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Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment

David S. Abrams and Chris Rohlfs^{*}

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Abstract

This paper performs a cost-benefit analysis to determine socially optimal bail levels that balance the costs to defendants against the costs to other members of society. We consider jailing costs, the cost of lost freedom to incarcerated defendants, and the social costs of flight and new crimes committed by released defendants. We estimate the effects of bail amounts on the fraction of defendants posting bail, fleeing, and committing crimes during pre-trial release, using data from a randomized experiment. We also use defendants' bail posting decisions to measure their subjective values of freedom. We find that the typical defendant in our sample would be willing to pay roughly \$1,000 for 90 days of freedom. While imprecise, our optimal bail estimates are similar to the observed levels of bail prior to bail reform.

JEL Classifications: J17, J19, K14, K42

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On a typical day in the United States, roughly 300,000 untried defendants are incarcerated and 700,000 are free on bail.¹ While the tradeoffs a judge must consider when setting bail are well-recognized, little is known about optimal bail policy. Our objective is to provide a rigorous framework for understanding bail policy and to estimate socially optimal levels of bail.

Following Landes (1973, 1974), we explicitly model the process of bail setting as a welfare maximization problem in which an optimal social planner would seek to minimize the total cost incurred by society. The four costs that enter the planner's problem are the social cost of jailing the defendant, the private cost to the defendant from being incarcerated, the cost of crimes a defendant may commit while awaiting trial, and the cost to society of a criminal absconding. An increase in bail levels would lead to higher numbers of defendants who could not afford to post bail and would have to remain in jail until their trials. Detaining these additional individuals would impose costs on the justice system (who must feed, house, and monitor the incarcerated defendants) and also on the defendants, who would suffer from lost freedom. A decrease in bail levels would increase the number of defendants who could afford to post bail. The justice system and potential victims would then face the risks of these defendants possibly absconding and/or committing new crimes.

The current study examines how to optimally balance these various consequences of changing the level of bail. First, we empirically estimate the effects of bail levels on the fraction of defendants posting, fleeing, and committing crimes during pre-trial release. Next, we assemble estimates of the per-unit costs associated with each of these pre-trial outcomes. We then combine these per-unit costs with our estimated causal relationships to measure the total cost incurred by society as a causal function of bail levels. Finally, using our estimates, we calculate optimal bail levels for different types of defendants, and we make recommendations for social welfare maximizing bail policies.

Ordinary least squares estimates of the effects of bail are likely to suffer from omitted variables bias. For example, judges assign higher bail levels to defendants deemed as “dangerous” in ways that may be unobservable to the econometrician. These defendants will also be more likely to commit subsequent crimes or to abscond. The omission of the “dangerous”

¹ Sources and calculations described in Table 1 and in the web appendix.

variable will cause ordinary least squares to understate the effects of bail on flight and rearrest risk.

We address this concern by using data from the 1981 Philadelphia Bail Experiment (Goldkamp and Gottfredson, 1982). In this experiment, judges randomly assigned to the treatment group were given bail guidelines to use, while members of the control group set bail as they had previously. The bail guidelines caused the treatment judges to set significantly lower levels of bail than those set by the control group. Since defendants were randomly assigned to judges, the experiment induced exogenous variation in the bail levels faced by defendants. We use this experiment to obtain unbiased estimates of the effects of bail on posting, failure-to-appear at trial, and rearrests.

In addition to estimating optimal bail policies, this study contributes to the economics of crime literature by estimating the value that defendants place on 90 days of freedom. This parameter is a necessary input into our estimates of the socially optimal bail and is of interest in its own right. This utility cost associated with incarceration is typically ignored in cost-benefit studies of crime (Levitt, 1996; Lochner and Moretti, 2004; Miller, Cohen, and Wiersema, 1996). To calculate the value to defendants of lost freedom, we apply the concept of revealed preference to defendants' bail posting decisions. If a defendant posts bail at a given level, we infer that his value of freedom exceeds the cost of posting that amount. Using a discrete choice framework, we construct a measure of the latent variable – the value of freedom – that motivates defendants' bail posting decisions.

After estimating the effect of bail on the probability of defendants posting, fleeing, and committing crimes during pre-trial release, we multiply these effects by the costs associated with the outcomes. We estimate the cost of detention to the defendant using the revealed preference approach described above. We use estimates from the literature for the cost to the justice system of incarceration and the cost of crime to victims, and we use expert estimates for the cost of apprehending an absconding defendant. Given our estimates of the total cost to society at each level of bail, we use a numerical optimization algorithm to solve for socially optimal bail levels for different types of defendants.

While imprecise, our estimates suggest that optimal levels of bail are similar to the levels set by the judges in our dataset in the absence of guidelines. Due to the structure of the experiment, our sample is restricted to defendants who were flight or rearrest risks or were

accused of serious crimes. Among these defendants, we estimate an elasticity of posting with respect to bail of -0.3. We find that the typical defendant in our sample has a willingness to pay of roughly \$1,000 (measured in 2003 dollars) for 90 days of freedom. This seemingly low estimate may result in part because they pertain to a particularly poor segment of the population. Credit constraints may also affect the estimate.²

For the typical defendant, we estimate optimal bail to be roughly \$17,700, which is close to the average bail observed in the data (\$19,000) and more than twice the average levels recommended by the guidelines in the bail experiment (\$6,380). For defendants with low, medium, and high levels of dangerousness, we estimate subjective values of freedom of \$6,800, \$800, and \$980; however the estimate for the first group is very imprecise. For these same three categories, our optimal bail estimates, while imprecise, are \$12,300, \$15,800, and infinity (*i.e.*, certain detention), respectively.

The rest of the paper is structured as follows. In Section II, we discuss the history and purposes of bail and of the Philadelphia Bail Experiment. Section III contains a model of optimal bail. In Section IV, we describe the data from the Philadelphia Bail Experiment. We describe the econometric methods that we use in Section V. Section VI details our main empirical results. Section VII concludes.

II. Background

Bail policy as it is commonly applied in the United States derives from English common law. The historical purpose of bail was to allow potentially innocent defendants to go free while awaiting trial, but to provide monetary incentives for them to appear. In the early to mid-20th century, however, a number of criminologists criticized the bail system for being arbitrary and unfair (Beeley, 1927; Foote, 1954). To address these concerns, researchers have studied ways of making bail more fair and systematic, such as accounting for defendant community ties in the Manhattan Bail Project (Ares, Rankin, and Sturz, 1963), judicial guidelines (Goldkamp and Gottfredson, 1984) and using economic modeling and empirical estimation of the costs and benefits of changing bail levels (Landes, 1973, 1974).³

² These considerations are discussed in detail in the web appendix.

³ In addition, two more recent studies examine ways in which such economic modeling of bail cases can be used to test for racial discrimination in bail-setting (Ayres and Waldfogel, 1994; Bushway and Gelbach, 2006).

We attempt to build upon the economic studies by Landes in three ways. First, we use experimental data to disentangle the effects of bail policy from confounding unobservables, such as the degree to which defendants appear dangerous. Second, we use data on the decision to post bail to estimate defendants' private valuations of being free before their trials. Third, we perform cost-benefit analysis in a more explicit way than has been done before, and we directly estimate the welfare-maximizing bail levels for different types of defendants.

Table 1 presents some general features about bail in the United States. In mid-year 2000, roughly 2.1 million persons were incarcerated in America. Of those, we estimate that 300,000 had not yet been tried and were potentially innocent. At the same time, we estimate that roughly 700,000 felony defendants were free and awaiting trial. The average bail amount was nearly 10 times higher among defendants who were detained (\$59,700) than among defendants who were released (\$6,200).

The statistics shown in Table 1 illustrate the tradeoffs between the costs and benefits of bail. On average, defendants who were released on bail are considerably less likely to be eventually sentenced than were defendants who were detained. Among those detained, roughly 64% were eventually sentenced to jail or prison. Among those released, only 25% were eventually sentenced. Hence, of the 1 million people awaiting trial, about 600,000 never received a jail or prison sentence, although roughly 100,000 of those people had been detained. Of the remaining 400,000 who were eventually sentenced, about half were free until their trials. Crime and flight are not uncommon among released defendants. In 16% of cases, released defendants were rearrested for other crimes during the pre-trial period. Moreover, roughly 22% failed to appear for at least one scheduled court appearance.

Because this study uses data from Philadelphia, it is helpful to establish some key features about bail in that city. Bail procedure in Philadelphia is similar to that in many large cities. Bail hearings typically occur within 24 hours of arrest, usually earlier. The hearings typically last a few minutes and include brief arguments and recommendations by the prosecutor and the defense attorney before the judge decides upon an amount (Goldkamp, 1984). In order to be freed, defendants were required to deposit 10% of the bail amount.

Bail bondsmen were illegal in Philadelphia during the time of the experiment; thus, defendants had to pay the bail amount themselves or borrow funds from friends or relatives. If they did pay bail, defendants were free until trial, unless they violated bail by failing to appear

for scheduled court appearances or by being arrested for new crimes. Defendants not posting bail remained in jail until trial, an average of 90 days after arrest. Defendants could post bail at any time, but most did so within a few days of arrest. Following the completion of the trial, the 10% deposit was returned to defendants minus a 3% administrative fee, for a net of 7% of the original bail amount. The 3% was charged to all defendants regardless of the outcome of the trial. Defendants who violated the terms of release were liable for 100% of bail; however, the court was typically only able to extract a fraction of that amount.

III. Conceptual Framework

In this section, we develop a conceptual framework for estimating optimal bail amounts. First we consider the costs of bail and pre-trial detention to the defendant. Second, we consider the costs of bail to the justice system and to society at large. Finally, we model the bail-setting decision of a social planner who wishes to minimize total costs.

A. Costs to Defendants and the Value of Freedom

Consider a one-period model in which defendant i 's utility depends on consumption c_i and freedom f_i :

$$(1) \quad u_i = u_i(c_i, f_i)$$

Define $f_i = 1$ and $f_i = 0$ to be the levels of freedom under the cases of pre-trial release and detention, respectively. All defendants are endowed with initial wealth w_i and freedom $f_i = 0$. Wealth may be used for consumption or for payment of bail. Each defendant has the right to not post bail and to remain in jail, consuming w_i . In addition, each defendant is assigned a bail amount, $bail_i$. A defendant who posts bail can be free for the period until the trial, so that $f_i = 1$. A defendant who does not post bail is detained until trial, so $f_i = 0$.

To post bail, the defendant must pay 10% of the bail amount to the court. Once the trial is over, 7% of the bail amount is returned to the defendant. The total cost of posting bail is the permanent loss of 3% of $bail_i$ plus the temporary loss of 7% of $bail_i$ for 90 days. For this study,

we assume a 90-day discount rate of 0.10, giving a discounted present cost of posting bail of $0.037 * bail_i$.⁴ The defendant posts if the following condition holds:

$$(2) \quad u_i(w_i - 0.037 * bail_i, 1) \geq u_i(w_i, 0)$$

Let V_i be the discounted value of bail at which i would be indifferent to posting. That is:

$$(3) \quad u_i(w_i - V_i, 1) = u_i(w_i, 0)$$

By revealed preference, defendant i chooses release if and only if:

$$(4) \quad V_i \geq 0.037 * bail_i$$

V_i represents the lump sum that defendant i would regard as equivalent to the freedom lost in detention, which we refer to as the value of freedom. The value of freedom modeled here includes all the costs and benefits of release, including the option to flee or commit new crimes. It is this composite measure that is relevant for cost-benefit analysis.

Let $Post_i$ be a dummy variable for whether the defendant chooses release or not. Many other factors such as family ties, probability of conviction, wealth, income, or employment may also be correlated with V_i . Some of these determinants of the value of freedom are observable to the social planner, and some are not. Hence, conditional on $bail_i$ and defendant characteristics $\mathbf{X}_i$, both $Post_i$ and V_i are random variables. Let $P^{Post}(bail_i, \mathbf{X}_i)$ be the probability that a defendant with characteristics $\mathbf{X}_i$ chooses to post at the bail level $bail_i$. In the case of release, the total cost imposed on the defendant is $0.037 * bail_i$. In the case of detention, the cost to the defendant is V_i . Putting these together, the expected cost associated with bail amount $bail_i$ can be expressed as:

$$(5) \quad E[C_i^{Defendant} | bail_i, \mathbf{X}_i] = P^{Post}(bail_i, \mathbf{X}_i) * 0.037 * bail_i + E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i]$$

where $C_i^{Defendant}$ is the total cost imposed on the defendant.

B. Costs to the Justice System and to Potential Victims

In addition to costs to defendants, pre-trial detention and release impose costs on the justice system and on society at large. In the case of detention, the justice system must pay the administrative, food, and housing costs of the defendant in jail. Let C^{Jail} be the cost to the justice

⁴ This 10% discount rate is taken from the typical interest rate charged for bail bond services in the U.S. We explore the sensitivity of our results to this assumption in the web appendix.

system of jailing a defendant for 90 days. The expected cost to the justice system of detention is then $(1 - P^{Post}(bail_i, \mathbf{X}_i)) * C^{Jail}$.

In the case of release, the defendant may impose costs on the justice system or on potential victims by fleeing or committing new crimes. Let $P^{Flight}(bail_i, \mathbf{X}_i)$ and $P^{Crime}(bail_i, \mathbf{X}_i)$ be the probabilities that a defendant with characteristics $\mathbf{X}_i$ facing bail level $bail_i$ will flee or commit new crimes.⁵ These two terms are calculated as the total number of incidents of flight and the total number of new crimes, each divided by the total number of defendants. Hence, these terms represent unconditional probabilities, and defendants who do not post are still included in the totals in the denominator. Let C^{Flight} and C^{Crime} represent the costs associated with flight and new crimes, respectively. C^{Flight} includes the administrative cost of rescheduling court dates plus the cost of recapture. C^{Crime} represents the total cost that an additional crime imposes on society and the justice system. In expectation, the total costs of flight and new crimes can be expressed as $P^{Flight}(bail_i, \mathbf{X}_i) * C^{Flight}$ and $P^{Crime}(bail_i, \mathbf{X}_i) * C^{Crime}$, respectively.⁶

Finally, in the case of release, the justice system benefits by receiving the defendant's deposit of 10% of $bail_i$. The justice system holds 7% of the bail amount for 90 days and it keeps the remaining 3%. For simplicity, we assume that the justice system discounts at the same rate as the defendant and that the total value of the transfer to the justice system is about $0.037 * bail_i$.

