

Misgovernance and Human Rights:
The Case of Illegal Detention without Intent
Supporting Information

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Contents

A1 Incidence of Pre-Trial Detention Globally, Estimates of Illegal Pre-Trial Detention	A-3
A1.1 Incidence of Pre-Trial Detention	A-3
A2 Theoretical Model	A-4
A2.1 Observable Implications with SUTVA	A-6
A2.2 Observable Implications of Substitutable Effort	A-6
A2.3 Observable Implications of Congestion	A-8
A3 Pretrial Detention in Haiti: Historical Trends	A-8
A4 Descriptive Profile of Detainees	A-9
A4.1 Demographic Information	A-9
A4.2 Charges	A-9
A5 Data Collection	A-11
A5.1 Interviews	A-11
A5.2 Experimental Data Collection	A-11
A6 Research Design Details and Validation	A-13
A6.1 Balance Table	A-13
A6.2 Pre-Treatment Missingness and Attrition	A-14
A6.3 Manipulation Check	A-14
A7 Reasons for Non-Compliance	A-14
A8 How many detainees could have been released?	A-18
A9 Change of President and Rate of Case Advancement	A-19
A10 Heterogeneity in Case Advancement	A-19
A10.1 Original Agent with Case File	A-19
A10.2 Previous Case Advancement	A-20
A11 Liberation During Intervention Period	A-21
A12 No Evidence of Congestion	A-22

A1 Incidence of Pre-Trial Detention Globally, Estimates of Illegal Pre-Trial Detention

A1.1 Incidence of Pre-Trial Detention

Figure A1 shows the incidence of pre-trial detention cross nationally, using data on 186 countries from the Institute for Criminal Policy Research (2017). Note that this data contains only rates of pre-trial detention, not illegal or prolonged pre-trial detention. Figure A1 graph the proportion of each nation’s prison population held in pre-trial detention. This includes, but does not delineate, illegal from legal remand detention. However, high rates of pre-trial detention in this measure are an appropriate proxy for rates of prolonged pre-trial detention.

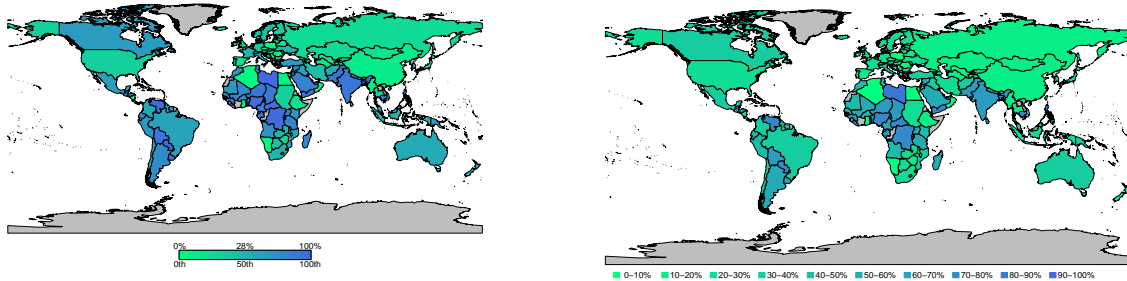


Figure A1: This graph depicts the share of national prison populations comprised by pre-trial detainees. Data from Institute for Criminal Policy Research (2017). The figure on the left reports the absolute share while the figure on the right represents countries’ comparative ranking on this measure.

We provide a rough estimate of the number of illegal pretrial detainees cross-nationally by bounding the share of prisoners that can be accommodated by the courts while preserving statutory or constitutional protections in terms of the length and conditions of pretrial detention. In OECD countries, the average rate of pretrial detention is ≈ 20 percent. While abuses still occur, they are less pervasive.¹ The following simulation depicted in Figure A2 is estimated using Equation 1, where countries providing prison data are indexed i , the rate of pretrial detention is denoted r_i and the population of prisons nationwide is χ_i . We vary the threshold $\tau \in [0, 1]$ over which detainees are assumed to be illegally detained.

$$Illegal\ detainees = \sum_{i=1}^{186} \max\{0, (r_i - \tau)P_i\} \quad (1)$$

The results across the range of possible τ 's is depicted in Figure A2. We posit that a reasonable range for τ is 0.15 to 0.25 (indicated by the vertical and horizontal). At $\tau = .2$, we estimate slightly more than 1 million illegal pretrial detainees in these 186 countries.

¹Consider the tragic case of Khalief Browder, who was detained illegally in a New York prison for three years. This case was noteworthy both for the egregiousness of the human rights violation and for its relative rarity.

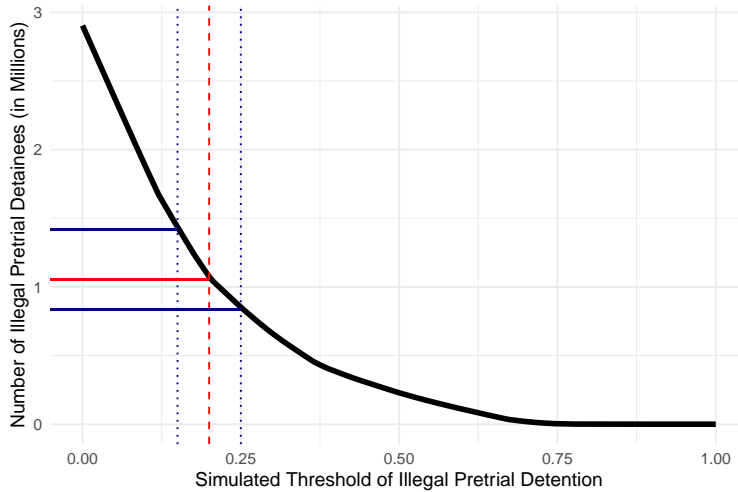


Figure A2: This graph depicts the number of pretrial detainees per 100,000 in the national population. Data from Institute for Criminal Policy Research (2017).

Where are these illegal detainees detained? Figure A3 plots the geographic distribution of these presumed illegal detainees when $\tau = .2$.

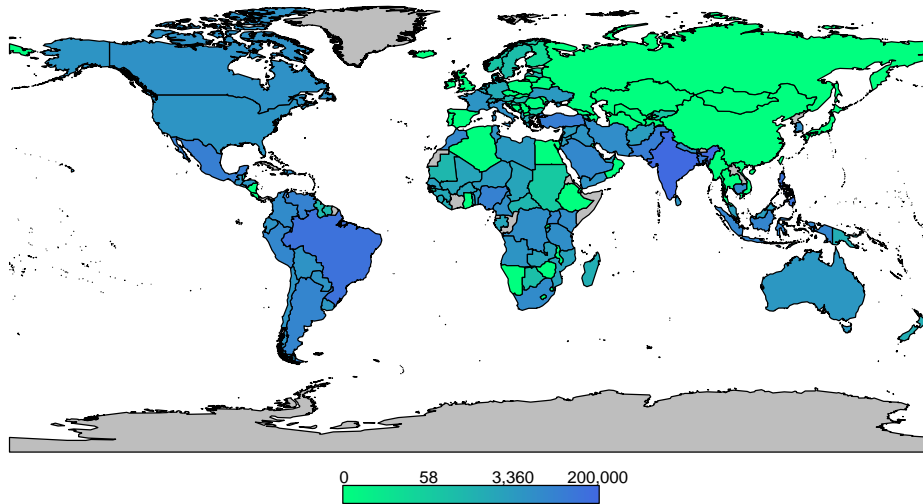


Figure A3: This graph depicts estimates of the number of illegal pretrial detainees per country. Estimated from Equation 1, assuming $\tau = .2$. Note the shading uses a log scale. Data from Institute for Criminal Policy Research (2017).

A2 Theoretical Model

Here we introduce a theoretical model that generates the testable hypotheses presented in the main paper. While a somewhat simpler model would yield these hypotheses, we present a version that helps to understand the dynamics underlying possible SUTVA violations.

