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

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Abstract: *Existing explanations of human rights abuses emphasize a strategic logic of repression. Yet certain classes of abuses may arise absent the intent to repress because of the misaligned bureaucratic incentives of state agents. To separate accounts of strategic repression from bureaucratic incentives, we study the responses of state agents working within the Haitian criminal justice system to a randomized, free legal assistance intervention for detainees held in illegal pretrial detention. Legal assistance addresses moral hazard problems of the bureaucrats responsible for processing cases. We demonstrate that legal assistance accelerates case advancement and liberation, in line with the view that large-scale human rights abuses in the justice system can result from poor governance and not repressive intent.*

Verification Materials: The data and materials required to verify the computational reproducibility of the results, procedures, and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/Q5PV4U.

The "cogs in the machine" defense has prominently been invoked by those tried for human rights abuses and war crimes. Normative political theory analyzes these arguments at length, drawing seminally from Arendt's (1963) characterization of the "banality of evil." Critical to normative considerations is the question of the extent to which "cogs" or the "machine" can promote or undermine the objectives of a political principal who seeks to repress. We ask, instead, how widespread human rights abuses occur absent this intent to repress.

We make two arguments. First, we establish the conditions for the case in which banality alone—absent evil—undermines the protection of rights. Second, we document that these types of abuses, which occur absent intent, are understudied by political scientists even though they may account for a substantial amount of human rights abuses globally. Specifically, we study how the incentives of agents—here, state employees—and the structure of state institutions influence the protection of rights under different government objectives. Consider the role of these employees in a simplified account of *intended* repression. We begin with the standard definition of repression, which is the implementation of government policies that violate physical integrity rights.[1] In this case, a principal orders agents to repress and agents decide whether to exert effort to carry out the directive. The outcome—repression—results from the agent's decision to carry out orders or "work."

In the case of *misgovernance*, we consider an agent tasked with ensuring that rights are upheld (e.g., by ensuring a timely trial) who chooses not to exert effort. In contrast to the case of intended repression, a qualitatively

[1] Physical integrity rights include freedom from arbitrary arrest or imprisonment, ill treatment and torture, extrajudicial executions, mass killings, and disappearances (e.g., Davenport 2007).

similar abrogation of rights occurs precisely because the agent decided to "shirk" or not carry out her orders. Consistent with the concept of misgovernance, this form of abuse occurs when the political principal or government is welfare oriented (Banerjee 1997)—in this case, not pursuing policies of repression. Though there is no directive to repress, systematic violations of rights, including physical integrity rights, may arise in certain domains. In this scenario, rights abuses occur because the agent does *not* do her job.

Though agency problems in repression are increasingly explored,[2] a comparison with a logic of misgovernance illuminates several departures from existing accounts of agency and human rights. First, theories of intended repression are premised on an executive's (the principal's) choice to employ repression. Principals typically opt to employ repression to remain in power. In most models, if the principal did not set a policy of repression or contract her security apparatus to repress, repression would not occur in equilibrium (Dragu and Lupu 2018; Dragu and Polborn 2013; Dragu and Przeworski 2019; Tyson 2018). Indeed, unless agents value implementing repression for noninstrumental reasons, as implied by Conrad and Moore (2010), these arguments do not predict rights abuses in the absence of a principal's directives.

Second, we expand focus to agents charged with preserving rights, including those outside a regime's security forces. In contrast to security forces whose considerations include regime survival and future influence (Geddes, Frantz, and Wright 2014; Hassan 2017; Myerson 2008; Svolik 2012; Wintrobe 2012), we argue that more quotidian agency problems across regime types are responsible for a number of frequent but understudied human rights violations. By examining these agency problems within a large subset of bureaucrats tasked with upholding rights, we elucidate a mechanism linking limited state capacity to human rights noncompliance (Chayes and Chayes 1993).

In this article, we consider the logic of misgovernance in the context of prolonged pretrial detention. Illegal prolonged pretrial detention occurs when the detention of the accused overruns constitutional or statutory time limits. In 186 countries that provide data on imprisonment, there are nearly three million individuals held in pretrial detention (Institute for Criminal Policy Research 2017). Though not all pretrial detainees are *illegally* imprisoned, we estimate the number of illegal pretrial detainees under different assumptions about the share of pretrial detainees (see Appendix 1 in the supporting information [SI]). Our best approximation suggests that there are over one million such detainees. While systematic measures of illegal

[2]See Mitchell (2004) for a seminal contribution.

detention of political prisoners are not available cross-nationally, within-country comparisons suggest that pretrial detention dwarfs the population of political prisoners by several orders of magnitude. Ironically, illegal pretrial detention is not included in standard measures of cross-national human rights data.

To provide evidence in favor of a misgovernance account of human rights noncompliance in the context of prolonged pretrial detention, we argue that a principal—the government—should not be pursuing a purposeful strategy of (illegal) detention. Instead, moral hazard problems of the agents responsible for investigating and trying the accused hinder compliance with constitutional time limits on pretrial detention. The shirking of individual agents may be exacerbated by the institutional structure of the justice system.

We analyze these dynamics from Haiti, where illegal pretrial detention beyond time limits specified by the Haitian Constitution represents a persistent form of human rights abuse. To establish the Haitian government's objective with respect to pretrial detention, we draw upon 88 qualitative interviews with current and former justice system officials, lawyers, and rights advocates and an original descriptive data set on the detained. We find no evidence indicating intent to repress. We then develop a novel, large-N randomized rollout experiment that tests the effect of free legal assistance on pretrial detainees' case trajectories. The intervention targets bureaucrats working within prosecutors' offices (*parquets*) and courts. Legal assistance should accelerate case advancement and reduce pretrial detention by (a) reducing agents' marginal cost of effort to process cases and (b) alleviating failures to coordinate between judicial institutions. This treatment thus allows us to assess the degree to which case outcomes respond to bureaucratic behavior, testing our argument that this form of illegal imprisonment occurs due to bureaucrats' (in)action within these institutions.

The randomized rollout experimental design allows us to leverage variation in both the presence and dosage (duration) of the legal assistance treatment to examine their effects on case advancement and release from pretrial detention. We also find that legal assistance increases the probability that individuals exit prolonged pretrial detention within 9 months. These results show that justice system capacity and, consequently, rights compliance are undermined by the actions of bureaucrats. Further, our results imply that compliance can be improved by interventions targeting agents rather than principals.

Our theory and evidence contribute to several literatures beyond human rights. First, our theory and research design open the "black box" of state capacity. Conceptually, a state's capacity to implement laws, and thus protect

rights, represents some function of the quantity of state agents, the quality of agents, and agents' actions.[3] Empirically, disentangling these components of capacity is difficult (Berwick and Christia 2018); yet, policy implications depend on the sources of limited capacity. We present novel measurement of agents' actions, arguably the hardest input to measure, complementing recent analysis of agent quality in the domain of state-sanctioned repression (Scarpf and Gläßel forthcoming) and policing (Goncalves and Mello 2018). Second, our evidence on how agents' behavior within justice system institutions conditions justice system outcomes contributes a mechanism to enduring debates on the effects of justice system design on outcomes (e.g., La Porta et al. 1998). Our characterization of pretrial detention rationalizes observations on the effects of legal reform (Cavise 2013; Langer 2007) and describes the distributive consequences of backlogs in justice systems (Chemin 2009; Dixit 2004).

Theoretical Framework
How Do Abuses Emerge?

We develop a theoretical framework linking misgovernance and human rights performance. In the seminal account of misgovernance, Banerjee (1997, 1289) argues that one must "posit the existence of a welfare-oriented constituency within the government in order to explain red tape and corruption." The distinction between the government's objective implied by a misgovernance account and a repression-centered account of why human rights violations occur serves as the critical distinction between our argument and theories of repression. We do not posit that repression does not occur; there exists substantial evidence that repression is widespread. Instead, we suggest that a class of rights abuses occur even under a (relatively) benevolent government and are not well explained by existing theory.