To obtain the total expected cost to the justice system and to society, we combine these four components. For a defendant with characteristics $\mathbf{X}_i$ facing bail level $bail_i$, this total expected cost is:

$$(6) \quad E[C_i^{Society} | bail_i, \mathbf{X}_i] = (1 - P^{Post}(bail_i, \mathbf{X}_i)) * C^{Jail} + P^{Flight}(bail_i, \mathbf{X}_i) * C^{Flight} \\ + P^{Crime}(bail_i, \mathbf{X}_i) * C^{Crime} - P^{Post}(bail_i, \mathbf{X}_i) * 0.037 * bail_i$$

where $C_i^{Society}$ is the cost imposed on the justice system and society by defendant i 's pre-trial detention and release.

⁵ In principle, a defendant might flee multiple times or be arrested multiple times during pre-trial release. In practice this phenomenon is rare, and we ignore this possibility.

⁶ Due to data limitations, we assume that C^{Jail} , C^{Flight} , and C^{Crime} are constant across criminals.

C. The Social Planner's Problem

Next, we consider the problem of the optimal social planner. This social planner sets bail to minimize the total expected cost to defendants, the justice system, and to society at large. In principle, the social planner might place different weights on the costs imposed on these different groups. As a benchmark, we suppose that the planner values all three equally. Combining the cost terms from Equations (5) and (6) we can express the planner's problem as:

$$(7) \quad \min_{bail_i} E[C_i^{Total} | bail_i, \mathbf{X}_i] = \min_{bail_i} E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i] + \\ (1 - P^{Post}(bail_i, \mathbf{X}_i)) * C^{Jail} + P^{Flight}(bail_i, \mathbf{X}_i) * C^{Flight} + P^{Crime}(bail_i, \mathbf{X}_i) * C^{Crime}$$

The first term in the minimization, $E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i]$, measures the expected value of the freedom that is lost by detaining the defendant. The second term, $(1 - P^{Post}(bail_i, \mathbf{X}_i)) * C^{Jail}$, measures the expected cost to the justice system of jailing the defendant. The third term, $P^{Flight}(bail_i, \mathbf{X}_i) * C^{Flight}$, measures the expected administrative cost associated with the possibility of the defendant fleeing. The fourth term, $P^{Crime}(bail_i, \mathbf{X}_i) * C^{Crime}$, measures the expected cost to potential victims of new crimes that the defendant may commit if released. Because the planner values the defendant's and justice system's welfare equally, the cash transfer of posting bail does not affect social welfare. The four terms appearing in Equation (7) represent the total expected cost associated with the pre-trial detention or release of defendant i .⁷

IV. Data and Descriptive Evidence

To obtain unbiased estimates of the derivatives of the probabilities in Equation (7), we use data from the 1981 Philadelphia Bail Experiment. This experiment was conducted by criminologists in conjunction with the Municipal Court of Philadelphia and is described in detail in Goldkamp and Gottfredson (1984, 1985). The experiment examined trials assigned to 16 Philadelphia bail judges from 1981 to 1982. Of these 16 judges, 8 were randomly assigned into a treatment group, in which they were asked to set bail amounts according to a pre-determined set of guidelines. The remaining 8 judges were assigned into a control group and asked to continue assigning bail as they had before. For each judge, the researchers collected data on 120 cases

⁷ Potential criminals might also change their behavior in anticipation of the bail amounts they would be assigned if arrested. However, these costs are likely to be small relative to other costs associated with arrest. We assume that these effects are negligible.

with varying degrees of charge seriousness. The resulting sample includes a total of 1,920 observations, including 960 felony cases.

Table 2 shows the guidelines matrix that was presented to judges in the treatment group. The horizontal dimension of the grid shows probability of failure, a 5-point index based on observable defendant characteristics, including criminal history, age, and community ties. The vertical dimension of the grid shows charge severity, a 15-point index based on the statute the defendant is accused of violating. The 75 cells in the grid correspond to all the possible combinations of the 15-point and 5-point indices.

Each cell contains a range of recommended dollar amounts for bail or “release on recognizance” if the recommended bail amount was zero. Both the minimum and maximum recommended amounts increase with charge severity and the estimated flight or rearrest risk of the defendant. In the upper left corner, charges are the least severe, defendants are the least risky, and the guidelines recommend \$0 bail. In the lower right corner, charges are the most severe, defendants are the most risky, and recommended bail ranges from \$3,000 to \$10,000 in 1981 dollars (\$6,070 to \$20,200 in 2003 dollars). For the purposes of this study, the most important aspect of the guidelines is that they tended to recommend lower bail amounts than were typical at the time. Consequently, for many types of defendants in the study, assignment into the treatment group can be used as an instrument for bail amounts.

There are two key experimental features that are necessary to verify: random assignment of subjects and that there was a significant treatment effect. We present evidence for the latter feature in Figure 1. Panel A of Figure 1 compares the recommended bail amounts to the bail amounts assigned by judges in the control group. The horizontal axis shows in 2003 dollars the midpoint of the recommended bail range from the cells in Table 2. The dashed, 45-degree line shows bail levels that would be observed if judges always assigned the midpoint of the assigned range. The circles show the average bail amounts that judges in the control group actually assigned. Since the control judges were not told what the recommended bail ranges were, the bail amounts tell us the levels at which judges would assign bail in the absence of the guidelines. On average, the guideline amounts are substantially lower than the bail levels chosen by the control group judges, particularly for more severe and riskier offenders.

An important aspect of the experimental design is that the randomization applies to each cell of the guidelines. Thus, each cell may be seen as a separate experiment. For some cases,

observed bail amounts among control judges were similar to the recommended levels. In these cases, judges were asked to do what they were already doing, and we should not expect to see an effect of treatment on bail amounts. In other cases, however, the recommended bail levels differ significantly from the levels observed among control judges. It is these cases that are useful for the purposes of this study, because in these cases, assignment to the treatment group has the potential to affect bail levels. The black circles in Panel A of Figure 1 indicate the cells for which the recommended bail levels differ significantly from the bail levels observed among judges in the control group. These are the cells that are included in the sample for the regressions. The white circles indicate cells for which the recommended bail levels do not differ significantly from bail levels in the control group. These cells are excluded from the regression sample, because for these cases, we do not expect to see an effect of treatment on bail levels.⁸

Panel B of Figure 1 compares observed and recommended bail amounts for the treatment group. In general, the observed bail amounts are higher than the recommended bail amounts; however, these differences are smaller for the treatment group than for the control group. For both the \$15,000 to \$30,000 and \$30,000 to \$45,000 ranges on the graphs, we see more cells in the control group than in the treatment group, indicating that defendants in the control group were assigned higher bail levels. Notably, at the highest recommended bail levels, where the defendants are likely to have been the most dangerous, the treatment judges were most likely to deviate from the guidelines and assign high bail amounts.

Table 3 presents descriptive statistics for the data used in this study. Column (1) shows means for the treatment group, column (2) shows means for the control group, and column (3) shows the difference in means between the two groups. Column (4) shows robust standard errors for the differences in means, and column (5) shows these standard errors after clustering at the judge level. Of the 75 cells shown in Table 2, the regression sample includes data from 17 cells for which the recommended bail level was significantly different from the level observed in the treatment group (the black circles in Figure 1). Because the regressions take the log of bail, the sample also excludes 81 observations for which bail was equal to zero. Of the 1,920 defendants in the original sample, 487 satisfy these criteria and are included in the regression sample.

⁸ Restricting the sample in this way should not affect the consistency of our estimates. We explore the implications of this sample restriction in detail in the web appendix.

The evidence from Table 3 supports our conjecture that the experiment was truly random. The defendants in the treatment group are similar to defendants in the control group across a wide range of characteristics. Importantly, the midpoint of the recommended bail range is almost identical for defendants regardless of their treatment status (\$6,350 for treatment defendants and \$6,420 for control).

To verify that there was a significant treatment effect, we compare bail amounts assigned by treatment and control judges. For the sample shown here, observed bail is significantly lower for the treatment group than for the control group. Among treatment defendants, the average assigned bail is \$13,500. Among control defendants, the average observed bail is considerably larger, at \$19,000. At these rates, 68% of the treatment defendants posted bail – substantially more than the 54% in the control group. This difference is significant using robust standard errors but is only marginally significant with clustering by judge.

Panels A through D of Figure 2 visually display differences between treatment and control for the variables of interest in this study. Panel A shows differences in observed bail. Panels B, C, and D show differences in the fractions posting, failing-to-appear, and rearrested, respectively. For each outcome, differences are plotted for the 75 different cells.

Treatment status appears to have had effects on some of the outcomes of interest. For the cells in the regression sample, the difference in bail set between the treatment and control groups is generally negative. For these same cells, we also observe positive differences for release and rearrest, and slightly positive differences for failure-to-appear. Two of these 3 differences are significantly different from zero. For the remainder of the study, we focus on the cells that are included in the regression sample (the black circles). To increase the precision of our estimates, we consider all 17 of these cells together as a group.

V. Econometric Methods

In the next few paragraphs, we develop a revealed preference framework for estimating $E[V_i | \mathbf{X}_i]$ from defendants' release decisions. We then outline an instrumental variables probit approach for estimating $P^{Post}(bail_i, \mathbf{X}_i)$, $P^{Flight}(bail_i, \mathbf{X}_i)$ and $P^{Crime}(bail_i, \mathbf{X}_i)$. Finally, we describe our computational approach for estimating the socially optimal bail amount.

A. Revealed Preference and the Value of Freedom

We assume that the value of freedom, like wages, wealth, and many other dollar-denominated individual characteristics, is lognormally distributed. Suppose that $\ln(V_i)$ can be expressed as a linear combination of defendant characteristics $\mathbf{X}_i$ and normally distributed error ε_i^{Post} :

$$(8) \quad \ln(V_i) = \boldsymbol{\beta}^{Post} \cdot \mathbf{X}_i - \varepsilon_i^{Post}$$

As stated in Equation (4), defendant i posts bail if and only if i 's subjective value of freedom exceeds the cost of release. Hence, defendant i posts if and only if $V_i \geq 0.037 * bail_i$. The probability of this event can then be expressed as:

$$(9) \quad P_i^{Post} = \Pr(V_i \geq 0.037 * bail_i)$$

Taking logs and substituting Equation (8) into Equation (9), we obtain:

$$(10) \quad P_i^{Post} = \Pr(\boldsymbol{\beta}^{Post} \cdot \mathbf{X}_i - \varepsilon_i^{Post} \geq \ln(0.037 * bail_i))$$

Rearranging terms, we can express this probability in terms of $F^{Post}(\cdot)$, the cumulative distribution function of ε_i^{Post} :

$$(11) \quad P_i^{Post} = F^{Post}(\boldsymbol{\beta}^{Post} \cdot \mathbf{X}_i - \ln(0.037 * bail_i))$$

By the assumption that ε_i^{Post} is normally distributed, $F^{Post}(\cdot)$ is a cumulative normal distribution function. Let σ_{Post} be the standard deviation of ε_i^{Post} . If we divide the term inside the cumulative distribution function by σ_{Post} , we obtain a probit regression specification:

$$(12) \quad P_i^{Post} = \Phi\left(\frac{\boldsymbol{\beta}^{Post} \cdot \mathbf{X}_i - \ln(0.037 * bail_i)}{\sigma_{Post}}\right).$$

In a typical discrete choice setting, the latent variable (in this case V_i) has no natural units. In such cases, probit estimation identifies the parameters of interest up to scale, so that $\frac{1}{\sigma_{Post}} \boldsymbol{\beta}^{Post}$ would be known, but not $\boldsymbol{\beta}^{Post}$. In our case, however, both V_i and $bail_i$ are measured in dollars. The defendant's decision rule compares these two variables directly. We use this direct comparison to convert our predicted values from the probit regression from probability

units into dollar units. Our estimated coefficient on $\ln(bail_i)$ in the posting equation is $-\frac{1}{\sigma_{Post}}$.

To estimate β^{Post} , we divide the coefficients on $\mathbf{X}_i$ by negative one times the coefficient on $\ln(bail_i)$.⁹

Next, we consider the estimation of the value of freedom. There are two measures of freedom that we estimate in this study. The first measure is the average value of freedom for an individual with a specific set of characteristics. This term is expressed as $E[V_i | \mathbf{X}_i]$. This parameter provides information about the degree to which freedom is valued among criminal defendants. $E[V_i | \mathbf{X}_i]$, depends on β^{Post} , the coefficients from the defendant's release decision. Substituting our formula for $\ln(V_i)$ from Equation (8), we can express $E[V_i | \mathbf{X}_i]$ as

$$(13) \quad E[V_i | \mathbf{X}_i] = E\left[e^{\beta^{Post} \cdot \mathbf{X}_i - \varepsilon_i^{Post}}\right] \\ = e^{\beta^{Post} \cdot \mathbf{X}_i + \frac{\sigma_{Post}^2}{2}}$$

where $E\left[e^{\varepsilon_i^{Post}}\right] = e^{\frac{\sigma_{Post}^2}{2}}$ by normality.

The second measure that we estimate is the expected value of the loss in freedom among individuals who do not post bail. This expected loss is the measure that appears in the social planner's cost function. As mentioned earlier, this expected value can be expressed mathematically as $E[(1 - Post_i) \cdot V_i | bail_i, \mathbf{X}_i]$. Figure 3 illustrates this value graphically. The diagonal line shows the demand for freedom (*i.e.*, the fraction posting bail) for individuals with characteristics $\mathbf{X}_i$.¹⁰ The value of freedom varies among defendants with these characteristics, due to heterogeneity in ε_i^{Post} . At a given bail level $bail_i$, individuals with values of freedom lower than $0.037 \cdot bail_i$ do not post. These individuals appear to the right of $P^{Post}(bail_i, \mathbf{X}_i)$ on the horizontal axis. The shaded area under the demand curve represents the total value of these individuals' lost freedom.