Consider two agents indexed by agency, $j \in \{P, C\}$, representing the parquet (P) and the court (C). The prosecutor (parquet agent) and investigative judge (court agent) earn a lump sum wage w_j . Each employee has a unit mass of backlogged cases. We consider the cases from each agency in the context of a multitask contracting framework where the two tasks consist of working on unrepresented (U) and represented (R) cases, denoted α_{Uj} and α_{Rj} , respectively such that $\alpha_{Uj} + \alpha_{Rj} \in [0, 1]$. Outputs within an agency are a function of effort and mean-zero, standard normal variance covariance matrix Σ_j .

$$q_{Uj} = \alpha_{Uj} + \epsilon_{ij} \quad (2)$$

$$q_{Rj} = \alpha_{Rj} + \epsilon_{ij} \quad (3)$$

Agents have constant absolute risk aversion (CARA) utility, where η represents the coefficient of absolute risk aversion.

$$U_A(w, \alpha_j) = -e^{-\eta(w_j - Sp(1 - q_{Uj} - q_{Rj}) + \psi(\alpha_{Uj}, \alpha_{Rj}))} \quad (4)$$

In this formulation, w is a fixed public sector wage. S is a sanction imposed on the basis of (lack of) outputs with probability p . Following Holmstrom and Milgrom (1991), we parameterize $\psi(\cdot)$ as:

$$\psi(\alpha_{Uj}, \alpha_{Rj}) = \frac{c_{Uj}\alpha_{Uj}^2 + c_{Rj}\alpha_{Rj}^2}{2} - \delta(\alpha_{Uj}\alpha_{Rj}) \quad (5)$$

where $0 \leq \delta \leq \sqrt{c_{Rj}c_{Uj}}$ represents the substitutability of effort between the two tasks. The worker's certainty equivalent is thus:

$$w + Sp(\alpha_{Uj} + \alpha_{Rj}) - \frac{\eta}{2}(Sp(\sigma_{Uj} + \sigma_{Rj})) - \frac{c_{Uj}\alpha_{Uj}^2 + c_{Rj}\alpha_{Rj}^2}{2} - \delta(\alpha_{Uj}\alpha_{Rj})$$

Maximizing with respect to α_{Uj} and α_{Rj} yields optimal effort:

$$a_{Uj}^* = \frac{Sp(c_{Rj} - \delta)}{c_{Rj}c_{Uj} - \delta^2}, a_{Rj}^* = \frac{Sp(c_{Uj} - \delta)}{c_{Rj}c_{Uj} - \delta^2} \quad (6)$$

Within the period of the study, we assume that the principal does not alter w , S , or p ; thus we treat these parameters as exogenous. As such, we characterize a partial equilibrium.

We now turn to the mapping of outputs between agencies. We assume that σ_j is a partition from a larger mean-zero multivariate normal covariance matrix \mathcal{B} , which indexes both the the assignment status of the case, $z \in \{R, U\}$ as well as the agency j . Without loss of generality, assume $\sigma_{Rj} = \sigma_{Uj} = 1 \forall j$; the notation below simply clarifies the structure of the matrix.

$$\mathcal{B} = \begin{bmatrix} \sigma_{UP}^2 & 0 & 0 & 0 \\ 0 & \sigma_{RP}^2 & 0 & \rho \\ 0 & 0 & \sigma_{UC}^2 & 0 \\ 0 & \rho & 0 & \sigma_{RC}^2 \end{bmatrix}$$

Legal assistance increases ρ from 0 in the unrepresented state to $\rho > 0$ in the represented state. We assume that covariance terms except for ρ are zeros, we can thus represent case outputs using a bivariate probit function.² Cases advance when they clear a threshold T_j , e.g. $q_j > T_j$. Liberation occurs when they clear the thresholds in both offices, e.g. $q_j > T_j \forall j$. We can thus write the probability of case advancement and liberation as a function of the T_j . Φ_2 denotes the bivariate probit function.

$$\begin{aligned} Pr(\text{Case Advancement}|z) &= 1 - \Phi_2(T_P - \alpha_{zP}^*, T_C - \alpha_{zC}^*, \rho) \\ Pr(\text{Liberation}|z) &= \Phi_2(\alpha_{zP}^* - T_P, \alpha_{zC}^* - T_C, \rho) \end{aligned}$$

²In the empirical setting, the random assignment of case types makes this assumption plausible.

A2.1 Observable Implications with SUTVA

First, assume that $\delta = 0$ so that efforts on represented and unrepresented cases are independent. Further, assume that $p < \frac{c_{Rj}c_{Uj}-\delta^2}{S(c_{Rj}+c_{Uj}-2\delta)}$, to ensure that total effort $(\alpha_{Uj}^* + \alpha_{Rj}^*)$ remains interior. These assumptions imply that SUTVA holds in the present model.

In this case, it is straightforward to show that $\alpha_{Uj}^* = \frac{Sp}{c_{Uj}}$ and $\alpha_{Rj} = \frac{Sp}{c_{Rj}}$. Since legal assistance reduces the cost of effort, $c_{Rj} < c_{Uj}$ implies that $\alpha_{Rj}^* > \alpha_{Uj}^*$.³ The difference in the probability of case advancement, $\Delta_{Advancement}$, is thus:

$$\Delta_{Advancement} = 1 - \Phi_2(T_P - \alpha_{RP}^*, T_C - \alpha_{RC}^*, \rho) - (1 - \Phi_2(T_P - \alpha_{UP}^*, T_C - \alpha_{UC}^*, \rho))$$

Further, it is straightforward to show that the difference in probability of liberation, $\Delta_{Liberation}$, is thus:

$$\Delta_{Liberation} = \Phi_2(\alpha_{RP}^* - T_P, \alpha_{RC}^* - T_C, \rho) - (\Phi_2(\alpha_{UP}^* - T_P, \alpha_{UC}^* - T_C, \rho))$$

While there are no closed form comparative statics, the behavior of the bivariate probit function is well established.

Proposition 1. *The difference in the probability of case advancement for represented and unrepresented cases is positive when $c_{Uj} > c_{Rj}$ and $\rho \geq 0$.*

Proof: $c_{Uj} > c_{Rj}$ implies that $\alpha_{Uj}^* < \alpha_{Rj}^*$, as shown above. Thus, $\Phi_2(T_P - \alpha_{RP}^*, T_C - \alpha_{RC}^*, \rho) < \Phi_2(T_P - \alpha_{UP}^*, T_C - \alpha_{UC}^*, \rho)$. This implies that the difference, $\Delta_{Advancement} > 0$.

Because the second task – represented cases – is experimentally induced, Proposition 1 yields two observable implications.

- The probability of advancement among represented cases should be greater than the probability of advancement in control. For the purposes of our data, this corresponds to cases that spent a longer duration in treatment.
- The rate of advancement among represented cases should increase relative to the rate of advancement prior to the start of the legal assistance.

Proposition 2. *For any $\alpha_{Rj}^* > \alpha_{Uj}^*$, the probability of liberation is increasing in the correlation in outputs between the two agencies, ρ , for all $\rho > 0$.*

Proof: $\frac{\partial \Delta_{Liberation}}{\partial \rho} > 0 \forall \rho \geq 0$.

Collectively Propositions 1 and 2 imply that legal assistance should increase the rate of liberation through some combination of the shock to the marginal cost of effort and the correlation induced in the outputs. These results are illustrated in Figure A4.

A2.2 Observable Implications of Substitutable Effort

First, we relax the assumption that the tasks – represented and non-represented cases – are technologically independent, e.g. that $\delta = 0$. This is akin to saying that by creating represented cases as a task, we exogenously induce some increase in δ . Prior to treatment, random assignment ensures that $\delta = 0$. We assume that represented cases and non-represented cases are substitutes. The effect of legal assistance will be exaggerated as δ increases. Figure A5 varies $\delta \in [0, c_{Rj}]$ to demonstrate the possible range of results possible such that $\alpha_{Uj}^* \geq 0$.

Notably, while a SUTVA violation of this form may lead to overstatement of treatment effects where $\alpha_{Rj}^* > \alpha_{Uj}^*$, if there is indeed no change in equilibrium effort, presumably because $c_{Rj} = c_{Uj}$, the difference in probability of

³It is possible that legal assistance also increases the likelihood of sanctions by providing information about poor performance to the principal. In this case, $p_R > p_U$. The two mechanisms are complements and the effects cannot be separated empirically, though all of the following results continue to hold.

