Institutional Effects of Military Deployments and Corporate Leadership Change

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The views expressed in this article are those of the author and do not reflect the official policy or position of the United States Air Force, Department of Defense, or the U.S. Government.

DEDICATION

To those who have gone before.

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ABSTRACT

INSTITUTIONAL EFFECTS OF MILITARY DEPLOYMENTS AND CORPORATE LEADERSHIP CHANGE

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Contemporary economic and business literatures have reached near consensus that national and corporate institutional environments which promote trust, reduce corruption, encourage teamwork, and improve relationships between agencies and individuals generally lay the groundwork for macro and micro-economic growth. It is less clear how such institutions are developed, both nationally and within corporations. This dissertation explores three questions regarding the development and diffusion of corporate, economic, and political institutions.

Within corporations, the consensus appears to be that leadership matters. Numerous studies from widely varying disciplines demonstrate that leadership provided by the CEO impacts how and how well a company performs. While evidence mounts regarding the importance and influence of CEOs, the influence of second-tier leadership appears to be

an open question. This paper begins to address this knowledge gap by seeking to determine which (if any) other corporate officers appear to affect market expectations of corporate performance identified by changes in stock price volatility associated with official management change announcements. Utilizing a new dataset provided by *Liberum's Management Change Database*, I analyze the volatility of daily closing and intra-day stock market prices to (1) confirm established findings that the volatility of equity returns increases when CEOs are replaced with someone hired from outside the company or when a CEO resigns or retires from their position, and (2) find that, other than Directors, no other senior leadership positions generated consistently significant predictions of volatility changes for announcements overall.

Recent work by Jones and Kane identify a robust positive correlation between the natural log of U.S. troops deployed and economic growth among the countries receiving those troops. This paper explores the viability of institutional diffusion as a mechanism driving this growth. Using troop deployment data provided by DMDC and Economic Freedom of the World Index scores, OLS regression with country and regional fixed effects, and controlling for multiple independent variables (internal conflict, foreign aid, and democracy) and their interaction with troop deployments, this study examines the relationship between troop deployments and change in institutional development. Contrary to expectations, results provide very little evidence of institutional diffusion due to U.S. troop deployments and provide some evidence that troop deployments may in fact

hinder institutional development as measured by the Economic Freedom of the World Index.

In Chapter 3, I evaluate the efficacy of using U.S. troops to promote and enforce the transition to and development of democratic institutions. Whereas previous researchers have found limited positive effects of U.S. military intervention on target countries' Polity scores, I use the panel data described above to explore two previously unaddressed questions. First, I use OLS regressions to examine correlations between troop deployments over time (i.e. total troops deployed to each country over a fixed-length assessment window), instead of specific incidents of intervention (e.g. the invasion of Panama to capture Manuel Noriega), and positive change in Polity scores. Second, I identify differing effects of various explanatory variables on change in Polity score given the country's score at the beginning of the assessment period. These results imply that positive movement in the Polity Index does not necessarily equate with progress toward democracy. Therefore, I convert the scale into a categorical dependent variable, and use this categorical dependent variable to test whether troop deployments are correlated with an increased rate of transition to the democratic category, rather than just positive movement along the Polity Scale.

1. WHICH CORPORATE LEADERS MATTER TO FINANCIAL MARKETS?

Introduction

Recent literature demonstrates that "leaders matter." Jones and Olken find that countries' leaders matter for economic growth. Collins finds that CEOs matter in companies' quest to move from *Good to Great*. Adams and Mansi find that CEO turnover is associated with lower bondholder values and higher stockholder values. Some earlier and related literature indicates that corporate leadership changes aren't necessarily associated with changes in stock prices; however, Bonnier and Bruner find that when firms are performing poorly, management change announcements produce significant returns to stockholders. Clayton et al identify increased volatility in equity returns for up to two years following turnover in CEO positions. And Pan, Wang, and Weisbach confirm this finding by identifying a pattern of decreasing stock price volatility as investors update their expectations regarding the abilities of new CEOs. Evidence suggests that countries grow more quickly with better leaders, companies perform better with better leaders, and capital markets recognize that leadership makes a difference.

Up to this point, the vast majority of corporate leadership research has focused on CEOs, Directors, and Chairmen. Management literature has examined CEO decision-making based on education, previous work experience, military service, and demographics. Equity return and volatility event studies examine effects of CEO turnover

under various circumstances. And intellectual capital studies have examined the importance of CEO relationships and organizational capital created by CEO policies and technology use. However, very little has been said about the effects of other corporate leadership. While evidence mounts regarding the importance and influence of CEOs, the influence of second-tier leadership appears to be an open question. This paper begins to address this knowledge gap by seeking to determine which (if any) other corporate officers appear to affect market expectations of corporate performance.

Following methodology similar to that used by Clayton, Hartzell, and Rosenberg (2005) and Pan, Wang, and Weisbach (2014), this study utilizes a new dataset provided by *Liberum's Management Change Database* to (1) confirm established findings regarding equity volatility and CEO turnover and (2) to explore equity return volatility created by turnover of other "Chief" corporate officers. *Liberum*'s database records leadership change announcements on a daily basis from media outlets relating to publicly traded companies beginning in 2004. Analysis of these data confirms that the volatility of equity returns increases when CEOs are replaced with someone hired from outside the company or when a CEO resigns or retires from their position. These results confirm findings of Clayton et al and Pan, Wang, and Weisbach.

Background

While recent literature has demonstrated that leaders matter, and capital markets respond to changes in leadership and leadership practices, there are a significant number of unanswered questions and some contradictory and findings. Early studies produced conflicting findings regarding the sensitivity of the stock market to changes in top

management. Warner, Watts and Wruck (1988) confirmed the efficient market hypothesis and found that while there appeared to be some market fluctuations associated with changes in CEO, president, or board chairmen, the mean change was not significantly different from zero. Bonnier and Bruner (1989) found that stockholders enjoyed significantly positive returns when poorly performing corporations announced changes in top management. Other literature has identified conditions which contribute to stockholder returns associated with leadership change announcements: entrenchment (Salas, 2010), riskiness of firms' debt (Adams and Mansi, 2009), reason for the announced change (Denis and Denis, 1995), and source of new officers (Furtado and Rozeff, 1987).

If CEOs affect the productivity to a firm, their contribution must be a function of the intellectual capital portfolio the CEO provides for the company, i.e., structural, human, and customer capital. This capital comes in the forms of knowledge and experience, existing relationship networks, leadership and business philosophy, and risk or policy preferences which impact the costs and productivity of the firm through implementation of management practices. Principle-agent theory suggests that alignment of incentives throughout the entire organization reduces monitoring, control and transactions costs. However, Collins finds that implementation of better management practices are not sufficient to produce the type of changes which encourage significant corporate growth. If it were easy to reproduce, competitors would be able to do it easily. Rather, Collins finds that getting the "right" people in place first is more important than their initial managerial practices. Thus, the first step in the acquisition of intellectual

capital is not the structural element, but instead the human element. Similarly, Brynjolffson, Hitt, and Yang find that investment in technology is not enough to improve performance, but technology purchases only significantly increase productivity when combined with leadership practices associated with healthy relationships within the firm (2002). Thus, the first step in the development of organizational capital is not the technology element, either, but again, the personal element. And if this is the case, an intriguing question is which people matter most?

Management research has been devoted to determining what characteristics and leadership styles produce the best outcomes. And much of this research focuses on CEOs. CEO values appear to be correlated with the type of culture (innovative vs control, etc.) prevalent throughout companies (Berson, et al, 2008). Giberson, et al (2009) discovered that CEO characteristics are correlated with which type of organizational values develop. Other studies have examined how CEO's cognitive characteristics affect decision making (Calori, Johnson and Sarnin 1994). Others look at demographic characteristics (Goll and Rasheed 2005). Some look at previous work experience, including military service and ethnic origin (Benmelech and Frydman 2014). Regardless of the outcomes of these studies, the consensus must be that the decisions made by leaders matter, otherwise these studies are irrelevant. This study assumes that leaders make rational decisions to optimize the outcomes within the context of their individual intellectual capital portfolios, tangible and intangible, and then seeks to identify which leaders the market is most interested in.

While multiple studies confirm the proliferation of organizational culture created by the CEO, it could be argued that this is achieved by the senior officers selected by the

CEO, since they are the ones more likely to interface and work with a larger percentage of a company's workforce. Thus, the intellectual capital portfolios of other senior leaders may be just as important as the CEO's. Ge, et al (2011), find that individual CFOs have unique patterns in their decision making as they travel from firm to firm, even when working under different CEOs. If this is the case, this demonstrates that different officers carry their own intellectual capital portfolios which impact firms differently. However, for changes in personnel to affect investors' expectations about company profitability, the individuals involved must have sufficient purview and autonomy to affect companywide performance. And while Ge et al confirm that CFO's discretion impacts accounting practices, if their decisions were to negatively affect long-run profitability, one would expect CEOs and boards of directors to step in quickly to mitigate damage. If those practices were to be material to long-run growth of the company, perhaps CFOs would find the level of discretion allowed by CEOs to be decreased. Thus, long-run profitability remains the jurisdiction of the CEO and leaves open the question of whether any senior officers other than the CEO really matter to financial markets. This is the empirical question this study seeks to answer. If CFOs have their own styles, do capital markets care? What about COOs, CIOs, or CTOs?

Data

To address these research questions, this study utilizes data from recorded daily publications of announced changes in corporate leadership positions recorded by Liberum's *Management Change Database* (MCD) from Dec 2004 – Jun 2014. Daily stock data and dividend declaration and payment dates were obtained from CRSP. Intra-

day stock price data for S&P 500 companies were purchased from kibot.com. Quarterly reporting dates were accessed from ExecuComp via Wharton Research Data Services (WRDS). For the purposes of this study, I limited the sample of management changes to S&P 500 and Fortune 500 companies (as of 2013) and focused on senior leadership positions including CEO, President, Chairman, Director, and common leadership positions containing the 'Chief' designation, i.e., Chief Financial Officer, Operations, Information, and Technology. Statistics generated using the intra-day data are limited to the intersection of companies listed in the S&P 500 and Fortune 500 as of 2013.

For each management change, the MCD provides the following data fields: company, date of announcement, date of personnel change, departing personnel, incoming personnel, position title, status code, summary of announcement, and market capitalization of the company. The status code for each entry identifies either the reason for departure or source of incoming personnel. Reasons for departure include: leaving (unidentified reason), retiring or resigning (not differentiated), or terminated (fired). Sources for incoming personnel include: joining (external hire), promotion (from inside the company), or lateral transfer (from inside the company). Separations and hires are recorded in the MCD as separate events, and many change announcements involve multiple departures and hires at the same time. Additionally, the database records whatever announcements are published on a given day, and many publications reiterate previously announced changes. For the purposes of this study, all announcements other than the earliest identified publication of a management change are eliminated. However, announcements for departures and hires are considered independent announcements and

are preserved, even if the announcements are separated by a period of months. Maintaining announcements for both separations and new hires increases the number of announced changes. While this dilutes the effect of any individual announcement as a news event, this also allows us to look at separations and new hires independently. Because duplicate announcements will dilute the effect of a single news event, the significance of any estimated effects of my analysis should represent a lower bound regarding news impacts. Thus, real sensitivities could be greater than reported findings.

Methods

According to the efficient market hypothesis, when new information changes stock price levels or volatility, this information must have changed investors' expectations regarding the company's future stream of income. While there may be conditions which create exceptions, previous research indicates that, in general, stock price levels do not systematically increase or decrease when CEO change announcements are made (Friedman and Singh 1989). However, the circumstances of CEO change announcements predictably influence the volatility of stock prices. An increasingly vast trove of research exists devoted to volatility modeling, built on the influential foundational research by Engle (1982) and Bollerslev (1986). Such works employ and evaluate different models of stochastic volatility, including generalized autoregressive conditional heteroskedasticity models (GARCH/ARCH), and high-frequency intraday data models to predict future market volatility as well as fulfill a series of confirmed stylized facts regarding market volatility. While many studies have focused on which types of models best forecast volatility, the objective of this study is not to construct a

new volatility model or argue for one technique over another, rather to determine the relative impacts of changeover of various management positions using the measurement mechanisms provided by existing volatility literature. With this in mind, existing volatility modeling research provides several methods for evaluating the volatility of stock prices.

At the most basic level, stock prices and volatility can be evaluated based on their realized return. This approach captures proportional stock price changes at a set interval and then evaluates the variance of those changes over a period of time. This simple approach is employed when rough approximations of volatility are sufficient, because data are readily available and computations are not labor intensive. However, because this type of analysis measures stock prices only at certain intervals, it may fail to capture what may have been large price changes during the time period. For example, recording the changes in price from month to month may demonstrate that the price is relatively steady even if the stock price experiences large upswings and equivalent downswings between the recorded measurements. Thus, the longer the interval between measurements, the less resolution is provided by the analysis. If volatility associated with news events is very short lived, measuring price changes even at the daily level may be insufficient to identify news events.

In the past 15-20 years, improvements in computer technology have increased the ability to use high-frequency intra-day data. With stock price data available by the minute or even by the tick, problems with non-synchronous trading data, unmeasured price swings, and estimating appropriate trade price weighting have been mitigated

significantly. However, if the purpose of the analysis is for relative comparison, even with high-frequency trading data, simple realized return calculations don't consider events which might affect a larger portion of securities markets.

One approach which forms the foundation of other models, though demonstrated weaknesses limit its usefulness, is the capital asset pricing model (CAPM). This model is designed to capture relative changes in price, not just the changes of a single stock. CAPM assumes the validity of the efficient market hypothesis, and that current prices reflect the NPV of the stream of future income for the company. Thus, all pertinent information is contained in the market price, and changes in one stock price which differ from the overall market return represent shifts in investors' expectations regarding relative future profits. The CAPM assumes a linear relationship between overall market changes and individual stock price volatility. Relative volatility can be calculated using the variance in residuals generated by the following model:

$$(1) \qquad r_i - r_f = a + b\,(r_m - r_f) + e_i$$

where r_i represents the return on a given stock, r_f is the risk free interest rate, r_m is the average market return over the period, and e is the residual difference in stock price not explained by overall market changes. The resulting measure of idiosyncratic volatility is subject to the same weakness in limited resolution associated with the length of the measurement interval as realized return volatility. The primary reason the CAPM has fallen out of favor is its inability to explain cross-sectional variation in stock returns.

To help explain the cross-sectional variation in stock returns, Fama and French identified several correlates to improve the CAPM model and reduce omitted-variable

bias in resulting measures of idiosyncratic volatility. They find that adding a variable for size and one for the ratio of book equity to market equity capture effects linked to market leverage, book leverage, price-earnings ratio, and size (1973). Thus, their 3-factor model adds two factors to the CAPM: size and book-to-market equity. Idiosyncratic volatility can be evaluated using the residuals resulting from a regression using the following model:

$$(2) \qquad r_i - r_f = a + b(r_m - r_f) + c(SMB) + d(HML) + e_i,$$

where SMB and HML represent the size and book-to-market equity measures. This study uses the daily r_m-r_f, SMB, and HML data published by Fama and French to construct estimates of idiosyncratic volatility for each stock involved in this analysis.

Engle and Ng introduce the News Impact Curve (NIC) to compare the power of a variety of GARCH and non-parametric models (1993). The NIC identifies the relationship between realized news events (price changes) and the subsequent volatility in the market price due to those news events. Engle and Ng show that moderately good news reduces volatility, but bad news and very good news increase volatility. This finding suggests that both reductions and increases in volatility provide information about the market's appreciation of news. Chen and Ghysels (2010) confirmed increasing stock price volatility associated with bad or very good news, while moderately good news can reduce volatility. If volatility increases after a management change announcement, that news may have been bad or very good, but a reduction in volatility following a change announcement conveys useful information as well, since this is indicative of moderately good news easing investor concerns.

To comprehensively search for changes in stock market volatility produced by leadership change announcements, this study utilizes each of the principles outlined above. I analyze realized return volatility using end-of-day prices and 15-minute intraday prices, with and without adjustments to control for market effects identified by Fama and French, and use Wilcoxon signed-rank tests to compare volatility metrics immediately preceding and following management change announcements.

The following sections present the construction and evaluation of several dependent variables and the various results generated by a number of parametric and non-parametric tests. The following sections are designed to accomplish two objectives. Initially, comparison of results from this analysis with existing literature confirm (1) previous authors' conclusions regarding CEO change announcements with this new data set, and the propriety of the volatility techniques I am using to examine management change announcements for other corporate officers, and (2) evaluate the effects of those announcements.

Objective

This analysis explores stock price volatility associated with announcements of changes in senior leadership in large corporations. According to the efficient market hypothesis, changes in stock price volatility associated with leadership change announcements indicate changes in investors' expectations regarding future profitability. Thus, changes in volatility caused by management change announcements indicate whether or not an announcement is expected to impact a corporation's performance. By

identifying if/which officer change announcements affect stock price volatility, we can project which positions the market believes are most influential to company performance.

The following null hypotheses will be considered for common 'Chief' designated positions:

(1) Volatility of equity returns is unaffected by an announced change in a leadership position.

(2) Volatility of equity returns is unaffected by source of replacement personnel.

Two-sided *t*-tests of estimated coefficients from regressions with various volatility indicators are used to identify changes in volatility. When more than half of the tests corroborate a result, I believe this represents a considerable result. Because of the large number of regressions, individual statistically significant cells are likely to be spurious correlations and provide very limited support for any conclusions. Two-sided *z*-tests of Wilcoxon signed-rank results are also used to identify changes in volatility.

Realized Return Volatility (RRV) & Fama-French 3-Factor (FF3F) Model

To examine realized return volatility, I constructed multiple measures of volatility, first with end-of-day closing prices, then with 15-min intraday prices. Percent returns between closing periods were used to construct the following variables: absolute value of percent daily return, square of percent daily return, and the standard deviation of 3-, 5- (one calendar-week), 10- (two-calendar weeks), and 21-day (one month) daily percent returns (daily closing data), and standard deviation of 15 minute prices for one day and the week containing an announcement (15-minute data). Because of the possibility of information leakage prior to official announcement, I also constructed the 3

and 5-day standard deviation variables with the announcements occurring on the day in the middle of the event window. A similar set of variables were constructed using the Fama-French 3-Factor Model (FF3F) residuals described above. Due to concerns with overlapping data, the multi-day statistics are only used for Wilcoxon Signed-Rank tests, which only compare the volatility of the period following the event to the volatility of the window ending immediately prior to the announcement. A sample of the results for the various single-day dependent variables are presented for each of the regressions in each section of analysis.