⁹ Warner and Pleeter (2001) use a similar approach to estimate private discount rates from military employees' pension plan decisions.

¹⁰ For Figures 3 and 4 and for the estimates in Table 7, the set of control variables $\mathbf{X}_i$ includes only the fixed effects for the different cells shown in Table 2.

Because defendants choose to post or not, the cost of lost freedom is lower than it would be if detention were randomly assigned. If $Post_i$ and V_i were independent, then the expected loss of freedom would simplify to $(1 - P^{Post}) * E[V_i | \mathbf{X}_i]$. However, $Post_i$ and V_i are correlated in a systematic way through revealed preference. The defendants who remain in jail will be those whose subjective values of freedom are especially low. Given the formulation of V_i in Equation (8), $E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i]$ can be simplified to the following expression:¹¹

$$(14) \quad E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i] = \left[1 - \Phi \left(\frac{\boldsymbol{\beta}^{Post} \cdot \mathbf{X}_i - \ln(0.037 * bail) + \sigma_{Post}^2}{\sigma_{Post}} \right) \right] * E[V_i | \mathbf{X}_i]$$

This expression differs from $(1 - P^{Post}) * E[V_i | \mathbf{X}_i]$ through the term σ_{Post}^2 , which appears inside the cumulative normal. This σ_{Post}^2 term adjusts for the covariance between V_i and $Post_i$.

B. Estimation of Posting, Flight, and Additional Crime Probabilities

Next, we consider a probit framework for estimating each of the two remaining binary decisions: failing to appear and committing new crimes. Suppose that defendant i takes the action if a linear combination of $\ln(bail_i)$ and defendant characteristics exceeds some normally distributed error. Assuming that our relevant error terms, ε^{Flight} , and ε^{Crime} , have zero means and variances σ_{Flight}^2 , and σ_{Crime}^2 , we obtain a probit formulation:

$$(15) \quad P_i^j = \Phi \left(\frac{\alpha^j_0 + \alpha^j_1 \ln(bail_i) + \boldsymbol{\beta}^j \cdot \mathbf{X}_i}{\sigma_j} \right), j \in \{Flight, Crime\}$$

where $\Phi(\cdot)$ is the standard normal distribution.

One potential problem in estimating the effects of bail on posting, failure-to-appear, and additional crimes is omitted variables bias. Judges set especially high bail amounts for defendants whom they suspect may flee or commit new crimes. This policy on the part of judges may cause cross-sectional estimates to overstate bail's effects on flight and new crimes. These risky defendants may also be career criminals who place particularly low values on freedom

¹¹ The derivation is provided in the web appendix.

from jail. Hence, cross-sectional estimates may underestimate the effect of bail on posting. Judges also frequently consider defendants' income levels and set especially low bail amounts for poor defendants. Hence, wealthier defendants (who are particularly capable of posting) may receive high bail amounts. These wealthier defendants may also face higher costs of committing crimes or fleeing, due to employment or ties to the community. Due to these wealth effects, cross-sectional estimates may overestimate the effect of bail on posting. For the same reasons, cross-sectional estimates may underestimate bail's effects on flight and new crimes. These two factors (risk and wealth) bias the cross-sectional probit coefficients in opposite directions. Hence, the direction of the overall bias is unclear.

We use data from the Philadelphia Bail Experiment to avoid these omitted variables problems. Through the bail guidelines, judges in the treatment group were given recommended bail amounts that were lower than they would otherwise assign. Let $Treatment_i$ be a dummy variable for whether defendant i is in the treatment group. We can now write $\ln(bail_i)$ as a linear combination of $Treatment_i$, $\mathbf{X}_i$, and normally distributed random error u_i :

$$(16) \quad \ln(bail_i) = \gamma * Treatment_i + \boldsymbol{\delta}' \mathbf{X}_i + u_i$$

Our identifying assumption is that, due to the randomization, $Treatment_i$ is uncorrelated with ε_i and u_i . Substituting Equation (16) into Equations (12) and (15), we obtain the following reduced-form probit equations for release, failure-to-appear, and additional crime:

$$(17) \quad P_i^j = \Phi \left(\boldsymbol{\Pi}^j, \begin{bmatrix} 1 \\ Treatment_i \\ \mathbf{X}_i \end{bmatrix} \right), j \in \{Post, Flight, Crime\}$$

where $\boldsymbol{\Pi}^j = \frac{1}{\sigma_{j,u}} \begin{bmatrix} \alpha_0^j \\ \alpha_1^j \gamma \\ \boldsymbol{\beta}^j + \alpha_1^j \boldsymbol{\delta} \end{bmatrix}$, $\sigma_{j,u}^2 = Var(\varepsilon_i^j + \alpha_1^j u_i)$, $\alpha_0^{Post} = -\ln(0.037)$, and $\alpha_1^{Post} = -1$. We

then estimate the parameters of Equation (15) using instrumental variables probit specifications.

C. Computation of the Socially Optimal Bail Amount

We estimate the optimal bail amount by finding the value of $bail_i$ that minimizes the social cost as in Equation (7)

$$(7) \quad \min_{bail_i} E[C_i^{Total} | bail_i, \mathbf{X}_i] = \min_{bail_i} E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i] + \\ (1 - P^{Post}(bail_i, \mathbf{X}_i)) * C^{Jail} + P^{Flight}(bail_i, \mathbf{X}_i) * C^{Flight} + P^{Crime}(bail_i, \mathbf{X}_i) * C^{Crime}$$

Empirically, we construct this cost by combining separate estimates of $P^{Post}(bail_i, \mathbf{X}_i)$, $P^{Flight}(bail_i, \mathbf{X}_i)$, $P^{Crime}(bail_i, \mathbf{X}_i)$, C^{Jail} , C^{Flight} , C^{Crime} , and $E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i]$. Our approach for estimating $P^{Post}(bail_i, \mathbf{X}_i)$, $P^{Flight}(bail_i, \mathbf{X}_i)$, and $P^{Crime}(bail_i, \mathbf{X}_i)$ is described above. We obtain estimates of C^{Jail} , C^{Flight} , and C^{Crime} from other studies and from discussions with industry experts, as we discuss in Section VI.

Given our 7 parameters of interest, we construct an empirical counterpart to the social cost function in Equation (7). Next, we plug different values of $bail_i$ into our estimated probability functions $P^{Post}(bail_i, \mathbf{X}_i)$, $P^{Flight}(bail_i, \mathbf{X}_i)$, and $P^{Crime}(bail_i, \mathbf{X}_i)$, and $E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i]$. We then compute estimates of the social cost function for \$100 increments of $bail_i$ from \$1, \$100, \$200, \$300, . . . , up to \$100,000.¹² Our estimate of the socially optimal bail amount is the value of $bail_i$ that produces the lowest estimated social cost.

VI. Results

In this section, we present the results of our analysis. First, we estimate the cross-sectional relationships between bail and posting, rearrest, and failure to appear in court. Second, we present instrumental variables probit estimates of the causal relationships between bail and these pre-trial outcomes. Third, we split the sample into defendants for whom the guidelines recommended low, medium, and high bail levels. As Table 2 showed, the guidelines from the experiment were constructed as increasing functions of charge severity and the defendant's probability of violating the terms of bail. Consequently, these recommended values can serve as a composite measure of the dangerousness of different defendants. We then estimate the subjective value of freedom and the socially optimal bail levels for the average defendant in each of these 3 categories of this proxy for dangerousness.

¹² At very high levels of bail, the probabilities of posting, flight, and rearrest are extremely close to zero. Hence, the social cost at \$100,000 bail is a reasonable approximation to the social cost of infinite bail (*i.e.*, certain detention).

A. Cross-Sectional Probit Estimates

Table 4 presents cross-sectional probit regressions of bail on the pre-trial outcomes of interest. To examine the relationship between bail and these outcomes in a typical cross-section, we include only the 242 observations from the control group. Columns (1) and (2) show the effects of $\ln(bail_i)$ on bail posting. Columns (3) and (4) show the effects of $\ln(bail_i)$ on failure-to-appear. Columns (5) and (6) show the effects on rearrest. The probit specifications in Columns (1), (3), and (5) only include $\ln(bail_i)$ as a regressor. Columns (2), (4), and (6) include additional controls for defendant characteristics. The controls include prior convictions, prior arrests, age, weekly earnings, dummy variables for married, employed, has a fixed address, and owns a car, and the 17 fixed effects for the cells in the guidelines matrix. All 6 columns show the elasticity with respect to bail (*i.e.*, the marginal effect of $\ln(bail_i)$) for the mean observation.

The estimates from Table 4 show negative and significant relationships between bail and both release and failure-to-appear in the cross-section. For bail posting, we estimate an elasticity of -0.17 to -0.18. We observe smaller elasticities for failure-to-appear and rearrest. For failure-to-appear, we estimate an elasticity of -0.05 to -0.06 with respect to bail. For rearrest, we estimate an insignificant elasticity of 0.00 to +0.02. Adding controls does not affect our estimated effects in any of the regressions. As discussed earlier, however, there are many reasons why these cross-sectional comparisons may produce biased estimates. Next, we exploit the structure of the experiment to estimate the causal relationships between bail and release, failure-to-appear in court, and rearrest.

B. Instrumental Variables Estimates

Table 5 reports first-stage and reduced-form probit estimates using our instrumental variables strategy. As in Table 3, the sample used here is the “regression sample” of the 17 cells in which the bail levels recommended by the experiment are significantly different from those observed in the control group. Columns (1) and (2) show the first-stage effects of treatment on $\ln(bail_i)$. Columns (3) and (4) show the effect of treatment on the fraction released. Columns (5) and (6) show the effects on the fraction rearrested. Columns (7) and (8) show the effects of treatment on the fraction failing to appear. Columns (1), (3), (5), and (7) include only treatment as a regressor. Columns (2), (4), (6), and (8) add defendant controls.

The first two rows of Table 5 show that, on average, treatment reduced bail amounts by more than 50%. This result is consistent with the large difference in bail amounts between the treatment and control groups in Table 3. The magnitudes of the reduced-form effects are the same as in Table 2, and adding controls does not affect the estimates.

Table 6 presents the main results from the instrumental variables probit specification for the effects of bail on release, rearrest, and failure-to-appear. The specifications are the same as in Table 4; however, $\ln(bail_i)$ is instrumented with a dummy for treatment status. The estimated effect of $\ln(bail_i)$ on release is significant in both regressions. The elasticity estimates, -0.27 and -0.32, are roughly twice as large as those obtained from the cross-sectional comparisons. Hence, it appears that there are many important omitted variables that do not appear in our dataset. Our estimated effects of bail on failure-to-appear are insignificant but are roughly the same magnitude as in the cross-section. Using the experiment, we also obtain larger effects of $\ln(bail_i)$ on rearrest than we find in the cross-section. By using the Philadelphia experiment, we are able to obtain unbiased estimates that do not suffer from these confounding factors.¹³

C. Estimates of the Value of Freedom

Next, we estimate defendants' subjective values of freedom using the revealed preference approach described in Section V. First, we graphically illustrate our estimation strategy by plotting empirical demand curves. Then, we use our instrumental variables probit estimates to calculate the willingness to pay for release for 3 different categories of defendants.

Figure 4 shows empirical demand curves for freedom for the 17 different categories of defendants in the regression sample. These curves are the empirical analogue to the theoretical demand curve in Figure 3. The gray boxes show means for the treatment group, and the black boxes show means for the control group. Within each line segment, the charge severity and probability of failure indices are the same for the black box and the gray box. For the gray boxes, however, the judge was randomly assigned to use the bail guidelines and consequently assigned lower bail amounts. Hence, each line segment estimates a separate causal relationship between

¹³ One alternative explanation for the differences between the OLS and instrumental variables estimates is heterogeneity in both the first-stage and second-stage coefficients (*c.f.*, Heckman, Urzua, and Vytlačil, 2006; Lochner and Moretti, 2004). We discuss this possibility in detail in the web appendix.

bail and release. The quantity of freedom consumed – the fraction released – is plotted along the horizontal axis. The price – the bail amount – is plotted along the vertical axis.

These estimated demand curves confirm the general results from Tables 3 and 6 and Panel A of Figure 2. Reductions in bail generally increase posting rates, and we observe negative slopes in 14 and zero slope in 1 of the 17 line segments in Figure 4. The two cases with upward slopes involve small changes in bail and are probably attributable to sampling error.

We can use the demand curves in Figure 4 to construct a preliminary back-of-the-envelope estimate of $E[V_i | \mathbf{X}_i]$. Along the dotted vertical line in Figure 4, 50% of the defendants choose release. When the fraction posting is 50%, the median defendant is indifferent between detention and release. One informal way to estimate the value of freedom is to look at the bail level at which the fraction posting equals 50% for specific groups of defendants. This graphical analysis is one transparent way to estimate the value of freedom that does not require strong functional form assumptions. Two of the demand curves in Figure 4 intersect or come close to the dotted vertical line. The bail levels at which they intersect range from roughly \$21,000 to \$24,000. Our estimates of the discounted present values of these bail amounts are $0.037 * \$21,000 = \780 and $0.037 * \$24,000 = \890 . Hence, the rough estimates for these two cells suggest values of freedom between \$780 and \$890.