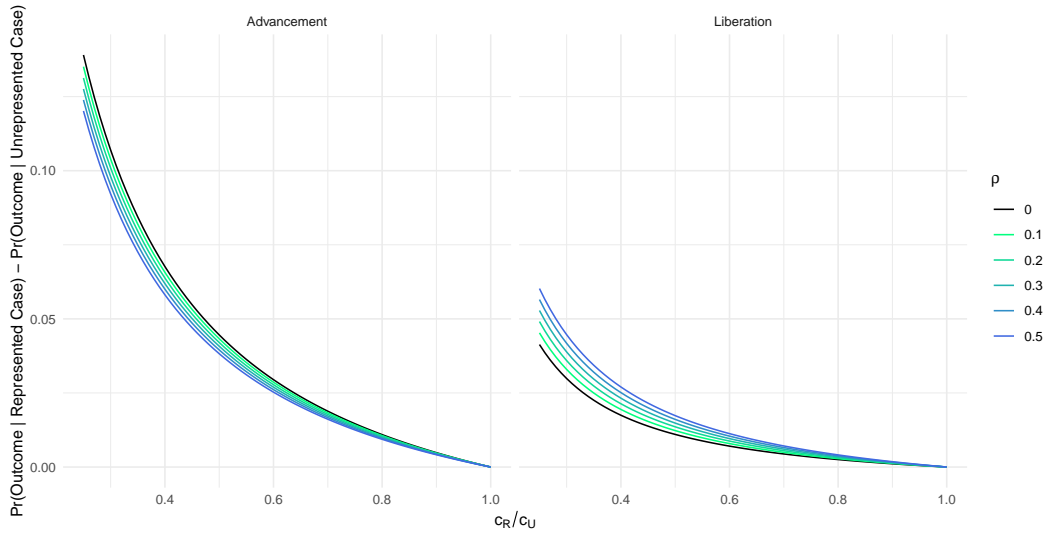


Figure A4: The difference in probability in case advances (left) and liberation (right) between represented and unrepresented cases. In this simulation $c_{Uj} = 1 \forall j$, $S = 1$, $p = .1$, $\delta = 0$, and $T_P = T_C = 1$.

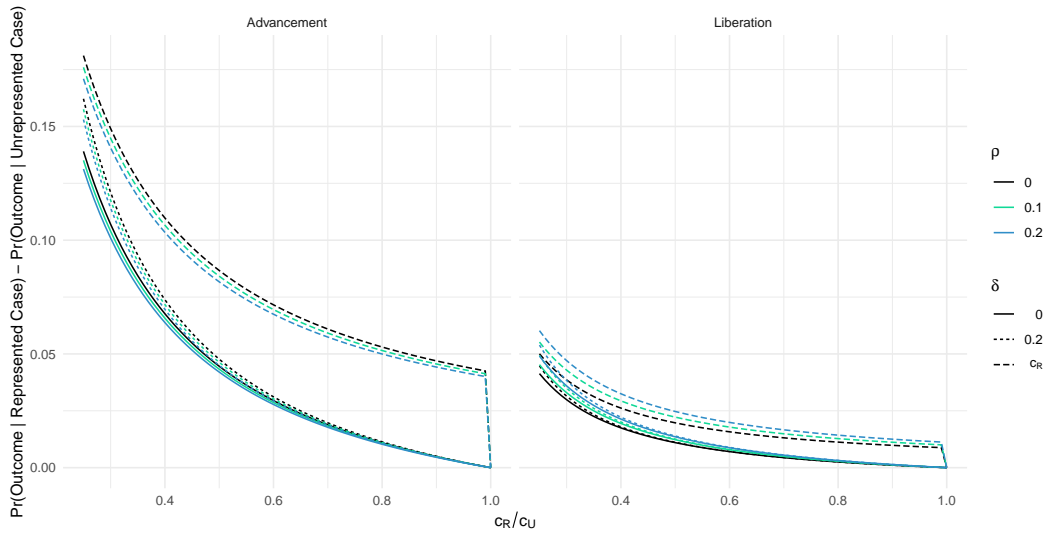


Figure A5: The difference in probability in case advances (left) and liberation (right) between represented and unrepresented cases, varying δ . In this simulation $c_{Uj} = 1$, $S = 1$, $p = .1$, and $T_P = T_C = 1$.

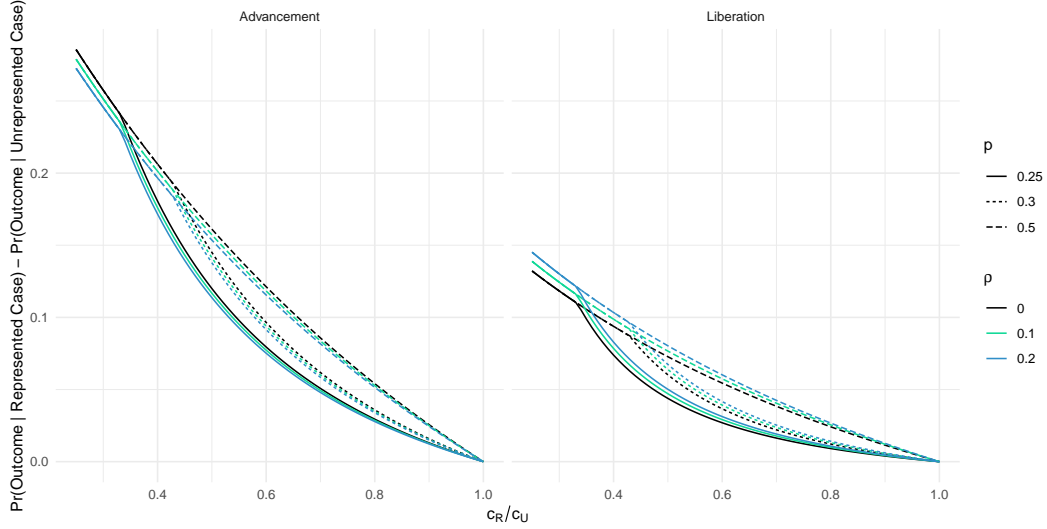


Figure A6: The difference in probability in case advances (left) and liberation (right) between represented and unrepresented cases, varying p . In this simulation $c_{Uj} = 1 \forall j$, $S = 1$, $d = 0$, and $T_P = T_C = 1$.

advancement is 0 for all δ . The degree of bias (difference in probabilities from the independent case) is quite sensitive to δ . However, we can take advantage of the randomized timing of the intervention to compare advancement in treated cases before and after the intervention, which will mitigate the extent of any bias induced by a simultaneous increase in δ . In this case, the equilibrium efforts that we compare are:

$$a_{Uj}^* = \frac{Sp}{c_{Uj}}, a_{Rj}^* = \frac{Sp(c_{Uj} - \delta)}{c_{Rj}c_{Uj} - \delta^2}$$

A2.3 Observable Implications of Congestion

Finally we relax the assumption that equilibrium effort is interior by relaxing the assumption that $p < \frac{c_{Rj}c_{Uj} - \delta^2}{S(c_{Rj} + c_{Uj} - 2\delta)}$. If the agents are working close to capacity prior to the intervention, a reduction in the cost of effort may be stymied if it overwhelms the capacity of the parquet/court to process cases (e.g. via congestion). To model this, we vary p holding $\delta = 0$. Figure A6 presents the simulations that demonstrate that for sufficient reductions in cost that that render $\alpha_{Rj}^* + \alpha_{Uj}^* > 1$, the observed difference in probability of advancement/liberation will be attenuated.

This analysis suggests that evidence that agents are not working at “full” capacity should reduce concerns about the role of this SUTVA violation in biasing estimates.

A3 Pretrial Detention in Haiti: Historical Trends

- The US Department of State’s annual Human Rights Country Report on Haiti document the number of prisoners and the number of pre-trial detainees held nationwide at the end of each year. These graphs provide trends over the ten years preceding the experiment. The left panel depicts the total number of detainees (blue solid lines) and the number of pre-trial detainees (green dotted lines). The right panel depicts the proportion of the prison population that are held in pre-trial detention.
- The 2010 earthquake destroyed the main prison in Port au Prince that houses nearly 40% of the nation’s prisoners. This damage led to a massive jail break.

