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Advanced Methods in Comparative Politics: Modeling Without Conditional Independence

David George Carlson
Washington University in St. Louis

WASHINGTON UNIVERSITY IN ST. LOUIS
Department of Political Science

Dissertation Examination Committee:
Jacob M. Montgomery, Chair
Roman Garnett
Jeff Gill
Guillermo Rosas
Margit Tavits

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by

David George Carlson

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David George Carlson

Washington University

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Dedicated to Elif Özdemir

ABSTRACT OF THE DISSERTATION

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One of the most significant assumptions we invoke when making quantitative inferences is the conditional independence between observations. There are, however, many situations when we may doubt this independence. For instance, two *seemingly* distinct data-generating processes may in fact share unobserved relations. Time-series and cross-sectional studies are also plagued by a lack of independence. If we ignore this common violation of our fundamental modeling assumptions we may draw improper conclusions from our data. This dissertation introduces two methods to the political science literature: a zero-inflated multivariate ordered probit and Gaussian process regression for time-series cross-sectional analyses. This latter model is then applied to demonstrate that executives in Latin America enjoy increased public support following ideological moderation, but executives are less willing to moderate during election years. These effects, however, are conditional on the extremity of the executive. The dissertation as a whole contributes both methodologically and theoretically to the field.

Introduction

When making quantitative inferences in political science, one of the most significant assumptions we invoke is the conditional independence between observations. There are, however, many situations when we may doubt this independence. For instance, two *seemingly* distinct data-generating processes may in fact share unobserved relations (Zellner 1962; Zellner and Huang 1962). Time-series and cross-sectional studies are also plagued by a lack of independence (Pang 2014). If we ignore this common violation of our fundamental modeling assumptions we may draw improper conclusions from our data.

Although political science has made great strides to better recognize and address these issues (e.g., Monogan 2015, Ch. 9), the best practices for the analyses of certain types of data remain elusive. For example, time-series cross-sectional analyses have become increasingly sophisticated, yet there is no default solution, and for any given problem the "best" solution is often still not ideal. Similarly, multivariate analyses for related processes are under-utilized and have not been expanded in scope to some of the more advanced and newly developed models.

In this dissertation, I present three chapters to fill these gaps. The first, "Modeling Related Processes with an Excess of Zeros," extends existing models and develops a zero-inflated multivariate ordered probit model. Political science research frequently models binary or ordered outcomes involving related processes. However, traditional modeling of these outcomes ignores common data issues and cannot capture nuances. There is often an excess of zeros, the observed outcomes for different actors are inherently related, and competing actors may respond to the same factors differently. The proposed model is ideal for capturing strategic interactions between competing parties when there exist resource constraints. The model allows and estimates correlations between the competing actors' decision processes. Not only does it relax our assumptions that these outcomes are independent, but it provides the means to measure the degree of interaction. I apply the model to presidential campaign strategies in Mexico.

The next chapter, "Modeling Without Conditional Independence: Gaussian Process Regression for Time-Series Cross-Sectional Analyses," utilizes a machine-learning approach to regression and develops a novel technique to model time-series cross-sectional data. Simulations show that it out-performs extant models commonly used for these types of data. I apply this model to better understand the effect of inflation on anti-Americanism in Latin America, and I replicate an analysis on the effect of rocket threat on the right-wing vote in Israel.

The next chapter, "Executive Moderation and Executive Approval in Latin America," is a more detailed application of the Gaussian process to show the relationship between Latin American executives' use of position-taking in annual addresses and public approval. This is an ideal application for the Gaussian process regression model and an important substantive question. I will now outline the three chapters in more detail.

1.1 Modeling Related Processes with an Excess of Zeros

Political scientists frequently test hypotheses in which the outcome variable is binary or ordered. However, there are often two distinct challenges analyses of this sort encounter. The outcome variable exhibits an excess of zeros, and the data-generating process for multiple outcomes may be related. This is particularly true when studying strategic interactions between political actors who must allocate scarce resources. As an example, consider campaign decisions by competing parties to visit municipalities. Campaigns can only realistically visit a small proportion of these municipalities, so the outcome, a visit, exhibits an excess of zeros. Further, there are likely decisions made based on covariates to never even consider visiting certain municipalities. To add to the complication of the true data-generating process, the decisions made by the parties to visit are likely highly interdependent. Parties are very likely responding to the anticipated or observed behavior of their competitors.

Ignoring either of these issues – zero inflation and strategic interdependence – can bias parameter estimates, and typical modeling strategies tend to have inefficient estimators. Furthermore, by failing to address these problems, we miss an opportunity to better understand important nuances of the underlying dynamics of the data generating process. Returning to the example of campaign visits, we should be interested in the factors that lead to a municipality being considered for a visit, even if the party never actually visits (the outcome is a zero). There are thus two types of observed zeros, and we wish to be able to discriminate between them to better understand the parties' calculi. We also want to test the proposition that these decisions are in fact related, and the parties are strategically interacting.[1] Finally, different actors may have different decision-making criteria. For example, parties may not respond to covariates in the same way. We want to be able to test for this heterogeneity and explore the various relationships of our variables of interest.

In this chapter, I extend the zero-inflated ordered probit (Harris and Zhao 2007) to better address these issues by allowing for interdependent multivariate outcomes, developing a zero-inflated multivariate ordered probit (ZIMVOP). This model is novel not only to political science, it has yet to be developed in any literature. It consists of two steps. The first

[1]Because these decisions are being made at an unobserved time, or simultaneously, standard strategic interaction models are inappropriate (Bas, Signorino, and Walker 2008; Carson and Roberts 2005; Signorino 2002; Signorino 2003).

step models the observation as a potential non-zero, splitting the population into "always zeros" and "potential non-zeros." The second step is a multivariate ordered probit, allowing correlations of the disturbance terms across equations over dimensions.

To make this model more concrete, and to provide a running example, I re-analyze a dataset of Mexican presidential campaign visits in 2006 and 2012 for the three major parties – the Partido Revolucionario Institucional (PRI), the Partido Acción Nacional (PAN), and the Partido de la Revolución Democrática (PRD) (Langston and Rosas 2016). The outcome of interest is the level of visitation by each of the parties – no visit, hold a meeting, or hold a rally. Because there are three parties, the outcome is trivariate. In other words, each municipality has three outcomes, one for each party, that are inherently related. Further, the vast majority of the municipalities were never visited by any party.

Extant models cannot capture the nuances I have described. Zero-inflated models would not test for or allow the heterogeneity if the data are pooled. If separate zero-inflated models are run for each party, we could not test if there exists strategic interdependence in their visit strategies. Models allowing correlations between outcomes (e.g., seemingly unrelated regressions) would not capture the zero inflation. I show through simulation exercises that ZIMVOP also outperforms the extant alternatives by reducing bias and increasing efficiency. Therefore, if ZIMVOP were not employed on data suffering these two issues, besides not capturing nuances, we may come to incorrect substantive conclusions. The contribution of this chapter, therefore, is to provide a model that can correctly account for *both* zero inflation and strategic interactions to allow political science researchers to better understand these kinds of processes.

1.2 Modeling Without Conditional Independence: Gaussian Process Regression for Time-Series Cross-Sectional Analyses

Researchers in political science very frequently need to analyze panel data or time-series cross-sectional data. There are well-known problems these analyses encounter, however, such as time-varying confounders, serial correlation in the variables of interest across time, between-subject heterogeneity, spatial correlations, and more, that make inferences particularly difficult. Both parameter estimates and their standard errors can be misleading and biased when inappropriately modeled, often leading to spurious results (Granger and Newbold 1974). Esarey and Menger (2017) provide a thorough analysis of the more common solutions to these problems, including hierarchical modeling and various ways of adjusting standard errors. Although the article offers good suggestions for various situations, there is no default solution and the best option for a given data set can still produce excessive false positives and negatives, biased estimates, and tend to be inefficient. This chapter offers a different modeling strategy utilizing Gaussian process regression (GPR) that surpasses extant alternatives on many criteria across a range of situations, and may serve as a better default option for applied research than any used in current practice. It offers the simplicity of standard inferential techniques while handling complex underlying data-generation.

GPR is primarily known for its uses in machine learning classification and prediction (Rasmussen and Williams 2006), but the models have been utilized to make inferences about populations as well (e.g., Kirk and Stumpf 2009; Huang, Zhang, and Schölkopf 2015; Garg, Singh, and Ramos 2012; Qian, Zhou, and Rudin 2011; Gibson et al. 2012). Monogan and Gill (2016) use a GPR approach, which they refer to as Bayesian kriging, to estimate a posterior density blanket of citizens' ideologies across the United States. Despite having relatively sparse data, the method allows for any level of geographic aggregation and provides an estimate, with uncertainty, of the ideology of the region by smoothing across space.

This smooth blanket over the U.S. is a useful introduction to conceptualize GPR. Data, in this case, some measure capturing ideology, is not independent. The average ideology in a town is likely similar to the ideology in its neighboring towns. We can consider these outcomes (ideology) as coming from a joint normal distribution if we assume each realization comes from a normal distribution. A Gaussian process is a distribution over *function space*, with each observed outcome coming from a normal distribution, making the joint distribution a multivariate normal. We do not need to consider data as independent, and we do not need to impose many assumptions on the relationship of the correlational structure.

The "smoothing" across space is intuitive, but we can smooth over any input dimension we choose, including, of course, time. The same way neighboring towns are likely heavily correlated, so too are temporally proximate observations, or observations sharing similar explanatory variables. These correlations can also vary from dimension to dimension. In other words, temporally proximate observations need not share the same correlation as geographically proximate observations. Rather than considering data as independent, or even sequential, we can think of all observations as coming from one joint distribution, with data points close to each other in the hyperplane likely similar. The flexibility of the model makes it ideal for modeling situations in which there are violations of the conditional independence assumption but the nature of these correlations is not known *a priori*.

1.3 Executive Moderation and Public Approval in Latin America

It is very common for executives in Latin America to shift their professed ideology or policy positions over the course of their tenure as president (Arnold, Doyle, and Wiesehomeier

2017). However, the literature on the region has not fully investigated the effect this has on executive public approval, and as such is missing a critical explanation for this movement. I argue that moderation, i.e., moving closer to the median voter, boosts public approval overall. Further, these benefits are enjoyed most by extreme executives, whose movement is more noticeable and whose moderation increases the utility of voters more than moderation by centrist executives. This gives an added incentive for presidents to moderate their professed ideology, because public approval increases their legislative bargaining power (Calvo 2007).

There are costs associated with this movement, however. Executives, particularly extreme executives, rely on relatively extreme supporters who turn out to vote and are more politically active (Samuels and Shugart 2010; Samuels 2008b). Because of this, presidents have an incentive to shift their professed ideology towards the extremes during either executive or legislative election years. This helps turn out their voters and activists to ensure personal and / or party electoral success.

To test these claims, I rely on ideal points estimated from the constitutionally mandated annual addresses of presidents (Arnold, Doyle, and Wiesehomeier 2017) to capture their professed positions on a left-right scale over time. I also utilize time-series public approval data estimated from representative surveys (Carlin et al. 2016). Standard analyses of these data are problematic for all of the reasons discussed in the previous chapter, making Gaussian process regression a proper strategy. The models support my hypotheses. Alternative modeling strategies provide mixed results, but in general are largely consonant with GPR.

1.4 Concluding Remarks

The following three chapters all highlight the issues with ignoring violations to the commonly invoked assumption of conditional independence. Traditional modeling in political science can lead to biased estimates, tend to be inefficient, and, perhaps the worst drawback, may lead to incorrect substantive conclusions. There is very frequently reason to doubt conditional independence when analyzing social science data, yet, as the chapters will argue, ignoring the violation is unfortunately quite common in the discipline.

Besides discussing the issues associated with this problem and arguing for more careful modeling, I also propose novel solutions to model some of the more common types of data encountered in political science, particularly comparative political science. The first model, a zero-inflated multivariate ordered probit, has never been developed in any literature. The second model, Gaussian process regression for time-series cross-sectional (TSCS) analyses, is a unique parameterization of an under-utilized statistical model specifically for TSCS data. I thus am both introducing the model to political science and demonstrating how it can be modified to fit our purposes as social scientists.

The dissertation also adds to our theoretical understanding of core political concepts. The first of the three chapters demonstrates that parties in Mexico in competition with one another while campaigning have varying motivations and strategies to visit or hold rallies in particular municipalities. Further, the chapter demonstrates that these decisions are related to one another. This is both an interesting substantive finding and justifies the use of a model that does not assume conditional independence.

The second of the three chapters demonstrates that across Latin America inflation leads to less anti-Americanism in the region. Latin American citizens view the United States as a source of economic well-being. When they feel the pressure of declining purchasing power they want their countries to increase relations with the United States to improve their economic situation. Standard approaches to modeling these data fail to uncover this interesting finding.

Finally, the third chapter explores in depth the effects of Latin American executive ideological moderation on public approval, shedding light on the motivations presidents in the region have for dampening their policy or ideological stances. While executives, especially extreme executives, benefit from moderation, they do so at the cost of disappointing party activists and extreme voters who turn out. Executives are therefore less willing to moderate during electoral years to benefit themselves and / or their party electorally.

This dissertation therefore examines a methodological issue in the discipline, begins to address and solve some of these issues, and contributes to our understanding of Latin American politics and politics more generally. Following the three chapters I have discussed thus far, I conclude with a discussion of the contribution and directions for future work. There is also a short appendix for the second and fourth chapters.

Modeling Related Processes with an Excess of Zeros

In many settings, political scientists wish to test theories where they must confront two distinct data challenges: (1) there is often an excess of zeros in the outcome variable, and (2) the data generating process for multiple outcomes may be related. This is particularly true when studying related decisions between political actors who must allocate scarce resources. For instance, consider the decision-making processes of competing parties regarding candidate visits during a presidential campaign. In practice, these campaigns can only visit a small proportion of localities within a country during a single campaign. (The outcome exhibits an excess of zeros.) Thus, many municipalities are never considered worthwhile for visits by any candidate due their small populations or non-competitive nature. To make things more complicated, however, of those municipalities that *are worthwhile* to (potentially) visit, decisions by campaigns are also interdependent. That is, parties may visit municipalities simply because they anticipate that they will be visited by their opponents, or actors may make decisions based on similar but unobserved factors.

Ignoring either of these issues – zero inflation and interdependence – can bias parameter estimates and decrease the efficiency of the estimators. Furthermore, by failing to address them, we miss an opportunity to better understand important features of the data generating process. What factors are related to being either a unit that is considered for resource allocation or excluded completely? Is there really evidence that the behavior of one actor is related to, or even shaped by, the (anticipated) behavior of other actors?[2] Finally, do different actors respond heterogeneously to different factors?

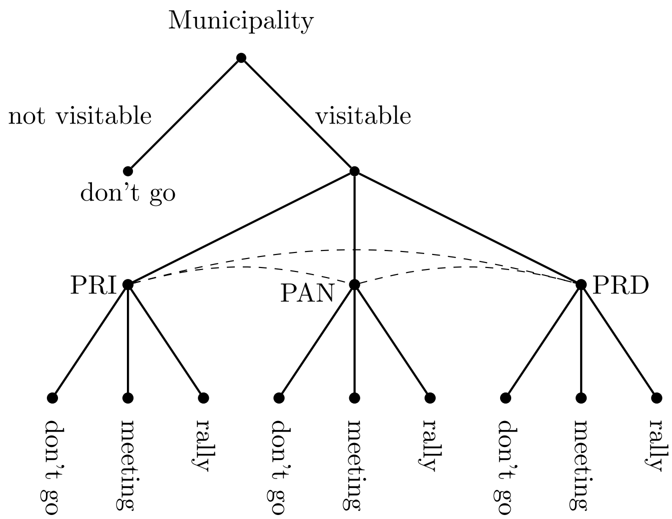
In this chapter, I extend the zero-inflated ordered probit (Harris and Zhao 2007) to better address these issues by allowing for interdependent multivariate outcomes, developing a zero-inflated multivariate ordered probit (ZIMVOP). It consists of two steps. The first step models the observation as a potential non-zero, splitting the population into "always zeros" and "potential non-zeros." The second step is a multivariate ordered probit, allowing correlations of the disturbance terms across equations over dimensions.

To make this model more concrete, and to provide a running example, I re-analyze a dataset of Mexican presidential campaign visits in 2006 and 2012 for the three major parties – the Partido Revolucionario Institucional (PRI), the Partido Acción Nacional (PAN), and the Partido de la Revolución Democrática (PRD) (Langston and Rosas 2016). The outcome of interest is the level of visitation by each of the parties – no visit, hold a meeting, or hold a rally. Because there are three parties, the outcome is trivariate. In other words, each municipality has three outcomes, one for each party, that are inherently related. Further, the vast majority of the municipalities were never visited by any party. Figure 2.1 provides a graphical depiction of the parties' decisions.

Unfortunately, no current model can accurately capture the decision tree described. Zero-inflated models cannot measure the extent of interdependence between parties' decisions. Models allowing correlations between outcomes (e.g., seemingly unrelated regressions) would not capture the zero inflation. Both approaches are inefficient and could lead to inaccurate estimates and potentially incorrect conclusions. The contribution of this chapter, therefore,

[2]Because these decisions are being made at an unobserved time, or simultaneously, standard strategic interaction models are inappropriate (Bas, Signorino, and Walker 2008; Carson and Roberts 2005; Signorino 2002; Signorino 2003). Further, the interdependence can arise from unobservables as well as strategic interactions.

Figure 2.1: Parties' decision trees.



Note: The decision of a party is split into two steps. The first decision is whether or not a municipality is visitable. The second decision is the type of visit, conditional on the municipality being visitable. This decision is likely related to the decisions of other parties.

is to provide a model that can correctly account for *both* zero inflation and interdependence to allow political science researchers to better understand these kinds of processes.

The outline of the chapter is as follows. First, I discuss the issues of zero-inflated and correlated outcomes, and explain how ZIMVOP addresses both. As part of the discussion, I discuss both its relationship to existing statistical approaches and also its distinct advantages. Second, I provide the details for the ZIMVOP model. Third, I demonstrate its effectiveness using simulated data and an application to the Mexican Elections example discussed above. I conclude with a discussion of the limitations of the approach as well as potential future applications.

2.5 Zero-Inflated and Correlated Errors: Issues and Solutions

In this section, I discuss the issues associated with outcomes that exhibit an excess of zeros and current approaches to dealing with these issues. I then do the same for multivariate models that have correlated error terms, known as seemingly unrelated regressions (SUR). Neither of these families of models adequately addresses the problems of both zero-inflated outcomes and correlated error terms. Through the discussion, I highlight the advantages to current approaches and demonstrate that ZIMVOP, a synthesis of the two families of models, is an intuitive extension when dealing with data that raise both of these concerns.

2.5.1 Models with Zero-Inflation and Seemingly Unrelated Regressions

King and Zeng (2001a and 2001b) introduce a unique approach to modeling rare outcomes, focusing primarily on international conflict. They argue that modeling conflict on all country dyads underestimates the effect of certain factors, producing biased estimates. This is due to the fact that the vast majority of dyads will never go to war, regardless of certain observed characteristics that may actually be deterministic in other dyads. The approach they suggest is to save data collection, maintain all non-zero observations in the data, randomly sample zero outcomes, and focus more time on the quality of the data than the quantity of data.

This recommendation saves data collection and may lead to less biased estimates relative to running a standard probit on lower-quality data. However, observed zeros may have distinct data-generating processes, suggesting a split-population approach (Harris and Zhao 2007). This split-population method differs from the rare events method by modeling the outcome in two steps. The first step models the probability that an observation is a potential non-zero, and the second step models the outcome conditional on the observation being a potential non-zero. The split-population refers to splitting the population into "potential non-zeros" and "always zeros." An intuitive example relates to civil conflict. Bagozzi, Hill, Moore, and Mukherjee (2015), using a zero-inflated ordered probit, find that a country's GDP has a reliable and negative effect on the potential for political violence, but on a potential non-zero, the effect is positive. That is, rich countries are less likely to experience political violence, but on a potential non-zero, income has a positive effect on the outcome, likely due to greater resources.

This example highlights both the issues related to ignoring an excess of zeros and the benefits in addressing them. If the two steps were ignored, the nuanced effects of these covariates would be lost and the estimates of the effects would be biased, because a standard model with no inflation would lead to a correlation between the error terms and the explanatory variables (Bagozzi and Marchetti 2014; Dunne and Tian n.d.).

In addition to the problems associated with an excess of zeros, outcomes also may share related data-generating processes. The SUR class of models stacks regressions and allows the error terms across these stacked regressions to be correlated (Zellner 1962). Jointly estimating a set of equations improves asymptotic relative efficiency over the equation-by-equation case by combining information across equations (King 1989; Zellner and Huang 1962). In other words, in the limit, the estimators produce estimates with smaller mean squared errors and smaller variances.

2.5.2 Partial observability in strategic settings

The ZIMVOP model I propose below seeks to combine the approaches above in order to achieve three simultaneous goals: (1) understand the relationship between the main explanatory variables and the outcome, (2) understand the process that leads some observations to be excluded from consideration, and (3) detect inter-dependencies in the data generating process for multiple outcomes. While several of the models above can achieve one or two of these goals, none can accomplish all three simultaneously.

Nonetheless, it is important to note that there are several other models in the literature that are similar in important ways to my own. Gurmu and Dagne (2012) developed a zero-inflated bivariate ordered probit, but it does not easily extend to the multivariate case (see also Kadel 2013).[3] ZIMVOP generalizes this zero-inflated ordered probit to have a theoretically unbounded number of dimensions in an intuitive, straight-forward manner.

Another project for settings with partially observable outcomes is presented by Nieman (2015), who proposes a model for strategic interactions in a two-player, sequential game. Similar to ZIMVOP, we observe the same outcome from two distinct data-generating processes (status quo or government acquiesces). Despite the seeming similarity, the underlying behavior modeled by ZIMVOP and Nieman are quite distinct, with the former estimating related decision-making or processes, and the latter modeling a strategic game.

2.6 ZIMVOP Specification

ZIMVOP has two major components that in combination set the model apart from current approaches. The first is a zero-inflation step. This is simply a univariate standard probit that models the probability that an observation is a potential participator, or a potential non-zero. In the Mexico example, this is whether or not *any* party will even consider visiting a given municipality. This follows the zero-inflated ordered probit approach developed by Harris and Zhao (2007).

The second component is a multivariate ordered probit for the final outcomes. For each observation, there is a vector of outcomes, one scalar outcome for each dimension. In the Mexico example, the "dimensions" are each party, one dimension for the PRI, one for the

[3] The principles of ZIMVOP do not vary substantially from these bivariate models, but the implementation is much more straight-forward and these bivariate approaches only allow for one correlation parameter.

PAN, and one for the PRD. Each observation is a municipality in a given time period, and the outcome is a vector of length three, with one outcome for each party. The outcome for each component takes one of three values: 0 for no visit, 1 for a meeting, and 2 for a rally. In this step, conditional on being a potential non-zero (e.g., a visitable municipality), an ordered outcome is modeled separately for each dimension, but the error terms are allowed to correlate across dimensions (parties). In other words, the decision processes at the second step for each party are not assumed independent in a given municipality and time period.

In presenting the model, I follow the presentations of Harris and Zhao (2007), Gurmu and Dagne (2012), and Kadel (2013). This setup requires that we model the observed outcome for unit i on dimension r, y_{ri}, as the product of two unobserved discrete latent parameters, $y_{ri} = s_i z_{ri}$, where s_i indicates whether unit i is a potential non-zero, and z_{ri} is the estimated level of outcome conditional on observation i being a potential non-zero. In our Mexico example, $s_i \in \{0, 1\}$ represents whether a municipality is "visitable" by one of the parties, while $z_{ri} \in \{0, 1, 2\}$ is the model's estimate of whether there will be no visit (0), a meeting (1), or a rally (2) by party r in that municipality.

2.6.1 First Step

Both the first-step probit and the second-step multivariate ordered probit follow Albert and Chib's (1993) data augmentation approach such that we include in our model latent parameters. Sampling of latent parameters leads to probability distributions for the observed outcomes, and significantly improves computational tractability.

Let s_i^* be a latent parameter capturing the potential for observation i to be a non-zero (visitable) observation such that the probability of i being a non-zero is equal to $\Pr(s_i^* > 0)$. This latent value is modeled as a linear function of a matrix of covariates, $\mathbf{V}$, with each row, $\mathbf{v}_i$, being a vector of observation-level covariates (including a constant). Specifically, we let $s_i^* = \mathbf{v}_i'\boldsymbol{\gamma} + \mu_i$, where $\boldsymbol{\gamma}$ is a vector of unknown parameters to be estimated and μ_i is the error term such that $\mu_i \sim N(0,1)$.[4]

Now, let s_i, the latent categorical parameter indicating potential non-zero observations, be defined as:

$$s_i = \begin{cases} 0 & \text{if } s_i^* \leq 0, \\ 1 & \text{if } s_i^* > 0. \end{cases}$$

Let $\Phi(\cdot)$ denote the normal cumulative distribution function. Then,

$$\Pr(s_i = 1) = \Phi(s_i^*) = \Phi(\mathbf{v}_i'\boldsymbol{\gamma})$$

is the probability of the observation being a potential non-zero.

2.6.2 Second Step

In the second step, let $r = 1, \ldots, D$, where D is the total number of dimensions. The Mexico example has a trivariate outcome ($D = 3$), with r equal to 1, 2, and 3, each number indicating a party. Again following the standard data augmentation approach, let z_{ri}^* be the latent parameter related to the outcome level for observation i on dimension r conditional on observation i being a potential non-zero. These levels of participation for the Mexico parties are, in order: do not go (0), go for a meeting (1), or go for a rally (2). Let $\mathbf{X}_r$ be a n by p matrix of predictors for the level of participation on dimension r (which includes a constant). Let the n by p by D array of all second-step predictors for all dimensions be denoted $\mathcal{X}$. We let $z_{ri}^* = \mathbf{x}_{ri}'\boldsymbol{\beta}_r + \epsilon_{ri}$, where ϵ_{ri} is the error term and $\boldsymbol{\beta}_r$ is the unknown vector to be estimated for dimension r.