Next, we apply the formula $E[V_i | \mathbf{X}_i] = e^{\hat{\beta}^{\text{Post}} \cdot \mathbf{X}_i + \frac{\hat{\sigma}_{\text{Post}}^2}{2}}$ to formally estimate defendants' subjective values of freedom. The first row of Table 7 shows estimates of $E[V_i | \mathbf{X}_i]$ for the average defendant and for defendants at the three different levels of dangerousness, as proxied by the bail amounts recommended by the guidelines of the experiment. These estimates are computed as $e^{\hat{\beta}^{\text{Post}} \cdot \mathbf{X}_i + \frac{\hat{\sigma}_{\text{Post}}^2}{2}}$ for the mean defendant in each category. The terms $\hat{\beta}^{\text{Post}}$ and $\hat{\sigma}_{\text{Post}}^2$ are estimated separately for each of these three groups using the instrumental variables probit specifications with no controls, as in Column (1) of Table 6. The standard errors are calculated using the delta method. Column (1) of Table 7 shows estimates for the average defendant in the regression sample. Columns (2), (3), and (4) show estimates for defendants at low, medium, and high levels of dangerousness, respectively. Our proxy for dangerousness is the midpoint of the recommended bail range in the defendant's cell. For the average defendant in the regression sample, we estimate a subjective value of freedom of \$1,050, which is marginally different from zero (at the 10% level). For the least dangerous defendants, we find that the subjective value of

freedom is \$6,770; however, this estimate is very imprecise. For defendants at medium and high levels of dangerousness, we estimate subjective values of freedom of \$800 and \$971, respectively. Both of these values are significantly different from zero.

D. Calculation of the Socially Optimal Bail Amount

Next, we put our probit estimates together with estimates for C^{Jail} , C^{Flight} , and C^{Crime} to calculate the total cost of different bail policies to defendants, the justice system, and potential victims of new crimes. We then determine the bail amounts that minimize this cost function for our 3 categories of defendant dangerousness.

Levitt (1996) reviews a handful of studies of the judicial system's cost of jailing a defendant, with estimates ranging from \$84 to \$126 per day. We calculate C^{Jail} as the midpoint of these figures (\$105) times 90 days, or approximately \$9,500.

The cost of failure-to-appear (C^{Flight}) consists of the cost of re-capturing a fugitive defendant along with administrative court costs. We assume that the administrative costs are second order. Hence our estimate of C^{Flight} is simply the cost of recapturing a defendant. There are no well-known studies that include estimates of these costs, so we turned to industry experts for these values. In private conversations, two bail bond experts provided us with rough estimates of this cost: one saying \$500 and another saying 5% of the bail amount. For the average control group defendant who failed to appear, 5% of the bail amount is \$395. It is this latter estimate of the cost of flight that we use in the main specifications. Previous research has shown that bail bondsmen are more effective than the public sector at catching fugitive defendants (Helland and Tabarrok, 2004). Hence, for court systems such as Philadelphia (in which bondsmen are illegal), this estimate may understate the cost of flight.¹⁴ Due to the small number of failures to appear in the control group, we cannot obtain precise estimates of 5% of the bail level for each of the 3 categories of defendant dangerousness. Consequently, we use the \$395 estimate for each of these 3 groups.

Our estimates of the social cost of crime, C^{Crime} , include both costs to victims as well as detention and rearrest costs. Our estimated costs to victims are taken from Miller, Cohen, and Wiersema (1996). This study measures a variety of different costs including medical expenses,

¹⁴ The effects of this bias should be fairly small in our context, because the costs of failure-to-appear are small relative to the costs of additional crimes.

loss and damage of property, and pain and suffering (estimated from data on jury awards). These cost estimates, together with FBI arrest data (Carlson, 1998) imply that crimes resulting in arrest account for roughly 13% of the total cost of criminal victimizations. We combine the Miller, Cohen, and Wiersema estimates on charge-specific costs of crime with charge-specific counts of rearrests among defendants in our regression sample. We estimate that the average rearrest in our sample was for a crime with a social cost of \$5,790. After taking into account the cost of crimes that do not result in rearrest, we estimate that for each observed rearrest, society has incurred roughly \$44,700 worth of damage. It is this figure that we use for C^{Crime} . Sources and calculations described in greater detail in the web appendix. As with C^{Flight} , our estimate is the same for all three defendant types due to small sample sizes.¹⁵

Next, we combine these costs with our estimated effects of bail on pre-trial outcomes. Figure 5 shows the functional relationships between bail and release, rearrest, and failure-to-appear. Each curve is an estimated causal relationship between bail and a pre-trial outcome for the average defendant in the regression sample. The solid black curve shows the predicted fraction posting bail. The dashed curve shows the predicted fraction failing to appear, and the solid gray shows the predicted fraction rearrested. These curves correspond to the instrumental variables probit regressions in Columns (1), (3) and (5) of Table 6.

All the components for the total social cost function have now been estimated empirically or obtained from other sources. Hence, we can now construct an estimate of this cost function. Figure 6 plots the estimated total social cost as a function of bail for the average defendant. This curve is the empirical analogue to the social cost in Equation (7). We compute this cost as a weighted average of the curves shown in Figure 5 and the value of lost freedom.

The sharp decline at low levels of bail is primarily attributable to the effect of bail on rearrest. As this effect flattens out, our estimated social cost of bail reaches a minimum and begins a slight rise. For the specification shown here, the cost-minimizing bail amount is \$17,700. One notable feature Figure 6 is the asymmetry in the estimated social cost on either side

¹⁵ This cost per arrest figure is higher than the estimates used in other studies (e.g., Lochner and Moretti, 2004), in part because the cost estimates from Miller, Cohen, and Wiersema (1996) include long-term medical costs, lost hours of work and leisure, and more precise estimates of pain and suffering costs than had been used previously. The study also includes the costs of many criminal victimizations (particularly domestic abuse and rape) that are typically not reported. The Miller, Cohen, and Wiersema study appears to be one of the most comprehensive studies available on the economic costs of crimes, which is why we use their estimates for our benchmark specification. Nevertheless, it is worth noting that our estimated optimal bail levels vary considerably depending on the assumed cost per rearrest, as we show in the web appendix.

of the estimated optimal bail level. We find that the social cost of inefficiently low levels of bail is considerably higher than the social cost of inefficiently high levels. Hence, after taking into account the imprecision in our estimates of the socially optimal bail, the bail level that minimizes expected social cost may be higher than the cost-minimizing level shown here.

Table 7 shows our estimates of the socially optimal bail for this average defendant and also separately for our 3 categories (low, medium, and high) of defendant dangerousness. Column (1) shows estimates for the average defendant in our 487-observation regression sample. Columns (2), (3), and (4) show estimates broken down into low, middle, and high categories of bail recommended by the guidelines of the experiment. Row 1 reports our estimates of the subjective value of freedom, as discussed in part C. Row 2 shows the bail levels that minimize the estimated cost to society for each category of defendant, along with bootstrapped 95% confidence intervals. For these same defendant types, row 3 shows the average bail level for defendants in the control group, and row 4 shows the average bail level recommended by the guidelines of the experiment. Rows 5, 6, and 7 show our estimates of the total cost per defendant incurred by society at each of these three bail levels.

Our estimates of the socially optimal bail are imprecise, and in three of the four cases, either our estimate is infinity or the 95% confidence interval includes infinity (*i.e.*, certain detention). Given the considerable imprecision in our estimates, it is difficult to make policy recommendations in a conclusive way. Nevertheless, our estimates provide suggestive evidence that, in the absence of bail guidelines, judges behaved in a roughly efficient way.

For the average defendant, our estimate of the socially optimal bail is \$17,700, as in Figure 3. For defendants at low, middle, and high levels of dangerousness, our estimated socially optimal bail levels are \$12,400, \$15,600, and infinity, respectively. With the exception of this infinite value, our optimal bail estimates are fairly close to the average bail levels observed for defendants in the control group. Hence, our socially optimal policy is reasonably close to common practice among judges prior to the implementation of the bail guidelines. In all four cases, the bail levels recommended by the guidelines of the experiment are considerably lower than our estimated social optima.

Across the four columns of Table 7, our estimates of the total cost to society at the optimal bail levels range from \$6,060 to \$10,700 and are increasing with the dangerousness of the defendant. For defendants at high levels of dangerousness, our estimate of the cost to society

is 20% higher for bail levels assigned by control group judges than for the social optimum. For the average defendant and for the low and medium levels of dangerousness, our estimated cost for the control group is within 5% of our estimate of the cost at the social optimum. Hence, we find fairly small social benefits associated with adjusting bail from commonly assigned levels to our estimated social optima. By contrast, our estimate of the social cost associated with the bail levels recommended by the guidelines ranges from 17% to 116% more than our estimate of the cost at the optimal bail level. Hence, the social cost associated with adopting bail guidelines could be large – particularly for the most dangerous defendants.

VII. Conclusion

This paper uses experimental data and cost-benefit analysis to estimate the socially optimal bail amount for the average defendant. We estimate the effect of bail on bail posting, flight, and rearrest. We combine these estimates with data from a variety of sources to calculate the net social costs of these pre-trial outcomes. We calculate the cost of detention to defendants using a revealed preference approach from data on bail posting decisions. In 2003 dollars, we estimate a subjective value for 90 days of freedom of roughly \$1,000 for the average defendant in our sample.

Putting these estimates together, we arrive at a total social cost of pre-trial release and detention. We estimate the bail amount that minimizes this social cost for the average felony case. While imprecise, our estimates suggest that the socially optimal bail amount for the average defendant in our sample is roughly \$17,700. Our estimate of the socially optimal bail level is close to the levels that judges assigned in the absence of guidelines and is considerably higher than the levels recommended by bail reform policies.

By applying cost-benefit analysis and willingness-to-pay estimation, we show that data from the Philadelphia Bail Experiment can produce a more comprehensive set of policy implications than previously believed. This paper provides one specific example of the benefits of empirical economic analysis in the judicial context.

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Figure 1: Observed and Recommended Bail Amounts for Control and Treatment Groups

Notes: See notes to Tables 2 and 3. Each point in the graph corresponds to one of the cells in Table 2. The value on the vertical axis shows the observed bail amounts assigned at each recommended bail level. Treatment judges were assigned to use the recommended ranges when assigning bail amounts. Control judges were not. Unlike in the tables and in Figures 3 to 5, observations with zero bail amounts are included in Figures 1 and 2.

Figure 2: Differences in Bail, Posting, Rearrest, and Flight between Treatment and Control

Notes: See notes to Figure 1 and Tables 2 and 3. These graphs show differences between the treatment and control group for each of four pre-trial variables: bail, posting, rearrest, and failure-to-appear. Bail Posting is coded as one if the defendant posted bail within 7 days of arrest and zero otherwise.

Figure 3: Illustration of Demand Curve for Freedom

Figure 4: Estimated Demand for Freedom for Seventeen Charge Severity X Risk Combinations

Notes to Figure 4: See notes to Figures 1 and 2 and Tables 2 to 4. Each line segment plots a demand curve for a different Midpoint of Bail Range Recommended Under Experiment. The gray squares show means for the treatment group, and the black squares show means for the control group. The line segments connect observations with the same levels of Midpoint of Bail Range. The sample is the same as in Tables 2 to 7

Figure 5: Estimated Probabilities of Release, Rearrest, and Failure-to-Appear for the Average Defendant

Notes: See notes to Figures 1 to 4 and Tables 2 to 6. Predicted probabilities from instrumental variables probit regressions are shown. Specifications without controls are shown, is in Columns (1), (3), and (5) of Table 6.

Figure 6: Estimated Total Social Cost for Average Bail Case

Notes: See notes to Figures 1 to 5 and Tables 2 to 7. This estimated social cost is constructed using the probabilities shown in Figure 5 and the social cost expression in Equation (7). Each probability in Figure 5 is weighted by the social cost of that outcome. The social cost of detention is estimated as the sum of the cost to the state plus the average defendant's value of freedom. The cost to the state is estimated to be \$9,450. The average value of freedom is estimated as the area under the demand curve for defendants who do not post, as illustrated in Figure 3 and in Equation (14). The cost per rearrest is estimated to be \$44,700, and the cost per failure-to-appear is estimated to be \$395. Additional details in the text.

Table 1: Summary Statistics on Bail in the United States, May 2000

At Midyear 2000. . .	
Total Persons Incarcerated	2.1 million
Total Untried Defendants Incarcerated (Est.)	0.3 million
Total Untried Defendants Not Incarcerated (Est.)	0.7 million
Among Those Detained. . .	
Average Bail Amount	\$59,700
Fraction Eventually Sentenced	0.64
Among Those Posting . . .	
Average Bail Amount	\$6,200
Fraction Eventually Sentenced	0.25
Fraction Rearrested for Other Crimes	0.16
Fraction Missing at Least One Court Appearance	0.22

Notes: Data sources: Beck and Karberg (2001) for total persons incarcerated, State Court Processing Statistics (U.S. Bureau of Justice Statistics, 2006) for additional characteristics about trials. Calculations described in the web appendix.

