Misgovernance and Human Rights: Experimental Evidence of Illegal Detention without Intent *

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Please find the appendix to this paper.

Abstract

Existing explanations of human rights abuses emphasize a strategic logic repression. Yet certain classes of abuses may arise absent the intent to repress because of the misaligned bureaucratic incentives of state agents. To separate strategic repression from bureaucratic incentives we study the responses of state agents working within the Haitian criminal justice to a randomized free legal assistance intervention. 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 abuses in the justice system result from poor governance and not repressive intent.

Keywords: human rights, misgovernance, bureaucracy, judicial system, legal assistance, Haiti, field experiments

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1 Introduction

The "cogs in the machine" defense has prominently been invoked by those tried for human rights abuses and war crimes. Normative political theory has analyzed 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. We approach this question from the opposite angle asking whether widespread human rights abuses can occur with a welfare-oriented principal or government and how. We argue that there are indeed cases in which banality alone – absent evil – undermines the protection of rights. To the extent that the protection of human rights is an increasingly stringent normative standard and goal, it is critical to understand the conditions under which such abuses occur in the absence of intent.

We study how the incentives of "cogs" (state employees) and the structure of the "machine" (the state) influence the protection of rights under different government objectives. We first consider the role of these employees in a simplified account of repression. In this case of intent, an agent of the state chooses to exert effort to carry out a directive to repress. The outcome – some form of abrogation of rights – is endogenous to the agent's decision to "work" or carry out their orders. As in the broader human rights literature, the decision of this agent sits at the core of our understanding of moral, ethical, and legal conceptions of culpability for the perpetration of human rights abuses.

Second, and in converse to the first case, we consider an agent tasked with ensuring that rights are upheld – for example, by ensuring a timely trial – chooses not to exert effort. A qualitatively similar abrogation of rights occurs precisely because the agent decided to "shirk" or not carry out their orders. In this case of misgovernance, though the directive is not to repress, there may be a systematic violation of human rights in certain domains. The extant literature provides neither theoretical nor empirical guidance on how widespread this form of human rights abuse may be, or the conditions under which shirking public officials or inefficient institutional arrangements may indeed violate rights. Consistent with the concept of misgovernance, this form of abuse occurs when the political principal or government is welfare-oriented (not pursuing a strategy of repression) (Banerjee, 1997). In order to understand when and why these abuses occur, we seek to characterize agents' incentives and the structure of the institutions into which they are embedded (Banerjee, 1997; Shleifer and Vishny, 1993).

We consider the role of state officials and the organization of criminal justice institutions in noncompliance with constitutional protections of rights of the accused. Illegal prolonged pretrial detention occurs when the detention of the accused overruns constitutional or statutory time limits. In the 186 countries that provide data on imprisonment, there are nearly three million individuals held in pretrial detention (Institute for Criminal Policy Research, 2017). Certainly not all pretrial detainees are *illegally* imprisoned, but excessive rates of pretrial detention suggest widespread rights abuses. Figure 1 suggests that high rates of pretrial detention are concentrated in poor countries and that there is a generally positive association between rates of over-crowding and rates of pretrial detention, especially in these settings. This hints at chronic backlogs of cases as a potential source of these abuses, a theme which we develop.

In Appendix 1, we estimate the number of illegal pretrial detainees in these 186 countries under different assumptions about the share of pretrial detainees that could reasonably be legally detained. Our best approximation suggests that there are over one million such detainees. While systematic measures of illegal detention of *political* prisoners (or prisoners of conscience) are not available cross-nationally, comparison within countries suggests 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 (Fariss and Dancy, 2017).[1]

[1] In contrast to the academic literature on human rights, rates of pretrial detention are used as an indicator of Sustainable Development Goal (SDG) 16 performance with respect to access to justice (United Nations, 2017).

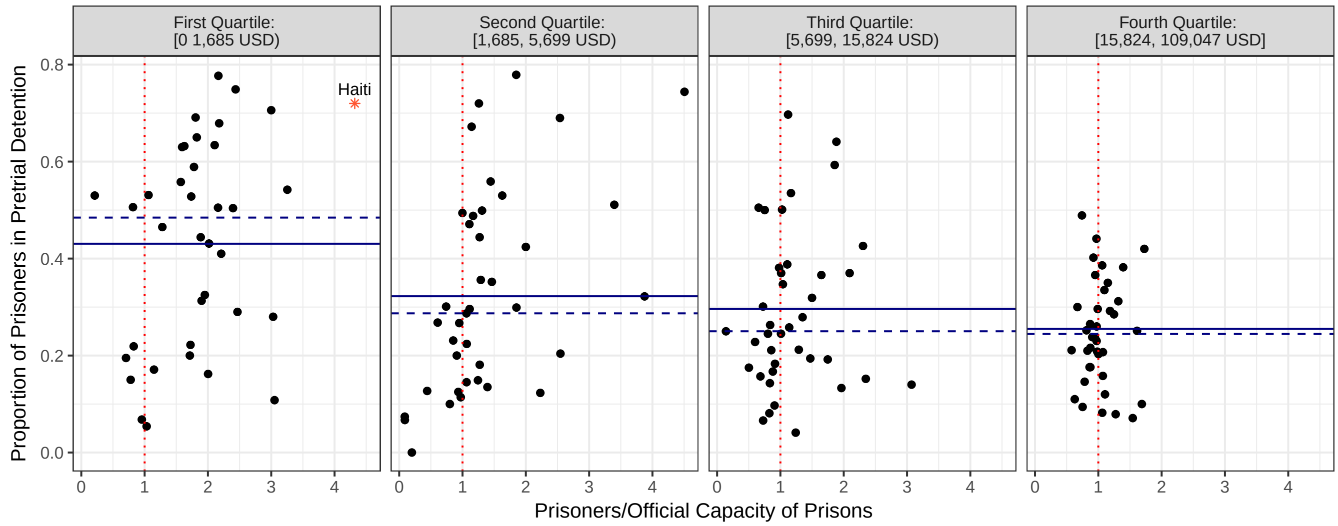

Figure 1: Graphs of country-level ratios of prisoners to reported prison capacity (x-axis) and prisoners held in pretrial detention (y-axis). The panels divide countries into quartiles by per capita GDP (2015). In each panel, the solid and dashed navy lines represent the mean and median rate of pretrial detention, respectively. Haiti is labeled and marked with a star.

We study these dynamics in Haiti where illegal pretrial detention represents a persistent source of human rights abuses (United States Department of State, 2017). The Haitian Constitution clearly specifies the time limits for pretrial detention and detainees' right to free legal aid. However, in practice, these time limits are routinely exceeded by months or years and state public defenders have not been hired in recent memory, if ever. In addition to the human rights concerns, illegal pretrial detention drains limited state resources to finance prison stays and longer periods of pretrial detention make cases harder to prosecute, resulting in low conviction rates. Further, given gang recruitment in prisons, longer exposure to detention may also strengthen Haiti's gangs (Skarbek, 2016; Lessing, 2017; Jung and Cohen, 2018).

We test our argument about misgovernance and illegal imprisonment in the Haitian context in two ways. First, we draw on annual human rights reports, 88 qualitative interviews with justice system officials and bar members, and an original descriptive dataset on the detained, all of which demonstrate that there is no evidence of an intent to repress. Second, we 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 prosecutor's offices (*parquets*) and Courts of First Instance. This treatment allows us to assess the degree to which case outcomes respond to by-the-books interventions in these agencies, testing our argument that this form of illegal imprisonment occurs due to bureaucrats' actions and the structure of these institutions.

We gather two original datasets to measure outcomes. First, we utilize lawyers' case files on the detainees assigned to (a non-zero dosage of) treatment. These records, including a list of interventions attempted, legal documents procured, and case advancement represent a particularly rich source of outcome data. Second, we received permission from the Haitian Ministry of Justice to conduct baseline and endline censuses and surveys of our subjects in the prisons. To our knowledge, these are the first ever surveys of an illegally detained population.