Testing the Data, Model, And Previous CEO Literature

Table 1-1. CEO Change Announcements Associated with Volatility – Multiple Measures

	Estimated Coefficients *[t-statistics]*					
	Dependent Variables Using End-of-Day Prices			Dependent Variables Using Intra-Day Prices		
	Absolute Value of Daily Pct Change	Absolute Value of FF3F Residuals	Squared Daily FF3F Residuals	Single-Day Standard Dev. 15-Minute Prices	Single-Day Sum Abs. Value of 15-Minute Returns	Single-Day Sum of Squared 15-Minute Returns
Quarterly Report Date	1.585	1.693	10.361	0.283	0.072	-0.012
	[61.10]	*[69.17]*	*[0.43]*	*[82.04]*	*[59.20]*	*[0.11]*
CEO Change Announcement	0.458	0.496	1.619	0.085	0.016	-0.012
	[3.58]	*[4.10]*	*[0.01]*	*[5.13]*	*[2.78]*	*[0.02]*

Table 1 provides the results of six different regressions, each with a different measure of volatility. To mitigate omitted-variable bias, I have included a dummy variable for quarterly earnings statement declarations. May (1971) and Hotchkiss and Strickland (2003) identify increased stock price volatility associated with quarterly earnings statements. Including this dummy should reduce noise within the data and mitigate spurious conclusions if leadership change announcements occur in the same time

period as quarterly reports. Four of six regressions yield statistically significant positive coefficients for both (1) days on which a quarterly earnings report is published and (2) days on which CEO changes are announced. Thus, we observe that CEO change announcements are indeed associated with increased volatility in daily and even weekly measures. However, if poorly performing companies replace senior leadership more frequently than well performing companies, then increased volatility around management change announcements may not be a result of the announcement as much as a cause, and simple OLS regressions using the dependent variables described above and the various management change announcements are likely to lead to spurious conclusions.

Additionally, the results of the regression in Table 1 using the squared end-of-day and 15-minute returns (farthest to the right for each type of data) are surprising. The large t-statistics on the previous regressions for quarterly reporting dates, at least, signal that there may be a problem with the construction of these regressions. This might be indicative of a number of outliers overwhelming effects when they are squared. Three alternative specifications confirm that this is likely to be the case. First, employing a quantile regression minimizes the sum of absolute value of residuals rather than squared residuals, decreasing the influence of large potentially confounding outliers. Regression results generated with quantile regression were more in line with expectations and consistent with the other four regressions. Subsequent tables include results of these quantile regressions. Second, running the previous regressions with a dataset limited to those observations within three standard deviations from the mean produced coefficients

and t-statistics in line with the other four regressions, as well. Descriptive statistics and histograms of the various dependent variables are contained in Appendix II.

The following two tables present the results of mechanisms employed to consider the argument that stock price volatility might drive CEO change rather than result from it. First, to control for poor performance causing management change, I calculated the return on each stock for the period immediately preceding a management change announcement and included a dummy variable for 90-day returns less than zero to indicate poor performance. Table 2 expands the regressions above to include the 90-day dummy for poor performance and an interaction term between poor performance and CEO announcements.

Table 1-1. Volatility from CEO Change Announcements , Adjusting for Poor 90-Day Stock Returns – Multiple Measures (Robust Standard Errors)

	Estimated Coefficients *[t-statistics]*					
	Dep. Var. with End-of-Day Prices			Dep. Var. with Intra-Day Prices		
	Absolute Value of Daily Pct Change	Absolute Value of FF3F Residuals	(Quant Reg) Squared Daily FF3F Residuals	Single-Day Standard Dev. 15-Minute Returns	Single-Day Sum Abs. Value of 15-Minute Returns	(Quant Reg.) Single-Day Sum of Squared 15-Minute Returns
Qtrly Rpt Date	1.6	1.71	2.624	0.00319	0.072	0.0008
	[23.52]	*[24.07]*	*[178.80]*	*[37.14]*	*[38.76]*	*[282.16]*
CEO Change Ancmt	0.383	0.394	0.134	0.000669	0.0128	0.00004
	[2.64]	*[2.80]*	*[1.45]*	*[3.57]*	*[3.76]*	*[2.59]*
Poor 90-Day Perf.	0.545	0.343	0.288	0.00115	0.0258	0.0001
	[38.21]	*[29.65]*	*[79.34]*	*[18.91]*	*[31.56]*	*[142.38]*
CEO*Poor Perf.	0.32	0.35	-0.065	0.000121	0.116	0.00014
	[1.17]	*[1.31]*	*[0.44]*	*[0.38]*	*[1.48]*	*[5.21]*

Table 2 confirms the findings in Table 1. Quarterly reporting days significantly increase volatility in stock prices, as do CEO change announcements. Additionally, it

seems reasonable to conclude that poor performance in the previous 90 days predicts increased volatility in stock prices. The magnitude of the increase in volatility varies depending on which statistic is employed. The mean of the absolute value of the daily FF3F residuals is about 1.6. Thus, the coefficient from CEO change announcements of about .4 indicates a roughly 25% increase from normal daily volatility. The mean of the single-day sum of squared 15-minute returns is .014, so the percentage increase in volatility due to CEO change announcements is considerably smaller, though still statistically significant. Again, a table reflecting the mean, standard deviation, etc., for each of the measures of volatility is included in Appendix II. Positive coefficients from all of the regressions and statistically significant coefficients on five of the six CEO change announcement variables support a conclusion that CEO change announcements contribute to increased volatility in general.

Finally, controlling for fixed effects in regressions mitigates concerns about stock-specific effects, which could bias results one way or another if aggregate correlations are different from the individual effects. For example, if poorly performing companies change CEOs more frequently than well performing companies, then CEO change announcements might be correlated with poor performance even if individual stock prices don't change when announcements are made. While I explored this potential using fixed-effects regressions, differences in results using the fixed-effects regressions were negligible and generated the same conclusions as regressions without controls for fixed effects.

This finding, using a dataset previously unemployed, that CEO change announcements increase stock price volatility regardless of company performance immediately preceding the announcement, confirms and supports a number of other studies, including Pan, Wang, and Weisbach (2014), and Clayton, Hartzell, and Rosenberg (2005).

Another way to mitigate the potential for volatility to be explained by poor performance (or other "company-specific" effects), rather than from the resulting CEO change, is to difference the observed volatility for each stock in the period of time immediately following the announcement with the period of time immediately preceding it. Tables 3 and 4 present the results of Wilcoxon signed-rank tests on differences between various volatility measures from the period immediately preceding and following each announcement. One advantage of this test is that it allows us to look at longer time frames of volatility than the regressions used above, since it eliminates concerns about overlapping time periods.

Table 1-2. Wilcoxon Signed-Rank Tests - Difference in Volatility Subsequent to CEO Change Announcements

	Dep. Var. Using End-of-Day Prices			Dep. Var. Using Intra-Day Prices	
	Absolute Value of Daily Pct Change	3-Day Sum of Abs. Value of Daily FF3F-Residuals	5-Day Sum of Abs. Value of Daily FF3F-Residuals	Single-Day Standard Dev. 15-Minute Prices	Single-Day Sum Abs. Value of 15-Minute Returns
Positive	349	355	348	287	303
Negative	280	274	278	199	183
z-score	3.479	4.56	3.502	4.863	6.593
P-value	0.0005	0.0000	0.0005	0.0000	0.0000

Table 1-3. Wilcoxon Signed-Rank Tests - Difference in Volatility Subsequent to CEO Change Announcements (Extended)

	Dep. Var. Using End-of-Day Prices		Dep. Var. Using Intra-Day Prices
	Two-Week Sum of Abs. Value of Daily FF3F-Residuals	1-Month Sum of Abs. Value of Daily FF3F-Residuals	1-Week Standard Dev. 15-Minute Prices
Positive	326	314	278
Negative	279	285	210
z-score	2.163	0.793	4.408
P-value	0.0306	0.4277	0.0000

Statistically significant z-scores (less than 5%) for all but the longest time period reinforce the findings in the previous table and demonstrate a trend of decreasing volatility over time, consistent with increased volatility coinciding with CEO change announcements.

Clayton, Hartzell, and Rosenberg (2005) also find that incoming CEOs joining from outside the company generate greater volatility in stock prices than CEOs promoted or transferred from inside the company. Table 5 explores the current data set for similar effects.

The mean of the daily sum of squared 15 minute returns is about 0.014. Thus the coefficient on the quarterly report dates indicates a roughly 5 percent increase in volatility on dates declaring quarterly financial results. Announcements of CEOs hired from outside the company appear to increase daily stock price volatility in general by about 1.5 percent, with an additional 2 percent of increased volatility for stocks with negative returns over the last 90 days. Promotions and internal transfers to CEO do not appear to generally increase volatility significantly. However, when internal transfers to CEO are announced for poorly performing stocks, we observe a roughly 1.5 percent increase in volatility as measured by the daily sum of squared 15-minute returns.

Table 1-4. Varying Volatility Effects With Different Sources of Incoming CEO

Quantile Regression Using the Daily Sum of Squared 15 Minute Returns			
	Estimated Coefficients *[t-statistics]*		
Qtrly Rpt Date	0.0007825	0.0007825	0.0007825
	[282.24]	*[282.22]*	*[282.12]*
Poor 90-Day Perf.	0.000099	0.000099	0.000099
	[142.48]	*[142.45]*	*[142.39]*
Outside Hire	0.0002052		
	[4.65]		
Outside*Poor Perf.	0.000314		
	[4.65]		
Promoted from Inside		0.0000123	
		[0.58]	
Promoted*Poor Perf.		0.0000223	
		[0.62]	
Internal Transfer			0.0000445
			[1.21]
Internal* Poor Perf.			0.0002355
			[4.15]

The findings within this section reinforce existing literature by confirming increased stock price volatility associated with CEO change announcements, especially when those announcements come from companies with negative stock returns in the last 90 days or involve CEOs hired from outside the firm. The volatility observed from CEO change announcements appears to arise in addition to volatility due to poor stock price performance and associated with quarterly financial statements.

The following section uses the same techniques, OLS regression, quantile regression, and Wilcoxon signed-rank tests, to evaluate volatility associated with other management change announcements.

Management Change Announcements Other Than CEO

The senior corporate officers other than CEO considered in this study include Directors, Chairmen, Presidents, Chief Financial Officers (CFO), Chief Operating Officers (COO), Chief Information Officers (CIO), and Chief Technology Officers (CTO). In constructing this analysis, two issues necessitated researcher interpretation and manual data manipulation. First, corporate titles published within the MCD are not universal, and many corporations employ chief executives with unique titles and position descriptions. One amusing example is the Chief Chicken Officer employed by KFC in 2011, but more frequently titles such as Vice President – Financial Operations, Chief Accounting Officer, or Financial Controller might be used rather than CFO. Because of the variety of position titles with potentially comparable responsibilities, I utilized only the most common positions specifically designated as "Chief" officer positions. This reduced the number of usable observations considerably. Another solution that would facilitate the utilization of a larger portion of the data contained in the MCD would be to categorize positions according to their hierarchy and function identified on company organizational charts. While this alternative technique might allow for more complete data analysis, I believe the data utilized in this study are sufficient to provide significant insight into the research question. Appendix I lists the number of announcements by office, source of new personnel, and listed reason for departure utilized in this study.

Additionally, many executives are multi-hatted in their positions, for example, serving as CTO and COO, or CFO and CEO, or Director, Chairman, and CEO. For officers in this situation, I reviewed the comments of the published changes and tried to

ascertain which part of their position was changing. For example, in some situations, the current CEO was recently elected to be the Chairman of the Board. Within the MCD this change includes the CEO and Chairman, but counting this as a change in CEO is inappropriate. Thus, each announcement needed to be reviewed to identify on the real changes taking place. For an occurrence such as this example, I manually changed the entry to reflect change in just the Chairman position and not the CEO. However, when someone moves into a Chairman, Director, and CEO position all at the same time, this change is identified in each of the three categories. Where I believe it's reasonable, I have tried to mitigate potential biases from overlapping announcements by including interaction terms to control for multi-hatted positions or filtering data for change announcements which don't overlap.

The second issue requiring manual entry and researcher discretion was the identification of primary versus subsidiary management. A large number of senior leadership announcements contained in the MCD are for senior officers of subsidiary companies rather than the larger overall conglomerate. For example, a number of announcements were made regarding changes for GE's subdivision for Healthcare Information Technology. While a CFO change for a prominent subdivision might be cause for additional stock volatility in GE, it seems less likely that such a change would increase volatility than changing the CFO responsible for the reporting and practices of all the subsidiaries. Thus, after deciding which officers to include in this study, the data were further filtered to only include changes in top tier management, not subsidiaries.

This was accomplished by reviewing the comments section of each entry in the MCD for the positions of interest and selecting only those for the company proper.

Table 6 summarizes results of regressions using the various measures of volatility utilized in the previous tables as the dependent variables with dummy variables for quarterly earnings announcement dates, negative 90-day returns, CEO announcements, and the other officers outlined above.

Table 1-5. Other Management Change Announcements - Multiple Measures of Volatility (Robust Standard Errors)

	Estimated Coefficients *[t-statistics]*					
	End-of-Day Dep.Var.			Intra-Day Dep. Var.		
	Absolute Value of Daily Pct Change	Absolute Value of FF3F Residuals	(Quant. Regression) Squared Daily FF3F Residuals	Single-Day Standard Dev. 15- Minute Prices	Single-Day Sum Abs. Value of 15-Minute Returns	(Quant. Reg.) Single-Day Sum of Squared 15-Minute Returns
Quarterly Report Date	1.6	1.709	2.624	0.00319	0.072	0.0008
	[23.53]	*924.07]*	*[178.79]*	*[37.01]*	*[38.72]*	*[282.27]*
Negative 90-Day Stock Rtn	0.545	0.343	0.288	0.00115	0.0259	0.0001
	[38.25]	*[29.70]*	*[79.35]*	*[18.86]*	*[31.54]*	*[142.51]*
CEO Change Announcement	0.477	0.474	0.165	0.0005809	0.0111	0.00005
	[3.76]	*[3.83]*	*[2.03]*	*[3.32]*	*[2.91]*	*[3.32]*
Chairman	0.049	0.0661	0.021	0.000145	0.00847	0.00001
	[0.62]	*[0.91]*	*[0.32]*	*[0.95]*	*[1.63]*	*[1.11]*
Director	**0.075**	**0.082**	0.29	**0.000146**	**0.00373**	**0.00002**
	[1.92]	***[2.49]***	*[0.90]*	***[2.58]***	***[2.77]***	***[4.18]***
President	-0.0225	-0.0099	**-0.112**	0.0000	0.00044	0.000006
	[0.57]	*[0.25]*	***[3.09]***	*0.00*	*[0.36]*	*[0.84]*
CFO	-0.015	0.033	0.117	0.000141	0.0052	**0.00003**
	[0.18]	*[0.44]*	*[1.74]*	*[1.18]*	*[0.90]*	***[2.75]***
COO	-0.075	0.00036	0.159	0.000196	0.00373	**0.00005**
	[0.73]	*[0.00]*	*[1.68]*	*[1.13]*	*[0.90]*	***[3.07]***
CIO	1.184	0.128	-0.135	0.000167	0.0039	-0.00002
	[1.07]	*[0.59]*	*[1.24]*	*[0.60]*	*[0.50]*	*[1.20]*
CTO	**-0.237**	**-0.205**	-0.134	-0.000138	-0.00397	0.00004
	]1.99]	***[2.85]***	*[0.84]*	*[0.83]*	*[1.10]*	*[1.59]*

In five of the six regressions change announcements for Directors generated an estimated coefficient which is positive and statistically significant over the 95% level. Thus, it appears that changes in the board of directors should be considered news events, increasing the volatility of stock returns at the time of announcement. However, the increase in volatility seems to be small in magnitude. The overall mean of the absolute value of the FF3F residuals is roughly 1.27, or 1.27 percent price difference from the daily return predicted by the FF3F. Although the coefficient for volatility produced by announcements of changes in the Board of Directors is statistically significant, the magnitude is perhaps negligible, less than one tenth of one percent. The magnitudes of effects conveyed by the other regressions are similar. Additionally, there may be some omitted variable bias skewing results in these regressions. This is further addressed in subsequent tables.

While several of the officer announcements produce statistically significant coefficients for a couple of the different volatility measures, the significance of this result is made questionable by the inconsistency and lack of robustness of these findings. The various coefficients only approach statistical significance in at most 2 of the 6 constructions (announcements regarding CTOs), and for that officer the sign of the coefficient switches, going from reducing volatility in end-of-day measures of volatility to increasing volatility using statistics constructed with intra-day prices.

