# Democracy and Human Rights to Personal Integrity: *History Matters*

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**Abstract**

We reconsider the dynamic relationship between democracy and human rights abuses by distinguishing the effects of repression on political competitors and mass publics. This distinction implies that democracy, conditional on past practice, may or may not influence respect for personal integrity rights. We test this claim in a first-order Markov ordered probit model of global human rights abuses covering the period 1976-2003 across multiple measures of human rights abuses and both linear (e.g. Poe and Tate (1994) and Poe, Tate and Keith (1999)) and threshold (Davenport and Armstrong 2004, Bueno de Mesquita, Downs, Smith and Cherif 2005) functional forms. We find that the effect of political democracy depends on the past history of respect for human rights. At low levels of prior repression, democracy limits human rights abuses; when past repression is targeted beyond elites, democracy has no effect on respect for personal integrity rights. Democracy alone is likely insufficient to eliminate the abuse of personal integrity rights.

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# 1 Introduction

International human rights are a subject of significant academic and popular attention.[1] Disappearances, torture, murders, and other forms of repression remain commonplace in substantial parts of Africa, the Americas, Asia, Europe, and the Middle East. Resulting from a human history littered with repression and abuse, scholars have paid critical attention to the determinants of human rights abuses with a view toward limiting a host of reprehensible practices, e.g. Apodaca (2001), Boswell and Dixon (1990), Bueno de Mesquita, Downs, Smith, and Cherif (2005), Bueno de Mesquita, Morrow, Siverson and Smith (2003), Davenport (1995, 1996*a*, 1996*b*), Davenport and Armstrong (2004), Fein (1995), Gartner and Regan (1996), Hafner-Burton (2005*a*, 2005*b*), Hafner-Burton and Tsutsui (2005), Hathaway (2002), Henderson (1991), Keith (1999, 2002), McCormick and Mitchell (1997), McKinlay and Cohan (1975), McKinlay and Cohan (1976), Mitchell and McCormick (1988), Meyer (1996), Neumayer (2005), Park (1987), Poe, Carey and Vazquez (2001), Poe, Milner and Leblang (1999), Poe and Tate (1994), Poe, Tate and Keith (1999), Richards, Gelleny and Sacko (2001), and Zanger (2000). While many of the correlates of human rights abuses are static or exceptionally difficult to change, some manipulable variables have been consistently related to decreases in human rights abuses.

No single factor is thought to more robustly discourage repression and human rights abuses than political democracy (Davenport 1995, Davenport 1999, Davenport and Armstrong 2004, Fein 1995, Henderson 1991, Mitchell and McCormick 1988, Regan and Henderson 2002, Poe and Tate 1994, Poe, Tate and Keith 1999, Zanger 2000). In both academic and popular circles, democracy is argued to be a powerful pacifying force in environments riddled with repression and terror. Despite broad agreement that there is a relationship between democracy and human rights abuses, recent scholarship on human rights and democracy debates the proper functional form. Poe and Tate (1994) and Poe, Tate and Keith (1999) find evidence of a linear relationship between democracy and human rights, Davenport and Armstrong (2004) demonstrate a threshold effect, and Bueno de Mesquita et al. (2005) demonstrate a threshold effect emanating from subdimensions of commonly

[1] We use the terms abuses of human rights, repression, and personal integrity abuses interchangeably.

used measures of democracy. This paper reorients the debate on a functional level to demonstrate that the targets of repression are an important element in understanding the relationship between human rights and democracy.

In the aftermath of recent Iraqi elections, widespread concern arose about the possibility of paybacks directed against Sunni Iraqis by formerly repressed Shiites and Kurds. Though the political system under construction is almost certain to be democratic, there remains widespread apprehension that democratic political institutions will prove insufficient to guard against the exercise of coercion. If we do not believe that democracy will prevent human rights abuses in Iraq, why should we believe that democratic institutions discourage the abuse of human rights more generally?

The influence of political democracy on human rights abuses can be distinguished by the targets of repressive instruments; for our purposes, two targets are of interest: elites and mass publics. Competition among elites for political office, a goal with some private value, provides a basis for believing that political repression has different effects on elites and mass publics.[2] The exercise of a voice in collective decision making requires two things: (i) a belief that negative consequences will not result and (ii) sufficient stakes in the outcome to participate in the collective choice. The "paradox of voting" casts doubt on the latter, by itself, but augmented by the potential for negative consequences arising from participation, there are strong reasons to believe that mass publics are easily driven away from participation. For elites, for whom holding office is a reasonable possibility, negative consequences (to participation) can never have the same deterrent effect that they have on citizens of similar political persuasions. When the citizenry need not fear the application of coercive measures, political democracy inhibits the abuse of human rights. If citizens are not free to choose without the potential for adverse consequences from participation, structural democracy is missing a key part of the causal mechanism linking democracy with the inhibition of abuse – the role of the citizenry in choosing among alternatives, replete with the possibility that the incumbents are removed.

[2]Though distinct, Bueno de Mesquita et al.'s (2003) *Logic of Political Survival* provides a motivation for considering the problem in this way by arguing that democratic societies produce considerably higher quantities of public goods, and fewer private goods, than other types of political systems.

In societies with high levels of previous abuse, there is effectively no relationship between democracy and human rights abuses because retrospective citizens fear the continued application of repressive instruments against them. In societies with low and moderate levels of prior abuse, democracy decreases levels of abuse further. In other words, where the rules of the game reflect an underlying prohibition of repressing mass publics, democracy reinforces human rights. Where such prohibitions are lacking, democracy has little meaningful impact. At the risk of oversimplifying, it is considerably harder to build demand-side relationships for good governance than it is to increase the supply of potential governors.

This paper makes three central contributions. First, we theorize that the relationship between democracy and human rights abuses depends on past history. Second, we examine appropriate techniques for ordinal time series; our tests employ ordered Markov models for discrete time series that allow the immediate past history to influence present realizations. Third, we test our theoretical claims on multiple measures of human rights abuses and on multiple conceptualizations of the relationship between democracy and human rights abuses. Because our theoretical claims run counter to much existing literature, we have maximized the potential for falsifying the empirical expectations by relying on six different indicators of human rights abuses and seven operationalizations of the effect of democracy. Virtually all of the results showcase a pattern consistent with the theoretical arguments.

One policy implication drawn from existing findings reported by a host of scholars including Poe and Tate (1994), Poe, Tate and Keith (1999), Davenport and Armstrong (2004), and Bueno de Mesquita et al. (2005) is that transforming all societies into functioning democracies would, in some finite length of time, result in a world largely free of repression. This paper demonstrates that increasing global democracy need not provide an impetus for low levels of abuse as an absorption state, given any reasonable distribution of initial conditions that mirrors the global status quo. In short, global democracy need not result in a world free of repression.

# 2 Human Rights, Past History, and Democracy

The theoretical argument is structured around three central points. First, we define the human rights under consideration. Recognizing that human rights abuses are both difficult to define and measure, we focus on broad definitions capable of cross-national generalization. After defining political democracy, we argue that changes in structural features of the polity are far more likely to immediately induce behavior in politically active citizens than in the mass public. These arguments generate hypotheses relating past history with the level of democracy to produce changes in respect for human rights in ways that differ from previous characterizations. We first turn to the object of explanation – human rights abuses.

The measurement of human rights abuse is contentious. Debate occurs between proponents of events-based approaches (Davenport 1996*a*, Davenport 1996*b*) and proponents of standards-based measures (Cingranelli and Richards 1999, McCormick and Mitchell 1997, Poe and Tate 1994, Poe, Tate and Keith 1999). Events data are measured as the number of particular types of events measured in a specified span of time. Standards-based approaches evaluate reported occurrences of a host of these events and categorize them on an ordered scale.[3] In part, these approaches reflect different concerns, frequency versus level. The frequency of particular types of abuse may be best understood by the incidence of these events, see Davenport (1999), but such studies fail to differentiate the central focus of this inquiry – the targets. For this reason, we have chosen a standards-based approach.

Within standards-based approaches, debate centers on the number of relevant dimensions and categories (Poe and Tate 1994, McCormick and Mitchell 1997, Poe, Tate and Keith 1999, Cingranelli and Richards 1999, Gibney 2004). Political Terror Scales (PTS) (Poe and Tate 1994, Poe, Tate and Keith 1999, Gibney 2004, Gibney and Dalton 1996) and more complex polychotomous scaling techniques (Cingranelli and Richards 1999) map to a single dimension, analogous to a universalist conception of human rights: all country's human rights abuses can be arrayed on a single

[3]Given certain assumptions about the process generating both, it is straightforward to show that they should generate identical conclusions, see Alt, King and Signorino (1999). It should also be noted that the data produced by Cingranelli and Richards (2004) are merely truncated events.

**Level 1**: "Countries . . . under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional . . ., political murders are rare."

**Level 2**: "There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, torture and beating are exceptional . . . political murder is rare."

**Level 3**: "There is extensive political imprisonment, or a recent history of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without trial, for political views is accepted . . ."

**Level 4**: "The practices of **Level 3** are expanded to larger numbers. Murders, disappearance are a common part of life. . . In spite of its generality, on this level terror affects primarily those who interest themselves in politics or ideas."

**Level 5**: "The terrors of **Level 4** have been expanded to the whole population. . . The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. . ."[a]

[a]Source: Poe and Tate (1994: 867); Gastil (1980), in original.

Table 1: The Political Terror Scale

dimension from minimal to maximal respect. McCormick and Mitchell (1997) argue that there are two relevant dimensions, deprivation of life (minimalist and universal) and deprivation of individual liberty (expansive and culturally determined). They argue that there are important differences among these two dimensions and that research should seek to disentangle "umbrella concepts". In an attempt to remain agnostic on measurement issues, we choose to apply our models to variants of both approaches and demonstrate that our theoretical claims are not dependent on any particular measure of human rights abuses.

Poe and Tate (1994) and Poe, Tate and Keith (1999) utilize the Purdue University "Political Terror Scale" (PTS) a five-category ordinal scale that measures human rights abuses from lowest to highest, according to increasing levels of imprisonment, torture, execution, disappearance, and more general forms of political terror. State Department and Amnesty International Reports are encoded according to five basic criteria that are reported in Table 1.

While a number of distinct policy choices by governments are included in the evaluation, the resulting scale is ordered on a single dimension. At **Level 1** and **Level 2**, a fundamental respect

for life remains, though liberties may be circumscribed. By contrast, **Level 3**[4] is increasing in brutality but still largely influences the political sphere; **Level 4**[5] expands the numbers and the deprivation of life is widespread. **Level 5**[6] represents widespread and arbitrary societal violence. Though these definitions do not make it transparent at what point members of the mass publics are being systematically repressed, it is clear that Levels 1 and 2 are distinct from Levels 4 and 5. Having cemented a notion of human rights abuses for further exploration, we now define political democracy before reconsidering the linkages between democracy and human rights abuses.