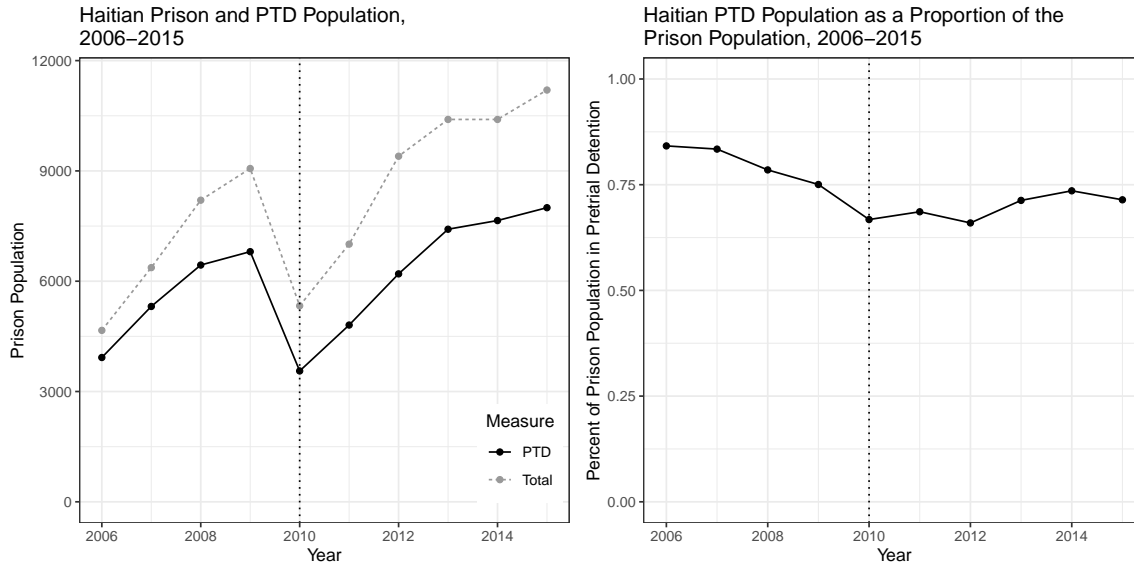


Figure A7: This graph depicts the rate of pre-trial detention in Haiti, as per US Department of States Human Rights Country Reports, 1996-2015.

A4 Descriptive Profile of Detainees

A4.1 Demographic Information

Figures A8 and A9 provide descriptive information on the detainees in the sample.

- Figure A8 reports the age and gender of detainees in the sample. Age data is self-reported (for the 876 detainees in the baseline) and from prison registers for those that were not present at baseline.
- Figure A9 depicts the self-reported asset ownership prior to detention. This data comes from the endline survey. Re-interview rates among those that were detained were 85.4% and among those that were freed was 9%. As such, the sample overrepresents those that were still detained. Rates of asset ownership for each asset are compared to responses to the same question from the 2012 DHS Haiti survey. In the DHS survey, we subset to the relevant jurisdictions. The results suggest that if anything, detainees recall having assets at slightly higher rates than the median respondent in the DHS survey. This suggests that, if illegal detention is indeed inflicted upon those that cannot afford to hire a lawyer or pay a bribe, a substantial portion of the Haitian population is liable to lack the resources to avoid illegal pretrial detention if arrested.

A4.2 Charges

Here we consider the charges against the detainees in our sample. Figure A11 documents that the frequency of broad classes of charges in the prison registers and in the court records are quite similar. These are computed by looking for common words (e.g. “vol” for theft) across recorded charges and recording the number of times the word appears. Some defendants are charged with multiple offenses. Each time the word appears in any charge, it is counted. Thus, some detainees contribute to multiple categories.

Now we consider the correspondence of charges between different sources, by detainee. To do this, we simply correlate indicators for each class of charge between different data sources. The panels consider the following data sources:

- **Measure 1:** Baseline survey (self-reported) measures for $n = 876$ detainees detained at baseline.
- **Measure 2:** Endline survey (self-reported) measures for $n = 640$ detainees detained at endline or successfully recontacted.

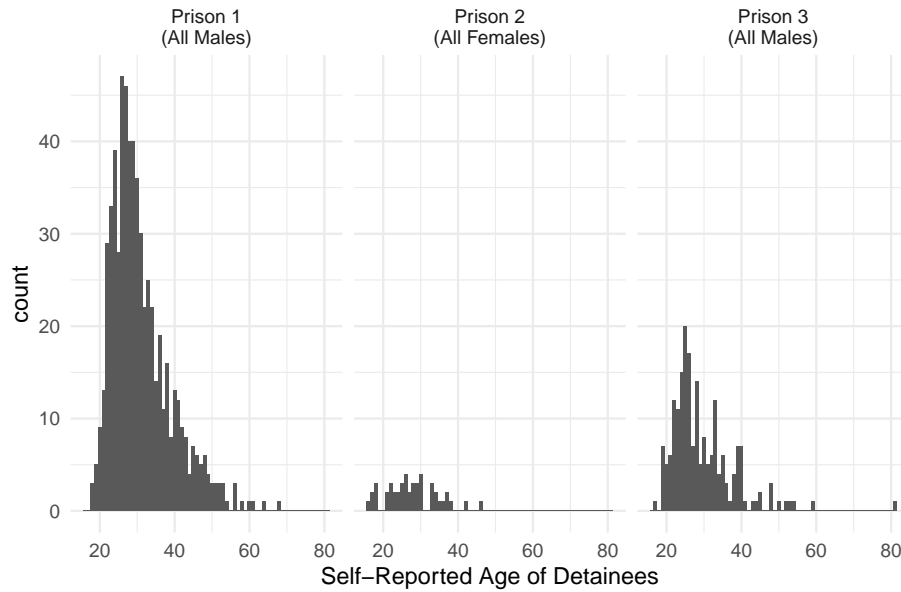


Figure A8: Reported age of and gender of detainees in the sample, by prison. Data from baseline survey and, where unavailable, prison records.

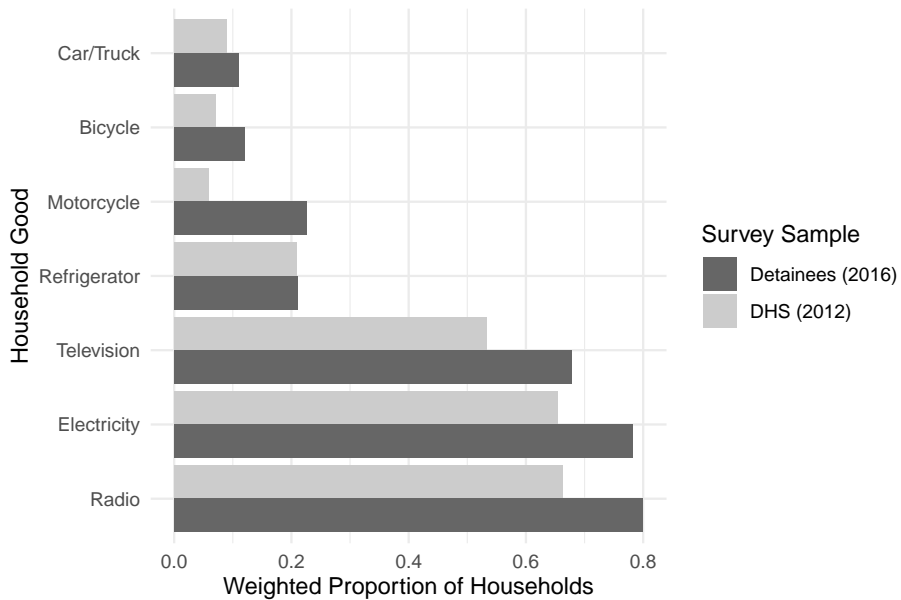


Figure A9: Reported asset ownership of detainees prior to detention compared to DHS survey data from a representative sample in the same jurisdictions, 2012.



Figure A10: Relative frequency of different classes of charges across the prison records and case files data. The case files cover a (random) subset of those in the prison records, given the random assignment of treatment. These records suggest very similar patterns across the two sources of data.