The randomized rollout experimental design allows us to leverage variation in both the presence and dosage of the legal assistance treatment to examine their effects on case advancement and release from PTD. Given that the subjects in our study were prisoners at the start of the study, we follow guidance from the bioethics literature on the allocation of scarce treatments in determining our assignment procedure. Consistent with this literature, our randomized rollout design maximizes the number of subjects who ultimately receive some dosage of treatment while allocating treatment in a just and impartial manner. We sample 1,080 pretrial detainees from the population of pretrial detainees in three prisons within two jurisdictions. Within each prison, we randomized the order in which each detainee was assigned to the legal assistance treatment. Legal assistance was provided by a full-time staff of 18 attorneys until an exogenous end date when the program's budget was exhausted (not disclosed to attorneys in advance). In total, approximately half the sample was assigned to receive treatment (their order was reached), albeit at different dosages since those with higher order of assignment to treatment received treatment over a longer period of time (0 to 10 weeks).

Overall, we find evidence that cases advance proportionately to the dosage of legal assistance:

longer periods of assistance increased the probability and number of case advancements during the intervention period. A marked increase in the rate of advancement relative to the pre-intervention period further suggests that legal assistance was effective in eliciting greater effort. We also find that legal assistance increases the probability that individuals exit prolonged pretrial detention within nine months. Further, the data collected for the experiment allows us to estimate the dynamics of pretrial detention in the absence of legal assistance, characterizing the implications of these rights abuses empirically in Haiti and potentially in other contexts as well.

This paper makes four principal contributions. First, we develop a new misgovernance-based explanation for some classes of human rights abuses that are not well explained by extant theories of repression. Second, we make the case for rights abuses resulting from "banality alone" while focusing empirically on prolonged pretrial detention. Prolonged pretrial detention is the largest form of illegal detention but ironically, it is virtually absent from existing social scientific research on human rights. Third, we present novel descriptive and experimental evidence from Haiti which adds to a nascent body of experimental human rights studies (Bracic, 2016; McEntire, Leiby, and Krain, 2015); contributes to debates on legal reform (Langer, 2007; Cavise, 2013; Kronick, 2018; Blanco, 2012); provides evidence from criminal courts in a developing setting to complement work on civil courts (e.g., Kondylis and Stein, 2018; Chemin, 2009); and finally, links experimental evidence from the formal justice sector to informal justice sector interventions in similar political settings (Blattman, Hartman, and Blair, 2014; Sandefur and Siddiqi, 2013). Fourth, we emphasize the role of ethical considerations in explicitly informing our research design. We believe that the large-n randomized rollout design developed for this project is the first of its kind in political science.[2]

[2]Randomized rollout and stepped wedge designs have been applied as to be small-n designs (e.g., Gerber et al., 2011).

2 Theoretical Framework

2.1 How do Abuses Emerge?

We develop a theoretical framework linking misgovernance and human rights performance. In the seminal account of misgovernance, Banerjee (1997: p. 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 occurs serves as the critical distinction between our argument and dominant accounts of human rights performance. We do not posit that repression does not occur; indeed, there exists substantial evidence that repression is widespread. Instead, we suggest that there exists a class of rights abuses that occur even with a benevolent (or relatively benevolent) government and are not well explained by existing theory.

Assuming that there exists a welfare-oriented political principal, 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; Langer, 2007; Cavise, 2013). The tasks required to advance a criminal case require substantial effort as well as the participation of multiple agents, possibly within different agencies. In resource-constrained settings, 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 (e.g. case outcomes), in a 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. Consistent with standard arguments, we argue that lack or other diversion of effort by bureaucrats reduces the rate of case advancement. Thus, in contrast to the adverse selection problems emphasized in existing accounts of misgovernance, we focus on moral hazard problems within the justice sector.

The structure of criminal justice institutions further limits the rate at which cases can reach a 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. In order 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.[3] While agents could, in theory, 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.

Furthermore, limited information transfer between institutional entities implies that monitoring or oversight of case outcomes and compliance with human rights protections is particularly 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 an individual judge or prosecutor faces for not exerting effort on a particular case. Thus, the moral hazard problems that we identify are further exacerbated by the institutional structure into which agents are embedded.

Our argument generalizes to environments characterized by case backlogs and it relates closely

[3]Our description of "failure to coordinate" is distinct from game theoretic conceptions of coordination failures. If agents do not value outputs as we assert, the formulation of the present scenario as a coordination game is not evident.

to arguments about the structure of public sector organization in conditioning outcomes. In such settings, institutional arrangements undermine the efficiency of case advancement through the criminal justice system, resulting in illegal detention of the accused and non-compliance with both domestic and international human rights agreements.

2.2 The Role of Legal Assistance

Given the exposition of how illegal pretrial detention can emerge absent repressive intent by the government, we argue that legal assistance increases compliance with existing law by accelerating 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, the imposition of legal assistance exogenously 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 Appendix 2. The model is a straightforward adaptation of a standard two-task contract, where the tasks are represented and unrepresented cases (Holmstrom and Milgrom, 1991; Bolton and Dewatripont, 2005). In the empirical setting, the random assignment of legal assistance implies random assignment of the types of tasks, providing some additional traction on model parameters. This model allows us to map the research design onto the theory and aids in subsequent discussions.

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 investigative judge to advance the case. Cynically, the lawyers do the first portion of the state 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 of the defendant. However, ultimate advancement requires court officials to take positive action. We argue that legal assistance reduces the marginal cost of exerting effort to advance a case by either the prosecutor or investigative judge.

A reduction in the cost of effort of processing a case induced by legal assistance should increase equilibrium levels of effort. Two observable implications follow from this simple formulation, which allow us to test whether the actions of state officials are sensitive to this reduction in cost.

Hypothesis 1. *Detainees' cases that are subject to legal assistance should advance procedurally with higher probability than detainees' cases that are not subject to legal assistance.*

Hypothesis 2. *The rate of procedural advancement subsequent to the imposition of legal assistance should exceed the rate of procedural advancement prior to 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. Further, 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 positive correlation in the outputs of the parquet and court. That is, the work accomplished in one entity is communicated to and informs the tasks of the other and vice-versa. In the present setting with large caseloads and backlogs – whether due to lack of staffing, limited effort, or some combination thereof – 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 legal assistants bring a prosecutor or judge's attention to a file. Thus, beyond increasing the effort of individual agents, this should introduce positive correlation in the outputs of the two entities.

Higher correlation in outputs will ensures that a larger share of cases clear both the parquet and court. Through this mechanism, legal assistance alleviates the severity of the two institutions' failure to coordinate. Given the sequential nature of case progression through the criminal justice system, this should be sensitive to both the presence of legal assistance and the duration or dosage of legal assistance. Notably, the ability of lawyers to track a case from one office to the other requires a sufficient period of legal assistance.

Hypothesis 3. *Cases assigned to (higher dosages of) legal assistance are more likely to reach a final disposition within the period of the study.*

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

3 Prolonged Pretrial Detention in Haiti

Prolonged pre-trial detention represents one manifestation of problems within Haiti's judicial institutions. For at least a decade, Haiti has experienced staggering and relatively constant levels of prolonged pretrial detention (see Figure A9). While this does not provide the proportion held in *prolonged* pretrial detention, given the 48-hour limit on pre-arraignment detention, the vast majority of these pretrial detainees are illegally detained. In Port-au-Prince, the situation is particularly acute. 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 widespread communicable disease (Farmer and Gastineau, 2002).

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. In practice, however, these rights are frequently violated, particularly for those prisoners that cannot afford a private attorney. In these cases, prisoners are in *prolonged* or *illegal* pretrial detention. Further, as per the Haitian

Constitution, indigent defendants are to be provided access to a "public defender." In practice, however, due to resource constraints, the Haitian government does not presently employ any such public defenders. Ironically, however, imprisonment is quite expensive: housing a detainee for a year is estimated to cost approximately $480 USD, or 58% of Haiti's GDP per capita.

