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MEASURING AND DETECTING POLITICAL FORCES

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ABSTRACT

The essays in my dissertation explore the impact of political action on everyday life by utilizing the modern theory of political economy and investigating the “unintended” consequences of political behavior. Specifically, the first essay studies the impact on communities when the Army activates reservists and guardsmen. The second essay challenges traditional ways of measuring electoral data, helping in the understanding of election campaigns. The third essay demonstrates the new electoral measure to ask and answer, “Did Bush bring home troops to bolster campaign support in 2004?”

DEDICATION

For my husband, Joshua Zerkle, with love and gratitude.

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CHAPTER ONE

INTRODUCTION

The essays in my dissertation explore the impact of political action on everyday life by utilizing political economy theory and investigating unintended consequences of political behavior. Specifically, the first essay studies the impact on communities when the Army activates reservists and guardsmen. The second essay challenges traditional ways of measuring electoral data, hence helping in the understanding of election campaigns. The third essay demonstrates the innovation of the second essay to ask and answer, “Did Bush bring home troops to bolster campaign support in 2004?”

My first essay stresses one unintended consequence of reserve mobilization. I exploit the tendency of military reservists to have civilian jobs and voluntary contributions in public safety by correlating emergency response times to vehicle accidents with data on Army Reserve mobilization. First, I find evidence, supported by previous literature, that suggests people who are (or were) members of the armed services are more likely to participate in community service positions. Using the National Longitudinal Survey of Youth 1997 Cohort, I find that 8 percent of people ever in the military had paying jobs in protective services. Then, using the National Highway Traffic Safety Administration’s Fatality Analysis Reporting System and the Department of Defense’s monthly press releases, I correlate emergency response times to fatal accidents with reserve mobilization data. I estimate that mobilization of reserves increases response times between 2 and 44 seconds at the average. This estimate is probably an underestimate due to the nature of state level data. The unintended consequence of activating troops is important because the longer a patient waits for medical care, the higher the risk of death or disability. This is a hidden cost borne by the community when reservists are called to active duty.

The second essay challenges the use of *ex post* election outcomes as a measure of pre-election closeness. Using data from the US Election Atlas, I utilize poll data, campaign spending and state visits to develop a method of measuring *ex ante* election closeness with the goal of mimicking how campaigns perceive closeness leading up to Election Day. I find little correlation between my measure and the *ex post* measures used in previous literature. This suggests that election outcomes are endogenous with regard to campaign behavior and treating *ex post* measures as exogenous is inappropriate.

The third essay demonstrates how to utilize my *ex ante* closeness measure by investigating whether the Bush Administration strategically deactivated reserve troops to garner political support in states with close races. I combine my closeness measure with Department of Defense monthly press releases, which record unit level reserve mobilization. All results estimating electoral closeness must be interpreted with care. The closeness measure may be approximating something else that closely corresponds to state level differences. In this case it could merely be a coincidence of the expansion and contraction of reserve troops that happens during wartime. Perhaps states with higher number of reserves in general are states that are battleground states, which correspond with returning troops. This essay demonstrates the *ex ante* electoral closeness measure and finds some evidence that the Bush Administration strategically deactivated reservists in states that were closely contested in the 2004 presidential election.

I discuss the literature relevant to each essay in the individual essays to follow.

CHAPTER TWO

THE HIDDEN COST OF MOBILIZING RESERVES

Introduction

Mobilizing reserves removes productive individuals from local economies. This imposes costs on the people remaining those economies[1]. I quantify one cost by utilizing the tendencies of reservists to work in protective services. Specifically, I find that emergency response times to fatal accidents increase as more reservists are mobilized. This increase in response times is one identifiable cost of mobilization borne by members of the local economy. This cost is important because the risk of disability and death increases as response times increase (Athey and Stern 2002).

The Costs of Mobilization

The market for reservists follows the same structure as any labor market: wages are determined by supply and demand. The market supply is characterized by people who have costs lower than or equal to the market wage. Assuming a lack of market failures, the personal costs (i.e., absence from family, risk of death, and so on) of mobilization are captured in wages and therefore are internalized.

However, one must consider Title 38 of the United States Code, which entitles reservists to reemployment rights upon deactivation and protects

[1] For the purpose of this paper,“ mobilization” and “deployment” refer to reservists being activated to full time services, and “reservists” refers to Army Reserves and Army National Guard members.

reservists from discrimination[2]. This means that employers must treat deactivated employees as if there were no work interruption[3]. As a result, the market mechanism in the civilian labor force cannot operate freely. Specifically, wages cannot adjust for the probability of military activation, and reservists cannot be replaced upon activation. Employers must choose to hire temporary workers, expand their workforce (hire permanent workers and maintain the reservists' positions), or simply leave the position vacant. In this sense, Title 38 disrupts typical market adjustments resulting in extra costs to employers. The cost may be particularly high when reservists are very specialized.

Reservists activated from the civilian protective services result in an internalized cost to the wage-paying organization (i.e., the police force budget) *and* an extra cost to the public served by the protective services[4]. This extra cost occurs for two reasons. First, there is no competition in protective services. Whereas other markets would compete away extra costs, communities generally are only serviced by one police force and one emergency response system. Second, these are specialized occupations that either are filled by "the next best candidate," a temporary worker, or are left vacant when a reservist is called up.

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[2]The antidiscrimination laws have an active enforcement mechanism. Steel (2004) reports about 3,800 cases of reservists filing complaints between 2000 and 2003, with approximately 90 percent of cases relating to job benefit reinstatement.

[3] Employers have the opportunity to file for exemptions to Title 38 reemployment rights. Specifically, Title 38 states employers may not have to reemploy reservists if reemployment is "impossible or unreasonable" or "impose[s] an undue hardship on the employer."

[4] "Protective services" refers to the police force, firefighters or emergency medical service crews (EMS).

Additionally, communities that are serviced by volunteer emergency response systems may suffer greater costs as volunteers are activated because volunteer markets lack wage response mechanisms to attract temporary or new workers.

Finding a statistical link between reservists and protective service members suffers from the lack of data and the relatively small number of reservists represented in the usual person level data sets. Data regarding volunteer emergency responders are even rarer. The Army does not keep cumulative data concerning reservists' civilian occupations. There is evidence in the public sector literature that public sector workers have greater tendencies to community service than their non-public sector counterparts (Goddeeris 1988). Also, Steel (2004) finds that reservists have greater tendencies to work in the public sector. He states, "Only 14 percent of civilian males are employed by the public sector, compared to 32 percent of all reservists." His reservist data from 2002 finds that more than 10 percent of all reservists work for local governments.

I turn to the National Longitudinal Survey of Youth 1997 Cohort (NLSY97) to draw a broad connection between those who serve in the military and those in protective services. The NLSY97 follows a cohort that was 12-17 years of age in 1997. Demographic summary statistics for this sample can be found in Table 5.1 in the Appendix. There are detailed employment histories for the 8,984 people in this dataset.

Approximately 5 percent of the sample enters the military between 1997 and 2005. Table 2.1 presents yearly counts of youths in the military and protective services, given that they are in the workforce. More than 8 percent of people who have been in protective services have also been in the military. Given that these numbers only reflect people in paid protective services positions, it is probably an underestimate. This evidence suggests at least a link between members of the armed services and civilian protective services.

Table 2.1 Protective Services and the Military

Year	Mil	PS	Both	Labor Force	Mil/Total	PS/Total	Both/PS
1997	3	26	0	2246	0.13%	1.16%	0.00%
1998	19	73	1	4399	0.43%	1.66%	1.37%
1999	59	102	4	5467	1.08%	1.87%	3.92%
2000	120	153	8	6489	1.85%	2.36%	5.23%
2001	156	170	8	6697	2.33%	2.54%	4.71%
2002	251	154	6	6688	3.75%	2.30%	3.90%
2003	284	161	10	6844	4.15%	2.35%	6.21%
2004	272	186	10	6709	4.05%	2.77%	5.38%
2005	51	195	9	6614	0.77%	2.95%	4.62%
1997-2005	462	641	56	8718	5.30%	7.35%	8.74%

Total Observations: 8,984

PS: People in the Protective Services
Mil: People in the Military
Both: People in Protective Services and the Military
Labor Force: Number of people in the labor force

Source: National Longitudinal Survey of Youth 1997 Cohort

Literature and Data

The literature on emergency services largely focuses on accident and mortality rate differences across socio-economic classes by location. Emergency response times are generally used as controls in regressions exploring emergency services inputs, like emergency services budgets. It is largely accepted that physical factors, like greater urban sprawl and poorer road conditions, lead to higher death rates (American Farmland Trust 1998; Ewing, Schieber and Zegeer 2003; Felder and Brinkmann 2002; Lambert and Meyer 2006; Keeler 1994). Lambert and Meyer (2006) use response times as a dependent variable in a preliminary regression to support an urban sprawl theory. I draw on this model in developing my own model of emergency response times.

Empirical work about reservists falls into two main categories. First, many studies explore the impact of service on reservists and/or veterans. Lyle (2006) finds a negative correlation between standardized test scores and parental absence due to military service. Lyle (2006) examines all service absences, not just reserve mobilization. Drummet, Coleman and Cable (2003) survey the Sociological literature concerning generally military stressors. They conclude that there are three major stressors that are unique to service members that sociologists study: relocation, separation and reunion. Angrist and Johnson (2000) focus on Gulf War veterans to study divorce rates, spousal employment and child disabilities. Hirsch and Mehay (2003) use the Reserve Components Surveys to show that reservists who have served active duty earn more than reservists who have not served active duty. MacLean (2005) explores the impact of education disruption that reservists may experience when called to duty. In general, these reservist level studies focus on the impact of service on reservists and their families.

The second type of literature concerning reservists explains the relationship between reservists, the military and civilians. One example is Feaver, Filer and Gronke (2004, 208). Using surveys, they find that evidence that Army Reserve and Army National Guard members, "…bring military values into society, providing a vital source of contact between the civilian population and the armed forces." A study by Winkler et al (2004) analyzes the attributes that the reserve system adds to labor supply and demand. This market level study focuses on the mutual benefit the reserve system offers the military and reservists. Schrock (2004) combines Department of Defense data with Federal regulations to examine the current and future military practices concerning reservists.