Table 1-6. Wilcoxon Signed-Rank Tests - Difference in Volatility Subsequent to Other Change Announcements

		End-of-Day Dep.Var.			Intra-Day Dep. Var.		
		Absolute Value of Daily Pct Change	3-Day Sum of Abs. Value of Daily FF3F-	5-Day Sum of Abs. Value of Daily FF3F-	Single-Day Standard Dev. 15- Minute	Single-Day Sum Abs. Value of 15-Minute Returns	Single-Day Sum Squared 15-Minute Returns
Chairman	Positive	315	295	**288**	273	276	269
	Negative	306	327	**331**	267	264	271
	z-score	0.187	-1.519	**-2.097**	0.638	0.566	0.024
	P-value	0.852	0.1288	**0.036**	0.5234	0.5716	0.9809
Director	Positive	1545	1535	1511	1244	1267	1287
	Negative	1496	1489	1508	1289	1266	1246
	z-score	0.543	0.289	0.252	0.535	-0.139	0.488
	P-value	0.5873	0.7729	0.8011	0.5929	0.8892	0.6256
President	Positive	**1175**	1197	1197	1000	1009	1015
	Negative	**1249**	1230	1225	959	950	944
	z-score	**-1.767**	-0.859	-0.581	0.911	1.379	1.173
	P-value	**0.0773**	0.3903	0.5613	0.3625	0.1679	0.241
CFO	Positive	356	346	343	245	247	253
	Negative	310	317	318	250	248	242
	z-score	1.231	0.326	0.602	0.402	0.455	0.178
	P-value	0.2182	0.7446	0.5475	0.6874	0.6493	0.8589
COO	Positive	162	153	152	124	115	122
	Negative	155	163	161	117	126	119
	z-score	0.152	-0.204	-0.166	0.484	-0.045	0.092
	P-value	0.8795	0.8386	0.8679	0.6283	0.9643	0.9268
CIO	Positive	127	128	139	111	109	115
	Negative	137	138	131	116	118	112
	z-score	-0.219	-0.1	0.276	-0.23	-0.514	-0.234
	P-value	0.8266	0.9204	0.7825	0.818	0.6074	0.8148
CTO	Positive	56	61	59	56	61	56
	Negative	65	62	64	56	51	56
	z-score	-1.01	-0.008	0.215	0.075	1.382	0.836
	P-value	0.3125	0.994	0.8301	0.9398	0.167	0.4031

Table 7 presents the results of Wilcoxon signed rank tests for management change announcements other than CEO. Other management change announcements occurring on the same day as CEO change announcements or quarterly reporting dates are not included in these tests. Only two cells in this table indicate statistically significant effects at the 90 percent significance level (identified in bold). However, in each of these cases, the other five tests yielded insignificant results, even changing sign when moving from end-of-day to intra-day prices. Combined with the results above, this provides very little support for an argument that corporate changes in leadership positions other than CEO are significant news events in the stock market. Additionally, in Table 6, CTO change announcements

also produced a negative coefficient associated with volatility. However, the statistical significance of that coefficient is much lower than in Table 7. This might be symptomatic of a few outliers biasing results in the regressions for Table 6. Rerunning the regressions for CTO in Table 6 using quantile regressions reduced the significance of this finding as did running the regressions with observations where the dependent variable lies within three standard deviations from its mean.

While Directors were the only individual officers other than CEOs consistently demonstrating increased volatility associated with change announcements, aggregating all of the management announcements other than CEO with a dummy variable to indicate any change announcement other than CEO generates some observed volatility effects. Table 8 presents the results of regressions using an aggregate dummy for any change announcement other than CEO. Regressions were performed using robust standard errors, limiting data to within 3 standard deviations of the mean of the dependent variable, and excluding any observations occurring on the same day as CEO change announcements. Consistent with previous tables, quantile regressions were used for each of the right-most columns under the end-of-day and intra-day dependent variables headings. Controlling for fixed-effects produced negligible differences in regression results.

Table 1-7. Aggregate Dummy Variable for All Changes Other than CEO, Multiple Measure of Volatility (Robust Standard Errors), Data within 3 Standard Deviations of Mean of Dependent Variable

	Estimated Coefficients *[t-statistics]*					
	End-of-Day Indep.Var.			Intra-Day Indep. Var.		
	Absolute Value of Daily Pct Change	Absolute Value of FF3F Residuals	(Quantile Reg) Squared Daily FF3F Residuals	Single-Day Standard Dev. 15- Minute Returns	Single-Day Sum Abs. Value of 15-Minute Returns	(Quantile Reg) Single-Day Sum of Squared 15-Minute Returns
Quarterly Report Date	1.234	1.294	2.622	0.178	0.0675	0.00078
	[27.12]	[28.05]	[178.5]	[35.24]	[40.38]	[281.63]
Negative 90-Day Stock Rtn	0.42	0.261	0.288	0.0307	0.0228	0.000099
	[49.21]	[38.97]	[79.39]	[17.49]	[43.11]	[142.70]
Change Other than CEO	0.023	**0.042**	-0.016	**0.0106**	**0.00312**	**0.0000131**
	[1.25]	**[2.66]**	[0.73]	**[5.12]**	**[4.04]**	**[3.37]**

Four of the six regressions generated positive, statistically significant coefficients, indicating some relationship between announcements for changes in officers other than CEO and increased volatility. To explore the potential that this observed volatility is due to Director announcements as Table 6 might suggest, the regressions from Table 8 were run again including a variable for director change announcements. The results of these regressions are contained in Table 9.

Table 1-8. Aggregate Dummy Variable for All Changes Other than CEO, Multiple Measure of Volatility (Robust Standard Errors), Data within 3 Standard Deviations of Mean of Dependent Variable - Controlling for Director Change Announcements

	Estimated Coefficients *[t-statistics]*					
	End-of-Day Indep.Var.			Intra-Day Indep. Var.		
	Absolute Value of Daily Pct Change	Absolute Value of FF3F Residuals	(Quant. Reg) Sqrd Daily FF3F Residuals	Single-Day Stan. Dev. 15- Min Returns	Single-Day Sum Abs. Val. of 15-Min Returns	(Quant. Reg) Single-Day Sum of Sqrd 15-Min Returns
Quarterly Report Date	1.234	1.294	2.622	0.179	0.0675	0.00078
	[27.12]	[28.05]	[178.5]	[35.25]	[40.38]	[281.43]
Neg. 90-Day Stock Rtn	0.42	0.261	0.289	0.0307	0.0228	0.000099
	[49.21]	[38.97]	[79.44]	[17.49]	[43.11]	[142.60]
Change Other than CEO	-0.024	-0.0072	-0.055	**0.0063**	0.0016	0.000008
	[1.11]	[0.41]	[1.92]	**[2.47]**	[1.83]	[1.60]
Director	**0.107**	**0.113**	**0.091**	**0.0095**	**0.0035**	0.000014
	[2.91]	**[3.89]**	**[2.10]**	**[2.34]**	**[2.47]**	[1.74]

In every specification above, including Director as an additional explanatory variable in the regressions in Table 8 reduced the significance of announced changes other than CEO, with only one specification yielding a statistically significant coefficient for changes other than CEO (and directors). The results for Director in Table 9 are roughly equivalent with Table 6 as those regressions included all of the positions captured by the "Change Other than CEO" variable displayed here. However, from this regression we also see that announced changes in positions other than Director and CEO do not appear to robustly affect volatility in aggregate.

While there does not appear to be observable volatility effects associated with announcements in general other than CEO or Director, just as CEOs hired from outside the firm appear to be associated with additional volatility, the last portion of this section will examine whether volatility effects vary with different sources of new personnel for management positions other than CEO and with varying stock performance in the 90-days preceding announcements.

Table 10 presents the results of regressions evaluating volatility effects potentially created by source of incoming personnel, regardless of the position they are filling. For changes in volatility to be attributable to changes in specific management positions, rather than the source of the people filling the positions, the effects must be different from those identified in the following table. The regressions in Table 10 only include observations where the dependent variable lies within three standard deviations of its mean and excludes observations coincident with CEO change announcements.

Table 1-9. Volatility Effects - Source of Incoming Personnel - Various Volatility Measures (Robust Standard Errors)

	Estimated Coefficients *[t-statistics]*					
	End-of-Day Indep.Var.			Intra-Day Indep. Var.		
	Absolute Value of Daily Pct Change	Absolute Value of FF3F Residuals	(Quantile Reg) Squared Daily FF3F Residuals	Single-Day Standard Dev. 15- Minute Returns	Single-Day Sum Abs. Value of 15-Minute Returns	(Quantile Reg) Single-Day Sum of Squared 15-Minute Returns
Quarterly Report Date	1.234	1.29	2.622	0.179	0.0675	0.00078
	[27.12]	[28.05]	[178.52]	[35.25]	[40.38]	[281.35]
Negative 90-Day Stock Rtn	0.42	0.261	0.289	0.0307	0.0228	0.000099
	[49.21]	[38.97]	[79.41]	[17.49]	[43.11]	[142.51]
External Hire	**0.068**	**0.0777**	0.0362	**0.01**	**0.0055**	**0.000028**
	[2.62]	**[3.52]**	[1.22]	**[3.56]**	**[5.58]**	**[5.22]**
Internal Transfer	-0.041	0.0086	-0.0034	0.007	-0.0021	-0.000007
	[0.78]	[0.19]	[0.05]	[1.07]	[0.98]	[0.61]
Internal Promotion	0.021	0.028	-0.0527	**0.0112**	**0.0035**	0.0000002
	[0.69]	[1.19]	[1.50]	**[3.62]**	**[2.76]**	[0.31]

It appears that even for announcements which don't include CEO changes, external hires correlate with increased volatility. However, over 60 percent of the announcements involving people moving into senior management positions from outside the company involve Directors. Since Director-change announcements appear to increase volatility in Table 6, it is unclear which variable is driving the volatility with the previous tables. The results in Table 10 could simply be a reflection of Director-change volatility rather than from the external hires. However, these regressions continue to deliver positive and statistically significant coefficients even when the data include only changes other than CEOs and Directors, though the magnitude and significance of the estimated coefficients are slightly reduced.

The high correlation between external hires and Director replacements also creates the possibility that the increased volatility associated with Director change announcements may in fact be associated with external hires rather than Directors,

specifically. Table 11 presents the results of regressions exploring this potential. Indeed, this appears to be the case. When a dummy for external hires is included in the regression, the coefficient on Director Announcement decreases in magnitude and becomes statistically insignificant, while the External Hire coefficient is roughly the same magnitude as the previous Director Announcement coefficient, and is statistically significant at about the same level of confidence. Thus it appears that the main driver in the additional volatility is not the fact that it's a Director being replaced, but rather that the replacement is coming from outside the company and potentially increases uncertainty about future policies and profitability.

Table 1-10. External Hires are more likely the cause of volatility attributed to Director Change Announcements (Robust Standard Errors)

	Sum of Absolute Value of 15-Minute Returns Estimated Coefficients *[t-statistics]*	
Quarterly Reporting Date	0.0722	0.0721
	[38.76]	*[38.77]*
Negative 90-Day Rtn	0.0258	0.0258
	[31.58]	*[31.58]*
CEO Announcement	0.0159	0.0159
	[3.67]	*[3.70]*
Director Announcement	**0.00417**	0.000603
	[3.12]	*[0.32]*
External Hire		**0.00494**
		[2.92]

Of note, several of the estimated coefficients within cells of Table 10 are negative, giving the impression that internal transfers *might* have a tendency to decrease volatility, potentially coming as good news to stockholders. However, the sign is

inconsistent between the various volatility statistics, and the t-statistics continue to be insignificant. And while two of the coefficients were significantly positive for promotions, one of the regressions yielded a fairly strong negative coefficient. Thus, a claim that internal transfers or promotions generally increase or decrease stock price volatility is not supported by these results.

If the source of incoming personnel conveys different information for different positions, omitting these variables may dampen the overall effect observed in the general data for each position change announcement. To explore this potential omitted-variable bias, Table 12 displays the results of multiple regressions with each of the chief officers and the source of the incoming personnel for each position change. Each regression excludes days with CEO and Director change announcements and includes controls for quarterly reporting days, poor stock performance in the preceding 90 days, and only includes data within three standard deviations of the mean of the dependent variable.

Results seem to indicate that source of personnel does matter for officers other than CEO, and the source of the incoming personnel influences the significance of volatility observed with various change announcements. Evidence suggests that volatility increases when Chairmen are selected from outside the company, and when COOs are selected from inside the company. Subsequent research could reveal the mechanisms responsible for these changes in volatility. However, based on these results, it appears that while announcements regarding officers other than CEOs may not change market volatility, in general, the market seems to be interested in other senior leadership changes

depending on the circumstances of the changes. Additional research may shed light on the specifics of these circumstances.

Table 1-11. Volatility Associated with Different Sources for Different Corporate Officers - Regressions Control for Quarterly Reporting Dates and Negative Stock Returns for the Preceding 90 Days

		Quantile Regressions - Estimated Coefficients *[t-statistics]*					
		End-of-Day Indep.Var.			Intra-Day Indep. Var.		
		Absolute Value of Daily Pct Change	Absolute Value of FF3F Residuals	Squared Daily FF3F Residuals	Single-Day Standard Dev. 15- Minute Returns	Single-Day Sum Abs. Value of 15-Minute Returns	Single-Day Sum of Squared 15-Minute Returns
Chairman	External Hire	0.165	**0.287**	**0.507**	**0.053**	**0.0155**	**0.00008**
		[1.14]	**[2.54]**	**[2.61]**	**[3.36]**	**[3.46]**	**[2.49]**
	Internal Transfer	0.116	0.184	0.229	**0.038**	0.00621	0.00005
		[0.87]	[1.77]	[1.29]	**[2.49]**	[1.43]	[1.54]
	Promotion	-0.014	-0.016	-0.0289	-0.0156	0.00517	0.00003
		[0.16]	[0.24]	[0.24]	[1.61]	[1.85]	[1.26]
President	External Hire	-0.0035	0.0085	0.0101	0.0033	**0.0043**	0.0000224
		[0.07]	[0.21]	[0.15]	[0.55]	**[2.51]**	[1.76]
	Internal Transfer	**-0.168**	-0.074	-0.091	-0.0007	-0.0038	-0.00002
		[2.02]	[1.14]	[0.82]	[0.08]	[1.39]	[1.01]
	Promotion	**-0.086**	**-0.062**	-0.089	-0.001	-0.00074	-0.000006
		[2.32]	**[2.15]**	[1.80]	[0.27]	[0.62]	[0.63]
CFO	External Hire	0.0039	0.101	0.166	0.003	**0.0085**	0.000035
		[0.04]	[1.40]	[1.34]	[0.27]	**[2.63]**	[1.44]
	Internal Transfer	-0.061	0.048	0.069	-0.007	0.0017	0.00005
		[0.46]	[0.46]	[0.39]	[0.44]	[0.36]	[1.56]
	Promotion	0.109	0.105	0.163	0.0088	0.0047	0.00003
		[1.37]	[1.70]	[1.54]	[0.99]	[1.83]	[1.83]
COO	External Hire	0.242	0.199	0.325	-0.00092	**0.013**	0.00007
		[1.28]	[1.35]	[1.28]	[0.04]	**[2.07]**	[1.54]
	Internal Transfer	**0.979**	**0.797**	**1.786**	0.0088	**0.046**	**0.00034**
		[2.87]	**[3.01]**	**[3.92]**	[0.23]	**[4.06]**	**[4.10]**
	Promotion	0.062	0.065	0.096	0.0014	0.0055	0.00004
		[0.63]	[0.86]	[0.74]	[0.13]	[1.66]	[1.46]
CIO	External Hire	-0.093	**-0.231**	-0.281	-0.0126	-0.0036	-0.000027
		[0.76]	**[2.43]**	[1.73]	[0.92]	[0.91]	[0.93]
	Internal Transfer	-0.085	0.141	0.11	**0.191**	0.0187	0.000041
		[0.26]	[0.56]	[0.25]	**[5.07]**	[1.70]	[0.50]
	Promotion	-0.103	-0.002	-0.0055	-0.017	-0.00213	-0.000027
		[0.79]	[0.02]	[0.03]	[1.23]	[0.53]	[0.93]
CTO	External Hire	-0.08	-0.0286	-0.043	-0.012	0.0041	0.000043
		[0.42]	[0.19]	[0.17]	[0.60]	[0.70]	[0.99]
	Internal Transfer	0.472	-0.112	-0.388	0.039	0.0201	0.000146
		[1.10]	[0.34]	[[0.67]	[0.89]	[1.49]	[1.45]
	Promotion	-0.131	-0.247	-0.332	0.0167	0.0033	-0.00006
		[0.71]	[1.74]	[1.35]	[0.86]	[0.59]	[0.14]

As a first step in exploring the circumstances which produce increased volatility with management change announcements for positions other than CEO, the final set of tables combines source of personnel with a dummy variable for positive or negative 90-day stock returns in the previous 90 days. To evaluate the impact of various management change announcements with alternate sources of incoming personnel and when 90-day stock returns have been negative, the following interaction terms were created with the three sources of personnel listed in the MCD (external hire, internal transfer, or internal promotion): office*source, office*negative 90-day return, source*negative 90-day return, and office*source*negative 90-day return. To clarify, the interaction terms are for each of the seven offices utilized in this study (Chairman, Director, President, CFO, COO, CIO, CTO) and each of the three sources for personnel (external hire, internal transfer, internal promotion). There are seven x three (21) "office*source" interaction terms to be analyzed. The other interaction terms are constructed similarly. To mitigate concerns regarding too many variables in regressions, each regression focused on one office at a time and one source of incoming personnel at a time. Each regression also included the independent variables which are used to form the interaction terms, including the position changing, the source of incoming personnel, and negative 90-day stock return. Regressions also included the previous variables shown to increase volatility: quarterly reporting dates, negative 90-day return, and CEO change announcements; however, because the significance of these variables has already been established, the coefficients and t-statistics will be omitted from the following tables.

Each of the following three tables was constructed using quantile regression with the single-day sum of squared 15-minute returns as the dependent variable. Table 13 focuses on management change announcements in which the inbound personnel come from outside the company. In Table 14, the individuals filling the positions have been classified as internal transfers. In Table 15, incoming officers are being promoted from inside the company. Because these regressions include multiple interaction terms, some additional explanation is provided to prevent misinterpretation of these results. Each of these variables is a dummy variable. Coefficients on interaction terms apply only in instances where both (all) independent variables are one. Thus, the terms without the interactions are then only considering the instances in which one or more of the other interacting independent variables are zero. For example, *Position* is included in each regression as an independent variable, as well as three interaction terms including *Position*, *Position*External Hire*, *Position*Negative 90-Day Return*, and *Position*External Hire*Negative 90-Day Return*. For the *Position*External Hire*Negative 90-Day Return* coefficient to apply, all three of the independent variables must be one. Thus, the coefficient for *Position*Negative 90-Day Return* only applies to announcements that did not include an external hire, and the basic *Position* coefficient is only for cases that don't include the source included in the regression and don't have negative 90-day returns. With this is mind, a statistically insignificant coefficient in the *Position* cells do not necessarily mean that all change announcements involving that position don't increase volatility, only those not included by the other interaction terms.

The results in Table 13 are consistent with the observed coefficients in Table 10 for External Hires. Additionally, several other cells appear to be statistically significant: Chairmen and CTOs joining firms during times of poor stock performance, and CFOs and COOs changing during times of poor stock performance.