The absence of state-sanctioned legal assistance has strong implications for the distributive consequences of pretrial detention. The two complementary means of avoiding such forms of prolonged detention are to employ a private attorney or pay bribes to justice system officials. These strategies are typically available to and used by all but the poorest individuals. As such, the phenomenon of prolonged pretrial detention reveals some of the socioeconomic inequality endemic to Haiti.

3.1 The Government's Objective

We first turn to validate our assumption that the Haitian government does not strategically detain individuals for political or economic gain. In this context, the government refers to Haiti's president and the Minister of Justice. We aim to deduce the government's objective from four sources of evidence.

First, we draw on information from the 2016 US State Department report on Haiti, which finds that there were no reports of political prisoners or detainees or politically motivated disappearances. These reports serve as the primary source for cross-national human rights data (Fariss and Dancy, 2017). 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. As we document in detail in Appendix 14, all allegations focus on low-level state agents as opposed systematic political directives from above.

Second, we draw systematic evidence from 88 semi-structured interviews conducted in 2015. Our subjects include but were not limited to judges, prosecutors, prison officials, and bar association members in five of Haiti's 18 Courts of First Instance. No interview subjects described

Haiti's overflowing pretrial prison population as a symptom of "repression" or political coercion among current pretrial detainees. Leveraging the differences in relationship with the Haitian government, the unanimity is highly suggestive. 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 seven years, with wide ranging access to prisons and courts across the country. Further, this program represents just one of multiple legal assistance programs enacted by aid donors. The degree of access and transparency afforded to donors and international organizations is seemingly inconsistent with a goal of wide scale repression via pretrial detention.

Third, we can attempt to deduce the government's objective as an empirical question. Appendix 4 describes the profiles of detainees gathered from our surveys and pre-treatment prison data. Consistent with existing work on crime and violence, detainees are disproportionately young men and resemble the median resident of the respective districts in terms of position of assets prior to arrest. 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 (Figure A12). Furthermore, within our records of individual cases, self-reported charges by detainees are highly correlated between baseline and endline. Moreover self-reports correlate positively with the two administrative data sources for all offenses (Figure A13). Further, in an open-ended question at endline asking "why do you think you are [in prison]," administered at the endline, just one detainee (of over 650) suggested any political suspicion. Detainees spoke openly to enumerators during both survey rounds. The data thus does not suggest that illegal pretrial is used or perceived as a form of state-sanctioned repression in Haiti.

Fourth, in our experimental research design, we exploit the timing of an unplanned transition from a sitting an interim president that occurred during the rollout of the experiment.[4] In Appendix

[4] The second round of the 2016 presidential election was canceled amid concerns of first-round election fraud. After several weeks of Constitutional uncertainty, an interim president was sworn

A10, we document that the identity of the president did not change lawyers' access to the prisons or courts or the apparent efficacy of the treatment. These findings provide no evidence to contradict our characterization of the Haitian government's objective.

4 Research Design

4.1 Ethical Considerations

The population of subjects in this experiment consists of detainees held illegally in three Haitian prisons. We address the ethical challenges inherent to the prison setting through a combination of preliminary fieldwork and design-based considerations. In order to ascertain the potential for negative unintended consequences of legal assistance, we assessed case files from existing legal assistance cases and conducted many interviews with stakeholders during exploratory fieldwork. We attempted to discern whether legal assistance could make a detainee *worse* off. 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. As such, we sought to maximize the number of individuals that would receive legal assistance during the experimental period.

We opt for a randomized rollout design to maximize 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 thought to be a fair and efficient method of allocating treatments, particularly in settings of limited information (Persad, Wertheimer, and Emanuel, 2009; Cao and Huang, 2012). During the implementation of the experiment, all subjects had the right to opt out of any portion of this evaluation including the surveys or legal assistance at any time.[5]

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in. He served for one year, encompassing the remainder of the experiment and data collection.

[5]Refusal of legal assistance was very rare and constitutes one source of non-compliance.

4.2 Population and Sample

Using a novel large-n randomized rollout experimental design, we test if the provision of free legal assistance to detainees affects the effort of state agents working within the Haitian criminal justice system and ultimately the case advancement and liberation of detainees. As we described in detail above, Haiti provides a useful case to isolate the relationship between misgovernance and human rights abuse because, annual human rights reports, qualitative interviews, and original administrative data provide no evidence that the government purposefully intends to repress over 8,000 individuals held in prolonged pretrial detention.

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. Inmates who were not presently in *prolonged* pretrial detention were not included in the experimental sample. Because this determination is difficult to make from prison records alone, we sample from detainees that had spent at least six months in pre-trial detention at the beginning of the intervention, indicating that procedural time limits had been overrun. Further, as per donor guidelines, legal assistance was not offered to detainees accused of rape, human trafficking, or drug trafficking. Potential detainees for which 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.

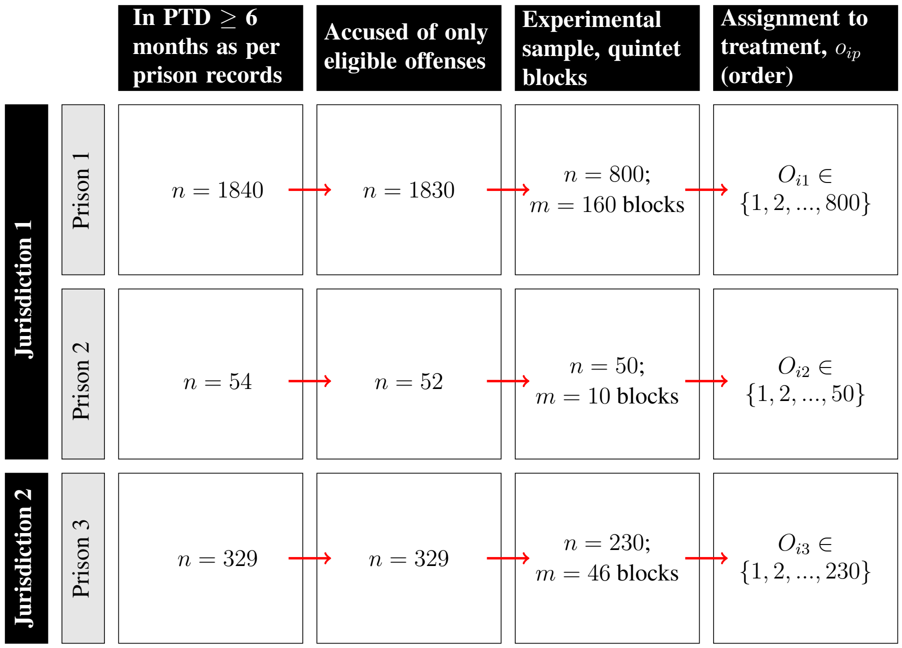

Figure 2: Process of selection and assignment within each prison in the sample.

4.3 Assignment

In order to maximize the beneficiary population, we utilize a randomized rollout design. In this design, each prison serves as a block, within which, detainees in our sample were assigned an order. 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 program, we used blocking to ensure that the randomization yields maximal heterogeneity among the full schedule of potential subjects. Within each of the three prisons and then 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 (where available). 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. The mapping from the population of detainees to the assignment is illustrated in Figure 2.

We conduct a balance check on the resultant randomization by regressing the order indicator on the pretreatment covariates from the prison register in Table A1. Due to the different numbers of inmates in the sample by prison and hence different scales of the treatment variable, we adopt two approaches. First, we estimate the regression using prison fixed effects (Columns 1 and 2). Among the pretreatment variables, all of the coefficients on the pre-treatment variables are near zero and all confidence intervals bound zero. Second, we estimate the regression using the percentile of the order within each prison as the dependent variable. This scales the order random assignment on a common scale for each prison. We estimate these regressions without fixed effects. Again, all coefficients are estimated to be near-zero with considerable precision. We fail to reject null hypothesis in an F-test of the joint significance of the coefficients (Columns 3 and 4).