I use two types of data: emergency response time data and mobilization data. The emergency response time data comes from the Fatality Analysis Reporting System's Web-based Encyclopedia (FARS). These data are collected by the US Department of Transportation and the National Highway Traffic Safety Administration. Each entry in FARS contains very specific information about the fatal accident, including emergency response times[5]. Data about the mobilized reservists were collected from the US Department of Defense website.

Table 2.2 contains a summary of the variables used in the state level analysis. The dependent variable (LN(RESPONSE)) is the natural log of the emergency response time to each accident.

[5] A fatal accident is an accident that results in a death within 30 days of as a result of the accident.

Table 2.2 Variable Definitions

LN(RESPONSE)	Logged number of minutes for emergency response crews to arrive
RESDENS	Number of mobilized reservists divided by population (in 1000s), scaled by 1000
SPEEDX	Dummy Variables indicating the speed limit is X mph at the accident site
LANEX	Dummy Variables indicating the number of lanes is X at the accident site
HW	Dummy variable indicating the accident took place on a highway
SURF	Dummy variable indicating the accident took place on asphalt or concrete
DRY	Dummy variable indicatingthe accident took place on a dry surface
DAY	Dummy variable indicating the accident took place during daylight hours
DARK	Dummy variable indicating the accident took place during night hours
RAIN	Dummy variable indicating the accident took place during rain
SNOW	Dummy variable indicating the accident took place during snow
CLEAR	Dummy variable indicating the accident took place with clear skies
WEEKEND	Dummy variable indicating the accident occurred on Friday, Saturday or Sunday
CITY	Dummy variable indicating the accident took place within city limits
RESPONSE	The raw response time
RESERVES	The raw number of reserves

Sources: The Fatality Analysis Reporting System and Department of Defense News Releases

The main variable of interest is reserve density (RESDENS). Reserve density is the monthly number of reserves mobilized divided by the population on a state level and is scaled by 1,000. This measure captures how many reservists are mobilized with respect to the population of the state. The remaining independent variables describe conditions that emergency response crews encounter while responding to accidents. For instance, CLEAR, RAIN and SNOW are dummy variables indicating if the weather is clear, raining or snowing respectively. Other dummy variables account for the roads themselves. HW is a dummy variable equal to one if the road is a state or national highway. The dummy variable SURF represents roads that are surfaced with asphalt or concrete. I also use dummy variables describing the speed limit and number of lanes at the accident site. Information concerning the city is used to construct a dummy variable equal to one if the wreck occurs within city limits. Also, a dummy variable accounts for accidents that happen on weekends (WEEKEND).

I limit the data to accidents with response times less than one hour[6]. Table 2.3 shows the summary statistics for all observations included in this state-level analysis. The data set is limited to the period between October 2001 and December 2005 due to the limited information about reservists prior to October 2001. The mean response time is about 9.8 minutes.

According to the FARS statistics, most fatal accidents happen in the early morning hours of Saturdays and Sundays; thus it is not surprising that this sample has about half of all wrecks occurring on the weekend. The speed limit ranges between 5 and 90 miles per hour. I combine the observations that have speed limits greater than 70 miles per hour into one category[7]. The low minimum speed limit is a result of the inclusion of wrecks that happen in parks and other special jurisdictions.

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[6] I exclude about 2,000 cases because the emergency call was canceled, but response times were reported as greater than an hour, in several cases they were more than 24 hours.

[7] See Table 5.5 in the Appendix for a regression with expanded speed and lane catagories.

Table 2.3 Summary Statistics All Observations

Variable	Mean	Std. Dev.	Min	Max
RESPONSE	9.765	7.333	1	59
RESDEN	0.407	0.371	0.000	2.854
HW	0.284	0.451	0	1
SURF	0.954	0.21	0	1
DRY	0.814	0.389	0	1
DAY	0.514	0.5	0	1
DARK	0.442	0.497	0	1
CLEAR	0.869	0.338	0	1
RAIN	0.081	0.272	0	1
SNOW	0.025	0.156	0	1
WEEKEND	0.501	0.5	0	1
CITY	0.383	0.486	0	1
LANE 1-2	0.793	0.405	0	1
LANE 3-4	0.173	0.378	0	1
LANE 5-6	0.022	0.147	0	1
LANE 7 & Greater	0.012	0.110	0	1
SPEED5-20	0.005	0.068	0	1
SPEED25-40	0.245	0.430	0	1
SPEED45-60	0.559	0.496	0	1
SPEED60 & Greater	0.191	0.393	0	1

Number of Obs: 89,220

Sources: The Fatality Analysis Reporting System and Department of Defense News Releases

About 95 percent of accidents occur on asphalt or concrete roads and 28 percent of the sample happens on state or national highways. The accidents that happen within one hour of sunrise or sunset are categorized as “twilight” wrecks, and account for less than 5 percent of the sample. The dummy variable DRY indicates if the road itself is dry. Approximately 8 percent of the wrecks happen in the rain, but almost 20 percent of accidents happen on wet roads. About 2 percent of wrecks happen in the snow. The omitted weather category includes wrecks that happen in fog, sleet or wintry mix.

Table 2.4 shows summary statistics for wrecks that occur within city limits. The average response time for this subset of observations is about 7 minutes. This subset has similar characteristics to the entire sample. For instance, the subset mirrors the total sample with about 50 percent of wrecks occurring on weekends. One difference the statistics reveal is that the city wrecks happen in areas with lower speed limits. About 25 percent of all wrecks occur in places where the speed limit is 40 miles per hour or less. Of the subset of wrecks that occur in city limits, more than 45 percent happen in low speed areas.

Table 2.4 Summary Statistics for Observations Within City Limits

Variable	Mean	Std. Dev.	Min	Max
RESPONSE	7.079	5.6	0	60
RESDEN	0.378	0.362	0	2.854
HW	0.342	0.474	0	1
SURF	0.975	0.155	0	1
DRY	0.821	0.383	0	1
DAY	0.480	0.500	0	1
DARK	0.479	0.500	0	1
CLEAR	0.879	0.326	0	1
RAIN	0.081	0.273	0	1
SNOW	0.021	0.142	0	1
WEEKEND	0.497	0.500	0	1
LANE 1-2	0.663	0.473	0	1
LANE 3-4	0.276	0.447	0	1
LANE 5-6	0.040	0.197	0	1
LANE 7 & Greater	0.021	0.145	0	1
SPEED5-20	0.008	0.087	0	1
SPEED25-40	0.455	0.498	0	1
SPEED45-60	0.422	0.494	0	1
SPEED60 & Greater	0.115	0.320	0	1

Number of Obs: 34,181

Sources: The Fatality Analysis Reporting System and Department of Defense News Releases

Empirical Model

My empirical model is based on of Lambert and Meyer's (2006) preliminary model. Below is the basic regression equation:

$$LN(RESPONSE_i) = \beta_1 + \beta_2 X_i + \beta_3 RESEDEN_i + \varepsilon_i \qquad (2.1)$$

where the X vector is the wreck-specific variables discussed above. I expect β_3 to have a positive coefficient. This would indicate that as a greater percentage of the state population is mobilized, the longer response times become proportionately. One can easily predict the signs of most of the variables. I expect the variables that indicate better road conditions to have negative coefficients (i.e., faster response times). Therefore, I expect wrecks that happen on highways, in dry, clear conditions and in cities to have faster response times than those with opposite conditions. I expect weekend wrecks to have longer response times because a majority of wrecks happen in the early hours of weekends, which may strain the capacity of emergency response systems.

I also estimate fixed effects models that take on the basic form:

$$LN(RESPONSE_i) = \beta_1 + \beta_2 X_i + \beta_3 RESEDEN_i + \mu_i + \varepsilon_i \qquad (2.2).$$

Equation 2.2 has an X-vector of accident specific variables and reserve density like Equation 2.1. The main difference is that Equation 2.2 has another error term that accounts for the county-level differences in wrecks. This error term accounts for county-level sprawl because much of the previous literature relies on county-level sprawl indices, like the one developed by Lambert and Meyer (2006).

Results

Table 2.5 displays results of estimates without monthly dummy variables. Column 1 reports OLS results. Column 2 shows OLS with clustered standard errors by county. Clustering the standard errors by county assures that standard errors estimates are not too small because of heteroscedasticity. Column 3 reports county level fixed effects, and Column 4 shows city level fixed effects. Standard errors are clustered by county in Columns 3 and 4.

The main variable of interest, reserve density, is positive in all estimates. It is not surprising that the coefficient is weaker in the city-only estimates. Cities are more likely to have non-volunteer forces and can attract new/temporary workers through wages. The other independent variables show the signs expected. Wrecks that happen on the weekend have longer response times than their non-weekend counterparts. Response times are shorter during clear days. Accidents on highways have shorter response times compared to their non-highway counterparts. Emergency crews respond more quickly to accidents that happen on roads with asphalt or concrete surfaces compared to dirt or gravel roads. The lane dummy variables all have negatives coefficients. This indicates that roads with more than 2 lanes have faster response times than their 2 lane counterparts.

The estimates are smaller for the city fixed effects model than the county fixed effects or the OLS models. This is due in part to the subset of city wrecks having lower response times in general. For instance, in the OLS estimates, wrecks that occur on highways result in a -10 percent change in response time.

For the county and city fixed effects estimates, this drops to a -1.8 percent change.

The positive coefficients on reserve density indicate that as response times increased as reserve density increased. Strictly speaking, the coefficient's estimates on reserve density are interpreted as a one hundred percent increase in reserve density leads to a 10 percent increase in response times[8]. However, the maximum reserve density is less than 3. In the case of the OLS estimates, if reserve density goes from zero to the sample average, there is a 4.4. percent change in response times. At the average response time, 582 seconds, these results mean response times being an estimated 26 seconds longer at the average reserve density. Using the same technique, the estimated response times using county fixed effects and city fixed effects are 4 and 2 seconds respectively.

[8]This interpretation is due to the scaling of the variable.