Table 2: Guidelines from the Philadelphia Bail Experiment (1981 dollars)

BAIL GUIDELINES: JUDICIAL DECISION

Date _____ Log # _____ Name of defendant _____ Police photo # _____ Calculated by _____

Guidelines Matrix

		Probability of failure				
		LOW ← → HIGH				
		Group I	Group II	Group III	Group IV	Group V
Charge severity	1	ROR	ROR	ROR	ROR	ROR
	2	ROR	ROR	ROR	ROR	ROR
	3	ROR	ROR	ROR	ROR	ROR-\$500
	4	ROR	ROR	ROR	ROR	ROR-\$500
	5	ROR	ROR	ROR	ROR	ROR-\$1,000
	6	ROR	ROR	ROR	ROR-\$1,000	\$300-\$1,000
	7	ROR	ROR	ROR	ROR-\$1,000	\$300-\$1,000
	8	ROR	ROR	ROR	ROR-\$1,000	\$500-\$1,000
	9	ROR	ROR	ROR	\$500-\$1,500	\$500-\$1,500
	10	ROR	ROR	ROR-\$1,500	\$500-\$1,500	\$500-\$2,000
	11	ROR-\$1,500	ROR-\$1,500	ROR-\$1,500	\$500-\$2,000	\$500-\$2,000
	12	ROR-\$1,500	ROR-\$1,500	\$500-\$1,500	\$800-\$2,500	\$800-\$3,000
	13	\$800-\$3,000	\$800-\$3,000	\$1,000-\$3,000	\$1,000-\$5,000	\$1,500-\$5,000
	14	\$1,000-\$3,000	\$1,000-\$3,000	\$1,000-\$3,000	\$1,000-\$5,000	\$1,500-\$5,000
	15	\$2,000-\$7,500	\$2,000-\$7,500	\$2,000-\$7,500	\$2,500-\$7,500	\$3,000-\$10,000

Guidelines Decision _____

Judicial Decision: ☐ ROR

☐ Financial (amount) \$ _____

☐ IF DECISION DEPARTS FROM GUIDELINES, REASON(S):

- ☐ High probability that prosecution will be withdrawn
- ☐ High probability of conviction
- ☐ Defendant's demeanor in court room
- ☐ Defendant's physical or mental health
- ☐ Defendant's relationship to complaining witness
- ☐ To cause guardian to be informed of defendant's arrest
- ☐ Defendant poses specific threat to witness or victim
- ☐ Presence of warrants, detainers, or wanted cards
- ☐ Other (explain): _____
- ☐ Low probability of conviction
- ☐ Sponsor present at hearing
- ☐ Defendant's history of court appearance

Decision by _____

Table 3: Sample Means by Treatment/Control Status for Regression Sample

Variable	(1) Treatment	(2) Control	(3) Difference	(4) SE For Difference (Robust)	(5) SE For Difference (Clustered)
Released	0.68	0.54	0.14	(0.04)**	(0.05)*
Rearrested	0.11	0.05	0.06	(0.02)**	(0.02)**
Failed to Appear	0.13	0.10	0.03	(0.03)	(0.02)
Bail Amount (in 2003 Dollars)	\$13,492	\$19,006	-\$5,514	(3,075)	(4,596)
Midpoint of Recommended Bail	\$6,348	\$6,419	-\$71	(334)	(316)
Charge Severity	13.0	13.0	-0.03	(0.18)	(0.18)
Index of Flight/Rearrest Risk (1 to 5)	3.94	3.86	0.08	(0.12)	(0.12)
Time Until Trial	85.93	86.77	-0.8	(3.6)	(2.9)
Prior Convictions	1.69	1.47	0.22	(0.24)	(0.14)
Prior Arrests	5.39	5.08	0.31	(0.58)	(0.40)
White	0.16	0.17	-0.02	(0.03)	(0.03)
Male	0.92	0.94	-0.02	(0.02)	(0.02)
Age	27.0	27.9	-0.93	(0.85)	(0.86)
Married	0.21	0.27	-0.06	(0.04)	(0.04)
Employed	0.26	0.26	0.01	(0.04)	(0.04)
Weekly Income	\$75.3	\$68.5	\$6.8	(14.0)	(12.4)
Fixed Address	0.76	0.73	0.03	(0.04)	(0.04)
Owns a Car	0.08	0.10	-0.01	(0.03)	(0.03)
Observations	245	242	487	487	487

Notes: See notes to Figures 1 and 2 and Table 2. Regression sample includes all cells (from Table 2) in which observed bail levels for the control group were significantly different from recommended levels. Excludes 81 observations for which bail was zero. Fixed address is coded as one if the defendant has lived at his/her present address for at least one year. For clustered standard errors, observations are grouped by judge. Additional details in the text.

Table 4: Cross-Sectional Probit Estimates for Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
Elasticity with Respect to Bail Shown for the Mean Observation						
	Pr(Released)		Pr(Failed-to-Appear)		Pr(Rearrested)	
Ln(Bail Amount)	-0.17	-0.18	-0.05	-0.06	0.00	0.02
Robust SE	(0.03)**	(0.03)**	(0.01)**	(0.02)**	(0.01)	(0.04)
SE Clustered by Judge	(0.04)**	(0.04)**	(0.01)**	(0.01)**	(0.01)	(0.04)
Controls?		Yes		Yes		Yes
Pseudo-R ²	0.15	0.29	0.04	0.18	0.00	0.39
Observations	242	242	242	242	242	242

Notes: See notes to Tables 2 and 3 and Figures 1 and 2. Sample includes observations from Control group with Midpoint of Recommended Bail greater than \$6,000. Each estimated effect comes from a different probit regression. The regressor of interest is Ln(Bail Amount). The elasticity estimated here is the marginal effect of log bail on the probability of interest. Controls include prior convictions, prior arrests, age, weekly earnings, and dummies for charge severity X flight/rearrest risk interactions, married, employed, has a fixed address, and owns a car. Efron's Pseudo-R² measure is used. Standard errors for marginal effects calculated using the delta method.

** Denotes 5% significance.

* Denotes 10% significance.

Table 5: First-Stage Linear Regressions and Reduced Form Probit Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Effect of Unit Change in Treatment for the Mean Observation Shown for Probit Regressions							
	Ln(Bail Amount)		Pr(Released)		Pr(Failed-to-Appear)		Pr(Rearrested)	
Treatment	-0.52	-0.53	0.14	0.16	0.03	0.03	0.06	0.08
Robust SE	(0.11)**	(0.10)**	(0.04)**	(0.05)**	(0.03)	(0.03)	(0.02)**	(0.03)**
Clustered SE	(0.21)**	(0.22)**	(0.05)**	(0.05)**	(0.02)	(0.02)*	(0.02)**	(0.02)**
Controls?		Yes		Yes		Yes		Yes
R ²	0.04	0.36						
Pseudo-R ²			0.02	0.16	0.00	0.04	0.01	0.06
Observations	487	487	487	487	487	487	487	487

Notes: See notes to Tables 2 to 4 and Figures 1 and 2.

Table 6: Instrumental Variables Probit Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Elasticity with Respect to Bail Shown for the Mean Observation					
	Pr(Released)		Pr(Failed-to-Appear)		Pr(Rearrested)	
Ln(Bail Amount)	-0.27	-0.32	-0.05	-0.06	-0.14	-0.16
Robust SE	(0.06)**	(0.07)**	(0.05)	(0.05)	(0.07)*	(0.05)**
Clustered SE	(0.06)**	(0.09)**	(0.03)*	(0.02)**	(0.07)**	(0.07)**
Controls?		Yes		Yes		Yes
Pseudo-R ²	0.13	0.21	0.05	0.07	-0.49	-0.47
Observations	487	487	487	487	487	487

Notes: See notes to Tables 2 to 5 and Figures 1 and 4. Each column shows results from a different instrumental variables probit regression. Bail is instrumented using treatment status. The corresponding first-stage regressions appear in Columns (1) and (2) of Table 4.

Table 7: Estimated Value of Freedom, Optimal Bail, and Social Cost at Different Bail Levels

	(1)	(2)	(3)	(4)
	Entire Regression Sample	Separately by Recommended Bail Levels		
		Low	Medium	High
1. Estimated Average Value of Freedom	\$1,050 (631)	\$6,770 (28,700)	\$800 (344)	\$971 (214)
2. Estimated Optimal Bail [Bootstrapped 95% CI]	\$17,700 [1, 30,100]	\$12,400 [1, ∞]	\$15,600 [1, ∞]	∞ [1, 67,800]
3. Average Bail for Control Group	\$19,000 (2,560)	\$8,430 (1,530)	\$21,600 (2,870)	\$36,400 (7,090)
4. Average Bail Recommended by Guidelines	\$6,380 (157)	\$2,850 (75)	\$7,280 (125)	\$11,700 (150)
5. Estimated Social Cost at Optimal Bail Levels	\$8,140 [2,950, 10 mil]	\$6,060 [160, 10 mil]	\$8,770 [30, 10 mil]	\$10,700 [350, 10 mil]
6. Estimated Social Cost at Control Bail Levels	\$8,150 [4,070, 11,200]	\$6,280 [3,380, 7.6x10 ¹⁸]	\$8,930 [4,710, 2.9x10 ⁹]	\$13,300 [3,620, 39,900]
7. Estimated Social Cost at Recommended Bail Levels	\$10,100 [3,310, 27,500]	\$10,600 [4,090, 7.6x10 ¹⁸]	\$10,300 [4,850, 2.9x10 ⁹]	\$23,100 [2,550, 30,500]
Observations	487	197	192	98

Notes: See notes to Figures 1 to 6 and Tables 2 to 6. Average defendant's value of freedom is calculated as

$E[V_i | X_i] = e^{\hat{\beta}^{Post} + X_i + \frac{\hat{\sigma}_{Post}^2}{2}}$, as in Equation (13). Standard errors for value of freedom calculated using the delta method. Optimal bail is calculated as the bail that minimizes the estimated social cost, as shown in Equation (7). For bail amounts other than the optimum and for value of freedom, standard errors, clustered by judge, are shown in parentheses. For optimal bail amount and social costs, bootstrapped 95% confidence intervals are calculated using a bootstrap with 1,000 repetitions of sample size N . Bootstrapping is clustered by judge, and C^{Crime} , C^{Flight} , and cutoffs for the dangerousness tertiles are recalculated in repetitions. However, the construction of the "regression sample" is taken as given. Infinite values for optimal bail indicate that no finite bail amount produced lower estimated social cost than did certain detention. All causal relationships are estimated using the instrumental variables probit regressions with no controls, as in Column (1) of Table 6. Additional details in the text.

Web Appendix for “Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment.” Updated April 2007

This appendix describes additional features of the data, calculations, sensitivity checks, derivations, and supplementary results for “Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment.” This appendix is included for the benefit of referees and is not intended for publication. All the data used in this study are public. Datasets and programs are available from the authors on request.

Credit Constraints

One major limitation of the willingness to pay measure used in this study is that of credit constraints. Criminal defendants and their families are generally poor and do not have easy access to credit. To understand defendants’ posting behaviors under the current regime, the constrained willingness to pay for freedom estimated in this study is the relevant measure. However, if long-term payment plans were available for bail, then it is likely that more defendants would post. In practice, the introduction of payment plans might not be socially desirable, because they would dilute the incentive effects of bail. However, to determine the long-term social welfare cost of lost freedom, the unconstrained willingness to pay is the relevant measure. Unfortunately, the data used in this study do not allow for identifying the effects of credit constraints. Consequently, our estimates may understate defendants’ true valuations of freedom, and they may overstate the socially optimal bail amounts.

The problem of credit constraints will necessarily affect any estimation strategy that measures value using willingness to pay. This problem is particularly pronounced, however, for populations with low wealth, income, and access to credit, as is generally the case for felony defendants. Credit is often available to defendants from friends or relatives, but in many cases, these available sources of credit will not be sufficient to meet bail requirements.¹⁶

The effect of credit constrained individuals in our sample will bias our estimates of defendant value of freedom downward. If pre-trial detention is more costly than we currently estimate, then the true optimal bail level is lower than the amount that we estimate. One type of indirect evidence of the importance of credit constraints may be obtained from states where bail bondsmen are legal. In these states, defendants generally are required to pay the full amount of bail or pay (and forfeit) a fraction of the face value (usually under 10%) to a bail bondsmen. One measure of the importance of credit constraints to the population will be the fraction of defendants who make use of the services of bail bondsmen. Unless a defendant anticipates an extremely long trial, the cost of using a bail bondsman is substantially higher than market rates. Thus, we may take the use of a bail bondsman as an indication of credit constraints.

Across the United States, the use of bail bondsmen is very common. In the 1990 to 2000 State Court Processing Statistics data, 66% of pre-trial releases consisted of surety bonds. This high ratio of bondsmen use to personal funds use suggests that credit constraints play a major part in defendants’ decisions.

¹⁶ While bail bondsmen provide a source of credit for such individuals in other markets, they were illegal in Philadelphia. Given the policies regarding bail in Philadelphia, however, it is unlikely that the presence of bail bondsmen would have affected our estimates. Bail bondsmen typically charge an upfront fee of 10% of bail, which is the same amount defendants were required to deposit with the court.

One possible means of correcting for the importance of credit constraints would be to use data on the time until trial. To the extent that defendants value freedom, the relevant tradeoff can be expressed in terms of dollars per day of freedom. However, if defendants are credit constrained, their decisions will be affected by the total bail amount, regardless of the amount of freedom that they can purchase with that amount. Hence, the extent of credit constraints could be measured with the following estimation equation:

$$(A1) \quad P_i^{Post} = \Phi \left(\frac{\beta^{Post} \cdot \mathbf{X}_i - \lambda * \ln(0.037 * bail_i) + \rho * \ln(time_i)}{\sigma_{Post}} \right),$$

where $time_i$ is the time until trial. If all defendants were credit constrained, then we would expect ρ to be zero. Unfortunately, the data on time until trial are not sufficiently precise to test this hypothesis in the Philadelphia data. We are looking into the possibility of estimating this equation in future work using data from Chicago.

Low Estimates for Value of Freedom

There are no previous estimates we are aware of in the academic literature for a price of freedom from incarceration, which makes it difficult to assess precisely how reasonable our results are. Thus, it is instructive to perform some simple back-of-the-envelope calculations for comparison. One potential basis for comparison is foregone wages. By this measure, a defendant with average characteristics would need to earn roughly \$4,000 per year to imply the values we find. For the sample we examine, this low figure is not out of the realm of possibility.

Table 3 from the main text reports weekly income for defendants of approximately \$75. This figure is unconditional on employment status, so the 26% employment rate corresponds to an employed salary of approximately \$300/week or \$7.50/hour. Since the unconditional rate is the appropriate value to use, we convert it into yearly earnings, which are approximately \$3,750; not very different from the calculated value of freedom.

There is good reason to believe that many of the individuals in the data set have substantial unreported earnings, and that the average annual income is actually quite higher. However, in the calculation above, we have ascribed no value to incarceration, when in fact it provides inmates with food, shelter, and medical care, which would presumably reduce net income to the defendant when free. Still, as discussed in detail above, it is likely that credit constraints are causing a downward bias to the value of freedom estimates.