Given the randomly assigned order within each prison, legal assistants provided services sequentially within each prison. During the ten-week period when the legal assistants provided services 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.

4.4 Outcomes and Measurement

Our outcome data is collected from two principal sources, spanning both administrative and survey data at different points in time, as depicted in 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 course of the three-month intervention and immediate aftermath. These files also provide detailed records of lawyers' actions in the provision of legal assistance.[6]

We analyze two primary outcomes, both measures of case advancement. First, we examine whether each treated case advanced procedurally through the Haitian criminal justice system during the experiment and in the two weeks following the cessation of legal assistance. Case advance-

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

ment is coded from lawyers' case files and is documented with facsimiles of legal documents. While we only have these records for treated detainees, we make inferences on the basis of the randomly assigned rollout of the treatment.

Second, we seek to measure whether cases reached their final disposition in the courts. In practice, this could be release (via acquittal or dropped charges) or conviction. In practice, given extended periods of pre-trial detention, conviction can also result in the release of prisoners when they are credited for time served. Our main outcome—liberation after nine 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 and recorded prison records codifying the detainee's status.[7]

4.5 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, a binary treatment indicator can be constructed. Those detainees whose randomly-assigned orders were reached during the course of service provision are coded as assigned to treatment; those orders were not reached are coded as assigned to control. Because all detainees in the experimental sample in the smallest prison were assigned to treatment, this prison is dropped from analyses using this operationalization of the treatment indicator.

[7] 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.

We estimate intent-to-treat effects using straightforward OLS specifications with and without block (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 pre-analysis plan distinguishes between one- and two-sided hypothesis tests. We report the tests implied by the pre-analysis plan.

4.6 Non-Compliance

As predicted, not all detainees assigned to treatment were ultimately treated. In total, 157 of the 503 detainees assigned to treatment (at any non-zero dose) were not treated. Table A2 provides the frequency of each reason for non-compliance among individuals assigned to treatment. Empirically, this does not pose a threat to the validity of our estimates. We estimate both intent to treat (ITT) effects 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 3 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.

As specified in our pre-analysis plan, 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 (as described above). 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.

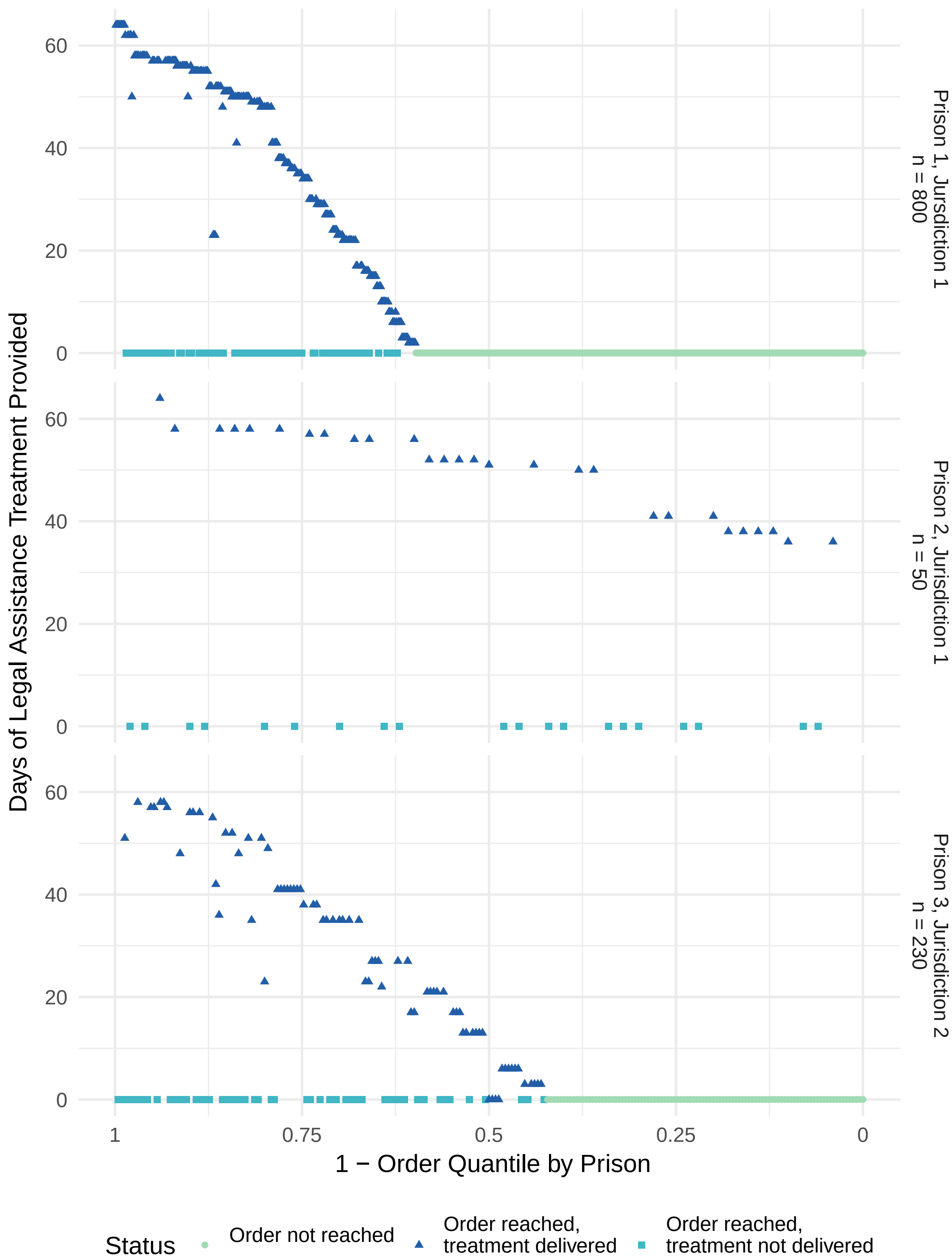

Figure 3: This graph depicts units by assignment status (quantile within prison) and treatment status. Treatment assignment is indicated by quantile of the order within a prison. Treatment delivery (status) is measured in terms of the number of days of treatment on the y-axis. Non-compliance is one-sided.

$$D_{ib} = \gamma_1 Z_i + \boldsymbol{\eta}_b + \epsilon_i \quad (1)$$

$$Y_{ib} = \beta_1 \widehat{D_{ib}} + \boldsymbol{\kappa}_b + \xi_i \quad (2)$$

5 Results

5.1 Treatment Delivery and Manipulation Check

More than 2,000 pages of case records recorded by lawyers provide ample evidence that treatment was assigned, attempted, and delivered in the randomly-assigned order. Figure 4 indicates that, on average, earlier assignment to treatment led lawyers to make more interventions on behalf of a detainee. In the smallest prison in which 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.

A manipulation check demonstrates that treated detainees were aware that they were recipients of legal assistance. Table A3 provides estimates of the intent to treat (ITT) effect of treatment assignment on recollection of a visit by lawyers from the program. It demonstrates positive, highly significant effect of assignment to a non-zero dosage of treatment on recollection of such a visit (lower panel). While these data come from the endline survey that suffered high levels of attrition among those released from detention, point estimates and extreme value bounds support the interpretation that detainees assigned to treatment were indeed treated and recalled the legal assistance at substantially higher rates than those not assigned to treatment.