Table 2.5 Dependent Variable Logged Response Time

Variable	1(OLS)	2(Cluster)	3(County FE)	4(City FE)
RESDEN	0.110 ***	0.110 ***	0.018 **	0.018
	[0.006]	[0.016]	[0.008]	[0.016]
HW	-0.146 ***	-0.146 ***	-0.076 ***	-0.028 **
	[0.006]	[0.013]	[0.010]	[0.011]
SURF	-0.090 ***	-0.090 ***	-0.158 ***	-0.068 **
	[0.011]	[0.017]	[0.015]	[0.031]
DRY	-0.024 ***	-0.024 ***	-0.037 ***	-0.023 *
	[0.008]	[0.009]	[0.008]	[0.014]
DAY	-0.034 ***	-0.034 ***	-0.043 ***	-0.052 ***
	[0.011]	[0.011]	[0.010]	[0.018]
DARK	-0.005	-0.005	[0.000]	-0.014
	[0.011]	[0.011]	0.010	[0.017]
CLEAR	-0.071 ***	-0.071 ***	[-0.068] ***	-0.063 **
	[0.014]	[0.016]	0.014	[0.026]
RAIN	-0.071 ***	-0.071 ***	-0.080 ***	-0.060 **
	[0.017]	[0.018]	[0.016]	[0.028]
SNOW	0.055 ***	0.055 **	0.046 **	0.069 **
	[0.020]	[0.022]	[0.020]	[0.035]
WEEKEND	0.037 ***	0.037 ***	0.030 ***	0.023 ***
	[0.005]	[0.005]	[0.005]	[0.008]
CITY	-0.343 ***	-0.343 ***	-0.370 ***	-
	[0.005]	[0.014]	[0.013]	
LANE 3-4	-0.144 ***	-0.144 ***	-0.125 ***	-0.108 ***
	[0.006]	[0.011]	[0.009]	[0.010]
LANE 5-6	-0.230 ***	-0.230 ***	-0.157 ***	-0.136 ***
	[0.015]	[0.027]	[0.020]	[0.018]
LANE 7 & Greater	-0.094 ***	-0.094 ***	-0.065 ***	-0.069 ***
	[0.020]	[0.026]	[0.023]	[0.024]
SPEED25-40	-0.045	-0.045	-0.030	-.048
	[0.033]	[0.041]	[0.034]	[0.041]
SPEED45-60	0.195 ***	0.195 ***	0.158 ***	0.163 ***
	[0.033]	[0.041]	[0.034]	[0.042]
SPEED60 & Greater	0.426 ***	0.426 ***	0.306 ***	0.313 ***
	[0.033]	[0.044]	[0.036]	[0.046]
Constant	2.181	2.181	2.324	1.836
OBS	89,220	89,220	89,220	34,181
R Squared	0.17	0.17	.16	0.05
rho	-	-	.24	0.44
Between R Squared	-	-	.24	0.04

*** Significant at 1%; **Significant at 5%; * Significant at 10%

Standard Errors in Brackets

Column 1 Unclustered OLS, Column 2 Clustered OLS; Clustered by County

The power of the model drops significantly when considering accidents within cities. The coefficient on reserve density is not significant at any reasonable level. This may be a result of incredible variance in cities within the sample. For example, New York City (NYC) is given the same treatment as Clemson, S.C.. The city fixed effect will account for across-city differences, but otherwise the model assumes that a highway in NYC has the same effect on response time as a highway in Clemson, S.C.. This is a general problem across all of the estimates and results in lower R-square values.

For comparison, I also run a random effects model. According to Kennedy (1998, 227), "If the data are a drawing of observations from a large population… and we wish to draw inferences regarding other members of that population, the fixed effects model is no longer reasonable; in this context, use of the random effects model has the advantage that it saves a lot of degrees of freedom." Since the data set is inherently a sub-sample of all wrecks, it makes sense to test the random effects model for consistency. The results of this are reported in the Appendix in Tables 5.2 and 5.3. However, after comparing the random effects model to the fixed effects model, which is a consistent estimator, I reject that the random effects model is a consistent estimator[9].

The results in Table 2.6 use dummy variables to control for month-specific factors. The predictions range from a non-significant city fixed effects 5 second

[9] See Table 5.4 in the Appendix for Hausman Test results.

increase to the OLS over-estimate of a 44 second increase. However, the county fixed effects model predicts about a 6 second increase on average.

Adding monthly dummy variables yield higher coefficients on reserve density across the board. In both sets of results wrecks that occur where speed limits are 24-40mph are not statistically different than wrecks that happen in 5-20mph zones[10]. However, wrecks in speed zones greater than 40mph have slower response times than the omitted category. The other results also do not change dramatically. For instance, the dummy variable indicating that a wreck happens on paved roads in the county fixed effects model has a coefficient indicating a -15.8 percent change without monthly effects. In the county fixed effects model, wrecks that happen on paved roads indicated a -16percent change compared to the wrecks that happen on unpaved roads.

[10] See Table 5.5 in the Appendix for extended speed and lane catagories.

Table 2.6 Dependent Variable Logged Response Time with Monthly Controls

Variable	1 (OLS)	2 (FE County)	3 (FE City)
RESDENS	0.189 ***	0.0261 **	0.023
	[0.021]	[0.011]	[0.018]
HW	-0.143 ***	-0.076 ***	0.010
	[0.013]	[0.009]	[0.011]
SURF	-0.079 ***	-0.161 ***	-0.040
	[0.017]	[0.015]	[0.038]
DRY	-0.031 ***	-0.043 ***	-0.019
	[0.009]	[0.009]	[0.014]
DAY	-0.037 ***	-0.045 ***	-0.038 **
	[0.011]	0.010	[0.019]
DARK	-0.003	0.001	-0.007
	[0.011]	[0.010]	[0.019]
CLEAR	-0.070 ***	-0.070 ***	-0.026
	[0.016]	[0.015]	[0.028]
RAIN	-0.075 ***	-0.084 ***	-0.013
	[0.018]	[0.016]	[0.030]
SNOW	0.065 ***	0.053 ***	[0.110] ***
	[0.022]	[0.020]	0.039
WEEKEND	0.037 ***	0.029 ***	0.016 **
	[0.005]	[0.005]	[0.008]
CITY	-0.340 ***	-0.370 ***	
	[0.013]	[0.013]	
LANE 3-4	-0.140 ***	-0.124 ***	-0.081 ***
	[0.011]	[0.009]	[0.010]
LANE 5-6	-0.228 ***	-0.157 ***	-0.078 ***
	[0.029]	[0.020]	[0.018]
LANE 7 & Greater	-0.100 ***	-0.067 ***	-0.041 *
	[0.025]	[0.023]	[0.024]
SPEED25-40	-0.039	-0.029	-0.048
	[0.040]	[0.034]	[0.043]
SPEED45-60	0.200 ***	0.159 ***	0.106 **
	[0.041]	[0.034]	[0.044]
SPEED60 & Greater	0.429 ***	0.306 ***	0.239 ***
	[0.043]	[0.036]	[0.047]
Constant	2.088	2.322	1.74
OBS	89,220	89,220	34,181
R Squared	0.17	0.16	0.05
rho	-	0.24	0.532
Between R Squared	-	0.24	0.06

*** Significant at 1%; **Significant at 5%; * Significant at 10%

Month Dummy Variables Included, but not reported.
Standard Errors in Brackets
All Columns: Clustered Standard Errors by County

I assert that any of these estimates are underestimates. I cannot directly link wrecks to specific reservists. Thus, the averages include wrecks affected by troop deployment and wrecks completely unaffected by troop deployment. Some of the estimates indicate that the response times affected by troop deployment are large enough to affect the overall average. If I could separate out only the affected accidents, I would find a stronger effect. Even a slightly longer delay might have a real world consequence. Athey and Stern (2002) find that a 30 second decrease from average response times resulted in a statistically significant decrease in mortality.

However, Army reservists mobilization may be highly correlated with another variable affecting response times. Perhaps the population make-up of an area is reflected by the number of reservists in general, and mobilization figures are just reflections of the sheer number of reservists. Another issue is heteroscedasticity. Using clustered standard errors prevents underestimating standard errors in OLS regressions. A GLS estimation is another way to deal with this issue, finding a correct weight for a weighted regression has not proved successful.

Because the FARS database only contains fatal accidents, there may be some backwards causation. Perhaps accidents are fatal because response times are long. Because the database does not contain non-fatal accidents, a Heckman selection model is not feasible. However, according to the American Association of State Highway and Transportation Officials (AASHTO) the average emergency response time in rural areas during 2002 was about 12 minutes. The sample used in this study has a non-city response time average of 10.4 over all years in the sample. The AASHTO found the response time average in 2002 for urban emergencies was about 7 minutes, while this study's sample had a city average of about 7 minutes. Taking these facts into consideration, there does not seem to be evidence of a selection bias in the dataset.

Conclusions

There is evidence that mobilizing reservists imposes an extra cost on the communities they leave. At the average reserve density, response times increase anywhere from 2 to 44 seconds due to mobilization. This means that decision makers may be miscalculating the marginal costs and benefits of mobilizing reserves. Specifically, the costs are underestimated, which may result in over-mobilization.

CHAPTER THREE

MEASURING ELECTORAL CLOSENESS

Introduction

When politicians make decisions in real time, they use *ex ante* measures to determine strategy. However, many economists have used *ex post* measures of closeness to explain political behavior, namely comparative election outcomes. I develop two *ex ante* measure of election closeness and compare them with *ex post* measures of closeness. I find that my two methods are not compatible with *ex post* measures, and I argue that the *ex ante* measure is the more appropriate way to characterize political behavior.

Previous literature on presidential behavior relies on current and previous election outcomes. Crain, Messenheimer and Tollison (1993) and Grier, McDonald and Tollison (1995) employ a typical closeness measure that relies on margin of victory variance, which is weighted by electoral worth. Wright's (1974) closeness measure focuses on party-specific concentration within state. These papers, and those that build from them, share two major aspects: weighting closeness measures by electoral worth and any *ex ante* measures are based on the previous presidential election(s).