Our conceptualization of democracy comes from Robert Dahl's (1971) contemplations on polyarchy and the work of Przeworski, Alvarez, Cheibub and Limongi (2000). Both share a focus on the centrality of political competition as the defining feature of democratic governance and a shared focus on two basic groups: competitors and citizens. By competitors, we mean those who have some positive probability of governing; citizens are those endowed with the ability to choose among the competitors. To borrow terms from Przeworski et al. (2000, p. 16), we turn to an explicit discussion of contestation defined by ex ante uncertainty, ex post irreversibility, and repeatability to render the roles of competitors and citizens concrete.

*Ex ante uncertainty* suggests that the results of elections not be known before the election. This requires that either the composition of the electorate [who will actually choose] or the decisions of the particular electors [the ballot itself] not be known in advance. Implicit in ex ante uncertainty is the notion that citizens are free to decide whether or not to choose who rules them and, if they choose to take part in the decision, that their choice not be predetermined. Relatedly, *Ex post irreversibility* requires that the decision of the citizens bind the competitors. The mere contestation of an election is insufficient to denote political democracy because, as has often been the case, reversible elections need not constrain leaders in any meaningful way. More importantly,

[4]Countries receiving this rating at least 12 times during the sample period from one or both of the State Department and Amnesty International include Albania, Bahrain, Bulgaria, Bangladesh, Chile, China, Ecuador, Egypt, Haiti, Honduras, Jordan, Mexico, Morocco, Paraguay and Syria.

[5]Countries receiving this rating at least 12 times during the sample period from one or both of the State Department and Amnesty International include Brazil, Guatemala, India, Indonesia, Iran, Pakistan, Peru, the Philippines, South Africa, Sri Lanka, Turkey, and Uganda.

[6]Countries receiving this rating at least 8 times during the sample period from one or both of the State Department and Amnesty International include Algeria, Angola, Colombia, Guatemala, Iran, Rwanda, Sri Lanka, and the Sudan.

irreversibility implies that the decision of citizens from among the set of contestants provide a resolution to the competition. *Repeatability* provides the crucial mechanism of repetition to guarantee that citizens can choose among the competitors and that their choice will be reflected in who governs. These three features taken together establish an important dynamic in which people compete for office, citizens choose among the competitors, and those who might wish to lead must consider the consequences of their actions in light of uncertainty emanating from those who decide or will decide. In short, democracy is a basic interaction between those who govern, those who have the potential to govern, and those who choose the governor from the set of those with the potential to govern; "democracy is a regime in which government offices are filled by contested elections" (Przeworski et al. 2000, p. 19).

The conventional wisdom suggests that regimes in which government offices are filled by contested elections are inherently less repressive. Democratic political institutions make repression more costly while limiting the benefits from applying coercive measures. Democratic political institutions, with their well understood and relatively inexpensive methods of removing leaders from office, make repression more costly because (i) future leaders can repay repression in kind and (ii) citizens are assumed to be averse to the use of repression and willing to punish leaders that are repressive (taking advantage of the relatively inexpensive methods for changing leaders). Taken in combination, leaders of democratic polities avoid the employment of coercive tools to avoid being removed from office and to avoid potential reprisals from the coerced who may lead in the future. The aforementioned logic has guided most of the previous treatments of human rights and democracy.

Recent scholarship has debated the functional form relating human rights and democracy. A host of studies have argued that democracy is linearly related to human rights abuses. Practically, this implies that making a polity more democratic will decrease human rights abuses; in the limit, human rights abuses will disappear. Davenport and Armstrong (2004) argue that the achievement of a certain level of democracy insures that human rights abuses will be limited. Countering the linear relation, Davenport and Armstrong argue that every step that makes a polity more demo-

cratic need not translate into limitations on the exercise of coercive force. Instead, it is only at the point of fully institutionalized democracy that democracy is certain to inhibit the abuse of human rights. Bueno de Mesquita et al. (2005) argue that it is the achievement of thresholds on subdimensions of democracy – *Executive Constraints, Competitiveness of Executive Recruitment, Openness of Executive Recruitment, and Competitiveness of Participation* – that generates the improvement in the human rights records of nominal democracies. This debate over functional form has paid inadequate attention to the functional motivations for repression and their innate political justifications.

We now render a theoretical rationale for distinguishing repression applied to political competitors and mass publics. Assuming the existence of concrete benefits to office-holding, political competitors have strong incentives to enter or reenter the political arena even in the presence of repressive action. Mass publics, by contrast, derive benefits from participation in politics only to the extent that they influence resultant policies. In a nutshell, there are both nonexcludable and (potential) private benefits that accrue to legitimate contenders for political office while only the nonexcludable public consequences of a particular leader induce mass publics to political participation. Put simply, private inducements make political competitors less manipulable by coercive measures than are mass publics who must solely derive benefits (or costs) from the policies of the duly chosen leader.

From this point of departure, we should expect that past history plays a significant role in conditioning the effect of political democracy on the abuse of human rights. Where repression is applied solely against political competitors, potential leaders must weight the potential costs of being the target of repressive action against both public and private benefits should they be elected. By contrast, mass publics have only the public benefits of policies and, as is widely noted in the literature on rational turnout, only a small probability of being pivotal in determining the outcome of competition among potential office-holders. With this distinction in mind, we turn to the formation of concrete hypotheses that depend on past history.

For political competitors, there are strong and constant inducements to return to the political

fray, even in the presence of potential coercion, because the private benefits of holding office can only accrue to those that remain in the "electable" set. For this reason, past histories of repressive activity, where repression has been exclusively applied to the politically active population, should not have significant impacts in deterring the entry of political competitors. Furthermore, because the citizenry at large has not been the target of repressive activity, citizens are minimally influenced by past histories of human rights abuse. As a result, there is no reason to expect that the widely studied dynamic in which democracy decreases human rights abuses should be disrupted. Citizens do their part and political competitors are a constant feature because of private inducements. But, when repression has been applied to the mass public, we should expect this dynamic to break down.

Mass publics, given a past history of repressive activity, have lesser inducements to perform the vital function of choosing among competitors because the benefits are both widely dispersed and their probability of decisiveness is minimal. Indeed, this is where we should expect the dynamic whereby democracy reduces human rights abuses to break down; ordinary citizens have less to gain and more to potentially lose by continuing in their role as the arbiter – it is easier to simply opt out. Yet, we recall that the vital linkages between democracy and human rights abuses require an aversion to repression among the citizenry and a willingness to punish repressive leaders. When the incentives are such that punishing repressive leaders is potentially quite costly, the crucial role of citizens in guaranteeing that democracy leads to lesser repression breaks down. Borrowing the language of one prominent scholar of democracy, the repression of mass publics leads to a breakdown of "elections as instruments of democracy" because repressing mass publics undercuts the essence of democratic government: Tocqueville's sovereignty of the majority.[7] With this idea in mind, we can derive the following hypotheses.

Where past human rights abuses have solely been applied to political competitors, the necessary elements are in place to suspect that democracy limits further abuse. *With low prior levels of repression, democracy should limit human rights abuses.* However, when past human rights

[7]The conceptualization of elections as instruments of democracy owes to Powell (2000).

abuses have been applied beyond potential competitors for political office, the theoretical rationale for democracy limiting human rights abuses breaks down. As a result, there is no necessary relationship between democracy and human rights abuses in the presence of previous repression that deters the mass public from performing its vital role. *With high prior level of repression, democracy should have no effect on the incidence of human rights abuses.* With these ideas in mind, we turn to methods for testing these claims.

# 3 An Ordered Markov Model

This section develops a Markov model for ordered dependent variables. Because virtually all measures of human rights abuses are in some way discrete, special concerns arise that require appropriate models. For example, the widely used Political Terror Scale is a rating distilled from State Department or Amnesty International reports of human rights abuses. The range of this rating is the integers including one and five. Regression models for discrete phenomena are commonly supposed to arise from a latent variable regression, e.g. $y_i^* = X_i\beta + \epsilon_i$ where $y_i^*$ is an unobserved latent regressand, $X_i$ is a row vector of exogenous regressors, $\epsilon_i$ is a stochastic disturbance, and $\beta$ is a column vector of parameters relating $X_i$ to $y_i^*$. To facilitate the generation of discrete data, all that remains is $y_i = g(y_i^*)$ that maps $y_i^*$ into $y$.[8]

Markov models are surprisingly simple and treated in a variety of econometrics texts, e.g. Amemiya (1985). To specify our model, we need to focus on $X\beta$. While $X$ generally consists of exogenous and predetermined covariates, the Markov model incorporates lagged values of the dependent variable in a way similar to the work of Beck and Katz (1995, 1996). The crucial difference is that the dependent variable is discrete and the lagged dependent variable is also discrete.

To facilitate a general discussion, assume a $J$ category dependent variable; the lagged dependent variable also has $J$ categories. To appropriately capture the relationship between the present and past, define $J$ dummy variables $z_1 \ldots z_J$. For Beck and Katz (1995, 1996) [and Poe and Tate

[8]This function takes the form $Pr(y_i = j) = \int_{j-1}^{j} f(y_i^*)dy^*$ where $f$ is the density function of $\epsilon_i$.

(1994), Poe, Tate and Keith (1999), and Davenport and Armstrong (2004)], a single row of $X$ consists of $k$ truly exogenous regressors and $y_{t-1}$ where $t$ indexes time. The first-order Markov process is identical except that a single row of $X$ now consists of $k$ truly exogenous regressors and $J-1$ of the $J$ dummy variables (denoted $z$) capturing the effects of previous states.[9] In simple terms, the systematic component of the latent variable regression can be stated as $y^* = f(x_1, \ldots, x_k, z_2, \ldots, z_J)$.[10]

If we arbitrarily label democracy as $x_k$, we note that democracy only influences $y$ through a single parameter and is insufficiently flexible to capture our hypothesized effects. Fortunately, the general class of Markov transition models are sufficiently flexible that it can be adapted to test interactions between democracy and past repression.[11] Analogous to a regression model with multiplicative interactions, rewrite $y^* = f(x_1, \ldots, x_{k-1}, democracy, democracy * z_2, democracy * z_3, \ldots democracy * z_J, z_2, \ldots, z_J)$ so that the effect of democracy depends on the prior state. For example, we can interpret a regression coefficient on *democracy* as the effect of democracy when the prior state is $z_1$.[12] If we wish to know the effect of democracy when the prior state is $z_2$, (in the metric of the latent variable), this can be inferred from the coefficient on *democracy* plus the coefficient on $democracy * z_2$. Because the model can be estimated by maximum likelihood, likelihood-ratio and Wald tests can be used to test whether or not state dependent effects of *democracy* are equal to zero.

