximately 40 jurisdictions where the sample was designed to represent the 75 most populous U.S. counties. The data contain detailed information on arrest charges, the criminal background of the defendant (e.g. number of prior arrests), sex and age of the defendant,[15] release type (surety, cash bond, own recognizance etc.), rearrest charges for those rearrested, whether the defendant failed to appear and whether the defendant was still at large after one year among other categories.

In addition to the main release types, there are minor variations on a theme. Some counties, for example, release on an unsecured bond for which the defendant pays no money to the court but is liable for the bail amount should he fail to appear. Because the incentive effects are very similar, we include unsecured bonds in the deposit bond category.[16] Instead of a pure cash bond it is sometimes possible to put up property as collateral. Since property bonds are rare (588 observations in our data, less than 2% of all releases), we drop them from the analysis.[17] Finally some counties may occasionally use some form of supervised release. In the first year of our dataset, supervised release is included in the own recognizance category. Supervised release often means something as simple as a weekly telephone check-in, so including these with own recognizance is reasonable. Supervised release is not a standard term, however, and other forms, such as mandatory daily attendance in a drug treatment program are likely to be more binding. To maintain comparability across years we follow the practice established in the first year of the dataset by classifying supervised release with own recognizance. Because supervised release is more binding than pure own-recognizance, this can only lower FTA rates and other results in the own recognizance sample thus biasing our results *away* from finding significant differences among treatments.[18]

In Table 2, the mean FTA rates for release categories are along the main diagonal with the number of observations in square brackets. The preliminary analysis suggests that FTA rates are lower under surety bond release than under most other types of release. Off diagonal elements are the difference between the FTA rate for the row category and the FTA rate for the column category. The FTA rate for those released under surety bond

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[15] The SCPS is more complete and better organized than the NPRP data. The former, for example, includes information on the race of the defendant that the latter does not.

[16] We drop observations with missing data on the bail amount.

[17] Another reason to drop property bonds is that it's difficult to compare the bail for these releases for other release types. A defendant, for example, may put up a $250,000 house as collateral for $25,000 in bail. Although we know the bail amount we do no know the value of the collateral property other than that it must, by law in many cases, be higher

is 17 percent. Compared to surety release, the FTA rate is 3 percentage points higher under cash bonds, 4 percentage points higher under deposit bonds and 9 percentage points higher under own recognizance (all these differences are statistically significant at greater than the 1 percent level). Put slightly differently, compared with surety release, the FTA rate is approximately 18 percent higher under cash bond, 33 percent higher under deposit bond, and more than 50 percent higher under own recognizance.

We also present some information in Table 2 on emergency release. Emergency releasees are defendants who are released solely because of a court-order on prison overcrowding. Emergency release is not a treatment – the treatment is own recognizance – but rather an indication of what happens when neither judges nor bond dealers play their usual role in selecting defendants to be released.[19] One would expect that relative to those released under other categories these defendants are likely to be accused of the most serious crimes, have the highest probability of being found guilty and have the fewest community ties. In addition, these defendants have neither monetary incentive nor the threat of being recaptured by a bounty hunter to induce them to return to court. As a result, a whopping 45 percent of the defendants who are given emergency release fail to appear for trial. The large differences between the FTA rates of those released on emergency release and every other category indicate that substantial and successful selection occurs in the release versus not released decision. Emergency release is thus of some special interest, although not directly related to the focus of this paper.

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than the value of the bail amount. A cash or surety bond, therefore, is not equivalent to a property bond for the same bail amount.

[18] We find similar results by restricting the dataset to the years in which supervised release is given a distinct category.

[19] Even under emergency release some selection can occur. Judges and jailers, for example, could order that more inmates be paroled to make room for the most potentially dangerous accused defendants or inmates could be shipped out-of-state or the court-order could be (temporarily) ignored. The costs of selection, however, clearly rise substantially when jail space is tightly constrained.

Although the preliminary data analysis is suggestive, the difference in means analysis could confound effects due to treatment with effects due to selection on, for example, defendant characteristics such as the alleged crime.

6. Results

6.1 Propensity Scores from Ordered Probit

We generate propensity scores for matching using an ordered probit model. By law, judges must release defendants on the *least restrictive* conditions that they believe are compatible with ensuring appearance at trial.[20] Own recognizance, the least restrictive form of release, is our first category followed by release on deposit bond. Although defendants released on deposit bond must put up some cash, which they will forfeit if they fail to appear, the amount of the cash is typically less than $500.[21] Few people are ever held because of a failure to raise cash for a deposit bond. Defendants who were offered financial release (but not a deposit bond) and who paid their bonds in cash are the third category of release. Cash bond is more expensive than a deposit bond but does not involve the monitoring of sureties. Defendants released via surety bond are the fourth category. Although the Constitution guarantees that excessive bail shall not be required it does not require that bail should always be set low enough for a defendant to be able to afford release. Indeed, judges sometimes set bail in the expectation (and hope) that the defendant will not be able to raise bail. Thus, we include defendants held on bail or detained without bail as the final, most restrictive category, not released. Emergency

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[20] The Federal Bail Reform Act of 1966 required that defendants be released on the least restrictive conditions that will ensure their appearance at trial and almost all states have adopted similar laws since that time.
[21] The median deposit bond amount is $5000 and releasees typically must deposit 10 percent or less of the bond amount.

releases are also included in the final category because, had it not been for the emergency, these individuals would have not have been released.

Thus, stringency of release, measured by z,* is a linear function,

$$z^{*} = \beta' x + \gamma_t + \lambda_k + \varepsilon ,$$

where x includes all of the independent variables in the sample, γ_t are year specific intercepts for 1990, 1992, 1994, and 1996 and λ_k are county effects. The observed values of stringency are discrete and take on the value of 1 for those released on own recognizance, 2 for those on deposit bond, 3 for those on cash bond, 4 for those on surety bond and 5 if the defendant was not released. That is,

$$\begin{aligned} z &= 1 \text{ if } z^{*} \leq 0 \\ &= 2 \text{ if } \mu_1 < z^{*} \leq \mu_2 \\ &\text{M} \\ &= 5 \text{ if } \mu_4 \leq z^{*} \end{aligned}$$

where $\mu's$ are the unknown cut points that can be estimated. Probabilities for each release type can then be constructed (see, for example, Greene 2000). From the ordered probit we generate conditional propensity scores for each possible pairwise comparison.[22]

Variables in the ordered probit specification include individual-specific indicators denoting whether the crime the defendant has been accused of is a murder, rape, robbery, assault, other violent crime, burglary, theft, other property offense, drug trafficking, other drug related, or driving related (with misdemeanors and other crimes in the constant). We also include variables for past experience with the criminal justice system. Three binary variables are set equal to one respectively if the defendant had some active criminal justice status at the time of the arrest (e.g. was on parole or probation), had prior felony arrests, or had a prior failure to appear at trial. The defendant's sex and age are also included. Note that these variables are exactly the sorts of variables that judges use to make treatment selection decisions.[23] Other, non-individual variables include the police clearance rate, defined as the number of arrests divided by the number of crimes per county. The clearance rate provides a crude measure of police availability that may affect FTA rates.

County and year effects are included in the selection equation (county 29 and 1988 are excluded to prevent multicollinearity). The use of county effects in the selection equation is noteworthy because it implies that matching will occur with "quasi"-fixed effects. A true fixed-effects estimator would require that comparables come from within the same county. The matching estimator takes into account county effects when seeking a match but does not insist that every match must be within-county. In particular, some counties do not release on deposit bond and others do not release on surety bond. A fixed-effects estimator would not use information from these counties in estimating the effect of the deposit and surety treatments. The matching estimator will use information from these counties if matching is strong on other variables. A pure fixed-effects estimator may also be important, however, so we discuss this at greater length in the section below on unobservables. The results of the ordered probit estimation are in Table 3.

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[22] We have also estimated the results using a multivariate logit model. The results are substantively similar.