Assuming that a welfare-oriented political principal exists, how do rights abuses emerge? In the case of criminal justice, the responsibility to protect both the accused and victims is delegated to a set of state officials working in formal judicial institutions. The procedures required to process a case are remarkably similar across different settings and comparatively complex to actuate (Glaeser and Schleifer 2002). The task of advancing a criminal case requires substantial effort as well as the participation of multiple agents, possibly within different entities. When resource constraints lead to insufficient staffing levels or existing state agents shirk, these features impose barriers to the protection of the rights of the accused.

Within the criminal justice setting, agents face weak incentives to exert the effort required to advance cases. The *right* to an efficient trial implies that the price collected by the bureaucrat for serving a detainee "by the books" is effectively zero for all detainees. In contrast to models of service provision, the agent's task is explicitly not to screen applicants or allocate services differentially among types of applicants (Banerjee 1997; Ting 2017). This setting gives rise to two pathologies. First, if agents do not value outputs (case outcomes), in a public-sector setting with low-powered incentives, agents are prone to shirk. Second, whereas a piece rate price for processing a case is not present when these services are a right, agents may circumvent "by the books" processes, accepting side payments to advance cases around the state. Lack or diversion of effort by bureaucrats reduces the rate of case advancement. Thus, in contrast to the adverse selection (hidden information) problems emphasized in existing accounts of misgovernance, we focus on moral hazard problems (hidden action) within the justice sector.

The structure of criminal justice institutions further limits the rate at which cases can reach final disposition. Case backlogs—induced by shirking agents or simply a lack of personnel—accrue in both the parquet (prosecutor's office) and the court. To reach a final disposition via dismissal or trial, cases must pass between these institutional entities multiple times. Within two offices administered by separate (immediate) principals and characterized by limited transfer of information, failure to coordinate reduces the rate at which cases advance through both entities.[4] Although agents could advance a larger number of cases to a final disposition by focusing on the same subset of cases across the parquet and court, limited information transfer precludes this strategy.

Limited information transfer between institutional entities implies that monitoring or oversight of case outcomes and compliance with human rights protections is costly. Detecting shirking is more difficult when tasks are delegated to multiple agents. Moreover, the possible consequences of lack of case advancement are shared across the two justice-sector institutions, which reduces the overall probability and severity of sanctions faced by an individual judge or prosecutor for not exerting effort on a particular case. Thus, the moral hazard problems

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[3]Our concept of capacity draws upon Soifer's (2008) "national capability" refinement of Mann's (1984) concept of "infrastructural power."

[4]Our description of "failure to coordinate" is distinct from coordination failures in game theory. When agents do not value outputs, this is not a coordination game.

that we identify are further exacerbated by the institutional structure of courts.

The Role of Legal Assistance

We aim to understand this misgovernance equilibrium as it pertains to prolonged pretrial detention. We use a legal assistance intervention as a "shock" to some cases (tasks) to generate evidence that moral hazard problems of bureaucrats induce such abuses. We argue that legal assistance accelerates the advancement and final disposition of cases through two institutional mechanisms. First, legal assistance reduces the marginal cost of effort required to process a case. Second, legal assistance induces higher levels of positive correlation in the outputs of the parquet and court. To understand how these two institutional channels function together, we develop a simple model of how legal assistance affects case outcomes and derive our hypotheses formally in SI Appendix 2. The model is an adaptation of a standard two-task contract, where tasks are represented and unrepresented cases (Bolton and Dewatripont 2005; Holmstrom and Milgrom 1991). Empirically, the random assignment of legal assistance implies random assignment of the types of tasks.

The first channel through which legal assistance accelerates case advancement is a reduction in the marginal cost of effort required to process a case. By locating a case file and identifying the next procedural step, lawyers minimize the cost incurred by a prosecutor or an investigative judge to advance the case. Cynically, the lawyers do the first portion of the agent's job to process a case. These efforts include finding a case among hundreds or thousands of paper case files, identifying the procedural state of a case, and locating the defendant. However, ultimate advancement requires court officials to take positive action.[5]

A reduction in the cost of effort of processing a case induced by legal assistance increases equilibrium effort. Two observable implications from this formulation allow us to test whether the actions of state officials are sensitive to this reduction in cost. First, we hypothesize that detainees' cases that are subject to legal assistance should advance procedurally with higher probability than detainees' cases that are not subject to legal assistance. Second, we hypothesize that the rate of procedural advancement after the imposition of legal assistance should exceed the rate of procedural advancement before the imposition of legal assistance.

The distributional impacts of reducing the costs of effort for some detainees' cases but not others depend critically on assumptions about the substitutability of effort on represented versus unrepresented cases. Furthermore, the possibility of expending more effort depends on the degree to which court officials are working to capacity absent legal assistance. Nevertheless, predictions are unambiguous: A reduction in the marginal cost of effort should increase the probability of case advancement. We present the empirical evidence in this regard in subsequent discussion.

The second channel through which legal assistance accelerates case advancement is by exogenously inducing higher levels of coordination between agents working on cases in the parquet and court. That is, the work accomplished in one entity is communicated to and informs the agenda of the other. In the present setting with large caseloads and backlogs—whether due to lack of staffing, limited effort, or some combination—a case that advances through one agent may be lost or otherwise backlogged among the cases of the second agent. By communicating the progress and status of a case, the lawyer brings a case file to the attention of the prosecutor or judge. Because the lawyer needs the legal input of effort from both the prosecutor and judge, coordination on the case (positive correlation in the legal outputs) should occur between the two institutional entities.

Higher correlation in outputs ensures that a larger share of cases clears both the parquet and court. Through this mechanism, legal assistance, in terms of both duration or dosage, alleviates the severity of the two institutions' failure to coordinate. Notably, the ability of lawyers to track a case from one office to the other requires a sufficient period of time (dosage) of legal assistance. Finally, we hypothesize that cases assigned to (higher dosages of) legal assistance are more likely to reach a final disposition within the period of the study.

The two conceptual mechanisms are complements. As such, we cannot separate them empirically. We limit our discussion to partial equilibrium effects of legal assistance, assuming that the Ministry of Justice does not respond to legal assistance by changing contracts or its monitoring of agents, at least within the relevant time frame.

Prolonged Pretrial Detention in Haiti

Prolonged pretrial detention represents one manifestation of problems within Haiti's judicial institutions.

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[5]In the model, lack of effort incurs sanctions probabilistically; the possibility of sanction is necessary to observe nonzero effort. Legal assistance could also increase the likelihood of exposure to sanctions by providing more information to the principal about low effort or corruption. We describe this alternative mechanism in more detail in a later subsection, "Corruption by Agents.".

For at least a decade, Haiti has experienced staggering sustained levels of prolonged pretrial detention (see SI Figure A7). While this does not provide the proportion held in *prolonged* pretrial detention, the vast majority of these pretrial detainees are known to be illegally detained. The abuses implied by this level of prolonged pretrial detention are exacerbated by the poor state of Haiti's prisons. These facilities are overcrowded, often exceeding intended capacity by a factor of 5 or 6, with poor sanitation, insufficient nutrition, and communicable disease.

The Haitian Constitution outlines the procedure for penal and correctional cases, with maximum allowances of time for each step. For example, the Constitution requires that detainees receive a hearing within 48 hours of their arrest and be provided with a lawyer. The subsequent steps in criminal procedure have similarly specified time frames (3–6 months). These rights are frequently violated, particularly for prisoners who cannot afford a private attorney. In these cases, prisoners are in *prolonged* or *illegal* pretrial detention. Ostensibly due to resource constraints, the Haitian government does not presently employ any public defenders. Ironically, however, imprisonment is quite expensive: Housing a detainee for a year was estimated to cost approximately US$480, or 58% of Haiti's GDP per capita during the study.