- **Measure 3:** Court records (administrative) for the $n = 503$ individuals assigned to treatment during the rollout.
- **Measure 4:** Prison records (administrative) for the $n = 1080$ individuals in the experimental sample.

Note that all correlations are positive. Those between (a) court and prison records and (b) baseline and endline surveys are quite strong. Nevertheless, the positive correlations in the third and fourth panels suggest a general awareness of the accusations among prolonged pretrial detainees.

A5 Data Collection

The data was collected in two phases: (i) qualitative fieldwork consisting of interviews with relevant actors; (ii) the field experiment, including baseline, implementation, and endline data.

A5.1 Interviews

We conducted interviews and focus groups in the jurisdictions of five of Haiti’s 15 courts of first instance (CFI) in June and July 2015, prior to initiating the experiment. The sample was defined by allocation existing investments of the donor (USAID). Nevertheless, these four jurisdictions represent the local CFIs for $\approx 58\%$ of the Haitian population. In sum, we interviewed 88 individuals. We report the actors interviewed in each jurisdiction in Table A1. The numbers in the cells represent the number of actors interviewed in each jurisdiction.

We note that some Haitian and Haitian-American program officers employed at USAID and most of the program administrators (employed by USAID contractor) formerly held positions within the Haitian justice system. Several were former judges and prosecutors; others were former lawyers. These past experiences allow for honest revelation of sensitive information with substantial knowledge of the institutional context.

A5.2 Experimental Data Collection

The experimental data collection consists of 5 rounds, enumerated as follows:

1. **Collection of prison records** (October-November 2015): Because prison records are believed to be more complete and accessible than court records, lawyers went to each experimental prison in November 2015 to create

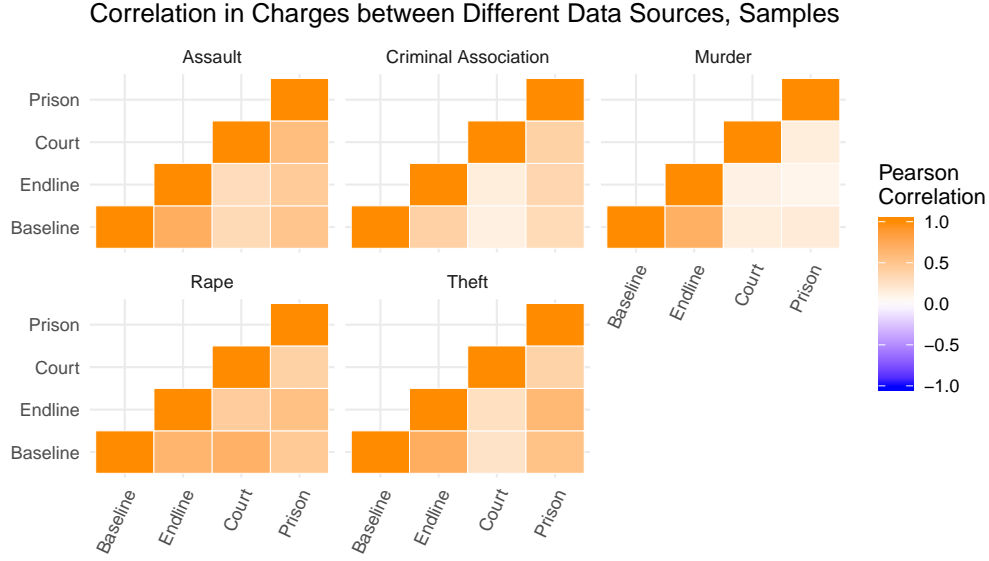


Figure A11: Correlation of the charge indicator variables reported by different data sources. Fraud and threatening were not recorded in the baseline survey so they are missing from the final three panels.

<i>Department:</i>	Ouest	Ouest	Artibonite	Nord	Nord-Est
<i>CFI:</i>	Port-au-Prince	Croix-des-Bouquets	Saint-Marc	Cap-Haïtien	Fort Liberté
<i>Prison(s):</i>	National Penetentiary	Croix-des-Bouquets	Saint-Marc	Cap-Haïtien	Fort Liberté
<i>Experimental sample:</i>	✓	✓			
A: LAWYERS, RIGHTS ADVOCATES, $n = 37$					
(i) Local bar association leadership		2	3	1	1
(ii) Defense attorneys working privately or for aid program(s)	5	6	6	7	6
B: COURT OFFICIALS, $n = 22$					
(i) Judges (CFI)		2	1	1	1
(ii) Prosecutors (CFI)	2	1		1	1
(iii) Clerks (CFI court and parquet)	3	2	1	1	2
(iv) Justice of the Peace (Peace Tribunal)	1		1	1	
C: PRISON OFFICIALS, $n = 10$					
(i) Haitian National Police officers staffing/administering prisons	5			2	3
D: DONORS, LEGAL ASSISTANCE PROGRAM ADMINISTRATORS, $n = 19$					
(i) USAID Program officers	7				
(ii) Program administrators (employed by contractor)	8	1	2	1	
TOTAL	31	14	14	15	14

Table A1: Interviews conducted, by type of actor and jurisdiction. We attempted to meet with at least one judge and one prosecutor in each jurisdiction; in two cases, these officials were absent or unavailable for interviews at the scheduled times. Note that USAID is based in Port-au-Prince so there were not parallel populations to interview in other jurisdictions.

a register of pretrial detainees believed to be held in the prisons. They collected detainee names, prison identification numbers, charges, age, date of arrest, and education to the extent that it is recorded in prison records. USAID contracted lawyers to obtain these files given their experience working with the prison records. The resultant list constituted the population from which the experimental sample is drawn.

2. **Baseline survey/census** (January-February 2016): The baseline survey data collection enabled us to assess which prisoners were actually held within a prison and collect baseline covariates from the experimental sample. USAID hired a survey firm and negotiated surveyor access to the prisons to conduct these surveys. Surveys required substantial cooperation from prison officials (members of the Haitian National Police) because detainees had to be extracted from their cells/area to meet with enumerators. For the security of enumerators and detainees alike, surveys were conducted in private areas removed from the main areas where detainees were held. The surveys allowed us to understand the accuracy of the prison records; measure any pre-treatment transfers or releases (i.e. between November 2015 and January 2016); and measure the effective sample of detainees.
3. **Case files** (February-May 2016): During the implementation period, we measure case advancement among cases assigned to various dosages of treatment using lawyers' case files. These files are compiled by the lawyers responsible for representing subjects. Importantly, recall that the lawyers did not know this was an impact evaluation. This allows us to measure lawyers' representation strategies; the case advancement observed; and past details of the cases. We scanned these files in June 2016 immediately after the final assessment of each case. The data from the files is hand-coded.
4. **Court records** (late 2016): A separate team of lawyers was hired to track cases in the Courts of First Instance from August-November 2016 to measure further advancement. However, more treatment cases were located than control cases which suggests increases in legibility. However, it also implies differential attrition levels by treatment group. As such we do not analyze these data in the main text.
5. **Endline survey/census** (September-November 2016): The endline survey enabled us to assess which prisoners were actually held within a prison as of September 2016. USAID hired a survey firm and negotiated surveyor access to the prisons to conduct these surveys. As in the baseline, surveys required substantial cooperation from prison officials (members of the Haitian National Police) because detainees had to be extracted from their cells/area to meet with enumerators. For the security of enumerators and detainees alike, surveys were conducted in private areas removed from the main areas where detainees were held. Where detainees were absent, enumerators checked prison files to measure case disposition and release. We use the combination of survey and administrative data collected to examine the liberation outcome. We also ask a number of attitudinal outcomes reported elsewhere.

The sequencing of the quantitative data collection is depicted in Figure A12.

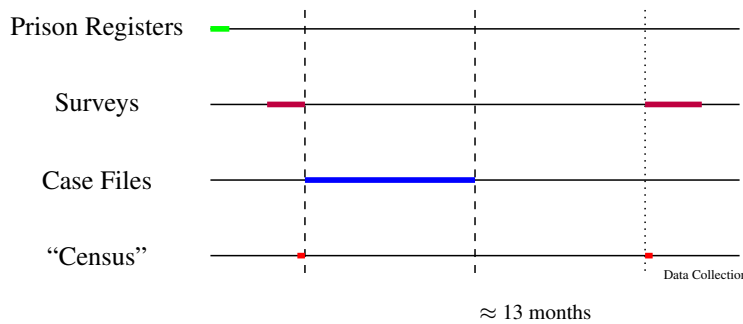


Figure A12: Depiction of data sources and temporal coverage as well as the sequencing of data collection.