Our goal is to estimate the latent categorical parameter z_{ri}, which is the level of participation for observation i on dimension r conditional on being a potential non-zero. In the data augmentation approach, to allow for multiple categories, we need to also estimate a vector of "cutoff" parameters (Albert and Chibb 1993). Let $\mathbf{a}_r$ be the vector of cut-off points of

[4]The standard normal distribution is used to identify the model, although other choices could be used. I discuss all the prior distributions in Section 3.3.

length j_r, where j_r is the maximum possible outcome on dimension r (in our example $j_r = 2$ for all r), and a_{rk} is the k^{th} cutoff on dimension r. We can now define z_{ri} as:

$$z_{ri} = \begin{cases} 0 & \text{if} \quad z^*_{ri} \leq a_{r1}, \\ k & \text{if} \quad a_{rk} < z^*_{ri} \leq a_{rk+1}, \quad k = 1, \ldots, j_r - 1, \\ j_r & \text{if} \quad z^*_{ri} > a_{rj_r-1}. \end{cases}$$

Critically, in the second step we want to allow the error terms to be correlated.[5] We therefore let the vector of error terms across dimensions, $\boldsymbol{\epsilon}_i$, be distributed multivariate normal with mean $\mathbf{0}_D$, a vector of zeros of length d, with variance-covariance matrix $\boldsymbol{\Sigma}_D$: $\boldsymbol{\epsilon}_i \sim \mathcal{N}_D(\mathbf{0}_D, \boldsymbol{\Sigma}_D)$. Finally, we model the observed vector of outcomes for a municipality, y_{ri} as the product $s_i z_{ri}$.

2.6.3 Likelihood

In the likelihood function that follows, allow i to index observations. Let $\mathbf{Y}$ denote the matrix of observations for all dimensions. To write the likelihood for any number of dimensions and because the outcomes are not assumed independent, we must let $\mathbf{g}$ index the vectors of potential outcomes. Let $\mathrm{I}(\mathbf{z}_i = \mathbf{g})$ be an indicator function as to whether the observation is equal to $\mathbf{g}$. For example, to simplify the likelihood, consider the probability of different outcomes in the trivariate case. A zero outcome on three dimensions would be the probability that $s_i = 0$ added to the probability that $s_i = 1$ multiplied by the probability of all outcomes equaling zero ($\mathbf{g}$ would be equal to [0,0,0]): $\Pr(\mathbf{y}_i = [0, 0, 0]) = \Pr(s_i = 0) + \Pr(s_i = 1) \times \Pr(z_{1i} = 0, z_{2i} = 0, z_{3i} = 0)$. An outcome of, for example, $[1, 0, 2]$, would be: $\Pr(s_i =$

[5] Harris and Zhao (2007) allow the errors from the first and second-step equations to be correlated. However, Gurmu and Dagne (2012) find that when moving from the zero-inflated univariate to the bivariate ordered probit allowing this correlation does not improve the model. Substantively, if we let the second-step error terms be correlated with the first step, the estimated correlations between error terms at the second step would be biased and lose substantive meaning, as they would covary with the univariate error and potentially induce less efficient estimation. For example, if the first-step errors are positively correlated with the second-step errors, and there is a correlation between the second-step errors, this latter correlation could either be estimated as a joint correlation to the first-step errors or a correlation between second-step errors, leading to a poorly identified model.

1) $\times$ Pr$(z_{1i} = 1, z_{2i} = 0, z_{3i} = 2)$. The likelihood function for any number of dimensions is:

$$\mathcal{L}(\mathbf{Y}|\mathcal{X}, \mathbf{V}, \boldsymbol{\beta}_1, \ldots, \boldsymbol{\beta}_D, \boldsymbol{\gamma}, \mathbf{a}_1, \ldots, \mathbf{a}_D, \mathbf{s}, \mathbf{Z}) = \\ \prod_{i=1}^{N} \Big(\prod_{\mathbf{g}=\mathbf{0}} [\Pr(s_i = 0) + \Pr(s_i = 1)\Pr(\mathbf{z}_i = \mathbf{g})]^{\mathrm{I}(\mathbf{z}_i = \mathbf{0})} \\ \times \prod_{\mathbf{g} \neq \mathbf{0}} [\Pr(s_i = 1)\Pr(\mathbf{z}_i = \mathbf{g})]^{\mathrm{I}(\mathbf{z}_i = \mathbf{g})} \Big).$$

Now that I have specified the model's two-step process and the generalized likelihood, I will discuss the prior distributions for the parameters of interest to fully specify ZIMVOP.

2.6.4 Priors

Let the first-step error terms, μ_i, follow the normal distribution with mean 0 and standard deviation 1: $\mu_i \sim \mathcal{N}(0, 1)$. In frequentist statistics, setting the standard deviation to 1 is necessary to identify the model (Cameron and Trivedi, 2005). Though in a Bayesian context we could put a hyperprior on the variance, I choose not to in order to make the model easier to interpret and to hasten convergence.

The precision matrix $\boldsymbol{\Sigma}_d^{-1}$ is distributed inverse-Wishart with the $d \times d$ identity matrix as the mean and degrees of freedom ν: $\boldsymbol{\Sigma}_d^{-1} \sim \mathcal{IW}(\mathbf{I}_d, \nu)$.[6]

Because the variance is unconstrained in the specification, two cut-offs along each dimension are set to identify the model.[7] By setting two cut-offs rather than just one at zero, the variance along each dimension is identified. Set a_{r1} to 0 and a_{r2} to some positive constant c_r for each dimension. We can let all undefined a_{rk} follow a log-normal distribution with mean 0

[6]The inverse-Wishart is a conjugate prior for the multivariate normal distribution and it ensures generating positive-definite matrices. However, the inverse-Wishart has been criticized for the lack of independence between the variance and the correlations when sampling (Barnard, McCulloch, and Meng 2000). The best strategy to address this is to vary the degrees of freedom, ν, to ensure robustness of the results to different prior specifications. ν should always be equal to or greater than d to be uninformative. Note that the expected value of the precision matrix is a square matrix with diagonal elements equal to ν and off-diagonal elements equal to 0.

[7]Again, this is not strictly necessary, but aids in convergence and interpretability.

and variance σ^2: $a_{rk>2} \sim \ln \mathcal{N}(0, \sigma^2)$. Note that no order is imposed on these cut-offs. In our trivariate Mexico example, there are only two cut-offs, which are both defined as constants, so this choice in prior is for generalizability only and is not implemented in the application. Finally, we let our coefficients, $\boldsymbol{\gamma}$ and $\boldsymbol{\beta}_{1:d}$, have diffuse normal priors centered at zero. The model is written in `JAGS` and the code is provided in the Appendix. Note that these priors can be changed to meet the needs of a particular data analysis, but this subsection, aside from specifying how I choose to set the priors, highlights the considerations necessary.

2.7 Applying ZIMVOP

This section first shows illustrative examples on simulated data to demonstrate the problems that can arise when researchers ignore the zero-inflation or the correlations in the underlying data-generating processes. I then apply the model to presidential campaigns in Mexico.

2.7.1 Implementation on Simulated Data

ZIMVOP synthesizes zero-inflation and SUR models. To isolate the gains of ZIMVOP in comparison to either models not accounting for zero-inflation or not accounting for correlated errors, I perform two sets of simulation exercises. The first set compares ZIMVOP to a multivariate ordered probit without zero-inflation, varies the degree of zero-inflation, and does not impose a correlation on the generated error terms. The second set compares ZIMVOP to an unpooled (i.e., separate equations for each dimension) zero-inflated probit, varies the correlation of the error terms, and does not vary the degree of zero-inflation.[8] By performing these simulation exercises separately, as opposed to comparing all three models on the same sets of data, I can set up the data to make the competing model better able to capture the parameters of interest, allowing for a harder test of ZIMVOP.

[8]All competing models are also run in `JAGS`.

Simulation Exercise I: Zero-Inflation

The first set of simulations compare ZIMVOP to a model without the zero-inflation step, a multivariate ordered probit (a SUR model). Data are generated through eight different zero-inflated processes. The generation of the data involves a zero-inflation step with an intercept (γ_0) of -1.5 and a coefficient (γ_1) changing from four to 11 by increments of one.[9] The first-step equation is therefore;

$$
\begin{aligned}
&s_i^* = -1.5 + v_i \times \gamma_1 + \mu_i,\\
&\mu_i \sim \mathcal{N}(0,1), \quad \text{and}\\
&s_i = \begin{cases} 0 & \text{if } s_i^* \leq 0,\\ 1 & \text{otherwise.}\end{cases}
\end{aligned}
$$

The first-step variable of interest is a single vector, $\mathbf{v}$, of length 500, drawn randomly from a standard normal. For each simulation analysis, these data are resampled in this manner, but I analyze each unique data set by the competing models to ensure comparability. These data are not nested in the second-step variables, which are independently generated. This is a harder test than nesting the values, because some of the variation of the zero-inflation step should be accounted for in the second-step intercept estimates, and much of it could be accounted for by the modeled correlation. In other words, if the zero-inflation step is unmodeled, the second-step estimates can in theory predict reasonable non-zero outcomes, and use the correlation and variance of the error terms to explain the excess zeros not following the pattern of the second step.

The second step consists of three levels of outcome on three dimensions. In generating the outcome, the intercept term on each dimension is set to 0, and the three dimensions each have one predictor, set to 2, 2.5, and 2. The second-step equation is therefore;

[9] The Appendix contains tables of the true parameters for both sets of simulations.

$$\mathbf{z}_i = \mathbf{x_i} \begin{pmatrix} 2 \\ 2.5 \\ 2 \end{pmatrix}' + \boldsymbol{\epsilon}_i, \qquad \boldsymbol{\epsilon}_i \sim \mathcal{N}(\mathbf{0}_3, \mathbf{I}_3).$$

The cut-offs for every dimension are set to 0 and 2.[10] To demonstrate only the issues arising from not modeling the inflation, all correlations are set to zero, meaning the random errors used to generate the outcome are completely independent. This further allows the first step to be captured by the correlation estimates, acting as an observation-level random effect. The matrix $\mathbf{X}$ is a 500×3 matrix of random draws from the standard normal, with a column for each dimension. Again, these are redrawn for each simulation, but I analyze each unique data set by both models. These simulations are repeated 100 times for each of the γ_1 values, for a total of 800 sets of data and 1,600 analyses.[11]

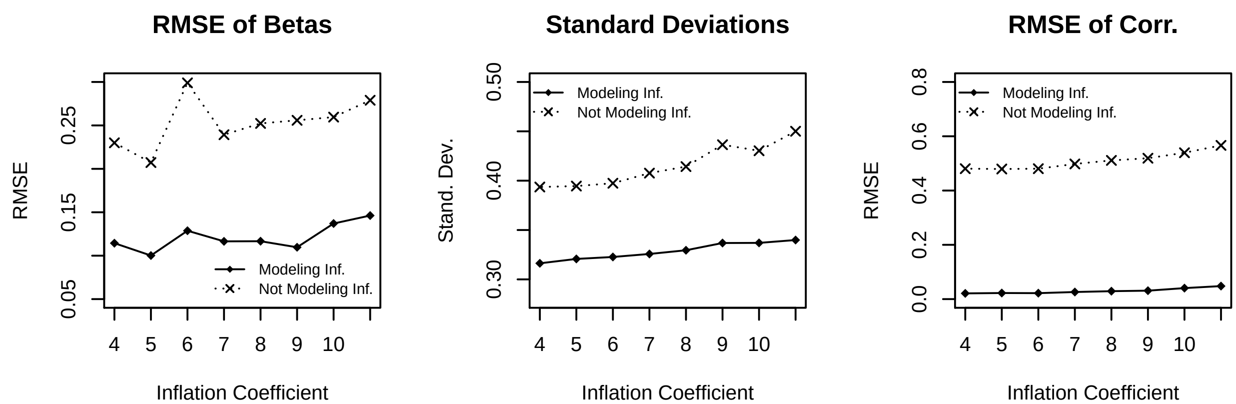
Despite the difficulty of the test, Figure 2.2 shows that across specifications, the model accounting for the zero inflation performs better. The root mean squared error (RMSE),[12] a measure of bias and inefficiency, of the second-step estimates is smaller, while the standard deviations, a measure of precision, of the posteriors are smaller. The average bias across specifications is very close to zero for both models, suggesting that in the aggregate neither has an expectation of bias, but, as shown by the relatively high RMSE, any given estimate using the MVOP estimator is much less likely to be close to the true value of interest. Further, the estimation of the correlation is much closer to the true values when modeling the zero-inflation. If we have a substantive interest in the correlations, we will get very

[10] Although the coefficients are relatively large for a probit model, the large cut-offs across dimensions ensures a reasonable number of outcomes that are one or two. Nevertheless, I analyze a smaller set of simulations using smaller coefficients and smaller cut-offs, and another set increasing the noise to signal ratio, and the improvements to the estimates hold. The choice of larger numbers for both the coefficients and cut-offs was made out of convenience only.

[11] The first of every simulation set-up, for both the first and second set, were run for 10,000 iterations and two chains. All $\hat{R}$'s were close to one and lack-of-convergence tests with the package `superdiag` indicated no problems. The remaining were run for 20,000 iterations to make convergence likely without having to test for convergence on all models.

[12] RMSE is calculated by squaring the difference between the estimates and the true values and taking the mean.

Figure 2.2: Results of the simulations varying the degree of zero-inflation.



Note: The panel on the left shows the root mean squared error of the second-step estimates as the first-step zero-generating coefficient is increased from four to 11. The RMSE is larger for the models without a first step. The middle panel shows the standard errors of the second-step posterior estimates. The standard errors are always larger for the model without a first step. The right panel shows the RMSE of the correlation estimates for the two models. The model without a first step always has much greater RMSE and it increases with the inflation coefficient.

biased results if we do not account for zero-inflation. Finally, across all simulations and specifications, the coverage probability in the model with a first step is 0.92, while the model without the first step is 0.86.[13] This suggests that the decrease in standard errors is not leading to overly restrictive posteriors.

Simulation Exercise II: Correlated Error Terms

The second set of simulation exercises compares a zero-inflated multivariate ordered probit model with second-step correlated errors to the same model not allowing correlations. The simulations again generate data with a zero-inflation process, but vary the correlations of the second-step error terms. The true data generating process sets the first-step intercept, γ_0, to -1.5. The single coefficient of interest in the first step, γ_1, is set to nine. The first-step

[13]Coverage probability is the proportion of posterior distributions in which the true value falls within the 95% highest density region. Ideally this value would be 0.95.

equation is therefore;

$$s_i^* = -1.5 + v_i \times 9 + \mu_i,$$
$$\mu_i \sim \mathcal{N}(0,1), \quad \text{and}$$
$$s_i = \begin{cases} 0 & \text{if } s_i^* \leq 0, \\ 1 & \text{otherwise.} \end{cases}$$

The values of $\mathbf{v}$, which is of length 500, are again all drawn from the standard normal for each simulation, and each unique data set is analyzed by the competing models to ensure comparability. The values of the predictors are generated independently of the second-step values, which are generated as above, from a standard normal. They are not nested.

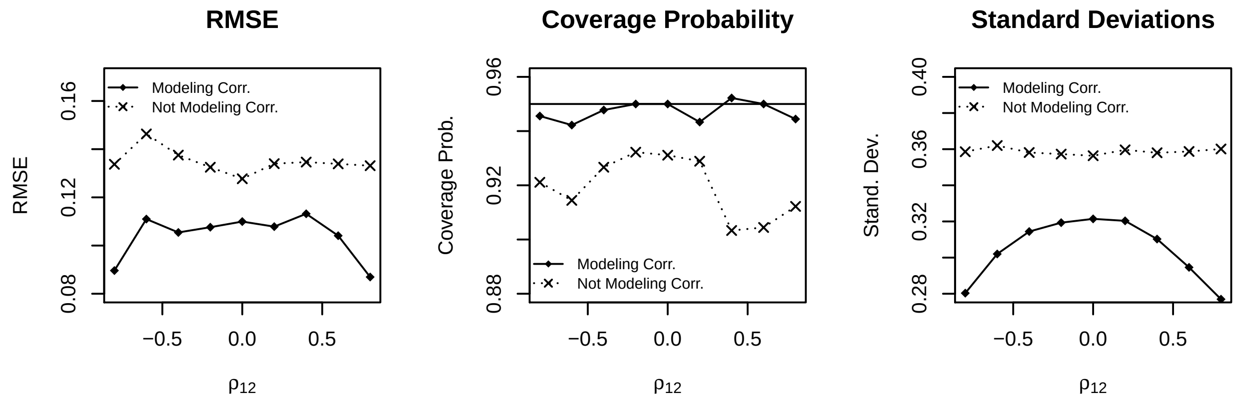
The second step has three outcome levels on each of three dimensions. The coefficients used are the same as the earlier round of simulations. Intercepts are set to zero and the parameters of interest are set to 2, 2.5, and 2. The outcomes generated however are determined using correlated error terms, with the correlation varying across simulations.[14] The second-step equation is:

$$\mathbf{z}_i = \mathbf{x}_i \begin{pmatrix} 2 \\ 2.5 \\ 2 \end{pmatrix}' + \boldsymbol{\epsilon}_i,$$
$$\boldsymbol{\epsilon}_i \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho_{12} & \rho_{13} \\ \rho_{12} & 1 & \rho_{23} \\ \rho_{13} & \rho_{23} & 1 \end{pmatrix}\right).$$

Again, the cut-offs for every dimension are set to 0 and 2. These simulations are repeated 100 times each, resulting in 2,700 sets of data, analyzed once by each model.

[14] There are twenty-seven different data generating processes. The first nine set the second and third correlations, ρ_{13} and ρ_{23}, to zero, and ρ_{12} varies from -0.8 to 0.8 by 0.2. The second nine keep the same ρ_{12} shift, setting ρ_{13} to ρ_{12}^2 and ρ_{23} to ρ_{12}^3. The final nine again maintain the same ρ_{12} shift and set ρ_{13} to ρ_{12} and $\rho_{23} = \rho_{12}^2$. This choice stemmed partly from the need to generate positive definite matrices. This means that when the first correlation coefficient is large (positive), the others are also positive or zero, while when it is small (negative), the other two are sometimes of opposite signs.

Figure 2.3: Results of the simulations varying correlations.



Note: The panel on the left shows the root mean squared error of the second-step estimates as the first correlation coefficient increases. The model allowing correlation performs better by this metric. The middle shows the coverage probability of the second-step estimates, with a line at 0.95. The right panel shows the standard deviations of the posterior distributions. Despite the decent coverage of the model allowing correlations, the standard deviation of the posterior distribution is markedly smaller, performing best when the absolute correlations are high.

Results of the exercise are shown in Figure 2.3, pooled by the first correlation coefficient. Again, we see a smaller RMSE in the second-step estimates with tighter posteriors as shown by the smaller standard deviations. The average bias across estimators is very close to zero, but any given estimate is less likely to be close to the true value of interest if ignoring the correlations in the error terms, as shown by the high RMSE. Despite the increase in precision, the average coverage probability is still close to 0.95. Further, the ZIOP does not produce correlation estimates, which are substantively interesting, for example to capture the relationship between parties' decisions.

When comparing the proposed model to both one not modeling the zero-inflation and one not modeling correlations, the proposed model outperforms these currently extant alternatives. Results hold across various specifications and different benchmarks. Overall, the RMSE of the second-step estimates is reduced, and the posterior densities are more precise while still maintaining approximately 0.95 coverage. ZIMVOP is more accurate, more efficient, and produces substantively interesting results by modeling both the zero-inflation step and the second-step correlations.

2.7.2 Application: Presidential Campaigns in Mexico

Having demonstrated the benefits to our inferences using ZIMVOP, I will now apply it to presidential campaigns in Mexico. Langston and Rosas (2016) argue that municipal-level party support and competitiveness are significant determinants to party campaigns' calculi when deciding which municipalities to visit, and whether that visit is a meeting or a rally. Visits can help assess and signal local party strength and their mobilization networks, and can signal a party's interest in a locality. If they hold a rally and it is not well-attended, however, this can impose more costs than benefits, signaling a lack of strength in the area. Rallies are also expensive, and if the return is not great enough the cost is not worth it. To test the saliency of certain factors entering into this decision, they analyze the Mexican presidential campaigns of 2006 and 2012, focusing on the three major parties – the PRI, the PAN, and the PRD, using a pooled zero-inflated ordered probit.

The current analysis builds on this with two main propositions: (1) the strategies of parties are not the same and will respond to local support differently, and (2) the decisions made by parties are related. Parties likely engage in a Colonel Blotto-type interaction, targeting the municipalities their rivals are targeting,[15] and their decisions are likely interdependent based on unobservable characteristics as well. ZIMVOP is uniquely suited to test these propositions because coefficient estimates vary between parties, and the correlation between the parties' decision processes captures the degree to which parties decisions are related. This proposed relationship between the parties should result in a positive estimate

[15]The Colonel Blotto game, first solved by Borel (1921), is a game based on the idea that battlefields will be won by whichever side sends the most troops, causing a pooling of resources at locations. This idea has been used as a metaphor for party competition in the political science literature (see Laslier and Picard 2002; Myerson 1993).

of the correlation. Further, the vast majority of municipalities are never visited. To obtain reliable estimates of the correlations between parties' calculi and the coefficients of interest, a zero-inflation step is necessary.

The outcome is a vector of ordered party outcomes – 0 for no visit, 1 for a meeting, and 2 for a rally. There are three dimensions, one for the PRI, one for the PAN, and one for the PRD. For example, if the PRI holds a meeting in a municipality at a given time period, the PAN do not visit, and the PRD hold a rally, the outcome would be [1,0,2]. The first-step predictors consist of one matrix of municipal characteristics. I use three variables: *Population size*, *Vote HHI*, and *2012 dummy*. The variable *Population size* is a measure of how populated a municipality is. Sparsely populated municipalities are not worth a visit. The variable *2012 dummy* is a dummy variable indicating if the observation is in 2012 rather than 2006. The visits are, according to Langston and Rosas (2016), a common strategy in newer democracies that still have clientelistic networks. As a state's democracy grows and evolves, these visits should be less common. Finally, to capture competitiveness, I include the HHI (Herfindahl-Hirschman) index of the previous vote as a general measure for party dominance in the municipality. This measure is a sum of the squared previous vote shares for each of the three parties. When it is high, it indicates one party has dominance over the others. These municipalities should be less appealing for parties to visit. Either it is the dominating party and there is little to be gained from a visit, or it is the weaker party and going would be both a waste of resources and potentially damaging.

The second step includes all of the variables used in Langston and Rosas (2016), including the first-step variables. Variables included that are not in the first step are *Gira*, *Concurrent*, *Coparty mayor*, and *Previous vote*. The variable *Gira* is a dummy variable for whether or not the visit was part of a multi-stop tour. The variable *Concurrent* is a dummy indicating whether or not the mayoral race is concurrent with the presidential race. The two main variables of interest are *Coparty mayor*, an indicator for whether or not there is a mayor of the same party, and *Previous vote*, the party's previous vote share. These capture the underlying political support for a party. In the original data, if a party visits a municipality more than once in a given time period it is included twice. For this analysis, to keep the number of observations the same and to not falsely give parties 0 outcomes or repeated outcomes, duplicates are dropped, keeping the highest outcome. This only affects about 150 out of over 3,000 observations. About half of the data are randomly selected municipalities that were not visited by any party.

Analysis

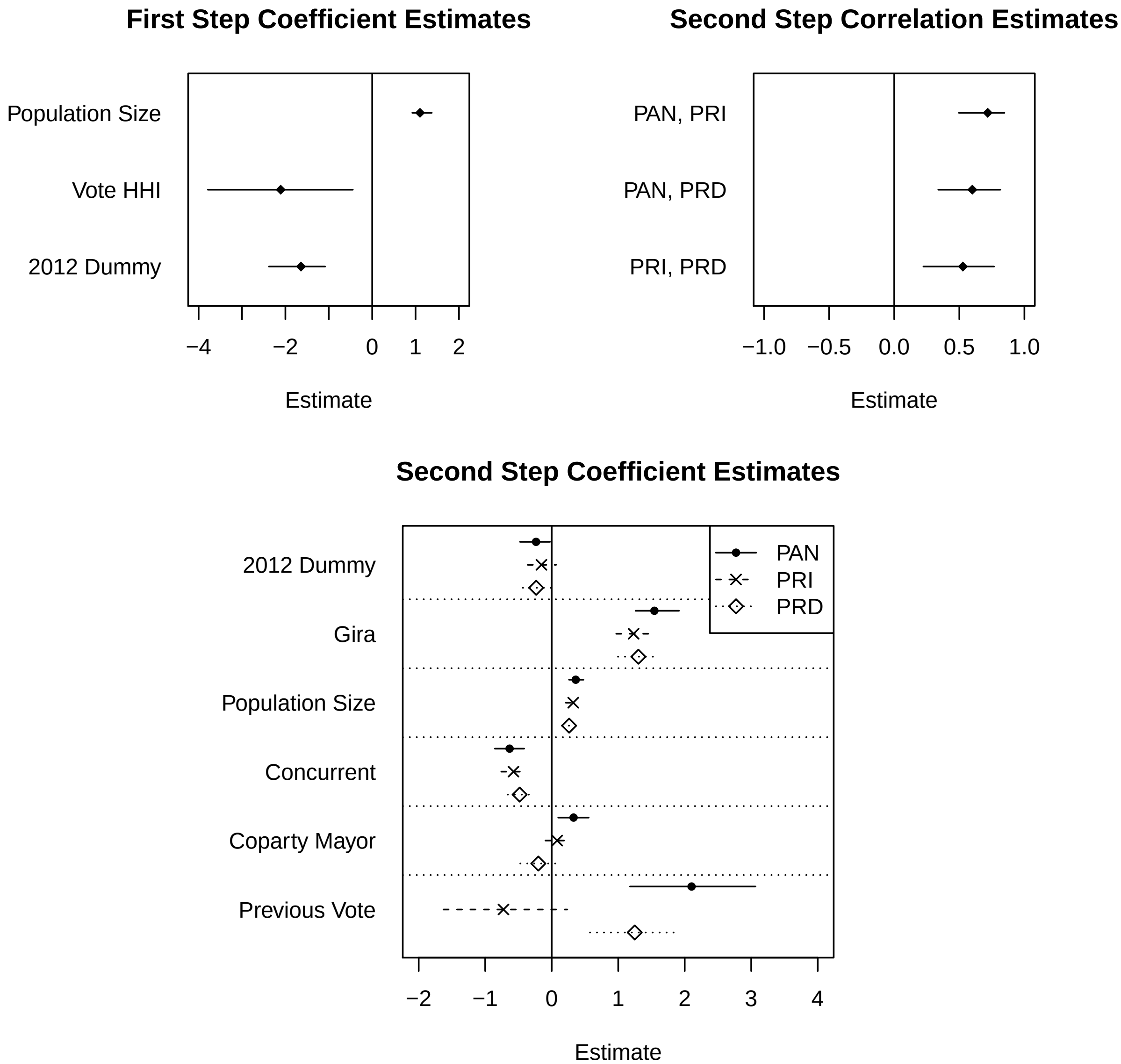
The results of the analysis are presented graphically in Figure 2.4.[16] There is strong evidence for the two propositions. First, the correlation estimates are all positive and reliable. This suggests parties are engaging in Colonel Blotto dynamics and targeting municipalities their competitors also target, and that municipalities have unobserved attributes that make them more or less attractive to parties. Second, the second-step estimates for the parties of the variables of interest vary considerably. The estimates for *Coparty mayor* and *Previous vote* vary across parties, suggesting different calculi for the parties.