The absence of state-sanctioned legal assistance has strong implications for the distributive consequences of pretrial detention. Two complementary means of avoiding such forms of prolonged detention include hiring a private attorney or bribing justice system agents.[6] These strategies are typically available to and used by anyone who can afford them. As such, the phenomenon of prolonged pretrial detention reflects Haiti's socioeconomic inequality. Paralleling findings of perverse consequences of long-term (legal) pretrial detention in the United States (Dobbie, Goldin, and Yang 2018; Leslie and Pope 2017), interviews with prison staff and lawyers indicate illegal detention increases exposure to gang recruitment (Jung and Cohen 2018) and recidivism; it also exposes nonincarcerated family members to extreme financial hardship. Given that poor Haitians are disproportionately exposed to pretrial detention, these practices likely magnify already extreme socioeconomic inequality (Lofstrom and Raphael 2016).

The Government's Objective

We first validate our assumption that the Haitian government (Haiti's president and the Minister of Justice)

does not strategically detain individuals for political or economic gain. We aim to deduce the government's objective from four sources of evidence.

First, we draw on information from the 2016 U.S. State Department report on Haiti, which finds that there were no reports of political prisoners or detainees or politically motivated disappearances.[7] The report focuses on the poor conditions and treatment of individuals who enter the judicial system (i.e., prolonged pretrial detention). There are allegations that some arrests are arbitrary in an effort to extort individuals. However, all allegations focus on low-level state agents as opposed to systematic political directives from above.

Second, we draw systematic evidence from 88 semistructured interviews conducted in 2015. Our subjects include current and former judges, prosecutors, and prison officials; defense lawyers and rights advocates; and program administrators in five of Haiti's 18 Courts of First Instance, as reported in SI Table A1. No interview subjects described Haiti's overflowing pretrial prison population as a symptom of "repression" or political coercion among current pretrial detainees. In a pool of subjects including reform advocates and critics of the current state of the judicial system, the unanimity of responses is highly suggestive evidence. Furthermore, given a common principal—the government of Haiti—and common directives, interviewees attributed variation in rates of pretrial detention (25% to 90% across five districts) to the behavior of agents administering and working in each court, not any action of the government.

Relatedly, the present legal assistance program was financed and administered by the United States Agency for International Development (USAID). The Haitian government allowed the present program to operate over a period of 7 years, with wide-ranging access to prisons and courts across the country. The degree of access and transparency afforded to donors appears inconsistent with a goal of widespread repression via pretrial detention.

Third, we assess the government's objective as an empirical question. SI Appendix 4 describes the profiles of detainees gathered from our surveys and pretreatment prison data. Consistent with studies of crime and violence, detainees are disproportionately poor young men. The most common criminal charges in our data are theft and criminal association; the relative frequency of charges does not vary substantially between the court and prison records (SI Figure A10). Detainees generally understand their charges. Their self-reports between baseline and endline are highly correlated; these

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[6]We have no evidence that bribes reach principals.

[7]These reports serve as the primary source for cross-national human rights data (Fariss 2014; Fariss and Dancy 2017).

self-reports also correlate positively with the two administrative data sources for all offenses (SI Figure A11). Furthermore, in an open-ended question at endline asking, "why do you think you are [imprisoned]," just one detainee (0.15%) suggested any political suspicion. Detainees spoke openly to enumerators during both surveys. The data provide no evidence that illegal pretrial detention is used or perceived as a form of state-sanctioned repression.

Finally, the timing of an unplanned transition to an interim president occurred during the rollout of the experiment varied the identity of the government. In SI Appendix 9, we document that the change in president did not change lawyers' access to the prisons or courts or the efficacy of the treatment.

Research Design

Ethical Considerations

The population of subjects in this experiment consists of detainees held illegally in three Haitian prisons. To ascertain the potential for unintended negative consequences of legal assistance, we assessed case files from existing legal assistance cases and conducted many interviews with stakeholders. Our exploratory work reached one central conclusion: Absent legal assistance, detainees are released from prison at a very slow rate. While there are some cases where legal assistance is not able to secure a release or conviction, there was no evidence that it worsened its clients' already bleak case trajectories.

Our randomized rollout design maximizes the number of treated individuals within the time constraints of the study while preserving the ethical and empirical benefits of random assignment. Consistent with the bioethics literature on the allocation of rare treatments, random assignment is a fair and efficient method of allocating treatments, particularly in settings of limited information (Persad, Wertheimer, and Emanuel 2009). During the implementation of the experiment, all subjects had the right to opt out of any portion of the evaluation, including the surveys or legal assistance.

Population and Sample

The population of interest includes individuals held in prolonged pretrial detention in prisons in the Port-au-Prince and Croix-des-Bouquets jurisdictions. When the list of these detainees was collected in November 2015, there were approximately 3,000 individuals in pretrial detention in the three relevant prisons. We sample from detainees who had spent at least 6 months in pretrial detention at the beginning of the intervention, indicating with certainty that procedural time limits had been overrun. Furthermore, as per donor guidelines, legal assistance was not offered to detainees accused of rape, human trafficking, or drug trafficking. Potential detainees for whom prison records include these charges were excluded from the experimental population.

Given resource constraints of the legal assistance program and the difficulties associated with collecting data for so many individuals given the state of records in Haiti's prisons and judicial system, we identified the population of participants meeting these eligibility conditions (N = 2,211). Stratifying by prison, we randomly selected 1,080 individuals from the prison lists of pretrial detainees meeting these eligibility criteria for inclusion in the experimental sample.

Treatment and Assignment

Treatment consisted of a legal assistance intervention constructed to resemble a public defender's office. The detainees in our sample were assigned to a lawyer. The lawyer's first actions were to identify and learn about the case. This consisted of meetings with a detainee in the prison and finding case records in the prosecutor's office or court. Once a case was found, the lawyer identified the next procedural step (typically to issue a legal document). The advocacy of the lawyers was directed to the assigned prosecutor or judge in possession of the case. Where these officials were absent or otherwise unresponsive, the lawyers appealed to the chief prosecutor or judge for reassignment to another judge, bringing an additional layer of scrutiny on the responsible bureaucrat.

The assignment of legal assistance to an individual case allows us to observe the effects of a shock to the incentives of court agents with respect to a specific task. We posit that these actions play two roles. First, gathering information about the status of a case from the detainee and court clerk clarifies what actions are needed to process a case, reducing the bureaucrat's marginal cost of effort. Second, the ability of lawyers to shepherd a case between prosecutors and judges mitigates "failures to coordinate" across the institutions.

The randomized rollout design maximizes the number of legal assistance beneficiaries in three prisons. The prison sample sizes are heterogeneous, with 800, 50, and 230 detainees from each prison, respectively. Since legal assistants proceeded through as many of the 1,080 cases as possible prior to the conclusion of the