A6 Research Design Details and Validation

A6.1 Balance Table

For a balance test, we regress covariates from the prison register data on treatment assignment coded in two ways:

- Columns 1-2 use the ordinal order indicator from each prison as the outcome variable. Since prisons are heterogeneous in the size of the experimental sample we employ prison fixed effects in both specifications.
- Columns 3-4 use quantile of the order assignment within a prison as the outcome variable. The F -tests in these specifications represent an appropriate test of the joint significance of all pretreatment covariates on the treatment indicator.

A6.2 Pre-Treatment Missingness and Attrition

There are two forms of missingness present in our data. We sampled from the list of prisoners in the three Haitian prisons that were collected from prison records two months prior to the baseline survey. From this sample, we formed the “quintet” blocks in each prison prior to the baseline. However, at the baseline, 19% of this sample was not located by enumerators in the prison during the census. This was unrelated to treatment: enumerators were not apprised of the design or the order in which detainees were expected to receive treatment. Baseline surveying occurred before treatment commenced. As such, missingness should not be systematically related to treatment assignment. We run a battery of empirical specifications, including Table A3 to assess the relationship between our treatment indicator and baseline missingness. In all specifications and subsamples, we fail to reject the null hypothesis of no relationship between treatment assignment and baseline missingness.

Baseline missingness was consistent with the case records of the lawyers during the treatment period. We did not apprise them of the findings of the survey: they aimed to locate both the prisoner and case files. We were able to identify whether each of the 876 individuals that was imprisoned during the endline survey nine months later. As such, for our liberation outcome, there is no subsequent attrition. Given the independence of treatment assignment and baseline attrition, this form of attrition will not bias our estimates. It also provides ancillary information about the state of records in the Haitian prison system: we estimate that absent legal assistance, approximately 1.6% pretrial detainees are released from prison per month (95% CI: [0.0074, 0.0258]). Accounting for detainee releases in the two months between collection of prison records and the baseline, we estimate that approximately 16% of the individuals prison registers are not presently held in a given prison.⁴

In our analyses of case files, lawyers collected data for all individuals assigned to treatment, so we have records for all 503 detainees assigned to treatment, regardless of compliance with treatment assignment, during the intervention period. While some case files are more complete than others, we collect basic outcomes for all detainees.

A6.3 Manipulation Check

This manipulation check comes from a question on the endline survey, inquiring whether detainees recalled a visit by a representative of [name of legal aid project].

A7 Reasons for Non-Compliance

In the legal assistance program that we evaluate, there are several reasons for which an individual assigned to treatment may not “take” treatment, as follows:

1. *Detainee is accused of an ineligible offense.* The donor funding legal assistance refuses to provide legal assistance to individuals accused of rape, human trafficking, or drug trafficking. Our sampling method attempted to minimize the prevalence of such detainees in the sample, but prison records are not fully accurate. Additionally, in French/Haitian Creole, the words for “theft” and “rape” (*vol* and *viol*) are quite similar and may have created some issues in the transcription of records.
2. *Case has multiple defendants.* Cases are difficult to prosecute when all defendants are not incarcerated. Given limited capacity to locate additional defendants, program implementers did not provide legal assistance to defendants in cases with more than five defendants or in cases where at least one co-defendant was at large.

⁴We cannot estimate the share of individuals presently in prison that do not appear in the registers. This is a more severe problem in terms of record-keeping and the preservation of detainees’ rights.

	<i>Dependent variable:</i>			
	Order		Order Quantile	
	(1)	(2)	(3)	(4)
Age	-0.710 (0.749)	-0.589 (0.768)	0.0004 (0.001)	0.0002 (0.001)
Incomplete Primary Ed.	-13.120 (19.094)	-15.580 (19.820)	0.012 (0.030)	0.022 (0.031)
Complete Primary Ed.	-11.484 (19.162)	-12.911 (19.566)	0.010 (0.026)	0.017 (0.027)
Incomplete Secondary Ed.	-16.655 (19.034)	-15.955 (19.200)	0.018 (0.027)	0.022 (0.028)
Complete Secondary Ed.	22.691 (38.905)	33.829 (40.044)	-0.035 (0.051)	-0.047 (0.053)
Number of charges	2.698 (10.685)	-0.088 (10.765)	-0.006 (0.014)	-0.001 (0.014)
Violent first charge		53.337 (39.003)		-0.065 (0.053)
Attempted murder charge		5.999 (27.992)		0.011 (0.042)
Conspiracy charge		2.151 (35.750)		0.008 (0.053)
Fraud charge		19.421 (32.963)		-0.013 (0.046)
Murder charge		44.392 (31.711)		-0.043 (0.047)
Other charge		34.737 (26.758)		-0.044 (0.039)
Theft charge		37.906 (40.211)		-0.069 (0.071)
Prison FE	✓	✓		
F-test, <i>p</i> -value			0.964	0.717
DV scale	[1,800]	[1,800]	[0,1]	[0,1]
Observations	1,080	1,080	1,080	1,080

Note: **p*<0.1; ***p*<0.05; ****p*<0.01

Table A2: Balance Table. The charges (violent first charge and all of the categorical charges) correspond to the first charge listed for each inmate in the respective prison's register. Estimated by OLS with heteroskedasticity-robust standard errors in parentheses.

Missing during Baseline Data Collection					
	(1)	(2)	(3)	(4)	(5)
Order Quantile by Prison	-0.018 (0.043)	-0.012 (0.043)	-0.031 (0.052)	-0.000 (0.156)	0.054 (0.087)
Quintet FE		✓	✓	✓	✓
Subsample	All	All	Prison 1	Prison 2	Prison 3
Proportion Missing	0.191	0.191	0.211	0.08	0.143
DV scale	0,1	0,1	0,1	0,1	
Observations	1,080	1,080	800	50	230

Note: *p<0.1; **p<0.05; ***p<0.01

Table A3: Effect of the order indicator (treatment assignment) on baseline missingness. Estimated by OLS with heteroskedasticity-robust standard errors in parentheses.

3. *Case proceedings in a different jurisdiction.* There is some transfer of prisoners between prisons in different jurisdictions. Because the legal assistance program worked in only two jurisdictions, they were unable to provide legal assistance in cases from other jurisdictions.
4. *Detainee represented by a private lawyer.*
5. *Detainee represented by a legal aid program from a different aid organization/NGO.*
6. *Detainee is not in prolonged pretrial detention.* If a detainee had already been convicted, acquitted, or released, she is not considered to be in pretrial detention. These cases of non-compliance were caused by inaccurate prison records or case progress between collection of prison records and assignment to legal assistance.
7. *Detainee not found in court records or in prison.* There were two names that were only located in prison registers. We do not know if the names were inaccurate or falsified.
8. *Detainee died in prison.* Two individuals died in prison between collection of records and their assignment to treatment.

The following table documents the frequency of each reason for non-compliance.

Reason for noncompliance	Frequency
Accused of ineligible infractions (rape, drug trafficking, human trafficking)	18
Case has multiple defendants	52
Case proceedings in a different jurisdiction	24
Detainee represented by private lawyer	15
Detainee represented by different aid organization/NGO	4
Detainee already judged or liberated	40
Detainee not found in prison or in court records	2
Detainee died in prison	2
Total	157

Table A5: Reasons for non-compliance and the frequency of occurrence among those assigned to treatment.

Note that with the rich case data that we collected on detainees assigned to treatment, there are alternate definitions of what constitutes compliance. In Table A6, we demonstrate that our inferences are robust to redefining the endogenous treatment in different ways. While the point estimates depend on the scale of the treatment and the covariance between the instrument and treatment, this analysis suggests that our inferences are robust to redefining compliance.