Time Until Trial

One parameter that varies across individuals that we do not measure in a precise way is “time until trial.” To measure this variable, we add the two variables “time at risk for failure” and “detention time,” both measured in days. The “time at risk” variable is topcoded at 90 days. To measure time until trial for the topcoded individuals, if trial date is observed in the data, we take the time between the date of arraignment and the trial date. We then take the mean time between arraignment and trial date for these topcoded individuals and we assign that mean to the topcoded defendants with missing data for trial date. For individuals with zero values for “time until trial,” we impute a value of one day. For the average defendant, we estimate a time until

trial of 86 days. In the main test, we interpret our estimated value of freedom as the value of 90 days of freedom.

Time until trial may be one variable that generates heterogeneity in defendants' posting decisions and estimated values of freedom. Defendants for whom the scheduled trial date is very soon after the arraignment have less of an incentive to post bail. Our estimates of time until trial are uncorrelated with treatment status, indicating that time until trial is not causing bias in our estimates. Nevertheless, understanding the definition of this variable is useful for interpreting the heterogeneity in our estimated values of freedom.

Definition of Rearrest

There are two possible variable definitions that we could use to measure "rearrest" in our bail sample. The first definition is the variable "rearrested within 90 days," which is defined in the original dataset. The second definition is "defendant has non-missing values for most serious rearrest offense." The advantage of the second variable is that a non-missing value for the offense is a strong indication that an arrest actually occurred. Moreover, the true outcome of interest is not "rearrested within 90 days," but "rearrested before the trial," where the time until trial might be longer than 90 days.

For our analysis, we set rearrest equal to one for all individuals who have non-missing values for "most serious rearrest offense." Roughly 25 defendants in the 1,920 observation sample are coded as "rearrested within 90 days," even though there are no codes for the offense that they committed. Similarly, there are 9 defendants who were coded as "not rearrested," yet had non-missing values for "most serious rearrest offense." Hence, for the 25 defendants coded as "rearrested" but with no known offense, we suppose that they were not rearrested. And for the 9 defendants coded as "rearrested within 90 days" but with non-missing codes for rearrest offense, we suppose that they were, in fact, rearrested.

Of the 25 defendants whom we recode as not rearrested, 10 appear in the regression sample. Of the 9 whom we recode as rearrested, 3 appear in the regression sample. Using alternative definitions of rearrest has little effect on the results from this study.

Assumed Discount Rate

In Philadelphia at the time of the experiment, posting bail involved both a permanent expenditure of 3% of the bail amount and a 90-day deposit of 7% of the bail amount. To calculate the present discounted cost of posting bail, we must apply a discount rate to the deposit component. For the main results, we use as a discount rate the typical price of bail bond services in areas (unlike Philadelphia) where bail bondsmen are legal. In cities where bail bondsmen are legal, the fee for posting a refundable deposit of 100% of the bail amount is typically 10% of that amount. We use this 90-day discount rate of 10% in the main specifications of this study.¹⁷

In the first section of Table A1, we examine how the results change when alternative discount rates are used. The alternative 90-day discount rates we consider are 3%, 50%, and

¹⁷ Using data from military pension decisions, Warner and Pleeter (2001) estimate annual discount rates ranging from 30% to 70% for enlisted men (Table 6, pg. 48, estimates are lower for officers). These discount rates would translate into 90-day discount rates of roughly 8% to 19%, which is similar to the figures used in our benchmark estimates. To the extent that criminal defendants have higher discount rates than enlisted men do, the alternative estimates considered in this subsection may be more appropriate.

100%. For small changes in the 90-day discount rate (10% to 3%), we observe very little change in our estimated value of freedom (\$1,050 to \$910). If, however, the 90-day discount rate is very high (50% to 100%), we obtain value of freedom estimates that are nearly 2 to 3 times as large as the benchmark (\$1,840 and \$2,830). Our optimal bail estimates are not very sensitive to assumptions about the discount rate, primarily because the value of lost freedom is small relative to the cost of jail to the justice system. As our assumed 90-day discount rate ranges from 3% to 100%, our optimal bail estimates range from \$17,900 to \$16,000, and our estimates of the social cost at the optimal bail range from \$8,090 to \$8,360.

Assumed Social Cost of Crime

One of the most important costs associated with releasing defendants is the risk that defendants will commit new crimes during the pre-trial period. Our estimates of the social cost of this risk depend crucially on the value we use for C^{Crime} , the social cost of crime. We construct these estimates using data from 3 sources. To measure the types of crimes committed by released defendants, we use data from the subset of the regression sample that was rearrested. For each rearrest, we observe the offense category. We translate that offense category into a dollar cost using estimates of the social cost per crime from Miller, Cohen, and Wiersema (1996).¹⁸ Put together, these figures provide us with estimates of the average social cost per crime resulting in rearrest for defendants in the regression sample. In 2003 dollars, our estimate for this average cost is \$5,785.

In addition to the crimes that are observed as rearrests in the data, a number of criminal acts occur that are not observed. To measure the social cost associated with these acts, we compare estimates of the costs and frequency of victimizations in the U.S. (using data from Miller, Cohen, and Wiersema, 1996, who use data from victimization surveys) with data on arrests in the U.S. by offense type. Our data on arrests are taken from the FBI's Uniform Crime Reports (Carlson, 1998). For each offense type, we calculate the total social cost of crimes resulting in arrest from 1987 to 1990 by multiplying the total arrests (from the Carlson data) by the social cost per arrest (from the Miller, Cohen, and Wiersema estimates, also estimated for 1987 to 1990). We calculate the annual social cost of crimes resulting in arrest in the U.S. as the total of these costs by arrest divided by the number of years. In 1993 dollars, we estimate this average annual cost at \$58.2 billion. Our estimate of the annual cost of victimizations (all crimes, regardless of arrest or reporting) is taken directly from Miller, Cohen, and Wiersema. In 1993 dollars, this figure is \$450 billion, so that crimes resulting in arrest account for $\$58.2/\$450 \approx 12.9\%$ of the social cost of criminal activity in the U.S. We then divide the cost per crime resulting in rearrest (\$5,785) by the fraction of the social cost of victimizations that is accounted for by crimes resulting in rearrest (0.129). Our resulting estimate of the social cost represented by a single rearrest in the bail data comes out to $\$5,785/0.129 \approx \$44,700$. In our optimal bail calculations, we suppose that the social cost of new crimes is proportional to the number of rearrests in the bail data, so that the total social cost associated with new crimes is estimated as \$44,700 times the number of rearrests.

Functional Form for V_i , Posting, Rearrest, and Failure-to-Appear

¹⁸ We treat drug offenses and misdemeanors as zero cost. We treat the "serious personal offense" code as equal in cost to rape.

One important assumption in the estimation of the value of freedom and the bail-release relationship is the functional form for V_i , the value of freedom. In the text, we assume that the natural log of the value of freedom is a linear function of observable characteristics $\mathbf{X}_i$ and normally distributed error. However, as Figure 4 showed, for each defendant type (set of values of $\mathbf{X}_i$), we only observe a treatment group and a control group. Hence, we only have two different values of $bail_i$ with which to construct our estimated causal relationship. For this reason, it is not possible to determine whether the causal relationship between posting and bail is linear, semi-log, quadratic, etc., for any specific group of defendants. Consequently, the shape of the functional relationship for V_i must be assumed in order to estimate $E[V_i | \mathbf{X}_i]$ and the socially optimal bail. Similarly, the shapes for the probabilities of posting, rearrest, and failure-to-appear must be assumed.

In the cross-section, the relationship between posting and $bail_i$ is highly concave and is very closely approximated by a semilog functional form.¹⁹ This cross-sectional relationship does not necessarily have to have the same functional form as the true causal relationship. Nevertheless, the functional form of the cross-sectional relationship provides a useful benchmark. Consequently, for the main specifications in the paper, we assume that the functional form of the true causal relationship is also semilog. However, in the third section of the sensitivity analysis in Table A1, we examine the degree to which the assumed functional form influences our results.

In the row labeled $\text{sqrt}(\cdot)$, the square root of V_i is assumed to be a linear function of observables $\mathbf{X}_i$ and normally distributed error. In the row labeled linear, the level of V_i is assumed to be a linear function of observables $\mathbf{X}_i$ and normally distributed error. In both cases, we assume that the same functional form carries over to the rearrest and failure-to-appear equations. Hence, we suppose that the square root of $bail_i$ enters as a regressor in the square root specifications, and we suppose that the level of $bail_i$ enters as a regressor in the linear specifications.

Changing the functional form assumptions does not have a large effect on our estimated value of freedom. In both the square root and the linear case, the estimated value of freedom is roughly 25% smaller (\$777 and \$757) than the benchmark (semilog) estimate of \$1,050. Hence, the adjustments to the functional form do not appear to have very large effects on our estimates of the subjective value of freedom. For optimal bail, however, the square root and linear functional forms produce much larger estimates (\$27,500 and \$65,000) than does the semilog functional form (\$17,700). Hence, the optimal estimates do appear to be sensitive to the assumption of the semilog functional form, and the estimates using the semilog form appear to be low relative to other commonly used functional forms. Our estimate of the total cost to society at the optimal bail level is slightly higher (\$8,920 and \$9,850) when we use the square root and linear functions than when we use the semilog function (\$8,110).

One problem with the use of the semilog functional form is that 81 defendants with zero bail amounts are dropped from the sample. Dropping these observations with low levels of bail is likely to produce bias in the estimated coefficients. Relatively little research exists examining the treatment of zeros in semi-log relationships. Young and Young (1975) find that the bias caused by dropping the zeros from the sample is less severe than the bias caused by adding some small

¹⁹ In both the linear probability model and the probit, when both $\ln(bail_i)$ and $bail_i$ are included as regressors, the coefficient on $\ln(bail_i)$ is significant and the coefficient on $bail_i$ is not (results not shown). In a Box-Cox test of the transformation of the right-hand variable in a linear probability model, the linear functional form is rejected, and the semilog functional form is not rejected (results not shown).

number (*e.g.*, one) inside the log transformation. While both procedures are imperfect, we use the former approach (dropping the zero observations) in the main specifications in this paper. For the other two functional relationships considered in the sensitivity analysis, however, the values are well-defined at zero bail levels. Hence, for those functional forms (square root and linear), it is possible to examine the effects of adding the zero observations to the data. The next two rows in the sensitivity analysis

In both the square root and the linear case, adding the zero $bail_i$ observations has small negative effects on our estimated value of freedom and our estimate of the social cost at the optimal bail level, and moderately-sized negative effects on our estimate of the socially optimal bail amount. Hence, the bias on the optimal bail estimates associated with dropping zero observations appears to be positive.

Distribution for Binary Choice

The results in the main text all rely upon probit specifications for posting, rearrest, and failure-to-appear. Next, we examine the effects of using a linear probability model (LPM) rather than a probit. For the LPM setting, we calculate the average value of freedom as the estimated value at which 50% of the defendants would post (*i.e.*, the value at which $P_i^{Post} = 0.5$). The probability of posting bail can be expressed as:

$$(A2) \quad P_i^{Post} = \frac{\beta^{Post} \cdot \mathbf{X}_i - \ln(0.037 * bail_i)}{\sigma_{Post}}.$$

And the average value of freedom is calculated as:

$$(A3) \quad E[V_i | \mathbf{X}_i] = e^{\beta^{Post} \cdot \mathbf{X}_i - 0.5\sigma_{Post}}$$

In general, the qualitative results of this study are not changed by switching to the LPM. However, some of the estimates – particularly those of the average value of freedom – change considerably. One primary reason for the change is that the estimates rely on out-of-sample prediction, which is especially sensitive to distributional assumptions.

As the next section of Table A1 shows, the LPM specification produces a substantially lower estimate of the value of freedom (\$388) than the benchmark (\$1,050). The LPM also produces somewhat lower optimal bail estimates (\$13,000 versus \$17,700). However, given the level of imprecision in these estimates, the change is not dramatic. We also find a considerably lower total social cost (\$5,420) than under the benchmark (\$8,110).

Choice of Regression Sample

One important factor that may influence the results from this paper is the way in which the regression sample is chosen. The complete data for the Philadelphia Bail Experiment includes 1,920 cases. Of these cases, 960 are misdemeanor cases and 960 are felony cases. Due to the semilog specification, 230 felony cases and 605 misdemeanor cases would naturally be dropped because bail equals zero for these observations. Hence, use of the semilog specification along would limit the dataset to 1,085 observations. We examine the effects of dropping these

zero bail observations in the “Functional Form” section of this appendix. In addition to this data restriction, however, we further restrict the sample by probability of failure and charge severity. The resulting sample used for the means and regressions in Tables 3 to 7 includes 487 observations.

As discussed earlier in the main text, each of the 75 charge severity by “probability of failure” combinations (the cells in Table 2) corresponds to a different recommended bail range. Within each cell, the assignment of treatment status was random. Hence, each of the 75 cells can be viewed as a separate experiment with a different treatment. For some cells, the bail range recommended by the guidelines was similar to common practice at the time. In these cases, the guidelines asked judges to do what they were already doing, and we would not expect the treatment to have any effects. For these cells, we do not expect the experiment to provide information about the effects of bail, because for these cells, the experiment does not produce exogenous variation in bail levels. In other cases, the bail range recommended by the guidelines was very different from common practice at the time. It is these cells that are useful for estimating the effects of bail, because in these cases, the experiment does produce exogenous variation in bail.

Including all 75 cells in the regressions leads to imprecise estimates, because many of the cells will have a zero first-stage effect. As Heckman, Urzua, and Vytlačil (2006) show, in the case of heterogeneous first- and second-stage effects, instrumental variables approaches identify “treatment on the treated.” The cells for which the first-stage effect is large (*i.e.*, the “treated” cells) will receive greater weight in the estimation than will the cells in which the first-stage is small (*i.e.*, the “untreated” cells). We expect the cells in which the first-stage effect is zero to have zero weight in the second-stage estimates. At the same time, including these cells with zero first-stage effect in the sample decreases the precision of the first- and second-stage estimates. To address this problem of efficiency, we restrict the sample to the cells in which we would expect treatment status to affect bail amounts.

To construct the 487-observation regression sample, we consider only those cells in which the mean bail level for the control group was significantly different from the midpoint of the bail range recommended by the experiment’s guidelines.²⁰ Of the 75 cells, 17 fit this criterion. These 17 cells include 568 observations, of which 81 had zero bail amounts and were dropped.