For the PAN, having a mayor of the same party reliably increases the odds ratio of moving up a category in the visitation scheme. It is unreliable for the PRI, but indicates that the PRD may be responding to this in the opposite direction. The estimates for *Previous vote* suggest something similar: parties are not responding in the same way to the same factors. Again, the PAN seem to go where they have support, but so do the PRD, while the PRI estimate, though (just barely) unreliable at a 95% level, suggests that the PRI is more likely to visit and hold rallies in municipalities with less support.

Langston and Rosas (2016) find that *Coparty mayor* has a positive effect, while they fail to reject the null that *Previous vote* has any effect. This is somewhat of an unfair comparison,

[16] Two chains of 150,000 iterations were run. The package `superdiag` indicated no evidence of lack-of-convergence, and all $\hat{R}$'s are close to one.

Figure 2.4: Results from the presidential campaign visits in Mexico.



Note: Estimates are shown with 95% credible intervals of the posterior distribution. The top-left panel shows the first-step estimates. All are reliable and in the predicted direction. The top-right panel shows the correlations between parties in the second step. All are reliable and positive, suggesting parties target municipalities that their competitors target. The bottom panel shows the second-step estimates. The variables of interest, *Coparty mayor* and *Previous vote* vary considerably in their estimated effects, indicating that parties strategize differently.

as they test a generalizable pattern on pooled data, but at the same time this highlights the advantage to allowing actors to respond to factors differently. Perhaps a fairer test is to compare these results to a model not allowing correlation, but allowing the second-step estimates to vary, as in the second simulation exercise. The first-step estimates are nearly identical, but the second-step posteriors are fairly different, with the estimate for previous vote of the PRI no longer reliable even at the 90% level. The second-step estimates from both models are compared in the Appendix. Perhaps most importantly, we are losing the ability to draw substantive conclusions from the correlations in the latter model. Further, the simulation exercises suggest we should trust the posteriors from ZIMVOP.

2.8 Conclusion

Binary and ordered outcomes are often of interest in political science, but analyses of these data can be problematic. There is often an excess of zeros, and the data generating processes of seemingly unrelated outcomes may in fact be related. These issues can lead to inaccurate and inefficient estimators. This chapter proposes a new extension to existing models, ZIMVOP, that appropriately addresses these issues and in doing so opens the door to answering questions we have been unable to answer. ZIMVOP not only provides better estimates of our parameters of interest as shown in the simulation exercises, it also helps us recover useful information that otherwise would be lost. We can investigate the nature of the related processes causing observed outcomes and analyze the varying effects of observables at the zero-inflation step and the outcome step. I applied ZIMVOP to presidential campaign visits in Mexico to illustrate the model's benefits.

Though ZIMVOP outperforms existing models in certain contexts, it is not as straightforward to interpret as its simpler alternatives. ZIMVOP is also fairly computationally intensive, with some models taking a very long time to converge. Further, it only applies to cases in which we believe processes are related and an excess of zeros suggests two steps of data generation. Nevertheless, the applicability of ZIMVOP is potentially wide.

For example, decision-making often results in unanimity. During U.S. Supreme Court agenda setting, most Justices vote to not hear the case. If we want to explain the likelihood of SCOTUS accepting a case, there is very likely a relationship between the processes of one Justice voting to hear the case and another Justice wanting to hear the case that is unexplained from observables. This would in fact be a very interesting question because some correlations, such as those of ideologically proximate Justices, may be positive, while those of ideologically distant Justices may be negative. Survey questions are also a very well-suited application of the model, with frequent pooling at “do not know” or “indifferent.”

Finally, ZIMVOP as proposed in this chapter has a zero-inflation step modeling the all-zero state. In other words, though the outcome is multivariate, the zero-inflation step is univariate. This is computationally less demanding and in general theoretically sensible. There are municipal-level characteristics that make no municipality visitable, for example. With this example, particularly when considering that the underlying processes are related, there would be no reason to deviate from this univariate zero-inflation. However, ZIMVOP could easily allow for inflation in each component, potentially opening up new avenues of application.

Modeling Without Conditional Independence: Gaussian Process Regression for Time-Series Cross-Sectional Analyses

In political science research, we frequently rely on the conditional independence assumption. That is, potential outcomes are unconditional on our explanatory variable, conditional on our covariates. Related, and perhaps more simply, we assume that our error terms are independently and identically distributed. Yet it is well-understood that this assumption is frequently violated.

These modeling and data features, representing violations to conditional independence, are particularly prevalent in time-series cross-sectional (TSCS) and panel analyses. To account for these violations, the most common solution is to model the data-generating process ignoring violations of conditional independence then correct the standard errors afterwards to account for non-independence. These procedures, such as robust clustered standard errors or panel corrected standard errors, have well-known problems, including biasing estimates and standard errors and leading to incorrect inferences (Esarey and Menger 2017; King and Roberts 2015).

In this chapter, I introduce Gaussian process regression (GPR) to political science as a solution by which we directly model non-independence (Gibson et al. 2012; Gramacy and Lee 2008; Rasmussen and Williams 2006; Gramacy 2005). GPR is a very flexible Bayesian machine-learning algorithm that concurrently models the error distribution as a function of the input space and the outcomes conditional on the covariates.

As a consequence of explicitly handling non-independence, GPR outperforms existing alternatives in terms of bias, efficiency, and false positives and negatives in TSCS analyses. GPR is also relatively insensitive to the common problems arising in real-world data analyses including small cluster sizes or small intra-cluster sample sizes.

The outline of the chapter is as follows. First, I discuss the issues and current solutions to TSCS analyses, highlighting the drawbacks and benefits of extant modeling strategies. Second, I specify the GPR model, with some discussion of the theory and spirit of the approach. Third, I apply the model, showing improvements to extant approaches through several simulation exercises. I then demonstrate that inflation in Latin America leads to improved sentiment towards the United States, while other modeling strategies cannot reject the null hypothesis of no effect. This exercise demonstrates that ignoring or improperly correcting for violations of the conditional independence assumption may lead to false conclusions. It also shows that the degrees of freedom lost by the most common strategies under-estimate the causal impact of an explanatory variable on the outcome. In this subsection, I also discuss issues of over-fitting, out-of-sample predictions, and model selection. I next replicate Getmansky and Zeitzoff (2014), showing that the authors under-estimate the effect of terror threats on right-wing votes in Israel. In the replication, different modeling strategies lead to very different substantive results. This highlights the importance of model choice, and the general under-estimation of the effect again demonstrates the potential issues that arise when decreasing the degrees of freedom in the model. I conclude in the final section with future directions. As part of this project, I provide Stan code as implemented in the `R` computing environment that is straight-forward to use.

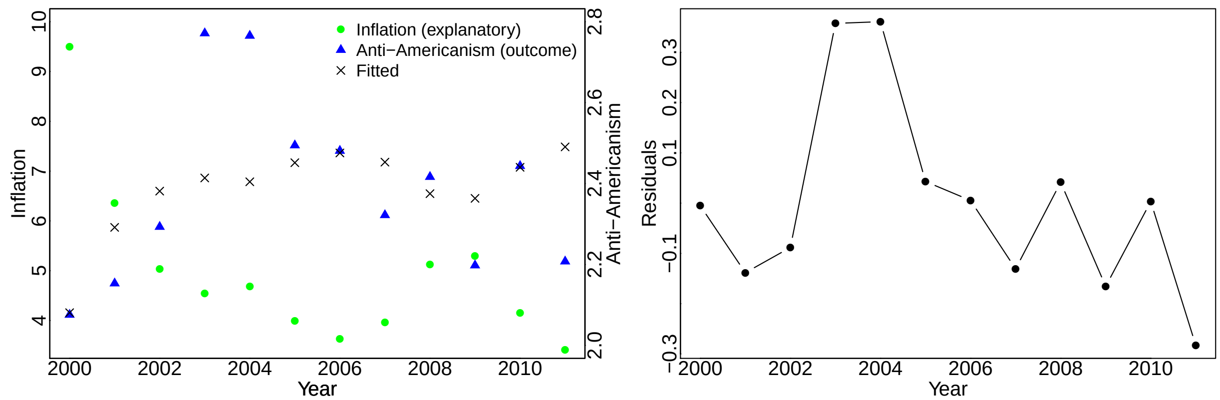
3.9 Time-Series Cross-Sectional Analyses: Issues and Solutions

TSCS data can be problematic to analyze for several reasons. Repeated observations of a unit and across units may be related to one another in a way our model cannot capture directly. This could be due to heterogeneity across units, within unit time trends, or time trends across units, for example. These issues lead to violations of the frequently applied modeling assumption that error terms are independently and identically distributed. Furthermore, average outcomes between units may vary based on unobservable or unmeasured factors. Both parameter estimates and their standard errors can be misleading and biased when inappropriately modeled under these conditions, often leading to spurious results (Granger and Newbold 1974).

As an example, consider how we would approach explaining anti-Americanism in Mexico as a function of its level of inflation. Figure 3.5 shows levels of inflation from 2000–2011 (Arel-Bundock 2013) and anti-Americanism as captured in the Latinobarómetro (Baker and Cupery 2013). I fit a simple linear model with inflation predicting anti-Americanism and include the fitted values in the plot. The right panel shows the residuals of the fit. There are two noticeable features of this model. First, we are assuming all variation in the outcome (anti-Americanism) is attributed to variation in the explanatory variable (inflation), and as such, variation in the fitted values mirrors variation in the explanatory variable. Second, and related, because we assume no trend in anti-Americanism that is unexplained by trends in inflation, the residuals show temporal patterns.

Figure 3.5: Inflation and anti-Americanism in Mexico

(a) Data points and fitted values (b) Residuals of model fit



Note: The left panel shows inflation in Mexico (green dots), anti-Americanism in Mexico (blue triangles), and fitted values of a linear model (black x's) with inflation explaining anti-Americanism. The right panel shows the residuals of the model. Trends in the outcome are assumed to mirror trends in the explanatory variable, yet the residuals demonstrate that there is likely a trend in the outcome unexplained by trends in the explanatory variable.

Common modeling approaches ignore the violations to the conditional independence assumption and adjust the standard errors afterwards, allowing for clustering at the unit of observation. Serial correlation of observations within units may also be accounted for through an autoregressive framework. Fixed effects may be employed both as a way to adjust the standard errors and account for heterogeneity of the units. Alternatively, random effects serve a related purpose in multilevel modeling approaches. Unfortunately, there is no best practice, as every data set is unique. In this section I discuss the advantages and disadvantages of the more common, although by no means exhaustive, strategies used in political science research, after a brief review of current practice.

3.9.1 Prevalence of Current Approaches

I reviewed all time-series cross-sectional articles[17] in the last three years[18] from *American Political Science Review*, *American Journal of Political Science*, *Journal of Politics*, and *Comparative Political Studies*, noting the strategies employed to deal with the data. Of the 320 TSCS articles reviewed, 82 run a model with group and time fixed effects (FE), 47 with only a group fixed effect, and 25 with only a time fixed effect.[19] 48 articles use random effects (RE) modeling strategies. 172 articles use robust clustered standard errors (RCSE), 17 use panel corrected standard errors (PCSE), and one uses Newey-West standard errors. 23 lag one or more dependent variables (LDV). 17 use some time trend specification. Finally, one article uses a first differences approach. Based on the prevalence, I will discuss and compare GPR to FE with RCSE, PCSE, and LDV, and to RE specifications, the most common strategies.

3.9.2 Fixed-Effects Specifications

Fixed-effects (FE) regressions are often used when the researcher believes there is heterogeneity in baseline outcomes across groups, and this heterogeneity is based on omitted variables that are correlated with included regressors (Greene 2003, p. 359). Essentially, a different intercept is estimated for each unit. Specifically, let i index units, $\mathbf{y}$ be a vector of outcomes, $\mathbf{X}$ be a matrix of covariates, and $\boldsymbol{\beta}$ our estimand. The group fixed-effect models the data according to:

$$\mathbf{y} = \boldsymbol{\gamma} + \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\epsilon}, \tag{3.1}$$

[17] Models such as survival or hazard models are not included in this discussion.

[18] The end date is July 2017.

[19] Note that articles often use a mixture of strategies in different models, so the number will not sum to the total, 320.

where $\boldsymbol{\gamma}$ is a vector of the unit-level intercepts for each unit i.

There are a few major drawbacks to this general modeling strategy. First, for the identification of the covariance matrix within-group variation of the data is required (Greene 2003, p. 362). If there is little variation of the explanatory variable the results of the model are not asymptotically unbiased or efficient. If there is no variation in a given variable, the effect cannot be estimated at all (Greene 2003, p. 360). Second, this method is only consistent in large data sets. In other words, while consistent asymptotically, we often have to analyze relatively small data sets and the method may produce misleading results in these situations (e.g., Esarey and Menger 2017). Fixed effects models also rely on the assumption that a unit's error term and the constant capturing the unit's unique characteristics are not correlated with other units (Greene 2003, p. 359).

In addition to including a fixed effect for each group, it is also common to include a fixed effect for each time period in TSCS analyses. We estimate a separate intercept for each group and for each time period, indexed by t. We then model the data according to:

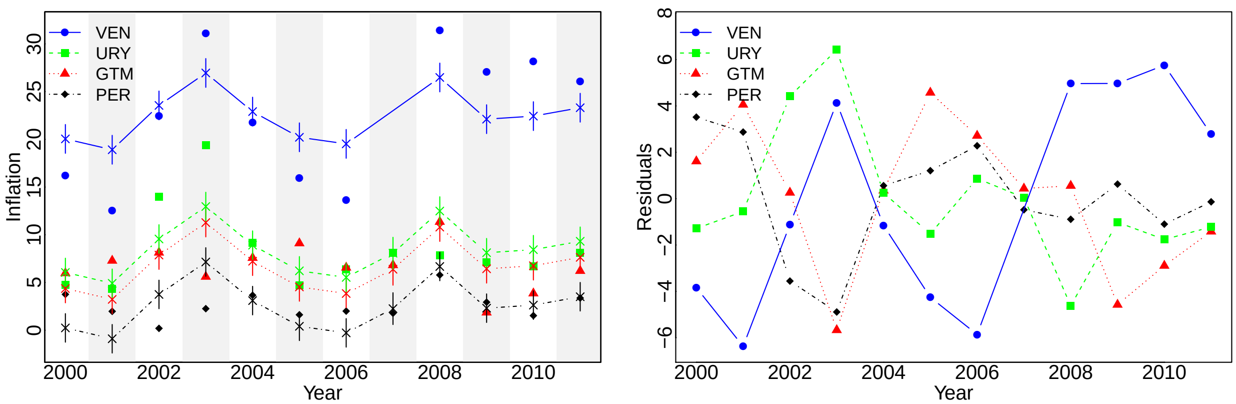
$$\mathbf{y} = \boldsymbol{\gamma} + \boldsymbol{\tau} + \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\epsilon}, \tag{3.2}$$

with $\boldsymbol{\tau}$ the vector of time-specific intercepts for each time t.

We might consider a model of this sort if we believe there are confounders across time experienced equally by every unit, again correlated with our variables of interest. While the goal is to allow for time effects, the same effect is assumed for each unit, which is often unrealistic. For example, returning to the question of inflation in Latin America, Figure 3.6 shows inflation in four Latin American countries from 2000–2011, with the posteriors of a fitted two-way fixed effects model.

Figure 3.6: Two-way fixed-effects model fit and residuals of inflation in Latin American countries over time

(a) Model fit (b) Residuals of model



Note: Displayed in the left panel are actual inflation in four Latin American countries, Venezuela, Uruguay, Guatemala, and Peru, from 2000–2011 with a 50% linear fixed-effects posterior of the observations. The only information in the model is an indicator for the country and a continuous time variable. The right panel shows the residuals of the fit.

Unrealistically, the trend for each country is assumed to be the same, leading to poor model fit, with only their averages, i.e. group-level intercepts, allowed to vary. Further, as seen in the residiual plot in the right panel, there are time trends in the residuals within countries, and because each year is essentially averaging the "effect" of time, the residuals are not independent, but instead mirror other countries. If the residual of one country is high, the others necessarily must be lower on average.

In addition to these issues, the substantial cost in terms of degrees of freedom in the two-way FE model is often not justified, and the model ignores timewise evolution. Time effects are unlikely independent from the previous time effect, for example, but this is ignored with the modeling strategy (Greene 2003, p. 364). In addition, these models can accentuate problems of multicollinearity among regressors (Baltagi 2008, p. 35).

3.9.3 Random-Effects Specifications

Fixed-effect regression can be thought of as applying only to the units in the study, not out-of-sample units. This both means the strategy does not allow out-of-sample inference, and more theoretically assumes the population being studied is complete. In this case it is reasonable to assume the model is constant (Greene 2003, p. 370). Alternatively, we can model the individual constant terms as randomly distributed across units (Greene 2003, p. 371).

This modeling strategy, known as random effects (RE) regression, is only appropriate if the omitted unit effects are uncorrelated with the regressors, and the sampled units are drawn from a large population. Random effect estimation reduces the number of parameters to be estimated, allows for time-invariant predictors, and allows for out-of-sample inference. However, if the assumptions are not valid, the estimators are inconsistent (Greene 2003; Mundlak 1978). Further, while the specification allows disturbances to be correlated within groups, it does not allow for a correlation between groups. As with fixed effects, random effects can be estimated for group and/or time.

To determine whether FE or RE is more appropriate given the data, the most common test is Hausman's specification test (Greene 2003; Hausman 1978). However, this test has been criticized for assuming the RE model as the null (Baltagi 2008, p. 65–73), and has been shown to perform poorly under certain violations to the underlying assumptions, with no clear alternative (Esarey and Jaffe 2017).

3.9.4 Clustering Standard Errors

Clustered standard errors are most common when coupled with a fixed effects model. Clustering the error terms allows for dependency of the residuals within units. The most widely used variants of clustering standard errors is the modification of robust standard errors

(White 1980) to allow for clustering, known as robust clustered standard errors (RCSE) (Cameron, Gelbach, and Miller 2011). Although consistent in various situations (Liang and Zeger 1986), the use and prevalence of RCSE has been criticized for leading to false positives and negatives when the number of units is small (Esarey and Menger 2017). Further, King and Roberts (2015) argue that when robust standard errors and the maximum likelihood standard errors diverge, this is evidence that the model is misspecified, suggesting their use should be limited to testing specification problems (see also Tauchen 1985; Newey 1985; White 1982). There is often a divergence of the two in the presence of serial correlation (Cameron and Miller 2015), suggesting that serial correlation can lead to a misspecified model.

Another clustering alternative is panel corrected standard errors (PCSE) (Beck and Katz 1995). PCSE are derived with the expectation that errors are serially correlated within units and correlated between units at a given time. It is a method for computing a heteroskedastic-consistent covariance matrix specifically for TSCS data. Again, this method is frequently coupled with fixed effects when the researcher believes each unit has a different baseline outcome, and the errors are clustered within units. This method also relies on a large number of units, and can lead to very misleading results when the number of units is small, with no general rule-of-thumb as to how small a number is inappropriate for this strategy. Further, when the errors are correlated with a regressor the standard errors can become much larger than desired (Cameron and Miller 2015). Finally, this two-way clustering does not pick up on all potential correlations in the data (Cameron and Miller 2012).

As outlined in Esarey and Menger (2017), there are other, more exotic clustering strategies in the statistics literature that are largely absent from the political science literature. For example, cluster-adjusted t-statistics (CATs) (Ibragimov and Müller 2010), wild cluster bootstrapped t-statistics (WCBSTs) (Cameron, Gelbach, and Miller 2008), and pairs cluster bootstrapped t-statistics (PCBTs) (Cameron, Gelbach, and Miller 2008) are all clustering alternatives. However, like in Esarey and Menger (2017), preliminary simulation exercises demonstrated that more traditional approaches out-perform these less-known clustering options.

3.9.5 Correcting for Serial Correlation

Serial correlation in the outcome can be either due to serial correlation of the error terms, in other words the outcome itself, or due to serial correlation of an explanatory variable. Ignoring the correlation can lead to very misleading results (Granger and Newbold 1974). Although PCSE corrects for serial correlation in the error terms, RCSE has no temporal component, nor does a varying-intercept multilevel model. However, adjustments, such as Newey-West standard errors (Newey and West 1987; Andrews 1991), can account for serial dependence in the error terms in static data-generating processes. If a dynamic data-generating process is suspected, lagging the dependent variable can account for both serial correlation and the underlying dynamics (Beck 1985). These models, however, perform poorly when the process is static or weakly dynamic, or the dependent variable is non-stationary (Keele and Kelly 2005). In the latter case, modeling first differences or including time trends may alleviate some of the issues, but these do not handle breaks in the trends well. Further, these approaches assume a sequential data-generating process, ignoring the possibility of endogeneity and an effect of anticipation. I will now discuss GPR as an alternative modeling choice for TSCS data and compare GPR to these most common strategies.

3.10 Gaussian Process Regression for TSCS Data

GPR is primarily known for its uses in machine learning classification and prediction (Rasmussen and Williams 2006), but the models have been utilized to make inferences about populations as well (e.g., Kirk and Stumpf 2009; Huang, Zhang, and Schölkopf 2015; Garg, Singh, and Ramos 2012; Qian, Zhou, and Rudin 2011; Gibson et al. 2012). Monogan and Gill (2016) use a GPR approach, which they refer to as Bayesian kriging, to estimate a posterior density blanket of citizens' ideologies across the United States. Despite having relatively sparse data, the method allows for any level of geographic aggregation and provides an estimate, with uncertainty, of the ideology of the region by smoothing across space.

This smooth blanket over the U.S. is a useful introduction to conceptualize GPR. Data, in Monogan and Gill (2016) a measure capturing ideology, are not conditionally independent. The average ideology in a town is likely similar to the ideology in its neighboring towns even conditional on covariates. We can consider these outcomes (ideology in neighboring towns) as coming from a joint normal distribution if we assume each realization comes from a separate normal distribution, with the outcomes sharing a correlation.

The mean of the multivariate normal can be parameterized allowing for standard inferences on parameters of interest, i.e., how changes in an explanatory variable correlate with changes in a dependent variable. The variance-covariance matrix is similar to kernel regularized least squares (Hainmueller and Hazlett 2013), but different in that the approach is fully Bayesian. This allows estimation of a much more flexible variance-covariance matrix in a kernel with weakly informative priors rather than setting the tuning parameters, and provides a posterior of variance-covariance matrices through MCMC. The effect of a variable on an outcome is also not easily interpretable in KRLS, whereas the sampled mean in GPR provides easily understood posteriors of our parameters of interest.

The "smoothing" across geographic space is intuitive, but in reality we can smooth over any input dimension we choose, including, of course, time. The same way neighboring towns are likely heavily correlated, so too are temporally proximate observations, or observations sharing similar explanatory variables. These correlations can also vary from dimension to dimension. In other words, geographic proximity and temporal proximity need not share the same correlation. Rather than considering data as independent, or even sequential, we can think of all observations as coming from one joint distribution, with data points close to each other in the covariate space likely similar. The flexibility of the model makes it ideal for modeling situations in which there are violations of the conditional independence assumption but the degree of these correlations is not known *a priori*.

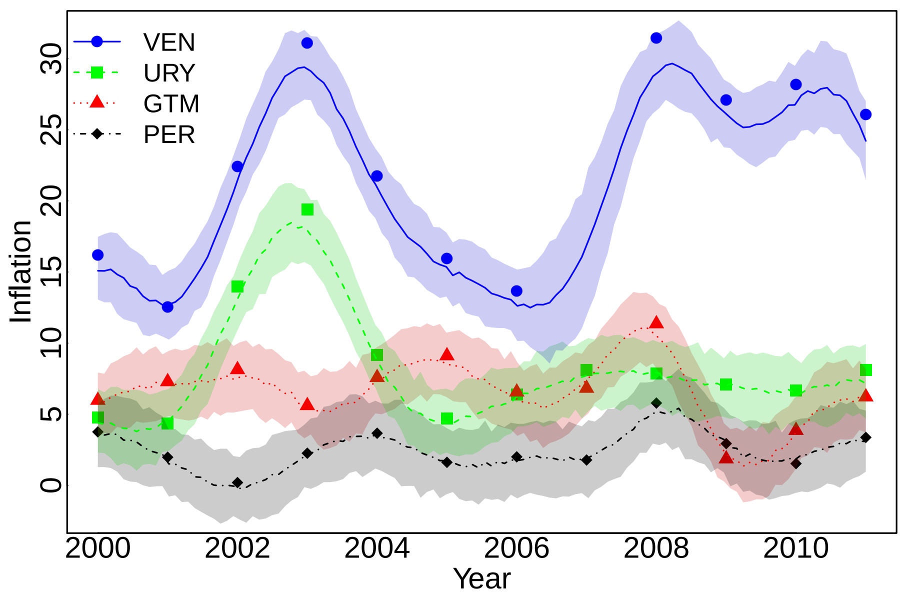
These features of GPR make the model uniquely suited to deal with TSCS data. Serial correlation within units and across units, between unit heterogeneity, and correlations between unit-level averages (i.e., intercepts) and unit-level explanatory variables can all be accounted for in the learning kernel and the parameterization of the mean function. Perhaps most importantly, we do not require heroic assumptions about the nature of these data features, unlike the most commonly applied approaches.

To continue with the example of inflation, Figure 3.7 shows the same inflation data as Figure 3.6 in four Latin American countries over time, with posterior predictive distributions generated with GPR using only a dummy variable for the country and a linear measure of time. Rather than model the outcomes with typical distributional assumptions where errors are assumed to be independent and identically distributed, all errors are modeled jointly as coming from a multivariate normal distribution that explicitly allows for non-independence between observations. The variance-covariance matrix is learned from the data, smoothing the posterior over the input space. As can be seen in the figure, the posteriors track the outcomes very well, and GPR provides reasonable predictions even where there are no covariates. Further, despite the flexibility, standard inferential techniques are still straightforward. GPR provides estimates of the posterior distributions of the parameters of interest in a fashion similar to any Bayesian model.