5.2 Case Advancement Analysis

We first assess whether the legal assistance accelerated the rate of case advancement during the intervention period. Here we analyze the case advancement data collected during the intervention

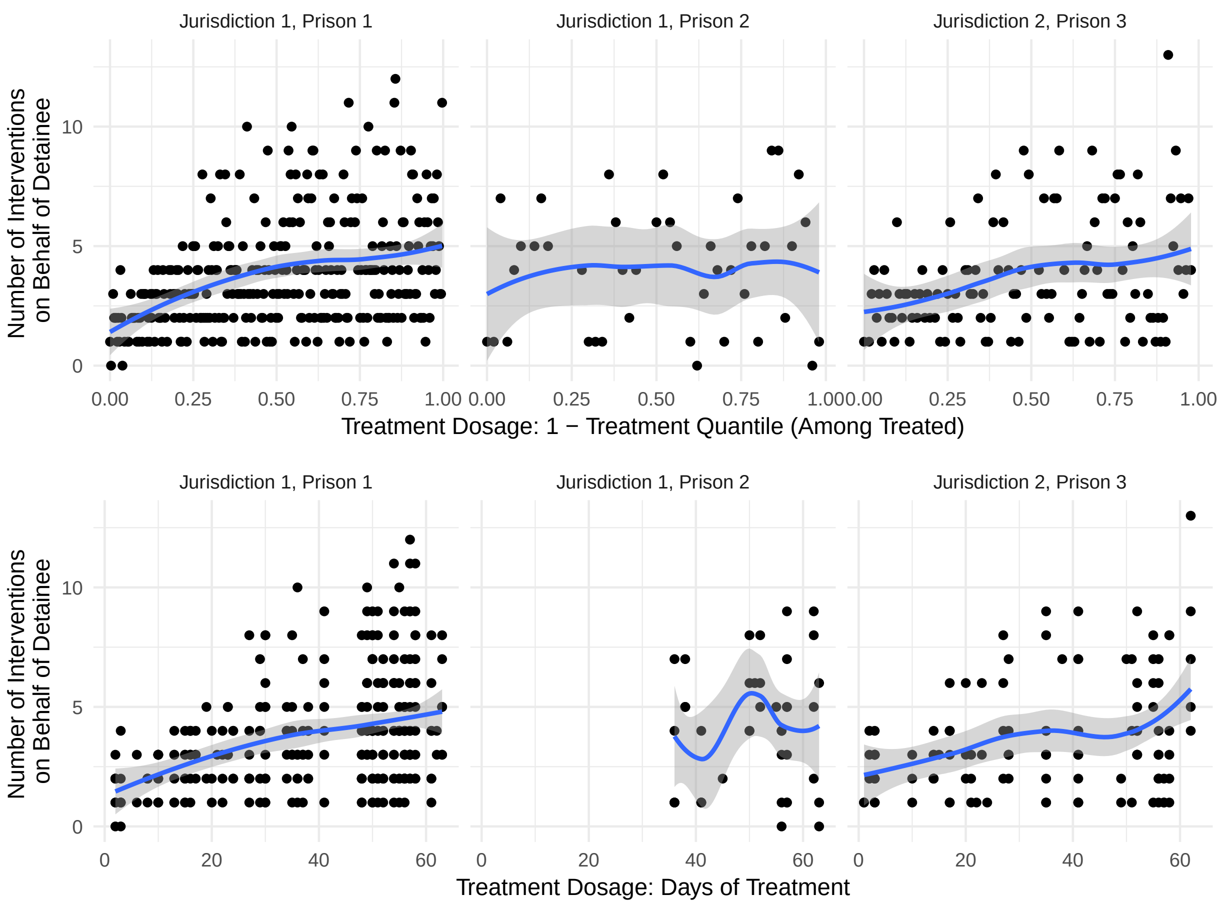

Figure 4: This graph illustrates that assignment to earlier treatment generally corresponded with higher doses of treatment (more interventions on behalf of the detainee). The number of interventions is the sum of interventions in the prisons, Parquets, and Courts on behalf of a detainee as recorded in lawyers' case files. Points depict the raw data and lines are fitted with Loess.

period and immediately thereafter for 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 the last detainee in a prison assigned to treatment.[8] 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 and ranges from zero to two.

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 probability 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-percent increase on the average number of documents issued for the last detainee in the order. 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 point estimate in the first jurisdiction is higher than the point estimate in the second jurisdiction (top panel), though the difference is not statistically significant

[8]To account for these differing proportions of treated individuals by prison, we use IPW or quintet block fixed effects in all specifications.

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. Non-compliers – those detainees unable to be treated – are coded as zeros.[9] One additional week of treatment causes an average of 0.044 additional documents to be issued during the intervention. Thus, the average case advancement for a detainee that receives 10 weeks of legal assistance is 0.44 documents issued. The estimate is quite similar with block fixed effects in Column 2. In Columns 3 and 4, the point estimates of the LATE in the two jurisdictions are almost identical, though this LATE is estimated with far less precision in the second jurisdiction.

We further compare temporal patterns of case advancement prior to the experimental period and after the intervention among cases assigned to some dosage of treatment. Figure 5 plots the last pre-treatment and first post-treatment record of case advancement across all detainees assigned to treatment. In many cases, the last recorded procedural step was the arrest (segments without a blue dot). However, after the imposition of the legal assistance treatment – the start of 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 reduces shirking by officials.

5.3 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 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

[9]In A7, we document 8 reasons for non-compliance, non of which includes an unwillingness to receive the free legal services.

	Dependent variable:			
	Case Advancement During Intervention (Ordinal)			
	(1)	(2)	(3)	(4)
Panel A: ITT Effects with Continuous Treatment Assignment Indicator				
Order Quantile Among Treated	0.239^{***}	0.260^{***}	0.296^{***}	0.173
	(0.070)	(0.070)	(0.076)	(0.154)
Mean DV, Order Quantile = 0	0.077			
Treatment Range	[0, 1]	[0, 1]	[0, 1]	[0, 1]
Estimator	OLS	OLS	OLS	OLS
Panel B: LATE (Marginal Effect) of Each Additional Week of Treatment				
Weeks in Treatment	0.044^{***}	0.052^{***}	0.051^{***}	0.054
	(0.013)	(0.018)	(0.017)	(0.062)
Mean DV, 0 Weeks	0.0146			
Estimator	2SLS	2SLS	2SLS	2SLS
Treatment Range	[1, 10]	[1, 10]	[1, 10]	[1, 10]
First-Stage F-Statistic	100.53	77.55	68.18	11.65
Quintet FE	no	yes	yes	yes
IPW	yes	no	no	no
Subsample	All	All	Jurisdiction 1	Jurisdiction 2
Hypothesis Test	Upper	Upper	Upper	Upper
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 1: ITT effects of the dosage of treatment on procedural advancement (number of legal documents issued) 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. Columns 3 and 4 correspond to the two jurisdictions in which legal assistance was provided. Heteroskedasticity robust standard errors in parentheses.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.