There are flaws with approaches that use current and previous election outcomes. First, using current election margin of victory implicitly assumes that campaign behavior does not affect electoral outcomes or the margin of victory. Campaigning does affect outcomes within states. Thus, margin of victories are endogenous to campaign behavior. This aspect is discussed in greater detail in the next section. Second, using previous presidential election information means treating each state's historical voting as the main indication of future voting for all states. Not only is historical electoral data "stale," but this method assumes states are either one party leaning or swing states.

For example, Grier, McDonald and Tollison's (1995) measure essentially picks up on historical election "noisiness" within each state. If a state is merely going through a change in electoral preferences by having a few electoral swings, the measure registers it as noise. One example of this is Wisconsin in the late twentieth century. In the four presidential elections before the 1992 election, the state switched parties three times and margin of victories were less than ten percent. In tradition electoral closeness measures, this would be measured as pure noise and considered a highly competitive state. However, starting in 1992 the state voted for Democrat candidates, usually in a landslide[11]. Thus, the Grier, McDonald and Tollison (1995) measure could accurately indicate a competitive state or simply be picking up a true party preference shift in a state that comes to be a stable trend in the long run.

Third, traditional measures weigh all states based on their electoral worth. This makes states with large electoral votes, like California and Texas, appear to have great importance even if the competitiveness within the state is relatively low.

[11] The exception to this is 2000 presidential election that was won by less than 6,000 votes in Wisconsin.

Campaign Behavior

Candidates make decisions based on pre-election conditions, and those decisions affect voting outcomes. Evidence to support this can be found widely across political science literature. According to Herr (2002), candidate appearances in states within five weeks of the 1996 presidential campaign secured victory for President Clinton. Holbrook (2002) studies the whistle-stop campaigns of the 1948 election and finds that Truman's tireless campaign methods clinched the race for him, especially with regard to Ohio. There are many other studies that show that pre-election actions affect the presidential election outcomes, but it is beyond the scope of this paper to review that literature. Because the actions of candidates influence election victory margins as much as those margins influence the candidates themselves, final vote count is not the optimal dependent variable. The final vote count contains information about victory margin, but in tight races, it contains more information about the quality of campaigning.

Ex Ante Closeness Measure for the 2004 Presidential Race: Method 1

Since measuring the *ex ante* "closeness" of a presidential race within a state is not as simple as using the final vote count, I use information available to campaigns *prior to* the 2004 presidential election: poll results. The data were collected from the USA Election Atlas website. The major polls include those by Survey USA, Capital Survey Research Center, Gallup, Mason-Dixon, Rasmussen, Strategic Vision and any major university poll within a state (USA

Election Atlas)[12]. Poll data are an ideal measure for this research because poll numbers are carefully assembled market ventures. The Gallup Organization alone has over 40 offices in 27 different countries (Gallup). Using these polls is an obvious way to capture information the market finds important and upon which candidates are likely to base decisions.

The US presidential election of 2004 between Republican incumbent George W. Bush and Democratic nominee John Kerry ended with Bush winning 286 electoral votes, carrying 50.73 percent of the popular votes and Kerry winning 251 electoral votes and 48.27 percent of the popular vote (US Election Atlas). The *ex post* measures suggest that the race was close, but the campaigns did not have this information during campaigning. The political science literature has much to say about *ex ante* measures of closeness. In particular, the beliefs about the usefulness of poll data are varied. Most of the literature agrees that poll data contains important information about the way people vote, but are flawed due to the gap in time between polling and the basic measurement issues that always plague survey data (Gelman and King 1993; Roll 1982). The literature disagrees about the reliability of poll data due to the latter issue. However, political scientists do find evidence that polls affect candidate behavior, therefore I utilize poll data as a component of *ex ante* closeness.

[12] See the complete list of polls in the Appendix.

Since polls are widely publicized, it seems logical that campaigns have easy access to this information. Specifically, I focus on how polls change. Changes in the polls, which will be referred to as flips, are counted based on the initial poll within a state. For instance, if the first poll within a state had Bush carrying the majority of the votes, the first poll in the timeline that had Kerry carrying the majority would be counted as one "flip." Kerry is winning in the next three polls but then Bush is winning again in the fourth poll count as two flips in this state. On many occasions, the polls result in a tie. Ties are not considered flips but are counted as a separate phenomenon. So, if the first three polls go Bush, tie and Bush, there are no flips and one tie within that state.

One can see from Table 3.1 that some state polls had no flips at all while the maximum, Florida, had 13 flips. Another revealing statistic is the number of polls within a state. One can see from the summary statistics that some states, like Alaska, had as few as one poll. Pennsylvania had the most polls conducted at 61 throughout the election period.

Table 3.1 Summary Statistics for Method 1

Variable	Mean	Std. Dev.	Min	Max
FLIPS	1.47	2.96	0	13
TIES	0.92	1.78	0	8
POLLS	16.43	15.01	1	61

Total Observations: 50

Source: USA Election Atlas

Though the number of polls is an indication of the interest within a state, it is only a vague indication of the "closeness" within a state because polling is a business. Thus, the number of polls within a state is an indication of the market interest within a state, which would certainly be higher in states that are close but may be high in other states for other reasons. For instance, Michigan was polled 12 times with Kerry squarely winning every single poll, while Alaska was only polled once with Bush squarely winning. So the number of polls is not as strong of an indication as the number of times the polls flipped within a state. Table 3.2 contains a frequency table for the number of flips, and one can see that the majority of states had zero flips. Table 3.2 shows the frequency of flips and ties together, which is referred to as "changes."

Table 3.2 Frequency of Flips and Changes

Flips	Freq.	Percent	Cum.
0	33	64	64
1	7	14	78
2	1	2	80
3	2	4	84
4	1	2	86
5	2	4	90
6	1	2	92
7	1	2	94
10	2	4	98
13	1	2	100

Changes	Freq.	Percent	Cum.
0	30	60	60
1	4	8	68
2	4	8	76
4	1	2	68
5	3	6	84
6	1	2	86
9	2	4	90
10	1	2	92
11	2	4	96
17	1	2	98
18	1	2	100

Total Observations: 50
Source: USA Election Atlas

Taking the information about flips and changes, I can construct an estimation of state importance in one of two ways. First, I capture state importance based on flips and ties as follows:

$$Importance_i = f(FLIPS_i, TIES_i) \qquad (3.1)$$

This means that the importance of state i is a function of the poll flips and ties in state i. However, I can also focus on changes, which essentially gives ties the same impact as flips:

$$\text{Importance}_i = f(\text{CHANGES}_i) \qquad (3.2)$$

This method means that the importance of state i depends on flips and ties. Taking either Equation 3.1 or Equation 3.2 as the right-hand side of an equation, I can measure correlation between state-level attributes that may be dependent on campaign importance. For example, I can put federal spending in each state on the left-hand side of the equation, and find the correlation between state campaign importance and federal fund distribution.

Ex Ante Closeness Measure for the 2004 Presidential Race: Method 2

Though changes in polls within a state yield important information, behavior of campaigns further reveals how prized each state is. Consider the use of campaign resources. There is a rich political science literature concerning the allocation of campaign resources. In particular, the *relative* amount of money that is spent in an area affects elections outcomes (Meltzer and Vellrath 1975; Jackson 1997). Data concerning the amount of money spent on TV advertising by campaign comes from FairVote, an independent research organization. The scope of this data is limited to September 26, 2004 until November 2, 2004. FairVote collected this data from the archives of cnn.com.

Another measure is the number of visits a candidate pays to each state. Again, this data is reported at cnn.com and aggregated by FairVote. Though this data is compelling in regard to the “closeness” of a state, it cannot stand alone. As Colantoni, Levesque and Ordeshook (1975) point out, a visit to New York City surely impacts voters in northern New Jersey. Incorporating television ad data into the closeness measure without accounting for secondary effects is an attempt to capture the large primary effects of in-state spending. This is a result of an implicit assumption that if a candidate makes decisions based on the primary effect, not the secondary spillover effects.

The number of visits to each state is another aspect of the closeness measure. FairVote also reports the number of visits to each state by the presidential and vice presidential candidates from September 26, 2004 until November 2, 2004. Again, this data are aggregated from cnn.com. This does not include visits to home states, as it is difficult to distinguish home state visits between personal and campaign motives. The primary effect is the main effect being captured in this data.

This more sophisticated importance measure includes: campaign money spent by state, TV ads per state, campaign visits per state, poll results by state, and electoral votes per state. I assume that presidential election operatives maximize electoral votes subject to their resources. I view campaigns as “black boxes” that optimize in unseen ways but are rational nonetheless. Thus, I can use the allocation of resources to reveal campaigns’ perceptions of critical state elections. As a result of this assumption, states that receive no campaign money or visits are essentially ignored. A basic implication of these assumptions yields the following functional form:

$$Importance_i = f(MON_i, VISITS_i, POLLS_i, EV_i \mid MON_i \neq 0). \qquad (3.3)$$

The importance of state i to political campaigns is a function of the campaign money spent (MON), the number of visits by the candidates (VISITS), polling results (POLL) and the electoral worth (EV), given that any money is spent in the state. Electoral worth only matters when a state receives attention. If a state is worth many electoral votes but is heavily leaning to one candidate, its worth is inconsequential to campaign allocation.

Generally, the importance of a state is used to test incumbent party action. Thus, Table 3.3 contains summary statistics for the 2004 campaign from the Republican campaign perspective.

Table 3.3 Summary Statistics for Method 2

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
MONEY	1,274,389	3,287,571	0	1.72E+07
ELECTORAL VOTES	10.7	9.618	3	55
POLL	14.64	13.785	0	53
VISITS	2.84	6.303	0	28

Total Observations: 50
Sources: USA Election Atlas, FairVote

One key difference between Method 1 and Method 2 is that Method 2 is party dependent. Thus the poll measure becomes very specific: the absolute value of the number of polls won by Bush minus the number of polls won by Kerry in each state.

Comparisons with Past Measures of Closeness and Caveats

I regressed the components of the Equation 3.3 on the closeness measure in Grier, McDonald and Tollison (1995) and found only a weak correlation. Specifically, the adjusted R-squared is about .18, with only money and visits having any significance. The results of this regression can be found in the Appendix, Table 5.7. This implies that my method of campaign importance is different from past literature. With this method of measuring importance or closeness within a state, economists can examine incumbent behavior toward specific states or areas based pre-election data. Since this accounts for information available to campaigns during the election, it provides a richer behavioral source of information that approximates the information used to make campaign decisions.