In addition to increasing the precision of the estimates, the sample restriction described here has the added benefit of limiting the sample to cases with relatively high bail amounts. By limiting the sample in this way, the bias associated with dropping observations with zero bail is lower than it would otherwise be. In the full sample, 43.5% of the 1,920 observations have bail levels of zero and must be dropped. In the regression sample, however, only 14.3% of the 568 observations have bail levels of zero.

One problem associated with the sample restrictions described here is that they may lead to selection bias. To impose the sample restrictions described here, it is necessary to estimate what bail levels constitute “common practice” for each of the 75 cells. Ideally, these “common practice” values would be estimated using data on bail levels in Philadelphia prior to the

²⁰ Implemented as cell-specific t-tests of the difference between the observed bail levels and the midpoint of the recommended values for the control group observations. Sixteen cells dropped due to insufficient (less than 3) observations in the control group.

experiment. Unfortunately, such pre-treatment data are not available.²¹ For this reason, our estimate of “common practice” in Philadelphia is the average bail level among observations in the control group. Restricting the sample in this way may introduce selection bias into the sample. In some cells, due to essentially random reasons, the observations in the control group may have especially large first-stage error terms (*i.e.*, u_i in Equation (16) of the main text). Cells in which the control group’s average first-stage error is abnormally high will have an elevated probability of passing the criterion for inclusion into the regression sample. For this reason, among the cells that end up in the regression sample, treatment status may be negatively correlated with u_i , the first-stage error term, leading to omitted variables bias in the first-stage regression equation.

While the construction of the regression sample introduces bias into our estimates, it does not lead to inconsistency. As the number of observations per cell tends to infinity, the selection bias described above will tend to zero, because for infinitely large cells, the average value of u_i will be identically equal to zero within each cell. The selection bias will be especially severe for cells in which the number of observations in the control group is small. In the 487-observation sample used in this study, the sample size per cell is not extremely small, but it is not extremely large, either. Of the 17 cells included in the regression sample, the average cell has roughly 17 observations in the control group, and 5 of the 17 cells have fewer than 10 observations in the control group.

To measure the importance of the sample restrictions we impose, the next section of Table A1 examines the effects of alternative sets of sample restrictions. The first row shows the benchmark estimates, which are obtained using the 487-observation regression sample. The next row shows estimates using all 1,085 cases in which bail does not equal zero. For the third row, the sample is restricted to the 730 felony cases in which bail does not equal zero.²² The next 3 rows show estimates for this 730-observation felony sample broken down into low, medium, and high levels of defendant “dangerousness.” As in Table 7, we proxy for defendant dangerousness using the midpoint of the bail amount recommended by the guidelines for that defendants’ cell.

As we change the sample, our qualitative results are similar in the sense that we still find interior (*i.e.*, non-infinite) solutions for the value of freedom and the optimal level of bail. The quantitative results do change somewhat; however, given the degree of imprecision in our estimates, these changes are not surprisingly large. When the entire 1,085-observation sample is used, we obtain a very large estimate of the average value of freedom of \$47,200. This estimate is extremely imprecise, and the standard error for this estimated value of freedom (not shown in Table A1) is \$345,000. Hence, when the entire sample is used, the model is considerably less informative than with just the regression sample, because the estimates are not sufficiently precise to make inferences. One reason that the estimates become imprecise is that the sample includes a large number of observations for which the first-stage effect (of treatment on bail amounts) is close to zero. Another reason that the estimates become imprecise is that many of the

²¹ Prior to the Philadelphia experiment, Goldkamp, Gottfredson, and Mitchell-Herzfeld (1981) did measure bail levels in Philadelphia as part of a feasibility study. Unfortunately, however, Goldkamp is unable to locate his copy of the data, the National Institute of Justice, who was the sponsor of the study, has since destroyed its copy of these data.

²² Of the 1,920 observations in the original dataset, 960 are felony cases. Bail is equal to zero in 230 of the 960 observations, leaving 730 for which it is possible to obtain estimates using the semilog functional form. In the 487-observation regression sample, charge severity ranges from 7 to 15, and all 487 observations are felony cases. Hence, all 487 observations from the regression sample are included in this 730-observation subsample.

observations in the full sample are misdemeanor cases. For misdemeanor cases, the fraction of defendants who post bail is much higher than for felony cases (86% versus 68%). Consequently, estimating the average value of freedom (*i.e.*, the bail level at $P_i^{Post} = 0.5$) requires a greater degree of out-of-sample prediction when for the full sample than for the regression sample. For the full sample, we find an optimal bail level that is somewhat lower than the benchmark (\$11,100 versus \$17,700), and we estimate that the social cost at the optimal bail level is considerably lower than with the regression sample (\$4,800 versus \$6,000).

When we restrict the sample to the 730 felony cases, our estimated value of freedom (\$3,600) is closer to the benchmark estimates. Our estimates are also more precise with the felony sample, and the standard error for this estimated value of freedom (not shown in Table A1) is \$8,580. Our estimate of the optimal bail level is \$10,000, which is somewhat lower than the optimal bail estimates for the benchmark sample. Our estimate of the social cost at the optimal bail level is also lower than the estimate for the benchmark sample (\$6,000 versus \$8,110).

Next, we break the felony sample down into low, medium, and high levels of defendant “dangerousness.” The observations with low levels of dangerousness consist primarily of cases that were excluded from the regression sample. That is, in most of these cases, the first-stage effect of treatment on bail levels is weak, and the resulting first- and second-stage estimates are very imprecise. For these low levels, we obtain fairly high estimates of the average value of freedom, and our estimated socially optimal bail is infinite. Hence, surprisingly, these estimates predict that the relatively “safe” defendants should not be let free. One reason for these counterintuitive estimates is that, for this 256-observation subsample, we find large positive effects of $\ln(bail_i)$ on the probabilities of posting and failure-to-appear, primarily due to a high degree of imprecision in the estimates. Hence, for the relatively “safe” defendants, our estimates are not sufficiently precise to make inferences.

For the defendants with medium and high levels of dangerousness, our estimates of the average value of freedom are similar to those obtained from the regression sample. Our optimal bail estimates are, on average, also similar to what we obtain in the benchmark case. However, we find a particularly low value (\$1,500) for defendants with medium levels of dangerousness. One reason for this especially low estimate is a large and imprecisely estimated positive coefficient for $\ln(bail_i)$ on failure-to-appear.

In general, changing the sample restrictions appears to have an effect on the estimates. Many of these (fairly large) changes appear to be attributable to increases in the variability of the estimates, which is driven by the inclusion of cells in which the first-stage coefficient is zero.

Two ways of testing the importance of selection bias in the regression sample appear in the main text of the paper. In Table 3, we test the degree to which the randomization was effective by examining the correlation between treatment status and a handful of possible confounding variables. We find no large or significant correlations between treatment status and our control variables. In Tables 5 and 6, we examine the degree to which our first-stage, reduced-form, and instrumental variables probit estimates are sensitive to the inclusion of these same variables. In each of these cases, we find that adding controls to the regressions has little effect on the estimates. In all of these cases, the test is applied on the regression sample. The results of these tests are consistent with the hypothesis that, even after applying the sample restrictions described in this section, treatment status is uncorrelated with unobserved determinants of bail, posting, rearrest, and failure-to-appear.

Infinite Optimal Bail Values

In our grid search for the optimal bail, we consider possible bail values in \$100 increments from \$1, \$100, \$200, . . . , up to \$100,000. We impose this upper bound of \$100,000 on the grid search to reduce computation time. For the “high” category, however, we obtain optimal bail estimates of \$100,000, which we interpret as ∞ (*i.e.*, certain detention). If the grid search is expanded, we do obtain a finite maximum bail value of \$370,000. However, this bail level is effectively infinite in that the probability of release at \$370,000 bail is only 0.001, and the social cost is \$10,400, which is very close to the \$10,700 cost of certain detention.

Omitted Judge

In the Goldkamp and Gottfredson (1985) analysis, the researchers drop one of the treatment judges from the sample because the judge misunderstood the rules of the experiment and did not set bail according to the guidelines. One potential problem with omitting this judge from the sample is that some omitted characteristic (ability to understand the rules) becomes correlated with the treatment designation, which could lead to omitted variables bias. We include this one judge in the treatment group for our analysis. Consequently, the reduced-form relationships shown in Table 5 of our study show the effects of “intent to treat” on the outcomes of interest.

Sampling Error and Selection Bias in Judges

One concern with the design of the Philadelphia Bail Experiment is that the randomization was applied at the judge level, and the sample includes only 16 judges. For this reason, the estimates from this study are likely to be imprecise, and the standard errors (which are adjusted for arbitrary autocorrelation at the judge level) are likely to be large. For many of the marginal effects shown in Table 6, the estimates are sufficiently precise that we reject the null hypothesis of zero effect. When we estimate the standard errors using a bootstrap (clustered by judge or not), we obtain very similar-sized standard errors as with the robust standard errors shown (clustered or not). Nevertheless, some of the estimates – particularly those of the socially optimal bail levels – are extremely imprecise. For this reason, the final policy implications of this study should be interpreted as suggestive evidence; obtaining more conclusive policy recommendations would require a larger-scale study.

One other problem with the sample of 16 judges is that all 16 agreed to participate in the study. Hence, these 16 judges may be systematically different (*e.g.*, more lenient or receptive to recommendations) than the typical judge. If judge-specific factors are correlated with inclusion in the experiment, then such factors might produce bias in the instrumental variables estimates presented in Table 6. Given the existing sample of 16 judges, it is not possible to evaluate directly how the judges in the experiment differed from the typical Philadelphia judge. Nevertheless, using these data, it is possible to evaluate the importance of judge-specific factors among those judges who selected into participation in the experiment.

The results from Table A2 can help to evaluate the magnitude of this selection problem. The 8 columns from Table A2 show results from 8 different reduced-form linear regressions. In columns (1) and (2), the dependent variable is the log of the assigned bail amount. In columns

(3) and (4), the dependent variable is a dummy for whether the defendant posted bail. In columns (5) and (6), the dependent variable is a dummy for whether the defendant was rearrested for new crimes during release. And in columns (7) and (8), the dependent variable is the dummy for failure to appear. Columns (1), (3), (5), and (7) each show the results of a bivariate regression of the dependant variable on a constant and a dummy for treatment. Columns (2), (4), (6), and (8) add judge fixed effects. The first row of Table A2 shows the R^2 for each of these 8 regressions. The bottom row shows an F-test for the significance of the judge fixed effects over and above the effect of the randomly assigned treatment designation. This F-test provides a measure of the degree to which the variance in these endogenous variables can be explained by idiosyncrasies in judges' behavior.

The results from Table A2 show that judge-specific factors do have a significant influence on bail levels. For defendant behaviors such as release, additional crimes, and failure to appear, however, judge-specific factors appear to be relatively unimportant, and the null hypothesis of zero coefficients is not rejected. This evidence does not rule out the possibility that the 16 judges are fundamentally different from non-participant judges. Nevertheless, the evidence presented in Table A2 lends support to the theory that judge-specific factors explain little of the variance in defendants' outcomes. If the hypothesis is correct that judge-specific factors are not important determinants of defendant outcomes, then choosing a sample of particularly lenient judges should not affect the instrumental variables estimates.

Heterogeneous Effects of Treatment Status and Bail

One factor that may affect the interpretation of the results in this study is heterogeneity in the second-stage coefficients. The models estimated in this study assume that the elasticities of posting, rearrest, and failure-to-appear with respect to bail are constant across defendants in the population. In reality, these elasticities are likely to vary across defendants. Defendants who are particularly poor or dangerous might be more sensitive than average to price changes. We might also expect the first-stage effect of treatment status on bail to vary across individuals. In the absence of judicial guidelines, bail amounts tend to increase with wealth and dangerousness of the defendant. Assignment to the treatment group, for whom bail amounts are relatively low and uniform, will disproportionately benefit wealthy and dangerous defendants.

As Heckman, Urzua, and Vytlačil (2006) show, in the case of heterogeneity in both first- and second-stage effects, instrumental variables approaches do not, in general, estimate the average treatment effect in the second-stage equation. In the context of the Philadelphia Bail Experiment, the groups who received greater levels of treatment are the wealthy and dangerous defendants, for whom the first-stage effect of treatment status on bail is likely to be large. Hence, if the effects of bail are heterogeneous, the instrumental variables estimation strategy identifies a weighted average of the effect of bail for different individuals in the sample, where wealthy and dangerous defendants receive greater weight than poor or less dangerous defendants.

In the main estimates in Table 7 of this paper, we allow for some heterogeneity in the effects of bail by estimating our model separately for defendants at low, medium, and high levels of dangerousness. Our results are consistent with the hypothesis that the optimal bail level increases with defendant dangerousness. Unfortunately, the sample used in this analysis is too small, and the estimates are too imprecise to measure and control for treatment effect heterogeneity in a conclusive way.

Despite this limitation of the estimates, the “treatment on the treated” effects that are identified in this study are relevant for the purposes of public policy. Many of the policy initiatives that have been designed to change bail levels have worked through judicial guidelines much like those used in the Philadelphia Bail Experiment. To the extent that the guidelines work in a similar way as the ones in this study (so that wealthy and dangerous defendants are disproportionately affected), the cost-benefit analysis provided in this paper identifies the treatment effects for the relevant population. For different forms of guidelines or bail policy initiatives, our results provide some evidence about the possible effects; however, this limitation about heterogeneous effects should be taken into account when generalizing our estimates to other contexts.