[23] Ayers and Waldfogel (1994) identify eight characteristics that judges may consider in setting bail: 1) the nature and circumstances of the offense (if relevant); 2) the evidence against the defendant; 3) the defendant's prior criminal record; 4) the defendant's prior FTA record; 5) the defendant's family ties; 6) the defendant's employment record; 7) the defendant's financial resources; and 8) the defendant's community ties. Although Ayers and Waldfogel's study deals only with Connecticut the criteria are similar in other states.

6.2 Matching Quality

A match is defined as the pair of observations with the smallest difference in propensity scores so long as the difference is less than a predefined caliper. If no observations can be matched within the caliper distance, the observations(s) is dropped. We use matching with replacement so the order of matching is irrelevant and every untreated observation is compared against every treated observation.[24]

The match quality is good, as we match large proportions of the sample despite using a caliper of only 0.0001.[25] Figure 1A presents a box and whiskers plot of the propensity scores for each treatment category (including the "treatment" of not-released) conditional on the actual treatment. The left most part of the graph, for example, gives the box and whiskers plot for the propensity of receiving the own, deposit, cash, surety and not released treatments for all defendants who received the own treatment.[26]

Figure 1B plots the box and whiskers for the pairwise (conditional) probabilities for the own v. surety comparison. The Pr. Own and Pr. Surety arrows indicate that we can find good comparables, statistical doppelgangers, for individuals released under either treatment. Many of the defendants released on surety bond, for example, were as likely to have been released on their own recognizance (3rd box from the left) as those who actually were released on their own recognizance (1st box from the left). Similarly, many of the defendants who were released on their own recognizance were as likely to

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[24] Dehejia and Wahba (1998) find that matching with replacement is considerably superior to matching with non-replacement.

[25] When matching on variables with fewer observations, such as fugitive rates conditional on FTA as we do below, we match using a caliper of .001. The caliper size makes little difference to the results.

[26] In a box and whiskers plot the box contains the interquartile range (IQR) the observations between the 75th percentile (the top of the box) and the 25th percentile (the bottom of the box). The horizontal line towards the center of each box is the median observation. The whiskers are the so called adjacent values which extend from the largest observation less than or equal to the 75 percentile plus 1.5 * IQR and the smallest observation more than or equal to the 25 percentile minus 1.5* IQR. Points outside the box and whiskers are called extreme values or outside points and for clarity are not plotted in this graph. In this plot, the width of the box is proportional to the square root of the number of observations in that category.

have been released on surety bond (2nd box from the left) as those who actually were released on surety bond (4th box from the left). Note that it's important that the boxes overlap *across* treatments, not that they overlap within treatments – i.e. the fact that in Figure 1A the propensity to receive the deposit bond treatment is everywhere lower than the propensity to receive own recognizance simply reflects the fact that deposit bond is a low probability event. More important is that the deposit bond treatment is a low probability event *regardless of actual treatment* – we can thus find good comparables across the treatments. Alternatively stated, the overlap in the boxes across treatments indicates that random factors play a large role in treatment selection thus aiding our effort to find true comparables.[27]

Although we can find good comparables across the release treatments we cannot find good comparables for those who were not released. Indeed, the Figure 1A box and whiskers plot of the propensity for not-released among those who in fact were not-released doesn't overlap *at all* with the propensity to be not-released for those who were released. Defendants who are not-released differ greatly from released defendants.[28] (This is consistent with the very high FTA rates we found for emergency releasees in Table 2). The fact that the model is capable of finding large selection effects if they exist, as they apparently do for those not-released, bolsters the finding that selection on observables is not overly strong among the release treatments.

6.3 Estimated Treatment Effects: Failure to Appear

In Table 4 the row variable denotes the treated variable and the column the untreated variable. For reference, the main diagonal includes the mean FTA rate in that

[27] Another interesting aspect of the box and whisker plot is that it suggests that almost everyone can be released on their own recognizance, even those who might in another time and place be released only with high bail. Thirty percent of released defendants accused of murder, for example, were released on their own recognizance.

category from the full sample.[29] Reading across the surety row, for example, we see the estimated difference in FTA rates caused by the surety treatment relative to the column treatment – i.e. the estimate of the effect of treatment on the treated. The matching estimator suggests that similar individuals are 7.3 percentage points or 28% less likely to fail to appear when released on surety bond than when released on their own recognizance. Similar individuals are also 3.9 percentage points or 18% less likely to fail to appear when released on surety bond than when released on deposit bond. The estimated treatment effect for those on surety bonds versus cash is small and not statistically significant.[30]

Two standard errors are presented in Table 4. The first takes into account uncertainty in the matched samples but assumes that the propensity score is known with certainty. The second estimate is a bootstrapped standard error that takes into account uncertainty propagating from the estimation of the propensity score. The "regular" and bootstrapped standard errors are close with the bootstrap errors being approximately 8-20 percent higher.[31] All the statistically significant results are significant at greater than the 1% level using either standard error. Since the estimation of the propensity score adds very little uncertainty to the matching estimators and because calculating bootstrap errors is very time and resource intensive we present only the regular standard errors in future

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[28] It is possible to find defendants who were released who might not have been released – thus the data is consistent with the adage that it is better to let 10 guilty men go free than jail one innocent man.

[29] The mean FTA rate for the full sample is included as rough guide to absolute effects. Note, however, that the matched sample is usually smaller than the full sample so the mean FTA rate for the matched and full samples can be slightly different.

[30] As a test of matching quality we also ran a linear regression on the matched samples that included Surety Bond and all the variables in Table 3. The results are similar, as they should be if the matched samples divide other covariates as if they were assigned randomly. The coefficient on Surety Bond in the surety versus own regression, for example, is –6.5 which is within one standard deviation of the –7.3 matching estimate. We do a more detailed comparison of linear regression and matching results further below.

[31] Not surprisingly, the smaller differences occur in comparisons using either of the largest groups, own or surety.

results.[32] Readers may add 15% to these errors to control for uncertainty in the estimation of the propensity score.

Unlike Table 2, both the top and bottom halves of Table 4 are filled in; this is because the estimate of the treatment on the treated is conceptually different from the estimate of the treatment on the untreated (differently treated). For example, the effect of the surety treatment relative to own recognizance for those who were released on surety bond is not necessarily the exact opposite of the effect of own recognizance relative to surety bond on those who were released on own recognizance. As it happens, however, our estimates of these effects are similar. The estimate of the effect of own recognizance relative to surety on those who were released on their own recognizance, for example, is 6.5 percentage points, similar in size but opposite in sign to the –7.3 surety effect relative to own recognizance of those who were released on surety bond. The similarities across diagonals suggest that either (or both) treatment selection or treatment effect does not interact strongly with defendant characteristics. One possible exception is that the deposit bond treatment relative to cash is estimated at 4.1 percentage points while the cash bond treatment relative to deposit is estimated at –1.5 percentage points.

In Table 5 we extend our matching algorithm so that it matches on the propensity score and the bail amount. Bail is determined by the same sorts of factors that enter into treatment selection (e.g. seriousness of crime, prior arrests etc.), and thus matching on p-score will match on bail to some extent. But in the matched surety bond sample, for example, the mean bail is $8243 but in the cash bond sample it is only $3883. The

[32] The bootstrap errors in Table 5 were calculated using 100 replications of the model. The procedure took over 48 hours on a reasonably fast Pentium computer.

difference is to be expected as defendants with low bail amounts will tend to self-select into cash rather than surety bail. If higher bail significantly discourages FTAs, differences in bail amounts could account for perceived treatment effects among release types. Thus, to ensure that the effects are not being caused by bail per se, we match on propensity score and the natural log of bail using the Mahalanobis distance as a metric.[33,]

In the surety v cash bond sample matching on bail suggests a small but statistically significant positive impact of surety bond on FTA rates. Matching does distribute bail amounts across the treatments. In the sample matched on bail and propensity score the mean bail amounts in the surety and cash bond sample are, $4011 and $3927 respectively. Thus, high-bail surety bond releases are thrown out in order to match surety releases to the cash bond sample. The results on the other treatment effects are similar to those found earlier. In particular, surety and cash bond both result in lower FTAs than deposit bond.