We employ the Polity IV indicator of democracy ranging from zero to ten (Marshall and Jaggers 2002). Polity measures authority patterns and the Polity IV Democracy measure is a composite of variables measuring the openness and competitiveness of executive recruitment, the degree of

[9] Only $J-1$ dummy variables can be included if $X$ is to be of full rank. Without loss of generality, we exclude $z_1$. If we label $\gamma$ to be the single scalar parameter estimating the linear effect of $y_{t-1}$ on $y_t$, this first-order Markov model is an unrestricted variant of an ordered probit with a lagged endogenous regressor. The implicit restriction is that the coefficient on lagged level two is twice the coefficient on lagged level one; the coefficient on lagged level three is three times that of lagged level one; the coefficient on lagged level four is four times that of lagged level one and twice that of lagged level two and so on.

[10] A model of this sort is estimated by Hafner-Burton (2005*b*).

[11] Because there is no reason to believe that the distance [in the latent metric] between ordered categories is constant, standard practice in estimating Markov models is to allow the exogenous variables to have effects that depend on the prior state.

[12] Because $z_1$ is omitted from the regression.

constraint on the exercise of executive authority, and regulations on political participation. While other measures are available, this measure is chosen because it is widely used in the literature, facilitating appropriate comparisons with past research, and because it is available for the broadest array of countries through time.

To fully specify the model, we have scoured prior literature for additional controls. In general, the literature suggests that economic development, economic growth, the size of a country's population, the change in the population, and involvement in civil and international wars are important determinants of human rights abuses. Relying on the International Monetary Fund's *International Financial Statistics*, we are able to measure economic development and growth and the size of a country's population (from which it is straightforward to calculate the change in population). Furthermore, the PRIO/Uppsala project has measured civil wars, internationalized civil wars, interstate wars, and other forms of violent conflict (Gleditsch, Wallensteen, Erikson, Sollenberg and Strand 2002). To follow previous literature, we employ their measures of interstate, civil, and internationalized civil war. The resulting model is remarkably similar to that estimated by Davenport and Armstrong (2004). The resulting dataset contains over 3300 observations between 1976 and 2003 on 180 countries.

The tests that we conduct will utilize two basic models and will incorporate two primary forms of tests. The first model is a Markov ordered probit as described above. In addition, we employ a random effects Markov ordered probit. If some unspecified heterogeneity in the set of countries that experience high levels of abuse were to exist, a random effects specification can prevent false attribution arising from the assumption of conditional independence given the previous states. To rule out the possibility that random country effects are driving these results, we reestimate the Markov ordered probit models with random effects. As we will show, our findings are not considerably impacted by the inclusion of random effects.

The two primary statistical tests that we will rely on are Likelihood Ratio and Wald tests. We will use Likelihood Ratio tests to examine the null hypothesis that the effect of Democracy does not depend on past history; this test is equivalent to testing the null hypothesis that the effect of

democracy is constant across *all* lagged levels of past abuse. In short, the Likelihood Ratio tests are employed here as omnibus tests of the null hypothesis that the effect of Democracy does not depend on the prior level of repression. We will also employ Wald tests to test the null hypothesis that the net effect of Democracy is equal to zero for a given lagged level of democracy. Given our construction of the models and tests, a rejection of the null hypothesis is tantamount to democracy decreasing human rights abuses *for that lagged level of abuse*, unless otherwise noted.

# 4 Testing the Model

We test our theoretical claims by estimating an ordered Markov model as specified above using the data reported in the previous paragraph. The final issue is the measures of human rights to be studied. To examine the robustness of our theoretical claims, we employ the Political Terror Scale and cross-validate the results using data on the incidence of particular human rights abuses compiled by Cingranelli and Richards (1999, 2004). We first turn to an examination of democracy and the Political Terror Scale.

Recalling that the Political Terror Scale ranges from one to five where one represents almost no human rights abuses and five represents society-wide terror, we distinguish categories one and two from categories four and five. In PTS categories one and two, political competitors are the targets of repression while in categories four and five, targets are either becoming or are the mass public.[13] Translating the hypotheses of the previous section, we should expect democracy to have no effect on human rights abuses given past values of four or five, but a net negative effect in categories one and two. We are uncertain where category three may fall. However, as we will show, classifying category three is not essential for the demonstration of the argument. Table 2 reports estimates of two Markov ordered probit models.

[13]Category three does not clearly fall into either group as it depends on how we classify those having "political views".

## 4.1 Linear Effects

The left two numeric columns in Table 2 report the results for the Political Terror Scale (PTS) coded from Amnesty International (AI) reports; the rightmost columns report estimates obtained from the United States State Department (SD).[14] $\hat{\beta}$ is the maximum likelihood estimate [in the second and fourth columns] and S.E. reports a standard error in the third and fifth columns.[15]

Overall, the models fit the data well; the likelihood that all coefficients are truly zero is almost zero ($AI : \chi^2_{16} = 1384$; $SD : \chi^2_{16} = 1468$) and almost all of the individual regressors are statistically different from zero. In short, the models provide a nice fit for the data.[16] The cutpoints separating the discrete categories ($\mu_1, \ldots, \mu_4$) are a strict order and, interpreting their standard errors, are statistically differentiable from one another. To provide evidence in favor of the ordered probit specification, Wald tests of equality of distances between categories reject the null hypothesis for at least one such comparison with $p < .005$ for all models estimated on State Department and Amnesty International Political Terror Scales.[17] With the general fit of the models in mind, we turn to the effects of the individual regressors.

The top four rows showcase intuitively pleasing results for the past values of repression. The expected level of repression is lowest when the past value is low (*Lag PTS=Low*) and the expected level of repression is consistently increasing at higher past values. *Civil Wars* increase human rights abuses as do *Internationalized Civil Wars*, while *International Wars* fail to significantly increase human rights abuses. Consistent with previous literature, economic development (proxied by *log(per capita GDP)*) discourages human rights abuses while countries with larger

[14] Poe, Carey and Vazquez (2001) assess the differences between Amnesty International and State Department reports of human rights abuses. Common practice in the literature is to substitute scores not available from one source from the other. To demonstrate that this practice has no influence on our findings, we report results from "pure" Political Terror Scales in Table 9. The left two columns reflect results derived solely from Gibney's available measures for the period 1980–2003 while the right two columns report results mixing scores coded by Gibney and Poe, Tate and Keith (1999) spanning 1976 to 2003.

[15] This standard error is the maximum of the negative inverse of the expected Hessian or the country clustered robust standard error.

[16] Before turning to the results, a note on interpretation is in order to avoid potential misconceptions. The Markov ordered probit models tell us, conditioned on a prior level of personal integrity abuses, whether higher (or lower) democracy leads to lesser, greater, or no effect on present abuses in comparison with the baseline. Democratization is the change in democracy.

[17] Formally, this is a test of the null hypothesis that $\mu_2 - \mu_1 = \mu_3 - \mu_2$, $\mu_2 - \mu_1 = \mu_4 - \mu_3$, or $\mu_3 - \mu_2 = \mu_4 - \mu_3$.

*log(Population)* engage in more human rights abuses, all other things equal. *Change in Population* has a statistically significant and positive effect on State Department measures, but no effect on Amnesty International measures; *Growth in GDP per capita* has no statistically discernible effect on either measure. With these results in mind, we turn to the influence of *Polity IV Democracy*.

Examining our central claim for Amnesty International PTS, we see that *Polity IV Democracy* is statistically and substantively significant and generally reduces human rights abuses. Examining the interactions with prior levels of human rights abuses, the effects appear to cluster. With the lowest level of past repression, *Polity IV Democracy* has a strong negative effect. In countries with low and medium levels of past repression, increasing levels of *Polity IV Democracy* still limit human rights abuses, but the effects are severely attenuated. For countries with relatively high and high levels of past repression, we find an interacted coefficient that is larger (in absolute value) than the raw effect of democracy. This implies that increases in the *Polity IV Democracy* score, in states with high levels of past repression, either increases or has no effect on the level of human rights abuses. These results contradict past literature and are uniquely explained by our theoretical differentiation between competitors and mass publics. When mass publics have been repressed, democracy no longer inhibits human rights abuses because this past repression disrupts the centrality of elections and mass participation as instruments of democracy.

For State Department PTS, we see similar dynamics. In countries with low levels of past repression, democracy still limits human rights abuses, but the effects are severely attenuated. In countries with medium, relatively high, and high levels of past repression, we find a net effect that is roughly zero. This implies that increases in the *Polity IV Democracy* score, in states with medium and high levels of past repression, have no effect on the level of human rights abuses. Once again, we find evidence that fails to falsify our central hypotheses. When mass publics have been repressed, democracy no longer inhibits human rights abuses.

To further verify the veracity of our theoretical perspective, we scrutinize whether or not the effect of democracy depends on past repression. The first test, a Likelihood-Ratio test reported in Table 2, compares the null hypothesis that the effect of democracy is independent of the prior state

against the alternative hypothesis that the effect of democracy depends on past levels of repression. The results (AI: $\chi^2 = 68.5$, 3 degrees of freedom; SD: $\chi^2 = 40.7$, 3 degrees of freedom) provide strong evidence that the Markov model provides a better fit than previous efforts, but contradicts the theoretical claims in the extant literature. Though this evidence is consistent with our central claim, the Likelihood Ratio test is an omnibus test and our central hypotheses relate to a subset of the Markov effects.[18]

We explore each conditional effect in turn using Wald tests reported in the bottom of Table 2. To reiterate, we employ Wald $\chi^2$ tests of the null hypothesis that democracy has no effect on human rights abuses conditional on the lagged level of political terror. Reading across the rows, countries with low levels of past repression allow us to strongly reject the hypothesis that democracy has no effect; democracy still inhibits the abuse of human rights in both standard and random effects Markov ordered probit models. When we move to medium levels of past repression, three tests reject [or are extraordinarily close to rejecting] the hypothesis of no effect thus indicating that democracy is at least somewhat likely to inhibit inhibit repression at medium past levels of abuse. However, when we move to high and maximal levels of prior abuse, democracy has little effect on human rights abuses.

