State-dependent Dynamic Systems: A Model and Application to "Naming and Shaming" for Human Rights Abuses

Justin Esarey and Jacqueline H. R. Demeritt

September 28, 2011

Abstract

A state-dependent dynamic system is one in which the relationship between two variables x and y depends on the past level of the dependent variable y. For example, we find that smaller bilateral foreign aid provision is associated with by "naming and shaming" for human rights abuses by the United Nations, but only for states receiving low levels of aid: UN condemnation is associated with *increased* bilateral aid to states already receiving a great deal of aid. We present a methodological strategy for dealing with state-dependent dynamic systems and demonstrate the consequences of ignoring state dependence.

Introduction

Does being "named and shamed" for human rights abuse have tangible consequences for a country's standing in the international community, or is the gesture merely symbolic? Recent work finds that the impact of naming and shaming is real but limited: while international organizations and other multilateral providers cut foreign aid to countries condemned by the United Nations' Commission on Human Rights (UNCHR) or Human Rights Council (UNHRC),[1] there is no relationship between bilateral aid and UN condemnaton (Lebovic and Voeten, 2009). However, it could be the case that bilateral foreign aid providers' response to a resolution is contingent on how much aid is already being provided. Donors might give more bilateral aid to strategically important partners and less to countries in whom they

[1] The Commission on Human Rights was formally disbanded in 2006, and replaced with the Human Rights Council.

have simple humanitarian or symbolic interests. International condemnation could break symbolic relationships that are already weak, but leave untouched or reinforce strategic ones that are already strong.[2] If this is the case, a statistical analysis not designed to detect the contingent nature of the relationship between bilateral aid and human rights condemnation might conclude there is no relationship at all.

But how would we look for evidence of this relationship, if it existed? The case of naming and shaming presents one example of what we call a *state dependent dynamic system*, one in which the effect of an independent variable x on a dependent variable y depends on the past level of y. Though a few related ideas have been articulated in substantive work,[3] political scientists do not routinely consider the possibility of state dependence or its methodological consequences. In an ordinary dynamic model, such as an autodistributed lag model (Keele and Kelly, 2006; De Boef and Keele, 2008), the relationship between the current value of y and its past value has subtle and substantively meaningful implications: an immediate change in x can have effects on y that propagate into the future. But in a state-dependent system, as we show in this paper, these implications are subtler still—and theoretically interesting. For example, a state-dependent relationship between x and y implies that the effect of other variables z on y depends on the value of x—even without a direct interaction effect between x and z. We believe that explicitly recognizing and modeling state dependent dynamic systems could change how political scientists think about many substantively important processes.

In this paper, we describe the analysis of state-dependent dynamic systems (or SDDS) in the context of naming and shaming. First, we introduce the problem of naming and shaming in greater detail, giving some reasons why it might be considered an SDDS. Second, we lay out a mathematical framework and an associated empirical model for studying SDDS. Some of the substantive relationships implied by such a model are complex and non-obvious;

[2]As encapsulated in the maxim: "a friend will help you move, but a really good friend will help you move a body."

[3]For example, Franzese (2002, Chapter 3) studies the relationship between government fractionalization and budget deficits. He finds that fractionalization contributes to fiscal policy inaction, meaning that deficits expand when the accumulated debt is already high and compound interest accumulates, but stay stable or shrink when existing debts are low.

we describe how an applied researcher can present these relationships and test theoretical hypotheses. Third, we use simulation evidence to verify that a properly specified model can accurately recover an SDDS data generating process (DGP) for a continuous dependent variable in a time series cross-sectional data set of reasonable length ($T \geq 20$). We also illustrate how an analyst can test for the presence of state-dependence in an empirical dataset. Finally, we show evidence of state-dependence in the relationship between public condemnation for human rights abuse and bilateral foreign aid: UNCHR resolutions condemning human rights abuse tend to reduce bilateral aid when aid provision is already low, but increase it when it is high. We believe that this evidence merits further investigation into the nature of the relationship between strategic importance, human rights abuses, and bilateral foreign aid.

Human rights, negative publicity, and state behavior

A large body of international human rights law establishes standards of appropriate conduct for states, yet governments regularly accede to human rights treaties and then routinely violate them (e.g., Hathaway, 2002) and few international mechanisms exist to effectively sanction such violations (e.g., Hafner-Burton and Tsutsui, 2005; Neumayer, 2005). How does the international community punish states for human rights abuse?[4]

One possibility is that bilateral aid allocations are conditioned on human rights practices in receiving states. The idea that donor states will sanction repression by restricting foreign aid is intuitively appealing, but empirical findings are mixed and those that support the insight are not robust to geography, time, or method (Abrams and Lewis, 1993; Apodaca and Stohl, 1999; Carleton and Stohl, 1987; Cingranelli and Pasquarello, 1985; Neumayer, 2003*a*,*b*; Poe, 1992). Instead, existing work on foreign aid suggests that aid flows depend largely on strategic considerations, and to a lesser extent on the economic needs of the

[4]Human rights abuse, or repression, is the violation of personal integrity rights including extrajudicial killing, torture, disappearance, and political imprisonment (Cingranelli and Richards, 1999; Poe and Tate, 1994). We use the terms human rights abuse, repression, and personal/physical integrity abuse interchangeably.

recipient. Donors are self-interested actors, using aid to reward a receiving country for its trade and security links, UN voting patterns, and the like (Alesina and Dollar, 2000; Alesina and Weder, 2002; Lebovic, 1988, 2005; Meernik, Kreuger and Poe, 1998; Schraeder, Hook and Taylor, 1998). With respect to human rights, these studies find that repression is at best a modest negative predictor of bilateral aid allocations for some sending states at some points in time.

Of course, states are not the only actors capable of punishing repressive regimes. International organizations (IOs), advocacy groups, and the global news media engage in advocacy campaigns, called "naming and shaming" in the literature, that are designed to shine a spotlight on human rights abuse. The goal is to use negative publicity to pressure repressive states to better respect human rights and abide by their international commitments (e.g., Davis and Murdie, 2012; Franklin, 2008; Hafner-Burton, 2008; Ron, Ramos and Rogers, 2005). Faced with evidence that bilateral aid is unaffected by repression, scholars have begun looking for indirect links between the two. Naming and shaming provides one possible link, as publicity provides information to and political cover for donor states seeking to punish abusive recipients. Lebovic and Voeten (2009) investigate this possibility, and find that states shamed by the United Nations Commission on Human Rights (UNCHR) through public resolutions between 1979 and 2002 received less multilateral and World Bank aid than states who had not been so condemned. But UNCHR resolutions had no significant impact on bilateral aid in their study.