3.10.1 GPR Specification

Now that I have explained the intuition behind the model, I will fully specify the TSCS GPR model, both for completeness and to further aid in the intuition. We would typically assume

Figure 3.7: GPR fit of inflation in Latin American countries over time



Note: Displayed are actual inflation in four Latin American countries, Venezuela, Uruguay. Guatemala, and Peru, from 2000–2011 with a 50% GPR predictive posterior of the observations. The only information in the model is an indicator for the country and a continuous time variable.

a data generating process of observations in a linear model according to:

$$y_i \sim \mathcal{N}(\mathbf{x}_i\boldsymbol{\beta}, \sigma^2), \tag{3.3}$$

with $\mathbf{x}_i$ a vector of covariates for observation i and σ^2 a scalar of the variance for all observations. However, with GPR, we assume a multivariate normal distribution modeling all outcomes jointly. Specifically, the vector of outcomes, $\mathbf{y}$, is distributed:

$$\mathbf{y} \sim \mathcal{MVN}(\mathbf{X}\boldsymbol{\beta}, \sigma^2\boldsymbol{\Omega}). \tag{3.4}$$

In Equation 3.4, the mean is still a linear function of the covariate space, but because it is a multivariate normal the input is the entire covariate matrix for all observations, $\mathbf{X}$. This covariate matrix includes unit indicators (akin to fixed effects) to account for unit heterogeneity. The linearity of the mean function allows for posterior inferences on our parameters of interest ($\boldsymbol{\beta}$), while allowing the kernel $\boldsymbol{\Omega}$ to learn the complex underlying data-generation from the data itself.

I deviate from previous implementations of the Gaussian process by allowing the data used in the mean function to be different from the data used in the kernel. For TSCS purposes, the kernel includes all covariates, including unit indicators, and includes a continuous time input. Time is not included in the mean function, as we have no interest in the effect of time, but we want to smooth over this dimension to account for time trends within and between units.

In order to smooth over our covariate space, we want observations that are close in the covariate space to share a correlation, while points further in the covariate space to share less correlation. Therefore, we sum the squared distances across input columns. We then take the exponential of the negative sum to capture the correlation. Let m indicate the number of columns of the input matrix to the kernel, and let p index these columns in the summation. Also let j and i indicate two observations. We start with setting the correlations according to:

$$\boldsymbol{\Omega}(\mathbf{x}_j, \mathbf{x}_i) = \exp\left\{-\sum_{p=1}^{m}(x_{pj} - x_{pi})^2\right\}. \tag{3.5}$$

However, we want time one and time two to perhaps have a different correlation than location one and location two, for example. For this purpose, we introduce another parameter to the kernel, $\boldsymbol{\zeta}$, a vector learned from the data that estimates the degree of correlation relative to distance for each dimension. The kernel then becomes:

$$\boldsymbol{\Omega}(\mathbf{x}_j, \mathbf{x}_i|\boldsymbol{\zeta}) = \exp\left\{-\sum_{p=1}^{m}\frac{(x_{pj} - x_{pi})^2}{\zeta_p}\right\}, \tag{3.6}$$

which is a symmetric matrix with all diagonal elements equal to one, because the sum of the distance of the covariate space will be zero for elements that are identical, and all off-diagonal elements represent a correlation. However, to ensure the matrix is non-singular, we add a small constant to the diagonal elements. This is necessary because points very close in the covariate space will have almost one in the kernel matrix, and this leads to numerical instabilities. This is called the nugget.

To distinguish between inputs to the mean and inputs to the kernel, allow $\tilde{\mathbf{X}}$ to indicate inputs to the mean and $\mathbf{X}^*$ to indicate inputs to the kernel. The fully specified model is therefore:

$$\mathbf{y} \sim \mathcal{MVN}(\tilde{\mathbf{X}}\boldsymbol{\beta}, \sigma^2\boldsymbol{\Omega}), \tag{3.7}$$

$$\boldsymbol{\Omega}(\mathbf{x}_j^*, \mathbf{x}_i^*) = \boldsymbol{\Omega}^\dagger(\mathbf{x}_j^*, \mathbf{x}_i^*) + \delta g_{j,i}, \tag{3.8}$$

$$g_{i,j} = \begin{cases} 1 & \text{if } j = i, \\ 0 & \text{otherwise.} \end{cases} \tag{3.9}$$

$$\boldsymbol{\Omega}^\dagger(\mathbf{x}_j^*, \mathbf{x}_i^* | \boldsymbol{\zeta}) = \exp\left\{ -\sum_{p=1}^{m} \frac{(x_{pj} - x_{pi})^2}{\zeta_p} \right\}. \tag{3.10}$$

This kernel is known as the separable-exponential kernel. Other choices are possible (e.g., Rasmussen and Williams 2006, Ch. 4), but are left out for simplicity in the presentation. The intuition is that the closer the inputs are in the hyperplane, the more correlated the observations will be. However, each dimension has a separate estimate of the ζ parameter, a scaling of the degree of correlation. The vector $\boldsymbol{\zeta}$ is estimated in the learning resulting in a posterior distribution. Now that the model is specified, I will discuss the priors used in estimation.

3.10.2 Priors

The choice of priors comes from a combination of computational efficiency, a lack of prior information, and constraints on the parameters. Other choices can be made, and inferences from simulation exercises are not sensitive to the choices. However, scaling variables is recommended if the scales are very different, or priors should be adjusted. The nugget, δ, needs to be greater than zero. Let $\delta \sim Exp(1)$ to ensure a positive term with weak prior information that it would not be sensible for it to be too large, as that would lead to a poorly fitting model. The nugget is only meant to allow the matrix to be non-singular, and large values would add noise around data points that is already accounted for by the variance σ^2.

The individual ζ terms also need be greater than zero, and we wish to be non-informative as to whether a given dimension has a wavy or smooth surface (Gramacy and Lee 2008). As such, we let each ζ be a mixture of gamma distributions, $\zeta_p = \frac{\zeta_{p1}+\zeta_{p2}}{2}$ with $\zeta_{p1} \sim G(1, 20)$ and $\zeta_{p2} \sim G(10, 10)$, giving roughly equal weight to parameterizations of wavy and smooth populations.

I set the variance that pre-multiplies the kernel to have an inverse-gamma prior distribution, $\sigma^2 \sim IG(1, 1)$, again to be fairly non-informative, and to ensure a positive term. Finally, $\boldsymbol{\beta}$ has a diffuse normal prior, specifically $\boldsymbol{\beta} \overset{iid}{\sim} \mathcal{N}(0, 3)$. Hyperpriors can be set, for example the variance of the β's, however this slows down estimation with no discernible gain in inferences. I fit the model using Hamiltonian MCMC in Stan (Carpenter et al. 2016) through the `R` package `RStan` (Stan Development Team 2016).

3.11 Applying GPR

I now apply GPR to simulated data to demonstrate its benefits. After validating the method through the simulation results, I analyze the effect of inflation on anti-Americanism in Latin America. I find that inflation decreases anti-American sentiment, but competing models are unable to reject the null. I then replicate and expand upon Getmansky and Zeitzoff (2014), showing that the threat of terror has a positive effect on right-wing voting, but the effect is under-estimated in the original analysis and when applying the competing models. Both applications show that modeling choice has a substantive impact on our conclusions, and that over-specifying the models and decreasing the degrees of freedom decreases the explanatory power of an independent variable. Further, there are likely issues with extant approaches due to small sample sizes, serial correlation, and more.

3.11.1 Implementation on Simulated Data

In this section, I compare the results from GPR to those from the most common approaches to handling time-series cross-sectional data under situations with problematic data-generating processes. Specifically, I compare GPR to regressions using group and time fixed effects using RCSE, PCSE, and lagging the dependent variable (LDV), and random effects models using group and time effects (RE). GPR outperforms these extant alternatives when there is serial correlation in the explanatory variable and the error terms within units, when there is correlation between the explanatory variable and the unit-level intercept, when there is correlation between an unrelated variable and the unit-level intercept, and when the number of units or observations within a unit is small. GPR performs at least as well as the extant alternatives even when these issues are less prevalent. In most circumstances, the estimator is more efficient, less biased, produces better coverage probabilities, and produces fewer false positives and negatives.

Serial correlation in error terms and explanatory variable within groups

A very common characteristic of time-series cross-sectional analyses is the existence of serial correlation in the explanatory variable (e.g., inflation at time t in a country is correlated with inflation at time $t-1$). Serial correlation also commonly exists in the dependent variable, or the error terms (e.g., anti-Americanism at time t in a country is correlated with anti-Americanism at time $t-1$). These correlations over time may arguably be unrelated to other variables, but simply trends that are not easily captured without heroic assumptions.

GPR is well-suited to modeling these data relative to extant alternatives, because the correlation matrix smooths over the input dimensions with minimal assumptions about the degree of these correlations. To demonstrate the GPR benefits, I simulate data from a simple process for 15 units at 15 points in time ($n = 15 \times 15 = 225$), varying the level of serial correlation within the units in 10 separate processes.

Specifically, the main explanatory variable, $\mathbf{x}$, at time $t = 1$ is generated from an independent standard normal across units. Subsequent $\mathbf{x}$ values are generated from a Markov chain within each group, with the correlation ranging from $\rho = 0.0$ to $\rho = 0.9$ by 0.1. All x's are generated according to:

$$x_{i,t} = \rho x_{i,t-1} + \sqrt{1 - \rho^2} F, \tag{3.11}$$

with F representing a draw from the standard normal distribution. The generation of the error terms, $\boldsymbol{\epsilon}$, follows the exact same procedure as the generation of $\mathbf{x}$. The outcome, $\mathbf{y}$, is simply generated as:

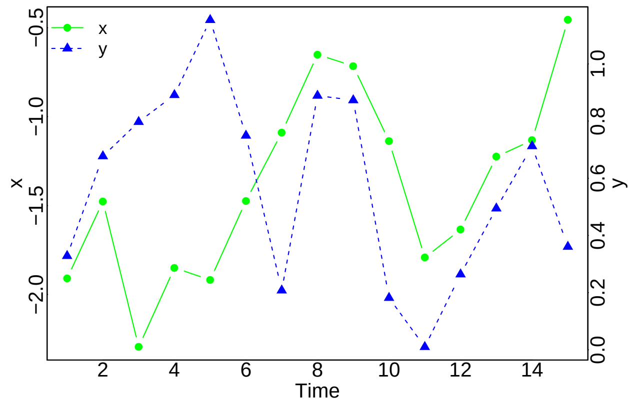
$$\mathbf{y} = 2 + \mathbf{x} + \boldsymbol{\epsilon}. \tag{3.12}$$

Each of the 10 processes is repeated 100 times, resulting in 1,000 analyses.

To visualize the simulated data, Figure 3.8 shows one unit over time when the serial correlation is equal to 0.9. Note that this does not appear unrealistic data, especially when considering the earlier real-world examples, and this is the highest level of serial correlation simulated.

As the serial correlation increases, all models decrease in performance, but the GPR outperforms them all easily. Figure 3.9 shows the average coverage probabilities and the mean squared errors of the estimates as serial correlation increases. All models always correctly identified $\mathbf{x}$ as having an effect distinguishable from zero at the 0.95 level, but they do not capture the true parameter of interest as correlation increases, as shown by the low coverage probabilities, and they have higher mean squared errors. The mean squared errors of the lagged dependent variable model become too large to show graphically and still

Figure 3.8: Example simulated serial correlation data, $\rho = 0.9$



Note: This is one example of a unit in the serial correlation exercise, when the degree of correlation equals 0.9. There is serial correlation in the explanatory variable (x) and serial correlation in the outcome (y) that is due both to the explanatory variable's trend and its own time trend.

compare the other competing models.

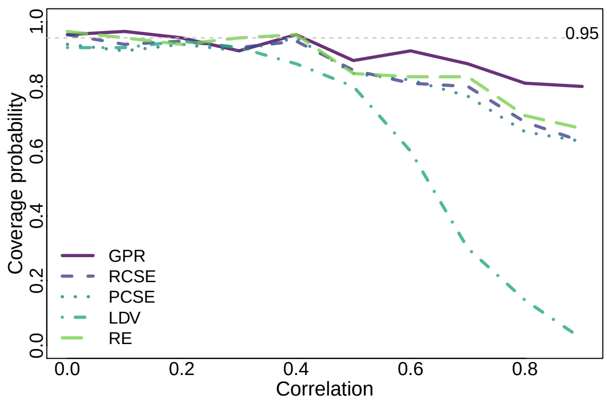
Correlation between the main explanatory variable and the group intercept

Another common problem with time-series cross-sectional analyses is the possibility of a correlation between the main explanatory variable within a unit and the unit intercept. For example, the average level of anti-Americanism within a country may be correlated with the average level of inflation. This makes inferences regarding how changes in the explanatory variable within a group impacts changes in the outcome particularly difficult. In this exercise, I again vary the level of correlation of simulated explanatory variables and the unit intercept and compare the performance of GPR to the most common modeling strategies.

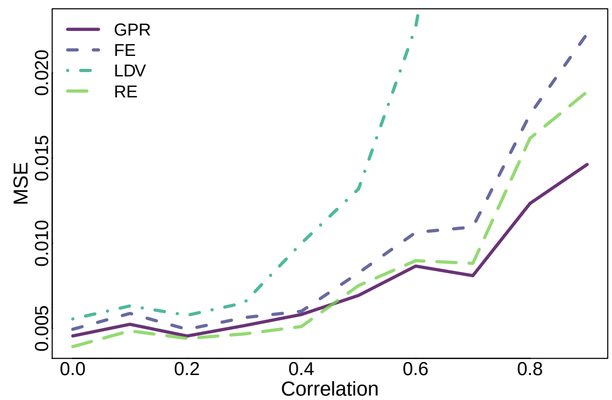
I simulate 15 units with 15 observations (time points) per unit. For each unit, an intercept, γ_i, is generated independently from the standard normal distribution. The explanatory variable, $\mathbf{x}$, is generated with a correlation again ranging from $\rho = 0.0$ to $\rho = 0.9$ by 0.1:

Figure 3.9: Results of simulations with varying degrees of serial correlation in error and explanatory variable

(a) Average coverage probabilities



(b) Mean squared errors of estimates



Note: The left panel shows the average coverage probabilities across models as serial correlation increases with a dashed line at the ideal 0.95 level. The right panel shows the mean squared errors. GPR outperforms the most common modeling strategies by these metrics.

$$\mathbf{x}_i = \rho\boldsymbol{\gamma}_i + \sqrt{1-\rho^2}F, \tag{3.13}$$

where F are draws from a standard normal. The error terms, $\boldsymbol{\epsilon}$, are generated independently from the standard normal. The outcome, $\mathbf{y}$, is generated:

$$\mathbf{y}_i = \boldsymbol{\gamma}_i + 0.35\mathbf{x}_i + \boldsymbol{\epsilon}_i. \tag{3.14}$$

As before, each process is repeated 100 times for a total of 1,000 simulations.

The random effects model has the best true positive rates across simulations, almost always estimating a reliable effect of $\mathbf{x}$ on the outcome. GPR is second best, modestly outperforming the various fixed effects models. However, the random effects model has a very poor coverage probability and a very high mean squared error relative to the other models as the correlation increases. This is shown graphically in Figure 3.10.

Correlation between an unrelated variable and the group intercept

Perhaps a more problematic situation is when there exists a variable in the model that is correlated with the unit intercept but has no effect on the outcome, as this can potentially lead to false positives. In this simulation exercise, I generate unit intercepts $\boldsymbol{\gamma}$ from a standard normal for 15 units, and generate 15 observations per unit, $\mathbf{x}$, from the standard normal. I also generate a vector of disturbances, $\boldsymbol{\epsilon}$, from the standard normal. The outcome is then generated according to:

$$\mathbf{y} = \boldsymbol{\gamma} + .25\mathbf{x} + \boldsymbol{\epsilon}. \tag{3.15}$$

However, we model the data-generating process with an additional variable, $\mathbf{z}$, that is correlated to the unit intercepts $\boldsymbol{\gamma}$ at $\rho = 0.0$ to $\rho = 0.9$ by 0.1. Specifically, I generate the unrelated variable according to:

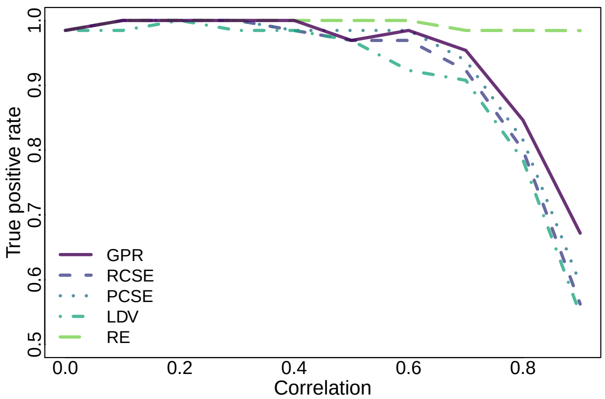
$$\mathbf{z} = \rho\boldsymbol{\gamma} + \sqrt{1-\rho^2}F, \tag{3.16}$$

where F are draws from the standard normal. The assumed data-generating process is therefore:

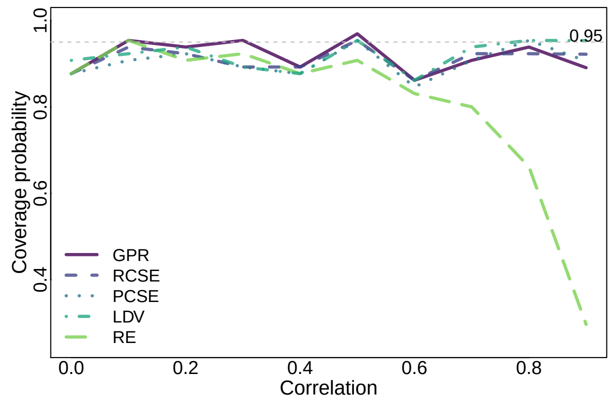
$$\mathbf{y} = \boldsymbol{\gamma} + \mathbf{x}\beta_1 + \mathbf{z}\beta_2, \tag{3.17}$$

Figure 3.10: Results of simulations with varying degrees of correlation between group intercept and explanatory variable

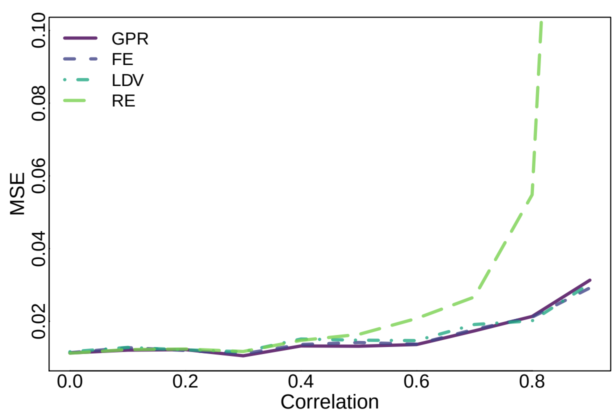
(a) True positive rates across models



(b) Average coverage probabilities

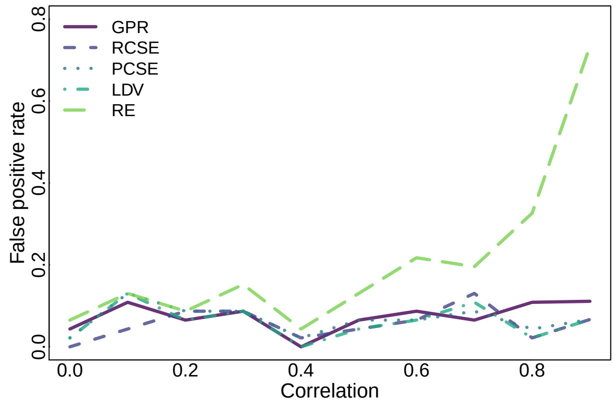


(c) Mean squared errors across models



Note: The top-left panel shows the true positive rates. The top-right panel shows coverage probabilities across models as the correlation of the intercept and explanatory variable increases, with a dashed line at 0.95. The bottom panel shows the mean squared errors of the estimates. While the random effects model has the best power as shown by the true positive rates, it also has the largest mean squared error and smallest coverage probability. GPR is on par with the other models, but mildly better at true positive rates.

Figure 3.11: False positive rates across models



Note: As the correlation between the unit intercept and a variable unrelated to the outcome increases, false positive rates stay low with the exception of the random effects model.

and we are interested in whether we incorrectly estimate $\mathbf{z}$ as having an effect on $\mathbf{y}$. In other words, we should not be able to reject the null that $\beta_2 = 0$. Again, each set-up is run 100 times for a total of 1,000 simulations.

All coverage probabilities, true positive rates, and mean squared errors of the effect of $\mathbf{x}$ on the outcome are very good across all specifications, as there were no issues in that variable. The false positive rates, that $\mathbf{z}$ is estimated as reliably affecting the outcome, are good across all specifications except for the random effects models, which have very high false acceptance rates. This is shown in Figure 3.11.

Varying the number of observations and number of units

Esarey and Menger (2017) find that modeling choices of TSCS data should in part be based on the number of units. In this exercise, I vary the number of units and the number of observations per unit when there is serial correlation as in the first exercise and when there is a correlation in the explanatory variable and the unit intercept as in the second exercise. Both of these issues are very common in TSCS data, and in comparative research in particular we may not have the luxury of a large dataset. It is important, therefore, to analyze the competing models' properties at different amounts of data.

To achieve this, I simulate data sets, varying the number of units from five to 20 by five and the number of observations in a unit from five to 20 by five. This results in data sets that are $5 \times 5, 5 \times 10, \ldots, 20 \times 20$. Each data set is generated identically, only changing the number of observations and units.

Group intercepts, $\boldsymbol{\gamma}$, are generated independently from the standard normal distribution. To create serial correlation in the $\mathbf{x}$ variable within units, the $\mathbf{x}$ is generated according to Equation 3.11, setting the correlation, ρ, to 0.5. This is then added to 1.5 times the group intercept to generate a serially correlated variable that is also correlated with the group intercept. Finally, the error terms are generated according to Equation 3.11 as well. The outcome is generated:

$$\mathbf{y} = \boldsymbol{\gamma} + 0.5\mathbf{x} + \boldsymbol{\epsilon}. \tag{3.18}$$

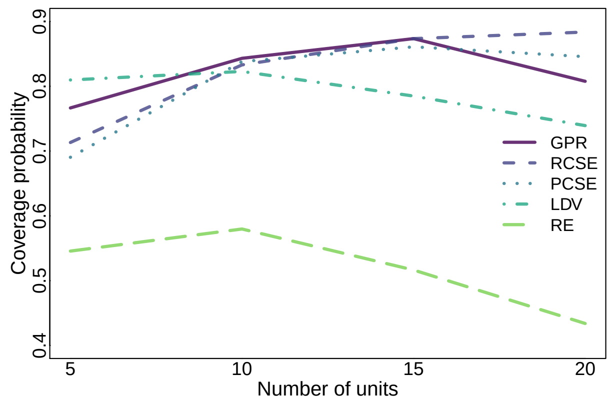
Each simulation set-up is repeated 100 times for a total of 1,600 simulations.

The results of the exercise are shown in Figure 3.12. The first column pools the results of the data set by the number of units varying from five to 20. The first data point, for example, shows the results when there are five units, and the number of observations per unit is five, 10, 15, and 20. The second column does the same but pools on number of observations in a unit.

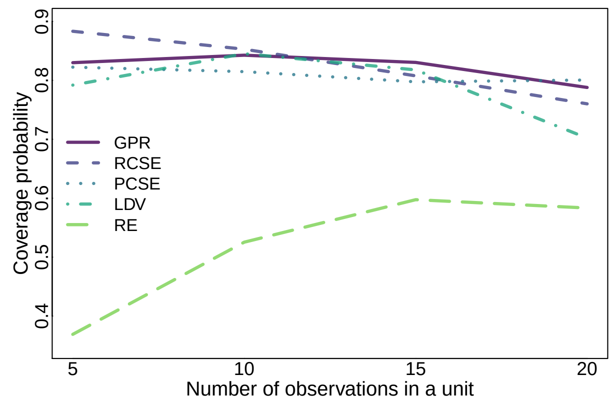
Because the data are problematic, and the average data set in any point is relatively small, the coverage probabilities are fairly low, with the random effects model very low. However, the random effects model has the highest true positive rate, but as demonstrated above this

Figure 3.12: Results of simulations with varying number of units and observations per unit

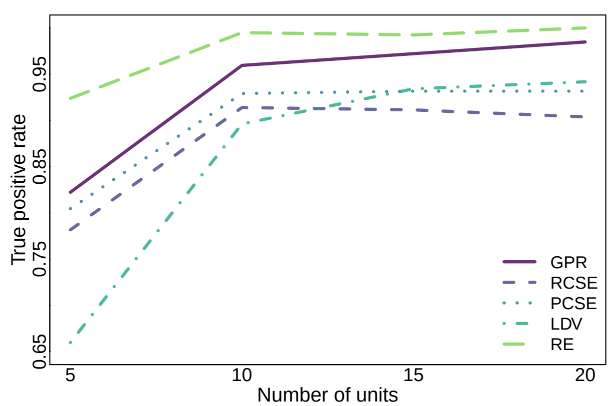
(a) Average coverage probabilities



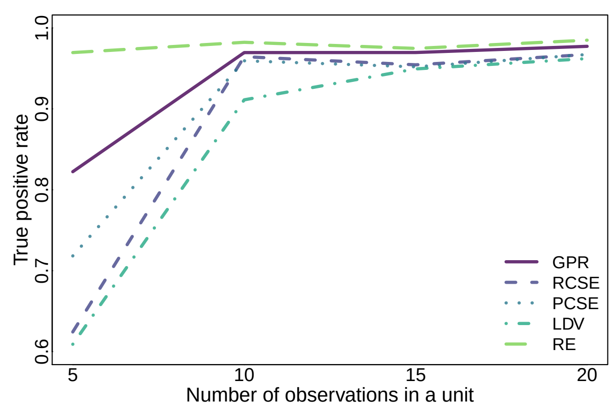
(b) Average coverage probabilities



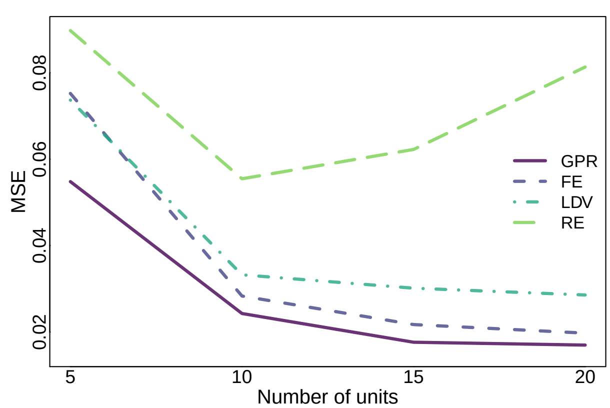
(c) True positive rates



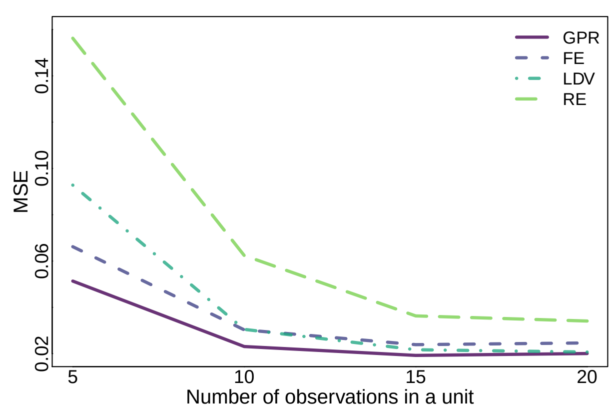
(d) True positive rates



(e) Mean squared errors of estimates



(f) Mean squared errors of estimates



Note: The first column pools on number of units and shows how the models perform as the number of units increases. The second column pools on the number of observations in a unit and shows performance as the number of observations (time periods) increases.

is generally at the expense of potential false positives under similar conditions. The mean squared errors are also highest for the random effects model, and GPR outperforms all other models at the true positive rate and has the lowest mean squared error of the estimates.