Recognizes Legal Assistance Program by Name				
	(1)	(2)	(3)	(4)
PANEL A: ITT EFFECTS WITH CONTINUOUS TREATMENT ASSIGNMENT				
Order Quantile within Prison	0.353*** (0.049)	0.403*** (0.058)	0.292*** (0.062)	0.809*** (0.139)
Mean of DV, Quantile = 0	-0.015			
Treatment Range	[0,1]	[0,1]	[0,1]	[0,1]
Subsample	All	All	J1	J2
Observations	638	638	494	144
PANEL B: ITT EFFECTS AND EVB WITH BINARY TREATMENT ASSIGNMENT				
Treatment Assigned (Binary)	0.244*** (0.030)	0.267*** (0.032)	0.208*** (0.036)	0.521*** (0.065)
Mean of DV, Treatment = 0	0.048			
Subsample	P1, P3	P1, P3	P1	P3
Treatment Range	{0,1}	{0,1}	{0,1}	{0,1}
Extreme Value Bounds	[-0.073, 0.432]	[-0.046, 0.437]	[-0.122, 0.391]	[0.211, 0.59]
Extreme Value Bounds CI	(-0.133, 0.487)	(-0.11, 0.491)	(-0.192, 0.453)	(0.069, 0.696)
Observations	611	611	467	144
Quintet FE		✓	✓	✓
IPW	✓			
DV Scale	{0,1}	{0,1}	{0,1}	{0,1}

Note: *p<0.1; **p<0.05; ***p<0.01

Table A4: Intent to Treat (ITT) effects of the probability that a respondent recalled meeting a lawyer from the legal assistance program. This serves as a manipulation check. In the second panel, the prison (P2) in which all detainees were assigned to treatment is dropped. Columns 3 and 4 correspond to the two jurisdictions in which legal assistance was provided. Heteroskedasticity robust standard errors in parentheses in panels 1 and 2. The third panel reports extreme value bounds (EVB) on the models from the second panel with 95% confidence intervals calculated off of heteroskedasticity-robust standard errors.

PANEL A: LATE (MARGINAL EFFECT) OF EACH WEEK OF TREATMENT				
Weeks of Legal Assistance (Standard)	0.051*** (0.015)	0.068*** (0.025)	0.086*** (0.031)	0.037 (0.041)
First-Stage <i>F</i> statistic:	73.822	44.006	23.783	23.288
Treatment Range	[0,10]	[0,10]	[0,10]	[0,10]
PANEL B: LATE (MARGINAL EFFECT) OF EACH WEEK OF TREATMENT (ALTERNATE MEASURE)				
Weeks of Legal Assistance (Alternate)	0.046*** (0.013)	0.055*** (0.020)	0.056*** (0.019)	0.054 (0.062)
First-Stage <i>F</i> statistic:	96.388	73.028	62.884	11.65
Treatment Range	[0,10]	[0,10]	[0,10]	[0,10]
PANEL C: LATE (MARGINAL EFFECT) OF EACH DAY OF ASSIGNMENT				
Weeks of Legal Assistance (Standard)	0.004*** (0.001)	0.004*** (0.002)	0.005*** (0.002)	0.003 (0.003)
First-Stage <i>F</i> statistic:	3215.919	2853.299	1572.04	2180.176
Treatment Range	[1,63]	[1,63]	[1,63]	[1,63]
Subsample	All	All	J1	J2
IPW	✓			
Quintet FE		✓	✓	✓
Instrument Range	[0,1]	[0,1]	[0,1]	[0,1]
DV Scale	{0,1,2}	{0,1,2}	{0,1,2}	{0,1,2}
Pre-Registered hypothesis test	Upper	Upper	Upper	Upper
Observations	503	503	371	132

Note: *p<0.1; **p<0.05; ***p<0.01

Table A6: Panel A replicates Table 1. Panel B, Panels B and C analyze different LATEs using alternate definitions of

A8 How many detainees could have been released?

In order to contextualize the magnitude our findings on release from detention, we consult the criminal code to determine how many detainees could have been released from detention during the period of study. We proceed in two steps. First, assume that all detainees are guilty and would be convicted if tried. In accordance with the Haitian Penal Code, we identify a set of offenses for which maximum statutory punishments are less (on paper) than some detainees had served in pretrial detention. As per *Loi Lespinasse*, such detainees should be given credit for time served and their sentences would expire when brought before the appropriate authorities. We focus on four offenses:

Charge	Clarification	Article	Sentence Duration
Theft	“Ordinary” theft; not armed robbery, theft by a domestic employee, or theft by a hotelier etc.	330	3 months-3 years
Fraud	General fraud or use of counterfeit	337	1 month-3 years
Bigamy	Not specified in criminal code	N/A	—
Abuse of Confidence	Not committed by public servant	338	2 months-2 years
Vagrancy	Over age 18	227-3	1-6 months
Begging		227-6	6 days-6 months

Table A7: Charges in the prison register with short sentences as per the Haitian Penal Code. The Article number refers to the article of the Haitian Penal Code.

Note that the crime of “criminal association” is used broadly. The criminal code does not specify a specific sentence for criminal association, but these sentences should be relatively short absent other offenses, given descriptions of what constitutes criminal association. In the following table, we present the proportion of detainees charged with the crimes in Table A7 (Column 3), the proportion accused of these crimes that would exceed the maximum sentence at endline (Column 4), the proportion of detainees held for criminal association (Column 5), and the proportion held for more than two years at endline (Column 6), and the proportion held for more than three years at endline (Column 7). These data come from baseline prison registers, not court records.

Prison	Proportion	No Charge	Charges in Table A7		Criminal Association		
		Share Detained	Share Detained	Share Detained beyond Max.	Share Detained	Share Detained ≥ 2 years	Share Detained ≥ 3 years
1	0.721	0	0.353	0.139	0.132	0.113	0.081
2	0.053	0.022	0.391	0.087	0.087	0.065	0.022
3	0.226	0.121	0.398	0.061	0.226	0.056	0.015
All	(1)	0.028	0.366	0.088	0.154	0.098	0.072

Table A8: Share of detainees by charge and prison. The column “Share Detained beyond [statutory] Max.” is the most conservative estimate of the share that should have been released by the end of the intervention if we consider all detainees guilty.

If we make the stark assumption that all detainees are guilty, the smallest proportion that should be eligible for release at endline is given in Column 4 of A8, or about 8.8% of detainees. Summing the detainees for whom there is no ostensible charge recorded in prison registers and those accused of criminal association that are held for more than 3 years, this share increases to 18.8%.

An alternative approach is to consider cases that fall into three categories:

1. Accused detainees that would be ruled guilty who have served their maximum sentence as pretrial detainees
2. Accused detainees that would be ruled guilty who have served the term that they would be sentenced to (if not maximum)
3. Accused detainees that would be acquitted if tried

4. Accused detainees for whom there is insufficient evidence (e.g. no witnesses) to continue to prosecute the case

The previous calculation characterizes the first category from the prison registers. Among the 91.2% of detainees that had not already served the maximum sentence, one could provide an estimate (albeit arbitrary) of the share of detainees that fall into the second, third, and fourth categories, q . Without discriminating by type of case or jurisdiction, the share of detainees that could have been released is:

$$0.088 + 0.912q$$

Our best guess is that $q \approx .2$, given the backlog of cases that can be processed in a trial (a prerequisite for #2 and #3). Setting $q = .2$, we would estimate that 27.4% of detainees could have been released during the experiment.

A9 Change of President and Rate of Case Advancement

We exploit an exogenous change of president during the implementation of the experiment to show that:

1. The operation of the aid-funded legal assistance program was maintained under both presidents (political principals).
2. The effects of the program do not appear to depend on the identity of the political principal.

We construct an indicator for whether a case was initiated under the president (Martelly, days 1-22) the interim president (Privert, days 23-63). Note that because the cases assigned to a longer period of legal assistance (e.g. those initiated under Martelly) are more likely to advance, the relevant comparison are the slopes of the quantile treatment indicator. We report our findings in Table A9. While “Interim president” is simply a pre/post indicator that could covary with other things, we do not find robust evidence that cases advance faster under either administration.