Lebovic and Voeten (2009) argue that naming and shaming makes abuse public and might provide a defensible reason to restrict bilateral foreign aid. But given the evidence above that strategic self-interest is a powerful motivator of state behavior, perhaps the effectiveness of naming and shaming should be considered in the context of state interests. When a state is strategically unimportant to a donor, and already receiving a comparatively smaller aid package, the donor may sanction repression by decreasing bilateral aid without sacrificing its own political and economic interests. In circumstances where strategic ties are stronger (as evidenced by large aid packages in the past), naming and shaming may not prompt donors to lower bilateral foreign aid provision. We will look for state dependence in this relationship using data from Lebovic and Voeten (2009), after we lay out a proper methodological approach.

We believe that, if supported by subsequent research, such a finding could have important substantive implications. For example, it would suggest that naming and shaming can be an effective strategy for punishing human rights abusers dependent on aid from the international community at large (e.g., from international organizations), but offenders with strong bilateral relationships may find those relationships reinforced by condemnation. This may help us to focus on naming and shaming states that are most susceptible, and to anticipate the consequences of focusing on states with strong bilateral relationships (namely, retrenchment of those relationships).

More importantly, we might gain a greater understanding of how ethical constraints function in the international system. When the international community cooperates on joint efforts (like World Bank or other multilateral aid projects), the results of Lebovic and Voeten (2009) suggest that moral arguments can be efficaceous. The community of donors, in its capacity as a collective, shows unwillingness to provide human rights abusers with assistance. For individual states, however, state dependence in this relationship would imply that ethical considerations are in tension with strategic political considerations. This does not mean that states are not influenced by moral arguments, if in fact smaller aid allocations drop in response to a human rights condemnation. However, these arguments can be ignored or actively resisted when they threaten a strategic partnership if large allocations of aid remain unchanged (or grow). All this is a way of saying that, even in the anarchy of the international system, there could be a moral code that pushes on individual state behavior—so much so that sometimes states push back still harder.

A dynamic interaction model for state-dependent dynamic systems

To see whether the relationship between foreign aid provision and human rights condemnation is state-dependent, we need a way to quantitatively study it. This includes articulating a statistical model of state-dependence, describing how to extract substantively important quantities from that model, verifying its reliability at recovering state-dependent DGPs, and finding a way to test for the presence (or absence) of state-dependence. We begin with a model.

Suppose that the relationship between an indepedent variable x_{it} and a dependent variable y_{it} for a unit i at time t is conditional on the contextual value of y at the previous time $t-1$. Where a linear DGP is appropriate, this suggests the following dynamic interaction model:

$$y_{it} = \beta_0 + \beta_1 y_{i(t-1)} + \beta_2 x_{it} + \beta_3 \left(x_{it} y_{i(t-1)} \right) + \beta_4 z_{it} + u_{it} \tag{1}$$

Here, i indexes a unit and t indexes time; the data is a time series cross-section. The relationship between x_{it} and y_{it} depends on the value of the lagged dependent variable $y_{i(t-1)}$. Suppose that β_2 and β_3 are both positive. In this case, changes in x_{it} cause y_{it} to grow, but growth is accelerated in an environment where $y_{i(t-1)}$ is already large. Other forms of state-dependence are possible, but we focus on this model because it is simple, has broad application, and the lessons learned from it apply to other models with a similar structure.[5]

The state-dependent relationship between x and y creates interesting temporal dynamics.

[5] For example, Franzese (2002, Chapter 3) employs the following error-correction model:

$$\Delta y_{it} = \beta_0 + \beta_1 \Delta y_{i(t-1)} + \beta_2 \Delta y_{i(t-2)} + \beta_3 y_{i(t-1)} + \beta_4 x_{it} + \beta_5 \left(x_{it} y_{i(t-1)} \right) + \beta_6 z_{it} + u_{it}$$

This model makes the change in y for unit i at time t a function of past changes in y, past levels of y, and exogenous variables, where the effect of certain exogenous variables is contingent on past levels. Because the exogenous variable is not interacted with the lagged DV $\left(\Delta y_{i(t-1)}\right)$, but with the aggregated sum of all previous values of the DV up to time $t-1$ $\left(\text{that is, } y_{i(t-1)}\right)$, the substantive story and dynamic implications are different than those we explore.

It is already well-understood that changes in x have long-term impacts (beyond the effect at time t) on the dependent variable through the lag coefficient, β_1, in a model with a lagged dependent variable.[6] But in a dynamic interaction model, this story is much more subtle: both the instantaneous and long-term marginal effects of x on y are highly contextual. Even more surprisingly, the long-term impact of independent variables that are not state-dependent, like z, are contingent on the level of variables with state-dependent effects like x—even without an explicit interaction term between these two variables.

The upshot is that a relatively simple theoretical concept—the idea that the effect of x on y depends on the prior state of y—has complex and important empirical implications for substantive inference that must be carefully teased out by an analyst. In this section, we illustrate how to derive the instantaneous and long-term marginal effects of independent variables, showing how these marginal effects are contextual and suggesting ways to make this contextuality clear to a reader.

Instantaneous marginal effects of x on y

In an OLS regression without state dependence, most marginal effects can be read directly off a coefficient table as a simple β coefficient. A dynamic interaction model has a more complicated marginal effect owing to the interaction term between x_{it} and $y_{i(t-1)}$ (Ai and Norton, 2003; Braumoeller, 2004; Kam and Franzese, 2007). We recommend displaying these effects using the technique of Brambor, Clark and Golder (2006): calculate the instantaneous marginal effect $\partial y_{it}/\partial dx_{it}$ and its standard error for multiple values of $y_{i(t-1)}$ using simulation, then display a plot of this relationship. Such a plot allows the reader to see how the effects of a change in the independent variable will differ in different contexts. For the model in equation 1, the instantaneous marginal effect is $\beta_2 + \beta_3 y_{i(t-1)}$.