FIGURE 1 Sampling and Assignment, by Prison
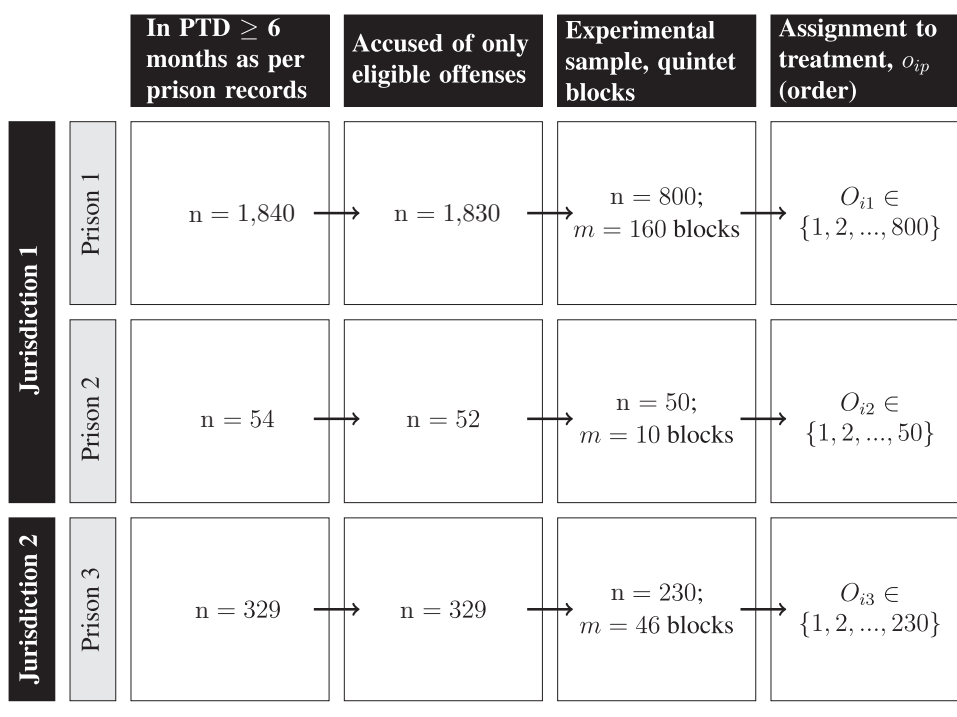
program, we used blocking to ensure that the randomization yields maximal heterogeneity among the full schedule of potential subjects. Each prison serves as a block. With each prison, we blocked detainees into "quintets" that minimize the multivariate distance on the age, duration of pretrial detention, violent infraction indicator, number of infractions, and education indicator. The three prisons thus contained $m = 160$, $m = 10$, and $m = 46$ blocks, respectively. The randomization ensures that one member of each quintet is included in each quintile of the order distribution, maximizing heterogeneity across the sample. Assignment is illustrated in Figure 1.

We check balance by regressing the order indicator on the pretreatment covariates from the prison register in SI Table A2. No evidence of imbalances in treatment status for any covariate exists; we fail to reject the null hypothesis for any coefficient estimate at the $\alpha = .05$ level across specifications and an F-test of the joint significance of the coefficients.

Given random assignment of the order of treatment, legal assistants provided services sequentially within each prison. During the 10 week period of legal assistance prior to the exogenous program end date, program lawyers attempted to provide legal assistance to the first k_p detainees on each prison's assignment, respectively.

Outcomes and Measurement

Our outcome data are collected from two principal sources, spanning both administrative and survey data at different points in time, as described in SI Appendix 5. First, we utilize the case files compiled by program attorneys during the course of the intervention to measure case characteristics and case advancement outcomes during the 3 month intervention and immediately afterward. These files also provide detailed records of lawyers' actions toward providing legal assistance.[8]

We analyze two primary outcomes of case advancement. First, we examine whether each treated case advanced procedurally through the Haitian criminal justice system during the experiment and in the 2 weeks following the cessation of legal assistance. Case advancement is coded from lawyers' case files and is documented with facsimiles of legal documents. Although we only have these records for treated detainees, we are able to make valid inferences about differences in the outcomes of these cases because of the randomly assigned order of treatment (differences in dosage).

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[8]Lawyers were not aware that this was an experiment. As such, we have no evidence that case files were inaccurately constructed or maintained.

Second, we measure whether cases reached their final disposition in the courts. In practice, this could be release (via acquittal or dropped charges) or conviction. Also, given extended periods of pretrial detention, conviction can result in the release of prisoners when they are credited for time served. Our main outcome—liberation after 9 months—is drawn from the "census" component of the baseline and endline surveys of detainees that sought to determine each subject's location. When enumerators could not locate a detainee in the prison, they consulted prison records codifying the detainee's status.[9]

Empirical Strategy

The randomized rollout design allows for several operationalizations of the treatment variable:

- *Quantile of order, by prison*: Given that the sample sizes by prison are heterogeneous, the order variable must be transformed onto a common scale. We code this indicator as $1 - q_p(o_{ip})$, where $q_p(\cdot)$ is the quantile function for a prison, p, and o_{ip} is the order indicator for an individual i in prison p.
- *Assignment to treatment (binary)*: Since the end date of legal assistance provision is exogenous and was not revealed to the lawyers in advance, we construct a binary treatment indicator. The detainees whose randomly assigned order was reached during the course of service provision are coded as assigned to treatment; if not, they are coded as assigned to control. Because all of the experimental sample in the smallest prison was assigned to treatment, this prison is dropped from analyses using this binary treatment indicator.

We estimate intent-to-treat (ITT) effects using ordinary least squares (OLS) estimators with and without quintet fixed effects. For case outcomes, we also disaggregate by the jurisdiction in which cases are located. There are two jurisdictions: one with two prisons and another with one prison. Our preanalysis plan distinguishes between one- and two-sided hypothesis tests. We report the tests implied by the preanalysis plan.

As predicted, not all detainees assigned to treatment were ultimately treated. In total, 157 of the 503 detainees assigned to treatment (at any nonzero dose) were not treated. SI Table A5 provides the frequency of each reason

[9]A team of enumerators sought to record all convictions in the courts during endline data collection. However, files of closed cases were not readily available, and attrition was substantial.

for noncompliance among individuals assigned to treatment. As such, we estimate both ITTs and local average treatment effects (LATEs).

In the context of our research design, compliance, like treatment assignment, can be operationalized in several different ways. Figure 2 provides a graphical description of treatment assignment and status for units within each prison. It suggests two straightforward operationalizations of treatment delivery. First, treatment delivery can be measured as a binary variable—whether or not an individual received legal assistance. The associated instrumental variables estimator with a binary treatment assignment instrument and binary treatment estimates the complier average causal effect (CACE). A second straightforward measure of compliance measures the number of days or weeks that a subject was "in treatment," utilizing the varied dosages of treatment to assess the marginal effect of a week in treatment.

We utilize two-stage least squares to estimate the LATEs of interest. This strategy is detailed in Equations (1) and (2). In the first stage, we regress a measure of compliance, D_{ib}, on a measure of treatment assignment, Z_i. The second stage regresses our case outcomes, Y_{ib}, on the predicted treatment, $\widehat{D_{ib}}$. Quintet blocks are indexed by b, and models are estimated with and without block fixed effects.

$$D_{ib} = \gamma_1 Z_i + \boldsymbol{\eta}_b + \boldsymbol{\epsilon}_i; \tag{1}$$

$$Y_{ib} = \beta_1 \widehat{D_{ib}} + \boldsymbol{\kappa}_b + \xi_i. \tag{2}$$

Results
Treatment Delivery and Manipulation Check

More than 2,000 pages of case records recorded by lawyers provide evidence that treatment was assigned, attempted, and delivered in the randomly assigned order. Figure 3 indicates that, on average, earlier assignment to treatment resulted in more interventions on behalf of a detainee. In the smallest prison, where treatment was delivered to all subjects within a month, the relationship is somewhat weaker. Nevertheless, the clear relationship between order of assignment and intensity of treatment forms the justification for analyses based on the dosage of treatment assigned. In a manipulation check, treated detainees were aware that they received legal assistance. SI Table A4 reports the ITT effect of treatment assignment on subjects' recollection of a visit by lawyers from the program. It demonstrates a positive, highly significant effect of