These simulation exercises have demonstrated that, under common issues plaguing TSCS data, GPR outperforms the most widely applied extant models. In general, GPR leads to fewer false positives and negatives, is a more efficient estimator, and decreases the error of the estimate. These results are even more pronounced when the sample size is low. Having demonstrated the benefits of GPR under common TSCS data characteristics, I will now apply the model to better understand the relationship between inflation and anti-Americanism in Latin America.

3.11.2 Application: The Effect of Inflation on Anti-Americanism in Latin America

While GPR performs well across an array of metrics, it is important to understand how our modeling choice impacts our substantive conclusions, the goal of quantitative research in political science. It is also important to investigate the potentially deleterious effect of making an inappropriate modeling decision. To show the differences in conclusions drawn from different models, I apply the same models from the simulation exercises to a novel substantive question regarding inflation and anti-Americanism in Latin America.

Do Latin American citizens blame the United States during periods of economic hardship, or do they value the U.S. as a potential avenue to better their economic situation? I argue that the latter is a more common reaction in the region. Specifically, as inflation, one of the most meaningful indicators of economic hardship in Latin America, increases, anti-Americanism will decrease. Or, in other words, pro-Americanism will increase.

Baker and Cupery (2013) find that economic exchange with the United States increases sentiment towards the U.S. in Latin America, arguing that citizens of the region see the economic hegemon as a source of material wellbeing and a destination for exports and emigrants (see also Weiss, Argüello, and Argüello 1995). In addition, there is empirical evidence that economic integration with the United States benefits Latin American citizens (Florensa, Márquez-Ramos, and Recalde 2015), and that Latin American citizens are distinctly in favor of trade openness and economic integration (Baker 2003; Milner and Kubota 2005). Because of this, it is reasonable to expect that a citizen facing lower purchasing power would increasingly value the wealth that relations with the U.S. provide. When they are relatively well-off, on the other hand, they will see less value in the economic effects of U.S. economic relations and focus more on its foreign policy, which unsurprisingly is generally viewed negatively (Fonseca 2008).

Data

To test the proposition, I utilize the question on the Latinobarómetro[20] used by Baker and Cupery (2013), asking respondents their opinion of the U.S. Following Baker and Cupery (2013), I average the responses from the representative sample in each country from 1995–2011 to obtain yearly estimates of the dependent variable, *Anti-Americanism*. 18 countries are included in the barometer, resulting in 256 total observations.[21]

The independent variable of interest is yearly country-level inflation.[22] This is a well-used variable in the literature to measure economic hardship in Latin America (e.g., Petras and Veltmeyer 2016, p. 14), because inflation captures individuals' economic well-being in the region. When inflation is high, goods as necessary as groceries become hard to purchase. Further, inflation directly harms foreign investment and trade, potentially decreasing the positive economic relationship between Latin American countries and the U.S. (Campello

[20] `http://www.latinobarometro.org/`
[21] The question was not asked in all countries every year.
[22] All inflation data was retrieved in `R` using the `WDI` package (Arel-Bundock 2013).

2015, p. 46, 184). As inflation rises, Latin American citizens are aware that the economic benefits of U.S. relations become in jeopardy.

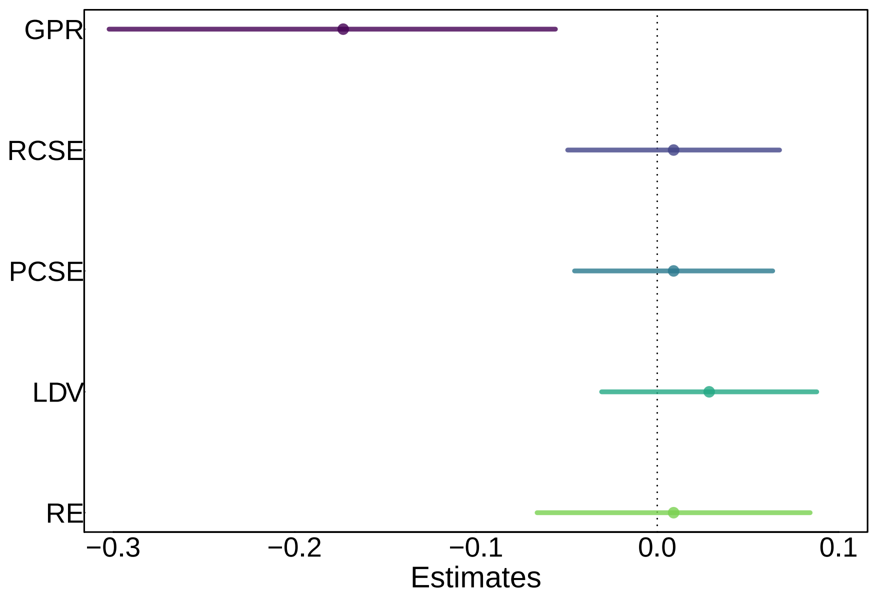
In addition to the main explanatory variable, I also control for *US imports* and *US exports*, as these economic exchanges may impact both the levels of anti-Americanism and the degree of inflation in the country. I control for *US aid flows* as a percentage of the country's GDP for similar reasons. Finally, I include the proportion of the population that are living in the United States, *US emigrants*. Emigrants may affect their home economy through remittances and may promote goodwill towards the United States back home. All variables are scaled for interpretability.

Method

As seen in Figure 3.7, there is a high level of autocorrelation in inflation within countries, but not all countries experience the same trend. We should also be concerned with autocorrelation in the outcome as this is a repeated measure within countries over time. Finally, we also need to account for heterogeneity across units. These features make GPR uniquely suited to analyze the data. I also compare inferences from GPR to RCSE, PCSE, LDV, and RE models. The results are shown in Figure 3.13.

As is clearly seen, inferences vary considerably when using GPR as compared to the other methods. While GPR provides evidence in favor of the hypothesized relationship, the other methods fail to reject the null hypothesis of no effect. Because analyzing this data is problematic for all of the reasons mentioned above, the results of the simulation exercises give us confidence that GPR is a more appropriate choice. Further, as demonstrated in Figure 3.14, GPR is fitting the data very well, while RCSE, the most common approach to analyzing TSCS data, has relatively poor fit.

Figure 3.13: Estimated effect of inflation on anti-Americanism



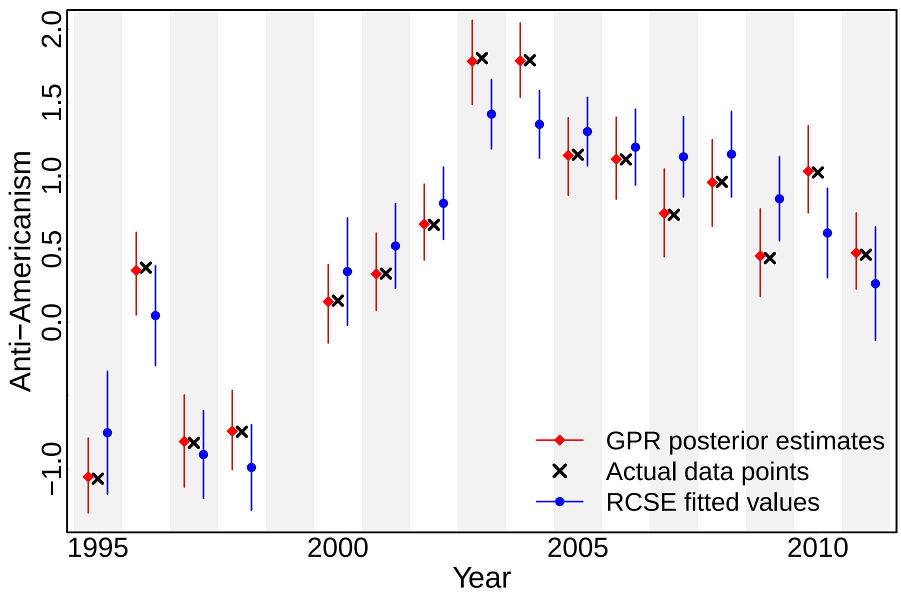
Note: This figure displays the estimated effect of inflation on anti-Americanism in Latin America using different modeling choices. Only GPR rejects the null hypothesis of no effect.

Over-fitting and out-of-sample predictions

While Figure 3.14 shows a very well-fitting model, we might be concerned about over-fitting. Over-fitting a model can lead to imprecise estimators and false conclusions (Hawkins 2004). To test for this possibility, out-of-sample prediction is a valid assessment.

I randomly select 80% of the Latin American data as a training set ($n = 204$), run the model, and test the remaining 20% ($n = 52$). The predictions for the test sample correlate with the observed values at 0.87, suggesting a highly accurate estimation. The predictive 0.95 intervals also capture all but one of the test observations (probability is 0.98 of the observations falling within the bounds). This gives us confidence that while the model is fitting the data extremely well, it is not over-fitting the data.

Figure 3.14: Actual data points and posteriors in Mexico



Note: This figure displays actual data points of the outcome variable plotted alongside GPR predictive posteriors and RCSE fitted values. GPR fits the data much better than the most common approach to analyzing TSCS data.

Model selection and comparison to the null

Related to model fit, it is common to determine whether or not adding an explanatory variable is justified. For Hamiltonian MCMC in Stan, it is suggested to use the Watanabe-Akaike or widely applicable information criterion (WAIC) (Watanabe 2010; Vehtari, Gelman, and Gabry 2016b). The WAIC is similar to the deviance information criterion (DIC), but fully Bayesian and more applicable to the discussed model. We can compare nested models, with and without the explanatory variable, using the WAIC to determine if its inclusion improves the model's validity. This is straight-forward using the `R` package `loo` (Vehtari, Gelman, and Gabry 2016a), but requires computing the log-likelihood in a "generated parameters" Stan block.

We can also use leave-one-out (LOO) cross-validation to assess model fit. To ease the computational load of fitting hundreds of complex models, and to stabilize estimates, we rely on truncated importance sampling (Vehtari, Gelman, and Gabry 2016b; Ionides 2008). Both WAIC and LOO are meant to capture the predictive accuracy of the model, relative to other choices. Asymptotically, the WAIC and LOO are equivalent. However, in practice, if there is a difference, the WAIC should be preferred when interested in a hypothetical replicated experiment, while the LOO should be preferred when interested in out-of-sample predictions.

In this example, the model including inflation significantly outperforms the model not including inflation in both LOO and WAIC, and the difference between the two approaches is negligible. The WAIC differs by 93.31 with a standard error of 22.90, while the LOO differs by 103.9 with a standard error of 25.81. Both are reliable differences and substantial, suggesting the model including inflation ought be preferred.

3.11.3 Replication: The Effect of the Threat of Rockets on the Right-Wing Vote in Israel

Again to demonstrate the differences in conclusions from various modeling strategies, and to further investigate an important substantive finding, I replicate the Getmansky and Zeitzoff (2014) analysis here with all models used thus-far. The replication demonstrates the various conclusions stemming from model choice, and also shows that the degrees of freedom lost to two-way effect models often underestimate the causal effect of an explanatory variable on an outcome.

Getmansky and Zeitzoff (2014) analyze the effect of the threat of terrorism on voting for right-wing parties. Exploiting the variation across time and space of rocket ranges in Israel, they argue that merely being threatened by an attack from Gaza, i.e., being within range, increases voting for right-wing parties. Right-wing parties are considered more aggressive towards terror organizations and less supportive of Palestine. They utilize a two-way fixed-effects regression with RCSE to support the argument.

Variables of Interest

The outcome variable of interest is *RightVote*, the proportion of votes in a given municipality that go to parties of the right bloc. The main explanatory variable is *Range*, which indicates if a given municipality in a given time period is within range of rockets or mortars.

The main analysis also controls for a host of time-varying demographic characteristics, including population size (logged), median age, ratio of males to females, the share of Jewish population in a locality, and the share of high school graduates among those aged 17–24. To control for the local economy, standardized locality mean wage adjusted for inflation using 2006 as the base year is included. Migration is included as a control measured as the net migration divided by locality's population. Finally, the logged number of local fatalities due to suicide attacks three months prior to an election is included. See the original paper for justification, but these are generally included because they affect right-wing voting and can therefore increase the precision of the estimate of the main effect. Whether these are causally prior to the explanatory variable is left out of this discussion.

Analysis

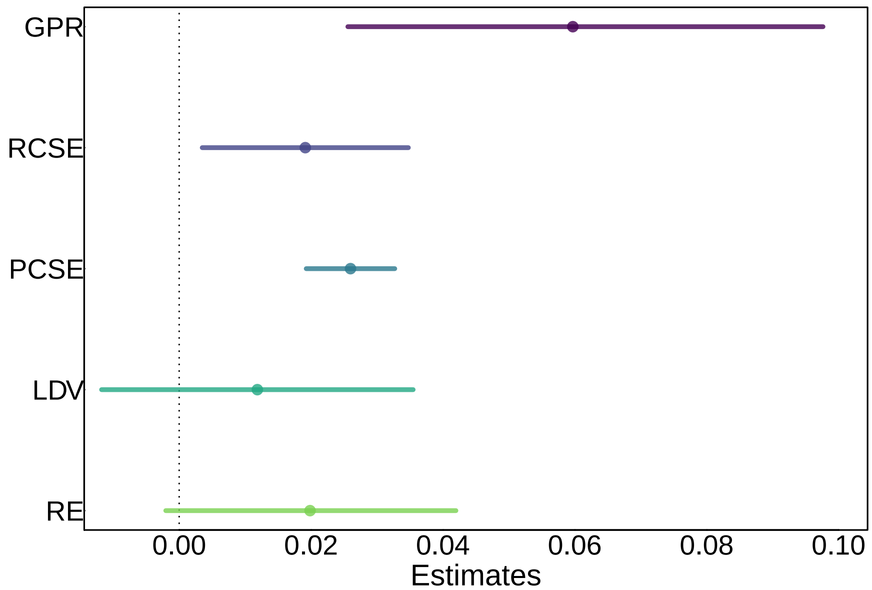
I replicate their analysis here, and argue that they in fact underestimate this effect. Because of the lack of explanatory power of a variable when variation is drastically impacted by so few degrees of freedom, the two-way FE model is under-representing the covariation of voting and being within rocket range. I compare results of the replication to GPR and the earlier-used methods in Figure 3.15. I maintain all controls used in the original analysis. Because the panel is unbalanced with few observations per municipality, in order to identify the PCSE model all municipalities with fewer than three observations need to be dropped. The LDV model also drops all municipalities with only one observation.

It is evident that the modeling strategy utilized in Getmansky and Zeitzoff (2014) underestimates the effect of the explanatory variable, being within rocket range. Further, the modeling choice makes a substantial difference on inferences, with some failing to reject the null. Also, because there are so few observations within municipalities, some modeling strategies require omitting an unacceptable number of observations. This is a problem common in comparative politics, and should not be ignored when relying on extant modeling strategies.

3.12 Conclusion

In this chapter, I presented Gaussian process regression (GPR) as a method to analyze time-series cross-sectional (TSCS) data. Analyses of these data often ignore a very likely

Figure 3.15: Estimated effect of being within bomb range on right vote



Note: This figure displays the estimated effect of being within bomb range on right vote choice in Israel. GPR provides stronger evidence in favor of the hypothesized relationship than any other modeling decision.

lack of conditional independence and adjust the standard errors after fitting the model. GPR, on the other hand, models the outcomes jointly and estimates the nature of the underlying correlational structure with minimal assumptions. I demonstrated the advantages through simulation exercises, showing that GPR produces fewer false positives and negatives, estimates posteriors with better coverage probabilities, and is more efficient and less biased than extant alternatives. While the model does not solve every problem in TSCS analyses, it may serve as a better default choice than any alternative. I then applied the model to demonstrate that inflation in Latin America leads to less anti-Americanism, and that terror threats increase the right-wing vote.

While the model performs quite well, there are two important and related drawbacks. It is much slower than other approaches, with the Latin America exercise running for fifteen minutes, and the replication running for almost two weeks. This runtime will increase dramatically as the number of observations or units increases. The method is not scalable, and as the data set becomes larger typical computers will be unable to estimate the model.

However, in future work I plan to make the method more tractable through approximation methods for large datasets (e.g., Rasmussen and Williams 2006, Ch. 8). Also, the model I presented has a linear specification, although generalizing it to use a link function is a very straight-forward extension. Finally, I treat units as discrete in this presentation. The kernel allows correlation between units, but if we are concerned about spatial correlations, latitude and longitude can easily be included in the kernel such that the smoothing can occur over space.

GPR can also be applied to many problems in political science, not just TSCS analyses. It is an ideal model for prediction, and could be extended as a less data-hungry alternative to the synthetic control method. GPR is also commonly used in text analysis, and could complement current approaches in the discipline.

Executive Moderation and Public Approval in Latin America

Executives in Latin America often change their professed ideologies over the course of their tenure, sometimes moving closer to the median voter and other times moving further towards the political extremes. The effect this movement has on presidential public approval has largely been ignored, and is an oversight in the literature on the region. Because the effect has not been investigated, we lack a complete understanding of the incentives presidents face in the region for professed ideological movement.

I argue that presidential moderation, that is, movement towards the median voter, boosts public approval overall, but these effects are enjoyed most by the more extreme executives across the region. This moderation is viewed by the public both as a necessary executive tool, and as sincere, sedulous representation. This gives an added incentive for movement, as public approval can also boost presidential legislative bargaining power (Calvo 2007). I further argue that moderation is conditional on the electoral cycle. While presidents gain support from the general electorate following ideological shifts towards the center, they do so at the risk of disappointing their core voters. Therefore, this moderation is less likely during election years. However, this effect is nuanced. Executives that are closer to the center are more likely to maintain their positions or continue moderating in order to increase overall support, while extreme executives move further to the extreme, targeting their core supporters.

To test these hypotheses I utilize presidential ideological points estimated from their annual addresses (Arnold, Doyle, and Wiesehomeier 2017), and data from the Executive Approval Project (Carlin et al. 2016). I apply Gaussian process regression (GPR) for time-series cross-sectional (TSCS) analyses as outlined in the previous chapter to test my theoretical expectations. Given the nature of the data, this novel approach is an appropriate choice. The models provide support for the argued relationships.

This ideological moderation has clear implications on policy outcomes. Presidents are able to shift the status quo in their professed ideological direction, even if the shift is not as far as their professed or publicly perceived ideal points. President Menem of Argentina, for example, was a very strong proponent of the Washington Consensus, complete free trade, and the privatization of the public sector. He moderated this extreme viewpoint and successfully proposed two bills, the Law of Economic Emergency and the Law of State Reform, decreasing some barriers to free trade and privatizing particular industries (Romero 2013, p. 288). This example demonstrates the effects executives can have on policy outcomes despite not necessarily having a vote in the legislature. While their veto power and common misalignment with the median legislator was theorized to likely lead to legislative gridlock (Linz 1990; Stepan and Skach 1993; Maeda 2010; Mainwaring 1993), examples such as these not only demonstrate that gridlock is not in fact the norm, but also that moderation helps alleviate this potential for inactivity.

The chapter is organized as follows. First, I outline my theoretical expectations. I develop four explicit hypotheses based on these expectations. I then discuss the data and methodology that I use to test my claims. Following this discussion, I evaluate the results, which support the hypothesized relationships, and compare alternative modeling strategies. I conclude with drawbacks of the study and thoughts for future work in this area.

4.13 Theoretical Expectations

Executives have an incentive to maximize their popular support. This is beneficial in its own right, and also because popular presidents in Latin America can more effectively bargain for policy in their favor (Calvo 2007). According to the literature, the main objective of executives across Latin America is to govern (Alemán and Tsebelis 2011; Saiegh 2009, 1342). That is, presidents wish to move the status quo closer to their ideal points (Strom 1990). Increasing their bargaining power is essential to their legislative success and influence over the legislature (Abranches 1988; Carlin, Love, and Martínez-Gallardo 2015; Cox and Morgenstern 2001; Martinez-Gallardo 2011; Raile, Pereira, and Power 2011; Samuels 2008a, p. 164).

I argue that one strategy at the disposal of executives is to moderate their professed ideologies towards the median voter. This moderation causes desirable boosts to their public approval, as they appeal to the larger electorate. If the increased ability to influence the legislature is not enough, the presidents can, or threaten to, "go public" with a bill, utilizing the public appeal to their advantage (Cox and McCubbins 2005, 226). In this way, presidents can raise the salience of bills, increasing the cost of denying bill consideration on the floor of the legislature. I further argue that executives who are relatively more extreme will benefit from this moderation the most.

However, presidents are constrained in their ability to moderate their professed ideologies. Executives in the region are often elected by extremist core supporters (Samuels and Shugart 2010; Samuels 2008b). If they moderate too much, they risk losing this support. This leads to variation in the degree of moderation across presidents in the region. Further, I argue that this moderation is therefore conditional on the electoral cycle. Relatively extreme executives will moderate less, or move further towards the extremes, during election years, in order to target the supporters who voted the president and / or the president's party in to office in the first place. Relatively centrist executives, on the other hand, will continue appealing to the median voter to increase overall support.

While I do not treat extremity as dichotomous in this chapter, simple two-by-two tables can help clarify my expectations before I go into greater detail. Table 4.1 shows my expectations for the effect of movement on public support. This leads to my expectations for executive movement given electoral year, as shown in Table 4.2.

Table 4.1: Theoretical expectations for the effect of movement

	Moderate	Move to extremes
Centrist executive	?	Decreased public support
Extreme executive	Increased public support	Increased core voter support

Table 4.2: Theoretical expectations for movement

	Non-election year	Election year
Centrist executive	Moderate or stay centrist	Moderate or stay centrist
Extreme executive	Moderate	Move to extreme

Executives need not maintain the expressed, campaigning ideal points throughout their tenure. Presidents may have different audiences while campaigning and while governing (Stokes 2001). Their core voters who are most likely to vote may be relatively extreme, but the public as a whole may have more centrist preferences in the aggregate.[23] The literature has not yet engaged with the effect this moderation has on public approval, and is therefore overlooking an important incentive for professing less extreme ideologies.

Appealing to the median voter ought lead to increased overall support. Saiegh (2015)

[23]Whether their sincere ideal points may be the extreme ideology expressed during elections or the governing ideal points expressed while in office following moderation is unknown, and outside the scope of this study. What we can safely assume, however, is that presidents will want to move the status quo in their preferred direction. We can also assume that an executive would not campaign on the opposite side of sincere ideal movement, i.e., a leftist would not campaign as a rightist and a rightist would not campaign as a leftist. Therefore, while centrist movement may or may not result in a status quo in line with the executive's sincere ideology, it may shift it closer to the sincere ideology.

finds that across Latin American countries there tend to be three distinct ideological clusters of voters: left-leaning, centrist, and right-leaning, of roughly equal size. Therefore, moderation, while necessarily displeasing one of these clusters, should appeal to the other two clusters. Further, if, as an example, a left-leaning president shifts towards the center, the left-leaning cluster of voters is still likely to approve of the executive despite the moderation. Lupu (2016, p. 27) argues that party movement towards the center in Latin America places the party closer to a larger segment of the population. Although this argument is framed at the party-level, it should apply to presidential shifts as well. This leads to the first hypothesis:

> H1: Public support for executives will increase as the executive moderates policy position.

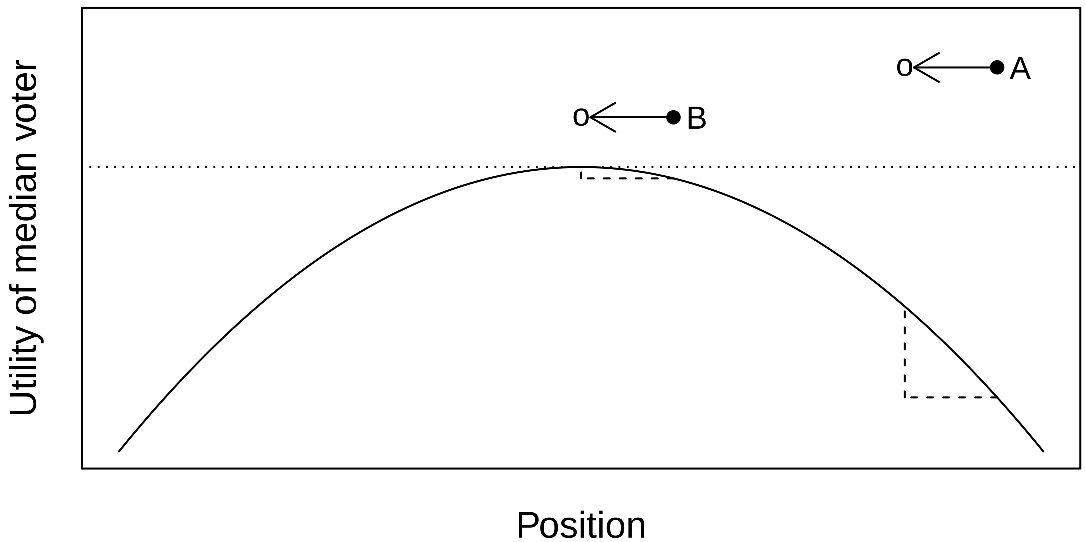
While I argue that this is a general trend, I also argue that it is conditional. That is, the more extreme an executive, the more these moderations will be observed by the public and the more benefit the president will accrue by movement in the form of public approval. Put slightly more formally, if we assume that voters in the aggregate have a quadratic utility function, shifts of more extreme candidates towards the median voter will cause a greater positive shift in utility for the voters and therefore a greater boost in approval. This is shown graphically in Figure 4.16, and leads to the second hypothesis:

> H2: The more extreme an executive's policy position, the greater the increased support she will experience following moderation of policy position.