To illustrate the process, we generated a time series cross-sectional dataset out of a DGP with $y_{it} = 0.2y_{i(t-1)} + 0.5x_{it} + 0.3 * x_{it} * y_{i(t-1)} + 0.5z_{it}$; the data set has 10 time periods and

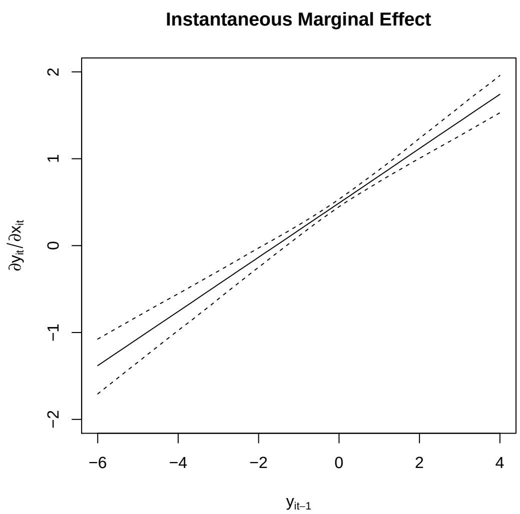
[6] See, e.g., Wilson and Butler (2007, p. 107), Keele and Kelly (2006, p. 189), and De Boef and Keele (2008).

Table 1: Model on Simulated Data

	β	s.e.
intercept	0.02695	0.02492
$y_{i(t-1)}$	0.20391	0.02742
$x_{it}y_{i(t-1)}$	0.31411	0.02572
x_{it}	0.49081	0.02210
z_{it}	0.53688	0.02156

OLS Regression Model. # of observations = 270. $R^2 = 0.8193$.

Figure 1: The Effect of x_{it} on y_{it}, from Table 1



30 units, and therefore 300 total observations.[7] Because the model includes a lag, one time period was discarded for each unit, leading to a final $N = 270$ in the regression. We estimated a correctly specified OLS regression on this data set; the results are shown in Table 1. We then drew 1000 samples out of the multivariate normal distribution of $\hat{\beta}$ using the variance-covariance matrix of the regression; for each draw, we calculated $\partial y_{it}/\partial dx_{it} = \hat{\beta}_2 + \hat{\beta}_3 y_{i(t-1)}$ for every value of $y_{i(t-1)} \in [-6, 4]$. We plot the median and 95% confidence interval of this derivative in Figure 1.

As the figure shows, changes in x_{it} can either increase or decrease y_{it} depending on the state of the world. When $y_{i(t-1)}$ is less than about -3, the marginal effect of increases in x_{it}

[7] x_{it} was drawn from the uniform distribution between -2 and 2. Starting values of y_{i1} were drawn from the uniform distribution between -1 and 1.

is negative; otherwise, the marginal effect is positive. Interpreted substantively, the DGP tends to be a self-reinforcing system: when y is already large, increases in x_{it} tend to make it even larger; when y is negative, increases in x_{it} tend to have little or even a negative effect on y.

Long term marginal effect of x on y

The first step for determining the long term marginal effect of x on y is to determine the steady state of y associated with a level of x. We can then determine how this steady state y changes as x_{it} changes. We present a technique that analysts can use to visually present long-term marginal effects for easy interpretation.

Determining a steady state y

The model in equation 1 establishes a differential equation that must be solved for y in order to determine the steady state value of y. Start by rearranging terms slightly:

$$y_{it} - y_{i(t-1)} = \beta_0 + \beta_2 x_{it} + \beta_3 \left(x_{it} y_{i(t-1)}\right) + \beta_4 z_{it} - (1 - \beta_1)\, y_{i(t-1)} + u_{it}$$

In a steady state, $y_{it} = y_{i(t-1)}$. So, setting these terms equal to a steady state y, we have:

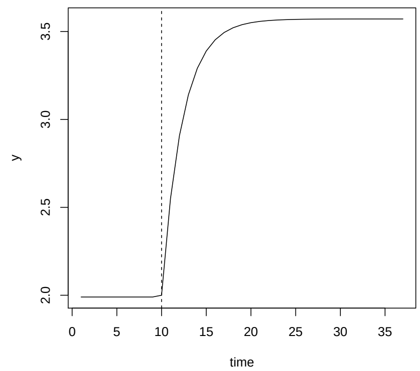
$$0 = \beta_0 + \beta_2 x_{it} + \beta_3 (x_{it} y) + \beta_4 z_{it} - (1 - \beta_1)\, y + u_{it}$$

We may now solve this equation for y (dropping the indices on x and z) and take expectations to eliminate the random term u_{it}:

$$E[y] = \frac{\beta_0 + \beta_2 x + \beta_4 z}{(1 - \beta_1 - \beta_3 x)} \tag{2}$$

Equation 2 shows the expected steady state of y associated with a particular value of x. It differs from the standard steady state calculation for a model with a lagged dependent

Figure 2: Long-term change in y after a change in x at $t = 10$



variable via the presence of the $\beta_3 x$ term in the denominator.

This process of calculating the long-term change in y caused by an instantaneous change in x_{it} is illustrated in Figure 2. The figure depicts the evolution of y_{it} for forty time periods of a model where $y_{it} = 0.2y_{i(t-1)} + 0.5x_{it} + 0.3 * x_{it} * y_{i(t-1)} + 0.5z_{it}$; we held $z_{it} = 1$. For the first ten time periods, $x = 1$ and the associated steady state $y = 2$. At $t = 10$, depicted by a dotted line in the graph, x increases to 1.5. The instantaneous change in y associated with this change in x is $(0.5 + 0.3 * 2) * 0.5 = 0.55$. However, the long-term change in y is far greater: over the next 25 periods, y increases to a new steady state of 3.57—a long term marginal effect of about 1.57.

To account for the change in the steady state x, we can use equation 2 to determine the change in the steady state of y associated with a change in x. The process is reasonably simple: calculate the steady state y for both values of x, then subtract the two. The change in steady state calculation for the change in x from 1 to 1.5 is:

$$\frac{0.5\,(1.5) + 1(0.5)}{(1 - 0.2 - 0.3\,(1.5)))} - \frac{0.5\,(1) + 1(0.5)}{(1 - 0.2 - 0.3(1)))} \approx 1.57 \tag{3}$$

Presenting long term marginal effects from an estimated model

As equations 2 and 3 make clear, the long term marginal effect of x_{it} on y depends both on the starting value of x_{it} and its ending value $x_{i(t+1)}$, not just the gap between them. The value of variables that are not state-dependent also matter, as the presence of x in the denominator does not allow the z values to cancel. Marginal effects must be calculated for changes of interest; they cannot be universally calculated for all possible changes. To illustrate this, we revisit an earlier example where we generated a time series cross-sectional dataset ($N = 30$, $T = 10$) out of a DGP with $y_{it} = 0.2y_{i(t-1)} + 0.5x_{it} + 0.3 * x_{it} * y_{i(t-1)} + 0.5z_{it}$. Using the results from Table 1, what can we predict about the change in steady state y as x changes? We used the 1000 draws of β that we simulated from the variance-covariance model to calculate the 95% confidence interval for the long term marginal effect using equation 2.