FIGURE 2 Treatment Assignment (Quantile) and Treatment Delivery
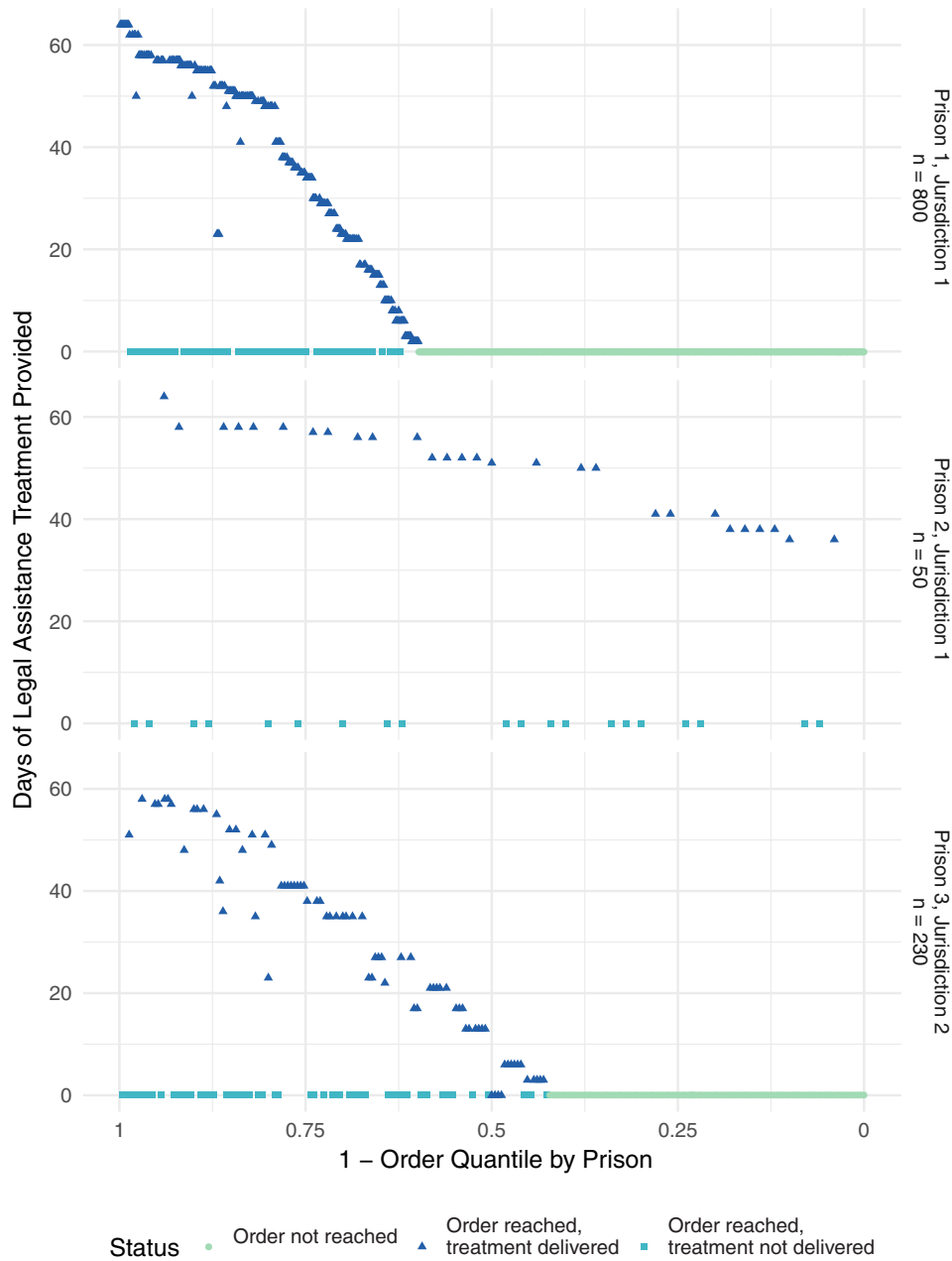
Note: Treatment delivery is measured in terms of the number of days of legal assistance. Noncompliance is one-sided.

assignment to a nonzero dosage of treatment on recollection of such a visit (lower panel).

Case Advancement Analysis

We first assess whether legal assistance accelerated the rate of case advancement by analyzing the case records of all detainees assigned to treatment. Because we do not have measurements of case advancement for those detainees whose cases were not examined by a lawyer, we rely on the randomized rollout of treatment over time to generate our counterfactual. Given the differing proportions of treated individuals in each prison, we operationalize treatment assignment as $1 - q_{ip}(o_{ip}|o_{ip} \leq k_p)$, where $q_{ip}(\cdot)$ is the quantile function and k_p is the order of

FIGURE 3 Earlier Treatment Assignment Corresponds to Higher Doses of Treatment (More Interventions on Behalf of the Detainee)
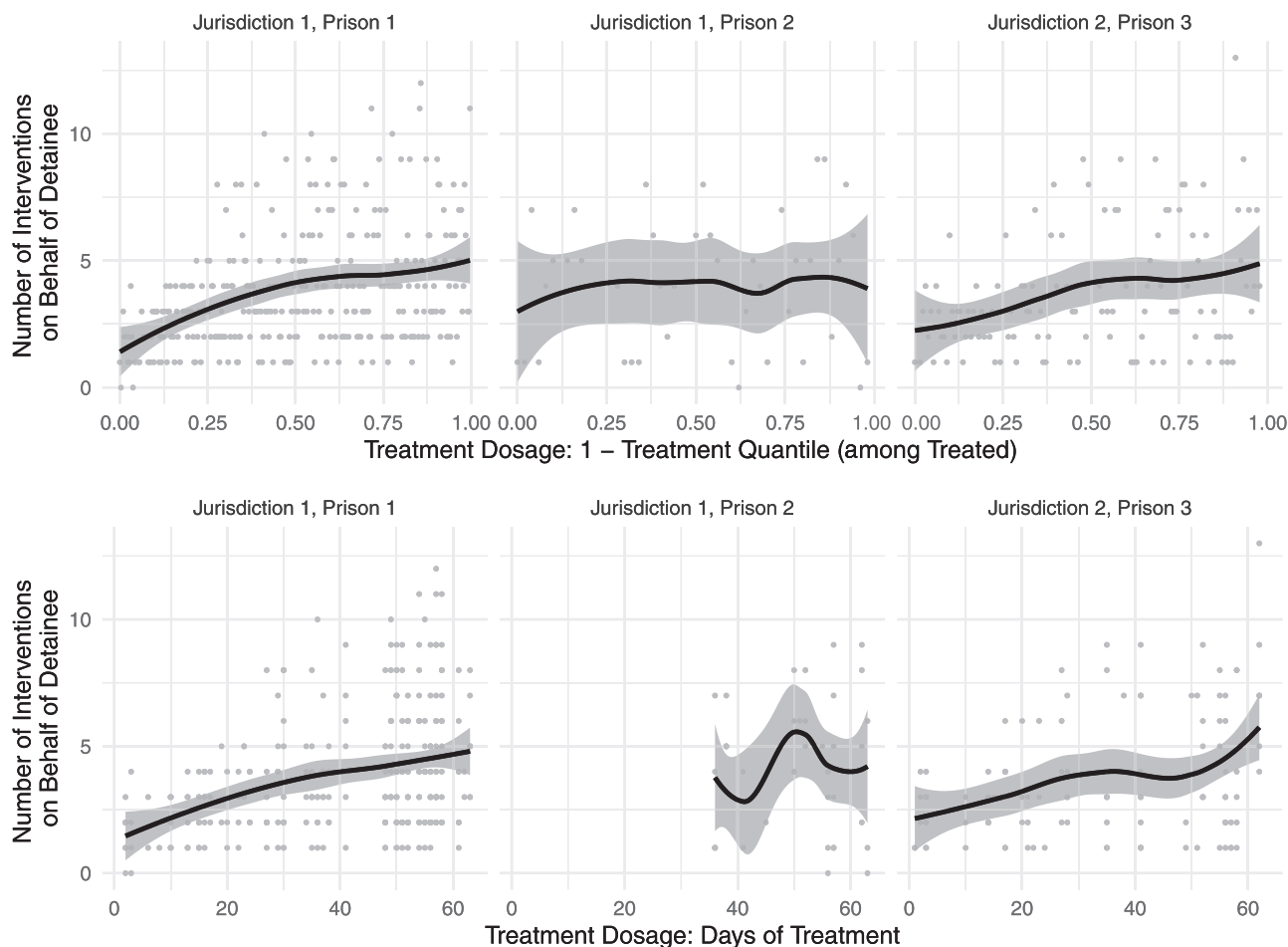
Note: The number of interventions is the sum of interventions in the prisons, parquets, and courts on behalf of a detainee as recorded in lawyers' case files.

the last detainee in a prison assigned to treatment.[10] We also instrument for the number of weeks that a detainee was assigned to treatment, to create a more interpretable treatment indicator.