For high levels of prior abuse, both standard and random effects ordered Markov models provide little evidence that democracy inhibits human rights abuses. The highest p-value associated with a test conditional on a high prior level of abuse suggests a one-in-four chance of having ascertained a similar test value by chance alone. Furthermore, given the estimated slope reported in the top of Table 2, this result would reject the hypothesis of no effect to conclude that democracy **increases** human rights abuses conditional on high past levels of abuse. For maximal levels of abuse, we find a similar pattern. Even accepting a one in ten chance of Type I errors, we fail to reject the hypothesis that democracy has no effect on human rights abuses given a past history of maximal abuse. Summarizing, there is no evidence to contradict our central theoretical claim, democracy

[18]Replicating the Likelihood Ratio tests for each individual effect yields a minimum $\chi^2_1$ statistic of 14.01, with an associated p-value of 0.0002.

has no impact on human rights abuses at the highest levels of past repression.[19]

Figure 1 displays the conditional effect of democracy on political terror. The top row presents results for the Amnesty International Political Terror Scales while the bottom graphics display the estimates derived from State Department Political Terror Scales. The x-axis measures changes in the Polity IV democracy score; the y-axis reports the probability that a country with median values on all of the other regressors would witness abuse at the discrete values of the Political Terror Scale [as defined in the legend at the bottom]. For example, in the far left panels of Figure 1, where past abuse is lowest, we see that increases in the level of democracy imply increasing likelihoods of the lowest levels of abuse that decrease the probability of low, medium and higher levels of abuse. In the middle left and middle panels, corresponding to low and medium levels of past abuse, there is a relatively weak dampening effect of increases in democracy on the incidence of human rights abuses. Increases in democracy weakly increase the likelihood of low and minimal levels of repression and weakly decrease the likelihood of medium, high, and maximal levels of abuse. The right two panels of both rows fail to falsify our central theoretical claim; at high and maximal levels of past abuse, democracy has no effect on the level of repression. Focusing on the Amnesty International measures, increases in *Polity IV Democracy* correlate with higher probabilities of high and maximal levels of abuse when the previous state is high and correlate with increasing probabilities of maximal levels of abuse when the previous state is maximal. For State Department Political Terror Scales, the lines are almost perfectly horizontal indicating that *Polity IV Democracy* has no effect on repression. With this evidence that does not run counter to our theoretical expectations in mind, we consider alternative conceptualizations of the relationship between democracy and human rights abuses that claim democracy must surpass some threshold to inhibit human rights abuses (Bueno de Mesquita et al. 2005, Davenport and Armstrong 2004).

[19]Readers of previous versions have suggested that the reason for these findings is the lack of variation in levels of democracy at the highest levels of lagged abuse. In response, we note that countries with high but not maximal levels of previous abuse (lagged value equals four) have higher standard deviations of the Polity score than do countries with medium prior abuse (the lagged value equals three). For example, formal F-tests of the equality of variances fail to reject the null hypothesis comparing categories three, four and five at the .05 level of statistical significance for Political Terror Scales derived from Amnesty International reports.

| | Amnesty International | | U. S. State Department | |
|---|---|---|---|---|
| **Variable** | $\hat{\beta}$ | **S.E.**[a] | $\hat{\beta}$ | **S.E.**[a] |
| Lag=Low | 0.939** | 0.189 | 1.144** | 0.160 |
| Lag=Med. | 1.897** | 0.200 | 2.212** | 0.187 |
| Lag=High | 2.838** | 0.225 | 3.416** | 0.212 |
| Lag=Max. | 3.894** | 0.275 | 4.648** | 0.283 |
| Polity IV Democracy | -0.141** | 0.023 | -0.136** | 0.021 |
| Polity*Lag=Low | 0.091** | 0.025 | 0.085** | 0.023 |
| Polity*Lag=Med. | 0.110** | 0.025 | 0.127** | 0.027 |
| Polity*Lag=High | 0.158** | 0.029 | 0.131** | 0.028 |
| Polity*Lag=Max. | 0.154** | 0.046 | 0.139* | 0.055 |
| Civil Wars | 0.779** | 0.093 | 0.833** | 0.098 |
| International Wars | 0.325 | 0.229 | 0.161 | 0.206 |
| Internationalized Civil Wars | 0.307* | 0.137 | 0.622** | 0.132 |
| Growth in GDP per capita | -0.007 | 0.004 | -0.006 | 0.004 |
| log(GDP per capita) | -0.102** | 0.024 | -0.141** | 0.024 |
| Change in Population | 0.001 | 0.018 | -0.032* | 0.015 |
| log(Population) | 0.139** | 0.020 | 0.135** | 0.020 |
| $\mu_1$ | 0.898* | 0.356 | 0.738* | 0.316 |
| $\mu_2$ | 2.810** | 0.363 | 2.722** | 0.330 |
| $\mu_3$ | 4.323** | 0.377 | 4.472** | 0.337 |
| $\mu_4$ | 5.835** | 0.405 | 6.108** | 0.361 |
| N | 3223 | | 3223 | |
| Log-likelihood | -2507.305 | | -2828.409 | |
| $\chi^2_{(16)}$ | 1384.31 | | 1468.319 | |
| **$H_0$: Effect of Democracy Does Not Depend on Past Abuses** | | | | |
| LR Test: $\chi^2_4$ | 68.5 | | 40.7 | |
| $Pr > \chi^2$ | 0.0000 | | 0.0000 | |

[a] The larger of the [country clustered] robust or asymptotic standard error.
Significance levels : † : 10% * : 5% ** : 1%

**Wald Tests of Individual Markov Effects of Democracy**

| | Amnesty International | | State Department | | Amnesty International *Random Effects* | | State Department *Random Effects* | |
|---|---|---|---|---|---|---|---|---|
| | $\chi^2_1$ | $Pr > \chi^2_1$ | $\chi^2_1$ | $Pr > \chi^2_1$ | $\chi^2_1$ | $Pr > \chi^2_1$ | $\chi^2_1$ | $Pr > \chi^2_1$ |
| Lag=Low | 26.66 | 0.00 | 18.35 | 0.00 | 33.69 | 0.00 | 34.28 | 0.00 |
| Lag=Med. | 7.68 | 0.01 | 0.33 | 0.57 | 22.07 | 0.00 | 3.82 | 0.0507 |
| Lag=High | 1.32 | 0.25 | 0.08 | 0.77 | 1.08 | .2976 | 1.26 | .2625 |
| Lag=Max | 0.24 | 0.63 | 0.00 | 0.96 | 2.70 | .1004 | 0.52 | .4686 |

Table 2: Markov Ordered Probit of Linear Functional Form

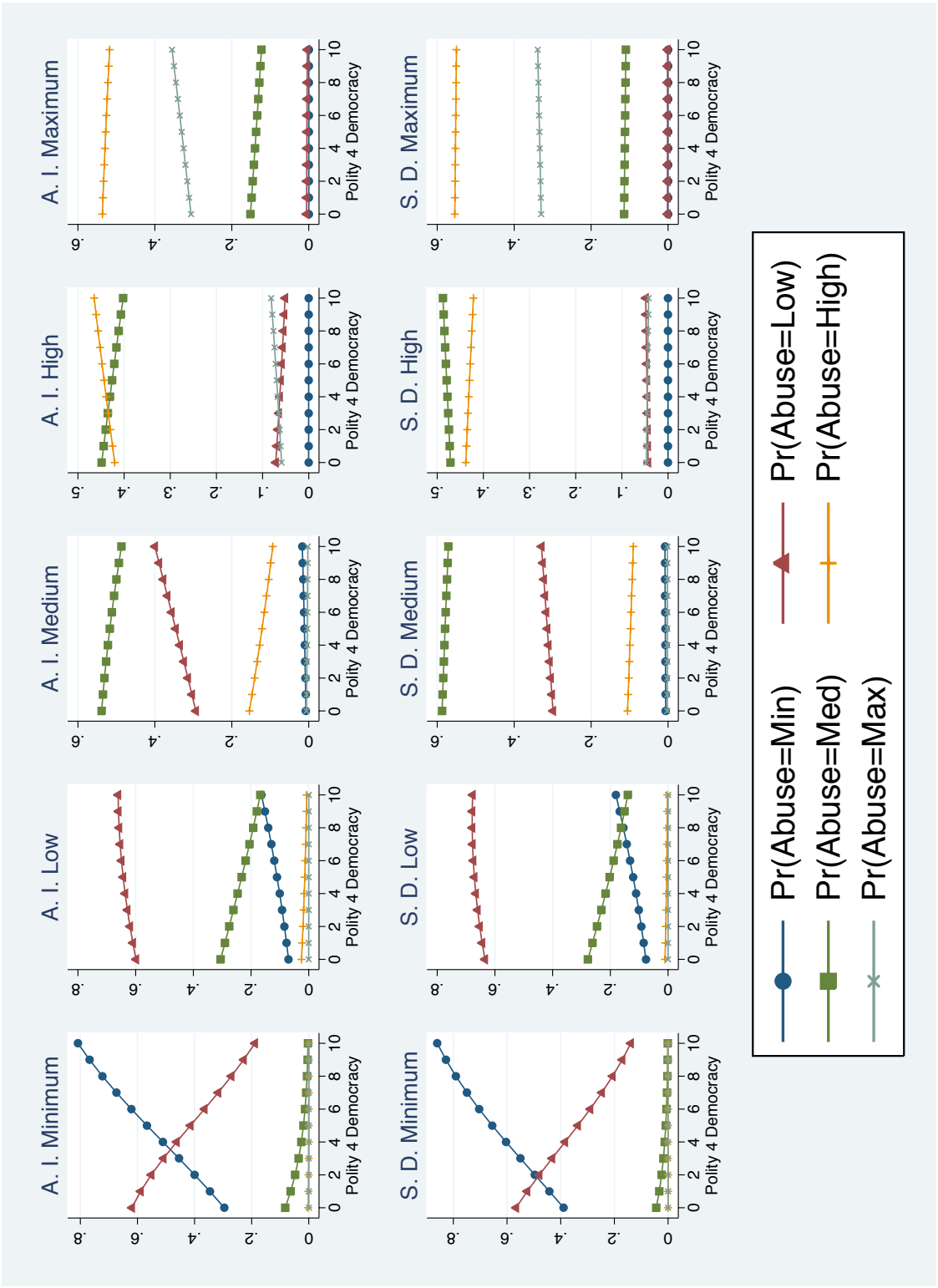


Figure 1: Markov Effects of Democracy on Human Rights Abuses

## 4.2 Threshold Effects

Davenport and Armstrong (2004) and Bueno de Mesquita et al. (2005) argue that the relationship between democracy and human rights abuses is nonlinear. Though the arguments differ slightly, they argue that countries must become "sufficiently democratic" for the pacific effects of democracy on human rights to emerge. We provide a more nuanced description before demonstrating that our arguments are robust to the functional form relating democracy and human rights.

Davenport and Armstrong (2004) identify two important thresholds. The first, where Polity scores are eight or nine, represents the first point at which democracy is first said to decrease human rights abuses. The second, Polity's most democratic countries (*Polity IV Democracy* equals 10), is said to robustly discourage the employment of political terror. With this conceptualization of thresholds based on the entire Polity scale in mind, we turn to the work of Bueno de Mesquita et al. (2005) that examines thresholds applied to the subcategories that combine to generate the entire Polity scale.

Bueno de Mesquita et al. (2005) unpack the elements of democracy and their relation to human rights abuses. Because Davenport and Armstrong (2004) utilize a threshold that is certainly related to the thresholds of Bueno de Mesquita et al. (2005), we should expect identical dynamics. The reasoning is as follows. To yield a Polity score of 10, all of the individual thresholds examined by Bueno de Mesquita et al. (2005) must be at their maximum. To be above the threshold but not maximally democratic in Davenport and Armstrong's (2004) trichotomy, at least one of the Bueno de Mesquita et al. (2005) thresholds must be zero and as many as two can be zero. Though crossing one of Davenport and Armstrong's (2004) thresholds is evidence that at least some of Bueno de Mesquita et al.'s (2005) thresholds are not zero, one cannot infer which of the Bueno de Mesquita et al. (2005) thresholds are crossed by which countries. However, there is no reason to expect that a similar pattern will not emerge with the four Polity subdimensions that Bueno de Mesquita et al. (2005) analyze.