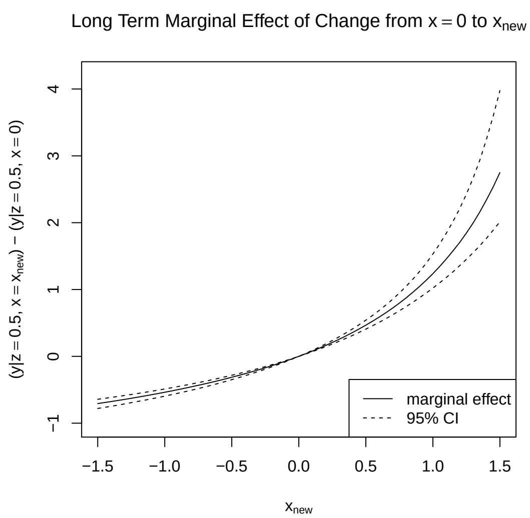
For example, suppose that a case starts at $x = 0$ and then moves to x_{new}. The marginal effect and its 95% confidence interval are presented in Figure 3 for values of x_{new} between -1.5 and 1.5. The figure shows that the marginal effect of a change in x varies greatly depending on the value of x_{new}. When x changes from 0 to -1, y declines by about 0.5. but when x changes from 0 to 1, y rises by nearly double that amount. In fact, a sufficiently large change in x can cause explosive growth; note that equation 2 for this DGP would indicate a steady state $y = \infty$ for $x \geq 2.6\overline{6}$.

Unexpected interaction among independent variables

One interesting and subtle implication of state-dependence is that independent variables (such as x and z in equation 1) have interactive effects on the steady state y even if there is no product term between them in the model. That is, the long term marginal effect of z on y is contingent on the value of x and cannot be read directly off of a coefficient table. The consequence is that marginal effects for z must be estimated using the steady state technique above, *even though no product term between x and z is present in the model.*

Equation 2 indicates that variables that are not interacted with y, which we labeled z in

Figure 3: Long Term Marginal Effect Plot for x with 95% Confidence Interval, $z = 0.5$



the previous example, are nevertheless a factor in determining the steady state value of y. If z changes from z_{lo} to z_{hi}, where $\Delta_z = z_{hi} - z_{lo}$, the change in steady state is:

$$\frac{\beta_0 + \beta_2 x + \beta_4 z_{hi}}{(1 - \beta_1 - \beta_3 x)} - \frac{\beta_0 + \beta_2 x + \beta_4 z_{lo}}{(1 - \beta_1 - \beta_3 x)} = \frac{\beta_4 \Delta_z}{(1 - \beta_1 - \beta_3 x)} \tag{4}$$

What this implies is that the long-term marginal effect of z is contingent on the value of the state dependent variable x—that is, that the effect of a change in z on y depends on the level of x. In terms of their effects on y, then, z and x are indirectly interactive. The equation also shows that changes in the steady state are contingent not on specific values of z but on the magnitude of change in that variable, which we call Δ_z. In short, the marginal effect of z on y depends both on the level of the state dependent variable x, and on the degree of change in the ordinary variable z. Thus, we should plot the long-term marginal effect for a fixed change in z at different values of x.

To illustrate the procedure, we continue using the data set and estimated model from Table 1. Using this information, we estimated the long-term marginal effect by simulating 1000 draws of β from the variance-covariance model, setting $\Delta_z = 1$, then calculating the 95% confidence interval for equation 4 for values of $x \in [-1.5, 1.5]$. The result is shown in

Figure 4: Long Term Marginal Effect Plot for z with 95% Confidence Interval

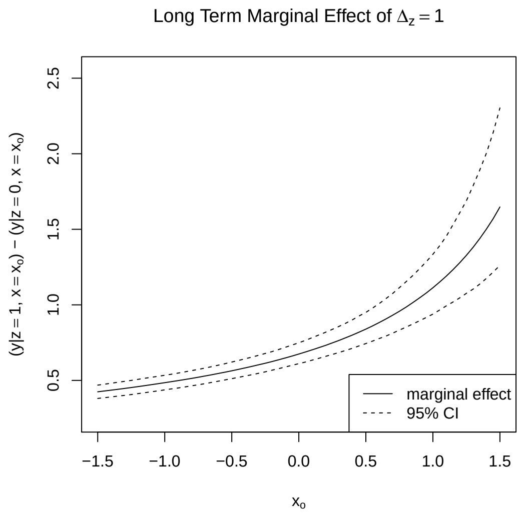


Figure 4. As the figure indicates, the long term marginal effect of a change in z on y depends on the value of x_{it}; larger values of x_{it} are associated with a larger marginal effect of changes in z_{it}.

Model performance and the consequences of misspecification

Based on the previous section, we can already conclude that ignoring state dependence is substantively harmful. When state dependence is present, the effect of changing independent variables—including variables that are not state dependent—is highly contextual. Furthermore, independent variables have both short-term and long-term effects on the dependent variable y. Without a properly specified dynamic interaction model, all these subtleties are lost. The result is that our empirical model may not be able to mirror the complexities of a dynamic theory of political interaction.

In this section, we use simulation analysis to assess: (1) whether an appropriately specified dynamic interaction model can accurately recover the data-generating process, especially in the presence of unit-specific effects; (2) the consequences of a likely misspecification; and (3) how an analyst can determine whether state dependence is present. First, past evidence suggests that models that include both a unit-specific intercept and a lagged dependent variable are intrinsically biased, though the bias is negligible in many cases (Wilson and Butler, 2007). Our simulation evidence shows that our model can correctly recover the data generating process when (1) a random-effects model is appropriate, or (2) when a fixed effects model has enough temporal observations to work with ($T \geq 20$), regardless of the number of units N. Second, we need to investigate whether the problem of ignoring state dependence is merely one of neglecting subtlety; it may be that a misspecified model can still accurately predict the dependent variable y_{it}. Simulations show that this is not the case: a misspecified model is much less capable of predicting the dependent variable. Finally, we investigate methods of detecting state dependence in a data set. Our simulations confirm that a t-test on the product term is a reasonable indicator of state dependence, while the Bayesian Information Criterion (BIC) does even better in this role.