Table 1-12. Volatility Effects: Change Announcements w/ External Hires. Interaction with poor stock performance and position changing

	Quantile Regression: Daily Sum of Squared 15-Minute Returns Estimated Coefficient *[t-statistic]*						
	Position Announced as Changing						
	Chairman	***Director***	***President***	***CFO***	***COO***	***CIO***	***CTO***
Position	0.0000003	0.0000114	-0.000007	0.000013	0.0000281	-0.000008	0.000052
	[0.02]	*[0.81]*	*[0.67]*	*[0.66]*	*[1.11]*	*[0.22]*	*[1.14]*
External Hire	**0.0000195**	0.0000189	**0.000017**	**0.0000197**	**0.0000188**	**0.0000197**	**0.0000189**
	[2.74]	*[1.68]*	***[2.23]***	***[2.75]***	***[2.69]***	***[2.80]***	***[2.71]***
Position*External Hire	-0.000005	-0.0000107	0.000019	-0.000018	0.0000398	-0.00001	0.0000548
	[0.16]	*[0.54]*	*[0.96]*	*[0.48]*	*[0.76]*	*[0.18]*	*[0.77]*
Position*Neg. 90	0.000008	-0.0000312	-0.000019	**0.0000647**	**0.0000748**	0.000003	0.0000641
	[0.33]	*[1.41]*	*[1.27]*	***[2.08]***	***[1.91]***	*[0.06]*	*[0.88]*
External Hire*Neg 90	**0.000028**	0.0000184	**0.000042**	**0.0000283**	**0.0000296**	**0.0000359**	**0.000031**
	[2.57]	*[1.09]*	***[3.52]***	***[2.60]***	***[2.77]***	***[3.34]***	***[2.91]***
Pos*Ext. Hire*Neg. 90	**0.0001568**	0.0000537	-0.0000253	0.0000212	-0.0001274	-0.0001	**0.0002917**
	[3.11]	*[1.74]*	*[0.85]*	*[0.39]*	*[1.49]*	*[1.27]*	***[2.60]***

Table 14 identifies the following cells to be evaluated further: Chairmen hired from inside the company, CFOs changing when 90-day stock returns are negative, and Chairmen, COOs and CTOs hired from inside the company when 90-day stock returns are negative.

Table 1-13. Volatility Effects: Change Announcements with Internal Transfers. Interaction with poor stock performance and position changing

	Quantile Regression: Daily Sum of Squared 15-Minute Returns Estimated Coefficient *[t-statistic]*						
	Position Announced as Changing						
	Chairman	***Director***	***President***	***CFO***	***COO***	***CIO***	***CTO***
Position	-0.000009	**0.000017**	0.000004	0.000015	0.0000341	-0.000001	0.0000431
	[0.52]	***[2.18]***	*[0.44]*	*[0.81]*	*[1.42]*	*[0.04]*	*[1.16]*
Internal Transfer	-0.000004	0.0000004	0.0000254	0.000005	0.000003	0.000004	0.000003
	[0.26]	*[0.02]*	*[1.35]*	*[0.30]*	*[0.24]*	*[0.25]*	*[0.24]*
Position*Internal	**0.0001**	-0.000005	-0.0000454	-0.000007	-0.00001	0.000004	0.000018
	[2.96]	*[0.15]*	*[1.58]*	*[0.15]*	*[0.17]*	*[0.05]*	*[0.17]*
Position*Neg. 90	**0.00006**	**0.000033**	-0.000015	**0.0000674**	0.0000253	-0.000057	-0.000056
	[2.42]	***[2.75]***	*[1.09]*	***[2.38]***	*[0.67]*	*[1.39]*	*[0.97]*
Internal*Neg 90	0.000002	0.00000191	-0.0000262	-0.000006	-0.000004	0.000002	0.000002
	[0.10]	*[0.78]*	*[0.93]*	*[0.28]*	*[0.18]*	*[0.11]*	*[0.12]*
Pos*Internal*Neg. 90	**-0.0001216**	-0.000089	0.0000758	0.0000935	**0.0001879**	0.0000388	**0.000558**
	[2.31]	*[1.87]*	*[1.74]*	*[1.41]*	***[1.98]***	*[0.30]*	***[2.90]***

Table 15 produces the following cells of interest: Directors promoted after negative 90-day stock returns, CFO changes when 90-day stock returns are negative, and Presidents and COOs promoted from inside the company. It should be noted that while these cells do appear to be statistically significant, the magnitude of the increase in volatility is potentially miniscule. The mean of the daily sum of squared 15-minute returns is about 0.014. Thus the statistically significant coefficient identified for COO and position in Table 15 (0.000151) represents about one tenth of one percent increase in volatility.

Table 1-14. Volatility Effects: Change Announcements with Internal Promotions. Interaction with poor stock performance and position changing

	Quantile Regression: Daily Sum of Squared 15-Minute Returns Estimated Coefficient *[t-statistic]*						
	Position Announced as Changing						
	Chairman	***Director***	***President***	***CFO***	***COO***	***CIO***	***CTO***
Position	0.0000135	**0.0000219**	0.0000223	0.0000205	**0.000151**	-0.00001	-0.000065
	[0.59]	***[2.69]***	*[1.64]*	*[0.87]*	***[3.90]***	*[0.27]*	*[1.31]*
Internal Promotion	0.000003	0.000005	0.0000137	0.000003	0.000001	0.0000006	0.0000019
	[0.39]	*[0.60]*	*[1.19]*	*[0.32]*	*[0.08]*	*[0.08]*	*[0.24]*
Position*Promotion	-0.0000177	-0.00003	**-0.0000441**	-0.0000226	**-0.000141**	0.0000207	-0.0000588
	[0.59]	*[1.42]*	***[2.15]***	*[0.67]*	***[2.96]***	*[0.40]*	*[0.84]*
Position*Neg. 90	0.0000247	0.000017	-0.000005	**0.0000932**	0.000059	-0.0000315	0.000034
	[0.72]	*[1.34]*	*[0.24]*	***[2.55]***	*[0.91]*	*[0.58]*	*[0.45]*
Promotion*Neg 90	**-0.000027**	**-0.000033**	0.000007	-0.0000225	-0.0000188	-0.0000133	-0.0000148
	[2.06]	***[2.59]***	*[0.43]*	*[1.83]*	*[1.55]*	*[1.11]*	*[1.25]*
Pos*Promo*Neg. 90	0.00005	**0.0001**	-0.000022	0.000002	-0.000003	-0.0000383	-0.0000995
	[1.14]	***[3.15]***	*[0.71]*	*[0.03]*	*[0.03]*	*[0.49]*	*[0.89]*

The robustness of the findings for each of the other cells of interest identified in Tables 13-15 was tested using Wilcoxon Signed-Rank tests, filtering for the appropriate conditions as identified by the cells with statistically significant coefficients.

Many of the signed rank tests generated z-scores with the same sign as the cells of interest identified above. However, none were statistically significant. The full table is in Appendix III. It might be the case that the small magnitudes of the increases in volatility may not be detectable by the Wilcoxon signed rank test, other possibilities may explain why none of the announcements appear significant. While these tests ignored days with coincident quarterly reporting dates and CEO change announcements, and several of the tests considered 90-day stock returns to control for poor company performance, differences in daily volatility might be muted if information leakage prior to official announcements increases volatility in preceding time periods. Increasing the size of the event window might capture increasing volatility in previous time periods, but doing so

also potentially dilutes effects observed in a shorter time frame. I repeated the sign-rank tests utilized above with a 3-day event window commencing on the day prior to official announcement with no significantly different findings.

Discussion and Conclusions

The results of the first set of analysis are perhaps not new, but they are important to the rest of this analysis. Using a database of announced corporate senior leadership changes (*Liberum's* Management Change Database), OLS regressions with robust standard errors and fixed effects, quantile regressions, and Wilcoxon signed-rank tests identify increased volatility in end-of-day and 15-minute stock price data on days when corporations announce changes in CEO. These results are robust to the inclusion of controls for increasing volatility around quarterly reporting dates and regardless of stock performance during the previous 90 days. The results of similar tests also confirm additional volatility associated with announcements of CEO replacements coming from outside the company. These findings suggest that CEO change announcements provide information of which investors were previously unaware and which increases disagreement or uncertainty regarding expectations of future profitability. These findings are important to this study for two reasons, (1) they confirm the work of previous researchers using a new dataset and shorter event windows, and (2) they demonstrate the ability of the tests used in the subsequent portions of study to identify changes in stock price volatility.

The second portion of analysis explored the impacts on the same volatility measures of senior leadership change announcements for chief officers other than CEO.

Consistent with the findings of Nguyen and Nielsen (2010), changes in the Board of Directors increases stock price volatility. However, additional analysis suggests that Directors appear significant because of their high correlation with external hires. When external hires are included as an explanatory variable, this independent variable overwhelms any effect attributable to changes specifically in the Director position. Other than Directors, no other senior leadership positions generated consistently significant predictions of volatility changes for announcements overall.

Although I found little evidence for generally consistent volatility effects for senior management change announcements other than CEO, including additional controls identified some volatility in various cells of the tables. When the sources of the incoming personnel were included in regressions, Chairmen hired from outside the company appear to increase volatility, and COOs appointed from lateral transfers within the company appear to increase volatility. When poor performance and source of incoming personnel were included, external hires during poor performance appear significant for several senior positions, and internal promotions of several officers appear to potentially calm investor concerns during poor performance. So while the initial finding that change announcements for officers other than CEOs are perhaps unimportant to the market, a more appropriate interpretation is perhaps that the value of the information conveyed by these announcements is determined by the details of the announcement, and those details may mean different things when different officers are replaced.

The number of observations within cells diminishes rapidly as additional controls are included, making further inquiry of specific details of management change

announcements unreliable with the current data set. However, as the first to explore volatility associated with senior management changes other than CEOs, Directors, and Chairmen, this study makes some progress toward identifying that while there may be very limited predictable volatility effects in general, additional conditions may affect volatility of stock prices when leaders other than CEOs change.

As a secondary and related finding, the mean of the price changes associated with the increased volatility do not appear to be equal to zero. However, the estimated coefficients are of varying significance depending on the type of regression. Nguyen and Nielsen find that prices fell when Directors died (2010). With a broader sample, I find that there might be an overall tendency for prices to rise when Director-change announcements are made. Also contrary to previous results, regression results with this data sample indicate a tendency for prices to rise when changes in CEO are announced. This may provide some support for Salas's findings regarding senior leadership entrenchment (2010). The significance of these findings is not robust when quantile regressions are used, but they are robust when limiting the data to within three standard deviations of the mean of the dependent variable. Results are included in Appendix IV.

Additional Considerations

Throughout this chapter, the analysis has focused on the identification of the specific positions which the market appears to value as capital holders. One alternative perspective to gain insight into the development of institutional capital within firms might focus on the type of organization or organizational structure which influences volatility

for various officer change announcements. The analysis above assumes that the type of officer is the influential determinant of individual-specific organizational capital held by the officers. Another fruitful line of questioning might consider the institutional structure in which specific officers appear to hold more capital than in others. For example, do more horizontal organizational leadership structures imply more or less individual-specific capital? Such a structure is designed to share more leadership responsibility and increase collaboration by placing more individuals on equal footing, but also may increase contention, infighting, or dissent during times of leadership transition if hierarchy is ill-defined and informal institutions take longer to recover from disruption. Thus, the organizational capital associated with stability at the CEO may go beyond the CEO position itself, and attributing the institutional capital to the individual filling the CEO position erroneously puts the emphasis on the individual or position rather than the organization producing the volatility associated with the leadership change. Additional factors that could influence the magnitude of the position volatility coefficient may include stages of corporate development, size of corporations, capital/labor ratios, type of industry, or amount of government regulation. Based on these considerations, while I have focused on identifying which leadership positions matter, a related set of questions might identify when various leaders matter.

Additional Recommendations for Further Study

Multiple studies have confirmed the autoregressive nature of stock price volatility. For example, Pan, Wang, and Weisbach identify elevated levels of volatility up

to 36 months following a change in CEO as markets update their expectations regarding a CEO's ability (2014). Such findings contribute to the adoption of autoregressive conditional heterskedasticity (ARCH) models and generalized ARCH (GARCH) models to better identify variations from expected volatility (Engle and Ng, 1993; Bollerslev, 1986; Chen and Ghysels, 2010). ARCH models allow increased volatility from such events to be included in volatility forecasts as the model incorporates the error terms of previous time periods into predictions of future volatility. Such models may begin with the same initial regression (Equation 2), but while the error terms have a mean of zero, their variance is assumed to be a function of previous error terms and therefore not constant. GARCH models assume conditional variance which includes previous error terms and the variance of the error terms in previous time periods.

Such an approach allows for the identification and analysis of effects from news events which may overlap, amplify, or dampen residual effects from previous events or disclosures. The numerical methods used by statistics software to accomplish ARCH and GARCH analysis will also analyze the contribution of other variables into both the moving average and volatility aspects of the model. This provides a direct test of the statistical significance of the level and variance effects of the variables of interest, in this case, the various chief officer change announcements on stock market price volatility.

Other researchers have used the volatility implied by options prices to examine various market effects (Dumas, Fleming, and Whaley 1998; Bollerslev, Gibson, and Zhou 2011; Giot 2005). The price of an option increases with the likelihood of its exercise. Thus, options become more valuable if stock prices are more volatile (Black

and Scholes 1973). Options pricing models use the current and strike prices of options, in conjunction with the market value of the option, to derive what traders in the market anticipate the volatility in the stock to be. Based on the efficient market hypothesis, and because options prices are determined by the market, the prices of the options imply stock volatility. When comparing different predictors of volatility, Day and Lewis found that implied volatility models contained elements of information not found in other volatility prediction models, such as historic volatility and GARCH models (1992). Thus evidence of increasing volatility (or at least anticipated volatility), even if not decipherable in realized volatility or historical prices, may be found in changes of stock options pricing and the associated implied volatility.

To verify the findings of this study, I recommend obtaining options implied volatility data and verifying the analysis above, as well as checking previous regression results by including ARCH/GARCH techniques.

Additionally, I believe two expansions in data could prove fruitful, though potentially labor intensive. First, the data utilized for this study were limited to the Fortune 500 companies, and sometimes to their overlap with the S&P 500. Increasing the number of companies could allow for additional exploration of effects currently unobserved due to limited observations. Second, I focused only on leadership change announcements using the most common position titles. Using corporate organizational charts to identify congruent organizational hierarchy and responsibilities could allow for consideration of positions with similar responsibilities but with different position titles. Additionally, evaluating positions based on their tier within the organizational chart could

differentiate between effects driven by the level of leadership positions versus the title of the position.

APPENDIX I.

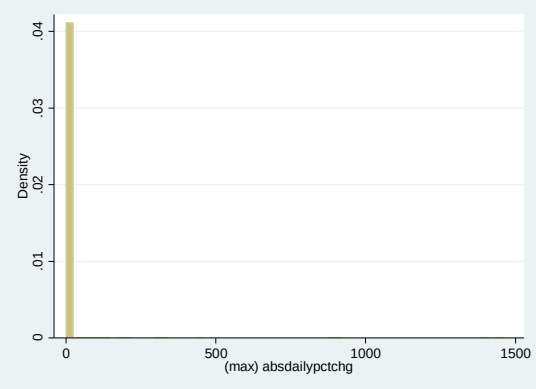
Number of Announcements from the Management Change Database, by Office, Source of New Personnel, Status of Departure

	n	J	I	P	L	R	T
CEO	651	113	154	380	16	258	3
CFO	778	232	120	348	31	284	1
COO	404	63	22	258	7	92	3
CIO	285	135	22	118	7	51	1
CTO	136	54	13	61	3	24	1
Director	3434	2126	210	430	9	396	2
Chairman	986	147	256	460	18	373	1

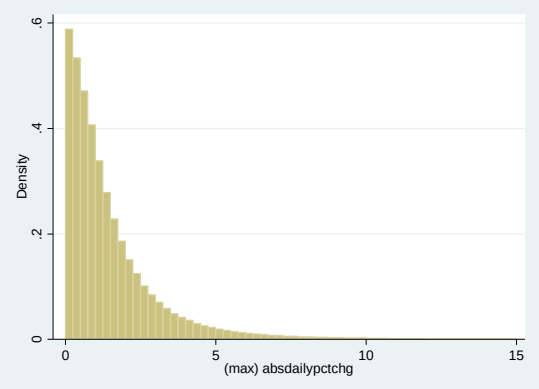
APPENDIX II.

	n	Mean	SD	Min	Max	3sigma threshold	Mean + 3 Sigma
absdailypctchg	1057200	1.58	3.2244	0	1458.6	9.6732	11.2532
sqrddailypctchg		12.898	3020	0	2127575	9060	9072.898
dailysd15min	802983	0.217952	0.378148	0	47.90827	1.1344452	1.3523976
sumsqpctreturn15min	802948	0.0138	11.84	0	10610	35.52	35.5338
sumabspctreturn15min		0.0712	0.133	0	104.2995	0.399	0.4702
absffres	1057200	1.26838	3.04	0.0000008	1457.984	9.12	10.38838
sqrdffres		10.877	3018	0	2125716	9054	9064.877
sabsr3dayffres	1056266	3.805198	5.779736	0.0152399	1479.672	17.339208	21.144406
sabsr5dayffres	1055332	6.342502	8.00886	0.184564	1498.347	24.02658	30.369082
sabsr10dayffres	1052997	12.68799	12.97702	1.086228	1586.392	38.93106	51.61905
sabsr21dayffres	1047868	26.65736	22.92484	3.834272	1631.861	68.77452	95.43188

Histogram of absolute value of daily % Change



Histogram of abs. val. of daily % change within 3 Standard Deviations



APPENDIX III.