	Case Advancement during Intervention Period (Count)			
	(1)	(2)	(3)	(4)
Order Quantile among Treated	0.105 (0.192)	0.262 (0.275)	0.460 (0.390)	0.116 (0.403)
Interim President	-0.142 (0.142)	-0.066 (0.168)	-0.051 (0.201)	-0.057 (0.346)
Order Quantile * Interim President	0.320 (0.274)	0.340 (0.304)	0.687** (0.340)	0.118 (0.599)
IPW	✓			
Quintet FE		✓	✓	✓
Subsample	All	All	J1	J2
Treatment Range	[0,1]	[0,1]	[0,1]	[0,1]
DV Scale	{0,1,2}	{0,1,2}	{0,1,2}	{0,1,2}
Observations	503	503	371	132

Note: *p<0.1; **p<0.05; ***p<0.01

Table A9: Analysis of heterogeneous effects on case advancement (ITTs) by president. All specifications use OLS. This test was not preregistered; all p -values are two-tailed.

A10 Heterogeneity in Case Advancement

A10.1 Original Agent with Case File

There are three categories as to where the case was found originally by the legal assistance team:

- In the Parquet, in the files of a prosecutor, the chief prosecutor, or the relevant clerk. $n = 112$
- In the Court, with a judge, an investigative judge, the chief judge, or a relevant clerk $n = 233$
- Other: In another jurisdiction, convicted, liberated, dead, or files never arrived in the office of the Parquet or Court. $n = 158$

	Case Advancement during Intervention Period (Count)			
	(1)	(2)	(3)	(4)
Order Quantile among Treated	0.052 (0.063)	0.047 (0.130)	0.102 (0.167)	-0.076 (0.235)
Case in Court	0.117* (0.063)	0.140 (0.124)	0.134 (0.147)	0.142 (0.239)
Case in Parquet	-0.042 (0.080)	-0.007 (0.152)	-0.044 (0.147)	0.098 (0.330)
Order Quantile * Case in Court	0.183 (0.125)	0.228 (0.209)	0.116 (0.260)	0.578 (0.379)
Order Quantile * Case in Parquet	0.332* (0.188)	0.344 (0.289)	0.469 (0.319)	-0.030 (0.566)
IPW	✓			
Quintet FE		✓	✓	✓
H_0 : Court = Parquet, p -value	0.402	0.65	0.231	0.245
Subsample	All	All	J1	J2
Treatment Range	[0,1]	[0,1]	[0,1]	[0,1]
DV Scale	{0,1,2}	{0,1,2}	{0,1,2}	{0,1,2}
Observations	503	503	371	132

Note:

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A10: Conditional ITT effects of the dosage of treatment on procedural advancement, on the basis of the agent with the case at the start of the intervention. The base category is “Other.” The p -value on the H_0 that the rate of advancement in the court and the rate of advancement in the parquet are equivalent is reported. Heteroskedasticity-robust standard errors in parentheses.

A10.2 Previous Case Advancement

We record whether there is a record of case advancement since arrest prior to the intervention period. This record comes from prison files, court files, and any documents the detainee has received. While there is suggestive evidence that cases that advanced prior to the intervention advance at faster rates, perhaps due to increased legibility, these differences are not statistically significant at conventional thresholds.

- No record of previous case advancement $n = 302$
- Record of previous case advancement $n = 201$

	Case Advancement during Intervention Period (Count)			
	(1)	(2)	(3)	(4)
Order Quantile among Treated	0.166** (0.084)	0.137 (0.116)	0.155 (0.122)	0.075 (0.277)
Pre-Treatment Advancement Indicator	-0.036 (0.067)	-0.039 (0.116)	-0.063 (0.126)	-0.013 (0.241)
Order Quantile * Pre-Treatment Advancement	0.201 (0.149)	0.285 (0.220)	0.362 (0.263)	0.183 (0.414)
IPW	✓			
Quintet FE		✓	✓	✓
Subsample	All	All	J1	J2
Treatment Range	[0,1]	[0,1]	[0,1]	[0,1]
DV Scale	{0,1,2}	{0,1,2}	{0,1,2}	{0,1,2}
Observations	503	503	371	132

Note: *p<0.1; **p<0.05; ***p<0.01

Table A11: Conditional ITT effects of the dosage of treatment on procedural advancement, on the basis of previous (pretreatment) case advancement. Heteroskedasticity-robust standard errors in parentheses.

A11 Liberation During Intervention Period

In the main paper, we present analysis of liberation at the time of the endline survey. Here we consider if individuals had been related by the end of the intervention.

	Liberation during Intervention Period			
	(1)	(2)	(3)	(4)
PANEL A: ITT EFFECTS WITH CONTINUOUS TREATMENT ASSIGNMENT				
Order Quantile among Treated	-0.021* (0.012)	-0.037 (0.025)	-0.016 (0.020)	-0.086 (0.068)
Mean of DV, Treatment = 0	0.022			
Treatment Range	[0,1]	[0,1]	[0,1]	[0,1]
PANEL B: LATE (MARGINAL EFFECT) OF EACH WEEK OF TREATMENT				
Weeks of Treatment	-0.004* (0.002)	-0.008 (0.006)	-0.003 (0.004)	-0.027 (0.023)
Mean of DV, Treatment = 0	0.010			
First-Stage <i>F</i> statistic:	96.388	73.028	62.884	11.65
Instrument Range	[0,1]	[0,1]	[0,1]	[0,1]
Treatment Range	[0,10]	[0,10]	[0,10]	[0,10]
Subsample	All	All	J1	J2
IPW	✓			
Quintet FE		✓	✓	✓
DV Scale	{0,1,2}	{0,1,2}	{0,1,2}	{0,1,2}
Observations	503	503	371	132

Note: *p<0.1; **p<0.05; ***p<0.01

Table A12: ITT effects of the dosage of treatment on liberation measured at the conclusion the 10-week intervention period. The top panel utilizes the quantile measure measure of the order of treatment assignment using OLS. The bottom panel uses this quantile measure to instrument for the number days of treatment *assigned* to a detainee, regardless of compliance. Heteroskedasticity-robust standard errors in parentheses.

A12 No Evidence of Congestion

We examine the advancement of cases within the first 15 or 30 days of treatment to test for evidence of congestion in the courts. Congestion would ostensibly mean that cases assigned to treatment later are less likely to advance within the first days of treatment than earlier cases. In the following tables, congestion of this form would be represented by a *negative* coefficient any of the three operationalizations of treatment assignment. We do not find any evidence of congestion in Table A13.

	Case Advancement within 15 days				Case Advancement within 30 days			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: QUANTILE AMONG THOSE ASSIGNED TO TREATMENT								
Order Quantile Among Assigned	-0.012 (0.024)	0.002 (0.040)	-0.004 (0.034)	0.017 (0.104)	0.017 (0.040)	0.015 (0.053)	0.014 (0.055)	0.017 (0.117)
Mean DV, Quantile = 0	0.05				0.093			
PANEL B: BINARY ASSIGNMENT INDICATOR (<32 vs. 32-63 DAYS)								
Assigned, Days 32-63	0.001 (0.019)	-0.010 (0.025)	-0.009 (0.019)	-0.011 (0.062)	-0.022 (0.028)	-0.025 (0.032)	-0.026 (0.030)	-0.023 (0.073)
Mean DV, Quantile = 0	0.043				0.109			
PANEL C: TERNARY ASSIGNMENT INDICATOR (<23, 23-42, AND >42 DAYS)								
Assigned, Days 23-42	0.037 (0.025)	0.007 (0.027)	0.042* (0.024)	-0.057 (0.067)	0.036 (0.034)	0.018 (0.036)	0.042 (0.042)	-0.015 (0.072)
Assigned, Days 43-62	0.010 (0.020)	-0.006 (0.030)	-0.024 (0.024)	-0.010 (0.071)	-0.013 (0.031)	-0.026 (0.039)	-0.049 (0.038)	-0.007 (0.084)
Mean DV, Quantile = 0	0.031				0.095			
IPW	✓				✓			
Quintet FE		✓	✓	✓		✓	✓	✓
Subsample	All	All	J1	J2	All	All	J1	J2
Observations	503	503	371	132	503	503	371	132

Note:

*p<0.1; **p<0.05; ***p<0.01

Table A13: Case advancement within 15 or 30 days depending on date of assignment to treatment. This test was not preregistered; all *p*-values are two-tailed.

Supplementary Appendix: References

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