6.4 Estimated Treatment Effects: The Fugitive

A surprisingly large number of felony defendants who fail to appear remain at large after one year, approximately 30%. Alternatively stated, some 7% of all released felony defendants skip town and are not brought back to justice within one year. We call FTAs that last more than one year, fugitives.

The surety treatment differs most from other treatments when a defendant purposively skips town because this is when bounty hunters enter the picture.[34] If the surety treatment works, therefore, we should see it most clearly in the apprehension of

[33] The Mahalonobis distance is a Euclidian (squared) distance that is weighted by the inverse covariance matrix for the matching variables. For details see Sianesi (2002).

[34] We use the term bounty hunter or bail enforcement agent to refer to private pursuers of felony defendants. Bond dealers typically pursue their own skips. Literal bounty hunters are typically not called in unless the skip is thought to

fugitives. Given that a defendant fails to appear, we ask what is the probability that the defendant is not brought to justice within one year and how does this vary with release type? Importantly, *once a defendant has decided to abscond* there is no reason why anything other than the different effectiveness of public police and bail enforcement agents should have a systematic effect on the probability of being recaptured.

Table 6 provides strong evidence that bounty hunters are highly effective at recapturing defendants who attempt to flee justice - considerably more so than the public police. The main diagonal of Table 6 contains the mean fugitive rate conditional on FTA along with the number of observations in each category. The estimated treatment effect for the row versus column variables are shown in the off diagonals with standard errors in parentheses. The probability of remaining at large for more than a year conditional on an initial FTA is much lower for those released on surety bond. The surety treatment results in a fugitive rate that is lower by 17, 15.5, and 25.6 percentage points compared to own recognizance, deposit bond and cash bond respectively. In percentage terms the fugitive rate under surety release is 53%, 47%, and 64% lower than the fugitive rate under own recognizance, deposit bond and cash bond respectively. Similarly, the own recognizance, deposit and cash bond treatments result in fugitive rates that are 29%, 47%, and 47% higher than under surety.

There are also some interesting non-surety effects in Table 6. Note that the fugitive rate *conditional on an FTA* is *higher* for cash bond relative to release on own recognizance. Earlier (see Table 5) we had found that the FTA rate was *lower* for cash bond relative to release on own recognizance. What this suggests is that defendants on

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have crossed state or international lines. Services like Wanted Alert, http://www.wantedalert.com, regularly post ads in USA Today that list fugitives and their bounties.

cash bond are less likely to fail to appear than those released on their own recognizance but if they do fail to appear they are less likely to be recaptured. The result is pleasingly intuitive. A defendant released on his own recognizance has little to lose from failing to appear and thus may fail to appear for trivial reasons. But a defendant released on cash bond has much to lose if he fails to appear and thus those who do fail to appear do so with the goal of not being recaptured.

The propensity score method can be very informative about the entire distribution of treatment effects. In Figure 2 we graph smoothed (running-mean) FTA and fugitive rates against surety p-scores for the own-recognizance and surety treatments (conditional on receiving either surety or own). (We omit graphs for the other treatment comparisons for reasons of length). The two downward-sloped less-thick curves graph smoothed FTA rates against the p-scores for those defendants released on their own recognizance or surety. The slope of each line indicates the direction and strength of the effect of observables on selection in that treatment. The difference between the own and surety lines at any given propensity score is an estimate of the treatment effect, controlling for observables. The difference is roughly constant which indicates that despite some mild selection the treatment effect is roughly independent of observables.

For both the own and surety treatments, FTA rates fall as the propensity for receiving surety increases. That is, FTA rates fall as observables move in the direction predicting surety release. The fall is gentle; moving from a near zero propensity to a near 1 propensity reduces the FTA rate by approximately 5 percentage points. The effect is sensible if we recall that many FTAs are short-term - the defendant forgets the trial date or has another pressing engagement. These sorts of FTAs are likely to be more common for defendants with observables that predict low p-scores because judges release most defendants on their own recognizance and reserve surety release for defendants accused of more serious crimes. Few people will forget to show up for their murder trial but some may do so if the trial involves a driving offense. At the same time, however, we expect that defendants accused of more serious crimes - who have more to lose from being found guilty - are more likely to purposively abscond. If this is correct, we ought to see a positive correlation between the surety propensity score and the fugitive rate (failing to appear and not found within one year) conditional on having failed to appear.

The two upward-sloped and thick lines plot smoothed fugitive rates against the surety propensity score. As before the slope of the plots gives the direction and strength of effects caused by selection on observables and the treatment effect for any given propensity score is the difference between the FTA rates at that propensity score. Unlike FTA rates, the selection effects for conditional fugitive rates are positive – that is, as observables move in the direction of a greater propensity to be selected for surety release the fugitive rate increases. Interestingly, the effect of selection on defendants released on surety bond is less than that of defendants released on their own recognizance, deposit or cash bond (i.e. the "slope" of the plot is less). What this suggests is that the surety treatment works well even for those defendants whose observables would predict higher FTA rates. We examine the issue of unobservables at length below but since selection by observables has little influence on fugitive rates, Figure 2 already suggests that unobservables would have to be very different from unobservables in order to greatly affect the results.

6.5 Kaplan-Meier Estimation of FTA Duration

The higher rate of recapture for those released on surety bond compared to other release types can be well illustrated with a survival function. For a subset of our data, just over 7000 observations, we have information on the time from the failure to appear until recapture (return to the court). A survival function graphs the percentage of observations that survive at each time period. We estimate a survival function for each release type using the non-parametric Kaplan-Meier estimator. Typically, the Kaplan-Meier estimator is used only for preliminary analysis and is then followed by a parametric or semi-parametric model. Although parametric and semi-parametric models allow for covariates they require sometimes-tenuous assumptions about functional form. Instead, we follow our earlier approach of creating matched samples. Thus, using the same procedure as earlier, we create three matched samples surety v. own, surety v. deposit and surety v. cash. We then compare the survival function across each matched sample. The matching procedure ensures that covariates are balanced across the matched samples so it is not necessary to include additional controls for covariates.

Figure 3 presents the survival functions. In each case the survival function for those on surety bond is markedly lower than that for own recognizance, deposit bond, or cash bond. The ability of bail enforcement agents relative to police to recapture defendants who skip bail is evident within a week of the failure to appear.[35] By 200 days the surety survival rate is some 20 to 30 percentage points or 50 percent lower than the survival rate for those on cash bond, deposit bond or out on their own recognizance, i.e.

[35] A number of estimates have been made that bounty hunters take into custody between 25,000 and 35,000 fugitives a year, depending on the year (see various sources in Drimmer 1996 also Barr 2000). These figures are consistent with a recapture rate of over 95 percent and are consistent with the number of fugitives on surety bond. It appears, therefore, that almost all fugitives on surety bond are recaptured by bail enforcement agents and not by the police. Bounty hunters, however, will sometimes track down defendants and then tip police as to their whereabouts so police will sometimes be involved in some aspects of recapture.

the probability of being recaptured is some 50% higher for those released on surety bond relative to other releases. (Note that there are three surety bond survival functions, one for each comparison group, but that these are nearly identical). Figure 4 presents a similar regression matching on propensity score and bail. The survival functions appear more ragged but otherwise the results are very similar.

Table 7 shows the results of a log rank test (Kalbfleisch and Prentice 1980). The log rank test confirms Figures 3-4; we can easily reject the null of equality of the survivor functions - defendants released on surety bond are much more likely to be recaptured (i.e. less likely to remain at large, "survive") than are those released on their own recognizance, deposit bond or cash bond.

7. Looking for Unobservables

Matching is a powerful and flexible tool, but like linear regression, it is not a research design that magically guarantees the identification of causal effects. In this section we use a number of techniques to test for robustness and to rule out the potentially confounding effects of unobservables. Analyzing unobservables requires identification assumptions and, as always, such assumptions are open to question. Nevertheless, we are able to offer several identification strategies that allow us to analyze three classes of potentially unobserved variables; 1) unobservables associated with counties, 2) variables associated with individuals that are unobserved by us but observed by judges and 3) variables associated with individuals that are observed neither by us nor by judges. Analyzing each of these possibilities we converge on the finding that treatment effects rather than unobserved variables explain why FTA rates and fugitive rates are much lower for defendants on surety bond compared to defendants on other forms of pre-trial release.