We now test our argument that the effect of democracy, measured as a threshold effect, depends on prior levels of abuse. Whether the thresholds arise from subcategories of the Polity score or

thresholds of the aggregate Polity score, we expect these measures of democracy to manifest effects that depend on past human rights practice. With this expectation in mind, we first reexamine the claims of Davenport and Armstrong (2004).

### 4.2.1 Davenport and Armstrong's (2004) Trichotomy

A simple cross-tabulation provides face validity for our expectation. At some point during the last 30 years, 32 countries [almost $\frac{1}{5}$ of the nations under study] have crossed the Davenport and Armstrong (2004) thresholds and have experienced the highest level of personal integrity abuse, at least one from every continent with the exception of Oceania/Australia. However, this evidence is no substitute for the rigorous tests of falsifiable hypotheses reported in Table 3.[20]

The model $\chi^2$ tests the hypothesis that all variables taken together have no effect and both results, for Amnesty International and State Department Political Terror Scales, are statistically significant to the level of computer precision. The cutpoints and the effects of the dichotomizations for previous states are a strict order and can be statistically differentiated from one another implying that the model well differentiates the ordered categories. Turning to the individual effects and considering both measures of abuses of personal integrity rights, *Civil Wars* result in statistically and substantively significant increases in human rights abuses. For Amnesty International assessments, *Internationalized Civil Wars* and *International Wars* have statistically and substantively significant effects to increase the likelihood of higher levels of human rights abuses. By contrast, State Department assessments signify increasing abuses of personal integrity rights in response to *Internationalized Civil Wars* but *International Wars* appear to have no effect. Higher *Growth in GDP per capita* decreases human rights abuses, though the effect yields greater statistical confidence from Amnesty International reports than from State Department assessments. As has been the case throughout, countries with larger *log(Population)* have higher levels of human rights abuses; countries with higher *GDP per capita* have consistently lower rates of abuse of the

[20]The highest levels of prior abuse do not coexist with maximal democracy for State Department reports and only coexist with maximal democracy for Amnesty International reports for a single country: Israel. Because a dummy variable to capture a single case confounds estimation, we fold this case into those with high (but not maximal) levels of prior abuse.

human right to personal integrity. *Change in Population* exerts no significant effect on human rights abuses. With this in mind, we now turn to the effect of Davenport and Armstrong's (2004) trichotomy and the question of whether or not this effect depends on past history.

As in Davenport and Armstrong (2004), high levels of *Polity IV Democracy* appear to statistically and substantively decrease human rights abuses. At the same time, there is clear evidence that these effects depend on past levels of abuse. The Likelihood Ratio tests reported at the bottom of Table 3 make clear that the effect of Davenport and Armstrong's (2004) thresholds depend on past levels of abuse. For example, the $\chi^2$ critical value with seven degrees of freedom is 26.02 at the .0005 level of probability and both of these results are considerably larger than this value. Thus, we can safely conclude that the the effect of democracy depends on past history.[21] Let us now examine the individual estimates. For the lowest two values of past human rights abuses, *Polity IV Democracy* scores of 8 and 9 correlate with lower levels of human rights abuses. Upon reaching medium levels of abuse, these effects diminish considerably and then vanish at the highest two past levels of abuse. A similar result arises for the highest level of *Polity IV Democracy*. At the lowest level of prior abuse, the highest *Polity IV Democracy* scores is expected to reduce repression by one level. At low and medium levels of prior repression, this effect is significantly diminished. In the presence of high past repression, there is evidence that being maximally democratic increases rather than decreases personal integrity abuses. While the consensus that democracy decreases human rights abuses holds for the lowest two levels of abuse, these effects vanish conditional on medium or higher levels of past abuse.

Table 4 makes clear that the thresholds of Davenport and Armstrong (2004) do not have consistent effects across levels of past abuse. For example, at low levels of prior abuse, both thresholds exhibit statistically and substantively important decreases on the incidence of political terror. However, at all but the lowest levels of prior abuse, their thresholds have little pacifying impact on political terror. Indeed, only for the lowest levels of abuse can democracy be said to decrease political terror. The rest of the effects are zero with one interesting exception. Most surprisingly, though

[21] Individual Likelihood Ratio tests of the two thresholds also reject the null hypothesis that the effect of democracy does not depend on past history with $p < .05$.

| | Amnesty International | | U. S. State Department | |
|---|---|---|---|---|
| **Variable** | $\hat{\beta}$ | **S.E.**[a] | $\hat{\beta}$ | **S.E.**[a] |
| Lag=Low | 1.194** | 0.159 | 1.327** | 0.139 |
| Lag=Med. | 2.134** | 0.171 | 2.431** | 0.162 |
| Lag=High | 3.168** | 0.193 | 3.680** | 0.187 |
| Lag=Max. | 4.226** | 0.232 | 4.909** | 0.261 |
| Tri-1 (Polity IV=8,9) | -0.543** | 0.192 | -0.762** | 0.183 |
| Lag=Low*Tri-1 | 0.288 | 0.248 | 0.434* | 0.207 |
| Lag=Med*Tri-1 | 0.484* | 0.212 | 0.786** | 0.239 |
| Lag=High*Tri-1 | 0.601* | 0.249 | 0.577* | 0.257 |
| Lag=Max*Tri-1 | 0.564† | 0.319 | 0.672† | 0.381 |
| Tri-2 (Polity IV=10) | -1.307** | 0.204 | -1.297** | 0.195 |
| Lag=Low*Tri-2 | 0.529* | 0.236 | 0.484† | 0.258 |
| Lag=Med.*Tri-2 | 0.560 | 0.401 | 1.233* | 0.502 |
| Lag=High*Tri-2 | 2.035** | 0.478 | 1.479* | 0.708 |
| Civil Wars | 0.802** | 0.106 | 0.832** | 0.109 |
| International Wars | 0.370† | 0.224 | 0.196 | 0.207 |
| Internationalized Civil Wars | 0.334* | 0.139 | 0.635** | 0.130 |
| Growth in GDP per capita | -0.008† | 0.005 | -0.007 | 0.004 |
| log(GDP per capita) | -0.073** | 0.025 | -0.117** | 0.025 |
| Change in Population | 0.010 | 0.02 | -0.023 | 0.016 |
| log(Population) | 0.147** | 0.02 | 0.143** | 0.021 |
| | | | | |
| $\mu_1$ | 1.536** | 0.374 | 1.288** | 0.343 |
| $\mu_2$ | 3.469** | 0.381 | 3.283** | 0.356 |
| $\mu_3$ | 4.984** | 0.396 | 5.034** | 0.367 |
| $\mu_4$ | 6.497** | 0.429 | 6.668** | 0.391 |
| | | | | |
| N | 3223 | | 3223 | |
| Log-likelihood | -2813.861 | | -2496.92 | |
| $\chi^2_{(20)}$ | 1394.87 | | 1336.4 | |
| | | | | |
| **$H_0$: Effect of Trichotomy Does Not Depend on Past Abuses** | | | | |
| LR Test: $\chi^2_7$ | 42.61 | | 34.24 | |
| P-value | 0.0000 | | 0.0000 | |

[a] The larger of the [country clustered] robust or asymptotic standard error.
Significance levels : † : 10% * : 5% ** : 1%

Table 3: Markov Ordered Probit for the Davenport and Armstrong (2004) Trichotomy

| | Amnesty International | | State Department | | Amnesty International[1] | | State Department[1] | |
|---|---|---|---|---|---|---|---|---|
| | $\chi^2$ | $Pr > \chi^2_1$ | $\chi^2$ | $Pr > \chi^2_1$ | $\chi^2$ | $Pr > \chi^2_1$ | $\chi^2$ | $Pr > \chi^2_1$ |
| Trichotomy = 1[Polity IV Democracy = 8,9]: Table 3 | | | | | | | | |
| Lag = Low | 6.29 | 0.01 | 10.75 | 0.001 | 12.4 | 0.00 | 17.11 | 0.00 |
| Lag = Med. | 0.33 | 0.56 | 0.05 | 0.82 | 3.75 | 0.05 | 0.29 | 0.59 |
| Lag = High | 0.19 | 0.66 | 1.45 | 0.23 | 0.58 | 0.45 | 2.11 | 0.15 |
| Lag = Max. | 0.01 | 0.94 | 0.07 | 0.8 | 0.84 | 0.36 | 0.26 | 0.61 |
| Trichotomy = 2[Polity IV Democracy = 10]: Table 3 | | | | | | | | |
| Lag = Low | 47.76 | 0 | 36.07 | 0 | 40.7 | 0.00 | 36.9 | 0.00 |
| Lag = Med. | **3.8** | **0.05** | 0.04 | 0.85 | 6.09 | 0.01 | 0.62 | 0.43 |
| Lag = High | **4.94** | **0.03** | 0.15 | 0.69 | **2.12** | 0.15 | 0.01 | .93 |
| Lag = Max. | | | | | | | | |
| Executive Constraints Threshold: Tables 5 and 6 | | | | | | | | |
| Lag = Low | 26.59 | 0 | 26.2 | 0 | 36.81 | 0.00 | 36.15 | 0.00 |
| Lag = Med. | 5.53 | 0.01 | 4.25 | 0.039 | 17.00 | 0.00 | 9.56 | 0.002 |
| Lag = High | 0.09 | 0.77 | 0.25 | 0.62 | 0.40 | 0.526 | 0.76 | 0.384 |
| Lag = Max. | 0.02 | 0.89 | 0.41 | 0.52 | 0.52 | 0.47 | 0.07 | 0.795 |
| Competition in Executive Recruitment Threshold: Tables 5 and 6 | | | | | | | | |
| Lag = Low | 14.59 | 0.0001 | 17.95 | 0 | 17.94 | 0.00 | 25.05 | 0.00 |
| Lag = Med. | 2.8 | 0.094 | 0.08 | 0.784 | 9.66 | 0.002 | 0.31 | 0.577 |
| Lag = High | **5.97** | **0.015** | 1.54 | 0.215 | 0.04 | 0.84 | 0.42 | 0.52 |
| Lag = Max. | 0.85 | 0.36 | 0.48 | 0.488 | 1.08 | 0.298 | 0.01 | 0.92 |
| Openness of Executive Recruitment Threshold: Tables 5 and 6 | | | | | | | | |
| Lag = Low | 7.03 | 0.008 | 2.43 | 0.12 | 19.06 | 0.00 | 13.48 | 0.0002 |
| Lag = Med. | 0.23 | 0.63 | 0.03 | 0.87 | 8.76 | 0.003 | 3.10 | 0.078 |
| Lag = High | 0.82 | 0.36 | 0.22 | 0.64 | 0.41 | 0.52 | 2.84 | 0.09 |
| Lag = Max. | **3.11** | **0.077** | 0.37 | 0.54 | 0.02 | 0.88 | 0.51 | 0.476 |
| Competitiveness of Participation Threshold: Tables 5 and 6 | | | | | | | | |
| Lag = Low | 47.6 | 0 | 36.33 | 0 | 37.06 | 0.00 | 38.56 | 0.00 |
| Lag = Med. | 3.13 | 0.077 | 0.76 | 0.385 | 7.11 | 0.008 | 2.17 | 0.14 |
| Lag = High | 1.73 | 0.188 | 0.03 | 0.86 | 0.62 | 0.43 | 0.04 | 0.85 |
| Lag = Max. | | | | | | | | |