One obvious drawback to this method is the data intensiveness. Also, this level of information is not available throughout U.S. presidential history. Because this is a state level analysis, often the number of observations will be limited to around 50 assuming only one campaign is analyzed. Another issue that may arise when implementing Method 2 is collinearly. Table 3.4 contains a summary of simple correlations between the variables used in Method 2. The money spent on TV advertising by Republicans, visits made by Republicans and the electoral worth given Republicans gave any attention to a state are highly correlated.

Table 3.4 Correlation of Variables Used in Method 2

Variable	MON	VISIT	EV*REP
MON	1		
VISIT	0.88	1	
EV*REP	0.72	0.67	1
POLL	0.29	0.21	0.33

50 Observations

Measuring *ex ante* importance differs from the literature in three main ways. First, I do not create a metric, but let estimations fit the data. Secondly, I use *ex ante* measures that would be available to the campaigns. Lastly, electoral worth only enters the estimation if any money is spent within a state. This prevents large electoral vote value from increasing the importance of states that have many electoral votes but receive little attention from campaigns.

CHAPTER FOUR

UTILIZING EX ANTE ELECTORAL CLOSENESS

Introduction

To demonstrate the *ex ante* electoral competition, I test the hypothesis that the Bush Administration deactivated reserve troops in states that may have impacted Bush's reelection in 2004 at the margin. I use the *ex ante* measures of expected electoral vote from Chapter 3 to quantify campaigns' perceived vote competition across individual states. This close wartime election gave the Bush Administration great incentive to treat wartime policy with an eye to win the presidency again. This research question is the type of question that an *ex ante* electoral closeness measure is designed to address. This paper follows in the spirit of Peltzman (1976) and Stigler (1971) in that it views political actions through the lens of political economy. Politicians are working in their own best interests, not trying to correct market failures.

I am not the first to study the troop allocation with respect to voting. Anderson and Tollison (1991) find statistical evidence that during the Civil War, President Lincoln strategically sent particular units into war with the intent of capturing votes. In my case, the support garnered from the return of troops could come from two sources: the families and reservists themselves feeling grateful for the return, coupled with local news reporting on troops returning home, sparking positive responses from non-military related people within a state. Though Anderson and Tollison (1991) studied actual soldiers voting, current election standards do not dictate that a person has to be physically at an election to cast a vote, so that mechanism would not operate here. Anderson and Tollison (1991) relied on Abraham Lincoln's integral involvement in placing troops. While there is little evidence that Bush is involved in the day-to-day actions of the United States military, it is well known that cabinet members, including the Secretary of State and the Secretary of Defense, are appointed by the president and would have incentive to keep Bush in office. The mechanism for strategically deactivating troops is unknown; thus I rely on correlating outcomes and not modeling reserve movements.

As in any close contest, the actions of either side may change the balance. The Bush Administration may have brought home reserve troops in key states to garner votes of reservists and reservists' families and communities. The mechanism operates through voters gaining more support for the war, which was a major issue in the campaign. Voters saw troops returning largely unharmed, and troops were generally in support of the war (Hodierne 2004).

Model and Results: Method 1

The basic theoretical model takes the following form:

$$\text{Reserve Mobilization}_{i,\,04} = f(\text{Importance}_i) \qquad (4.1)$$

The reserve mobilization in state i is a function of state i's importance in the presidential election. As discussed in Chapter 3, Importance can be measured using one of two methods: poll results only or poll results combined with campaign resource allocation. Using the simplest estimation of state importance, I estimate the following equation:

$$\text{Reserve Mobilization}_{i,\,04} =$$

$$\beta_0 + \beta_1\,\text{FLIPS}_i + \beta_2\,\text{FLIPS}_i^2 + \beta_3\,\text{TIES}_i + \varepsilon_i. \qquad (4.2)$$

where flips are the number of party lead changes in the polls in state i, and ties are the number of polls in state i that were too close to call. The dependent variable is the difference between the number of mobilized reserves in October 2004 and the number of reserves mobilized in January 2004 in state i. The construction of the dependent variable makes it easy to think about the variable being positive if more troops left a state than returned to state and negative if more troops returned to a state than left a state. This seems like a logical time span because elections take place the first Tuesday after the first Monday in November. Thus, any “feel-good points” Bush might receive from bringing home troops would accrue prior to November. The underlying assumption is that action must happen near Election Day in order to have an impact on Election Day.

The state level troop mobilization data is found at the Department of Defense website monthly press releases aggregate the data by state. Election data is gathered from the USA Election Atlas. The polls are pre-election polls that happen between the dates March 23, 2004 and November 2, 2004 because it spans the time from when Kerry essentially became the Democratic candidate (with John Edwards’ withdrawal) until Election Day. The poll data includes polls conducted by research firms, news outlets and universities. See the Appendix for a comprehensive list of polls. TV advertising spending and campaign visit data come from FairVote, an independent research organization. FairVote data span the time period September 26, 2004 until November 2, 2004 and were collected from the archives of cnn.com.

The main coefficients of interest for Equation 4.2 are β_1 and β_2 because one would expect that if the Bush Administration were to bring home reservists to garner any type of political support, it would be for states that are close, which would include states with larger flip counts. If the Bush Administration was indeed bringing mobilized troops home in close states, β_1 is expected to be positive, and β_2 would be negative. Also, β_3 is likely to be negative as ties are more likely in states with tight races.

Table 4.1 displays the summary statistics for all variables used in this study. Some of this information is discussed in Chapter 3. However, one can see that by 2003, mobilized reserves represented all 50 states. I include registered voters and population because, as discussed below, these may be important control variables depending on the nature of the research question. FairVote reports populations from the Census Bureau and registered voter data comes from Statemaster (2004). The summary statistics show that on average fewer troops were deactivated between September and October 2003 compared to the troops that were mobilized between September and October 2004. Also, the number of reservists activated varies greatly by state. For instance in September

2003, Alaska had the fewest reservits activated with 35 and California had the most reservists activated with 6,487

Table 4.1 State Level Summary Statistics.

Variable	Mean	Std. Dev.	Min	Max
MON	$1,274,609.00	$3,287,664.00	0	1.72E+07
VISIT	2.78	6.32	0	28
FLIPS	1.5	2.98	0	13
TIES	0.94	1.79	0	8
CHANGES	2.44	4.44	0	18
EV*REP	4.34	7.54	0	32
POPULATION	5,846,252	6.50E+06	502,816	3.56E+07
REGISTERED VOTERS	2,835,580	2.79E+06	265,000	1.42E+07
POLL	11.1	9.12	1	38
RESERVES OCT 2004	2,859.34	2,072.44	275	8,976
RESERVES SEP 2004	2,678.96	1,913.99	291	8,716
RESERVES JAN 2004	3,122.46	2,118.10	214	8,013
RESERVES SEP 2003	2,406.72	1,701.10	35	6,487
RESERVES OCT 2003	2,329.64	1,705.58	35	6,439
RESERVE MOBILIZATION 04	-263.12	1,366.73	-3,667	2,616
RES04	180.38	373.72	-278	1,811
RES03	-77.08	189.53	-890	543
RES04-RES03	257.46	469.72	-690	1,859

50 Observations

Sources: FairVote, Department of Defense Monthly Press Releases, and StateMaster Registered Voters 2004

Table 4.2 contains the estimations of Equation 4.2. The most striking part of the estimation is that the constant is negative across the estimations. This indicates that at the average, more troops returned home than left (this general troop return can be seen in the summary statistics as well). The estimates have little power. Column 1 shows that for every Flip, about 138 reservists returned.

Combining this with the negative constant, it can be thought of as each flip in state polls correlates to 138 troops returned over and above the average, non-flip state experienced. Though some of the coefficients are statistically different from zero, there is little indication of stability. Column 3 shows expected signs for all variables, but once controlling for population, Column 4, the sign on ties change. The Adjusted R squares are very low. Taking the estimates in Table 4.1 as a whole, there is little indication that electoral importance had any impact on reserve deployment.

Table 4.2 Method 1 Estimation, Dependent Variable Reserve Mobilization 04

Variable	**(1)**	**(2)**	**(3)**	**(4)**
FLIPS	-138.54 **	140.69	207.14	262.1
	[62.997]	[187.34]	[232.752]	[236.85]
FLIPS_SQ		-28.24	-31.6	-38.22 *
		[17.88]	[31.602]	[20.07]
TIES			-77.8	88.831
			[77.803]	[158.99]
POP				0.035
				[0.0305]
CONSTANT	-55.31	-164.04	-153.7	-357.92
Adjusted R Squared	0.07	0.085	0.085	0.09
Obs	50	50	50	50

*Significant at 10%, ** Significant at 5%, *** Significant at 1%

Standard Errors in Brackets

Table 4.3 repeats the same regression, except it combines the number of flips and ties within each state.

Table 4.3 Method 1 Combining Ties and Flips

Variable	(1)	(2)	(3)
CHAGES	-88.108 **	129.35	150.64
	[42.56]	[121.74]	[123.36]
CHAGES_SQ		-15.598 *	-17.701 *
		[8.21]	[8.45]
POP			-363.33 ***
			[0.03]
Constant	-48.134	-184.41	-363.33
Adjusted R Squared	0.06	0.11	0.11
OBS	50	50	50

*Significant at 10%, ** Significant at 5%, *** Significant at 1%

Standard Errors in Brackets

These results give little indication that the Bush Administration strategically deactivated reservists in states that were closely contested in the 2004 presidential election. The simplest regression, Column 1, shows that 88 troops returned for each Change. Weak Adjusted R Squared values and low statistical significance show some evidence, but it is not conclusive or causal. The main problem with this method is that using poll data is only a proxy for the states that the candidates thought were closely contested. In reality, candidates make strategic moves based on what they perceive to be the swing states. A more accurate way to measure the candidate's perceived closeness within a state would be campaign resource allocation.