Simulation details

For each simulation, 1000 data sets are generated out of the model:

$$y_{it} = \beta_0 + \beta_1 y_{i(t-1)} + \beta_2 x_{it} + \beta_3 \left(x_{it} y_{i(t-1)} \right) + \beta_4 z_{it} + \alpha_i + u_{it} \tag{5}$$

The resulting simulated data is a time-series cross-section (TSCS). The β_1 coefficient on y_{t-1} and the interaction coefficient β_3 are drawn from the uniform distribution between -0.4 and 0.4, while the β_2 coefficient on x_{it} and the β_4 coefficient on z_{it} are both drawn from the uniform distribution between -2 and 2. The error term u_{it} is normally distributed with zero mean and a standard deviation of 3.

Two types of simulation are run. One includes unit-specific effects α_i and no common intercept ($\beta_0 = 0$); unit effects are drawn from the uniform distribution between -3 and 3.

The other type of simulation has a common intercept β_0 drawn from the uniform distribution between -3 and 3 and sets all $\alpha_i = 0$. In both cases, we systematically vary the number of units $N \in \{10, 20, 50\}$ and the number of time periods $T \in \{5, 10, 20, 30, 40, 50\}$.

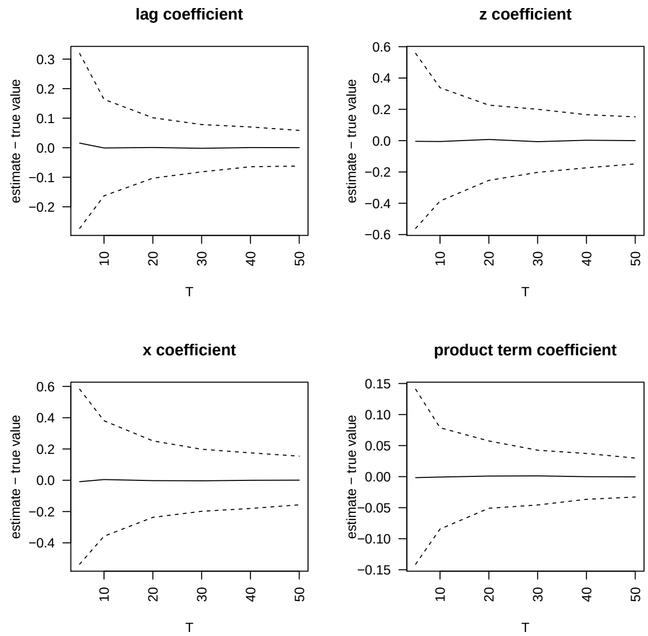
For each of the 1000 data sets, two models are fitted: one with an accurate specification, and one with a specification that drops only the interaction between x_{it} and $y_{i(t-1)}$. When unit effects are present, we consider two approaches to capturing them: a random-effects model, and a simple dummy variable specification. Results were substantively similar for simulations with and without unit-specific effects except where noted. Thus, we focus on the results from simulations with unit-specific effects.

Accurate recovery of the DGP

Our first concern is whether a properly specified model can accurately recover the data generating process. We begin by assessing a random effects model, which is appropriate for the simulated DGP because the unit effects are uncorrelated with other independent variables. Our simulation results for the smallest data sets ($N = 10$) are depicted in Figure 5; the figure shows the median bias of our 1000 simulations along with a 95% confidence interval. The simulations reveal that coefficient estimation is unbiased for all coefficients, even for the very shortest values of T, though estimate variability is considerably reduced for $T \geq 20$. This is good news for our model: when a random effects model is appropriate, dynamic interaction models can accurately recover the DGP structure from a TSCS data set.

But random effects models are not always appropriate, and coefficient bias is a special concern in the presence of a least-squares dummy variable model (Keele and Kelly, 2006; Wilson and Butler, 2007). Our simulation results for fixed effects models are similar to the results in Figure 5, except for the lag coefficient. We focus on results for the lag coefficient in Figure 6, showing results for three different values of N. The simulations indicate that the bias of the lag coefficient shrinks in increasing T and is negligible for $T \geq 20$, but is the same

Figure 5: Simulation Results from a Correctly Specified Random Effects Model, $N = 10$



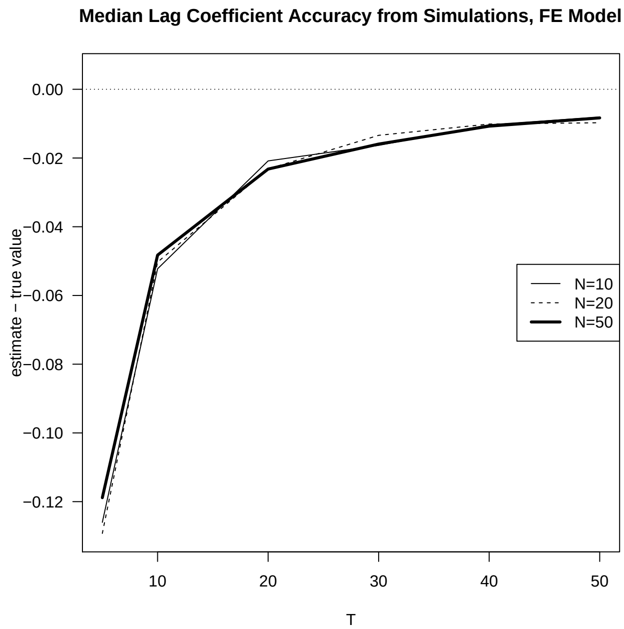
for different values of N. These results very closely conform to the earlier findings of Judson and Owen (1999), who investigate dynamic panel models and find that estimates of non-lag coefficients are generally unbiased and that bias in the lag coefficient diminishes with T.[8]

Taken as a whole, our results indicate that properly specified dynamic interaction models can recover the DGP, but are most reliable with enough temporal observations ($T \geq 20$) to allow dynamics to be properly observed. This property of the model makes intuitive sense, given the discussion of the previous section. State-dependent dynamic systems are associated with subtle, long-term dynamics; the full effects of a change in a variable may not be felt for many periods into the future. A data set must be "long" enough to see these effects unfold and model them properly.[9] This is especially important when a fixed effects model is used: "short" data sets will probably result in a biased estimate of the lag coefficient and subsequently biased marginal effects estimates.

[8]Judson and Owen recommend $T \geq 30$, rather than $T \geq 20$.

[9]This is not universally true for all dynamic panel models; see Keele and Kelly (2006) for details.