7.1 County Unobserveds

We begin with county unobservables. Counties vary on a wide set of dimensions such as size, population density, average crime rate, and prosecutorial and police strategies. If any of these are correlated across the data with FTA and fugitive rates and with the propensity to use commercial bail, this could bias our results. Earlier we noted that county variables in the ordered probit selection equation make the matching estimator a "quasi-fixed" estimator. We now examine whether we find similar results using a true fixed-effects regression. Some counties do not use some release programs. In running a particular fixed-effects regression, say of the surety versus own treatment, we could use every county that contains both treatments but instead we take a more conservative approach. Our fixed effects regression contains only those counties with *every* treatment program – we assume, in other words, that counties with *every* treatment program are the most comparable. This reduces the number of counties and observations by approximately 40 percent. The regression includes county fixed effects and all of the variables in Table 3. For comparison purposes we also run the matching estimator on the new sample and we run the probability model on the matched samples. By including fixed effects the identification of the treatment effect comes only from the *within-county* variation in FTA rates among treatment types. Thus, the fixed effects regression controls for *any* unobserved but fixed variable associated with counties.

In Table 8 we focus on the fugitive results and the surety treatment effect on the treated. The first number is the coefficient on surety bond in a linear probability model followed by the matching estimate; respective standard errors are in parentheses.[36] The percentage point treatment effects are somewhat smaller than in the full sample but they are smaller when estimated by either the linear probability model with fixed effects or the matching estimator thus suggesting that the differences are due to sample and not to estimation technique. In percentages the surety treatment effect results in estimated fugitive rates that are lower compared to own, deposit and cash bond rates by 15-22%, 54-38%, and 40.7-41.5% (where the first number uses the linear probability model and the second the matching estimator). The fixed effects and matching estimates are within a standard deviation of one another, or a shade of a standard deviation in the Surety v. Deposit comparison. We conclude that the fugitive rate for those released on surety bond is considerably lower than it is for defendants released under other categories even after restricting the sample to the most comparable counties and including county fixed effects.

Using county fixed effects throws out the cross-county variation but arguably this variation is the most revealing because it may be the most "fortuitously random." Consider those states that have banned commercial bail. It seems plausible that matching can find two individuals who are comparable but for the fact that one individual *could not* have been assigned surety bail while the other could and *was* assigned surety bail. Comparing these individuals gives as a measure of what would happen if a county lifted its ban on commercial bail.[37]

Table 9 demonstrates that states that ban commercial bail pay a high price. We estimate that FTA rates are 7 to 8 percentage points or approximately 30% higher under

[36] We use a linear probability model to allow for full fixed effects. The marginal effect (discrete method) of Surety Bond in a probit model with a constant and one county variable dropped are virtually identical, -4.4, -16.3, and -15.3 respectively. Restricting the linear model to the matched sample makes little difference. The coefficients on Surety Bond in the full sample are –4.9, -11.0, and -16.5 respectively.

deposit or own recognizance compared to what they would have been if the same individuals were released on surety bond.[38] As before, we find that cash bond is about as effective as surety at controlling FTA rates. The fugitive rate conditional on FTA is much higher, under own, deposit, or cash release than under surety; higher by some 15, 20, and 36 percentage points or 78%, 85 and 93% respectively - even larger figures than we found earlier.

7.2 Unobserved by us but Observed by Judges

Unobserved variables may be associated with individuals rather than with counties. Since we do not have repeated data on individuals, controlling for individual observables requires stronger identification assumptions. If unobservables associated with individuals are important, however, it's worthwhile noting that they are likely to bias the surety treatment effects *downwards*. In assigning defendants to release treatments, judges are supposed to choose the least restrictive form consistent with reasonable assurance that the defendant will appear at trial. "Cream skimming," therefore, is built into the release process and *the cream gets released on own recognizance and deposit bond* while the *skim are held or released on cash or surety bond.*

Defendants who are released on cash or surety bond were not released on their own recognizance presumably, although not necessarily, because a judge thought the FTA likelihood under such a release form would be too high. If judges observe some information that we do not, we would expect cash and surety releases to have more

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[37] Since we are interested in the cross-county variation, the propensity scores for these tests were generated from an ordered probit that did *not* include county fixed effects but was otherwise identical to that used earlier.

[38] Note that in Table 9 we examine the treatment effect of own, deposit and cash relative to surety because this is the relevant comparison when considering the experiment of lifting the ban on commercial bail. As noted earlier, the

unobserved variables pointing in the direction of higher FTA rates than those defendants released under other treatments. For example, if judges are more likely to assess bail when the evidence against a defendant is strong or when the defendant has a surly demeanor and if strong evidence or surly demeanor is associated with higher FTA and fugitive rates (as it should be if judges are doing their job) then our estimates of the surety treatment effects are *too low*.

We have already found some evidence which would suggest the bias in our results is downward. Recall from Figure 2 that the effect of selection on *observables* is to raise the fugitive rate (we focus on the fugitive rate because it is most dispositive statistic concerning the effectiveness of the surety treatment). If selection on unobservables is in the same direction as selection on observables, then our estimates of the surety treatment effect are too low. Unless there is reason to think that the process that makes one variable observed and another unobserved is correlated with the outcome it's best to assume that selection on unobservables is in the same direction as selection on observables (because if the process that determines what is observable is random we should learn something about all variables from those that we observe). In addition to this general argument, we have a specific argument. We know that what judges are supposed to do is to assign defendants with higher FTA and fugitive rates to more restrictive release categories and they should do so using *all* the variables that they observe even if some of these variables are unobserved by us.

If judges have access to information that we do not, we might expect this information to be incorporated into the bail amount. Thus, one way of accessing this

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treatment effect on the treated and untreated are similar so we could also have examined the surety treatment effect relative to the alternative release types.

information is to match on the propensity score *and* the bail amount thereby controlling for information that is unobserved to us but observed (and used) by judges.

Earlier we matched on propensity score and bail amounts when matching on FTA rates. The motivation at that time was to control for the incentive effect of bail. We can see now, however, that matching on bail also controls for information observed by judges but not observed by us. When the outcome is fugitive rates *conditional on having failed to appear*, however, there is no longer an independent effect arising from the bail amount - once a defendant has failed to appear for any significant amount of time his bail is sunk and therefore irrelevant. The only reason to match on bail when the outcome is fugitive rates is to control for potentially unobserved judicial information.

Table 10 presents the results for matching on propensity score and (log) bail. The estimate of the surety treatment v. deposit bond is lower than without matching on bail but the surety v. cash treatment effect is nearly identical to that found earlier. Unless judges act perversely they will assign defendants with a higher propensity to fail to appear to more restrictive release categories. The information theory predicts, therefore, that matching on bail will result in a larger estimated treatment effect than matching on a reduced information set. The surety v. deposit estimate is different when matching on bail but it's smaller not larger than that found when matching on propensity score. In addition, no bail effect shows up in the surety v. cash estimates. Overall, this suggests that judges do not have much information in addition to that which we observe.

We have also matched on only predicted bail generated from a Tobit model. Results (available upon request) are very similar to those presented already and are omitted here for reasons of length.

7.3 Unobserved Individual Effects

Variables associated with individuals may be observed neither by us nor by judges. We propose two identification strategies. First, some 14 percent of defendants out on pre-trial release are arrested for another crime before they are sentenced for the first crime. We assume that the probability of being rearrested is positively correlated with the probability of becoming a fugitive. Suppose, for example, that guilty defendants are less likely to show up for trial than innocent defendants and innocent defendants are less likely to be rearrested than guilty defendants. There is good evidence for some such assumption because in the raw data defendants who are never rearrested have an FTA rate of 11% but defendants who are rearrested for another crime have an FTA rate of 43%.

If rearrest is positively correlated with the probability of becoming a fugitive and *if treatment does not influence rearrest rates,* then rearrest rates by treatment will track unobserveds. Table 11 provides evidence for the second clause - in the raw data there is very little variation in rearrest rates across treatment categories.