Model and Results: Method 2

The core of my empirical model is that the number of troops returning to a state depends on the importance of that state in the 2004 presidential election. I characterize this using the following functional form:

$RES_{i,04} =$

$$\beta_0 + \beta_1*MON_i + \beta_2*MON^2_i + \beta_3*(EV*REP)_i + \beta_4*POP_i + \beta_5*POLL_i + \varepsilon_{i,04} \quad (4.3)$$

The difference in troops deployed between September and October 2004 ($RES_{i,04}$) is a function of the money Republicans spent in state i (MON), state electoral votes multiplied by a dummy variable if Republicans spent or visited state i (EV*REP), and a measure of polling results in state i (POLL)[13]. The error is state-time specific ($\varepsilon_{i,04}$). I include state population because I do not include the electoral worth of each state, one could use registered voters instead of population. The population measure is intended to control for state size differences that may influence campaigning beyond the electoral votes. I do not include visits in the empirical model because money spent is correlated so highly with visits that co-linearity becomes an issue.

However, it is not enough to simply estimate the returning troops in 2004. There are ongoing deployments and returns of reservists. Thus, the appropriate measure of troops is the number of troops returning *above and beyond* the number of troops that would have returned if the election were not happening. I use 2003 as a control year to create a difference in difference dependent variable. I estimate the number of troops returning in 2003 with the following equation:

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[13] The poll measure is the absolute value of the difference between polls with Kerry winning minus the polls with Bush winning.

$$RES_{i,03} = \lambda + \varepsilon_{i03} \quad (4.4)$$

The difference in troops deployed between September and October 2003 (RES_{03}) is a function of the state-time specific error (ε_{i03}) and a constant[14].

Combining Equations 4.3 and 4.4, I estimate the following equation:

$$(RES_{04} – RES_{03})_i = \mu + \beta_1*MON_i + \beta_2*MON^2_i + \beta_3*(EV*REP)_i + \beta_4*POP_i + \beta_5*POLL_i + (\varepsilon_{i,04} - \varepsilon_{i,03}) \quad (4.5)$$

A positive coefficient indicates that more troops were deployed (or fewer troops returned) between October and September in 2004 than 2003[15]. A negative coefficient indicates more troops returned (or fewer were deployed) between October and September in 2004 than 2003. I expect β_1 to be positive and β_2 to be negative. This would indicate that money has an exponential effect: higher amounts of money spent correspond to more returned troops (or fewer deployed). I expect β_5 to be negative. The closer the poll value is to zero, the less competitive the state. This indicates that the less competitive polls, the more troops are deployed.

Table 4.4 contains results of the OLS estimations. The signs are as predicted except for the coefficient on the poll measure. The coefficient is not significant at any reasonable level. Columns 1 and 3 are plagued by

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[14] Since I do not know the deployment mechanism, I assume that troops are called up randomly.

[15] If RES_{04} is positive, it means that more reservists were mobilized in October 2004 than September 2004. This means more reservists were leaving during October. If RES_{04} is greater than RES_{03}, it indicates that more reservists were leaving in 2004 than in 2003. This positive coefficients indicate more reserves mobilized in 2004 than 2003, and negative coefficients indicate few reserves mobilized in 2004 than 2003.

heteroscedasticity. Even using campaign resources to measure electoral importance does not yield any consistent results. This does not support the hypothesis that the Bush Administration brought home troops to garner votes.

Table 4.4 Method 2, All Variables from Equation 4.5

Variable	**(1)**	**(2)**	**(3)**
MON	7.16E-06	2.30E-05	8.86E-05
	[2.00E-05]	[6.00E-05]	[7.57E-05]
MON^2		-1.21E-12	-1.64E-12
		[5.54E-12]	[4.70E-12]
EV*REP			-5.46
			13.51
POLL			
POP			
Constant	248.329	241.9407	251.9187
Adjusted R Squared	-0.01	-0.03	-0.05
Observations	50	50	50

*Significant at 10%, ** Significant at 5%, *** Significant at 1%

Standard Errors in Brackets

Table 4.4 Method 2, All Variables from Equation 4.5 (Continued)

Variable	(4)		(5)		(6)	
MON	1.01E-04		2.28E-07		8.03E-05	
	[6.36E-05]		[7.21E-05]		[6.69E-05]	
MON^2	-5.81E-12		5.62E-13		-4.55E-12	
	[3.96E-12]		[4.46E-12]		[4.15E-12]	
EV*REP	-25.47	**	-10.68		-25.15	**
	[11.87]		[12.78]		[11.88]	
POLL			21.06	***	7.60	
			[7.65]		[7.61]	
POP	4.73E-05	***			4.21E-05	***
	[9.86E-06]				[1.12E-05}	
Constant	33.47473		62.93901		-10.63339	
Adjusted R Squared	0.28		0.07		0.28	
Observations	50		50		50	

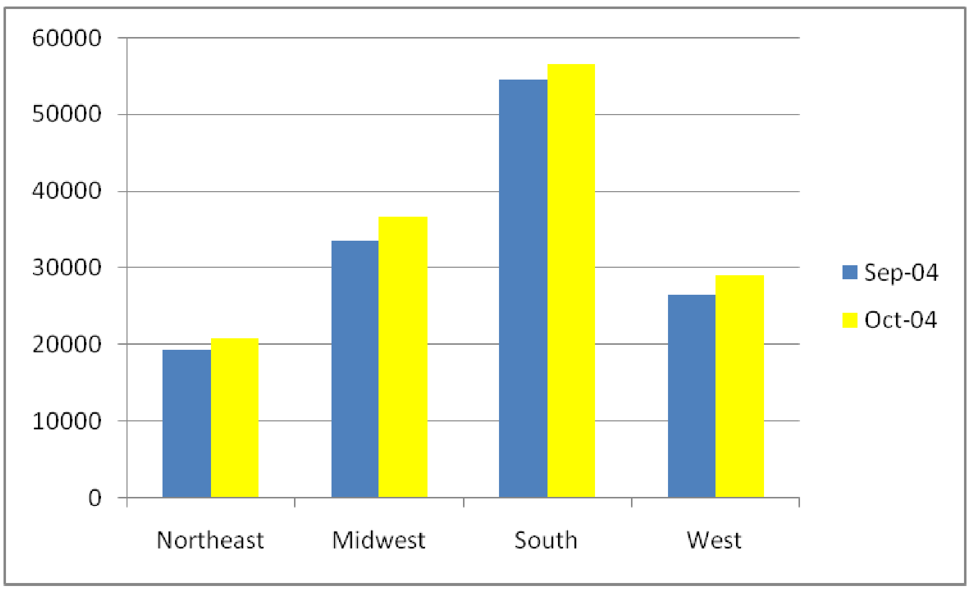
*Significant at 10%, ** Significant at 5%, *** Significant at 1%

Standard Errors in Brackets

These weak results may be a result of poor comparative time frames. For example, there were 26,300 more reserves mobilized in October 2004 than October 2003. If this is a reflection of changing reserve mobilization schemes, the differenced dependent variable may be inappropriate. Another possible reason for weak results is that the underlying deployment mechanism is unknown. Thus, the constructed dependent variable may be a poor approximation of troop levels changes beyond what would have been without the election. To see other variations of these importance components, see the Appendix Table 5.8.

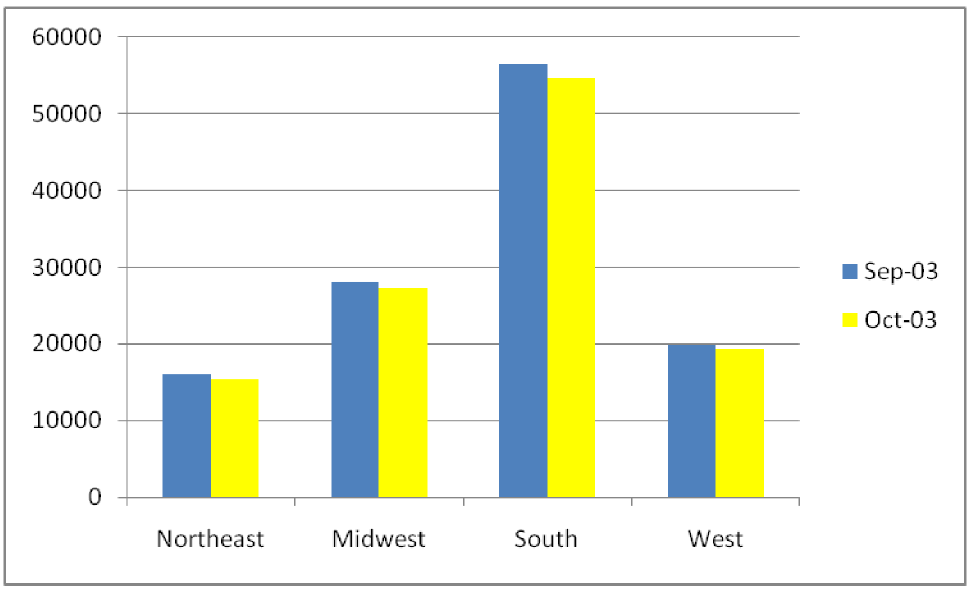
Because I am not modeling the mobilization mechanism and cannot prove causation, there may be a problem with mobilization mirroring population make up. To help address these concerns Figures 4.1 and 4.2 show Census region reserve mobilization. Figure 4.1 shows September and October 2004. During this time, every region had more reserves mobilized in October 2004 than September 2004. More reserves were mobilized from the South than any other region and the Northeast had the fewest reserves mobilized. These patterns are likely due to the traditionally strong Army recruiting in the South.

Figure 4.1 Regional Reserves Mobilized September and October 2004



Comparing the two time frames, the constructed dependent variable problem becomes clear. In 2004 more reserves were mobilized in October than September, but in 2003 the opposite is true. This suggests that subtracting the differences may not be the appropriate way to capture "extra" mobilization as a result of the presidential campaign. See the Appendix Figures 5.1 and 5.2 for Census Division reserve mobilization. The Census Divisions follow the same pattern as the Census Regions they compose.