Appendix III. Wilcoxon Signed Rank Tests of Cells of Interest from Tables 12-14, exluding dates with coincident Quarterly Report Dates and CEO announcements

		End-of-Day Dep.Var.		Intra-Day Dep. Var.		
		Absolute Value of Daily Pct Change	3-Day Sum of Abs. Value of Daily FF3F-Residuals	Single-Day Standard Dev. 15-Minute Prices	Single-Day Sum Abs. Value of 15-Minute Returns	Single-Day Sum Squared 15-Minute Returns
Chairman*Ext*Neg 90	Positive	32	30	26	27	26
	Negative	30	33	26	25	26
	z-Score	0.782	0.561	-0.009	0.355	0.291
	P-Value	0.4344	0.5745	0.9927	0.7225	0.7707
CTO*Ext*Neg 90	Positive	12	10	12	12	11
	Negative	11	11	9	9	10
	z-Score	-0.243	-0.261	-0.434	0.747	0.295
	P-Value	0.8078	0.7943	0.6639	0.4549	0.7677
CFO*Neg90	Positive	153	140	103	113	120
	Negative	130	140	117	107	100
	z-Score	1.621	0.058	-0.241	0.852	1.082
	P-Value	0.105	0.9541	0.8094	0.3944	0.2791
COO*Neg90	Positive	66	62	46	49	50
	Negative	69	71	51	48	47
	z-Score	-0.738	-0.365	-0.117	-0.031	0.286
	P-Value	0.4605	0.7152	0.9069	0.9756	0.7748
Chairman*Int	Positive	72	61	55	52	54
	Negative	57	68	53	56	54
	z-Score	0.873	-0.554	0.987	0.343	0.254
	P-Value	0.3825	0.5799	0.3237	0.7314	0.7992
Chairman*Int*Neg 90	Positive	36	28	29	25	28
	Negative	25	32	24	28	29
	z-Score	1.225	-0.309	1.076	0.279	-0.261
	P-Value	0.2207	0.7572	0.2821	0.7804	7940
COO*Int*Neg90	Positive	5	6	4	3	4
	Negative	3	1	2	3	2
	z-Score	0.28	1.183	0.524	0.524	0.943
	P-Value	0.7794	0.2367	0.6002	0.6002	0.3454
CTO*Int*Neg 90	Positive	Insufficient Data				
	Negative					
	z-Score					
	P-Value					
Director*Promo*Neg 90	Positive	68	69	61	71	60
	Negative	77	73	67	57	68
	z-Score	-1.41	-0.478	-0.947	0.121	-0.214
	P-Value	0.1585	0.633	0.3439	0.9035	0.8305
Pres*Promo	Positive	695	695	611	609	622
	Negative	728	732	566	568	555
	z-Score	-1.322	-1.033	1.508	1.311	1.331
	P-Value	0.186	0.3015	0.1315	0.19	0.1832
COO*Promo	Positive	102	88	73	62	67
	Negative	94	107	73	84	79
	z-Score	-0.719	-1.105	-0.3	-1.435	-1.361
	P-Value	0.4719	0.2691	0.7643	0.1513	0.1736

APPENDIX IV.

Appendix IV. Percent Price Change with Events Shown to Increase Volatility

	Estimated Coefficient *[t-statistics]*		
	OLS Regression - Full Sample	OLS Regression - Within 3 Sigma	Quantile Regression
Quarterly Report Date	**0.173**	**0.131**	**0.066**
	[5.98]	***[7.39]***	***[4.72]***
CEO	**0.483**	**0.315**	0.087
	[3.34]	***[3.59]***	*[1.24]*
Director	0.002	0.05	0.038
	[0.03]	*[1.31]*	*[1.24]*
Chairman*External Hire	-0.384	**-0.367**	-0.167
	[1.24]	***[1.95]***	*[1.11]*
COO*Internal Transfer	-0.573	-0.537	0.223
	[0.75]	*[1.17]*	*[0.60]*

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2. DO US TROOP DEPLOYMENTS BUILD BETTER FOREIGN ECONOMIC INSTITUTIONS?

Introduction

After nearly a decade of military deployments to Iraq between 2003 and 2012, the beginning of 2015 found US soldiers watching aghast as ISIS eradicated ten years' worth of hard fought gains within a matter of weeks. After thousands of US deaths, millions of man-hours and billions of dollars, Iraq seems to be in a worse condition now than when the US-led multinational military force ousted Saddam Hussein's dictatorial regime in 2003. Critics of US military actions claim that the current disorder in the Middle East was largely caused by the US. However, when catastrophe strikes anywhere in the globe, US military forces are usually considered the most ready and capable first-responders. With forces deployed in a number of regions around the globe, the military's efforts to improve conditions for the world's oppressed and disaffected demonstrate a pervading national interest in helping people around the world to enjoy better lives. However, with poor results in Iraq, deteriorating situations in Afghanistan, Egypt, Syria, Israel, the Korean peninsula, etc., one wonders if US military involvement is making any progress in achieving their goals. Are they indeed making things better? Or are they making things worse?

This research builds on findings from Jones and Kane (2012) that the natural log of total US troop deployments to a country are robustly correlated with increased economic

growth in that country. They suggest several mechanisms which might explain the relationship between troops and growth, namely: security provision, institutional diffusion, and Keynesian stimulus, but they leave empirical exploration of these mechanisms for subsequent research. Picking up where they leave off, this paper explores the relationship between US troop deployments and institutional indices prevalent in contemporary institutional economics literature (Economic Freedom of the World Index, Freedom House, etc.) to determine if data support institutional diffusion via property-rights protection, contracting practices, and personal interaction as a potential cause of economic growth.

Motivation for this research is two-fold. First, it is academically intriguing. Jones and Kane leave their theoretical explanations completely open for empirical investigation. Given the significant literature demonstrating the correlations between property-rights-protecting institutions and economic growth (inter alia; Acemoglu, Johnson and Robinson, 2001, 2005; North, 1990, 1991; Rodrik, 2004; Scully, 1988), and Mancur Olsen's conjecture regarding the potentially positive role of an occupying military force, there appears to be a remarkable paucity of literature examining relationships between institutional diffusion and military deployments. Especially since military deployments have historically been precursors to other developing international relationships.

Additionally, the US Department of Defense spends more than $600billion/year. Given the lack of research regarding long-run institutional effects of U.S. military action, I believe we should learn considerably more than we currently know about our impacts, good or bad, on the countries with whom our armed forces interact. We hope our military

efforts are improving conditions, providing relief and protection to oppressed populations. However, the endogeneity of institutional development within countries might suggest that imposing U.S. institutions on economies with alternative underlying social structures could be more disruptive than constructive. This possibility provides fuel for many of the arguments made by opponents of U.S. military action. Alternatively, if foreign institutions represent inefficient Nash equilibria resulting from a series of repeated coordination games within society, the exogenous shock of the arrival of U.S. troops might be enough to shift the foreign institutions to a more efficient equilibrium or growth path of institutional development. Jones and Kane suggest that receiving U.S. troops helps nations grow. This study seeks to determine if this is because of, or in spite of, institutional effects associated with involvement of the U.S. military.

Institutions Background

According to Robert Lucas, "Once one starts to think about [economic growth], it is hard to think about anything else (1988)." The overriding mission of research in economic growth is to seek opportunities to improve the quality of life for the global community, including the billions of people living short, brutal lives without access to clean water, sufficient nutrition, and durable shelter. At its core, the objective is to learn how to produce and distribute more goods and services to more people by learning to better utilize whatever resources are available. Institutions affect production and distribution costs by determining how people work and trade with one another. Political, economic, cultural, and social rules and norms (institutions) influence work ethic, negotiation costs, levels of trust, methods of communication, prices, preferences, and

resource allocation. North (1991) states that institutions are important because institutions influence transaction costs and, unlike the assumptions within many economic models, transactions costs are positive. Transactions occur at all levels of production and distribution. Thus, production and distribution costs are largely a function of the institutions underlying the economy as a whole.

Institutional analysis is based on the axiom that healthy relationships are more beneficial and less costly than unhealthy relationships. Thus, institutions which produce lower transactions costs, lower administration costs, lower costs of bureaucracy, and lower costs of government are generally associated with more healthy relationships. Low costs of transactions, bureaucracy, and government also reduce the costs of capital transfer and generate greater benefits from trade. By measuring the costs of these relationships, we gain insight into the effects created by the operating institutions. Several indices have been developed and are frequently cited in institutional economics literature. These include, though are not limited to, the Economic Freedom of the World Index (EFW), the Freedom House Index (FH), and the Corruption Perception Index (CPI).

Glaeser, et al (2004), identify two streams of competing literature regarding the evolution of political institutions and economic growth. On one hand, he states that institutional economics research has reached close to intellectual consensus that political institutions of limited government power cause economic growth. The other position he attributes to Lipset, who in turn credits Aristotle, is that political institutions of limited government power are a result of increased human capital among the population who then impose their will and demand more limited power for government. While Glaeser

seems to agree more with the latter argument, other contemporary economists, such as Acemoglu, Johnson, and Robinson, North and Tullock, etc., argue for the former. Fortunately, regardless of the causal direction, whether top-down or bottom-up, mechanisms exist which could in principle allow US troop deployments to motivate institutional change.

To support the institutional diffusion model theoretically, I believe there are three mechanisms which might facilitate economic growth via institutional diffusion. The first is government-to-government influence tied to military involvement. Property-rights-protecting institutions might result from political pressure associated with receiving supporting troops, or may be imposed by invading US forces. If supporting troop deployments are made conditional on desirable government behavior, then desirable policies such as better protection of private property rights or civil liberties may facilitate institutional progress. Protection of property rights may be provided directly by US military members or via training provided to local government, law enforcement, or native military forces. Additionally, invasive military deployments operate with the goal of changing government forcefully. Either way, US military deployments may produce institutional change from the "top-down", if you will. If this institutional change supports and increases the provision of property rights, then, according to the studies linking good institutions and growth, we should expect economic growth to accelerate.

While Acemoglu might argue that contracting institutions are less important for economic growth than property rights institutions (2005), support contracts require direct interaction between the US government and local industry to provide for the needs of

deployed US troops. When local businesses fill US contracts they must comply with US contracting institutions. Biglaiser and DeRouen find that US corporate investment increases in the locations troops deploy to (2007). U.S. contractors hire local personnel to fill US requirements, and US troops purchase goods and services from local businesses. As local populations, businesses, and government trade according to US business practices with US personnel, businesses, and government, it seems reasonable to think some elements of US institutions might infiltrate elements of local business. If this institutional technology improves productivity or decreases transactions costs, then this mechanism potentially increases the rate of growth within the economy.

Kane states that "U.S. troops are intimately involved in meeting with local leaders, helping to arrange elections, training police forces, resolving local disputes, and generally participating in … civic life." Given this intimate involvement between troops and locals, we might expect a degree of imitation. If locals recognize that U.S. habits or methods of associating or dealing with each other are more beneficial and less costly than existing local institutions, motivation exists to alter local dealings to capitalize on newly discovered institutional efficiencies. Additionally, if institutional diffusion represents a technology transfer, thereby increasing human capital among the general population, this may result in the type of outcome anticipated by Lipset and Glaeser as the populace demands more limits on government power, improving institutional outcomes reported in institutional ratings. To be clear, I present these mechanisms, not as hypotheses to test, rather mechanisms which have the potential to make institutional diffusion a viable driver of economic growth.

Having identified several channels of potential institutional diffusion, I also note the limitations of the institutional indices as measures of genuine institutional quality. Additionally, Arruñada identifies ways that changing some institutions may negatively impact others (2007), and improvement in institutional indices may mask the elimination of underlying benefits associated with institutions perceived as outdated. Thus, improvement in institutional ratings and indices may not represent real reductions in transactions costs. Additionally, the incorporation of some technologies, such as electronic records or registries, may not be perceived as a change in any type of policy, but may have remarkable implications for the protection of individual property rights. Because no policies have changed, such shifts are unlikely to produce measurable impacts in the various indices, but may create considerable reductions in transactions costs.

While the following analysis explores one convenient mechanism for evaluating the hypothesis that troops aid institutional diffusion, many other avenues exist for troops to impact institutional development which may be explored. Future projects might specifically look at the evolution of contracting practices or evaluate troops' effects on agency and individual relationship costs. Another avenue might be to look at changes in government and corporate organizational structures or contrast firm cost structures and the ratios between capital and operating expenses. Of course, one challenge in many developing countries is tracking data in the first place, but statistics such as applications for business licenses might signal shifts in public perceptions and institutional changes

not captured by a top-level view created by an overall index. Thus, the analysis presented in this chapter represents a first look, with a myriad of questions to follow.

Military Intervention Background and Research Objectives

Pickering and Kisangani bemoan the lack of literature related to economic and social consequences of military intervention and war (2006). Their continued development of the International Military Intervention (IMI) Database has facilitated some interesting literature regarding specific incidents. Notably, they identify differing political and economic impacts of hostile vs supporting military interventions. In contrast to Jones and Kane's finding regarding US deployments, by looking at global military interventions (not just US deployments), Pickering and Kisangani find that large-scale unilateral military deployments do not have significant long-term impacts on governing institutions, economic growth rates, or physical quality of life in developing democracies. Regarding economic growth rates and quality of life, Jones and Kane find that US deployments are correlated with greater economic growth, and Kane finds that US military deployments also seem to contribute to decreasing child mortality, increasing life expectancy, and increasing telephone lines per capita (2012). Thus, US military deployments may produce different effects than other military forces, or, differences in data and methodology between the two studies are capturing very different effects.

In contrast with the paucity of literature related to economic and social outcomes of military intervention, more researchers appear to be interested in the political consequences of military intervention, especially related to the objective of democratization. However, this growing literature base is also not without disagreement.

Meernik (1996), Hermann and Kegley (1998), and Peceny (1999) identify increased democratization from military interventions by liberal states. However, Pickering and Peceny (2006), while acknowledging that a few cases of hostile US intervention have led to increased democratization, use the IMI dataset and "find little evidence that military intervention by liberal states helps to foster democracy in target countries."

Regardless, while some may debate the merits of democratization for democratization's sake, Barro argues that after controlling for many institutions which may be correlated with democracy, such as access to education, rule of law, and life expectancy, democracy appears to slow economic growth. And his analysis of correlations between economic growth and democracy indicate that countries with low levels of economic development don't sustain democracy, but nondemocratic places with economic development become more democratic (1996). He cites several previously autocratic countries which improved institutions and fostered economic growth, producing subsequent changes in political systems (Chile, Korea, Taiwan, Spain, and Portugal). Rodrik takes a different tack, arguing that the most important institution is market exchange. Because of democracy's propensity for capturing local knowledge, democracy is the "meta-institution" for establishing and maintaining continuity in market structure. However, the foundational mechanism which promotes growth is the ability to trade (2000). Both Rodrik's and Barro's findings reinforce the necessity to focus on institutional impacts of US military deployments in explaining economic growth.

This study seeks to empirically investigate whether or not US troops improve the institutional environment of the places they deploy to. A comprehensive cost/benefit

analysis regarding whether or not troops should be deployed is far beyond the scope of this study. But as a starting point, this study seeks to find out if the U.S. military can at least claim that they aren't making institutions worse.

Methods and Data

To answer the question regarding how US troop deployments impact the economic institutions of the countries they deploy to, I use multiple OLS and GLS regression on panel data assembled from DMDC's troop deployment dataset (1950-2012) compiled by Kane and Jones, Penn World tables, the World Bank (World Development Indicators, WDI), the Economic Freedom of the World Index, Freedom House, the Polity IV Project, the UCDP/PRIO Armed Conflict Dataset, and the Corruption Perception Index (CPI) to examine correlations between countries receiving US troops over time and institutional development indicators.

The Defense Manpower Data Center (DMDC) publishes annual troop deployment data by indicating the number of "troop years" the US deploys to each country. Similar to work-years, each troop-year is equivalent to one soldier deployed there for one year, but it needn't be the same soldier that stays there the whole time. The measure is additive, and does not indicate duration of deployments. Thus, one troop-year could indicate a 12-man team deployed there for one month or a set of soldiers deployed there individually on a rotational basis for the year. Although troop-years provide limited insight into volume versus duration over each year, considering institutional diffusion, learning takes time and increasing the number of troops increases exposure. I expect the tradeoff

between time and exposure to be of little consequence and ignore any duration-versus-volume effects potentially masked by the statistic.

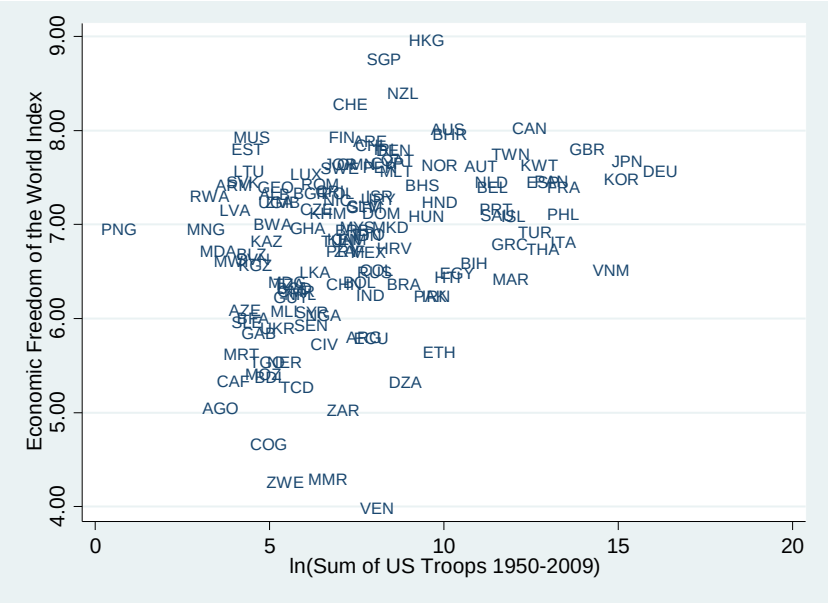

Figure 2-1 A scatterplot of EFW scores vs ln(Sum of Troop-Years from 1950-2009).

Various issues limited the availability or analysis of data. Several regions changed dramatically over the domain of years included in this analysis. For example, the former Soviet Union collapsed into variants of previous former countries, East and West Germany reunited, Czechoslovakia peacefully separated into the Czech Republic and Slovakia, and after significant conflict Yugoslavia eventually separated into five countries, Serbia, Montenegro, Croatia, Slovenia, and Bosnia and Herzegovina. Additionally, DMDC makes various changes each year in the format of the data it publishes, disrupting the continuity of the time-series, for example, sometimes reporting a couple small countries together and reporting separately at other times. Finally, each

institutional database differs in frequency and in the countries tracked over the years of interest, and sometimes the reports have expanded and contracted based on availability of information each year.

For each set of institutional development indicators, I have tried to maximize the breadth of the data set included in the analysis. For example, the initial CPI report in 1995 included just 41 countries, and its report in 2014 covered 175. The CPI rating system was overhauled in 2012 to make annual scores comparable over time, but the construction of the index previously made annual scores incomparable. Thus, I have included as many countries as possible for cross-sectional analysis when using the most recent CPI scores but limit the countries used for time-series comparison of rankings by other indicators.

Similarly, the number of countries evaluated by the EFWI has increased, and the frequency of the data has increased in 2000 from every five years to the current annual update cycle. Thus, when conducting longer time-frame analysis, such as change over 30 or 40 years, the set of countries for which data are available decreases from the set of country data available for 5- or 10-year analysis. Rather than limit analysis over the shorter time periods to those countries for which data are available across the entire period, I have tried to use as many countries as possible whenever possible.