The evidence suggests that treatment does not influence rearrest rates so any differences in rearrest rates across treatment categories can be assigned to the influence of unobserveds. Nevertheless, it is useful to consider two reasons why treatment might influence rearrest rates. First, bond dealers have an incentive to ensure that their charges show up at trial. Although the rearrest of a defendant is not usually grounds for the forfeiture of the bond dealer's bond,[39] bond dealers do monitor their charges and such

[39] The only circumstance where this might occur is if the defendant is arrested in another state and for this reason fails to show up at trial. Even in this case the surety has some time, usually 90 to 180 days, to locate the defendant before the bail is forfeited. Reynolds (2002) suggests that parole and probation bonds be created such that bond dealers would forfeit their bonds if the defendant was rearrested. If this were to occur then bond dealers would have the same incentives to reduce defendant rearrest as they today have to ensure that defendants appear at trial.

monitoring might reduce rearrest rates. Second, bond dealers might be able to select defendants who are unlikely to flee and thus also unlikely to be rearrested.[40] Note that both of this hypotheses imply that all else equal, rearrest rates should be lower for those released on surety bond.

Table 12 (matching on propensity score and bail) presents the rearrest "treatment effects" for the various release types. In no case is the rearrest rate lower for surety bond compared to other treatment types. Thus there is no evidence that monitoring significantly reduces rearrest rates or that bond dealers selectively choose defendants with low rearrest and FTA rates. Thus any "treatment effect" is best interpreted as the influence of unobserved variables and the direction of this influence is indicative of the influence of unobserved variables on FTA and fugitive rates. The surety v. own and surety v. deposit comparisons show positive but very small and statistically insignificant effects suggesting that unobserved variables have little influence on FTA and fugitive rates across these comparisons. The surety v. cash bond comparison suggests that the surety treatment increases rearrest rates by 4.5 percentage points which implies that unobserved variables operate in a direction that *offsets* the true treatment effect of surety on FTA and fugitive rates. Recall from Tables 4 and 5 that we found that FTA rates were slightly higher under surety than under cash bond. The evidence from rearrest rates suggests that unobservables may be responsible for part of this and that the true treatment effect is somewhat lower. Similarly, although we found large negative effects on fugitive

[40] Note, however, that no bond dealer could stay in business if she only bonded the innocent.

rates from the surety treatment (relative to cash treament), the evidence suggests that, if anything, that the true treatment effects may be even more negative.[41]

The rearrest data also allows for another interesting comparison. For a small subset of our data, 1331 observations from 1988 and 1990, we know the re-release type for those individuals who are arrested and released on a second charge. We do not know whether the individual failed to appear on the second charge, which is why we don't have repeated observations. Nevertheless, the second arrest and release data may be revealing.

Suppose that the initial release is own recognizance and the second release is via surety bond. By monitoring and possibly recapturing the defendant if he skips on the *second* trial, bail bondsmen and their agents create a positive externality with respect to fugitive rates on the *first* trial. This potential externality means that we need not compare own recognizance to surety releases to measure a surety treatment effect. Instead, we can compare defendants who received own recognizance with other defendants who received own recognizance in their first release and surety in their second release. Similarly, we can compare fugitive rates on the first trial for defendants whose first and second releases were own and own with those whose first and second releases were own and surety. With this comparison we control for any selection effects on the first release.

The unconditional fugitive rate of defendants who are released on their own recognizance and *not* rearrested is 8.48 percent.[42] The fugitive rate of defendants who are released on their own recognizance and who are rearrested and then released again on their own recognizance is almost identical, 8.04 percent. But the fugitive rate for those

[41] Since we find that rearrest rates vary little by treatment category we should also find that treatment effects measured in the rearrest sample, i.e. using only those defendants who were subsequently arrested for a second crime, should be similar to those found in the one-arrest sample. We have run these matching tests on propensity score and bail and do find similar results which we omit for reasons of length. Results available upon request.

defendants initially released on their own recognizance but then rearrested and rereleased on surety bond is just 1.9 percent. The difference between the own and the own-surety fugitive rate is statistically significant at the greater than 1% level. The difference between the own-own and own-surety rate, which controls for rearrest, is also statistically significant at the greater than 1% level. Table 13 summarizes.[43]

Our last identification strategy uses an instrumental variable. When jails become overcrowded judges are pressured to release individuals on their own recognizance rather than running the risk of setting bail that the defendant might not be able to secure. Bond dealers understand that overcrowded jails mean less surety business. One Arkansas newspaper headline, for example, read "Crowded jails put squeeze on bondsmen." The article noted that local bond dealers were "feeling the pinch of jail overcrowding" because more suspects were being released on their own recognizance resulting in a significant drop-off in business (Kimbrough 1989).

We define ratio as the county jail population divided by the official jail capacity. A ratio greater than 1 indicates overcrowding. We suggest that jail overcrowding is not likely to be correlated with unobservables that affect FTA and fugitive rates. In addition, to test whether ratio is an useful instrument for surety bond (relative to own recognizance) we run a first stage regression of surety bond on ratio and every other exogenous variable including the crime variables and county and year fixed effects.[44] The coefficient on ratio is –16.4 with a robust standard error of .0189 (t=8.67). A rule of

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[42] Earlier we focused on fugitive rates conditional on having FTA. We focus on unconditional fugitive rates here because we have fewer observations. We have data on rearrest and re-release type for 1988 and 1990.
[43] We have also run similar tests where we control for the charges by focusing only on those individuals whose second charge was the same as the first charge. We again find that surety release results in significantly lower fugitive rates. Results available upon request.
[44] We focus on surety versus own because the deposit bond sample is small and overcrowding is unlikely to have a large effect on the cash/surety margin. Although judges could lower bail amounts thus encouraging cash bond this

thumb is that a t statistic of 3.2 or greater suggests a reasonable instrumental variable so ratio looks like a good instrument for surety bond. Table 14 presents estimates for the effect of surety bond on FTA rates and fugitive rates conditional on FTA with ratio used as an instrument.

In the FTA equation the coefficient on surety is –17.2, consistent in sign but larger in size to that found via matching but not statistically significant at conventional levels (p=.16). Similarly the coefficient on surety in the fugitive conditional on FTA equation is negative and much larger than that found previously and is statistically significant at the greater than 5% level.[45] We have found very little evidence of selection effects using previous tests (e.g. from Figure 1-2, and using the information from rearrest rates) in which case instrumental variables add noise to the estimating equation. A Hausman test comparing the OLS results using surety bond and the IV results verifies this finding. The IV results, therefore, give us additional confidence that our previous estimates of the surety treatment effect are not greatly contaminated by unobservables and, to the extent that unobservables are important, the IV results are consistent with the earlier results in suggesting larger not smaller surety treatment effects.

In this section we have controlled in a variety of ways for county effects, individual effects observed by judges but unobserved by us and pure unobserved effects of a very general nature. We have also noted the cream that judges skim goes to own recognizance and deposit bond while the skim are released on cash or surety bond. Consistent with this, observable selection effects on fugitive rates are positive. The evidence from rearrest rates and the IV suggests that unobservables are not biasing our

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does not guarantee a reduction in overcrowding. Indeed, lower bail amounts will not necessarily increase the number of releases because lower bail amounts discourage release via surety bond.

results upwards. Taken together the evidence suggests that we have good estimates that surety release reduces FTA rates, survival times and fugitive rates.

8. Conclusions

When the default was for every criminal defendant to be held until trial, it was easy to support the institution of surety bail. Surety bail increased the number of releases relative to the default and thereby spared the innocent some jail time. Surety release also provided good, albeit not perfect, assurance that the defendant would later appear to stand trial. When the default is that every defendant is released, or at least when many people believe that "innocent until proven guilty" establishes that release before trial is the ideal, support for the surety bail system becomes more complex. How should the probability of failing to appear and all the costs this implies, including higher crime rates, be traded-off against the injustice of imprisoning the innocent or even the injustice of imprisoning the not yet proven guilty? We cannot provide an answer to this question but we can provide a necessary input into this important debate.