In addition to affecting the interpretation of the estimates, heterogeneous treatment effects provide an alternative explanation for the differences in our estimated effects between cross-sectional and instrumental variables approaches. Our instrumental variables estimates of the elasticities of posting and rearrest with respect to bail are considerably more negative than the cross-sectional estimates. One explanation is that there is some omitted variable that is positively correlated with both bail and posting and another omitted variable that is positively correlated with bail and rearrest. In the rearrest equation, this explanation is consistent with the hypothesis that dangerousness is an omitted variable that is positively correlated with both bail levels and rearrest. However, an alternative explanation for the differences between cross-sectional and instrumental variables estimates is that they accurately estimate the treatment effects for different populations. The “treatment on the treated” parameter that is measured by the instrumental variables estimation places more weight on especially dangerous individuals. Cross-sectional estimation, on the other hand, could represent a different weighted average of treatment effects, which could lead to different parameter estimates. If, for instance, dangerous individuals have especially elastic responses to bail amounts, and if the cross-sectional estimation places less weight on these dangerous individuals, then we would observe the same type of differences between cross-sectional and instrumental variables estimates as we observe in Tables 4 and 6.²³

Calculation of Statistics in Table 1

The statistics presented in Table 1 of the main text involve a handful of assumptions and back-of-the-envelope calculations. The Total Persons Incarcerated figure is taken from Beck and Karberg (2001) and applies to June 30, 2000. Fraction eventually sentenced indicates fraction who were convicted and sentenced to any prison or jail time. Average bail amounts and fractions sentenced, rearrested, and failing-to-appear calculated from the Bureau of Justice Statistics 2000 State Court Processing Statistics. Sample includes felony defendants from large urban counties who were arrested for state-level offenses on selected days in May, 2000. We also use this sample to estimate the Total Untried Defendants figures. We do this by measuring pre-trial time served, pre-trial time out on bail, and post-trial time served for each defendant. The fraction who are untried and detained is estimated as total pre-trial detention time divided by total time served, where totals are summed over all defendants in the data. The fraction who are untried and out on bail is estimated as total time out on bail divided by total time served, also summed over the defendants in the data. Sentence lengths are available for defendants in the data, but not time

²³ Lochner and Moretti (2004) discuss a similar argument for explaining the differences between cross-sectional and instrumental variables estimates of the effect of education on crime.

served. Following Durose and Langan (2003), we assume offenders sentenced to state or federal prisons served 55% of their maximum sentences. We assume offenders sentenced to local jails completed their entire sentences. For life and death sentences, maximum sentence length is assumed to last from the time of sentencing until age 75. To convert these fractions into totals, we assume that fraction untried is the same for these felony defendants in the data as for the overall prison population (including those held for municipal and federal offenses).

Limitations of Cost-Benefit Analysis

A number of important criticisms have been raised about how cost-benefit analysis is implemented in practice, as summarized by Sen (2000). By giving all parties equal weight in the social welfare function, we ignore the distributional effects of bail policy. These distributional effects could be particularly important in the case of bail, because redistribution toward defendants could increase equality (because defendants are poor), but could also generate adverse incentives to commit crimes. In addition to these distributional assumptions, the analysis in this paper assumes a Hicksian social welfare function in which policies' costs and benefits can all be measured in dollars. However, many reasonable social welfare functions take into account long-term and difficult-to-measure factors (such as rights, motives, and precedents) that we do not consider. While this cost-benefit study provides useful information about the tradeoffs associated with bail policy, the Hicksian efficiency associated with a policy is only one factor that the optimal social planner would consider in making policies.

Bail Bondsmen

One limitation of the current analysis is that we focus on a single municipality (Philadelphia) in which bail bondsmen are and were illegal. Previous research has shown that bail bondsmen are more effective than court systems at recovering fugitive defendants (Helland and Tabarrok, 2004). For this reason, the optimal bail amount may be lower in districts that allow bondsmen than in other districts, because the cost of flight is lower.

Credit toward Eventual Sentence

For defendants who are eventually found guilty, pre-trial detention time typically counts toward the defendant's eventual sentence. Hence, for defendants who expect to be found guilty, detention has an additional benefit of credit toward one's sentence. The value of freedom modeled here also takes into account this possible benefit of detention. It is the composite value of being freed (including all the privileges and costs) that is relevant for a cost-benefit analysis of bail. However, given the very specific conditions under which freedom is granted in this context, the value of freedom estimated in this study cannot easily be generalized to dollar-freedom tradeoffs in other situations.

Nonlinear Costs of Jailing

Following previous research (Levitt, 1996; Lochner and Moretti, 2004), we assume that the administrative cost of incarceration is a linear function of the number of days spent in jail. One bail clerk in Philadelphia, in private conversations, indicated that much of the administrative

cost associated with incarceration is due to fixed costs such as entering the defendant into the system and giving a medical examination. Hence, the total cost of 90 days of prison may be different from 90 days times the daily cost (\$105) used in our study. After taking into account the fraction of sentence time spent in jail (Durose and Langan, 2003), we estimate that the typical defendant in the 1990 to 2002 State Court Processing Statistics spent roughly 5.9 months in jail. Hence, the fixed costs per day of jail could be twice as large for bail defendants as for the typical prisoner. For these reasons, the cost of jailing a defendant may be higher than what we estimate in this study, and as a result, our estimates of the socially optimal bail may understate the true optimal levels.

Budget Constraint for Government

One constraint that we ignore in calculating the socially optimal bail policy is the budget constraint for the government. In Philadelphia, the 3% of bail amounts that the Municipal Court keeps is used to finance some of the court's activities (such as tracking down fugitive defendants). For the average defendant in our regression sample, we estimate an optimal bail level of \$17,700. At this level, we estimate that 35.4% of defendants would post bail, and 5.5% would fail to appear at one or more court date. Our estimated cost per flight is \$395. Hence, at our estimated optimal bail level, the expected cost associated with flight for the average defendant would be $0.055 * \$395 \approx \21.73 . The expected revenue to the court is calculated as 3% of the \$17,700 for each of the 35.4% who post bail. Hence, the expected revenue would be roughly $0.03 * 0.354 * \$17,700 \approx \187.97 . Hence, we expect that the government's budget would be balanced.

Unconditional versus Conditional Probabilities

The probabilities of rearrest and failure-to-appear in court that are used in this study are unconditional probabilities. Hence, the probability of rearrest is calculated as the total number of rearrests divided by the total number of defendants, and the probability of failure-to-appear is calculated as the total number of failures divided by the total number of defendants. In many cases, it is useful to know the probabilities of rearrest or flight conditional on release. These probabilities can be calculated by dividing the unconditional probabilities of rearrest or flight by the probability of release. For cost-benefit purposes, however, it is the unconditional probability that is relevant, because the bail decision is made before the defendant has decided whether to post or not.

Derivation of the Value of Lost Freedom

Given our assumptions about V_i and the release decision, the expectation $E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i]$ can be expressed as:

$$(A4) \quad E[I\{V_i < 0.037 * bail_i\} * V_i]$$

where $I\{\cdot\}$ represents the indicator function.

From Equation (8), we have that $\ln(V_i) = \beta^{Post} \mathbf{X}_i - \varepsilon_i^{Post}$. The expected value in Equation (A4) can now be expressed in integral form as:

$$(A5) \quad \frac{\int_{\frac{-\ln(0.037 * bail_i) + \beta^{Post} \cdot \mathbf{X}_i}{\sigma_{Post}}}^{\infty} e^{\beta^{Post} \cdot \mathbf{X}_i - \sigma_{Post} * u} d\Phi(u)}{\sigma_{Post}}$$

Integrating this expression produces:

$$(A6) \quad e^{\beta^{Post} \cdot \mathbf{X}_i + \frac{\sigma_{Post}^2}{2}} \left[\Phi(\infty) - \Phi\left(\frac{\beta^{Post} \cdot \mathbf{X}_i - \ln(0.037 * bail_i)}{\sigma_{Post}} + \sigma_{Post}\right) \right].$$

As shown in Equation (13), the expression $e^{\beta^{Post} \cdot \mathbf{X}_i + \frac{\sigma_{Post}^2}{2}}$ can be rewritten as $E[V_i | \mathbf{X}_i]$.

Substituting this formula for $E[V_i | \mathbf{X}_i]$ and evaluating the formula at the limits of integration produces Equation (14):

$$(15) \quad E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i] = \left[1 - \Phi\left(\frac{\beta^{Post} \cdot \mathbf{X}_i - \ln(0.037 * bail) + \sigma_{Post}^2}{\sigma_{Post}}\right) \right] * E[V_i | \mathbf{X}_i].$$

Formulas for the Value of Lost Freedom, Alternative Models

Linear Specification

Suppose that $V_i = \beta^{Post} \cdot \mathbf{X}_i - \varepsilon_i$, where ε_i is normally distributed. The average value of freedom among individuals in the population, $E[V_i | \mathbf{X}_i]$ can be expressed as:

$$(A7) \quad E[V_i | \mathbf{X}_i] = \beta^{Post} \cdot \mathbf{X}_i$$

The expected value of lost freedom, $E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i]$ can be expressed as:

$$(A8) \quad E[(1 - Post_i) * V_i | bail_i, \mathbf{X}_i] = \beta^{Post} \cdot \mathbf{X}_i - \sigma_{Post} \frac{\phi\left(\frac{0.037 * bail_i - \beta^{Post} \cdot \mathbf{X}_i}{\sigma_{Post}}\right)}{\Phi\left(\frac{0.037 * bail_i - \beta^{Post} \cdot \mathbf{X}_i}{\sigma_{Post}}\right)}$$

Square Root Specification

Suppose that $\sqrt{V_i} = \beta^{\text{Post}} \mathbf{X}_i - \varepsilon_i$, where ε_i is normally distributed. The average value of freedom among individuals in the population, $E[V_i | \mathbf{X}_i]$ can be expressed as:

$$(A9) \quad E[V_i | \mathbf{X}_i] = \frac{(\beta^{\text{Post}} \mathbf{X}_i)^2 + 1}{\sigma_{\text{Post}}^2}$$

The expected value of lost freedom, $E[(1 - \text{Post}_i) * V_i | \text{bail}_i, \mathbf{X}_i]$ can be expressed as:

$$(A10) \quad E[(1 - \text{Post}_i) * V_i | \text{bail}_i, \mathbf{X}_i] = \beta^{\text{Post}} \mathbf{X}_i + \sigma_{\text{Post}}^2 [1 - \lambda(a)(\lambda(a) - a)] - \sigma_{\text{Post}} * \lambda(a),$$

where $\lambda(a) = \frac{\phi(a)}{\Phi(a)}$ and $a = \frac{\sqrt{0.037 * \text{bail}_i} - \beta^{\text{Post}} \mathbf{X}_i}{\sigma_{\text{Post}}}$.

Linear Probability Specification

Suppose that $\ln(V_i) = \beta^{\text{Post}} \mathbf{X}_i - \varepsilon_i$, but that ε_i is distributed so that the probability of posting is a linear function of $\ln(0.037 * \text{bail}_i)$ and $\mathbf{X}_i$. The average value of freedom among individuals in the population, $E[V_i | \mathbf{X}_i]$ can be expressed as:

$$(A11) \quad E[V_i | \mathbf{X}_i] = e^{\beta^{\text{Post}} \mathbf{X}_i - 0.5\sigma_{\text{Post}}^2}$$

The expected value of lost freedom, $E[(1 - \text{Post}_i) * V_i | \text{bail}_i, \mathbf{X}_i]$ can be expressed as:

$$(A12) \quad E[(1 - \text{Post}_i) * V_i | \text{bail}_i, \mathbf{X}_i] = \frac{1}{\sigma_{\text{Post}}} \left(0.037 * \text{bail}_i - e^{\beta^{\text{Post}} \mathbf{X}_i - \sigma_{\text{Post}}^2} \right).$$

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Table A1: Sensitivity Analysis

Specification	Average Value of Freedom	Socially Optimal Bail Amount	Social Cost at Optimal Bail Level
90-day Discount Rate			
10% (benchmark)	\$1,050	\$17,700	\$8,110
3%	\$910	\$17,800	\$8,090
50%	\$1,840	\$16,900	\$8,230
100%	\$2,830	\$16,000	\$8,360
Social Cost of Crime			
\$44,700 (benchmark)	\$1,050	\$17,700	\$8,110
\$15,000	[no change]	\$5,800	\$5,700
\$30,000	[no change]	\$11,700	\$9,590
\$60,000	[no change]	\$23,900	\$8,610
Functional Form for V_i			
ln(.) (benchmark)	\$1,050	\$17,700	\$8,110
sqrt(.)	\$777	\$27,500	\$8,920
linear	\$757	\$65,000	\$9,850
sqrt(.) incl. zeros (N=568)	\$689	\$20,200	\$8,690
linear incl. zeros (N=568)	\$738	\$49,500	\$9,430
Distribution for Binary Choice			
Probit (benchmark)	\$1,050	\$17,700	\$8,110
Linear Probability Model	\$388	\$13,000	\$5,420
Choice of Regression Sample			
Benchmark (N=487)	\$1,050	\$17,700	\$8,110
Entire Sample (N=1,085)	\$47,200	\$11,100	\$4,800
All Felonies (N=730)	\$3,600	\$10,000	\$6,000
All Felonies by Tertile of "Dangerousness" Proxy			
Low (N=256)	\$16,600	∞	\$17,180
Medium (N=257)	\$1,050	\$1,500	\$10,730
High (N=217)	\$980	\$34,800	\$9,520

Notes: Calculations performed in the same way as in Table 7, but with slight modifications to the benchmark specification. Details in the text of this appendix.

Table A2: Predictive Power of Judge Fixed Effects in Reduced-Form Linear Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable							
	Ln(Bail)		Posted Bail		Rearrest		Failed to Appear	
R ²	0.04	0.15	0.02	0.06	0.01	0.04	0.00	0.01
Treatment Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge Fixed Effects		Yes		Yes		Yes		Yes
F-Stat for Judge Fixed Effects, F(14, 471)		4.13**		1.25		0.83		0.39

Notes: The regressions shown in this table measure the importance of judge-specific factors in determining bail levels and defendant outcomes. The specifications in Columns (1), (3), (5), and (7) are the same as in Table 5 of the main text. The specifications in Columns (2), (4), (6), and (8) add judge fixed effects, and the F-statistic tests the null hypothesis that, after controlling for treatment status, the coefficients on the remaining judge dummies are zero.

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