We examine case advancement, defined as the issuance of a legal document by the court or parquet. The documents range from orders of extraction (from the prison) for questioning to orders of liberation, among others. The specific document issued is a function of the procedural trajectory of a case prior to the intervention. As such, our outcome measure of procedural advancement is a count of the number of documents issued during the intervention period.

[10]To account for these differing proportions of treated individuals by prison, we use inverse probability weighting (IPW) or quintet block fixed effects in all specifications.

Table 1 reports ITT effects of varying dosages of treatment on case advancement. The findings are striking: The higher the dosage of treatment assigned to a detainee, the higher the rate of case advancement. These effects are quite substantial; the first specification suggests that moving from the last to the first detainee assigned to treatment in a prison corresponds to an additional 0.239 documents issued during the intervention period, a 210% increase on the average number of documents issued for the last detainee assigned to assistance. The second specification with quintet fixed effects is substantively similar. With block fixed effects, the maximum within-block variation with the continuous treatment indicator is 0.8 (four quintiles) for all blocks, so moving from first to last in a block results in the issuance of an additional 0.208 documents. The effects are positive in both jurisdictions. The estimate in the first jurisdiction is higher than in the

TABLE 1 ITT Effects of the Dosage of Treatment on Procedural Advancement

	Case Advancement during Intervention Period (Count)			
	(1)	**(2)**	**(3)**	**(4)**
Panel A: ITT Effects with Continuous Treatment Assignment				
Order Quantile within Prison	0.239**	0.260**	0.296**	0.173
	(0.070)	(0.093)	(0.103)	(0.192)
Mean of DV, Treatment = 0	0.077			
Treatment Range	[0,1]	[0,1]	[0,1]	[0,1]
Panel B: LATE (Marginal Effect) of Each Week of Treatment				
Weeks of Legal Assistance	0.051**	0.068**	0.086**	0.037
	(0.015)	(0.025)	(0.031)	(0.041)
Mean of DV, Treatment = 0	0.010			
First-Stage F-statistic:	73.822	44.006	23.783	23.288
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 Fixed Effects		✓	✓	✓
DV Scale	{0,1,2}	{0,1,2}	{0,1,2}	{0,1,2}
Preregistered Hypothesis Test	Upper	Upper	Upper	Upper
Observations	503	503	371	132

Note: The number of legal documents issued is measured at the conclusion of the 10-week intervention period. The top panel utilizes the quantile measure of the order of treatment assignment using OLS. The bottom panel uses this quantile measure to instrument for the weeks of treatment delivered to a detainee. Heteroskedasticity robust standard errors are in parentheses.
$^*p < .05$, $^{**}p < .01$.

second jurisdiction (top panel), though the difference is not statistically significant in a two-tailed test.

The second panel provides an estimate of the LATE of an additional week in treatment. The endogenous treatment variable is a count of the weeks of treatment received by a detainee, as defined by the implementer. Noncompliers—those detainees unable to be treated—are coded as zeros.[11] One additional week of treatment causes an average of 0.051 additional documents to be issued during the intervention. Thus, the average case advancement for a detainee who receives 10 weeks of legal assistance is 0.51 documents issued. The LATE estimates are positive across specifications and subsamples.

We further compare temporal patterns of case advancement prior to the experimental period and after the intervention among cases assigned to any legal assistance. Figure 4 plots the last pretreatment and first posttreatment record of case advancement across all detainees

assigned to treatment. In many cases, the last recorded procedural step was the arrest (segments without a dark dot). However, after the imposition of the legal assistance treatment (the start of the darker gray segment) the frequency of case advancement increases markedly. This is represented by the increased density (temporal concentration) of the white dots subsequent to treatment assignment. This analysis leverages both within- and between-subject (case) variation and finds support for the proposition that legal assistance increases officials' effort.

Liberation by Endline

We now turn our focus on the ultimate outcome of interest: does legal assistance reduce the probability that a detainee remains in pretrial detention (PTD) at endline? We operationalize this outcome as a binary indicator of whether or not an individual detainee had been released from detention in the second "census" of individuals in the sample. To improve efficiency, we condition the sample for this analysis on detainees who were

[11]In SI Appendix 7, we document eight reasons for noncompliance, none of which includes an unwillingness to receive the free legal services.

FIGURE 4   Last Pretreatment and First Posttreatment Case Advancement among Those Assigned to Any Dosage of Legal Assistance

Note: The relationship between the imposition of legal assistance treatment, as demarcated by a change in segment color from light gray to dark gray, and rates of case advancement are shown. The points indicate the last pretreatment and first posttreatment instance of case advancement.

imprisoned at baseline. Of the 1,080 individuals in the sample, 874 were held in the original prison at baseline. This is, by construction, independent of treatment assignment, which occurred prior to the baseline census and was not communicated to enumerators, and is tested in SI Appendix 6.2.

Table 2 provides estimates of this ITT using two treatment indicators. The top panel utilizes a continuous measure of treatment, namely, the quantile of the order within a prison. This leverages the rollout design but does not leverage the discontinuous cutoff. In the specification without quintet block fixed effects (column 1), an individual assigned to receive treatment last in a prison is predicted to be released from detention 9 months later with a probability of 0.138. Moving from last to first in the prison corresponds to a 4.9 percentage point increase in the probability of having been released from detention. This 36% ITT estimate is substantively quite large and in the predicted direction, albeit not significant. The inclusion of block fixed effects (column 2) increases the ITT estimate to 7.3 percentage points and provides a notable improvement in precision. Leveraging the maximal within-block variation of 0.8 on the quantile treatment indicator, moving from first to last in a block increases

the probability of liberation by 5.8 percentage points. The estimated conditional ITT effect appears to be larger in the second jurisdiction (column 4) than in the first (column 3), though both estimated ITTs are positive, and the difference is not significant in a two-tailed test.

Our second estimator of the ITT includes assignment to treatment as a binary indicator. Here, the sample is smaller, encompassing two prisons ("P1" and "P3") as all individuals in the final (womens') prison were assigned to a nonzero dose of treatment. This indicator also provides suggestive evidence of a positive ITT. In the first specification without block fixed effects, assignment to treatment causes a 2 percentage point increase in the probability that a detainee was released from detention 9 months later. Unsurprisingly, there is less precision when utilizing these operationalizations of treatment than with the former measure, which leverages variation in dosage. The use of block fixed effects improves the precision of the estimate, and we see a substantively larger effect in the second jurisdiction, as above. In sum, the ITT analysis indicates that assignment to treatment or assignment to higher doses of treatment increases the probability that detainees were liberated within 9 months.