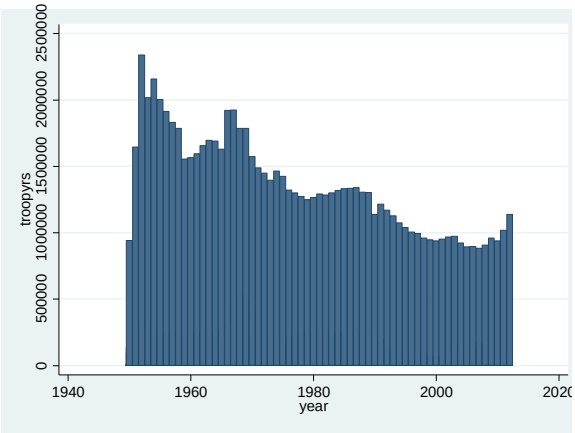

Figure 2-2. U.S. Troop Deployments Over Time

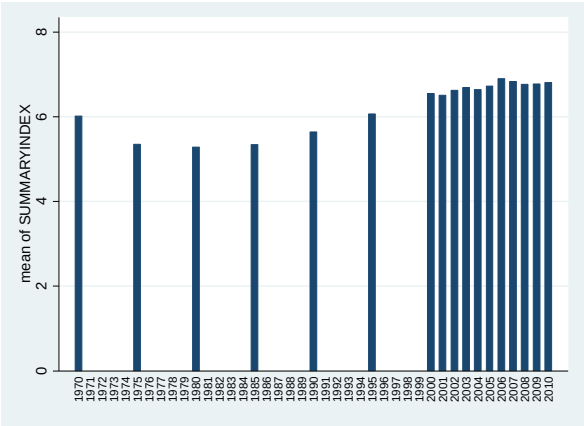

Figure 2-3. Average EFW Scores Over Time

Consistent with contemporary literature, I examine current levels of institutional development as well as change in institutions, consistent with analysis based on Mankiw, Romer, and Weil's growth regression models (1992). Equations (1) and (2) demonstrate the basic differences between the basic level regressions and the basic growth regressions.

$$I_t = \alpha_0 + \alpha_1 \ln(\Sigma Troops_{t,t-30}) + \alpha_2 I_{t-30} + \varepsilon \quad (1)$$

$$\Delta I(1970,2010) = \alpha_0 + \alpha_1 \ln(\Sigma Troops(1970,2010)) + \alpha_2 I_{1970} + \varepsilon \quad (2)$$

Where *I* is the institutional indicator such as the EFWI, CPI, WDI, or Freedom House scores, *Troops* indicates troop-years as reported by DMDC, and $\Delta I(1970,2010)$ represents the change in the institutional indicator between 1970 and 2010. I use the natural log of the sum of *Troops*, because Jones and Kane found this measure to be the most robustly correlated with economic growth.

To mitigate concerns of endogeneity, omitted-variable bias, and reverse causality, the following independent variables are included in the analysis: national conflict, natural

institutional growth over time, additional sources of foreign aid, and democratic governance.

Controlling for National Conflict

To control for national conflict, I use data from the UCDP/PRIO Armed Conflict Dataset. This dataset defines conflict as: "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of the state, results in at least 25 battle-related deaths." It is updated periodically and currently includes conflicts between 1946 and 2013. Although the database may list more than one conflict for any given country in a year, I use a binary indicator of whether or not a country experienced at least one conflict each year.

Table 2-1. Internal Violent Conflicts Reduce EFW Ratings

	Estimated Coefficients *(Standard Error)*	
Conflict	-0.947***	-0.342***
	(0.073)	*(0.076)*
Fixed Effects	N	Y

*=90% confidence, **=95% confidence, ***=99% confidence

Predictably, countries experiencing conflict receive lower institutional scores than those without. Table 1 displays the results of regressions with annual EFWI scores as the dependent variable and conflict as the independent variable, with and without country fixed effects. The mean EFWI score is roughly 6.5, so according to the regression results on the left, countries experiencing conflict receive about 15 percent lower scores than the overall mean. Additionally, when including country fixed-effects, countries currently

experiencing conflict receive about 5 percent lower score than the mean of their EFWI scores. Combined, these results indicate that countries experiencing conflict are generally institutionally worse off than countries not experiencing conflict, and during a conflict, each country has worse institutions than it would otherwise.

If institutional development is assessed based on comparative transactions costs and costs of governance, it is not surprising that indices reflect significantly worse institutions during years of violent conflict within countries. Just as Colaresi and Thompson (2003) find that external threats reduce democracy, it seems reasonable to argue that the effect of almost all national conflicts is an immediately perceived decrease in the limits to government power. One could expect a decrease in institutional indices due to significantly restricted freedoms associated with government's containment of violence. Just as Baltimore enacted a curfew to restore order in the wake of rioting in April 2015, the mechanism described by Colaresi and Thompson operates such that under threat of violent conflict power becomes more centralized to restrict freedoms and maintain order. If U.S. troops deploy into countries when institutional ratings are depressed, this produces a couple of confounding effects.

The first problem is reverse causality. If U.S. troops deploy to trouble spots around the globe, we can anticipate that U.S. military deployments will, in general, be correlated with worse institutions in the short run. Additionally, if depressed institutional performance is due to a specific violent incident, then ratings improve as soon as the conflict is resolved. U.S. soldiers might be excellent peacekeepers, protecting innocent bystanders and maintaining order, but if institutional indices simply return to prior levels

after the conflict is resolved, the improvement in institutional scores should not qualify as evidence of significant institutional change due to U.S. involvement. Thus, care is taken in the interpretation of each set of results, and alternative explanations are considered.

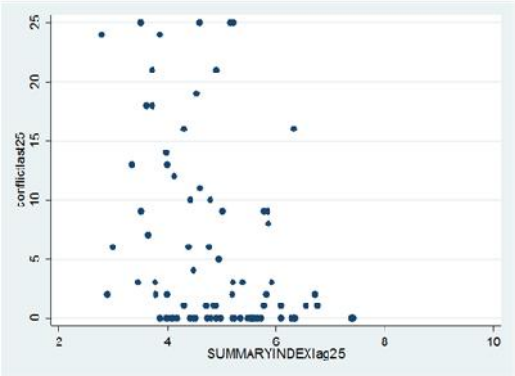

Figure 2-4. Past Institutions Scores and Years of Conflict

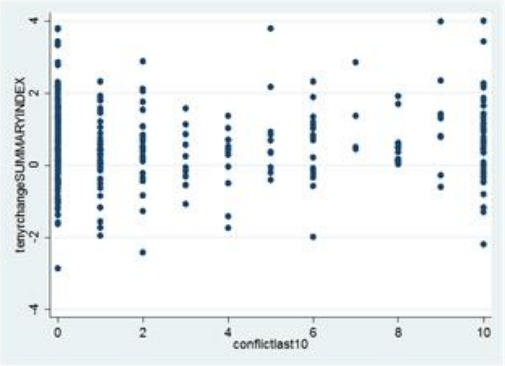

Figure 2-5 Conflict and 10-Yr Institutional Change

While the immediate correlation between internal conflict and institutional ratings seems intuitive and predictable, long run correlations are less so. Figure 4 plots the sum of the last 25 years in which a given country experienced conflict against their EFWI score from 25 years ago on the x-axis. There is an obvious tendency for fewer years of conflict for countries with higher EFWI scores. However, the results in Figure 5 are a little unexpected. This figure plots the 10-year change in EFWI score against the number of years of conflict experienced within the last 10 years. The striking result is that a large number of countries experience significant (even above average) growth despite a majority of years with significant conflict.

So while poor institutions predict conflict, conflict may not prevent long-run institutional growth, even if it is correlated with short run decreases in institutional

ratings. In fact, when one considers that revolution may be required to depose a despot, and changing traditions and cultural expectations frequently causes strife, it could be argued that national conflict may sometimes be a symptom of institutional progress rather than a retardant. For example, when the ruling on *Brown v. The Board of Education* began breaking down the "separate but equal" segregation policies of U.S. states' education systems in 1954 the transitions were anything but peaceful. President Eisenhower famously activated the National Guard in 1957 to protect black children attending the Little Rock, Arkansas, High School. If some foreign national conflicts are a result of similar institutional advancement, the anticipated effect of conflict over time on institutional growth could be positive, if not at least ambiguous.

Table 2 displays the results of regressions further exploring the relationship between conflict and institutional growth over time. The dependent variable is the 20-year change in EFWI score for each country in 2005. The first independent variable is the EFWI score at the beginning of the assessment window. We observe a strong negative effect on institutional growth consistent with convergence as institutionally poor countries catch up. As anticipated, when countries are experiencing conflict at the beginning of the assessment window, their 20-year institutional gains are greater. The magnitude of this effect is considerable. The mean EFWI score is roughly 6.4; a 10 percent increase in EFWI scores over 20 years may just be due to overcoming the conflict at the beginning of the assessment window. While there is not perfect co-linearity between the next two variables, there is certainly a strong positive correlation between the two. Without the total number of years of conflict, current conflict generates a

relatively large and significant negative coefficient. This indicates poor institutional change within the last 20 years when a country is experiencing conflict in the last year of the assessment window. However the significance of current conflict diminishes when we also include the total number of years each country has experienced conflict within the last 20. While theoretically we conjectured that conflict over time could have ambiguous effects, this term generated a statistically significant negative coefficient. However, the magnitude of this effect is negligible, perhaps 1% change in EFWI score over 20 years.

Table 2-2. Cross-sectional Regression - Prior Conflict and 20-Year Change in EFWI Scores

	Estimated Coefficients *(Standard Error)*	
Lagged EFWI Score	*-0.617****	-0.660***
	(0.088)	(0.089)
Conflict 20 Yrs Ago	0.635**	1.047***
	(0.265)	*(0.334)*
Current Conflict	*-0.598***	0.160
	(0.267)	(0.34)
Number of the Last 20 Years with Conflict		-0.070*
		(0.036)

*=90% confidence, **=95% confidence, ***=99% confidence

It is important to note that a significant portion of U.S. military deployments are not associated with conflict. Troops engage in cooperative exercises, training, emergency response, humanitarian missions, and other military operations other than war. For example, marines with specialized chemical, biological, and nuclear incident training were among the thousands of US military deployed to Japan to assist following the

catastrophic earthquake and tsunami in 2011. As another example, while the purpose was conflict related, the deployment of large numbers of U.S. troops to many peaceful NATO countries during the Cold War was independent of any internal national conflicts at the time.

Because lagged conflict, current conflict, and total years of conflict appear to affect the institutional metrics, ignoring these variables as we seek to determine the impact of U.S. military deployments on institutional growth could lead to inappropriate conclusions. If economic growth associated with U.S. troop deployments is associated with institutional diffusion, we should observe positive correlations between institutional change over time and the number of troops deployed. Additionally, we expect the magnitude and significance of troops effects to increase with time.

Institutional Growth Over Time

Figure 3 demonstrates that mean EFWI scores are increasing over time. Table 3 presents the results of two regressions with EFWI scores as the dependent variable and year as the independent variable, with and without fixed effects. These results indicate an average institutional growth rate of a little less than one percent per year.

Table 2-3. Institutional Growth Over Time - EFW Scores

	Estimated Coefficients *(Standard Error)*	
Year	0.046***	0.053***
	(0.0023)	*(0.0012)*
Fixed Effects	N	Y

*=90% confidence, **=95% confidence, ***=99% confidence

The previous section mentioned a strong convergence trend, and it should be noted that average institutional growth in countries with EFWI scores below the mean in 1980 significantly exceeded the average institutional growth in countries above the mean. EFWI scores have a maximum score of 10, and countries approach this maximum score asymptotically. In Figure 6 we observe significant institutional changes in the bottom and middle portions of the distribution, and smaller changes at the top. Thus, not only do we need to be mindful of growth effects over time, we also need to recognize the heteroskedasticity associated with low versus high institutional ratings in previous time periods. While this heteroskedasticity is not likely to bias results, statistical tests become unreliable because variances and covariances will be underestimated. Throughout this paper results will be reported with robust standard errors to mitigate the concerns produced by heterskedasticity.

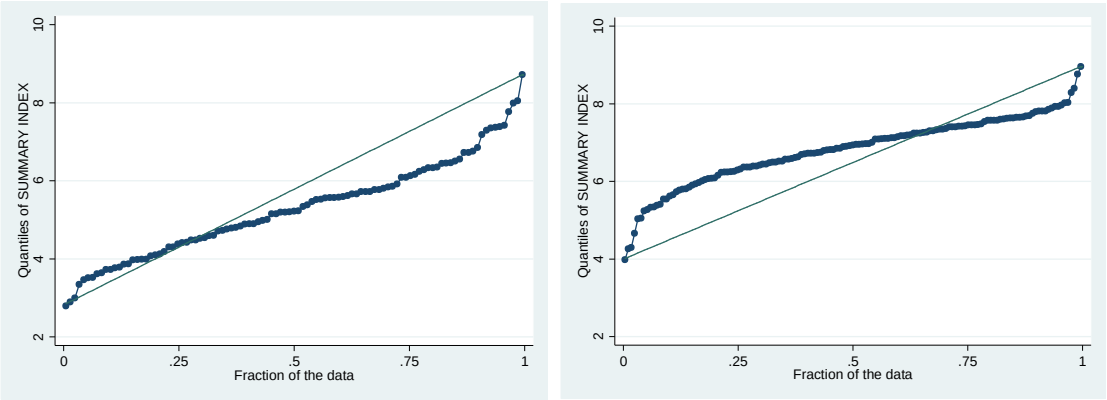

Figure 2-6. Comparing the Distribution of EFW Scores Over Time (Left: 1980; Right: 2010)

The asymmetry of institutional change based on historically lower EFWI scores also has the potential to confound results for much the same reason as with conflict discussed above if more troops deploy to countries with low institution scores; countries with low scores can improve more quickly than those with high scores. Institutional ratings from the first year of each assessment window are included in each of the growth regressions to mitigate such differences in growth effects.

Another potential confounding factor could be associated with the relatively steady reduction in U.S. troop deployments between 1950 and today. There is, of course, some variance in the reduction of global deployments, such as increases for the Vietnam conflicts, large decreases associated with the "Peace Dividend" from the end of the Cold War, and increases for the two Iraq conflicts and Afghanistan. As we observe institutions improving over time, we should expect the number of troops deploying to decrease. However, one cause of reductions in troop deployments is likely to be due to technological developments which change how the military operates. Changing

technological abilities alter business practices which, in turn, may alter institutional diffusion effects. For example, many procurement processes in the U.S. have been automated or moved online, with many of the functions consolidated to central processing facilities. Whereas, in the past, vendors may have had regular personal interaction to review invoices and provide payment, currently contractors in the U.S. submit their bills online. These bills are verified online by receiving organizations, matched against receiving reports (online), and payments are made electronically, potentially without any personal interaction. While this minimizes transactions cost within the U.S. and fits with the U.S.'s current institutional environment, such processes may minimize personal relationships between U.S. troops and foreign contractors or governments. If personal interaction is key to institutional diffusion, new processes may produce different institutional results than previous business practices.

Similar to a North-South growth model (Krugman 1979), the level of development and institutional learning in the U.S. may impact the institutions of the host country. Additionally, the structure of the military changed considerably in 1973 when the U.S. eliminated the draft and achieved an all-volunteer force. Military recruiting standards, leadership practices, and each armed service's culture has undergone continuous development. Inasmuch as troops operated under different sets of incentives, leadership, and culture over the years, troops in different timeframes may also impact host country institutions differently, due either to institutional development within the U.S. and armed forces or to changing business practices as technology develops.

Interaction terms between decades and troop-years will be utilized to evaluate this potential.

One last concern is that institutions may take a considerable amount of time to change, perhaps generations. If military deployments are indeed precursors which lead to and reinforce the progression of international relationships, I also expect that the longer the relationships continue, the greater will be the institutional impacts. And the greater the length of time which passes, the more likely it is to observe institutional proliferation. Therefore, various lengths of time will be considered. At several stages, results for increasing 5-year increments will be considered from 5 to 40 years.

Foreign Aid

A number of studies have found significant economic impacts of foreign aid. Many claim that foreign aid significantly hampers economic growth (Bauer 2000, Djankov, Montalvo and Reynal-Querol 2008, Easterly 2003), while others claim that the key to more growth is more aid (Sachs 2014). While the purpose of this study is not to decipher the impacts of other aid, the significance of previous findings indicate that ignoring other foreign aid in this analysis may lead to omitted-variable bias and spurious conclusions given strong correlations between aid and troops. I use data from the World Development Index (WDI) and include Official Development Assistance (ODA) as a percentage of Gross National Income (GNI) as an independent variable. To identify if troops and aid work synergistically, I create an interaction term by taking the natural log

of the sum of annual products of the annual ODA/GNI and troop years variables over the length of the assessment window.

Democracy

Foreign aid has been shown to create an amplification effect for political institutional development. That is, if the existing government generally promotes democratic institutions, then foreign aid appears to strengthen the development of those institutions. However, if government generally operates by autocratic institutions, then foreign aid seems to promote autocratic institutions and corruption within that country (Dutta, Leeson, and Williamson 2012). Dutta, et al, included characteristics of the existing political regime in their regressions and identified two very different effects of foreign aid. If military deployments can also be considered a form of aid, it is likely that characteristics of the hosting government could have a significant impact on the diffusion of institutions, as well.

Additionally, combining this idea with Pickering and Kisangani's findings regarding supportive versus hostile military interventions yields some interesting considerations. Although Barro contends that democracy slows growth after controlling for other productive institutions, such as public education and well-protected property rights, democracy is highly correlated with many of the institutions which seem to promote growth (Acemoglu, et al, 2014). If democracy promotes the development of better institutions, this portion of analysis seeks to identify if the amplification effect identified by Dutta, et al, is evident from U.S. military deployments in the development of institutions outside of the political realm. Some studies have identified that supportive

deployments to countries with democratic governments improve democratic institutions, and others find that democracy may indeed be promoted by force (Pickering and Peceny 2006). As the first study to specifically examine how U.S. military deployments affect institutional development in host countries, I believe I am also the first to consider whether democratic governance within the host country affects the diffusion of institutional technology from the deployed U.S. military to host countries.