Cingranelli and Richards (2004) Measures of Repressive Instruments: Table 8

| | Disappearances | | Imprisonment | | Torture | | Extrajudicial Killing | |
|---|---|---|---|---|---|---|---|---|
| Lag = Some | 0.33 | 0.57 | 27.9 | 0.00 | 7.06 | 0.01 | **5.25** | **0.02** |
| Lag = High | 0.77 | 0.38 | 14.22 | 0.00 | 0.01 | 0.91 | **4.72** | **0.03** |
| Lag = Some[1] | 0.15 | 0.70 | 80.7 | 0.00 | 4.17 | 0.04 | 0.58 | 0.44 |
| Lag = High[1] | 0.00 | 0.97 | 35.95 | 0.00 | 0.28 | 0.60 | 0.85 | 0.36 |

Cells report: $\chi^2$ with 1 degree of freedom, $Pr > \chi^2_1$

**Boldface indicates that the effect takes the wrong sign; Democracy increases abuse.**

[1] Row/Column is estimated with random effects.

Table 4: Wald Tests: Markov Effects of Democracy on Human Rights Abuse

not inconsistent with our central claims, the results for Amnesty International reports suggest that the highest threshold of democracy (Polity = 10) **increases** rather than decreases the incidence of human rights abuses conditional on high past values of repression. This directly and strongly contradicts the arguments in Davenport and Armstrong (2004), but is not inconsistent with our arguments. As the rightmost columns in Table 4 show, random effects are insufficient to falsify our theoretical claims. For example, conditional on high or maximal previous abuse, *Polity IV Democracy* equal to 8 or 9 yield an average $\chi^2$ of less than one and an average p-value of around 0.4. In now way is there strong evidence in favor of the first level of Davenport and Armstrong's (2004) from standard or random effects Markov ordered probit models. For the highest level of Davenport and Armstrong (2004) trichotomy – countries that are maximally democratic – and high prior levels of repression, the evidence is split between support for the claim that democracy has no effect on human rights abuses and democracy **increasing** human rights abuses. While not wishing to belabor this point, we have a result that no previous perspectives can explain, providing support for our view that the effect of democracy on human rights abuses critically depends on past history. Further, we have uncovered evidence that casts serious doubt on the threshold arguments set forth in Davenport and Armstrong (2004). With these results in mind, we turn to a reanalysis of the claims of Bueno de Mesquita et al. (2005).

## 4.3 The Bueno de Mesquita, Downs, Smith, and Cherif (2005) Thresholds

Tables 5 and 6 report the results for our reanalysis of the arguments of Bueno de Mesquita et al. (2005). We will describe the effects of the controls and the general fit of the models before turning to the individual thresholds and their effects. For all eight models, the overall fit is statistically significant to the level of computer precision. The effects of lagged values of repression are statistically different both from zero and from each other and reflect a proper order. *Civil Wars* increase the likelihood of human rights abuses; *International Wars* may weakly increase human rights abuses. *Internationalized Civil Wars* clearly increase the likelihood of repression, though the substantive effect derived from Amnesty International Political Terror Scales is double the ef-

fect derived from State Department scores. *Growth in GDP per capita* weakly retards human rights abuses; *log(GDP per capita)* clearly decreases the likelihood of repression. *Changes in Population* have no clear effect on either Political Terror Scale; countries with larger *Log(Population)* are consistently more repressive. With these effects in mind, we turn to our central concern – the effects of thresholds derived from subdimensions of *Polity IV Democracy* reported in Tables 5 and 6.

Starting with the measure of *Executive Constraints*,[22] we find that at low levels of prior repression, countries with maximal constraints on the executive are considerably less repressive. However, by the time we reach high levels of prior repression, we become unable to reject the hypothesis that *Executive Constraints* have no effect on the abuse of personal integrity rights. Turning to the results reported in Table 4, we find that this pattern of effects holds for both Amnesty International and State Department derived Political Terror Scales and for standard and random effects ordered Markov models. Conditional on high or maximum levels of prior repression, we find eight Wald tests of the null hypothesis that maximum *Executive Constraints* have no effect on human rights abuses that generate a maximum $\chi^2$ statistic of 0.76. Given knowledge that the critical value at the .05 level of statistical significance is 3.84, this evidence fails to falsify our arguments that the effect of democracy depends on past history.

Similarly, when examining *Competition in Executive Recruitment*[23] in Tables 5 and 6, we find that the maximum levels of competition combine with low prior repression to improve the respect for personal integrity rights. In the presence of middling prior repression, this effect weakly decreases repression. At the same time, in the presence of high and maximal prior levels of repression, there is some evidence that the most competitive of mechanisms for choosing executives worsen the respect for personal integrity rights. More importantly, for both State Department and Amnesty International Political Terror Scales and for both standard and random effects specifications of the ordered Markov model, we find no evidence to contradict the claim that high past levels of abuse invalidate the pacific influences of subdimensions of democracy. Turning to the formal

[22]Executive Constraints provide de facto measures of the operational independence of the chief executive.

[23]Competition in executive recruitment measures the extent to which executives are chosen through competitive elections.

tests reported in Table 4, *Competition in Executive Recruitment*, at low levels of prior repression, clearly limits repression while at medium levels the results are mixed. Conditional on high and maximum levels of past repression, the eight $\chi^2$ values yield one rejection of the hypothesis that the effect of *Competition in Executive Recruitment* is zero and this result arises because the statistical evidence suggests that the conditional effect is to **worsen** human rights abuses. In short, this evidence thus fails to falsify our claims.

The *Openness of Executive Recruitment* measures the extent to which non-elites have the possibility of attaining executive office. Referencing Tables 5 and 6, maximal levels of openness showcase a similar pattern to the other elements of democracy we have previously examined. In the presence of low prior levels of repression, systems with maximally open executive recruitment lead to lower levels of human rights abuse. At medium levels of prior repression, the evidence becomes mixed. However, at high levels of past repression, even the most open of systems with respect to executive recruitment do not lead to greater respect for human rights abuses. In the presence of past maximum repression, the Amnesty International results suggest that more open executive recruitment may worsen a nation's already abysmal human rights record. Turning to the results in Table 4, across eight Wald $\chi^2$ tests, we find two weak rejections ($.05 < p < .10$) of the null hypothesis of no conditional effect that are evenly split as to whether the *Openness of Executive Recruitment* worsens or betters repression and six failures to reject the null hypothesis. Across Markov ordered probit and random effects Markov ordered probit models, we conclude that, conditional on high levels of prior repression, maximal *Openness of Executive Recruitment* has no effect on repression. With this finding in mind, we turn to the competitiveness of political participation.

The *Competitiveness of Political Participation* measures the access of non-elites to institutionalized mechanisms of political expression. Before interpreting the findings, it is important to note that Bueno de Mesquita et al. (2005, p. 444) explain that maximal levels of this variable are equivalent to societies that have multiparty systems and multiparty competition. This is important because this is the subdimension that is never maximal in the presence of the highest levels of past

| | Constraints on Executive | | Competition in Executive Recruitment | | Openness of Executive Recruitment | | Competition in Participation | |
|---|---|---|---|---|---|---|---|---|
| **Variable** | $\hat{\beta}$ | **S. E.**[a] | $\hat{\beta}$ | **S. E.**[a] | $\hat{\beta}$ | **S. E.**[a] | $\hat{\beta}$ | **S. E.**[a] |
| Lag=Low | 1.157** | 0.157 | 1.092** | 0.146 | 0.921** | 0.282 | 1.342** | 0.149 |
| Lag=Med. | 2.146** | 0.174 | 2.068** | 0.156 | 1.818** | 0.281 | 2.292** | 0.165 |
| Lag=High | 3.196** | 0.194 | 3.033** | 0.185 | 2.803** | 0.322 | 3.351** | 0.186 |
| Lag=Max. | 4.240** | 0.232 | 4.102** | 0.246 | 3.651** | 0.358 | 4.397** | 0.226 |
| Polity IV Dimension | -1.122** | 0.204 | -1.027** | 0.175 | -1.067** | 0.284 | -1.042** | 0.190 |
| Lag=Low*Polity IV | 0.678** | 0.234 | 0.729** | 0.201 | 0.840** | 0.303 | 0.352† | 0.210 |
| Lag=Med.*Polity IV | 0.857** | 0.231 | 0.881** | 0.204 | 1.026** | 0.299 | 0.565† | 0.325 |
| Lag=High*Polity IV | 1.164** | 0.288 | 1.300** | 0.219 | 1.180** | 0.325 | 1.536** | 0.492 |
| Lag=Max.*Polity IV | 1.079** | 0.369 | 1.214** | 0.336 | 1.447** | 0.372 | | |
| Civil Wars | 0.792** | 0.106 | 0.750** | 0.106 | 0.762** | 0.105 | 0.796** | 0.106 |
| International Wars | 0.321† | 0.223 | 0.321 | 0.227 | 0.266 | 0.180 | 0.366† | 0.218 |
| Internationalized Civil Wars | 0.305* | 0.135 | 0.307* | 0.136 | 0.309* | 0.123 | 0.322* | 0.141 |
| Growth in GDP per capita | -0.008† | 0.005 | -0.008† | 0.004 | -0.009* | 0.004 | -0.009† | 0.005 |
| log(GDP per capita) | -0.102** | 0.025 | -0.124** | 0.023 | -0.153** | 0.016 | -0.077** | 0.024 |
| Change in Population | 0.011 | 0.019 | 0.023 | 0.022 | 0.024 | 0.015 | 0.021 | 0.021 |
| log(Population) | 0.131** | 0.020 | 0.135** | 0.021 | 0.128** | 0.015 | 0.146** | 0.022 |
| $\mu_1$ | 1.104** | 0.379 | 0.965** | 0.370 | 0.418 | 0.444 | 1.689** | 0.381 |
| $\mu_2$ | 3.014** | 0.384 | 2.861** | 0.377 | 2.306** | 0.447 | 3.619** | 0.391 |
| $\mu_3$ | 4.524** | 0.397 | 4.375** | 0.391 | 3.817** | 0.457 | 5.135** | 0.404 |
| $\mu_4$ | 6.031** | 0.426 | 5.887** | 0.421 | 5.325** | 0.485 | 6.642** | 0.433 |
| N | 3223 | | 3223 | | 3223 | | 3223 | |
| Log-likelihood | -2829.1 | | -2839.677 | | -2858.201 | | -2825.918 | |
| $\chi^2_{(16)}$ | 1343.65 | | 1357.68 | | 1375.88 | | 1413.02 | |
| $H_0$**: Effect of Democracy Does Not Depend on Past Abuses** | | | | | | | | |
| LR Test: $\chi^2_4$ | 49.93 | | 68.07 | | 42.52 | | 25.50 | |
| P-value | 0.0000 | | 0.0000 | | 0.0000 | | 0.0000 | |