That is, the effect of moderation is conditional on the extremity of the president.

If executives face these incentives to moderate their policy positions in order to pass legislation, there must be costs associated with the movement or we would not see variation in the degree of movement across executives. The first cost is relatively intuitive: they only

Figure 4.16: Theoretical expectations for the effect of moderation conditional on extremity



Note: If we assume that in the aggregate voters have a quadratic utility function, as candidate A, an extreme candidate, shifts towards the center, the gained utility and hence the increase in approval is greater than if candidate B shifts towards the center.

want to shift their ideal points far enough to be effective legislatively but also keep it close enough to their true ideal point so as to pass desired legislation.[24]

The second cost is related to the election period. While moderation may boost public support in general, it is at the risk of decreased enthusiasm from their core supporters, who may be relatively extreme in their positions. Presidents are able to move their policy positions because they do not necessarily have to toe the party line (Johnson and Crisp 2003; Wiesehomeier and Benoit 2009), and their individual preferences are key to understanding their behavior in office (Cheibub 2007; Kiewit and McCubbins 1991). However, as Stokes (2001, p. 66) argues, even if presidents in the region are not running for reelection, they have a strong incentive for their party to be victorious. While executives may have to build cross-assembly support in order to successfully form a coalition and pass legislation, this

[24]This assumes, however, that their campaigning ideology is their sincere ideology. We can relax this assumption as I discuss above, and simply rely on the assumption that they desire movement in their professed direction.

is much easier when the party has a strong presence (Alemán and Tsebelis 2011; Cheibub, Przeworski, and Saiegh 2004; Colomer and Negretto 2005; Negretto 2006). Further, ensuring party success stabilizes the presidential legislative agenda pursued during their tenure.

Therefore, during either presidential or legislative election years, to maintain core party supporters, to ensure their supporters turn out, and to distinguish themselves from other candidates and parties, executives in the aggregate will be less willing to moderate and likely move more towards the extremes, if that is where their core voters lie on the political spectrum. In other words, there are "centrifugal" forces during election years (Cox 1990).[25] That is, we would not expect the median voter theorem to hold during elections in the institutional environment of Latin American countries. Many candidates often run for office, and the elections are nationwide plurality systems. Campaigning executives therefore have incentives to distinguish themselves from other candidates, turn out core supporters, and move away from the median voter. This leads to the third hypothesis:

> H3: During election years, executives will be less willing to moderate in general, and will move towards the extremes.

Once again, however, there is likely heterogeneity of this strategy based on the extremity of the president. A centrist executive does not have the support of extreme and enthusiastic voters and activists (Samuels and Shugart 2010; Samuels 2008b). Centrists instead rely on appealing to the median voter to maximize voting returns. Therefore, rather than move towards the extremes to increase turnout for their vote, they are more likely to move towards the median voter, or stay close to the median voter, in order to maximize the number

[25] However, this raises the question, if the public is attuned to the policy positions of the executives throughout their tenure, why then would they forgive this governing moderation and continue voting for the executive and / or the executive's party? Either the public is aware that these moderations are necessary and trust the president to mandate, or, perhaps more realistically, these politically enthusiastic and active voters are going with their best possible option. Afterall, the voters, like the executive, wish to see policy moved in their preferred direction, even if they realize perfect conformity to their ideal point is unrealistic. Further, Lupu (2016, p. 17) argues that in Latin America, voters tend to make decisions based on very short-term retrospection.

of supporters during election years. Targeting core voters is less important, or less of a possibility, to centrist executives than increasing overall support and trying to win votes.

While a centrist executive may have incentive to either not move or even move more towards the center in order to appeal to the general electorate during electoral periods, extremist executives wish to distinguish themselves and ensure the parties' core supporters turn out to vote. Extremist executives and parties need to appeal to the extreme voters and activists rather than the general electorate for electoral success. This leads to the conditional expectation of hypothesis four:

> H4: During election years, centrist executives will either not move or move closer to the center, while more extreme executives will move towards the extremes.

That is, the more extreme the candidate, the larger the marginal effect will be of electoral periods on extremist movement.

The literature has thus far not analyzed the effect of executive ideological moderation on public approval. We therefore have an incomplete understanding of the incentives executives may have for movement and the effect of this movement. Arnold, Doyle, and Wiesehomeier (2017) argue that moderation towards the median legislator in the governing coalition improves executive legislative success. Through this ideological movement, presidents are able to more effectively bargain with the governing coalition to pass desirable legislation. Amorim Neto (2006) argues that executives foster coalition building, and extreme executives are less likely to be successful in coalition formation (see also Alemán and Tsebelis 2005; Chasquetti 2001; Cheibub, Przeworski, and Saiegh 2004; Negretto 2006). This implies that if an executive, particularly an extreme executive, wants to be involved in the coalition and have greater legislative influence, they have an incentive to appeal to multiple, or large, parties, and ideological movement may be necessary to achieve this goal. These factors help avoid the theoretically expected legislative gridlock in the region (Linz 1990; Stepan and

Skach 1993; Maeda 2010; Mainwaring 1993). Missing from these accounts, however, is the impact movement has on the population's perceptions of the executive.

I argue that moderation, particularly by extreme executives, will boost support overall. This boost in public approval allows the president to be more effective legislatively. Regardless of the incentives for moderation, ideologically shifting towards the median voter ought to generally raise approval ratings. While this should add an incentive for moderation, there are likely many reasons presidents shift towards the median voter. The current study primarily engages with the effect of the moderation.

I further argue that there are electoral incentives to profess extremist ideologies during election years, particularly for those presidents and parties that are perceived as extreme. Executives need to target their core supporters and mobilize voters. Appealing to the median voter is therefore less of a priority for extreme executives, and these presidents will moderate less during election years. I next discuss how I test these hypotheses.

4.14 Data and Method

Presidents across most of Latin America give annual addresses on the state of the nation. These addresses are highly institutionalized. Executives in the countries included in this study are required by their respective constitutions to address the legislature about the state of the nation. As part of these speeches, presidents reveal policy positions, with both the legislators and the citizens as their audience. Because these are comparable across countries and over time, they are an ideal representation of the ideologies presidents choose to profess. I will now elaborate on the variables that I use to test my hypotheses before discussing my modeling choice.

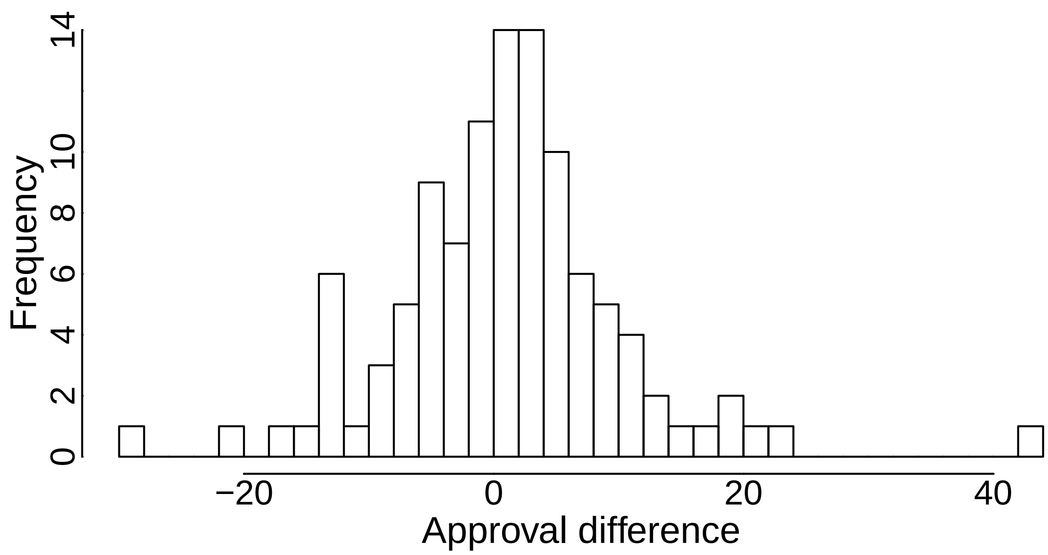
4.14.1 Variables

Main Variables of Interest

The first two hypotheses require a measure of public approval for executives in Latin America over time. The Executive Approval Project provides a time-series cross-sectional database capturing presidential approval across many countries based on representative public opinion polls (Carlin et al. 2016). We are interested, however, in how approval changes over time, and specifically how it changes following an annual presidential address. Therefore, I average the provided four quarters of approval data before the addresses, and the four quarters of data after the addresses, and take the difference. I label this variable *Approval difference*, and it is the outcome variable for the first two hypotheses. This variable ranges from -29.71 to 42.73 with a mean of 1.02. However, as I discuss below, all variables are scaled before running the models for interpretability and prior choice. The largest drop in approval was experienced by President Wasmosy of Paraguay after accusing an opposition leader of plotting a coup (Claude 2000). The largest gain in public approval was experienced by President Jorge Batlle of Uruguay following the largest executive moderation in the data, as I discuss more below. A histogram of this variable is shown in Figure 4.17.

To measure professed ideology, I rely on the annual presidential addresses given by most executives in the region. Using WORDFISH in a Bayesian framework to estimate political ideologies (Proksch and Slapin 2008), Arnold, Doyle, and Wiesehomeier (2017) place each presidential address on a continuous left-right scale. As they argue, this is the best way to capture the professed ideology on a continuous left-right scale for executives in the region. Presidents do not typically vote, so roll-call procedures are not useful. Further, survey data can not necessarily be analyzed *ex post* without suffering from temporal anchoring problems. Speech data, on the other hand, can be used to retrieve a reliable time-series measure of professed ideology.

Figure 4.17: Histogram of *Approval difference*



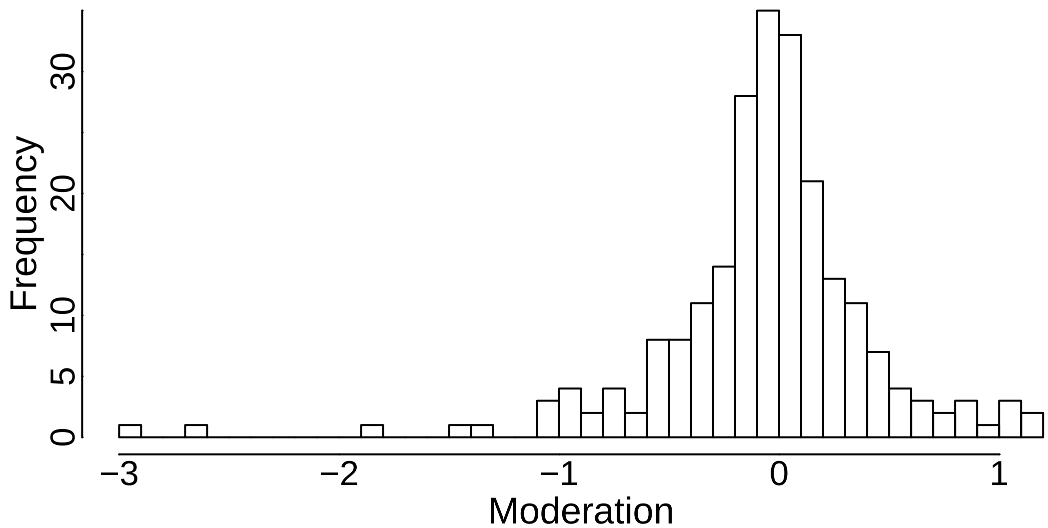
Note: Shown is the distribution of the variable *Approval difference*. This is the outcome variable for the first two hypotheses.

I utilize the estimated points to create a variable *Ideological moderation*, which captures the extent to which an executive has moved closer to the center on the left-right spectrum in these speeches. To be more precise, the variable captures movement away from the side the executive professed in her earlier speech.[26] This variable is the main explanatory variable for the first two hypotheses, and the outcome variable for the third and fourth hypotheses. While not necessarily capturing sincere ideology, these ideal points represent what is being professed and consumed by citizens. The mean of this variable is -0.10, with a range of -2.90 to 1.13. The minimum, representing shifts to the ideological extreme, was achieved by President Wasmosy of Paraguay during an election year. He ultimately lost this election, but the shift was likely a last-ditch effort to save his presidency which was marred by fraud and corruption (Lambert 2000). The maximum value was earned by rightist President Jorge

[26]There is one occurence in which the executive switches sides, and is is coded as positive moderation. In 1998, following a substantial drop in public approval, President Fujimori of Peru switched to the left. The following year he is estimated as being back on the right, but very close to center.

Batlle of Uruguay during a non-election year but in a period in which the left was becoming more unified and powerful (Cason 2002), and moderation was likely necessary to pass desired legislation. A histogram of this variable is shown in Figure 4.18.

Figure 4.18: Histogram of *Ideological moderation*

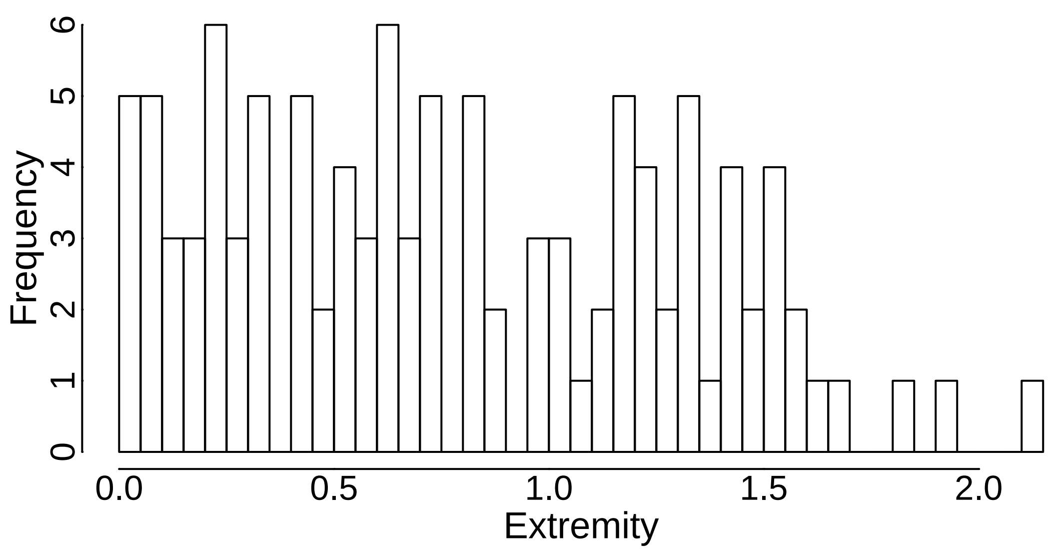


Note: Shown is the distribution of the variable *Ideological moderation*. This is the explanatory variable for the first two hypotheses, and the outcome variable for the third and fourth hypotheses.

The explanatory variable for the third and fourth hypotheses is *Election year*. This is an indicator as to whether or not the observation occurs in either an executive or legislative electoral year. These data were coded by the author. Of the data analyzed, 35% of the years are election years.

Finally, to test the conditional hypotheses two and four, I need a measure of the professed extremity of the president, *Extremity*. This is captured using the same ideological points as estimated in Arnold, Doyle, and Wiesehomeier (2017). It is the absolute distance from the mean of zero of the professed ideology in the previous annual speech. This captures the professed extremity of the president, prior to the annual address. The mean of this variable is 0.79 and ranges from 0.02 to 2.13. Notice that this maximum is less than the extreme movement by President Wasmosy, indicating that in the election year he moved further extreme than the most extreme executive during any president's tenure in the data. The most extreme president in the data is the rightist President Batlle, in the same year that he moderated the maximum amount. The most centrist president in the data is Fox of Mexico in 2004. A histogram of this variable is shown in Figure 4.19.

Figure 4.19: Histogram of *Extremity*



Note: Shown is the distribution of the variable *Extremity*. This is the variable used in the second and fourth hypotheses to test the conditionality claims.

Control Variables

In order to increase the precision of the estimates and account for confounders, I control for a number of variables. These variables are included in all models. Hypotheses one and two require their inclusion because they are likely causally prior to both the explanatory variable(s) and outcome variable. Although not temporally prior to election years, these variables are completely exogenous to election years and so are included to increase the precision of the estimates for hypotheses three and four. Further, they are causally prior to the expressed extremity of the executive, so to make the conclusions from testing hypotheses three and four comparable, I include all control variables in both tests.

The first two variables capture the economic conditions of the country, *Inflation (logged)* and *GDP growth*, measured in the year prior to the speech. Presidents have drastically changed policy positions in response to economic conditions (Campello 2014; Kaplan 2013; Samuels and Shugart 2010; Stokes 2001). Further, citizens are likely to, at least partially, blame or credit the executive for such circumstances. Controlling for the economic situation is therefore necessary.

The distribution of pork can aid an executive's bargaining power and legislative support (Ames 2001; Amorim Neto 2002; Arnold, Doyle, and Wiesehomeier 2017; Cox and Morgenstern 2001; Raile, Pereira, and Power 2011), making shifts and appeals to the public less necessary. These transfers, often public goods, are also likely to affect public support. I therefore control for central spending in the year prior, *Spending*, in all models.

Finally, trade openness may both affect public approval for the executive and executive behavior (Baker and Cupery 2013; Baker 2003; Milner and Kubota 2005; Weiss, Argüello, and Argüello 1995). I therefore control for *Trade*, lagged, as a proxy for openness.

Sample

The available data spans 39 democratically elected presidents across 11 countries: Brazil, Chile, Colombia, Costa Rica, Ecuador, El Salvador, Guatemala, Mexico, Paraguay, Peru, and Uruguay, ranging from 1990–2008. Because of the nature of the tests, the presidents need to have served at least two years in order to calculate the difference in their expressed ideologies, and to be included in the dataset. The control data is also unavailable for many country-years in the original data, and these are dropped from the analysis. While dropping the data is not ideal, the proportion of missingness makes imputation problematic.[27] The

[27]Specifically, Argentina and Venezuela had to be completely dropped from the data, as did other country-year pairs. The original data has 227 observations.

total sample size is a modest $N = 108$. Having discussed the variables I will employ, I will now specify the model and explain its merits.

4.14.2 Modeling Choice: Gaussian Process Regression

The data analyzed is time-series cross-sectional data, and analyses of these sort are often problematic. There are well-known problems these analyses encounter that make inferences particularly difficult: time-varying confounders, serial correlation in the variables of interest across time, between-subject heterogeneity, and more. In these circumstances, both parameter estimates and their standard errors can be misleading and biased when inappropriately modeled, often leading to spurious results (Esarey and Menger 2017; King and Roberts 2015).

As further outlined in the previous chapter, more standard approaches can bias results and tend to be inefficient, whereas Gaussian process regression (GPR) alleviates many of these concerns by relaxing the assumption of conditional independence on which other models rely. In TSCS data, that is, data in which observations within multiple units are observed at multiple points in time, there is strong reason to believe conditional independence does not hold. There exist time trends within units and across units, certain units share varying degrees of correlation with other units, there are often problems of multicollinearity of regressors, and more. The nature of these data features violating conditional independence is often not known *a priori*.

The most common approaches to modeling TSCS data include two-way fixed-effects regression with robust clustered standard errors (RCSE), panel corrected standard errors (PCSE), lagged dependent variable specifications (LDV), and random effects models (RE), following the previous chapter. Unfortunately, none of these can be considered a default approach for the problems associated with modeling these data, but under a range of situations GPR performs as well or better than each common approach. Further, the two-way effects models decrease in the degrees of freedom is often not justified (Greene 2003, p. 364), and this data is likely related in non-linear ways, which these approaches do not handle well.

Rather than modeling the outcomes conditionally independently as in a standard linear model, GPR models all of the outcomes jointly as a process. Specifically, allow $\mathbf{y}$ to be the entire vector of outcomes, and allow $\mathbf{X}$ to be a matrix of the covariates for all observations. In addition to the variables mentioned above, I also include indicators for each president to account for baseline heterogeneity across presidents. Let the parameters of interest be denoted $\boldsymbol{\beta}$. We model the outcomes as coming from a multivariate normal distribution:

$$\mathbf{y} \sim \mathcal{MVN}(\mathbf{X}\boldsymbol{\beta}, \sigma^2\boldsymbol{\Omega}), \tag{4.19}$$

with unknown variance-covariance $\sigma^2\boldsymbol{\Omega}$. This is learned from the data in a Gaussian learning kernel. The estimation of this kernel includes all covariates as well as the presidential indicators, to account for heterogeneity in the variance-covariance across presidents. I also use a continuous time variable in the estimation of the variance-covariance to account for temporal trends in the outcome that are unexplained by the covariates. Denote the covariates that are used to estimate the variance-covariance as $\mathbf{X}^*$.

To be more precise, the fully specified variance-covariance is:

$$\boldsymbol{\Omega}(\mathbf{x}_j^*, \mathbf{x}_i^*) = \boldsymbol{\Omega}^\dagger(\mathbf{x}_j^*, \mathbf{x}_i^*) + \delta g_{j,i}, \tag{4.20}$$

$$g_{i,j} = \begin{cases} 1 & \text{if } j = i, \\ 0 & \text{otherwise.} \end{cases} \tag{4.21}$$

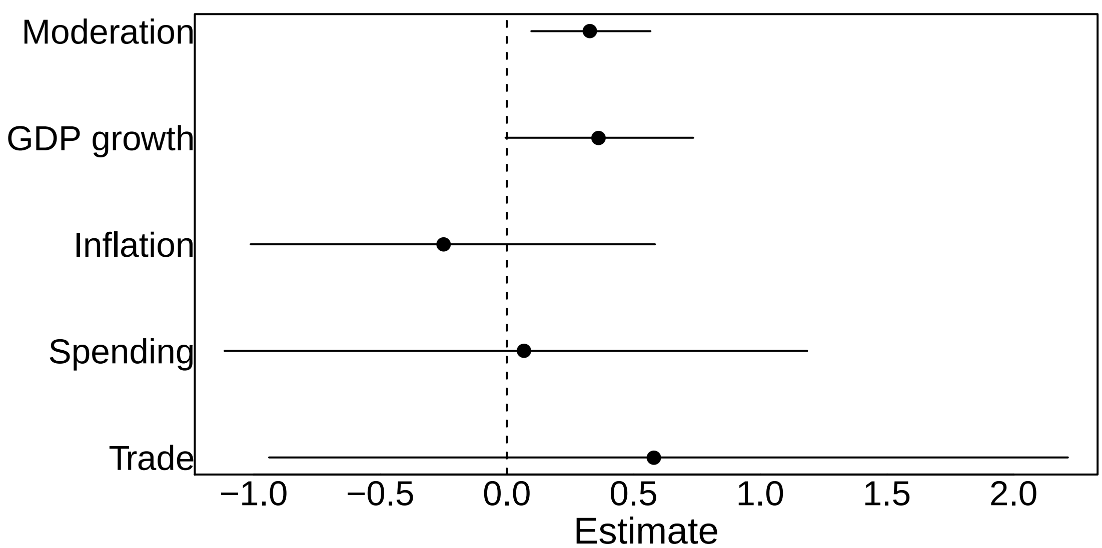
$$\boldsymbol{\Omega}^\dagger(\mathbf{x}_j^*, \mathbf{x}_i^* | \boldsymbol{\zeta}) = \exp\left\{ -\sum_{p=1}^{m} \frac{(x_{pj}^* - x_{pi}^*)^2}{\zeta_p} \right\}. \tag{4.22}$$

That is, the further away observations are in the covariate space, such as temporal proximity, the less correlation they share. However, the nature of this correlation is not assumed, and vague priors are placed on all unknown parameters, following the previous chapter. Further, for interpretability and prior choice, all variables are scaled before running the models. Having specified the model, I will now apply it to test the main arguments of the chapter.

4.15 Results

The first and central argument of the chapter is that executive moderation boosts public support. Figure 4.20 provides posterior estimates and 0.95 credible intervals for the main variable of interest, *Ideological moderation*, and the discussed controls, with changes in public approval as the outcome variable.[28]

Figure 4.20: The effect of moderation on changes in approval



Note: Shown are the posterior estimates with 0.95 credible intervals for the first hypothesis, that executive moderation increases public support, along with controls. As seen, the posterior for the variable of interest is both in the hypothesized direction and is reliable at conventional levels.

[28]All of the model results are included as tables in the Appendix.

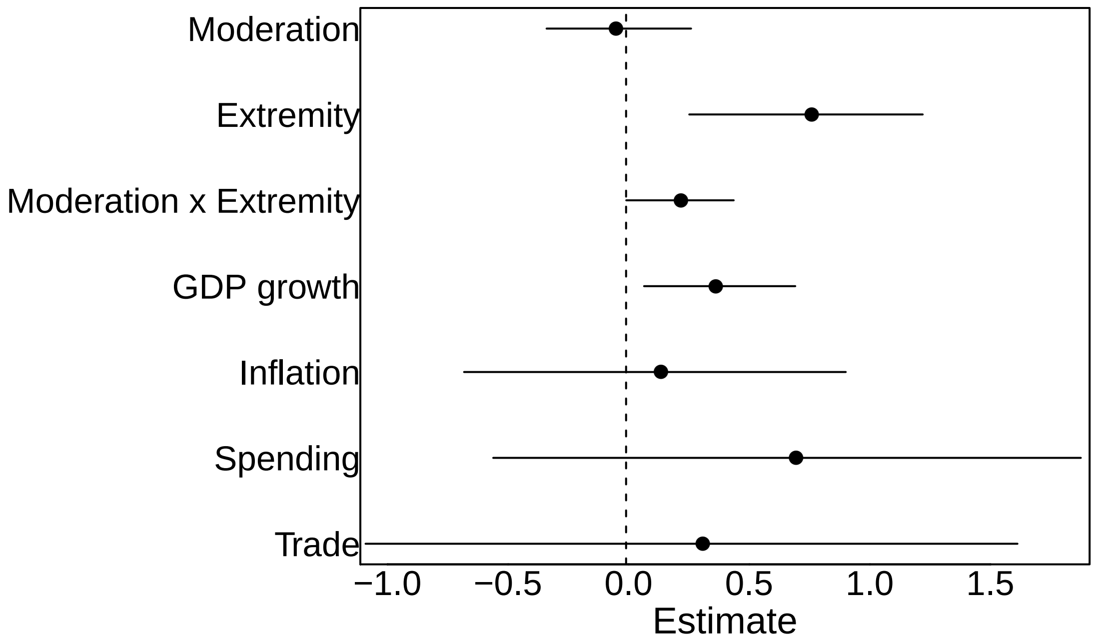
As seen in the figure, GPR provides support for the hypothesized relationship at conventional levels. Specifically, the estimated effect is approximately 0.33. Because all variables are scaled, this should be interpreted as a one standard deviation shift in the explanatory variable causes a shift in the outcome variable of 0.33 standard deviations. This translates to a boost in approval of over 3%. More concrete examples are given below, following the more nuanced hypothesis test.