Figure 6: Lag Coefficient Bias in Fixed Effects Models, Simulation Results Varying N and T



The predictive accuracy of correct and misspecified models

Do properly specified dynamic interaction models outperform models that ignore state-dependence? To answer this question, we measured the in-sample predictive capability of models that include a product term $\left(x_{it}y_{i(t-1)}\right)$ to those that do not but are otherwise correctly specified. For each of the 1000 data sets, we calculate the root mean square error of the estimated model's prediction:

$$RMSE = \sqrt{\frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T}\left(y_{it}^{o} - \hat{y}_{it}\right)^{2}}$$

where $y_{it}^{o} = \beta_0 + \beta_1 y_{i(t-1)} + \beta_2 x_{it} + \beta_3\left(x_{it}y_{i(t-1)}\right) + \beta_4 z_{it} + \alpha_i$, the prediction of the true model excepting the error term, and $\hat{y}_{it}$ is the estimated model's prediction of the same quantity.

Figure 7 displays a comparison of the RMSE for random effects models that include the product term ("correct" models) against those that do not ("misspecified" models).[10] As the

[10]Results are very similar for fixed effect models.

Figure 7: Predictive Performance for Correctly and Incorrectly Specified, RE Models, $N = 10$

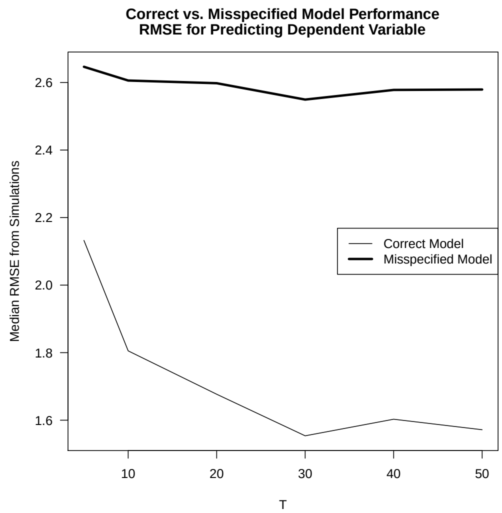


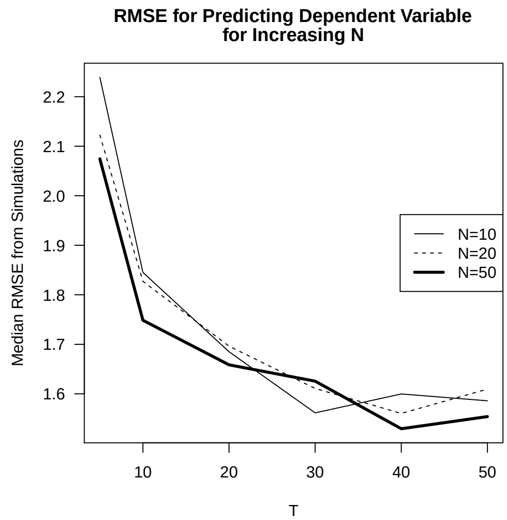
figure shows, the consequence of misspecification is poorer performance in predicting the dependent variable. For all values of T, the product term model outperforms the no-product term model. Furthermore, while the misspecified model's predictive performance remains constant for all levels of T, the correctly specified model with a product term gets better as T increases (presumably because of more efficient estimates).

It is also informative to examine the RMSE of correctly specified models for different values of N, as shown in Figure 8. The figure reiterates a lesson from the previous section: increasing the number of units N does not appreciably improve the performance of a dynamic interaction model, but increasing T does. Consequently, investigating a state dependent relationship is inadvisable with data sets shorter than $T = 20$, regardless of N.

How can analysts decide whether a relationship is state-dependent?

When state dependence is suspected, the prior subsection shows that including an interaction term between the lagged dependent variable and the relevant independent variable is essential. As a result, it is important to have a reliable procedure to determine whether state dependence is present. The statistical significance of the product term and the Bayesian Information Criterion (BIC) are both reasonable indicators of state dependence, but the BIC

Figure 8: Predictive Performance of Correctly Specified Dynamic Interaction Models, Varying N



is the best overall performer.

Our analysis is shown in Table 2. To generate this table, we use the simulation framework described in the previous section (and using all the same values for key parameters[11]) to generate 1000 data sets with $N = 10$ and $T = 20$. We then estimate a correctly specified model and determine whether the p-value on the interaction term ($\hat{\beta}_3$) is statistically significant (at $\alpha = 0.05$, two-tailed). We also estimate a random effects model without an interaction term, and compare this misspecified model to the model *with* the product term using the AIC and the BIC. This set of simulations allows us to assess the false negative rate of each of these two procedures. To assess their false positive rate, we repeat the simulations for DGPs that included no interaction term between y_{it} and x_{it} ($\beta_3 = 0$), examine the statistical significance of the product term in a model that includes one, and then compare this product term model to a no-product-term model with the AIC and BIC. The entire analysis is repeated for random and fixed effects models.

As Table 2 shows, examining the statistical significance of a product term between y_{it} and x_{it} has a reasonably low false positive rate (4.1% for RE models, 4.3% for FE models)

[11]We bound the absolute value of the product term (β_3) to be between 0.1 and 0.4 to ensure a non-zero level of state dependence.

Table 2: Specification Test Performance, $N = 10$ and $T = 20$ for RE and FE Models

	Random Effects Models	
	DGP **WITH** State Dependence	DGP **WITHOUT** State Dependence
BIC Prefers Model w/ Product Term	96.8%	0.1%
AIC Prefers Model w/ Product Term	98.8%	0.4%
Product Term is Statistically Significant	99.5%	4.1%

	Fixed Effects Models	
	DGP **WITH** State Dependence	DGP **WITHOUT** State Dependence
BIC Prefers Model w/ Product Term	99.3%	1.8%
AIC Prefers Model w/ Product Term	99.8%	15.9%
Product Term is Statistically Significant	99.4%	4.3%

and an extremely low false negative rate (less than one percent in both cases). But the BIC is much more resistant to false positives (0.1% for RE models, 1.8% for FE models) and is nearly as resistant to false negatives (96.8% for RE models, 99.5% for FE models). The AIC model has an excessive false positive rate for FE models (15.9%), and hence we do not recommend its use. On the basis of this evidence, we believe that the BIC is the best test for state dependence, with the statistical significance of the product term useful as a "quick and dirty" alternative indicator.

Does naming and shaming impact foreign aid?

Demonstrating the methodological usefulness of a dynamic interaction model is important, but we want to show that state dependence is an important theoretical concept that can enrich our understanding of substantive issues. We therefore continue the example of how "naming and shaming" of states that abuse human rights is associated with their foreign aid receipts.