[a] The larger of the [country clustered] robust or asymptotic standard error.
Significance levels : † : 10% * : 5% ** : 1%

Table 5: Markov Ordered Probit – *Amnesty International Political Terror Scales and the Bueno de Mesquita et al. (2005) Thresholds*

| | Constraints on Executive | | Competition in Executive Recruitment | | Openness of Executive Recruitment | | Competition in Participation | |
|---|---|---|---|---|---|---|---|---|
| **Variable** | $\hat{\beta}$ | **S. E.**[a] | $\hat{\beta}$ | **S. E.**[a] | $\hat{\beta}$ | **S. E.**[a] | $\hat{\beta}$ | **S. E.**[a] |
| Lag=Low | 1.462** | 0.144 | 1.208** | 0.139 | 1.105** | 0.217 | 1.430** | 0.132 |
| Lag=Med. | 2.625** | 0.172 | 2.313** | 0.161 | 2.233** | 0.264 | 2.586** | 0.158 |
| Lag=High | 3.822** | 0.196 | 3.470** | 0.191 | 3.484** | 0.269 | 3.793** | 0.180 |
| Lag=Max. | 5.037** | 0.272 | 4.740** | 0.260 | 4.585** | 0.339 | 5.045** | 0.255 |
| Polity IV Dimension | -0.892** | 0.199 | -1.136** | 0.160 | -0.900** | 0.223 | -1.073** | 0.189 |
| Lag=Low*Polity IV | 0.424† | 0.233 | 0.799** | 0.189 | 0.773** | 0.234 | 0.357 | 0.236 |
| Lag=Med.*Polity IV | 0.653** | 0.252 | 1.162** | 0.206 | 0.885** | 0.267 | 0.804 | 0.548 |
| Lag=High*Polity IV | 0.805** | 0.275 | 1.292** | 0.208 | 0.831** | 0.261 | 1.147† | 0.648 |
| Lag=Max.*Polity IV | 1.123** | 0.374 | 1.310** | 0.415 | 1.050** | 0.373 | | |
| Civil Wars | 0.838** | 0.110 | 0.812** | 0.110 | 0.826** | 0.109 | 0.823** | 0.109 |
| International Wars | 0.172 | 0.211 | 0.174 | 0.207 | 0.113 | 0.204 | 0.203 | 0.206 |
| Internationalized Civil Wars | 0.605** | 0.129 | 0.632** | 0.131 | 0.580** | 0.128 | 0.620** | 0.131 |
| Growth in GDP per capita | -0.007† | 0.004 | -0.007 | 0.004 | -0.007† | 0.004 | -0.008† | 0.004 |
| log(GDP per capita) | -0.150** | 0.025 | -0.158** | 0.023 | -0.197** | 0.024 | -0.125** | 0.022 |
| Change in Population | -0.017 | 0.016 | -0.012 | 0.017 | 0.000 | 0.017 | -0.010 | 0.017 |
| log(Population) | 0.125** | 0.020 | 0.133** | 0.021 | 0.125** | 0.021 | 0.138** | 0.021 |
| $\mu_1$ | 0.949** | 0.336 | 0.771** | 0.320 | 0.322 | 0.373 | 1.343** | 0.346 |
| $\mu_2$ | 2.928** | 0.350 | 2.748** | 0.331 | 2.277** | 0.385 | 3.334** | 0.358 |
| $\mu_3$ | 4.681** | 0.361 | 4.500** | 0.342 | 4.027** | 0.392 | 5.083** | 0.372 |
| $\mu_4$ | 6.318** | 0.387 | 6.139** | 0.362 | 5.665** | 0.409 | 6.716** | 0.393 |
| N | 3223 | | 3223 | | 3223 | | 3223 | |
| Log-likelihood | -2518.112 | | -2506.896 | | -2543.934 | | -2504.84 | |
| $\chi^2_{(16)}$ | 1396.62 | | 1432.27 | | 1339.81 | | 1282.7 | |
| $H_0$**: Effect of Democracy Does Not Depend on Past Abuses** | | | | | | | | |
| LR Test: $\chi^2_4$ | 27.83 | | 82.82 | | 26.98 | | 13.83 | |
| P-value | 0.0000 | | 0.0000 | | 0.0000 | | 0.003 | |

[a] The larger of the [country clustered] robust or asymptotic standard error.
Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Table 6: Markov Ordered Probit – *State Department Political Terror Scales and the Bueno de Mesquita et al. (2005) Thresholds*

abuse, as measured by the State Department. However, the evidence that is available regarding the effects of multiparty competition does not differ from the patterns that we have already uncovered. Examining the evidence in Tables 5 and 6, we fail to falsify our claims. At the lowest levels of prior abuse, there is considerable evidence that multiparty competition reduces abuses of personal integrity rights. However, at high levels of prior abuse, multiparty competition has no effect on repression. Across both Political Terror Scales and the inclusion or lack of random effects, there is little evidence that the effect of democracy does not depend on past history.

Turning to the evidence in Table 4, we find four Wald $\chi^2$ statistics that all fail to reject the hypothesis that, given high levels of past repression, the conditional effect of maximum *Competitiveness of Participation* is zero. We cannot sufficiently stress the importance of this finding. Were a state to become maximally competitive on all of the subdimensions examined by Bueno de Mesquita et al. (2005), the results in Table 4 make clear that there is no reason to expect this state to eventually rid itself of a repressive past. Why? Conditional on a high level of prior repression, the net effect of all four thresholds is statistically zero and there is some evidence that these thresholds worsen human rights. Across all of the extant subdimensions and functional forms, we find no evidence to reject our hypothesis that at high levels of past repression, democracy has no effect on respect for personal integrity rights. Having explored substantial variation in the measures of democracy and the functional forms relating democracy to human rights abuses, as a final and important cross-validation, we now examine our claims on data measuring specific types of personal integrity rights: disappearances, political imprisonment, extrajudicial killing, and torture.

## 4.4 Cingranelli and Richards Data

David Cingranelli and David Richards have undertaken an impressive effort at providing disaggregated ordinal measures of the specific types of abuses, extending the earlier work of McCormick and Mitchell (1997). In particular, Cingranelli and Richards have measured specific instruments that violate personal integrity rights. As a result, we have focused on the elements used to construct the more general Political Terror Scales that we have previously analyzed: disappearances,

political imprisonment, torture, and extrajudicial killing and we rely on the same analytical model, attempting to falsify the theoretical arguments relying on new measures of human rights abuses. While this seems relatively straightforward, there are reasons to be at least a bit skeptical. Before turning to this analysis, we highlight two important caveats.

First, it is not obvious that these particular types of abuse are not mere substitutes for one another. Intuitively, if the goal of repression is to silence opposition, imprisoning the opposition is possibly equivalent to either the opposition either disappearing or perishing. Certainly, the end result of disappearance or death is functionally equivalent. Substitution effects would render the results for individual types of abuse incorrect because movements in the individual indicators need not reflect broader increases and decreases in abuse.[24] That said, there is a surprising level of consistency to the following results.

Second, the scales for all of these subset measures of personal integrity abuses range from zero to two where zero represents the highest levels of abuse, one represents occurrences of the particular abuse, and two represents no such abuses. in short, the signs of the previous results should be inverted. The operational definitions of the measures are reported in Table 7.

We describe the results for all four instruments in summary form. In all four cases, the model is statistically significant to the level of computer precision; the cutpoints are well spaced and indicative of an underlying order and the effects of covariates are consistent with those for the broader Political Terror Scales. For example, *Civil Wars* increase the incidence of all four repressive instruments, *International Wars* generally have no effect, though they do appear to decrease the incidence of torture, and *Internationalized Civil Wars* increase disappearances and extrajudicial killing but have no effect on imprisonment and torture. Overlooking the impact of *Polity IV Democracy* for a moment, countries with larger populations are more likely to experience higher levels of any instrument of repression; *Change in Population* has no effect on disappearances and torture, increases imprisonment, and decreases extrajudicial killing. Countries with higher *log(GDP per capita)* are generally less repressive, though the effect is at the margins of statistical significance

[24]This is straightforward to prove [assuming certain regularity conditions hold] employing the algebra of nonrecursive simultaneous equations.

**Disappearances** are cases in which people have disappeared, political motivation appears likely, and the victims have not been found. Knowledge of the whereabouts of the disappeared is, by definition, not public knowledge. However, while there is typically no way of knowing where victims are, it is typically known by whom they were taken and under what circumstances. . .

**Political Imprisonment** refers to the incarceration of people by government officials because of: their speech; their non-violent opposition to government policies or leaders; their religious beliefs; their non-violent religious practices including proselytizing; or their membership in a group, including an ethnic or racial group.

**Torture** refers to the purposeful inflicting of extreme pain, whether mental or physical, by government officials or by private individuals at the instigation of government officials. Torture includes the use of physical and other force by police and prison guards that is cruel, inhuman, or degrading. This also includes deaths in custody due to negligence by government officials.

**Extrajudicial killings** are killings by government officials without due process of law. They include murders by private groups if instigated by government. These killings may result from the deliberate, illegal, and excessive use of lethal force by the police, security forces, or other agents of the state whether against criminal suspects, detainees, prisoners, or others. [a]

[a] Source: Cingranelli and Richards (2004, p. 1).

Table 7: Cingranelli and Richards (2004) Measures of Repressive Instruments

for imprisonment. *Growth in GDP per capita* weakly increases disappearances and has no effect on other repressive instruments. Most important for our purposes, the hypothesis of no Markov effects can be rejected at the .01 level of statistical significance for all four instruments. With these effects in mind, we turn to the effects of *Polity IV Democracy* on the repressive instruments measured by Cingranelli and Richards (2004).