As outlined in the previous chapter, the widely applicable information criteria (WAIC) and the leave-one-out cross validation (LOO) are useful for assessing model fit for GPR using Hamiltonian MCMC. The WAIC for the full model is -506.4, and the LOO is -645.3. However, when running the model without including the main explanatory variable, *Ideological moderation*, the WAIC and LOO are -674.7 and -901.3, respectively. The differences are therefore 168.3 for the WAIC and 256.0 for the LOO, with standard errors of 51.6 and 46.4, both substantial differences, suggesting the model including the main explanatory variable ought be preferred.

However, I argue that this effect is nuanced and conditional on the expressed extremity of the president. That is, the more extreme an executive, the greater this effect of moderation on changes in support. Testing this claim, Figure 4.21 shows the posterior estimates with 0.95 reliable intervals of the variables of interest, *Ideological moderation*, *Extremity*, and their interaction, along with the controls.

While the interaction is in the hypothesized direction, to understand the conditionality of the effect we have to explore the marginal effects. However, to fully understand the marginal effect that is not strictly linear, I run the model trained on the observed data and use it to predict values if *Executive moderation* was one unit larger than the observed values. I then take the difference in the outcome, *Approval difference*, and the predicted outcomes. These differences along with 0.95 credible intervals are shown in Figure 4.22. Each point is the predicted difference for a president in a given year. As seen in the figure, the more extreme

Figure 4.21: The effect of moderation on changes in approval conditional on extremity

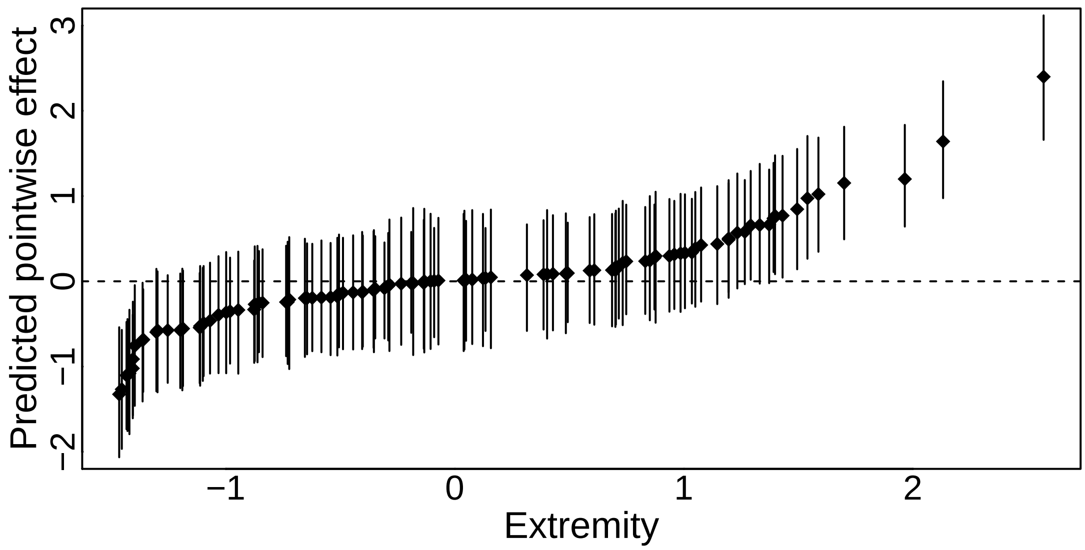


Note: Shown are the posterior estimates with 0.95 credible intervals for the second hypothesis, that the effect of executive moderation on increased public support is conditional on the extremity of the executive. The estimate for the interaction is in the hypothesized direction and reliable at conventional levels, but Figure 4.22 shows the marginal effect of moderation as extremity increases.

executives enjoy greater shifts in public approval following moderation, while more moderate executives do not enjoy the same boost in support, and in fact the estimated marginal effect is negative for the most moderate presidents.

While I do not strictly predict this negative relationship, it is an interesting empirical trend. Perhaps more moderate executives are viewed as non-committal when moving in the centrist ideological spectrum, even perceived as switching sides when moderating, while extremist executives' shifts towards the center are viewed as necessary moderation for negotiation. This warrants further investigation that is outside the scope of the current project. Nevertheless, looking into some of the president's moves and the associated changes in approval is a fruitful exercise.

Figure 4.22: The predicted point-wise effect of moderation on changes in approval as extremity increases



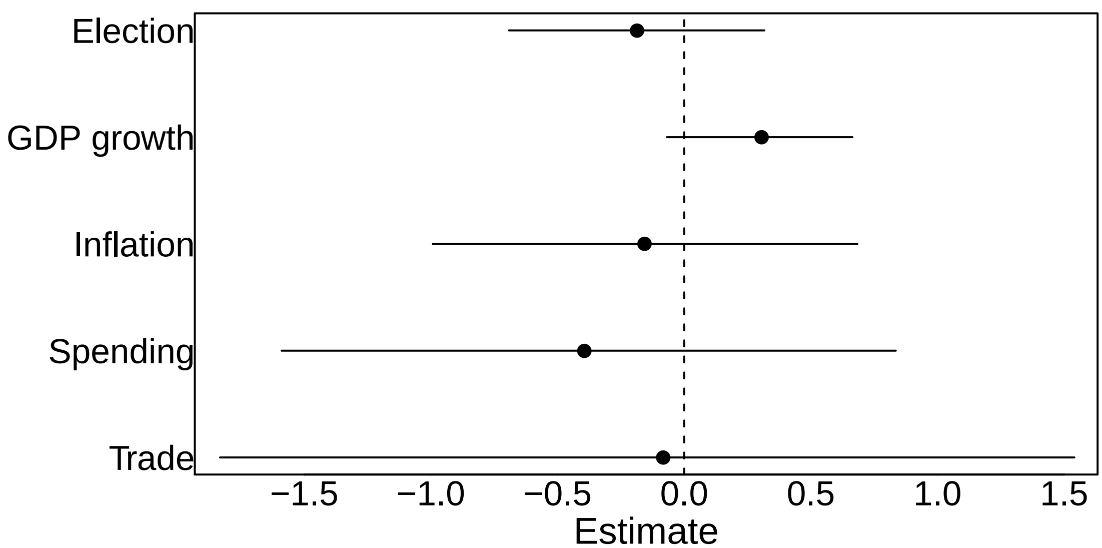
Note: Shown is the estimated difference in predicted values and actual values when moderation increases by one unit, with 0.95 credible intervals. This can be interpreted as a predicted marginal effect. This plot shows predicted values of changes in the outcome at different levels of extremity as moderation increases. Each point represents a president in a given year.

President Fox of Mexico is the most centrist president in the data. His second speech, given in 2002, is estimated as the most moderating after his first speech in 2001. His approval dropped by over 9% points following the speech. His two least moderating speeches, in which he moved further to the right, are associated with boosts of approval of over 2 and 5% points. President Garcia of Peru, on the other hand, a relatively extreme leftist, moderated substantially in 1990 and saw a boost in approval of over 13%, despite significantly negative GDP growth and overwhelming inflation.

To assess model fit, I compare the model both to the null model including only controls, and to a model including *Ideological moderation* and *Extremity* but not their interaction, for completeness. Compared to the previously described null model with only controls, the WAIC differs by 22.9 with a standard error of 6.67, and the LOO differs by 70.8 with a standard error of 6.2, both substantial differences. The model with the interaction has a WAIC of −651.8 and a LOO of −830.5, while the model without the interaction has a WAIC and LOO of −694.1 and −850.9. The differences are therefore 42.3 with a standard error of 3.3 and 20.4 with a standard error of 2.9, both substantial. This suggests we should prefer the fully specified model with the interaction.

Turning to the third hypothesis, that executive moderation is less likely during election years, Figure 4.23 shows the posterior estimates of the effect of the main explanatory variable, *Election year*, and all controls on the outcome variable, *Ideological moderation*, along with 0.95 credible intervals. While the estimated effect is in the hypothesized direction, it is not reliable at conventional levels. However, as I argue above, the effect is more nuanced, and again conditional on the extremity of the president.

Figure 4.23: The effect of election year on executive moderation



Note: Shown are the posterior estimates with 0.95 credible intervals for the third hypothesis, that executive moderation is less likely during election years, along with controls. As seen, the posterior for the variable of interest is in the hypothesized direction, but is not reliable at conventional levels.

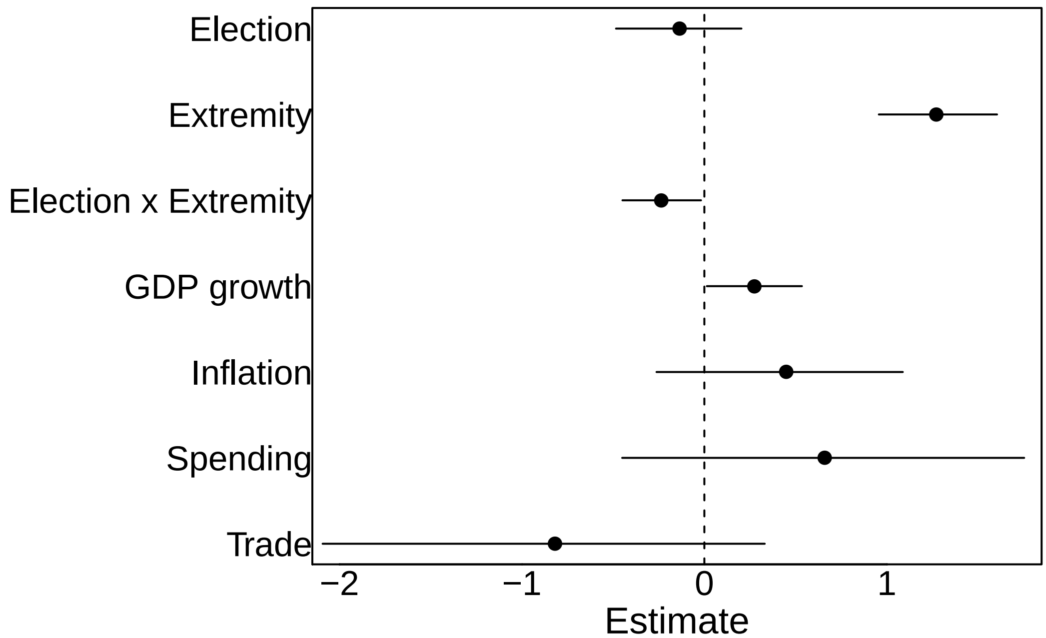
Further, the model fits substantially better than the null model not including *Election* *year*. The model with only the controls has a WAIC and LOO of −3768.9 and −1643.0, while the model including the explanatory variable has a WAIC and LOO of −1443.6 and −1643.0, making the differences 2325.3 with a standard error of 580.1 and 1296.1 with a standard error of 109.6, both large. This implies that while the explanatory variable is not estimated as having a reliable impact on the outcome variable at conventional levels, the model including the variable should still be preferred.

Figure 4.24 shows the estimated effect of *Election year*, *Extremity*, their interaction, and all controls on *Ideological moderation*, along with 0.95 credible intervals. The interaction is reliable at conventional levels and in the hypothesized direction. Again, however, we need to investigate the marginal effect of election years on executive moderation as extremity increases.

Again, the marginal effect is not strictly linear in prediction. I therefore proceed similarly as before. I train the model on all of the data, and use the model to predict moderation in all election years if there were not an election. I then take the difference between the observed level of moderation for these years ($N = 38$), and the estimated level of moderation had there not been an election. Figure 4.25 shows these predicted differences as extremity increases. I repeat this process for non-election years ($N = 70$), predicting the differences if there were an election. These predictive differences are shown in Figure 4.26. At higher levels of extremity, executives are indeed less likely to moderate during election years. Very moderate presidents, on the other hand, are estimated to moderate more during election years, likely attempting to target the median voter and increase overall support, as opposed to targeting the loyal, generally extreme supporters presidents and parties at far ends of the ideological spectrum tend to target.

Again, to assess model fit, I employ WAIC and LOO. The values are, respectively, −1303.8 and −1411.0. Compared to the null model only including controls, this leads to the substantial differences of 2465.0 with a standard error of 646.4 and 1528.1 with a standard

Figure 4.24: The effect of election year on executive moderation conditional on extremity

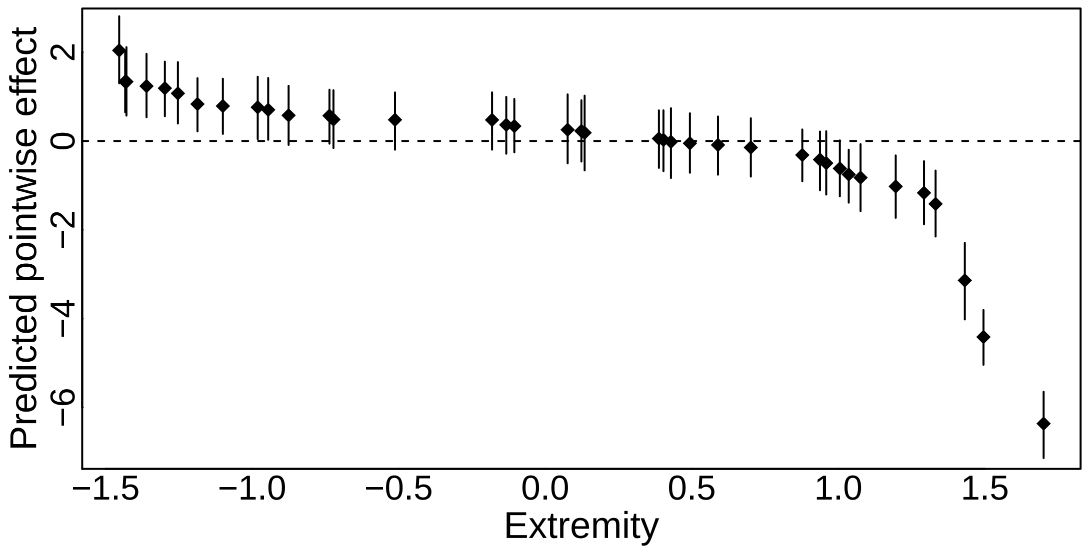


Note: Shown are the posterior estimates with 0.95 credible intervals for the fourth hypothesis, that the effect of election year on executive moderation is conditional on the extremity of the executive. The estimate for the interaction is in the hypothesized direction and reliable at conventional levels, but Figures 4.25 and 4.26 show the marginal effect of election as extremity increases.

error of 159.9. Compared to the model with no interaction but both explanatory variables, *Election year* and *Extremity*, the differences are 170.1 with a standard error of 78.9 and 182.4 with a standard error of 42.3. In short, the model including the interaction and the main explanatory variables is preferred to both null models.

As an example, the relative extremist President Uribe of Colombia, known primarily for his tough stance on security issues in the country, moved most dramatically further right during the two electoral years in which he was in office. The other five years he moderated in all but one. Meanwhile, President Figueres of Costa Rica, a relative centrist, moved very close to the center during the election year. This is a particularly interesting case, because presidents cannot rerun in subsequent elections in Costa Rica, and illustrates that even if

Figure 4.25: The predicted point-wise effect of election year in election years on moderation as extremity increases



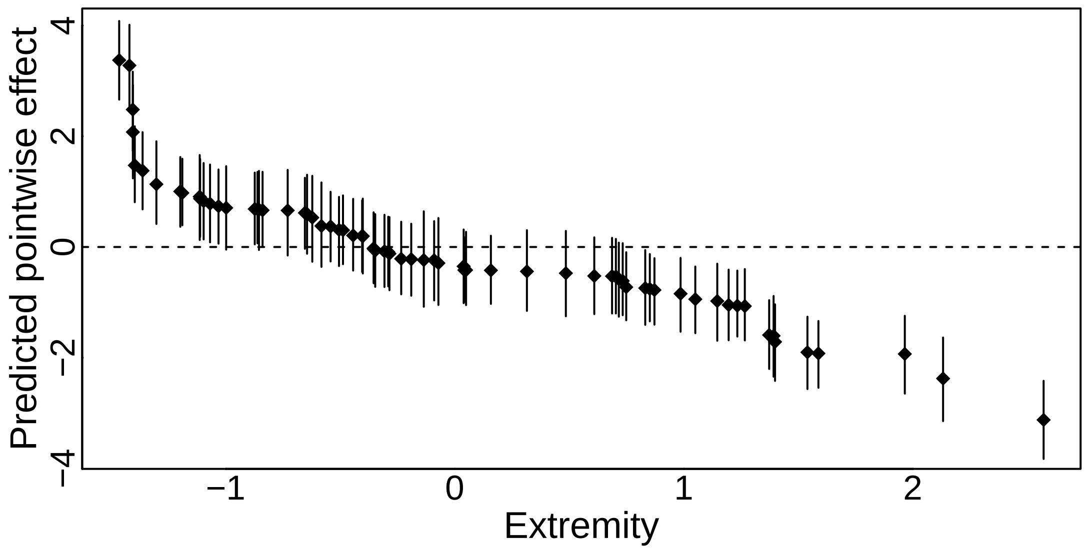
Note: Shown is the difference between moderation in an election year and the predicted moderation in a non-election year as extremity increases, with 0.95 reliable intervals. There are 38 election years, and each point represents the difference in actual moderation in these years and the predicted level of moderation, had there not been an election. Moderate presidents are estimated to moderate more in election years, and extreme executives are estimated to moderate less in election years.

the president is not running, there is incentive for party success.

4.15.1 Comparison to Alternative Modeling Choices

While analyses of these data suffer the common problems in TSCS applications, and I argue that the approach taken in the previous chapter is the best method of analyzing these data, comparison to more conventional modeling strategies may provide some completeness of the results. Across all four hypotheses, I compare GPR to a two-way fixed-effects regression using robust clustered standard errors (RCSE), panel corrected standard errors (PCSE), a lagged dependent variable model (LDV), and a two-way random effects model (RE), following Chapter 3.

Figure 4.26: The predicted point-wise effect of election year in non-election years on moderation as extremity increases

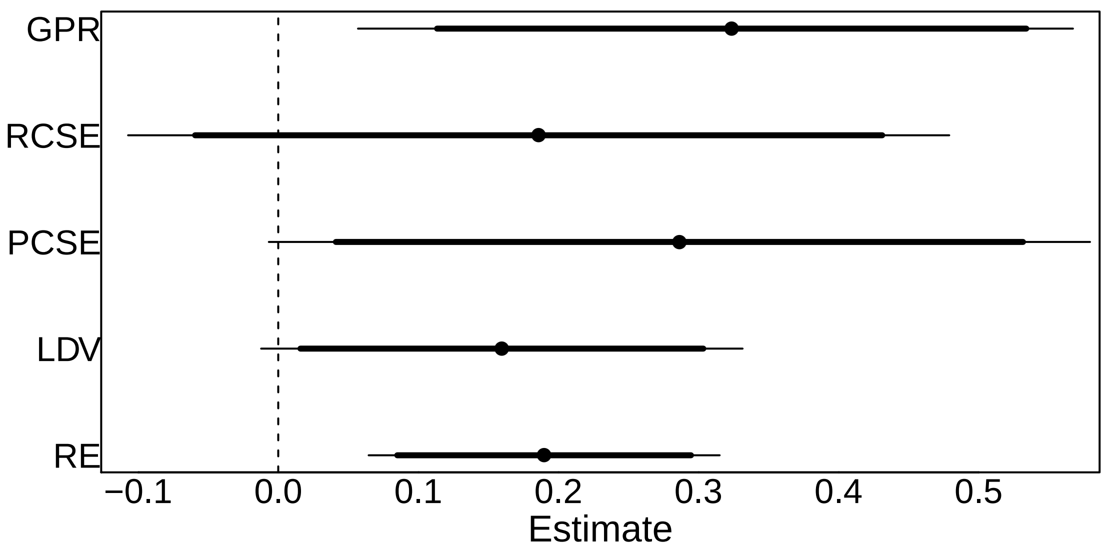


Note: Shown is the difference between the predicted moderation in an election year and the observed moderation in a non-election year as extremity increases, with 0.95 reliable intervals. There are 70 non-election years, and each point represents the difference in the predicted level of moderation if there had been an election, and the actual observed level of moderation in these non-election years. Moderate presidents are estimated to moderate more in election years, and extreme executives are estimated to moderate less in election years.

I argue that all of these models are over-specified, and the degrees of freedom lost from two-way effects models is not warranted (Greene 2003, p. 364). In addition, these models can accentuate problems of multicollinearity among regressors (Baltagi 2008, p. 35). Nevertheless, due to the novelty of the chosen modeling strategy, a comparison is useful.

For the first hypothesis, that *Executive moderation* positively affects *Approval difference*, the results are not reliable at conventional levels for any modeling strategy but the random effects model. RCSE, PCSE, and LDV all produce positive estimates, but are only reliable at the 0.90 credible level. Specifically, RCSE estimates the effect is 0.19 with a standard error of 0.11. PCSE[29] estimates the effect to be 0.29 with a standard error of 0.15. RE estimates the effect to be 0.19 (SE = 0.06). Finally, LDV estimates an effect of 0.16 (SE = 0.09). These estimates with 0.95 and 0.90 reliable intervals are shown in Figure 4.27.

Figure 4.27: The effect of executive moderation on changes in public approval, model comparison



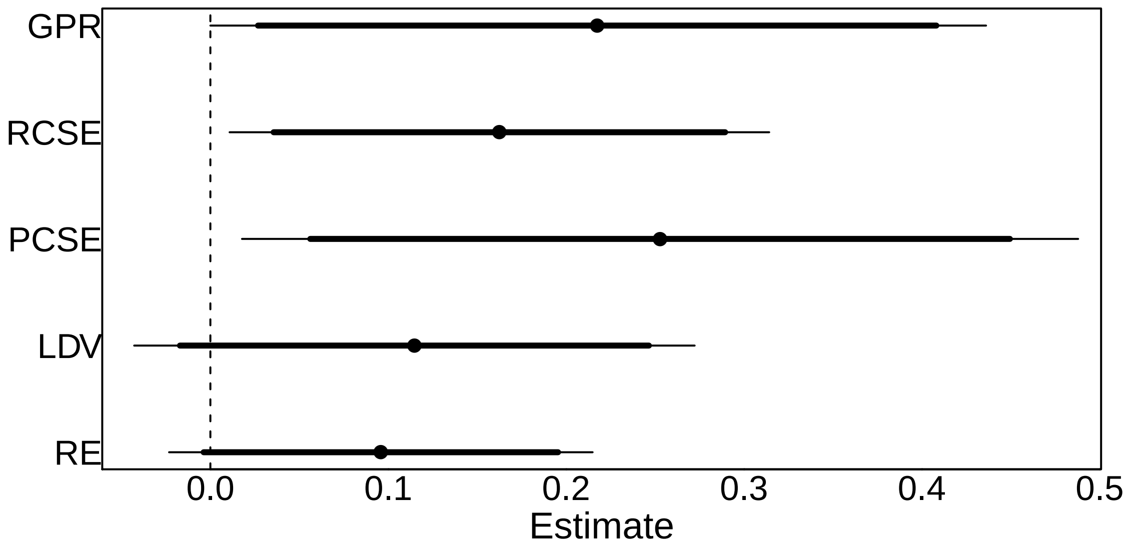
Note: Shown are estimates with 0.95 and 0.90 reliable intervals for the effect of executive moderation on changes in public approval using the most common modeling strategies for TSCS data, along with the estimated effect using GPR.

The second hypothesis is that this effect is conditional on *Extremity*. The interaction between *Extremity* and *Executive moderation* is reliable at conventional levels for RCSE and PCSE, but not RE or LDV. To be more precise, the estimate for RCSE is 0.16 with a standard error of 0.07. PCSE estimates an effect of 0.25 (SE = 0.11). The RE estimate is 0.10 (SE = 0.06), reliable at the 0.90 level but not 0.95. The LDV model estimates an effect of 0.11 (SE = 0.08). These estimates with 0.95 and 0.90 reliable intervals are shown in Figure 4.28.

GPR does not provide conventionally reliable support for the third hypothesis, that

[29] In order to identify the PCSE model, presidents and years with only one observation need to be dropped, so the estimates are not identical to RCSE.

Figure 4.28: The effect of executive moderation on changes in public approval conditional on extremity, model comparison

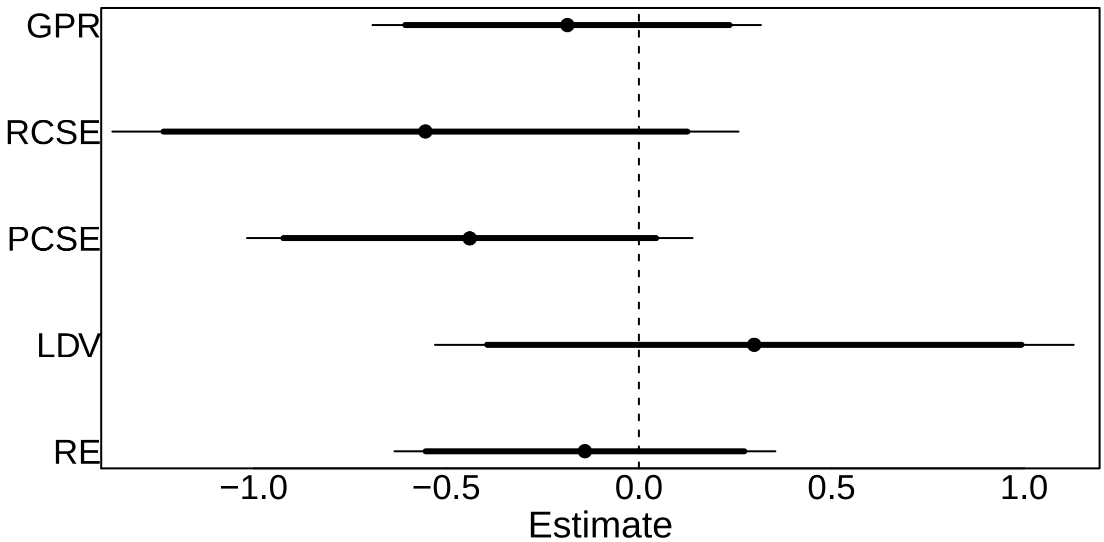


Note: Shown are estimates with 0.95 and 0.90 reliable intervals for the interaction effect of executive moderation on changes in public approval conditional on executive extremity using the most common modeling strategies for TSCS data, along with the estimated effect using GPR.