Defendants released on surety bond are 28 percent less likely to fail to appear than similar defendants released on their own recognizance and if they do fail to appear they are 53 percent less likely to remain at large for extended periods of time. Deposit bonds perform only marginally better than release on own recognizance. Requiring defendants to pay their bonds in cash can reduce the FTA rate to a similar rate than that for those released on surety bond. Given that a defendant skips town, however, the probability of recapture is much higher for those defendants on surety bond. As a result, the probability of being a fugitive is 64 percent lower for those released on surety bond compared to

[45] We find similar results using an instrumental variables probit.

those released on cash bond. These finding indicate that bond dealers and bail enforcement agents ("bounty hunters") are effective at discouraging flight and at recapturing defendants. Bounty hunters, not public police, appear to be the true long arms of the law.

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Table 1 : Algorithm for the estimation of $\theta_0^{m,l}$

Step 1	***Estimate the Propensity Score via an Ordered Probit*** For each treatment T=1…M and individual N=1…K obtain $[\hat{p}_N^1(x), \hat{p}_N^2(x), ..., \hat{p}_N^M(x)]$ and compute $\hat{p}_N^{m\|ml}(x) = \frac{\hat{p}_N^m(x)}{\hat{p}_N^l(x) + \hat{p}_N^m(x)}$.
Step 2	***Create a Matched Sample*** For a given pair of treatments m and l: i) Choose an observation *i* that received treatment m ii) Match i to an observation j in the treatment subsample that is less than the caliper distance and closest to *i* in terms of $\hat{p}_N^{m\|ml}(x)$. If no such observation exists drop observation *i*. (In the case of multivariate matching 'closeness' is based on the Mahalanobis distance.) iii) Repeat i) and ii) until no observations in m remain.
Step 3	***Estimate the Treatment Effect*** Subtract the mean outcome in the "untreated" matched group from the mean outcome in the matched treated group.

Table 2: Mean FTA Rates by Release Category, 1988-1996

	Own Recognizance	Deposit Bond	Cash Bond	Surety Bond	Emergency Release
Own Recognizance	26% [20,944]	5*	6*	9*	-19*
Deposit Bond		21% [3605]	1	4*	-23*
Cash Bond			20% [2482]	3*	-25*
Surety Bond				17% [9198]	-28*
Emergency Release					45% [584]

Mean FTA rates for release categories, rounded to the nearest integer, are along the main diagonal with the number of observations in square brackets. Off diagonal elements are the difference between the mean FTA rate for the row category and the mean FTA rate for the column category.
* Statistically significant at the greater than 1% level.

Table 3: Ordered Probit on Stringency of Release, also includes county and year effects (not shown).

Variable	Coefficient
Local Conditions:	
Time, in days, to scheduled start of trial	-0.5821*
	(0.0038)
Local Clearance Rate (total arrest/ total crime)	0.3957
	(0.1799)
Defendant is Charged with:	
Murder	0.35915*
	(0.051044)
Rape	0.376661*
	(0.032135)
Robbery	0.146899*
	(0.028193)
Assault	0.208538*
	(0.039397)
Other Violent	0.048705***
	(0.02932)
Burglary	-0.10109*
	(0.027554)
Theft	-0.16676*
	(0.029142)
Other Property Crime	0.212824*
	(0.026824)
Drug Trafficking	-0.1147*
	(0.027033)
Other Drug Crime	-0.01139
	(0.041254)
Driving Related Crime	-0.18755*
	(0.016514)
Defendant Characteristics:	
Age of defendant	0.000854
	(0.000653)
Female (yes=1)	0.873055*
	(0.080055)
Active Criminal Justice Status	0.191588*
	(0.013974)
Previous Felonies	0.244761*
	(0.013558)
Previous Failure to Appear	0.123918*
	(0.015137)
Number of Observations	58,585

Asymptotic Standard Errors in parenthesis
* Statistically significant at the greater than 1% level (two sided).
** Satirically significant at the greater than 5% level.
** Statistically significant at the greater than 10% level.

Table 4: Treatment Effects of Row versus Column Release Category on FTA Rates using Matched Samples, 1988-1996

	Own Recognizance	Deposit Bond	Cash Bond	Surety Bond
Own Recognizance	26%	3.2* (1.0/1.1)	4.8* (1.1/1.2)	6.5* (.78/.78)
Deposit Bond	-3.1* (1.1/1.2)	21%	4.1* (1.5/1.6)	3.1* (1.1/1.3)
Cash Bond	-5.8* (1.3/1.6)	-1.5 (1.6/2.0)	20%	1.8/2.0 (1.4/1.8)
Surety Bond	-7.3* (.78/.89)	-3.9* (1.1/1.2)	1.7 (1.3/1.4)	17%

Mean FTA rates for release categories for the full sample are along the main diagonal. Off diagonal elements are the estimated treatment effects of the row category versus the column category. Standard errors in parentheses – the first standard error assumes the p-score is estimated with certainty the second uses bootstrapping to estimate the standard error including uncertainty of the p-score.
Matching Caliper=.0001
* Statistically significant at the greater than 1% level (two sided).
** Satirically significant at the greater than 5% level.
** Statistically significant at the greater than 10% level.

Table 5: Treatment Effect of Row versus Column Release Category on FTA Rates using Samples Matched on Propensity Score and Bail Amount, 1988-1996

	Deposit Bond	Cash Bond	Surety Bond
Deposit Bond	21%	3.1 (1.9)	4.1* (1.2)
Cash Bond	-4.2** (2.0)	20%	-2.1 (1.7)
Surety Bond	-4.3* (1.3)	3.4** (1.6)	17%

Mean FTA rates for release categories for the full sample are along the main diagonal. Off diagonal elements are the difference between the mean FTA rate for the row category and the mean FTA rate for the column category. Standard errors are in parentheses.
Matching Caliper=.0001
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
** Statistically significant at the greater than 10% level.

Table 6: Treatment Effect of Row versus Column Release Category on the Fugitive Rate using Matched Samples, Conditional on FTA, 1988-1996

	Own Recognizance	Deposit Bond	Cash Bond	Surety Bond
Own Recognizance	32% [5440]	-3* (2.6)	-4.9* (2.9)	9.4* (2.1)
Deposit Bond	-.2 (2.6)	33% [766]	-6.2 (4.1)	12.1* (2.7)
Cash Bond	11.9* (3.0)	-3.8 (4.4)	40% [506]	18.6* (3.7)
Surety Bond	-17* (2.0)	-15.5* (2.9)	-25.6* (4.2)	21% [1537]

Mean fugitive rates, defined as FTAs that last longer than a year, for release categories for the full sample are along the main diagonal with the number of observations in that category conditional on an FTA in square brackets. Off diagonal elements are the difference between the mean fugitive rate for the row category and the mean fugitive rate for the column category estimated using matching. Standard errors are in parentheses.
Matching Caliper=.001
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
** Statistically significant at the greater than 10% level.

Table 7: Log Rank Test of the Equality of the Hazard Functions

	Matching on Propensity Score			Matching on Propensity Score and Bail	
	Surety v. Own	Surety v. Deposit	Surety v. Cash	Surety v. Deposit	Surety v. Cash
Surety	1033 [787]	883 [678]	852 [629]	685 [563]	501 [373]
Own	1167 [1412]				
Deposit		817 [1021]		716 [837]	
Cash			507 [729]		287 [414]
Total	2200	1700	1359		
χ^2 against null of equality of hazard rates	121*	105*	151*	44*	85*
Matched on:	Pr(surety)	Pr(surety)	Pr(surety)	Pr(surety) and bail	Pr(surety) and bail

Column entries equal the actual number of FTAs returned to court. Column entries in brackets represent the expected number of FTAs returned.
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
** Statistically significant at the greater than 10% level.

Table 8: Effect of Surety Treatment versus other Release Types on Fugitive Rates in Fixed Effects Regressions Using Only Counties with All Release Types, 1988-1996

	Surety v. Own Recognizance	Surety v. Deposit Bond	Surety v. Cash Bond
Treatment Effect	-4.3***/-6.2* (2.5/2.4)	-16.5*/-11.7* (3.9/3.4)	-15.6*/-16.9* (2.8/5.3)
Observations	1853	1670	1909

The first number is the coefficient on Surety Bond in a linear probability model run on matched samples with all the covariates in Table 3 plus county fixed effects. The second number is the treatment effect estimated via matching. Respective standard errors are in parentheses.
Matching Caliper=.001
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
*** Statistically significant at the greater than 10% level.