To explore these affects, this study includes dummies for Polity IV scores above zero for each year of the assessment window as an indicator of democratic governance. Interaction terms are created by taking the natural log of the sum of the annual products between these dummies and the number of troops deployed to each country in each year within each assessment window.

Results and Discussion

Preliminary Results

With the issues described above, it is unsurprising that initial results using the basic specification without any other independent variables provide a mixed picture. Table 4 presents the results of four specifications using the model without any other controls such as conflict or democracy. (1) and (2) regress the natural log of troop deployments and EFW scores 30 years prior on the current composite EFW score for each country, without (1) and with (2) regional dummies. (3) and (4) regress the same variables on the 30 year change in EFW composite scores. In specifications (1) and (3), troop deployments appear to have a statistically significant positive correlation with

institutional improvements. However, that relationship seems to disappear when regional dummies are included.

Table 2-4. Basic Specification - OLS Regression on EFWI Levels and Growth - With and Without Regional Dummies

	Estimated Coefficients - Robust Standard Errors *(Standard Error)*			
	2010 EFWI Composite Score		*30 yr Change in EFWI Composite Score*	
	(1)	*(2)*	*(3)*	*(4)*
30-yr Lagged EFWI Score	*0.328****	0.256***	-0.672***	-0.744***
	(0.072)	(0.097)	(0.072)	(0.097)
ln(30yr Sum of TroopYrs)	*0.078****	0.033	0.078**	0.033
	(0.025)	(0.028)	(0.025)	(0.028)
Regional Dummies	N	Y	N	Y

*=90% confidence, **=95% confidence, ***=99% confidence

While early construction of the CPI precludes comparison of scores over time, cross-sectional regressions on the CPI yield similar results as the table above. However, without a lagged institutional score the coefficient on the troop-years variable flips from being statistically significantly positive without regional dummies (coefficient = 2.1 with a t-stat >4) to being statistically significantly negative (coefficient = -1.8, t-stat < -2) when regional dummies are included. The two similar sets of results seem to indicate that regions which have received more troops have developed better institutions, but the specific countries receiving the troops within the region do not. This is consistent with the conflict discussion above. If troops deploy to troubled countries within regions, we should expect their institutional ratings to be lower. It is also possible that regions benefit institutionally if the presence of U.S. troops increases regional security and improves relations between countries due to proximity rather than direct interaction. Figure 7

displays the average number of troop-years per country deployed within the various regions in our dataset. The large number of troops deployed to Europe and the East Asia Pacific (EAP) regions may be responsible for the significant coefficients on the troop-years independent variables in the regressions not controlling for regional dummies. Once this regional growth is controlled for, U.S. troop deployments appear to have little influence. The fragility of the correlation between troop-years and institutional development in Table 4 demonstrates the need to include the various independent variables discussed in the previous sections in order to more clearly decipher how troops affect institutions. Additionally, a Ramsey Reset Test on these regressions generated an F-statistic of 2.78 which correlates with about a 95 percent probability omitted variables are affecting the regression results.

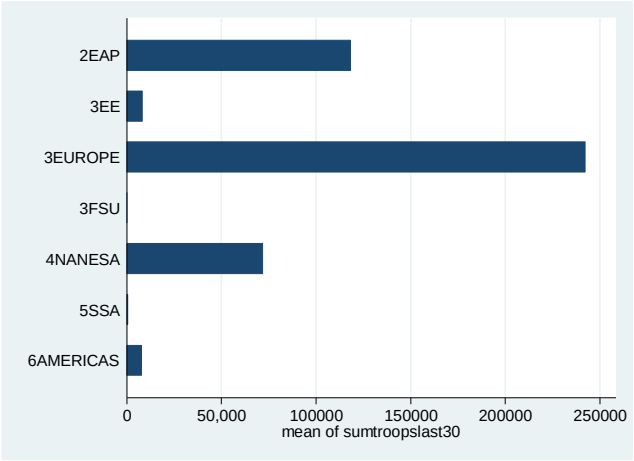

Figure 2-7. Average 30-Year Sum of Troops by Region

Table 5 displays the results of regressions exploring the relationship between troop deployments and EFWI composite scores over increasing five-year increments. EFWI data begin in 1970. Thus, there are eight non-overlapping five-year windows, four ten-year windows, and two 15- and 20-year windows. Results in this table are limited to timeframes which allow fixed-effects regressions. Including fixed-effects in the regressions and exploring different length assessment windows generated a statistically significant negative correlation between troop deployments and institutional development in the 20-year regression. This is notable, because while we might expect the short-run correlation to be negative, if troops encourage institutional diffusion over time, then the coefficients on longer periods should be more likely to be positive.

Table 2-5. Change in EFWI Score With Increasing Assessment Window Length - Non-Overlapping Windows

	Estimated Coefficients - Country Fixed Effects, Robust Standard Errors *(Standard Error)*			
	5-Year	*10-Year*	*15-Year*	*20-Year*
Lagged EFWI Score	-0.264***	-0.482***	-0.785***	-1.145***
	(0.022)	(0.039)	(0.061)	(0.195)
ln(Sum of TroopYrs)	-0.013	-0.001	0.010	-0.200***
	(0.026)	(0.055)	(0.064)	(0.059)

*=90% confidence, **=95% confidence, ***=99% confidence

Because the regressions in Table 5 use the change in institutional score, the negative sign on the lagged EFWI score coefficients is consistent with the institutional convergence identified in Figure 6. Countries with high institutional scores have less room to improve on the scale. For the five-year regression, this coefficient indicates that a one point increase in the EFWI score at the beginning of the assessment window correlates with one quarter of a point less than average improvement in EFWI score by

the end of the window. Correspondingly, a one point increase in EFWI score at the beginning of a 20-year assessment window predicts a little more than one point less than average improvement in EFWI score. It is interesting to note the increase in both magnitude of the coefficient and the standard error on the lagged EFWI score. This indicates that the longer the time frame, the greater the variation in scores, but also the greater the relative improvement of the slower developing countries; more time predicts more convergence. Both of these findings lend credibility to the construction utilized in this analysis.

The negative and statistically significant coefficient on the troops variable in the twenty-year regression indicates that, generally, the more troops a country has hosted in a twenty-year period, the worse their institutional development was. Because this is the natural log of the sum of troops, this indicates that a 100 percent increase in troops generates about -0.2 less points of improvement in EFWI score over the twenty-year period. As of yet, this finding does not indicate that troops reduce growth, just that their presence is correlated with less growth, perhaps due to the circumstances of their deployment, which is the subject of the investigations to follow.

Results With Conflict And Time Controls

Adding the three measures of conflict (conflict in the year beginning the assessment period, conflict in the year ending the assessment period, and total years of conflict during the assessment period) to the regression does not produce significant changes is the results above. Table 6 summarizes the results of fixed-effects regressions

across the previously used assessment period lengths including the conflict measures as independent variables. There are strong correlations between the three conflict variables, and dropping any one of the three changes the statistical significance of the other two. However, the coefficients for the independent variable of interest, troop-years, are unaffected.

As noted, not only does including conflict not change the significance of U.S. troop involvement, but increasing the length of the assessment window also has marginal effects. Additionally, regressions considering each decade of troop deployments as independent variables yielded no statistically significant coefficients for troops when regional dummies and conflict variables were included. Thus, it does not appear that variations in troops' impacts over time are confounding the findings above.

Table 2-6. Change in EFW Score With Increasing Assessment Window Length and Conflict - Non-Overlapping Windows

	Estimated Coefficients - Country Fixed Effects, Robust Standard Errors *(Standard Error)*			
	5-Year	*10-Year*	*15-Year*	*20-Year*
Lagged EFWI Score	-0.267***	-0.487***	-0.758***	-1.188***
	(0.023)	(0.042)	(0.066)	(0.204)
ln(Sum of TroopYrs)	-0.0088	0.019	0.0088	-0.157
	(0.024)	(0.063)	(0.066)	(0.166)
Final Year Conflict	-0.210	-0.140	-0.432	-0.562
	(0.136)	(0.274)	(0.419)	(0.86)
Base Year Conflict	0.227	0.610*	-0.354	0.744
	(0.177)	(0.362)	(0.349)	(1.30)
Sum of Conflict	-0.010	-0.036	0.0532*	-0.0125
	(0.052)	(0.063)	(0.032)	(0.063)
Number of countries	134	118	117	108

*=90% confidence, **=95% confidence, ***=99% confidence

Regressions over a forty-year assessment window delivered similar results when regional dummies are included. However, the thirty-year assessment window ending in 2010 returned a statistically significant and positive coefficient (coefficient = 0.104, standard error = 0.0374) for the troop-years independent variable when the conflict variables were included with regional dummies and robust standard errors. The significance of this finding is weakened considerably by the lack of significance identified in any of the other specifications. Thus, any conclusions drawn from this finding could be spurious. Additionally, the significance of the 30-year coefficients following the procedures in the following sections was not robust to the inclusion of additional explanatory variables.

While controlling for conflict by itself does not seem to change how troop deployments appear to affect institutional development, Table 7 explores the interaction of troop deployments with conflict. An interaction term was created by multiplying the troop-years term by the annual conflict term and summing over the assessment window. Comparison with the coefficient on the sum of conflict variable demonstrates that institutional development may be significantly different in countries when U.S. troops are deployed in countries with conflict. The statistically significant negative coefficients on the interaction terms identify a strong negative correlation between troops deployed during years with conflict and change in EFWI scores. At this stage it is unclear whether this is due to more significant conflicts or whether troop deployments prevent institutions from improving. The significance of the coefficient was not robust when the natural log

of troops was used rather than the total number of troops, though the sign continues to be negative.

Perhaps the most striking finding from Table 7 is that while the general effect of troop deployments remains inconsistent with two negative coefficients and one statistically significant positive coefficient, the interaction of troops and conflict generates a consistently significant negative correlation between troops deployed in countries with conflict and institutional growth. Again, it could be argued that the U.S. only deploys to areas with the worst conflicts; however, this interaction term appears in the same regressions as insignificant coefficients on troop deployments without conflict. If including conflict in the interaction term absorbs the suppressed institutional growth due to conflict, then the positive effects of troop deployments without conflict should become more visible. This is contrary to the findings in Table 7. Thus, this table provides no support for an argument that U.S. troops improve institutions in the places they deploy to, and strengthens an argument to the contrary.

Table 2-7. Change in EFWI Score With Interacting Troops and Conflict - Non-Overlapping Windows

	Estimated Coefficients - Country Fixed Effects, Robust Standard Errors *(Standard Error)*			
	5-Year	*10-Year*	*15-Year*	*20-Year*
Lagged EFWI Score	-0.262***	-0.495***	-0.539***	-1.22***
	(0.023)	(0.041)	(0.072)	(0.197)
Sum(TroopYrs)	-1.3E-7	4.4E-8	1.94E-7***	-4.86E-8
	(1.5E-7)	(8.4E-8)	(5.54E-8)	(7.0E-8)
Sum(Conflict * TroopYrs)	-5.4E-6***	-4.9E-6***	-1.72E-6**	-4.2E-6***
	(1.5E-6)	(1.76E-6)	(7.5E-7)	(6.7E-7)
Sum of Conflict	-0.025	-0.036	0.0532*	-0.031
	(0.051)	(0.062)	(0.032)	(0.049)
Final Year Conflict	-0.094	-0.121	-0.637**	-0.236
	(0.148)	(0.270)	(0.303)	(0.52)
Base Year Conflict	0.203	0.603*	0.831**	1.12
	(0.167)	(0.363)	(0.387)	(0.95)
Number of countries	134	120	120	110

*=90% confidence, **=95% confidence, ***=99% confidence

Results When Other Foreign Aid is Included

Table 8 summarizes the results of fixed-effects regressions with robust standard errors using the change in EFWI score as the dependent variable, including conflict, foreign aid, and the natural log of the sum of troops deployed within the assessment window as independent variables, and with the assessment window ranging from five to twenty years. The right column for each assessment window includes an interaction term created by multiplying the average ODA/GNI ratio over the assessment window by the natural log of the sum of troops deployed to that country during the assessment window. During my presentation of this paper at the annual Advancing Public Policy Conference in 2015, the discussant in my session suggested that the aid and troop variables might work in concert with each other, and troops might be important for ensuring that aid gets to where it needs to be. This interaction term is employed to examine if troops and aid work differently together than independently.

Table 2-8. Change in EFW Score: Various Assessment Window Lengths, Conflict, Troops, and Aid - Non-Overlapping Windows

	Estimated Coefficients - Country Fixed Effects, Robust Standard Errors *(Standard Error)*							
	5-Year		*10-Year*		*15-Year*		*20-Year*	
Lagged EFWI Score	-0.225***	0.224***	-0.457***	-0.449***	-0.540***	-0.785***	-1.142***	-1.187***
	(0.023)	(0.024)	(0.042)	(0.044)	(0.074)	(0.072)	(0.201)	(0.211)
ln(Sum of TroopYrs)	-0.0267	-0.0286	0.0168	-0.0004	0.128	0.120**	-0.109	-0.0237
	(0.025)	(0.026)	(0.065)	(0.068)	(0.093)	(0.060)	(0.113)	(0.111)
Average Aid/GNI	0.0220***	0.184	0.0543**	0.146	0.0670	0.197***	0.0978	0.746**
	(0.0084)	(0.013)	(0.024)	(0.032)	(0.041)	(0.068)	(0.065)	(0.311)
Aid*Troops		0.0010		0.0098		-0.0388***		-0.127**
		(0.0042)		(0.009)		(0.0131)		(0.063)
Base Year Conflict	0.158	0.160	0.527*	0.549**	1.105***	-0.212	1.106	0.930
	(0.128)	(0.129)	(0.270)	(0.262)	(0.33)	(0.27)	(1.031)	(1.027)
Sum of Conflict	-0.022	-0.023	-0.0417	-0.044	-0.0953***	0.040	-0.0393	-0.0193
	(0.040)	(0.04)	(0.045)	(0.044)	(0.036)	(0.027)	(0.047)	(0.052)
R-squared (overall)	0.087	0.088	0.154	0.157	0.161	0.284	0.307	0.235
N	134	134	118	118	118	117	108	108

*=90% confidence, **=95% confidence, ***=99% confidence

In each of the left columns (by assessment window length), we again observe no statistically significant coefficients on the deployed troops variable. It is interesting to note, however, that other foreign aid returns a positive and statistically significant coefficient in the five and ten-year regressions. This is contrary to expectations given the findings of Bauer and Easterly mentioned above. It should be noted that the size of these coefficients is very small, though. A 100 percent increase in aid to GNI ratio resulting in a 0.03 to 0.06 point increase in EFWI score. The significance of these coefficients deteriorates in the fifteen and twenty-year regressions.

The results generated by the regressions which include the interaction term include several significant coefficients and some interesting trends. The first item to note is that by including the interaction term, the aid/GNI variable increases in magnitude and becomes more significant in the longer assessment windows. This demonstrates one reason why previous studies may produce conflicting results regarding the effects of aid.

Because the majority of the previous studies have not considered the accompaniment of the provided aid with troops, their results may suffer significantly from omitted variable bias. This provides motivation to re-examine the effect of aid.

The coefficient on the aid*troops interaction term is interesting for two reasons. First, it is significantly negative in the longer assessment windows. Second, it appears to be increasing in magnitude over time. If economic growth associated with troop deployments is due to institutional diffusion, this sign is exactly opposite of what should be expected for a number of reasons. Aid and troops should be going to countries with the worst institutions, thus they should have the most improvement potential. Additionally, if troop deployments are supportive of local government, then we should expect countries receptive to troops to be more compliant with our policies and amicable to our institutions. One might expect such conditions to be associated with accelerated institutional diffusion. Alternatively, if deployments are conflicting with local government, we might expect that the resulting government after an invasion by US troops could be institutionally sounder than whatever regime was being replaced. Otherwise, such invasions are counter-productive.

The increasingly negative interaction term as the length of the assessment window increases also runs contrary to expectations. If troops deploy to areas with poor institutions, we might expect correlations to be negative in the short-run. However, if troops aid in institutional diffusion, more aid and troops over a longer time should assist in overcoming the poor conditions. One could argue that as institutions improve, less aid and troops are required. Thus, those which improve the quickest receive the least troops

and aid. However, the positive coefficient on the aid variable indicates that more aid improves institutions. The negative coefficient on the interaction term indicates that countries receiving aid and troops do worse than those which just receive aid.

Results Including Democracy

According to Acemoglu and Rodrik, we should expect that democracy should improve institutions. Similar to Table 8, Table 9 includes the results of fixed-effects regressions with robust standard errors over the four previously used assessment window lengths, from five to twenty years. In each case the dependent variable is the change in EFWI score over the assessment window, and the independent variables include a lagged EFWI score, the natural log of the sum of troops deployed during the period, the total number of years during the assessment window that the country received a Polity IV score over zero, and an interaction term generated by taking the natural log of the sum of troops deployed to a country in years during which their Polity IV score was greater than zero.

Table 2-9. Change in EFWI Score: Various Assessment Window Lengths, Troops, and Democracy - Non-Overlapping Windows

	Estimated Coefficients - Country Fixed Effects, Robust Standard Errors *(Standard Error)*							
	5-Year		*10-Year*		*15-Year*		*20-Year*	
Lagged EFWI Score	-0.341***	-0.340***	-0.580***	-0.580***	-0.904***	-0.904***	-1.186***	-1.179***
	(0.025)	(0.025)	(0.050)	(0.050)	(0.066)	(0.067)	(0.134)	(0.138)
ln(Sum of TroopYrs)	0.0087	0.0027	0.0288	0.0195	0.0695	0.060	-0.0835	-0.0771
	(0.025)	(0.029)	(0.062)	(0.068)	(0.053)	(0.055)	(0.060)	(0.0602)
Sum of Years w/ Dem	0.167***	0.155***	0.123***	0.114***	0.109***	0.105***	0.115***	0.122***
	(0.024)	(0.032)	(0.023)	(0.029)	(0.019)	(0.023)	(0.019)	(0.022)
Democracy*Troops		0.0130		0.0415		-0.0360		-0.0669
		(0.027)		(0.09)		(0.095)		(0.17)
R-squared (overall)	0.1	0.0995	0.167	0.163	0.329	0.321	0.293	0.301
N	134	134	118	118	117	117	108	108

*=90% confidence, **=95% confidence, ***=99% confidence

The sign of the coefficients on the democracy variable are all positive, significant, and of roughly the same magnitude. In each case, an additional year of democracy is associated with an increase in EFWI score of a little greater than 0.1 points.