Election year negatively affects *Executive moderation*, and neither do the competing models. All estimates are in the hypothesized direction, with the exception of the LDV model. RCSE estimates the effect as -0.55 with a standard error of 0.41. The PCSE estimate is -0.44 (SE = 0.29). The RE estimate is -0.14 (SE = 0.25). Finally, the LDV estimate is 0.29 (SE = 0.42). These estimates with 0.95 and 0.90 reliable intervals are shown in Figure 4.29.

Hypothesis four predicts that there is a negative interaction between *Extremity* and *Election year* on *Executive moderation*. This result holds at the 0.90 level for RCSE, the 0.95 for PCSE, and is in the hypothesized direction but unreliable for RE and LDV. The estimated effect of the interaction employing RCSE is -0.52 with a standard error of 0.30. PCSE estimates an effect of -0.43 (SE = 0.19). The estimated effect using RE is -0.31 (SE = 0.20). The LDV estimate of the interaction is -0.01 (SE = 0.33). These estimates with

Figure 4.29: The effect of election year on executive moderation, model comparison

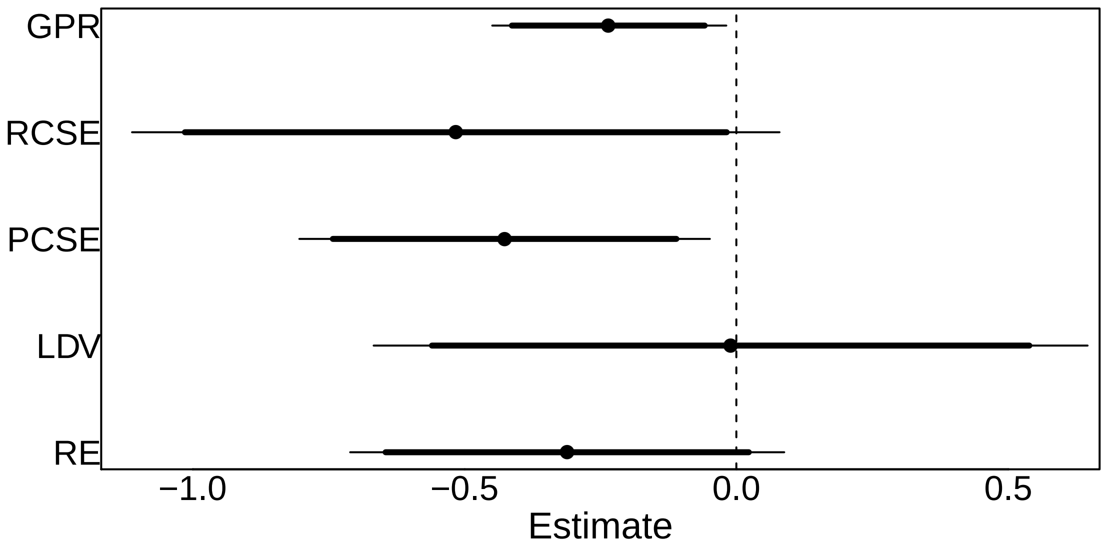


Note: Shown are estimates with 0.95 and 0.90 reliable intervals for the effect of election year on executive moderation using the most common modeling strategies for TSCS data, along with the estimated effect using GPR.

0.95 and 0.90 reliable intervals are shown in Figure 4.30.

This subsection highlights that modeling choice can sometimes make a difference on our substantive conclusions. However, the modeling strategies employed here are largely consonant with GPR. Where there is disagreement, because GPR makes fewer assumptions about the underlying data-generating process, and the assumptions made are relatively realistic, we should consider GPR as the best modeling strategy. While other methods produce mixed results with regards to the hypotheses, these results are based on models with problematic issues as discussed in the previous chapter (see also Esarey and Menger 2017). Further, as I already mention, the degrees of freedom lost to these two-way effect models is not justified, and severely diminishes the explanatory power of our variables of interest on the outcome.

Figure 4.30: The effect of election year on executive moderation conditional on extremity, model comparison



Note: Shown are estimates with 0.95 and 0.90 reliable intervals for the interaction effect of election year on executive moderation conditional on executive extremity using the most common modeling strategies for TSCS data, along with the estimated effect using GPR.

4.16 Conclusion

This chapter argues that presidents across Latin America enjoy a boost of public support following moderation of their expressed ideological points. These benefits are heterogeneous, however, and the more extreme an executive the greater the increased support following shifts. Further, executives, particularly extreme executives, are less willing to moderate during election years in order to target their core voters.

To test these arguments, I apply Gaussian process regression for time-series cross-sectional analyses. The data consist of yearly ideological points as estimated from the constitutionally imposed annual presidential addresses, along with yearly public approval data from representative surveys. The results are largely in line with expectations, and begin to fill the gap in the literature on the effect of executive ideological movement on public opinion, and therefore the incentives for executives to shift.

However, there are still open questions for future research. For example, empirically, more moderate presidents seem to be publicly damaged following ideological moderation. This could be due to perceptions of switching sides, and may even be linked to the nuances of coalition building and cross-assembly support. There is also the question of endogeneity. Less popular presidents may change their professed ideology in order to become more popular.

Further, more moderate executives do not seem to move to the extremes during election years, but instead move further towards the center. While this chapter argues this is due to different electoral dynamics of support for these executives and their parties, there is no test of the underlying causal mechanism for this argument.

Finally, while suggestive of which presidents benefit from moderation and why we see heterogeneity of presidential willingness to moderate, this chapter does not explore the true ideal points of executives, only their professed ideal points, and can therefore not make strong claims about the incentives presidents face for moderation. Further work in this area would be fruitful. To conclude, this project starts to help us better understand the causes and consequences of presidential movement, but also opens the door for more work in this area.

Concluding Remarks

The three main chapters of this dissertation argue that the modeling assumption of conditional independence is often violated, and this violation is often ignored in political science research. Ignoring this fundamental principle can lead to biased estimates, inefficient estimators, and incorrect substantive conclusions. We also lose some of the nuance of the theoretical understanding of political science by not addressing this problem. The dissertation in part is therefore a claim that the discipline needs to be more careful about understanding the true data-generating process that leads to our observations, and needs to model the data in a more thoughtful manner that represents the underlying relationships between variables in which we have interest.

I begin to address some of the more common analyses we encounter in political science, particularly comparative political science, by developing two models. The second chapter develops a zero-inflated multivariate ordered probit to handle ordered, multivariate outcomes that may share a relationship in the underlying dynamics that generate the observations. I apply the model to party campaign dynamics, but the model can be utilized for many questions in comparative political science. Ordered outcomes are very common, and in comparative politics there is often a reason to doubt the assumption that (1) the outcomes do not share commonalities in the underlying data-generating process, and (2) every actor or unit of observation responds to observables in the same manner. The chapter therefore provides a very useful model, and suggests how to carefully consider the underlying nature of the models chosen for analyzing data commonly encountered in the discipline.

The third chapter uniquely parameterizes Gaussian process regression (GPR) for applications to time-series cross-sectional (TSCS) data. While not a new model, it is underutilized in political science and this chapter not only helps introduce the model to the discipline but demonstrates how it can be changed for our purposes as social scientists. Though specific to TSCS data in this chapter, of which analyses commonly suffer problems that GPR helps alleviate, many other types of analyses can benefit from considering models of this class. Both of these chapters also discuss and show through example how these models can be applied in the discipline.

Further, all three chapters aid in our understanding of comparative politics in Latin America and politics more generally. The second chapter, using Mexico as a case study, shows that the choices for presidential campaigns to visit a municipality or hold a rally are related to other campaigns' decisions. Further, the strategic calculi for municipal-level behavior varies from party to party. Campaigns do not respond to the same variables in the same way. This is a novel finding that likely generalizes to other cases, and would be missed with traditional analysis techniques.

The third chapter demonstrates that inflation in Latin America leads to decreased anti-American sentiment in the region. Latin American citizens view the United States as a source of economic well-being. When the economy, specifically the citizens' purchasing power, a very tangible economic indicator, is not doing well, they want to increase relations with the United States and see the U.S. as a way out of their economic hardship. This finding helps us better understand the international perceptions of the United States and in which circustances these perceptions may change. This chapter also re-analyzes, with stronger results, the effect of rocket threat on the right vote in Israel. Voters who are within range of Palestinian rockets are substantially more likely to vote for parties on the right. This is in line with previous research, but the magnitude of the relationship is much stronger than previously thought.

The fourth chapter generates and tests more in-depth hypotheses, specifically regarding Latin American presidential position-taking and the effect this has on executive public approval, and therefore the incentives for executive ideological movement. I argue that presidential moderation in their professed ideology increases overall public support. This support is beneficial to the executive both in its own right and because it increases legislative bargaining power. However, extreme executives enjoy greater benefit from moderation. This moderation comes at a cost, particularly for relatively extreme executives, by decreasing the enthusiasm of their core supporters. Therefore, in order to turn out the vote and appeal to their core supporters and activists, this moderation is less likely during election years, and extreme executives will shift their professed ideology back to the extremes. This relationship between executive movement and public perception has been largely ignored in Latin America, and this chapter begins to address this oversight.

The dissertation contributes both methodologically and theoretically to political science, particularly comparative political science in Latin America. However, it also opens the door for more research, again both methodologically and theoretically. Before discussing the drawbacks of the dissertation and directions for future work, I will outline my plans for the progression of the dissertation.

5.17 Related Future Work

This dissertation opens the door for much future work. The second chapter develops a zero-inflated multivariate ordered probit, and applies it to an interesting substantive question. However, there are two drawbacks. The first is that the model is computationally very expensive. I intend to recode the model in Stan in order to increase the speed with which the model is estimated. I then plan to write an easy to use `R` package that will allow much easier estimation with the model for the end user. This will be published on CRAN shortly thereafter.

The second drawback of the ZIMVOP model is that consumers may not know how to apply it to their own substantive questions. I plan, as part of the package development, to write open-source vignettes that apply the model to a few more substantive questions using the developed package. While work on this model will not result in peer-reviewed publications, I strongly believe it will help increase the model's exposure and future use to researchers in the field.

The third chapter introduces Gaussian process regression (GPR) as a solution to time-series cross-sectional analyses. However, the applications are nearly endless. We have strong reason to doubt our modeling assumption of conditional independence in many situations, and I plan to capitalize on the model's relaxation of this assumption for many more applications. For example, while Monogan and Gill (2016) make great strides introducing Bayesian kriging to spatial applications, very little attention has been paid to this work in comparative politics. Further, the implications this work can have on spill-over effects, spatial correlations, and any question regarding geographic units have not been fully exploited.

Computer scientists and statisticians have also made numerous developments to the Gaussian process of which political scientists are seemingly unaware. There are almost countless projects that I envision introducing these strides to the political science literature. As an example, Roman Garnett, Jacob M. Montgomery, and I are working to improve sentiment analysis of political text using Gaussian process machine learning algorithms. I also have several ideas for future sole- and / or co-authored work further developing and introducing this class of models to the discipline.

Finally, the fourth chapter, while introducing and validating novel hypotheses, also leaves unanswered questions that I plan to continue investigating. For example, developing a more granular measure of professed ideal points for presidents to test richer hypotheses would be fruitful. Executives are communicating to the public in many ways other than the annual addresses. Unfortunately, this will require a major data-gathering exercise. Fortunately, with the funding and research assistance I will have in the coming years, I should be able to make great advances towards reaching this goal.

The fourth chapter also does not adequately test the effect of professed moderation or movements to the extreme by centrist executives on public perception. I believe that a survey experiment is the best avenue to approach this. It can help us better understand why moderation by centrist executives seems to harm the executive's public approval.

To conclude, this dissertation solves many methodological and theoretical problems in political science. It also, however, will continue developing novel work in methodology, comparative politics, and Latin American politics.

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Appendix

7.18 Modeling Related Processes with an Excess of Zeros Supplementary Information

7.18.1 ZIMVOP JAGS Model

The JAGS model follows the chapter's specification, and is quite general. It can handle any number of predictors, any number of dimensions, and any number of cut-offs within the different dimensions. It assumes that there is some observation-level data that can predict participation, and once participation is attained, it predicts the level of participation on all d dimensions. The estimated variance-covariance matrix can be used to determine the correlations between dimensions.

```
model{
  #y is a matrix of outcomes of dimension NxD
  #N is the number of observations
  #D is the number of dimensions
  #Z is the matrix of first-stage predictors of dimension NxK
  #K is the number of first-stage predictors, including an intercept
  #X is an array of predictors for the second stage
  #n.cut is a scalar, the number of cut points
  #c is a vector, the second cut point
  #R is the inverse-Wishart prior matrix
```

```
for(i in 1:N){
  #if outcomes have different number of levels, change n.cut to a vector

  #first stage
  S[i] <- max(min(S.star[i],1),0)
  probit(S.star[i]) <- s.star[i]  #participation regime probability
  s.star[i] <- inprod(Z[i,], gamma[1:K])

  #second stage
  for(d in 1:D){
      Xnew[d,i] <- inprod(X[i,,d], beta[1:J,d])
  }
  y.star[i, 1:D] ~ dmnorm(Xnew[,i], invW[,])
  for(d in 1:D){
      for (i.cut in 1:n.cut){
        probit(Q[i,i.cut,d]) <- tau.unsorted[i.cut, d] - y.star[i,d]
      }
  }
  for(d in 1:D){
      p[i,d,1] <- (1-S[i]) + S[i]*max(min(Q[i,1,d],1),0)
      for(i.cut in 2:n.cut){
          p[i,d,i.cut] <- S[i]*(max(min(Q[i,i.cut,d],1),0) - max(min(Q[i,i.cut-1,d],1)
      }
      p[i,d,n.cut+1] <- S[i]*(1 - max(min(Q[i,2,d],1),0))
      y[i,d] ~ dcat(p[i,d,])
  }

}
for(j in 1:J){
  for(d in 1:D){
    beta[j,d] ~ dnorm(0,0.0001)
  }
}
for(k in 1:K){
  gamma[k]  ~ dnorm(0,0.0001)
}
for(d in 1:D){
  tau.unsorted[1,d] <- 0
  tau.unsorted[2,d] <- c[d]
  #uncomment below for more than three outcomes
  #for(i.cut in 3:n.cut){
  #  tau.unsorted[i.cut,d] <- tau[i.cut, d]
```

```
    #  tau[i.cut, d] ~ dlnorm(0,0.025)
    #}

  }

  #can change D+1 to be anything greater than or equal to D
    #can pass this in as a parameter
  invW[1:D,1:D] ~ dwish(R[,],D+1)
  Sigma[1:D,1:D] <- inverse(invW[,])
  for(d in 1:D){
    vars[d] <- Sigma[d,d]
  }
  for(l in 1:(D-1)){
    for(q in l:(D-1)){
      rho[q+l-1] <- Sigma[l,q+1]/sqrt(vars[l]*vars[q+1])
    }
  }

}
```

7.18.2 ZIMVOP Simulation Exercise

Table 7.3 shows the true parameters for the data generating process for the first round of simulations. Table 7.4 shows the true parameters for the data generating process for the second round of simulations.

Table 7.3: True parameters for the first round of simulations

Simulation	γ_0	γ_1	β_{10}	β_{11}	β_{20}	β_{21}	β_{30}	β_{31}
1	-1.5	4	0	2	0	2.5	0	2
2	-1.5	5	0	2	0	2.5	0	2
3	-1.5	6	0	2	0	2.5	0	2
4	-1.5	7	0	2	0	2.5	0	2
5	-1.5	8	0	2	0	2.5	0	2
6	-1.5	9	0	2	0	2.5	0	2
7	-1.5	10	0	2	0	2.5	0	2
8	-1.5	11	0	2	0	2.5	0	2

Note: Each simulation was repeated 100 times. All correlation coefficients were set to zero. The first-stage parameters are in the second and third columns. The second-stage parameters are in the latter six columns. The subscripts on these parameters denote first the dimension, and second the indicator for the parameter. Zeros refer to intercepts, and ones refer to the variable of interest.

Table 7.4: True parameters for the second round of simulations

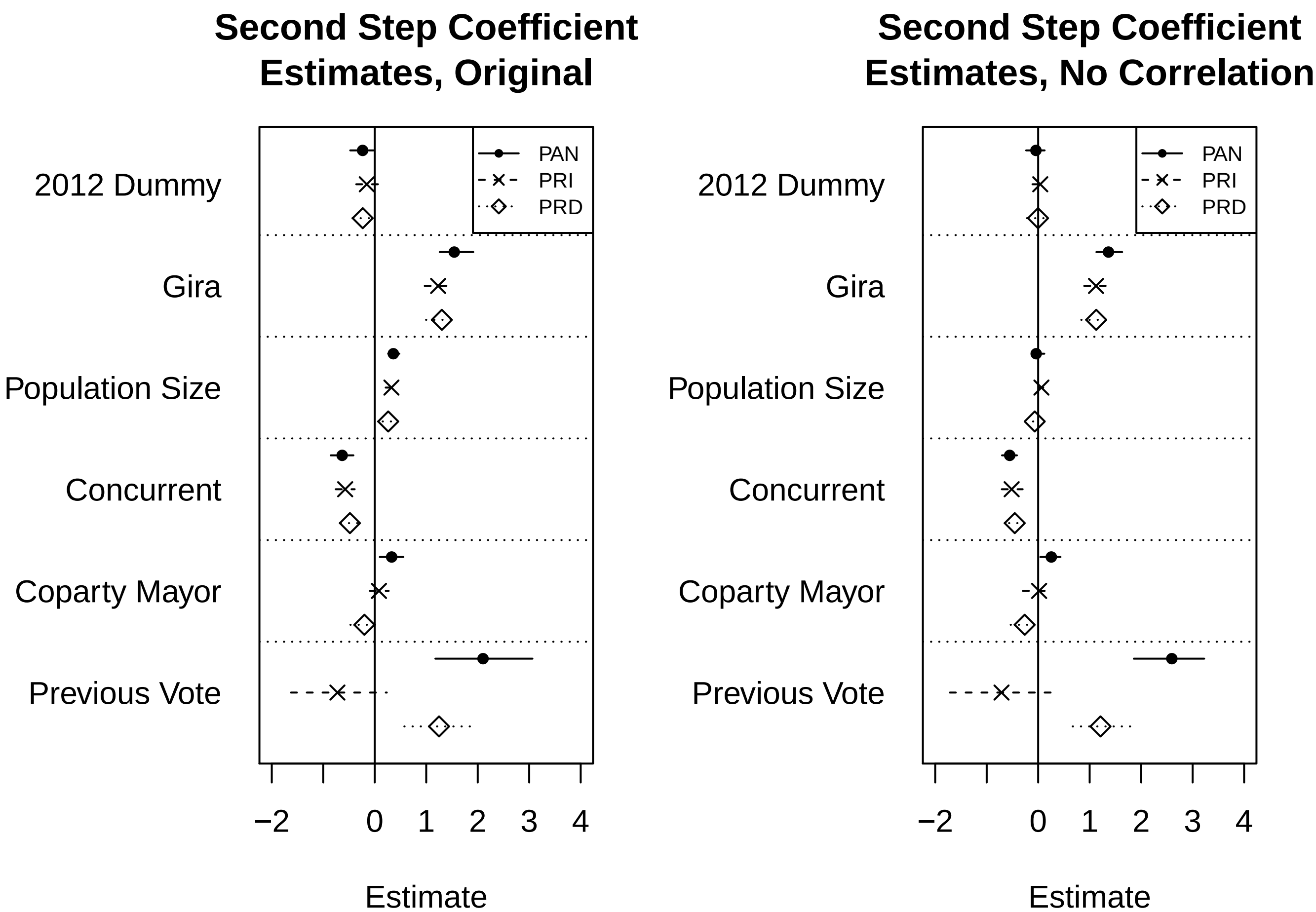
Simulation	γ_0	γ_1	β_{10}	β_{11}	β_{20}	β_{21}	β_{30}	β_{31}	ρ_{12}	ρ_{13}	ρ_{23}
1	-1.5	9	0	2	0	2.5	0	2	$-.8$	0	0
2	-1.5	9	0	2	0	2.5	0	2	$-.6$	0	0
3	-1.5	9	0	2	0	2.5	0	2	$-.4$	0	0
4	-1.5	9	0	2	0	2.5	0	2	$-.2$	0	0
5	-1.5	9	0	2	0	2.5	0	2	0	0	0
6	-1.5	9	0	2	0	2.5	0	2	.2	0	0
7	-1.5	9	0	2	0	2.5	0	2	.4	0	0
8	-1.5	9	0	2	0	2.5	0	2	.6	0	0
9	-1.5	9	0	2	0	2.5	0	2	.8	0	0
10	-1.5	9	0	2	0	2.5	0	2	$-.8$	.64	$-.51$
11	-1.5	9	0	2	0	2.5	0	2	$-.6$	.36	$-.22$
12	-1.5	9	0	2	0	2.5	0	2	$-.4$	.16	$-.06$
13	-1.5	9	0	2	0	2.5	0	2	$-.2$	.04	$-.01$
14	-1.5	9	0	2	0	2.5	0	2	0	0	0
15	-1.5	9	0	2	0	2.5	0	2	.2	.04	.01
16	-1.5	9	0	2	0	2.5	0	2	.4	.16	.06
17	-1.5	9	0	2	0	2.5	0	2	.6	.36	.22
18	-1.5	9	0	2	0	2.5	0	2	.8	.64	.51
19	-1.5	9	0	2	0	2.5	0	2	$-.8$	$-.8$	.64
20	-1.5	9	0	2	0	2.5	0	2	$-.6$	$-.6$	.36
21	-1.5	9	0	2	0	2.5	0	2	$-.4$	$-.4$	.16
22	-1.5	9	0	2	0	2.5	0	2	$-.2$	$-.2$	.04
23	-1.5	9	0	2	0	2.5	0	2	0	0	0
24	-1.5	9	0	2	0	2.5	0	2	.2	.2	.04
25	-1.5	9	0	2	0	2.5	0	2	.4	.4	.16
26	-1.5	9	0	2	0	2.5	0	2	.6	.6	.36
27	-1.5	9	0	2	0	2.5	0	2	.8	.8	.64

Note: Each simulation was repeated 100 times. The first-stage parameters are in the second and third columns. The second-stage parameters are in the fourth through tenth columns. For the first- and second-stage parameters, the subscripts denote first the dimension, and second the indicator for the parameter. Zeros refer to intercepts, and ones refer to the variable of interest. The final columns are the correlation coefficients of the second stage. The subscripts denote the dimensions (e.g. a subscript of 12 refers to the correlation between the residuals of the first and second dimensions). The first nine set the second and third correlations, ρ_{13} and ρ_{23}, to zero, and ρ_{12} varies from -0.8 to 0.8 by 0.2. The second nine keep the same ρ_{12} shift, setting ρ_{13} to ρ_{12}^2 and ρ_{23} to ρ_{12}^3. The final nine again maintain the same ρ_{12} shift and set ρ_{13} to ρ_{12} and $\rho_{23} = \rho_{12}^2$. This choice stemmed partly from the need to generate positive definite matrices.

7.18.3 Presidential Campaigns in Mexico

To better assess how our inferences would be changed if the main application in the chapter did not allow for correlations, I present a model that restricts the correlation parameters to be zero. This is similar to the simulation exercises comparing ZIMVOP to an uncorrelated multivariate probit. The first-step estimates are nearly identical between models, but the second-step estimates vary fairly substantially, with some estimates reliable in one model and not in the other. A main parameter of interest, *Previous vote*, though not reliable at the 95% level for the PRI, is not even reliable at the 90% in the second model. The results, presented next to each other, are shown in Figure 7.31. Not only would we be losing the information provided by the correlation estimates, the simulations suggest that we should trust the posteriors from ZIMVOP.

Figure 7.31: Comparing ZIMVOP to a model without correlations on the main application



Note: The second-step results from ZIMVOP are presented on the left panel, and the results not allowing correlations is on the right panel. As can be seen, the posteriors are fairly different. Not only would we be losing the information provided by the correlation estimates, the simulations suggest that we should trust the posteriors from ZIMVOP.

7.19 Executive Moderation and Public Approval in Latin America Supplementary Information

Below are the tables of the $\boldsymbol{\beta}$ estimates for all of the GPR models run in the fourth chapter. I suppress the estimates for the presidential indicators for space.

Table 7.5: Posterior $\boldsymbol{\beta}$ estimates for the first hypothesis

	mean	2.5%	97.5%
Executive moderation	0.328	0.097	0.567
GDP growth	0.362	−0.005	0.736
Inflation (logged)	−0.249	−1.012	0.585
Spending	0.068	−1.114	1.185
Trade	0.580	−0.939	2.215

Table 7.6: Posterior $\boldsymbol{\beta}$ estimates for the second hypothesis

	mean	2.5%	97.5%
Executive moderation	−0.052	−0.340	0.259
Extremity	0.760	0.252	1.220
Executive moderation × *Extremity*	0.217	0.009	0.418
GDP growth	0.362	0.064	0.691
Inflation (logged)	0.135	−0.682	0.901
Spending	0.695	−0.561	1.875
Trade	0.308	−1.090	1.613

Table 7.7: Posterior $\boldsymbol{\beta}$ estimates for the third hypothesis

	mean	2.5%	97.5%
Election year	−0.186	−0.692	0.317
GDP growth	0.306	−0.068	0.664
Inflation (logged)	−0.156	−0.992	0.684
Spending	−0.394	−1.589	0.835
Trade	−0.082	−1.832	1.541

Table 7.8: Posterior $\boldsymbol{\beta}$ estimates for the fourth hypothesis

	mean	2.5%	97.5%
Election year	−0.135	−0.484	0.203
Extremity	1.271	0.956	1.604
Election year × *Extremity*	−0.235	−0.449	−0.018
GDP growth	0.274	0.014	0.535
Inflation (logged)	0.449	−0.262	1.087
Spending	0.660	−0.450	1.752
Trade	−0.818	−2.092	0.331