Table 9: Effect of Alternative Treatment versus Surety Bond on FTA and Fugitive Rates (conditional on FTA) Matching Individuals from States that have Banned Surety Bonds with Similar Individuals Released on Surety Bond, 1988-1996

	Own Recognizance v. Surety Bond	Deposit v. Surety Bond	Cash v. Surety Bond
Treatment Effect on FTA Rates	+7.8* (1.6)	+6.2* (1.8)	-1.6 (4.4)
Treatment Effect on Fugitive Rates	+14.8* (2.3)	+19.8* (2.9)	+35.7* (8.0)

Standard errors are in parentheses.
Matching Caliper=.0001
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
*** Statistically significant at the greater than 10% level.

Table 10: Treatment Effect on the Fugitive Rate using Samples Matched on Propensity Score and Bail, Conditional on FTA, 1988-1996

Surety v. Deposit Bond	Surety v. Cash Bond
-9.4* (3.3)	-25.3* (5.5)

Matching Caliper=.0001
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
** Statistically significant at the greater than 10% level.

Table 11: Mean Rearrest Rates by Release Category, 1988-1996

Own Recognizance	14.9% [20,945]
Deposit Bond	13.3% [3605]
Cash Bond	14% [2482]
Surety Bond	12% [9202]
Percentage of rearrests by release category. Number of observations in square brackets.	

Table 12: Effect of Surety Treatment Effect versus other Release Types on Rearrest Rates using Samples Matched on p-Score and Bail, 1988-1996

	Surety v. Own Recognizance	Surety v. Deposit Bond	Surety v. Cash Bond
Surety Bond	0.7 (0.6)	.58 (1.0)	4.5* (1.3)
Matched Observations	14,925	9,740	7,064

Matching Caliper=.001
Matching estimators of the surety treatment effect.
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
*** Statistically significant at the greater than 10% level.

Table 13: Unconditional Fugitive Rates by Arrest-Rearrest Category, 1988,1990

	1) Own and Not Rearrested	2) Own-Own	3) Own-Surety	*t*-test 1-3	*t*-test 2-3
Fugitive Rate	8.48 [17,828]	8.04 [191]	1.49 [134]	2.9 $p_{1>3=.0019}$	2.6 $p_{2>3=.0047}$

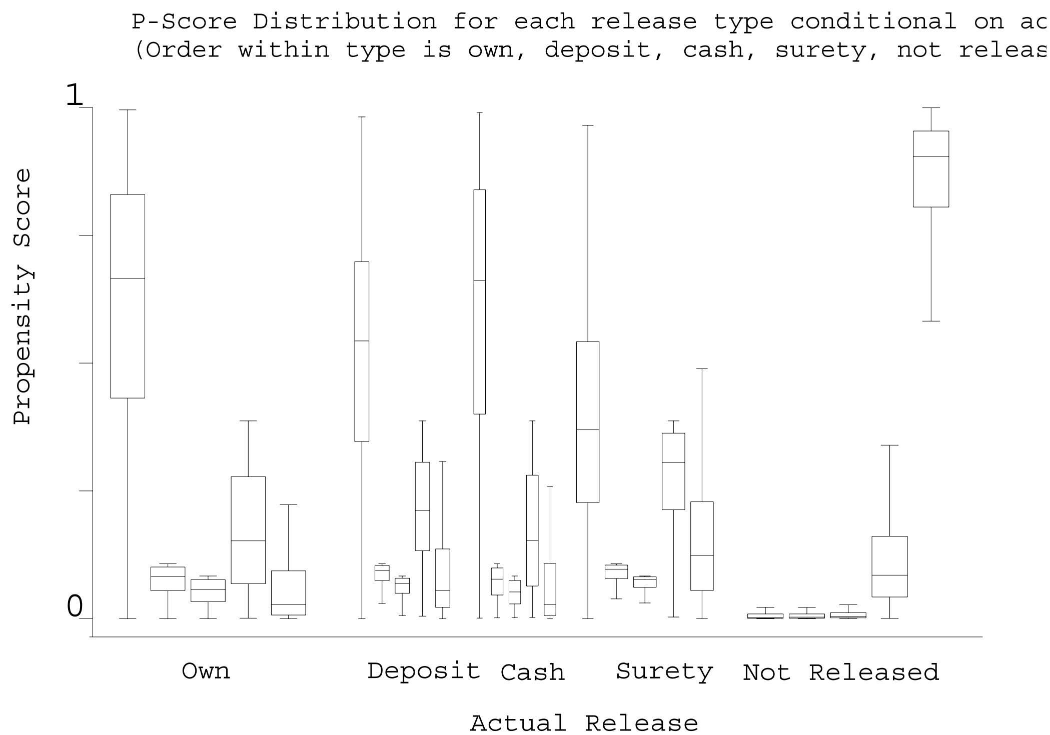
Own-Own indicates first release on own recognizance and second release on own recognizance. Own-Surety indicates first release on own recognizance, second release on surety.

Table 14: Surety vs. Own Recognizance Treatment Effect Estimated using Ratio as an Instrument for Surety Bond, 1988-1996

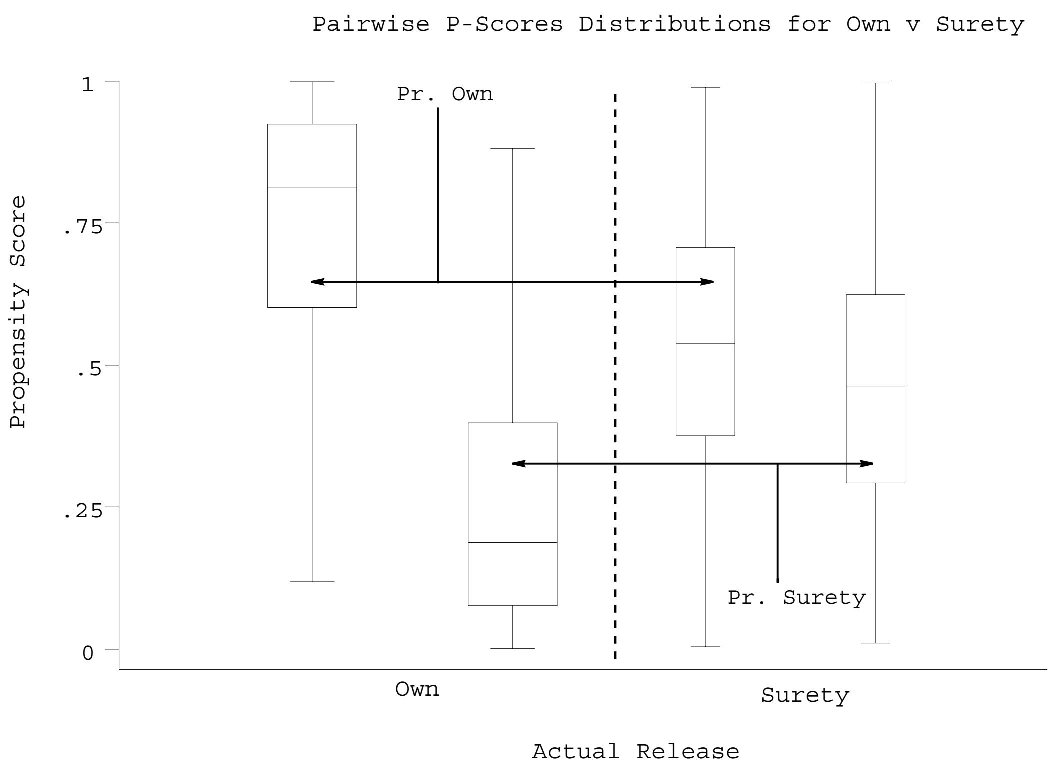
	FTA Rate	Fugitive Rate Conditional on FTA
Surety Bond	-17.2 (12.2)	-79.7** (36.3)
Observations	22,136	4698

Robust standard errors.
* Statistically significant at the greater than 1% level (two sided).
** Statistically significant at the greater than 5% level.
*** Statistically significant at the greater than 10% level.