To determine whether naming and shaming has a state-dependent relationship with bilateral foreign aid, we use the data set analyzed in Lebovic and Voeten (2009). Table 3 presents our results. The first column reprints the results from column 1 of Table 2 in Lebovic and Voeten (2009, 90); our replication in the second column closely matches the original printed results.[12] The third column adds an interaction term between lagged repression and UNCHR resolutions, allowing us to look for state dependence. The dependent variable in all models is the natural log per capita of bilateral aid commitments (in 2004 US dollars) from OECD countries to all 118 countries identified by the OECD as "developing countries" between 1979 and 2002.[13] The bottom row of Table 3 presents the BIC for each model.

[12]Lebovic and Voeten test the effects of CHR resolutions on bilateral, multilateral, and World Bank aid. We replicated all three models and found no evidence of state dependence with respect to multilateral aid and only weak evidence with respect to World Bank aid (which failed the BIC test but had a statistically significant coefficient on the product term). For these reasons, and because we seek to investigate the theoretical story told above, we focus here on the replication engaging bilateral aid.

[13]The dependent variable ranges from 0 to 6.64 (or \$1 to \$765 per capita). Following Lebovic and Voeten

Table 3: State-dependence in Lebovic and Voeten (2009)

DV=Log Bilateral Aid/Capita, in 2004 $	LV original	LV replicated	w/ interaction
$DV_{i(t-1)}$	0.553**	0.553**	0.539**
	(25.51)	(32.87)	(31.02)
UNCHR $Resolution_{i(t-1)}$	-0.03	-0.03	-0.392**
	(0.51)	(0.51)	(3.36)
$DV_{i(t-1)}$*UNCHR $Res_{i(t-1)}$			0.136**
			(3.60)
ΔPersonal Integrity Abuse	-0.037*	-0.037*	-0.038*
	(2.20)	(2.20)	(2.27)
Personal Integrity $Abuse_{i(t-1)}$	-0.018	-0.018	-0.019
	(0.96)	(0.96)	(1.05)
ΔCivil liberties	-0.015	-0.015	-0.015
	(0.80)	(0.80)	(0.78)
Civil $Liberties_{i(t-1)}$	-0.02	-0.02	-0.019
	(1.6)	(1.62)	(1.54)
Ln(GDP per $capita_{i(t-1)}$)	-0.107**	-0.107**	-0.117**
	(4.26)	(4.38)	(4.82)
Ln($population_{i(t-1)}$)	-0.162**	-0.162**	-0.159**
	(5.23)	(5.34)	(5.34)
Agreement with $USA_{i(t-1)}$	-0.123	-0.123	-0.145
	(0.85)	(0.86)	(1.02)
War	-0.088*	-0.088*	-0.097**
	(2.54)	(2.55)	(2.83)
Capabilities	-0.759	-0.759	-0.839
	(1.25)	(1.25)	(1.41)
Time (linear)	-0.005**	-0.005**	-0.005**
	(4.41)	(4.48)	(4.51)
Time (quadratic)	-0.00	-0.00	-0.00
	(0.54)	(0.54)	(0.44)
Constant	4.903**	4.903**	4.971**
	(8.66)	(8.83)	(89.08)
Observations	2324	2324	2324
Number of countries	118	118	118
Average observations per country		19.7	19.7
BIC		3705	3700

*p≤.05; **p≤.01 (two-tailed tests). Main entries are random effects MLE regression coefficients; Absolute value of z-statistics in parentheses.

As the table shows, the BIC for the dynamic interaction model is smaller than for the straight replication. In addition, the interaction term between the lagged DV and UNCHR resolutions is statistically significant. Both of these indications suggest that the relationship between UNCHR resolutions and bilateral aid is dependent on the current level on bilateral aid.

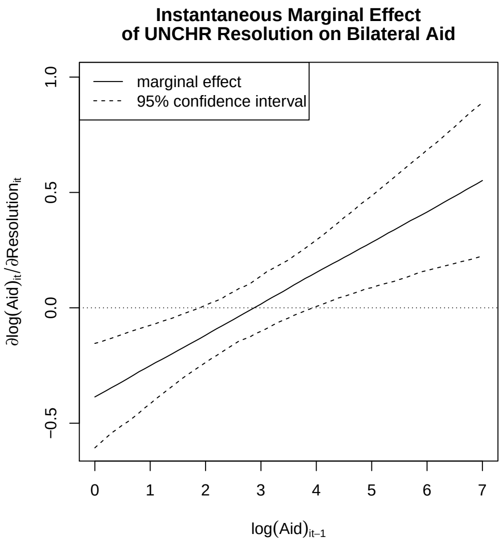
Instantaneous marginal effect of naming & shaming on bilateral aid

Figure 9 presents the immediate impact of a UNCHR resolution on bilateral aid, and shows how the marginal effect of such a resolution is associated with the level of previous aid. When the natural log of lagged bilateral aid is less than three (about $20 per capita), a CHR resolution shaming a state for human rights abuse is associated with a *decrease* in current aid. When the natural log of lagged aid is greater than three, the relationship inverts so that a CHR resolution is associated with an *increase* in bilateral aid.

When a receiving state has gotten little aid from donors in the past, and that state is shamed for abuse, it appears (in this data set) that the sending states respond by decreasing the amount of aid they send in the future. We think this is because relationships that are of low strategic importance derive their value from humanitarian or symbolic considerations, both of which are undermined when the abuse of a recipient government is made public. In these cases, the sending states attempt to punish the recipient in the face of public condemnation. But when a receiving state has gotten more aid from donors in the past, and that state is shamed for human rights abuse, the donor states respond by *increasing* aid in the next period. We think this is because the aid relationship is strategically important, and the donor states attempt to buttress their partner in the face of public condemnation.

(2009, p. 86), we exclude Egypt and Israel "to acknowledge the unique aid trajectory and amounts for these countries."