Figure 4.2 Regional Reserves Mobilized September and October 2003



Causation

Finding a link between pre-election action and state electoral closeness requires stable significant results across these regressions. Specifically, proper signs combined with statistical significant coefficients throughout different regression techniques are required to begin drawing conclusions about pre-election behavior. Even with significant results, two aspects of the data must be verified for these results using this method of state electoral importance to be causal. First, the dependent variable must be uncorrelated with anything else that might be driving the result. For instance, if the dependent variable is correlated with the state population make up, it could be proxying something else. Secondly, the independent variables cannot be correlated with the error term. In the case of electoral importance, if there is error correlation, then this is merely a correlation estimate and cannot be called causal.

Conclusions

This study demonstrates how to implement the *ex ante* electoral closeness measure explained in Chapter 3. Several difficult problems that researchers face when using *ex ante* measures of electoral competition. First, the timing of political action (or suspected political action), will be a matter of researcher discretion. The assumption of this study is that voters are affected by actions that take place in the month before Election Day. However, this may not be appropriate for all research questions. Secondly, proving causation may be difficult and causal estimates are evidence at best. One last issue concerning the *ex ante* measures of electoral closeness is that data may be scare for historical races.

There is no evidence concerning reserve troop deployment changes with respect to state electoral competition. However, this method is useful in testing other campaign behavior with respect to state level correlation. The underlying public choice theory combined with the *ex ante* electoral closeness measure allows researchers to test a number of campaign and political theories.

Specifically, federal allocation of funds to states, federal level voting records in regard to state issues, and regulations that impact states differently are just a few example topics that can be explored using the *ex ante* electoral closeness methods.

CHAPTER FIVE

CONCLUSION

The essays in my dissertation explore the impact of political action on everyday life by utilizing political economy theory and investigating unintended consequences. The first essay studies the impact on communities when the Army activates reservists and guardsmen. The second essay challenges traditional ways of measuring electoral closeness. The third essay demonstrates the new measure by asking, “Did the Bush Administration bring home troops to bolster campaign support in 2004?”

I find that communities bear costs of activated reserves beyond the labor market in form of longer emergency response times. The estimates at the average vary from OLS estimates of 26 seconds to just over 2 seconds. These estimates are underestimates because the state-level data do not allow me to connect activated reserves to exact accidents. Even with the state level data, the average response times decrease as activated reserves increase. Continuing research about community-level costs of activating troops can help make this picture complete. For instance, connecting reservists to cities and counties can be done with time. Once these data are acquired, city and county-level accident data can better estimate the costs of activation. Also, the state-level reservist data can help detect other costs to communities. This will help identify the actual marginal cost of activating troops and contribute a better understanding of the consequences of activating reserves.

The second essay finds an *ex ante* method of valuing states during a presidential election. I do not create a metric, but let estimations fit the data. Secondly, I use measures *ex ante* to the election that would be available to the campaigns during the election. Lastly, electoral worth only enters the estimation if any money is spent within a state. Comparing this method to traditional measures of electoral importance finds the two are not compatible. My *ex ante* method of estimating election closeness can be used to better understand the politics of elections.

The third essay exhibits the methods in the second essay to answer the question, “Did the Bush Administration bring home troops to win in 2004?” While there is little evidence that reservists were strategically deactivated, this study demonstrates the possible pitfalls of using this method. First, results cannot be seen as causal without proper verification. Researchers must verify that dependent variables are not approximating any other state level attribute. In particular, political action may be approximating some state level population make up. Secondly, there are no standard time frames to assess political action on election outcomes. Thus, the researcher must use his or her discretion when constructing dependent variables and including independent variables. Future research on the specific topic of reserve mobilization requires addressing some of the main problems with these estimations. Mainly, causation must either be established or the true causal link revealed. However, future research concerning the method of measuring electoral closeness can focus on revising past studies that use *ex post* measures of electoral closeness as well as analyzing all types of pre-election political action.

Appendix

Table 5.1 A Demographic Summary of the National Longitudinal Survey of Youth 1997

Variable	Mean	Min	Max
Military	0.049	0	1
Protective Services	0.07	0	1
Female	0.488	0	1
White	0.519	0	1
Black	0.26	0	1
Hispanic	0.212	0	1
Mixed	0.009	0	1
Age as of 12-31-96	13.99	12	16

8,984 Observations

Source: National Longitudinal Survey of Youth 1997 Cohort

Table 5.2 Dependent Variable Logged Response with Random Effects

Variable	1(RE County	2 (RE City)
RESDENS	0.031 ***	0.050 ***
	[0.008]	[0.012]
HW	-0.085 ***	-0.036 ***
	[0.009]	[0.010]
SURF	-0.139 ***	-0.025
	[0.014]	[0.031]
DRY	-0.036 ***	-0.026 **
	[0.008]	[0.013]
DAY	-0.043 ***	-0.040 **
	[0.010]	[0.018]
DARK	-0.001	-0.007
	[0.010]	[0.018]
CLEAR	-0.066 ***	-0.055 **
	[0.014]	[0.026]
RAIN	-0.078 ***	-0.051 *
	[0.016]	[0.029]
SNOW	0.05 **	0.085 **
	[0.019]	[0.036]
WEEKEND	0.031 ***	0.023 ***
	[0.005]	[0.008]
CITY	-0.371 ***	
	[0.012]	
LANE 3-4	-0.125 ***	-0.107 ***
	[0.009]	[0.009]
LANE 5-6	-0.165 ***	-0.124 ***
	[0.020]	[0.017]
LANE 7 & Greater	-0.068 ***	-0.074 ***
	[0.023]	[0.023]
SPEED25-40	-0.032	-0.026
	[0.034]	[0.041]
SPEED45-60	0.161 ***	0.186 ***
	[0.034]	[0.041]
SPEED60 & Greater	0.325 ***	0.323 ***
	[0.036]	[0.044]
Constant	2.355	1.792
OBS	89,220	34,282
R Squared	0.16	0.054
rho	0.142	0.32
Between R Squared	0.242	0.09

*** Significant at 1%; **Significant at 5%; * Significant at 10%

Standard Errors in Brackets

All Columns: Clustered Standard Errors by County

Table 5.3 Dependent Variable Logged Response with Random Effects and Monthly Controls

Variable	1(RE County	2 (RE City)
RESDENS	0.059 ***	0.081 ***
	[0.010]	[0.015]
HW	-0.086 ***	-0.037 ***
	[0.010]	[0.010]
SURF	-0.135 ***	-0.021
	[0.014]	[0.031]
DRY	-0.042 ***	-0.03 **
	[0.008]	[0.013]
DAY	-0.045 ***	-0.042 **
	[0.010]	[0.018]
DARK	0	-0.008
	[0.010]	[0.018]
CLEAR	-0.069 ***	-0.057 **
	[0.014]	[0.026]
RAIN	-0.083 ***	-0.055 *
	[0.016]	[0.028]
SNOW	0.054 ***	0.085 **
	[0.020]	[0.036]
WEEKEND	0.031 ***	0.023 ***
	[0.005]	[0.008]
CITY	-0.371 ***	
	[0.012]	
LANE 3-4	-0.124 ***	-0.106 **
	[0.009]	[0.009]
LANE 5-6	-0.166 ***	-0.122 ***
	[0.020]	[0.018]
LANE 7 & Greater	-0.072 ***	-0.076 ***
	[0.023]	[0.023]
SPEED25-40	-0.032	-0.028 ***
	[0.034]	[0.041]
SPEED45-60	0.161 ***	0.184
	[0.034]	[0.042]
SPEED60 & Greater	0.327 ***	0.321 ***
	[0.036]	[0.044]
Constant	2.332	1.781
OBS	89,220	34,181
R Squared	0.163	0.057
rho	0.124	0.317
Between R Squared	0.25	0.09

*** Significant at 1%; **Significant at 5%; * Significant at 10%

Month Dummy Variables Included, but not reported.
Standard Errors in Brackets
All Columns: Clustered Standard Errors by County

Table 5.4 Results of Hausman Test Comparing Fixed and Random Effects

Ho: Random Effects Estimate is Consistent
Ha: Random Effects Estimates are not Consistent

Tets Result :Chi Squared with 29 Degrees of Freedom: 305.05
Probability > Chi Squared = 0.000

Result: Reject Null Hypothesis

Table 5.5 Dependent Variable Logged Response Time, Expanded Variables

Variable	1(OLS)	2(Cluster)	3(County FE)	4(City FE)
RESDEN	109.084 ***	109.084 ***	18.851 ***	10.512
	[6.035]	[15.454]	[7.224]	[13.303]
HW	-0.156 ***	-0.156 ***	-0.100 ***	-0.036 ***
	[0.006]	[0.012]	[0.006]	[0.010]
SURF	-0.082 ***	-0.082 ***	-0.163 ***	-0.059 *
	[0.011]	[0.017]	[0.012]	[0.031]
DRY	-0.021 **	-0.021 **	-0.037 ***	-0.019
	[0.008]	[0.009]	[0.008]	[0.015]
DAY	-0.037 ***	-0.037 ***	-0.043 ***	-0.037 *
	[0.011]	[0.011]	[0.011]	[0.019]
DARK	-2.66E-04	-2.66E-04	0.002	-0.008
	[0.011]	[0.011]	[0.011]	[0.019]
CLEAR	-0.061 ***	-0.061 ***	-0.062 ***	-0.019
	[0.014]	[0.016]	[0.014]	[0.028]
RAIN	-0.061 ***	-0.061 ***	-0.075 ***	-0.011
	[0.017]	[0.018]	[0.016]	[0.031]
SNOW	0.056 ***	0.056 ***	0.049 **	0.108 ***
	[0.020]	[0.022]	[0.020]	[0.039]
WEEKEND	0.036 ***	0.036 ***	0.030 ***	0.015 **
	[0.004]	[0.005]	[0.004]	[0.007]
CITY	-0.313 ***	-0.313 ***	-0.339 ***	
	[0.005]	[0.013]	[0.006]	
LANE1	-0.101 ***	-0.101 **	-0.038	-0.014
	[0.033]	[0.041]	[0.032]	[0.041]
LANE2	0.046 *	0.046	0.043 *	0.002
	[0.023]	[0.030]	[0.023]	[0.029]
LANE3	-0.088 ***	-0.088 ***	-0.042 *	-0.057
	[0.025]	[0.031]	[0.024]	[0.031]
LANE4	-0.095 ***	-0.095 ***	-0.106 ***	-0.104 ***
	[0.024]	[0.030]	[0.023]	[0.030]
LANE5	-0.148 ***	-0.148 ***	-0.107 ***	-0.060 ***
	[0.032]	[0.041]	[0.031]	[0.039]
LANE6	-0.199 ***	-0.199 ***	-0.123 ***	-0.119 ***
	[0.031]	[0.041]	[0.030]	[0.038]
LANE7	-0.201 ***	-0.201 ***	-0.102 **	-0.157 ***
	[0.046]	[0.047]	[0.045]	[0.058]