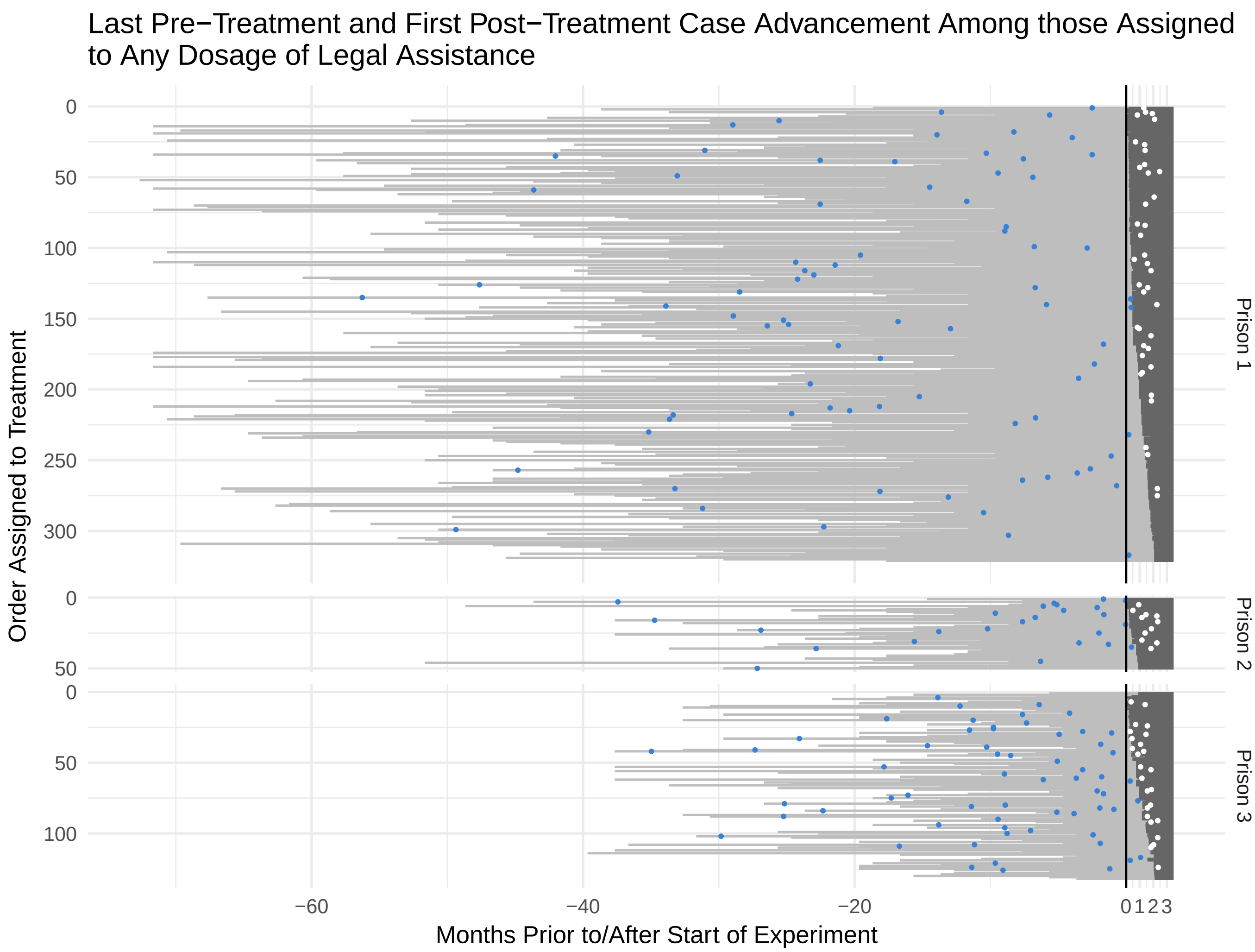

Figure 5: The relationship between the imposition of legal assistance treatment, as demarcated by a change in segment color from light gray to dark gray, and rates of case advancement. The points indicate the last pre-treatment and first post-treatment instance of case advancement. The order of treatment assignment is randomly assigned, so the length of detention prior to treatment is a pre-treatment covariate.

analysis on detainees that were imprisoned at baseline. Of the 1080 individuals in the sample, 876 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. Unsurprisingly, Appendix A6.2 suggests that baseline missingness is uncorrelated with the treatment indicator.

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 roll-out 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 nine months later with a probability of 0.136. Moving from last to first in the prison corresponds to a 6.1 percentage point increase in the probability of having been released from detention. This 44.8% ITT estimate is substantively quite large and is marginally 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 libeation 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, only encompassing two prisons ("P1" and "P3") as all individuals in the final (womens) prison were assigned to a non-zero 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 2.1 percentage point increase in the probability that a detainee was released from detention nine months later. Unsurprisingly, there is less precision when utilizing these operationalization of treatment than with the former measure that leverages variation in dosage. The use of "quintet" fixed effects increases both the estimate of the ITT and the precision of this estimate. Similar to the former measure, there is evidence of a substantively larger effect in the second prison/jurisdiction than in the first prison. In sum, the ITT analysis provides evidence that assignment to treatment, or assignment to higher doses of treatment increase the probability that detainees had been released from pretrial detention nine months later.

We now turn to assess the effects of receiving treatment on whether or not an individual was released from detention. As is standard practice, we estimate two types of LATE using two stage least squares, as described above. In the upper panel of Table 3, we utilize the rollout of the treatment to estimate the marginal effect of one 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 1.0 (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 10 to 12 percentage points. As in the ITT estimates on liberation, this effect is largest in the second jurisdiction (Column 4). In sum, we find robust support for the hypothesis that legal assistance increased the share of individuals that exited pretrial detention in nine months.

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. Again we use IPW to account for the differential probabilities of assignment to treatment in the absence of block fixed effects. We find that the CACE is positive and substantively sizable if not significant at conventional thresholds. However, the overall effect is driven by a dramatic 16 percentage point effect in the second jurisdiction (Column 4).

How large should we reasonably expect the effects on liberation to be? While the treatment-control group comparisons allow us to estimate of the rate of release within the nine months with

	Dependent variable:			
	Liberated, Nine Months Later			
	(1)	(2)	(3)	(4)
Panel A: ITT Effects with Continuous Treatment Assignment Indicator				
Order Quantile within Prison	0.061*	0.073**	0.049	0.152**
	(0.043)	(0.039)	(0.045)	(0.078)
Mean DV, Order Quantile = 0	0.136			
Treatment Range	[0, 1]	[0, 1]	[0, 1]	[0, 1]
Observations	876	876	678	198
Subsample	All	All	J1	J2
Panel B: ITT Effects with Binary Treatment Assignment Indicator				
Assigned to Treament (binary)	0.021	0.034*	0.017	0.107***
	(0.026)	(0.023)	(0.027)	(0.042)
Mean DV, Control	0.144			
Treatment Range	{0, 1}	{0, 1}	{0, 1}	{0, 1}
IPW	yes	no	no	no
Observations	830	830	632	198
Subsample	P1, P3	P1, P3	P1	J2 (P3)
Quintet FE	no	yes	yes	yes
Pre-registered Hypothesis Test	Upper	Upper	Upper	Upper
DV Scale	{0, 1}	{0, 1}	{0, 1}	{0, 1}
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 2: Intent to Treat (ITT) effects estimated using two operationalizations of the treatment indicator. 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.

	Dependent variable:			
	Liberated, Nine Months Later			
	(1)	(2)	(3)	(4)
Panel A: LATE (Marginal Effect) of Each Additional Week of Treatment				
Weeks of Legal Assistance (count)	0.010*	0.012**	0.009	0.024**
	(0.007)	(0.007)	(0.008)	(0.013)
Mean DV, Treatment = 0	0.136			
Pre-registered Hypo. Test	Upper	Upper	Upper	Upper
Treatment Range	[0, 11]	[0, 11]	[0, 11]	[0, 11]
Instrument Range	[0,1]	[0, 1]	[0,1]	[0,1]
First-Stage F-statistic	361.31	330.31	238.14	94.70
Observations	876	876	678	198
Subsample	All	All	J1	J2
Panel B: CACE of Non-Zero Dosage of Legal Assistance				
Detainee Treated (binary)	0.028	0.046*	0.023	0.160***
	(0.035)	(0.032)	(0.036)	(0.065)
Mean DV, Treatment = 0	0.144			
Treatment Range	{0, 1}	{0, 1}	{0, 1}	{0, 1}
Instrument Range	{0, 1}	{0, 1}	{0, 1}	{0, 1}
First-Stage F-statistic	1248.38	1205.59	1107.18	176.59
IPW	yes	yes	no	no
Observations	830	830	632	198
Subsample	P1, P3	P1, P3	P1	J2 (P3)
Quintet FE	no	yes	yes	yes
Pre-registered Hypo. Test	Upper	Upper	Upper	Upper
DV Scale	{0, 1}	{0, 1}	{0, 1}	{0, 1}
Note:	*p<0.1; **p<0.05; ***p<0.01			

Table 3: Two different Local Average Treatment Effects (LATEs) effects estimated using two operationalizations of the instrument (treatment assignment) and treatment (measures of compliance). 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 jurisdictions in which legal assistance was provided. Heteroskedasticity robust standard errors in parentheses.

and without legal assistance, they do little to contextualize the magnitude of the treatment effect. While we do not have an accurate estimate of the ratio of innocent to guilty defendants, we can estimate the rate of detainees that are imprisoned in illegal pretrial detention beyond the maximum sentence for the crimes that they stand accused of. In these cases, 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 a defendant is convicted. This allows us to benchmark treatment effects to the expected rates of release from detention in the case where all defendants are guilty. This should be seen as a lower bound on the conceivable share of individuals released from detention.