TABLE 2 ITT Effects Estimated Using Both Operationalizations of Treatment

	Liberated within 9 Months			
	(1)	**(2)**	**(3)**	**(4)**
Panel A: ITT Effects with Continuous Treatment Assignment				
Order Quantile within Prison	0.049	0.073	0.049	0.152*
	(0.043)	(0.045)	(0.052)	(0.089)
Mean of DV, Treatment = 0	0.138			
Subsample	All	All	J1	J2 (P3)
Treatment Range	[0,1]	[0,1]	[0,1]	[0,1]
Observations	874	874	677	197
Panel B: ITT Effects with Binary Treatment Assignment				
Treatment Assigned (Binary)	0.020	0.039	0.017	0.107*
	(0.026)	(0.026)	(0.031)	(0.048)
Mean of DV, Treatment = 0	0.149			
Subsample	P1, P3	P1, P3	P1	J2 (P3)
Treatment Range	{0,1}	{0,1}	{0,1}	{0,1}
Observations	828	828	631	197
IPW	✓			
Quintet Fixed Effects		✓	✓	✓
Preregistered Hypothesis Test	Upper	Upper	Upper	Upper
DV Scale	{0,1}	{0,1}	{0,1}	{0,1}

Note: 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 court jurisdictions. Heteroskedasticity robust standard errors are in parentheses.
*p < .05, **p < .01.

We now turn to assess the effects of receiving treatment on whether or not an individual was released from detention. We estimate two types of LATE using two-stage least squares. In the upper panel of Table 3, we utilize the rollout of the treatment to estimate the marginal effect of 1 additional week of treatment on the probability of release from detainment after 9 months. Utilizing the quantile measure as the instrument, we model the number of weeks during which a detainee was offered legal assistance, ranging from 1 to 10. We find that each additional week of treatment increases the probability that a detainee is released at endline by approximately 0.8 (column 1) or 1.2 (column 2) percentage points. As such, a move from no treatment to a full 10 weeks of treatment increases the probability of release by ≈12 percentage points. As in the ITT estimates, this effect is larger in the second jurisdiction (column 4). In sum, we find robust support for the hypothesis that legal assistance increased the share of individuals liberated during the study.

In the lower panel of Table 3, we estimate the principal stratum effect for compliers (i.e., the CACE). In this specification, we utilize binary treatment assignment and a binary measure of whether treatment was delivered.

Given the lack of variation in the instrument in the second (womens') prison, we focus the analysis on the other prisons, accounting for the slightly smaller sample. We find that the CACE is positive and substantively sizable, if not significant at conventional thresholds. The overall effect is driven by a dramatic 16 percentage point effect in the second jurisdiction (Column 4).

How large should the effects on liberation be? While the treatment–control comparisons allow us to estimate the rate of release within 9 months with and without legal assistance, they do little to contextualize the magnitude of the treatment effect. Though we do not have an accurate ratio of innocent to guilty defendants, we can estimate the rate of detainees who are imprisoned in illegal pretrial detention beyond the maximum sentence for the crimes for which they are accused. Regardless of innocence or guilt, detainees should be released either via dropped charges or through the application of the law that credits for time served when convicted. This benchmarks the treatment effects of the expected rates of release from detention in the case where all defendants are guilty, which is a lower bound on the conceivable share of individuals released from detention.

TABLE 3 LATEs of Legal Assistance Estimated Using Two Operationalizations of the Treatment Assignment and Treatment Delivered

	Liberated within 9 Months			
	(1)	**(2)**	**(3)**	**(4)**
Panel A: LATE (Marginal Effect) of Each Week of Treatment				
Weeks of Legal Assistance (Count)	0.008	0.012	0.009	0.024
	(0.007)	(0.008)	(0.009)	(0.015)
Mean of DV, Treatment = 0	0.149			
Subsample	All	All	J1	J2
Instrument Range	[0,1]	[0,1]	[0,1]	[0,1]
Treatment Range	[0,10]	[0,10]	[0,10]	[0,10]
First-Stage F-Statistic	410.823	329.532	237.875	94.077
Observations	874	874	677	197
Panel B: CACE of Nonzero Dosage of Legal Assistance				
Detainee Treated (Binary)	0.028	0.054	0.023	0.161[*]
	(0.035)	(0.036)	(0.041)	(0.075)
Mean of DV, Treatment = 0	0.149			
Subsample	P1, P3	P1, P3	P1	J2 (P3)
Instrument Range	{0,1}	{0,1}	{0,1}	{0,1}
Treatment Range	{0,1}	{0,1}	{0,1}	{0,1}
First-Stage F-Statistic	1305.941	1200.135	1103.905	174.88
Observations	828	828	631	197
IPW	✓			
Quintet Fixed Effects		✓	✓	✓
Preregistered Hypothesis Test	Upper	Upper	Upper	Upper
DV Scale	{0,1}	{0,1}	{0,1}	{0,1}

Note: The top panel estimates the marginal effect of one additional week of treatment. The bottom panel estimates the CACE, using binary treatment assignment and delivery indicators. Columns 3 and 4 correspond to the two court jurisdictions. Heteroskedasticity robust standard errors are in parentheses.
[*]p < .05, [**]p < .01.

Consulting the Haitian Penal Code, SI Appendix 8 details the sentence duration for common crimes in the data (i.e., theft, fraud, abuse of confidence, vagrancy, and begging). We estimate 8.8% of defendants are accused of these charges *and* detained beyond the maximum sentence. A further 2.8% of defendants are imprisoned without charges denoted in the prison register. The control rate of release of 14.9% and the ITT of 3.9 percentage points suggest that prisons are clearing this lower bound, and treatment effects are sizable relative to this bare minimum standard. This calculation represents a cynical approach to the path out of prolonged pretrial detention. When we relax the restrictive assumption that "all defendants are guilty" or that defendants should be sentenced to statutory maximums, this standard increases, shrinking the ratio of observed treatment effects to the desired efficacy of legal assistance.

The Mechanism: Moral Hazard of State Agents

Our analysis suggests that legal assistance induced higher levels of effort by bureaucrats on treated cases. These comparisons indicate excess capacity to advance cases that is not exercised without legal assistance. However, we have not yet presented measures of effort (as opposed to outputs). To establish levels, we estimate rates of absenteeism descriptively using evidence from case files.

We focus first on the clerks in both the court and parquet. The focus on clerks is motivated by their limited responsibilities away from the office and the parallelism in responsibilities across the two entities. Our rollout design provides an as-if random audit of clerks over approximately 10 weeks. Among found cases, we estimate the number of lawyer visits necessary to find the case

FIGURE 5   Estimated Levels of Absenteeism by Clerks
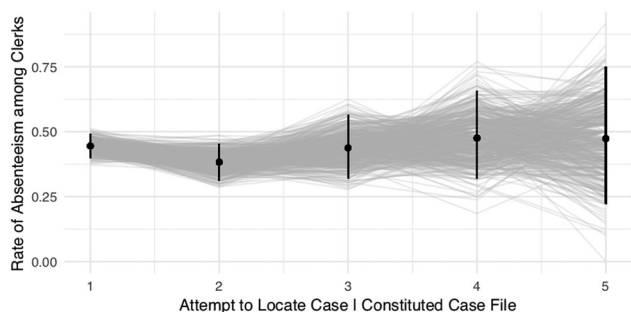
Note: Levels of absenteeism are calculated from the number of unique visits necessary to find a case given that the case files were constituted (found). Gray lines represent bootstrap iterations, and black lines indicate 95% confidence intervals.

files. This allows us to estimate the rate of absenteeism of clerks—the bureaucrats responsible for this procedural step. Figure 5 presents estimates of absenteeism for clerks, which is 48.9% on the first visit. The analysis of subsequent visits in Figure 5 is consistent with a model of behavior in which this rate of absenteeism is relatively constant across agents.

Qualitative evidence suggests that the absenteeism of clerks is far from unique among officials. Lawyers regularly scheduled meetings with officials of the court to consult on the status of a case. Even when judges and prosecutors were "scheduled" to be in the office, case files indicate high levels of absenteeism.

In sum, this evidence shows that illegal pretrial detention is sensitive to bureaucratic effort. These findings are novel because we measure bureaucratic outputs directly at the task (case) level. The findings establish that even holding constant a principal's directive and agent quantity and quality, changing the behavior of low-level agents can improve human rights compliance. By isolating bureaucratic action, we show that state capacity is reduced by agents' behavior, not simply the selection of bureaucrats by principals. To the extent that such shirking undermines the protection of rights, we also demonstrate that incremental improvements in rights performance can be made through policies targeting agents, not principals. Such interventions are rare among the set of policy instruments used to advance human rights compliance.