Continued on the next page

Table 5.5 Dependent Variable Logged Response Time, Expanded Variables (Continued)

Variable	1(OLS)	2(Cluster)	3(County FE)	4(City FE)
SPEED5	-0.775 ***	-0.775 ***	-0.575 ***	-0.495
	[0.172]	[0.123]	[0.168]	[0.268]
SPEED10	-0.407 ***	-0.407 ***	-0.233 **	-0.092
	[0.114]	[0.115]	[0.109]	[0.165]
SPEED15	-0.424 ***	-0.424 ***	-0.278 ***	-0.059
	[0.060]	[0.077]	[0.058]	[0.088]
SPEED20	-0.472 ***	-0.472 ***	-0.302 ***	-0.071
	[0.044]	[0.061]	[0.043]	[0.065] ***
SPEED25	-0.586 ***	-0.586 ***	-0.377 ***	-0.135
	[0.017]	[0.034]	[0.018]	[0.028] ***
SPEED30	-0.574 ***	-0.574 ***	-0.384 ***	-0.138
	[0.016]	[0.037]	[0.017]	[0.027] ***
SPEED35	-0.481 ***	-0.481 ***	-0.308 ***	-0.118
	[0.014]	[0.033]	[0.015]	[0.026] ***
SPEED40	-0.488 ***	-0.488 ***	-0.296 ***	-0.088
	[0.015]	[0.034]	[0.016]	[0.026]
SPEED45	-0.412 ***	-0.412 ***	-0.248 ***	-0.037
	[0.014]	[0.032]	[0.014]	[0.026]
SPEED50	-0.323 ***	-0.323 ***	-0.171 ***	0.042
	[0.016]	[0.035]	[0.017]	[0.030] ***
SPEED55	-0.211 ***	-0.211 ***	-0.070 ***	0.147
	[0.013]	[0.032]	[0.014]	[0.026] ***
SPEED60	-0.104 ***	-0.104 ***	-0.022	0.130
	[0.016]	[0.035]	[0.017]	[0.031] ***
SPEED65	-0.054 ***	-0.054 ***	0.022	0.251
	[0.014]	[0.031]	[0.015]	[0.029] ***
SPEED70	0.009	0.009	0.078 ***	0.316
	[0.015]	[0.035]	[0.016]	[0.038]
Constant	2.568	2.568	2.557	1.858
Observations	89,220	89,220	89,220	33,966
R Squared	0.176	0.176	0.169	0.065
Between R Squared	-	-	0.233	0.088
Rho	-	-	0.239	0.529

*** Significant at 1%; **Significant at 5%; * Significant at 10%

Standard Errors in Brackets

Table 5.6 List of Polls

List of Polls	
AR University	Mitchell/Detriot News
ARG	Montana State University
Arizona Republic	Moore Information Inc.
Arizona State University	Muhlenberg college
Bellwether Research & Consulting	Multi-Quest
Bennet, Petts and Blumenthal	Norbert College
Bluegrass Poll	Portland Tribune/KOIN
Brown University	Public Opinion Stratedgies
Capital Survey Research Center	Quinnipiac University
Ciruli Associates	Rassmussen
Consumer Logic	Research 2000
Dan Jones	Research and Polling Inc.
Davis Hibbits & Midghall	Riley Research
Eagleton-Rutgers	RKM Research and Communications
EPIC/MRI	Rocky Mountain
Fairleigh Dickinson University	Scripps Research Center
Field Research Corp	Selzer & Co
Fox News	Sienna Research Institute
Franklin and Marshal College	SMS Research
Franklin Pierce	Southern Media & Opinion Research
Gallup	Strategic Marketing Services
Gonzales Research	Strategic Vision
Hamiliton Beattie & Staff	Suffolk University
Humphrey Institute	Survey USA
Insider Advantage	Terrance Group
LA Times	Umass Poll
Lake Snell Perry	University of AR
List of Polls	University of Cincinnati
Marist College	University of Connecticut
Market Research Insight	University of New Hampshire
Market Shares Corp	University of Southern AL
Mason Dixon	University of Wisconsin
Merrimack College	West Chester University
Minnesota State University Moorhead	Wilson Research Strategies
Miss State U	Zogby

Table 5.7 Comparing Method 2 with Traditional Measures

Variable	(1)	
MON	3.10E-05	***
	[1.08E-05]	
VISIT	-10.86248	**
	[5.30]	
POLL	1.615341	
	[1.14]	
EV*REP	0.8788778	
	[2.79]	
CONS	1.03518	
Adj. R Squared	0.178	
Obs	50	

*** Significant at 1%; **Significant at 5%

Standard Errors in Brackets

Table 5.8 Other Results Related to Method 2

Variable	(1)	(2)	(3)	(4)
MON				
MON^2				
EV*REP	0.123	-15.80 *	-8.36	-17.76 **
	[8.99]	[8.33]	[8.89]	[8.34]
POLL			21.05 ***	10.43
			[7.34]	[7.18]
POP		4.41E-05 ***		3.79E-05 ***
		[9.68E-06]		[1.05E-05]
Constant	256.93	68.12	60.06	-2.71
Adjusted R Squared	-0.02	0.227	0.11	0.29
Observations	50	50	50	50

*Significant at 10%, ** Significant at 5%, *** Significant at 1%

Standard Errors in Brackets

Figure 5.1 Census Division Reserves Mobilized September and October 2004

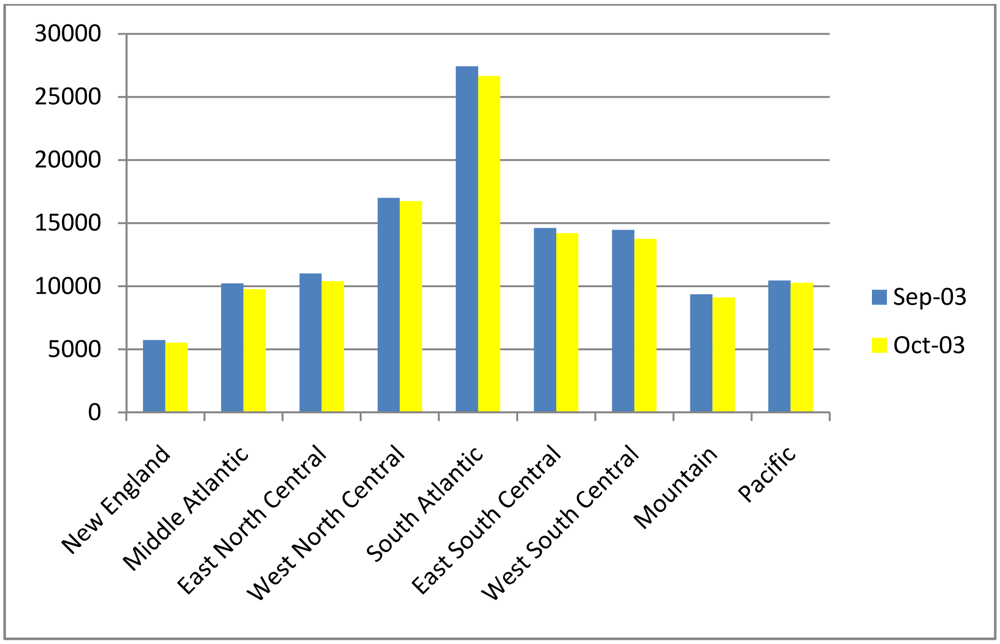
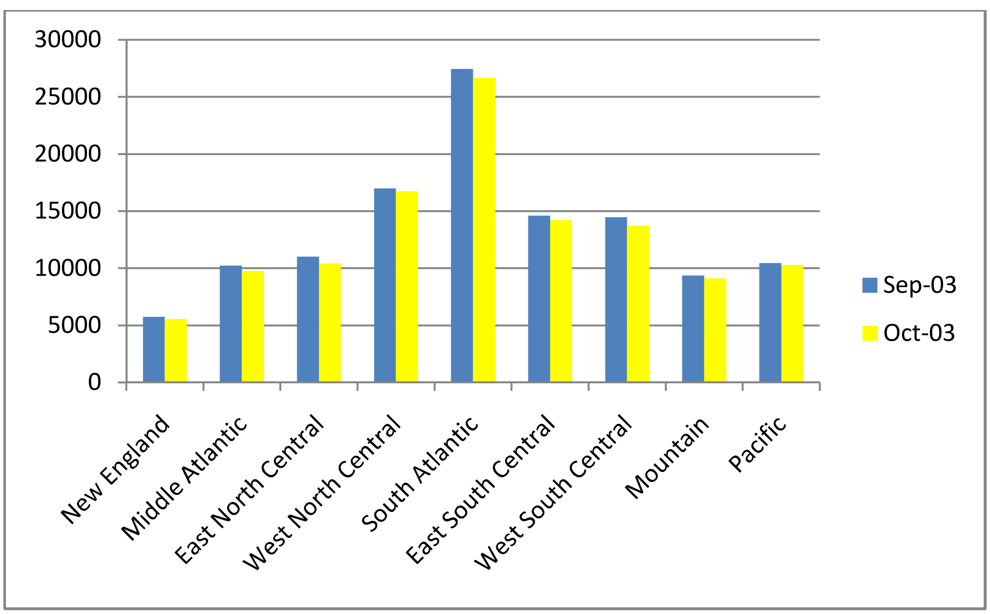


Figure 5.2 Census Division Reserves Mobilized September and October 2003



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Data

All data is available on disk. Contact Clemson University Libraries for more information.