Consulting the Haitian Penal Code, Appendix 8 details the sentence duration for common crimes in the data set (theft, fraud, abuse of confidence, vagrancy, and begging). We estimate that 8.8% of defendants are accused of these charges *and* detained beyond the maximum sentence for these crimes. A further 2.8% of defendants are imprisoned without charges denoted in the prison register. The control rate of release of 14.4 percent (binary specification) and the ITT of 3.4 percentage points and CACE of 4.6 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. Note that this relaxation requires that a sufficient number of defendants are brought to trial during the course of the study, a challenge in the present context.

In sum, our evidence suggests that legal assistance was effective in advancing the cases of defendants held in illegal pretrial detention and indeed increased the rate of liberation within nine months. This intervention at the level of the Courts of First Instance provides evidence that interventions with state agents charged with upholding rights were effective in improving human rights performance. These findings combined with our evidence characterizing the government's objective support our assertion that the actions of "cogs" within the confines of the "machine" have a measurable impact on the violation of rights, even when there does not exist a directive to repress. While we cannot isolate the proposed causal mechanisms, the findings are consistent with the observable implications of the theory. Thus, the evidence suggests that some noncompliance with existing domestic legal protections and international human rights standards represents a manifestation of misgovernance, rather than a deliberate government policy to repress or the repressive actions of individual state agents. The legal assistance intervention altered the behavior of bureaucrats, effectively inducing legal compliance.

5.4 Heterogeneity by Case Characteristics

We now turn to the distributional impact of legal assistance by asking whether which detainees' cases are bolstered most by legal assistance. We examine the conditional effects of legal assistance on the basis of how long a detainee had been detained. In principle, shorter-term detainees' cases are easiest to process: files are relatively easier to locate and witnesses are relatively easier to summon. Nevertheless, these cases are the most likely to continue on to trial as there is more likely to be a prosecutable offense. Trials represent another source of backlogs and delays. We thus seek to examine heterogeneity in the treatment effects of legal assistance on the basis of duration of detention.

Given the heterogeneous durations of detention across prisons (Figure 5), we bin each prison into terciles of length of detention. The substantive meaning of the terciles varies by prison. We then re-estimate ITT analyses using the quantile treatment indicator. Figure 6 provides evidence that the gains from legal assistance vary on the basis of duration of illegal detention. Across both jurisdictions, shorter-term detainees benefited most from legal assistance in terms of case advancement. We argue is likely due to increased legibility (findability) or ease of prosecution: court officials advanced the least time-consuming cases at disproportionate rates.[10]

[10]Supporting this finding, Appendix 10.2 divides the cases into those that had previously advanced (arguably more legible) versus those that had not previously advanced. Table A9 indicates

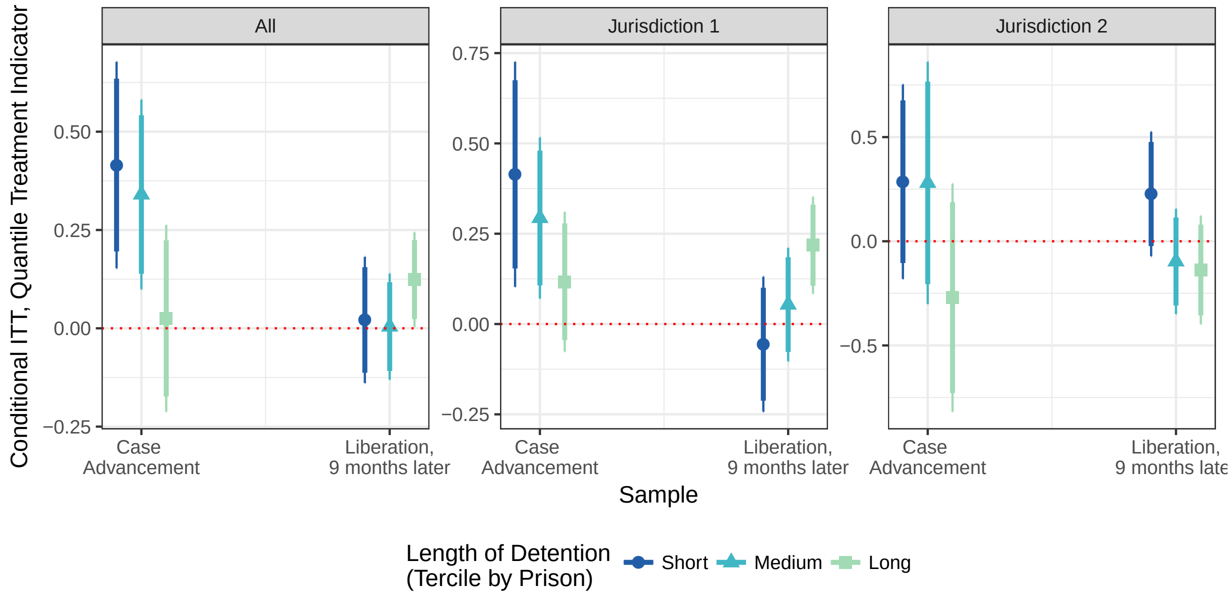

Figure 6: Conditional ITTs by duration of detention prior to experiment. While newer detainees disproportionately benefit from case advancement, older detainees are disproportionately more likely to be liberated as a result of the legal assistance treatment.

However, the second outcome in Figure 6 suggests that these benefits did not necessarily translate into differential effects on liberation. Quite the opposite, in the full sample and in Jurisdiction 1, the longest-term detainees were most likely to be liberated by endline. To the extent that liberating someone who has already served their maximal sentence requires less effort from the responsible officials, we interpret this evidence that the cases pursued by officials as a result of the lawyers' advocacy are those that are least costly. To the extent that heterogeneity in responsiveness to treatment assignment on the basis of variation in procedural complexity, these analyses provide further evidence that supports an account of illegal detention as a manifestation of misgovernance.

on average, for both groups, higher dosage was associated with a higher likelihood of case advancement. The effect of dosage appears to be substantially stronger in cases that had previously advanced procedurally. This difference in estimated ITTs is only marginally significant in the pre-specified two-tailed test.

6 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 detention, we turn to two alternative explanations for these findings. We provide evidence that no explanation, in isolation, can account for the observed findings. The first explanation is theoretical and the second considers the possible impact of violations of the stable unit treatment value assumption (SUTVA) that we invoke in interpreting the causal effects of the legal assistance intervention.

6.1 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 involved. In states with limited fiscal capacity, the simple costs of ensuring rights can be prohibitive. The costs of providing timely justice in Haiti are evident: the absence of Constitution-guaranteed public defenders is attributed to lack of public finances. Further, limited staffing to process the extent of backlogged cases may result from limited finances.

This argument is broadly supportive of our argument of misgovernance and the violation of rights but posits that rights may be undermined simply by lack of bureaucrats, not by bureaucrats' strategic choice to shirk. In this account in isolation, bureaucrats may be working at full capacity and simply not have the time or resources to process a sufficient number of cases. However, there is not evidence to support this conception of non-shirking clerks, prosecutors, and judges.