A)


P-Score Distribution for each release type conditional on ac
(Order within type is own, deposit, cash, surety, not releas
1
0
Propensity Score
Own
Deposit
Cash
Surety
Not Released
Actual Release

B)


Pairwise P-Scores Distributions for Own v Surety
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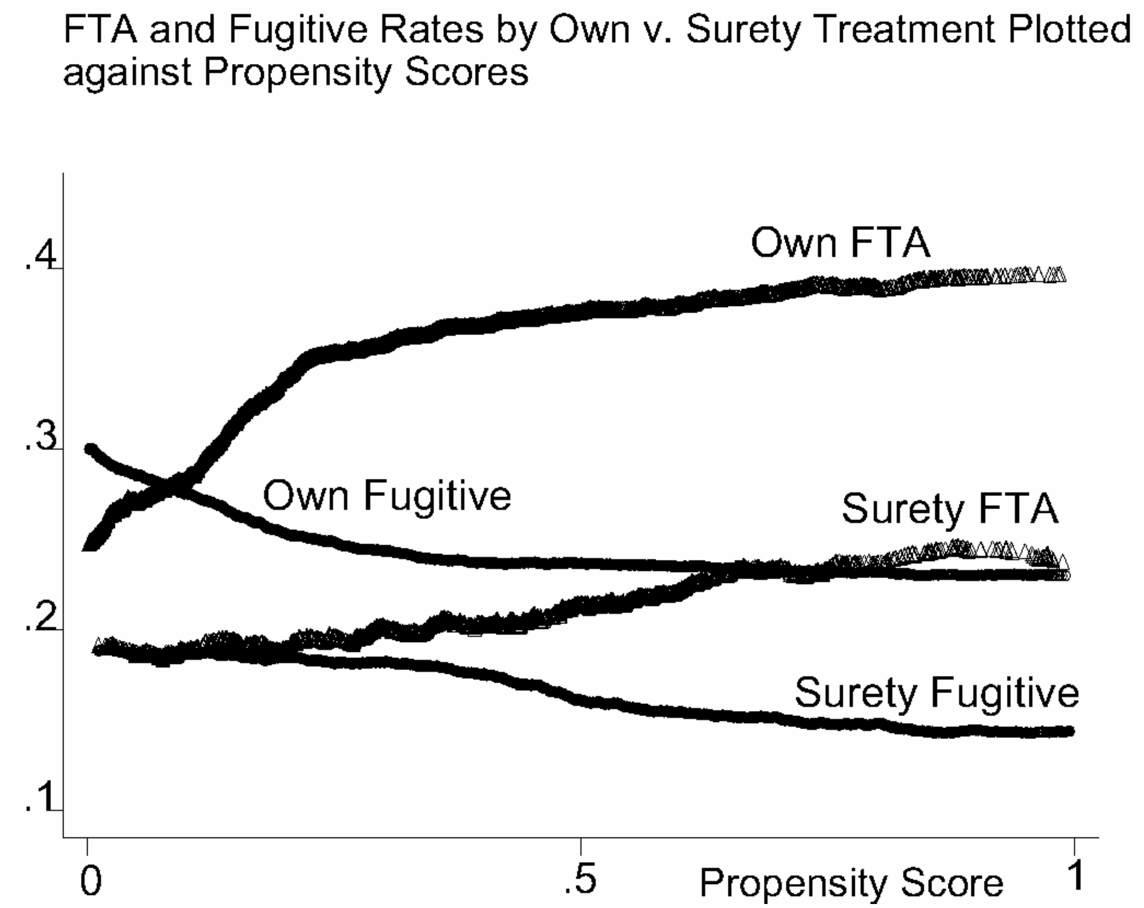
.75
.5
.25
0
Propensity Score
Pr. Own
Pr. Surety
Own
Surety
Actual Release

Figure 2


FTA and Fugitive Rates by Own v. Surety Treatment Plotted against Propensity Scores
Own FTA
Own Fugitive
Surety FTA
Surety Fugitive
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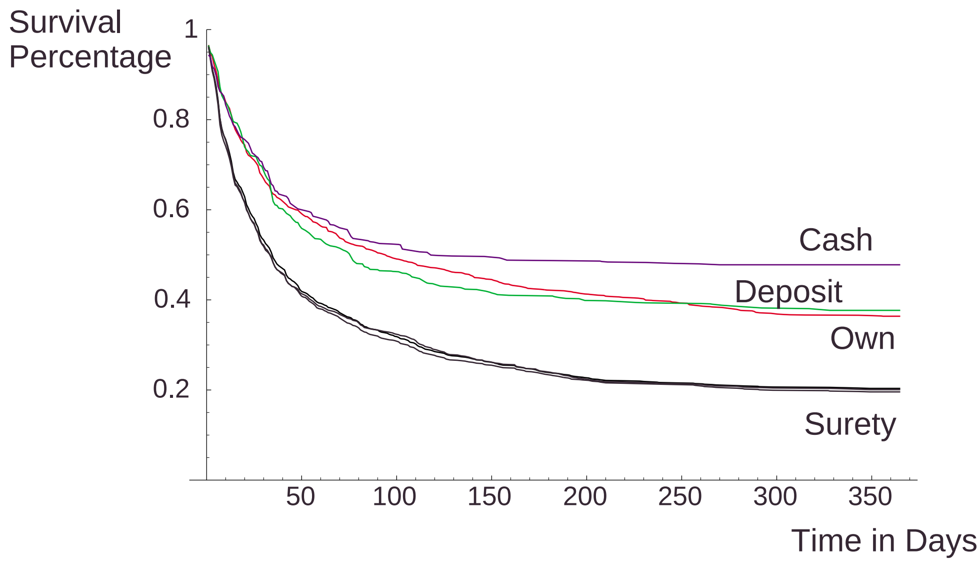
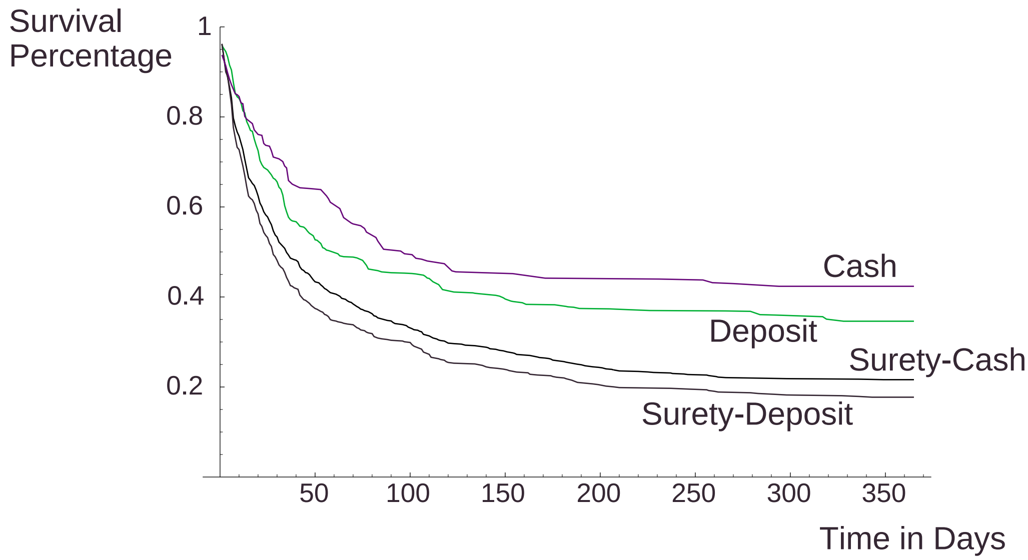
.3
.2
.1
0
.5
Propensity Score
1

Figure 3



Kaplan-Meier Survival Function for Defendants on Surety bond versus those on Cash bond, Deposit bond or released on their own recognizance - using matched samples

Figure 4



Kaplan-Meier Survival Function for Defendants on Surety bond versus those on Cash and Deposit bond - using samples matched on propensity score and bail