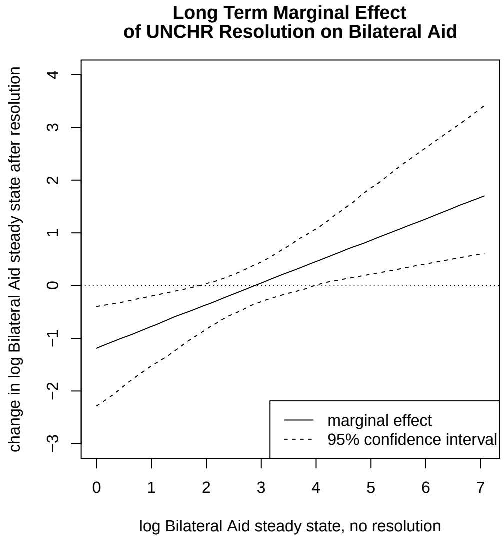
Figure 9: The Instantaneous Effect of a UNCHR Resolution$_{i(t-1)}$ on Bilateral Aid$_{it}$



Long term marginal effect of naming & shaming on bilateral aid

Figure 9 shows the immediate effect of a UNCHR resolution on aid, but what about the effect over time? The long term marginal effect of naming and shaming is relatively straightforward to illustrate in this example because the state dependent variable (UNCHR resolution) is binary. If the steady state of bilateral aid depended only on this variable, calculating the level of aid in the absence of a resolution (i.e., the starting steady state) would be a simple matter of fixing that variable at zero. Calculating the long-term effect of a resolution (i.e., the change in the starting steady state) would be an equally simple matter of raising that variable to 1. However, as discussed above, the starting steady state also responds to the other variables in the model. To account for those influences, we calculate a range of starting steady states that depend on the values of the other variables. Then, we calculate the change in each starting steady state that results from a UNCHR resolution. Thus Figure 10 shows the change in steady state levels of bilateral aid related to a UNCHR resolution at different levels of bilateral aid prior to the resolution, with 95% confidence interval. It shows that when a public resolution is issued, the natural log of aid declines (in this data set) when the steady state is less than about three (or about \$20 per capita) and rises otherwise; the effect

Figure 10: The Long-term Effect of a UNCHR $\text{Resolution}_{i(t-1)}$ on Bilateral Aid_{it}



is qualitatively similar, but greater in magnitude, when compared with the instantaneous effect of shaming.

Conclusions and implications

We believe that studying state-dependent dynamic systems via dynamic interaction models represents an important addition to a political scientist's theoretical and methodological toolkit, and that our simulation evidence and empirical example are a compelling case for making this addition. By applying our techniques to the study of naming and shaming, we uncovered an interesting and previously unknown effect of the public condemnation of human rights abuses: it seems to cut the shamed state's small-scale bilateral donations, but *reinforce* large-scale donor relationships.

We believe that, in confirmed in future studies, these results could deepen our understanding about how personal integrity abuse influences bilateral aid. In brief, naming and shaming by human rights organizations like the UNCHR may be most effective when the offending state is not strategically important for economic, political, or military reasons. In cases where a state has important strategic partners sending large amounts of aid, naming and shaming could simply reinforce these partnerships. On the other hand, states without these strategic partnerships *could* be isolated and punished by naming and shaming, and even those who do have them would be alienated by non-partner states in the international system.

In summary, our applied example highlights the opportunities for interesting substantive findings that are created when state dependence is considered. By demonstrating that "naming and shaming" might have a contextually sensitive relationship with bilateral foreign aid provision, our results contribute to a research agenda at the intersection of human rights, international organizations, and the political economy. In our view, an important next step would be to more directly measure the strategic importance of recipent states to donor states on a bilateral basis and see whether these strategic importance measures change the relationship between human rights condemnations and changes in bilateral aid. If so, the combined results would boost our confidence in the explanation for the association that we uncover.

More generally, we believe that many political processes may be state-dependent, and that discovering these processes could change how we view the substance of politics. In short, we think that state-dependent dynamic systems deserve greater attention from political scientists. We therefore conclude with two future applications for analysis that we find especially promising.

One potential candidate is the effect of democratization on trade liberalization. Milner and Kubota (2005) theorize and find evidence of a positive link between the two. They argue that democratization is characterized by an expanding selectorate. This expansion yields a change in the group's composition and thus a change in the median voter. In autocracies, the selectorate is a restricted group of capital-rich individuals who benefit from protectionist trade policy. Broadening the selectorate means incorporating the lower classes, who have relatively less capital than labor and benefit from liberalized trade. As these lower class citizens are increasingly present in the selectorate, then, that selectorate prefers lower levels of protectionism. To retain office, leaders appeal to this newly-enfranchised group by responding to its call for liberalized trade. Thus democracies are more oriented toward free trade than autocracies, and an increase in the degree of democracy should induce a move to liberalize trade (Milner and Kubota, 2005, 115-119).

This work builds on an assumption that expanding the selectorate means incorporating the working class (e.g., Collier, 1999; Rueschemeyer, Stephens and Stephens, 1992) and that this class would benefit from trade liberalization. But inclusion is not always class-based (e.g., Caraway, 2004) and not all autocracies favor protection. Nor, for that matter, do fully-integrated democracies always prefer full trade liberalization. The distribution of preferences for trade liberalization in both the selectorate and the population at large are undoubtedly a product of structural and historical factors, some of which may be unmeasurable or ill-understood. Together, all these facts suggest a state-dependent relationship between democratization and trade liberalization. Consequently, we anticipate that democratization's effect on trade liberalization will be positive, but inversely proportional in size according to the prior level of liberalization. If true, this could lead us to find a larger relationship between democratization and trade liberalization for the states that are most restrictive, underscoring the importance of the relationship.

Another candidate for state-dependence is the influence of partisanship on government size. Conventional wisdom holds that "the basic criterion distinguishing the left from the right concerns the role of government versus that of the market" (Blais, Blake and Dion, 1993, 43). Leftist parties traditionally favor large government, while rightist parties privilege the market and minimize government intervention. As a result, left parties in power are characterized by high levels of and increases in spending, while right parties in power spend less and reduce government size. Evidence of a statistical relationship between these forces is robust, but conditioned by a series of factors including (for example) majoritarian government and outstanding debt (Blais, Blake and Dion, 1993, 1996; see also Cameron, 1978; Cusack, 1997).

But the influence a party exerts on spending may be conditioned on previous spending changes, and particularly on the party's satisfaction with those change. A party in power will vary spending only if it is dissatisfied with the status quo. Left-leaning governments will expand only to the extent that previous expansion was low. If previous expansion was high, left parties may have already implemented or begun to implement their preference for big government. Similarly, right governments will contract only to the extent that previous contraction was low. If previous contraction was high, right parties may have already implemented or begun to implement their preference for small government. In short, changing government size is an ongoing process, such that a party's preferences may be implemented only to the extent that implementation has not already occurred and hence dissatisfaction with the status quo persists. More generally, the effect of partisan preferences on government size may depend on the previous change in spending. As in the prior case, by looking for state-dependence we may find that the government's ideological commitments are a greater influence on spending than previously believed.

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