Alternate Explanations

Having established that intervention aimed at low-level state agents in the justice system can indeed improve human rights performance in the case of illegal pretrial de-

tention, we turn to three alternate explanations for these findings. We provide evidence that no explanation, in isolation, can account for the observed findings.

The Cost of Rights

A prominent argument in legal scholarship emphasized by Holmes and Sunstein (2000) holds that rights are costly for states to protect. Indeed, the complexity of the Haitian justice system points to the number of state actors involved and the time intensiveness of the work. In states with limited fiscal capacity, the basic costs of ensuring rights can be prohibitive, as evidenced by our interviews in Haiti. The absence of constitutionally guaranteed public defenders and insufficient staffing levels relative to the extent of backlogged cases may result from limited finances.

This argument is broadly consistent with our argument about misgovernance and the violation of rights but posits that rights may be undermined by a lack of bureaucrats, not by bureaucrats' strategic choice to shirk. In this account, bureaucrats may be working at full capacity and simply not have the time or resources to process a sufficient number of cases. Our analyses of absenteeism, however, shows substantial shirking among clerks, prosecutors, and judges.

We thus contribute a sobering corollary to "cost of rights" arguments. Although some limitations to the capacity of Haiti's judicial institutions may be due to lack of finances, we provide evidence that protection of detainees' rights are further eroded by the actions of the bureaucrats working within the justice system. This evidence consists of observations about widespread absenteeism in addition to responsiveness to an intervention that reduced the cost of exerting effort on cases represented by legal assistants.

Corruption by Agents

The widespread observation of petty corruption or rent seeking by agents in the parquets and courts provides another alternative explanation for our findings (Bohara, Mitchell, and Mittendorff 2004). Legal assistance could have two effects if corruption is the main driver of low effort. First, it could overcome holdup problems if officials know that a bribe is unlikely to follow when a lawyer starts to represent a case. In this case, agents may respond to legal assistance by exerting effort to process the case. Second, by generating information on what cases are not being processed, legal assistance could

generate information about which agents are corrupt, which may be reported up the chain of command, increasing exposure to penalties for shirking.

However, all evidence suggests that these mechanisms, in isolation, are unlikely to explain our experimental findings. First, judges in the court have exceedingly low-powered incentives. During their 7 year mandates, if judges are removed from office, they will continue to receive their salary. Prosecutors in the parquet have no such mandate. As a result, possible sanctions are smaller for judges than for prosecutors and clerks. If legal assistance works by drawing the attention of superiors to corrupt practices (or shirking), then cases originating in the parquet should advance at faster rates than those that originate in the court. We find no evidence that legal assistance has a stronger effect on cases originating in the parquet in SI Table A10.[12]

Our interviews with legal assistants suggest that court officials are not shy about asking for bribes from aid-funded legal assistants despite the fact that the legal assistants cannot pay bribes. Officials do not demure from making such requests even once lawyers are involved. Collectively, this evidence suggests that holdup problems (if they exist) are not resolved by legal assistance, nor is there evidence that officials fear detection and reporting by legal assistants.

SUTVA Violations

We characterize two possible stable unit treatment value assumption (SUTVA) violations formally in SI Appendices 2.2 and 2.3. First, it is possible that cases represented by legal assistants are "cutting the line." This substitution of represented for unrepresented cases produces two concerns. Beyond the typical bias concern, if legal assistance is just inducing queue jumping, treatment effects may be mechanical and not indicate an increase in effort. We do not know the order in which court officials devote energies to cases in the absence of legal assistance, but we assume some unknown latent ordering. The order in which cases are assigned to legal assignment is obviously known.

In the simplest conception of the resultant bias, imagine that nothing changed about how officials process cases except for the order. The process of "holding back" control cases will mechanically inflate the difference in the rates of advancement of treatment versus control cases.

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[12]If the likelihood of oversight increases and the marginal cost of effort decreases, we cannot separate the (complementary) effects. However, this evidence suggests that any changes in oversight, if present, occur alongside reductions in the marginal cost of effort.

We rule out this explanation, at least in isolation, by considering the result presented in Figure 4. The mechanical effect stemming from this form of SUTVA violation does not account for the acceleration in the rate of case advancement after the imposition of legal assistance.

Second, consider the possibility that legal assistance induced officials to take on a higher rate of cases than is typical, as asserted by the theoretical shock to the cost of effort. The number of cases "on the desks" of officials may have induced congestion. The hypothetical effect of congestion could go either direction. If cases assigned to earlier treatment face less congestion, they may advance more quickly or advance further procedurally, overstating the slope of the ITTs using quantile measures. We rule out the first possibility empirically. Among the treated cases, we examine whether cases advanced within the first 15 or 30 days of treatment (relative to the date at which the order was reached). Congestion would suggest that cases at the end of the distribution advanced less frequently within the fixed time period than those at the beginning of the distribution. We find no evidence that this was the case in SI Table A13. Effects are precisely estimated zeros, giving little credence to the congestion account. In contrast, if congestion slows down all treated cases relative to a court without legal assistants, the estimated effects should understate the true magnitude of legal assistance absent congestion (the SUTVA violation). Our evidence of absenteeism suggests that even with legal assistance, courts were not operating at capacity.

Conclusion

Our theory and evidence posit misgovernance—"banality alone"—as a logic for some forms of human rights abuses. The assignment of legal assistance allows us to observe the actions of state agents in Haiti's courts, studying moral hazard problems of state agents who undermine compliance with the rights of pretrial detainees. This microlevel empirical evidence is necessary to establish the existence of misgovernance as a source of rights abuses. We conclude by considering the potential scope of misgovernance as a source of human rights abuses and its implications for the promotion of human rights.

While illegal pretrial detention is one of the most widespread forms of physical integrity violation and is important in its own right, how far does a theory of misgovernance travel in the realm of human rights? The scope conditions of our argument are based on the type of right and the justification for denial of a right. First, our argument focuses on positive rights—those requiring

agents to take positive action to implement. In addition to timely adjudication in the case of pretrial detention, such rights include, but are not limited to, many economic, social, and cultural rights. Second, principals (i.e., governments) must not benefit by denying the right. As such, unlike a large body of literature on intended repression and regime type, misgovernance occurs in autocratic or democratic regimes alike. The frequency of such abuses rooted in misgovernance should vary with institutional environment or context. Future work on the source of rights abuses (i.e., intended repression or misgovernance) may be particularly fruitful in domains of police violence, in addition to educational rights, healthcare, labor, disability, and other rights contained in the International Human Rights Treaties and Protocols.

We argue that interventions targeted at agents—like the legal assistance program studied here—may enhance rights compliance. The logic of much human rights advocacy work and research promotes rights compliance by identifying mechanisms that increase principals' costs of employing repression. Yet, in the case of misgovernance, such policies are unlikely to advance rights and may even exacerbate abuses. Critically, to design and target appropriate interventions, our characterization of misgovernance and human rights shows that we must identify the mechanism underpinning rights abuses and the relevant institutional actors to target interventions.

In sum, this article provides a new conceptual focus for scholars of comparative politics and human rights interested in understanding variation in human rights performance globally. By contrasting the logics of repressive intent with misgovernance, our research highlights the importance of characterizing the bureaucratic logic of human rights practice. Moreover, our research provides a conceptual justification for expanding the operational definition of human rights measurement to include other forms of human rights violations beyond those most closely associated with abuse.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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Appendix 5: Data Collection
Appendix 6: Research Design Details and Validation
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Appendix 11: Liberation During Intervention Period
Appendix 12: No Evidence of Congestion
Appendix 13: Mapping to Pre-Analysis Plan