We measure absenteeism by state officials, the most common empirical measure of bureaucratic effort in developing settings (Chaudhury et al., 2006). The intervention logs collected by lawyers providing legal assistance document substantial absenteeism by all types of officials. Lawyers regularly scheduled meetings with officials of the court to consult on the status of a case. They subsequently recorded when an official was not present in the court for a scheduled meeting. From these records, we infer substantial rates of absenteeism within both the Parquet and Court. While prosecutors and judges could ostensibly be occupied outside the office, regular absenteeism by clerks bolsters our argument that absence from the office corresponds to limited effort.

We thus contribute a sobering corollary to "cost of rights" arguments. While some limitations to the capacity of Haiti's judicial institutions may well be attributable 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.

6.2 SUTVA Violations

There are two possible SUTVA violations in our design. The first is typical: a subject's potential outcomes may be a function of the treatment assignment of other subjects. Additionally, the rollout design implies a subtler SUTVA violation. Subjects' assignment to treatment depends, in part, on the resources expended by the prosecutors and judges on others with lower numbered orders in the experimental sample.[11] Nevertheless, the mechanisms underlying either type of violation are the same. We describe these two mechanisms formally in Appendix 2.

First, it is possible that cases represented by legal assistants are simply "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 simply 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.

[11]Formally, this SUTVA violation can be expressed $Z^o_{ip}(\mathbf{z}^{\mathbf{o}<o}_p)$, where o is an individual's order and p indexes the prison. This translates into a SUTVA violation because observed potential outcomes are a function of treatment assignment, i.e. $y_i(Z^o_{ip})$.

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. We rule out this explanation, at least in isolation, by considering the result presented in Figure 5. 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.

Alternatively, 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 fifteen or thirty 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 Table A13. The effects are precisely estimated zeros, giving little credence to this account of congestion. 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.

Following recent advancements in estimating treatment effects with interference allay some possible SUTVA concerns. The basic form of the (directed) interference matrix which depicts (directed) interference between units is quite sparse: units only interfere within agents (much smaller than the district level) and the direction of the interference is dictated by the rollout from those with higher order to lower order within an agent. In this case, restricted interference assumption is plausible. Following Sävje, Aronow, and Hudgens (2018), the estimator used to estimate the ITT with the binary specification is a consistent estimator of the expected ITT (EITT).[12] In sum, we provide evidence that the aforementioned SUTVA violations in isolation cannot explain the empirical patterns documented in this paper. The evidence remains consistent with a strategic role for state agents in influencing rates of illegal detention in the Haitian setting.

7 Implications for Human Rights Compliance in Haiti and Beyond

We have provided evidence that in the Haitian political context, a legal assistance intervention targeted at bureaucrats working in the criminal justice system reduced the probability that an illegally detained individual remained detained nine months later. We also document that illegal pretrial detention represents a widespread human rights abuse across many national contexts (see Appendix 1). Can instances of high rates of illegal pretrial detention beyond Haiti be explained by the same logic and change with a similar rule of law program?

Ample anecdotal evidence suggests that backlogs in court are substantial across the developing world (e.g. Dixit, 2004; Kondylis and Stein, 2018; Chemin, 2009). Institutional design in the realm of courts and the judiciary is also quite similar across contexts, at least within former colonies with the same colonizer (Glaeser and Schleifer, 2002). Indeed even broader similarities exist: Haiti's legal institutions parallel the inquisitorial systems throughout Latin America prior to the legal reforms of the past 25 years in much of the region (Cavise, 2013). These countries continue to experience relatively high rates of pretrial detention, though rates of detention have been sensitive to the aforementioned reforms in criminal procedure (Kronick, 2018). To the extent that these reforms to institutions that altered both institutional procedures or the incentives of state employees appear to influence rates of prolonged pretrial detention, they further ground our argument that such rights abuses rest squarely within the domain of bureaucrats.

[12] Sävje, Aronow, and Hudgens (2018) proves these results for the EATE of pair-randomized designs.

The effectiveness of the rule of law intervention that we document provides behavioral evidence of bureaucrats' role in a misgovernance story of human rights violations. To what extent could similar legal assistance programs address illegal pretrial detention globally? We argue that such interventions hold promise in states with (i) underutilized institutional capacity and (ii) no apparent objective to repress from the top. As in Haiti, in such settings, legal assistance may help to address the capacity deficit, by increasing effort in order to facilitate increased compliance with legal commitments (Chayes and Chayes, 1993). Further, lack of pressure to detain from above allows bureaucrats to take positive action to accelerate case trajectories without the threat of punishment. If these two boundary conditions do not hold, then a similar intervention will likely not be effective. In this case, additional regulatory steps or larger scale reforms may need to be taken to in order to effectively address this form of illegal detention (Downs, Rocke, and Barsoom, 1996). Thus, the results from our study provide insights into how new programs might be designed in institutional settings similar to Haiti but also other contexts that vary along these two important dimensions.

Moving beyond prolonged pretrial detention, to what extent does the larger misgovernance logic provide an explanation for other human rights abuses? First human rights abuses that arise because of misgovernance and a lack of bureaucratic capacity will likely become more common in practice as states move from committing to protect rights to actually complying with their legal commitments (Börzel and Risse, 2013; Risse, Ropp, and Sikkink, 2013). Second, as the repertoires of human rights practices monitored by NGOs and IGOs expands, misgovernance-rooted abuses will likely become commonly observed as well (Fariss, 2014). Current concerns about access of displaced populations to public goods such as access to secondary education introduce new issues that are broadly consistent with a misgovernance account of human rights abuse (Börzel and Risse, 2013; Risse, Ropp, and Sikkink, 2013), but demand further investigation (Khawaja, Martinez, and Esveld, 2017).

8 Conclusion

Illegal pretrial detention is a widespread human rights abuse that is far more common in terms of the number of victims than other forms of illegal imprisonment. Human rights abuses that occur because of misgovernance or a lack of bureaucratic capacity are likely to become more common in practice as states move from committing to protect rights to actually working to comply with their domestic and international legal commitments (Börzel and Risse, 2013; Chayes and Chayes, 1993; Dai, 2005; Risse, Ropp, and Sikkink, 2013; Sikkink, 2011). Yet, the near wholesale absence of this abuse from human rights theory and measurement research implies a blind spot for understanding human rights patterns more broadly. The difference in theoretical logic between human rights abuses arising from intent compared to misgovernance implies wholly different policies that human rights defenders should pursue to reduce abuses. It also curtails our understanding of the relative prevalence of these two theoretical categories of abuses. Our project represents an important first step toward remedying these conceptual issues. The findings support the role of formal legal representation interventions for improving case outcomes in criminal cases, which to date, has focused on non-criminal cases in the United States (e.g., Greiner and Pattanayak, 2012; Greiner, Pattanayak, and Hennessy, 2013).

Further, our work complements a promising new literature on policing in developing countries (Karim and Gorman, 2016; Blattman et al., 2017). While rates of illegal pretrial detention in Haiti are quite high, we document that similar conditions abound in countries in which policing interventions are planned or underway. A necessary ethical consideration revolves around the possibility that altered policing practices could change the number or types of individuals arrested, possibly subjecting populations that would not otherwise be detained to illegal pretrial detention. It also suggests that interventions on crime and provision of security must not stop at policing. Criminal justice system interventions and reforms must be studied across the set of institutions potentially affected by them.

In sum, our paper provides the first micro-level evidence that misgovernance can undermine human rights on a large scale and suggests that a more intent focus on the agents that carry out tasks related to rights abuses is warranted. In addition to well-established arguments about the "banality of evil" (intent), we propose that there is a case to be made for "banality alone" (misgovernance) as a logic for some forms of human rights abuses.

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