In all cases, at the lowest levels of past abuse, *Polity IV Democracy* decreases the incidence of repressive instruments. Consistent with our claims, at some and high prior levels of abuse, the pacifying effect of *Polity IV Democracy* is attenuated and the degree of attenuation is increasing in the prior level of abuse. For three of the four repressive instruments, at the highest prior level of that instrument, the net effect of *Polity IV Democracy* is zero. Only in the case of imprisonment does democracy always seem to reduce the employment of this form of repression. A host

| | DISAPPEAR-ANCES | | IMPRISON-MENT | | TORTURE | | EXTRAJUDICIAL KILLING | |
|---|---|---|---|---|---|---|---|---|
| **Variable** | $\hat{\beta}$ | **S.E.** | $\hat{\beta}$ | **S.E.** | $\hat{\beta}$ | **S.E.** | $\hat{\beta}$ | **S.E.** |
| Lag=High | -1.896** | 0.164 | -2.004** | 0.134 | -1.977** | 0.166 | -0.709** | 0.155 |
| Lag=Some | -1.020** | 0.116 | -1.024** | 0.117 | -1.086** | 0.159 | -0.256* | 0.109 |
| Civil Wars | -0.864** | 0.116 | -0.530** | 0.150 | -0.542** | 0.138 | -0.362* | 0.145 |
| International Wars | 0.355 | 0.351 | -0.025 | 0.229 | 0.440* | 0.219 | 0.398 | 0.245 |
| Internationalized Civil Wars | -0.368* | 0.152 | 0.022 | 0.151 | -0.164 | 0.145 | -0.257* | 0.149 |
| Polity IV Democracy | 0.029† | 0.015 | 0.122** | 0.015 | 0.053** | 0.020 | 0.095** | 0.016 |
| Lag=High*Democracy | -0.064 | 0.044 | -0.054* | 0.024 | -0.054* | 0.024 | -0.130** | 0.030 |
| Lag=Some*Democracy | -0.038† | 0.022 | -0.023 | 0.024 | -0.022 | 0.023 | -0.100** | 0.020 |
| log(Population) | -0.128** | 0.026 | -0.171** | 0.027 | -0.176** | 0.021 | -0.216** | 0.030 |
| Change in Population | -0.031 | 0.027 | -0.042† | 0.023 | 0.010 | 0.022 | 0.065† | 0.037 |
| log(GDP per capita) | 0.135** | 0.032 | 0.053† | 0.030 | 0.152** | 0.028 | 0.159** | 0.042 |
| Growth in GDP per capita | 0.011† | 0.006 | 0.003 | 0.004 | 0.001 | 0.005 | -0.007 | 0.006 |
| $\mu_1$ | -3.421** | 0.531 | -3.879** | 0.441 | -3.221** | 0.385 | -2.508** | 0.529 |
| $\mu_2$ | -2.261** | 0.516 | -2.421** | 0.433 | -1.370** | 0.390 | -0.925** | 0.539 |
| N | 2701 | | 2694 | | 2696 | | 2691 | |
| Log-likelihood | -1250.582 | | -1772.184 | | -1950.123 | | -1757.581 | |
| $\chi^2_{(12)}$ | 1211.353 | | 2280.644 | | 1735.024 | | 1891.396 | |
| **$H_0$: Effect of Democracy Does Not Depend on Past Abuse** | | | | | | | | |
| LR Test: $\chi^2_{2\ d.f.}$ | 8.5 | | 8.5 | | 9.94 | | 27.51 | |
| P-value | $p = 0.01$ | | $p = 0.01$ | | $p = 0.01$ | | $p = 0.00$ | |

[a] The larger of the [country clustered] robust or asymptotic standard error.

Significance levels : † : 10% * : 5% ** : 1%

Table 8: Markov Ordered Probit – *Polity IV Democracy and Cingranelli and Richards's (2004) Measures of Repressive Instruments*

of reasons for this finding are sensible. For example, it may simply be that judicial independence which frequently accompanies democratic governance alters the incentives among different instruments. Because of the necessity for judicial complicity, democratic leaders seeking to violate human rights are advantaged by the employment of disappearance, killing, and torture. While a variety of potential explanations for this finding could be presented, we need to better understand leader's motivations for the choice of particular repressive instruments. With this general description in mind, we turn to formal tests of our claims.

The bottom of Table 4 reports Wald $\chi^2$ tests of the null hypothesis that the net effect of *Polity IV Democracy* is zero for given lagged values of the repressive instruments employing standard and random effects variants of the Markov ordered probit model. In all cases, with the exception of political imprisonment which is subject to the aforementioned caveats, the highest lagged level interacts to produce a zero effect of *Polity IV Democracy* on the present incidence of the particular repressive instrument. Across numerous specifications and across a host of available measures, we have found an overwhelming array of evidence that democracy need not decrease the abuse of human rights in environments where the systematic abuse of human rights has been common in recent history.

To summarize our findings, we have reported 56 Wald $\chi^2$ tests that attempt to falsify our claim that democracy has no effect conditional on high past repression. At the .05 level of probability, we have found 6 of 56 results that reject this hypothesis. The two that are consistent with previous literature both arise from explaining variation in the level of imprisonment measured by Cingranelli and Richards (2004). The other four rejections of our central claim suggest that **increasing levels of democracy increase repression conditional on high levels of past repression**. The theory from which we derive our central research hypothesis is uniquely able to explain the preponderance of the evidence. 54 of 56 results are not inconsistent with our core argument. Given a literature that argues that democracy decreases human rights abuses, 54 of our 56 results are inconsistent with the extant literature. That 96.4% of the evidence is consistent with our claims suggests that there is merit to our approach.

# 5 Concluding Remarks

To our knowledge, this is the most comprehensive investigation of the dynamic relationship between democracy and human rights abuses. Though it is widely agreed that democracy inhibits human rights abuses, we have provided a theory to explain why democracy and past history interact to both improve and have no effect on a country's human rights record. We have subjected this claim to scrutiny using both linear and threshold effects of democracy and have employed a host of varied measures of human rights abuses. For the Political Terror Scale, in four different specifications, torture, extrajudicial killing, and disappearances, we find a strikingly similar pattern of effects that comports with our differentiation of effects. We have provided both a theory and evidence in support of the claim that democracy can reduce human rights abuses, but the effect critically depends on past practice.

This investigation does raise a few questions. First, what is the functional relationship between different types of human rights abuses – extrajudicial killing, torture, disappearances, and political imprisonment? We can envision situations in which different tactics are both substitutable and complimentary. To answer this question requires clear attention to the goals of repression and the expected impacts of different methods of abuse. When combining all elements of abuse together, we cannot hope to understand the employment of varied strategies.

Second, we have applied new models for investigating the dynamics of discrete human rights abuses. For reasons we have mentioned, variants of ordinary least squares are inappropriate for the analysis of ordered variables. Furthermore, the inclusion of lagged variables and panel corrected standard errors do nothing to remedy the underlying problem – extant measures are discrete not continuous. The methods we employ can be used to analyze a host of other dynamic problems involving panels of discrete data and to explore hypotheses that posit state dependence in the effects of relevant covariates.

Most importanly, we have offered a significant qualification of almost universally agreed findings in the quantitative study of human rights. *Democracy need not decrease human rights abuses.* We have shown that the trichotomy of Davenport and Armstrong (2004) is equally susceptible to

this critique as are more standard approaches which argue that any increase in the level of democracy correlates with decreasing levels of abuse. Of larger importance, this finding results from a focus on conditional relationships that depend on a dynamic process. Democracy has important, but conditional, influences on the respect for personal integrity rights.

This research casts significant doubt on the view that a democratic world would necessarily find itself free of human rights abuses. So long as office holding remains valuable and repression remains an effective tool for discouraging political participation by mass publics, democracy cannot be the panacea that it is widely believed to be. In short, a democratic world will not rid itself of abuses of rights to personal integrity unless augmented by other conditions that make repressive histories a thing of the past. Democracy, alone, is quite likely insufficient.

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| **Variable** | $\hat{\beta}^a$ **S. E.** | $\hat{\beta}^b$ **S. E.** | $\hat{\beta}^c$ **S. E.** | $\hat{\beta}^d$ **S. E.** |
|---|---|---|---|---|
| Lag PTS=Low | 0.633** (0.175) | 1.195** (0.134) | 0.515** (0.160) | 1.161** (0.117) |
| Lag PTS=Med. | 1.629** (0.178) | 2.245** (0.142) | 1.479** (0.162) | 2.217** (0.126) |
| Lag PTS=High | 2.615** (0.193) | 3.531** (0.168) | 2.450** (0.175) | 3.423** (0.153) |
| Lag PTS=Max. | 3.506** (0.227) | 4.724** (0.216) | 3.508** (0.210) | 4.665** (0.202) |
| Polity IV Democracy | -0.129** (0.020) | -0.127** (0.016) | -0.148** (0.019) | -0.136** (0.014) |
| Democracy*Lag PTS=2 | 0.096** (0.021) | 0.075** (0.018) | 0.108** (0.020) | 0.084** (0.016) |
| Democracy*Lag PTS=3 | 0.101** (0.023) | 0.115** (0.019) | 0.120** (0.021) | 0.126** (0.018) |
| Democracy*Lag PTS=4 | 0.147** (0.025) | 0.112** (0.023) | 0.164** (0.023) | 0.132** (0.022) |
| Democracy*Lag PTS=5 | 0.167** (0.035) | 0.127** (0.039) | 0.161** (0.033) | 0.141** (0.038) |
| Civil Wars | 0.758** (0.109) | 0.872** (0.114) | 0.770** (0.106) | 0.832** (0.110) |
| International Wars | 0.308† (0.186) | 0.112 (0.204) | 0.267 (0.183) | 0.119 (0.201) |
| Int. Civil Wars | 0.227† (0.132) | 0.661** (0.137) | 0.230† (0.127) | 0.626** (0.131) |
| Growth in GDP per capita | -0.011** (0.004) | -0.006 (0.004) | -0.009* (0.004) | -0.007† (0.004) |
| log(GDP per capita) | -0.110** (0.020) | -0.151** (0.020) | -0.090** (0.019) | -0.144** (0.019) |
| Change in Population | 0.040* (0.017) | -0.026 (0.017) | 0.037* (0.016) | -0.033* (0.016) |
| log(Population) | 0.115** (0.017) | 0.142** (0.017) | 0.115** (0.016) | 0.141** (0.016) |
| $\mu_1$ | 0.183 (0.327) | 0.820** (0.310) | 0.199 (0.311) | 0.829** (0.294) |
| $\mu_2$ | 2.146** (0.329) | 2.809** (0.314) | 2.142** (0.312) | 2.807** (0.299) |
| $\mu_3$ | 3.666** (0.335) | 4.579** (0.322) | 3.668** (0.318) | 4.574** (0.306) |
| $\mu_4$ | 5.202** (0.343) | 6.280** (0.334) | 5.180** (0.326) | 6.233** (0.318) |
| N | 2475 | 2880 | 2700 | 3174 |
| Log-likelihood | -2317 | -2254 | -2536 | -2463 |
| $\chi^2_{(16)}$ | 2519.5 | 3815.4 | 2733 | 4154 |

[a] Gibney A.I. Political Terror Scale: 1980–2003 [c] Mixed A. I. PTS: 1976–2003
[b] Gibney S. D. Political Terror Scale: 1980–2003 [d] Mixed S. D. PTS: 1976–2003
Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Table 9: Appendix: Markov Ordered Probit – "Pure" Political Terror Scales