To be consistent with the findings of Dutta, et al, we should observe a statistically significant, positive coefficient on the interaction of troops and democracy, and a negative term on the troops independent variable when they are included in the same regression. This would identify that when troops assist countries who already have democratic governance, institutions progress more quickly, but more slowly when deployed to countries without democratic government. This is contrary to the results displayed in Table 9. In fact, removing the independent troops variable from the regression and only including the interaction term also yields no statistically significant relationship between troops and institutional development. It appears from this construction that any effect that troop deployments may have is not correlated with democratic government of the country when troops deploy.

Summary and Conclusions

To recap, I first identified differences in regressions with and without regional dummies and country fixed-effects. This indicated that regions which have received more troops have developed better institutions, but the specific countries receiving the troops within the regions do not appear to be affected differently than the other countries in the region. The exception identified in Table 5 was a statistically significantly negative coefficient in the twenty-year fixed-effects regression, indicating that the countries receiving the most troops appeared to improve the least.

When three conflict indicators were included in regressions, the coefficient on troops in the 30-year regression appeared to be positive and statistically significant, but the significance of this coefficient was not robust to the inclusion of other explanatory variables in subsequent sections. When regressions included an interaction variable between troops and conflict, the interaction term returned a statistically significant negative coefficient, indicating that countries receiving U.S. troops in years with conflict improved less quickly than other countries. The significance of the interaction coefficient was in contrast with statistically insignificant conflict and troop deployment coefficients.

I then presented the results of regressions which included controls for foreign aid and troops. The results in Table 8 indicated that when an interaction term between the ratio of ODA/GNI and number of troops deployed was included, the contribution of aid became more significantly positive, and the troops*aid interaction term was significantly negative in the longer fixed-effects assessment windows.

Results in the Democracy section indicate that democracy contributes significantly to institutional development, with each year of democratic rule correlating with about a 0.1 point improvement in EFW score. This finding may seem intuitive and expected, however, considering that democracy should be correlated with better institutions in the first place, and potentially less room in the institutional scale to improve, the consistent and significantly positive coefficient is noteworthy. Including democracy as an independent variable and in an interaction term with troops did not improve the correlation between troops and institutional improvement.

Each of the sections above has focused on change in EFWI score as the indicator of institutional progress. Additionally, fixed-effects regressions have been employed to examine the differences within individual countries when troops are present. If institutions improved more quickly when troops were present, this would be evidence of institutional diffusion. The results of the sections listed above appear to almost directly contradict this hypothesis. This is remarkable given the strong correlation between economic growth and institutional development and the strong correlation Jones and Kane identify between U.S. troop deployments and economic growth. These findings suggest that economic growth associated with troop deployments is most likely not driven by institutional diffusion, and growth is facilitated by some other factor.

Recommendations For Further Study

While the results of this analysis suggest that institutional diffusion is likely not the primary driver of economic growth associated with military deployments, potential

confounding factors remain. One potential issue is the prolific deployment history of U.S. military forces. Since 1950, troops have deployed to the vast majority of countries throughout the world. And while this study utilized controls to keep track of internal conflict, democracy, and foreign aid, a myriad of other issues influence the mission, effectiveness, and relationships of U.S. troops within the host country. Additionally, much of this analysis has considered the deployment of troops with the perspective that the purpose of the deployment is to assist the host country. And while this may frequently be the case, it should be pointed out that the overriding purpose of the U.S. military is to insure the security of U.S. personnel and U.S. national interests. Thus, any economic growth within a host country associated with troop deployments is a secondary objective or effect. It is possible that, given significant limitations of general institutional indices to identify multifaceted and intricate elements of institutional development, this analysis is simply too coarse to facilitate identification of institutional effects. Thus, further research exploring other elements of economic institutions, such as contracting practices and enforcement costs, capital/labor ratios, entrepreneurial efforts, or other indicators of shifting transaction costs may provide increased insight into institutional effects of U.S. troop deployments. A regional sampling and detailed historical case study approach may also be more fruitful in identifying institutional diffusion.

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3. DO US TROOP DEPLOYMENTS ENHANCE DEMOCRATIC INSTITUTIONS?

Introduction

A number of studies have identified limited positive impacts of U.S. military intervention on the development of democracy in target countries. For example, Meernik (1996) uses specific instances of U.S. military intervention to identify an increased propensity for democratic development in countries in which we have previously intervened. Hermann and Kegley (1998) and Peceny (1999) identify increased democratization from military interventions within qualified conditions. Pickering and Peceny (2006) claim to "find little evidence that military intervention by liberal states helps to foster democracy in target countries," but this claim is qualified with an acknowledgement that a few cases of hostile US intervention have led to increased democratization. In *After War*, Coyne identifies success rates of less than 30 percent success rate in promoting democracy within the ten years following conflicts resolved through U.S. military intervention (2008).

Frequently, these analyses utilize index scores such as Polity or Freedom House to identify progress toward democracy and protection of individual freedoms. One potential complication arises if the analytical construct using the Polity Index implies an ordinal structure to the scores reported in the Index. The developers of the index make it clear that multiple decision factors are included in the construction of the index score,

and individual countries may progress toward democracy along a variety of paths. The creators of the Polity Index coin a new term, "Anocracy," to describe the murky mix of political institutions lying between autocracy and democracy, and it is unclear that movements toward anocracy demonstrate real progress toward democracy. Some analysts may inappropriately interpret shifts along the index as progress toward or away from democracy which could bias results for or against the military. To avoid potential biases associated with any perceived ordinal ranking of polity scores, this study uses multinomial logistical regressions to analyze the probability under various conditions of a country finding its way to democracy from the various sections along the Polity spectrum.

Additionally, I claim that significant social institutional development is required for the establishment and sustainment of democracy. As Meernik notes, "democratic institutions cannot be created overnight… (1996)." As such, claims that large short-run democratic gains might be attributable to U.S. military involvement seem dubious. As Blechman and Kaplan note, military action may lead to dependence on the U.S. for national and even regional stability (1980). And while the victorious invading army may impose government on the conquered nation, it seems likely that the sustainment of democracy might necessitate some level of conversion of the populous to the ideals of the newly imposed government. Thus, if any real democratic growth is attributable to U.S. military involvement, it might come through sustained organic exposure to democratic principles. This exposure could occur through interpersonal communication, personal experience, or learning within government agencies. Sufficient exposure seems unlikely

to be generated by one-time military interventions, but more likely through a sustained presence and international relationship over an extended period of time. Of course, this type of long-term relationship may be at odds with the Clausewitzian principles of successful military operations, which usually prescribe quick and decisive military action. However, modern doctrine of deterrence and containment has necessitated a significant global presence of U.S. military throughout the post-WWII era. The vast majority of countries have hosted U.S. troops within the last 60 years, and countries which have not directly hosted U.S. troops may have enjoyed increased international security within their regions because of U.S. troops deployed close by. Whereas previous literature focuses on individual military interventions, this analysis uses the complete record of U.S. troop deployments to each country between 1950 and 2012 to analyze the influence of military relationships over time.

Analysis will be presented in two stages. First, I construct a typical OLS fixed-effects regressions model using panel data to confirm the findings of previous literature and explore the potential of democratizing influences over time. Second, I use a categorical dependent variable to conduct multinomial logistic regressions and various statistical tests to examine the probability of various transitions between the dependent variable's categories given various independent variables, including U.S. troop deployments.

Democracy Background

The 2015 U.S. National Security Strategy includes the following objective: "We will advance respect for universal values at home and around the world by…promoting and defending

democracy, human rights, and equality while supporting countries such as Tunisia and Burma that are transitioning from authoritarianism." Following suit, the 2015 U.S. National Military Strategy claims that the military will seek to "[uphold] our enduring commitment to the advancement of democracy and human rights and [build] new coalitions to combat corruption and to support open governments and open societies. In doing so, we are working to support democratic transitions…" The document also lists as one of the military's objectives to "Support Emerging Democracies." The mandate for the military to facilitate the expansion of democracy is certainly not a new national security objective. As early as 1898, the U.S. military assisted to control the Philippines in preparation for self-rule under democracy (Dolan 1991), and containment of communism and promotion of democracy drove U.S. involvement in the Korean, Vietnam, and Cold Wars.

Research exploring the success of military operations has a rich and broad history, and a number of authors have examined whether the utilization of military force can promote democracy. Classical pieces of war literature such as Sun Tsu and Clausewitz share military ideals of swift movement and decisive action for military victory. Modern researchers confirm the importance of defined objectives and unity-in-command (Summers 1982, 1992), and others find that domestic political support (George, et al 1971) and other signals of significant commitment, such as ground troops rather than naval troops (Blechman and Kaplan 1980), predict greater likelihood of success in achieving stated objectives. Promoting democracy seems to be an elusive objective, though, as a number of researchers identify that the path to democracy appears to be long and complex (Meernik 1996). One complicating factor seems to be that military conflict is generally correlated with a centralization of decision-making power and a reduction in personal freedoms as the government under attack seeks greater control to deal with the conflict (Colaresi

and Thompson 2003). Thus, in the short-term the threat of U.S. military action could prevent decentralization of power and result in the contraction of personal freedoms.

Regan (2002) and Lemke and Regan (2004) contribute an intriguing and related line of research by focusing on outside intervention specifically in civil wars. Consistent with intuition, when third-parties provide military assistance to the existing government, the government resolves the war more quickly. When third-parties assist rebellions, the rebels have a greater likelihood of prevailing. However, assisting the insurgents also increases the scale of the conflict, the level of violence, and the length of the war. Pickering and Kisangani use this theoretical foundation as they use the International Military Intervention (IMI) Database to identify the disparate effects of supporting interventions vs hostile interventions (2006). They find that supportive interventions appear to strengthen autocracies and hostile interventions can have immediate and long-lasting democratizing effects on non-democratic countries (citing the U.S. invasion of Panama in 1989 as an example). However, their use of the Polity index as dependent variable may bias their findings.

Considering the advancement of democracy from an alternative perspective, Barro (1989) finds that expanding political rights are positively correlated with economic growth. Glaeser, et al (2004), identify two streams of competing literature regarding this evolution of political institutions and economic growth. On one hand, he states that institutional economics research has reached close to intellectual consensus that political institutions of limited government power cause economic growth. The other position he attributes to Lipset (1959), who in turn credits Aristotle, is that political institutions of limited government power are a result of increased human capital among the population who then impose their will and demand more limited power for government. Such limits

to government power are closely linked with democracy. Alesina, et al, find no evidence that economic growth occurs more rapidly under democracy than under autocracy, but they find considerable evidence that political instability significantly retards economic growth (1996). Additionally, they find that political instability is persistent. Helliwell concurs with Alesina, that there is little evidence of increased economic growth under democracy, but the effect of economic growth on democracy appears to be strong and robustly positive (1994). Coincidentally, Jones and Kane (2012) find that the natural log of U.S. troops deployed to a target country is strongly correlated with increased economic growth within that country. If U.S. troops contribute to political stability, Alesina's and Helliwell's conclusions may explain Jones and Kane's findings.

Additionally, the links between political stability and economic growth, and economic growth and democracy, in combination with the persistence of political instability described by Alesina and impacts of third-party involvement in civil wars described by Regan and Lemke, could have prescriptive implications for the use of U.S. military power. Counterintuitively, supportive deployments to autocratic states could foster political stability and economic growth, promoting democracy in the long run. If these mechanisms function as outlined, such phenomena should be evidenced by an increased rate of transition to democracy among countries hosting U.S. troops.

While many recognize the difficulty in measuring democratic progress (Bollen 1993), the most frequently utilized measure I've observed in contemporary literature comes from the Polity Projects. The Polity Index scores each country's democratic development on a scale of -10 (hereditary monarchy) to 10 (full democracy). Within this

construction, countries with scores ranging from -10 to -6 are said to be autocratic, -5 to 5 are anocratic, and 6 to 10 are democratic. This construction may seem to imply that any movement in the positive direction is a move toward democracy. However, the Polity database also includes a number of cases in which the transition to democracy occurred as a relatively quick, one-time jump from autocracy to democracy (such as Benin, South Korea, and Greece), while other countries languish in anocracy (Haiti, Tajikistan, and Tunisia). Additionally, the Polity IV project has identified that as countries move toward anocracy from autocracy, a positive shift along the Polity scale, the probability of significant political violence increases (http://www.systemicpeace.org/polity/PTfig02.htm). Thus, if U.S. military involvement is correlated with decreasing the stability of otherwise stable monarchies but significantly increasing political violence and oppressive actions, this could be viewed as "progress" from a Polity score perspective, but might be the opposite of overall mission objectives. Because of this potential, we should be more interested in the movement to democracy than along the Polity index.

Research Problem

The primary purpose of this study is to determine if statistical evidence supports a claim that U.S. troop deployments improve democratic development in countries hosting troops. To accomplish this objective, I consider two potentially confounding aspects of existing literature, (1) independent variables which focus only on specific military interventions rather than long-term relationships, and (2) a dependent variable with may be used inappropriately to measure democratic progress. I seek to address these issues in

two stages: first, by utilizing a more comprehensive record of military deployments than has previously been utilized, and second, by using the Polity index as a categorical dependent variable of democratic status rather than an ordinal scale of democratic development.

Data and Method

To answer the question regarding how US troop deployments impact the democratic institutions of the countries they deploy to, I present two analytical constructions. First, a typical OLS fixed-effects regression model is employed to analyze a panel of data assembled from the Defense Manpower Data Center's (DMDC) troop deployment dataset (1950-2012) compiled by Kane and Jones, selected fields from the Penn World tables, the World Bank (World Development Indicators, WDI), the Economic Freedom of the World Index, the Polity IV Project, and the UCDP/PRIO Armed Conflict Dataset. Controlling for various explanatory variables, I examine correlations between the number of U.S. troops deployed to a target country and the probability of the target country experiencing a positive change in *Polity* score.

For the second portion of analysis, I use a categorical dependent variable implied by Polity Index scores and, through a multinomial logit analysis and chi-squared goodness-of-fit tests, re-examine the correlation of U.S. military troop deployments on the likelihood of transitioning between five categories of political status defined by the Polity Index: autocracy, closed anocracy, open anocracy, democracy, and other. If regressions produce significant t-statistics for estimated coefficients on the troop deployments independent variable, and chi-squared goodness-of-fit tests yield

statistically significant statistics, this provides statistical evidence in favor of a claim that U.S. troops help democratic development.

The Defense Manpower Data Center (DMDC) publishes annual troop deployment data by indicating the number of "troop years" the US deploys to each country. Similar to work-years, each troop-year is equivalent to one soldier deployed there for one year, but it needn't be the same soldier that stays there the whole time. The measure is additive, and does not indicate duration of deployments. Thus, one troop-year could indicate a 12-man team deployed there for one month or a set of soldiers deployed there individually on a rotational basis for the year. Although troop-years provide limited insight into volume versus duration over each year, considering institutional diffusion, learning takes time and increasing the number of troops increases exposure. I expect the tradeoff between time and exposure to be of little consequence and ignore any duration-versus-volume effects potentially masked by the statistic.

Various issues limited the availability or analysis of data. Several regions changed dramatically over the domain of years included in this analysis. For example, the former Soviet Union collapsed into variants of previous former countries, East and West Germany reunited, Czechoslovakia peacefully separated into the Czech Republic and Slovakia, and after significant conflict Yugoslavia eventually separated into five countries, Serbia, Montenegro, Croatia, Slovenia, and Bosnia and Herzegovina. Additionally, DMDC makes various changes each year in the format of the data it publishes, disrupting the continuity of the time-series, for example, sometimes reporting a couple small countries together and reporting separately at other times. To resolve these

issues, I have tried to link transitioning countries over time, in all cases seeking to maximize the sample of countries used in the analysis.

The most significant adjustment to the data from previous studies is the consideration of the four categories of political development implied by Polity Index scores as a categorically distributed dependent variable rather than an ordinal scale. Based on their annual Polity scores, countries are designated autocracies (-10 to -6), closed anocracies (-5 to 0), open anocracies (1 to 5), democracies (6 to 10), and other (-88, -77, -66: countries in transition, with interruptions or interregna between regimes). Provided by the Center for Systemic Peace and the Polity Project (http://www.systemicpeace.org/ polityproject.html), Figure 1 demonstrates the dramatic swings the world has seen in global governance. My analysis covers just the last quarter of the time period represented in this figure, 1950-2012. Perhaps the most notable event in the expansion of democracy within the last 200 years, the dashed vertical line in the figure indicates the collapse of the Soviet Union and the global shift in democracy accompanying it.

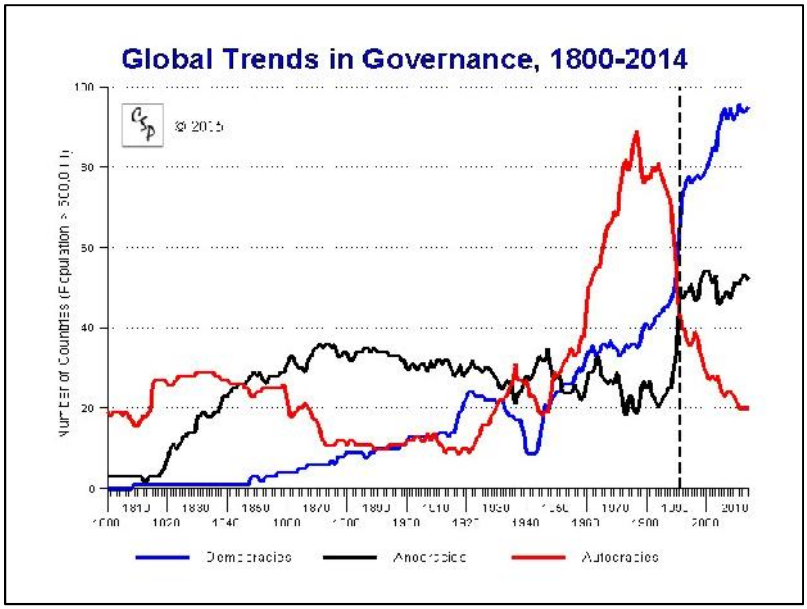

Figure 3-1. The Global Shift to Democracy (http://www.systemicpeace.org/polity/polity1.htm)

In the first portion of analysis, I examine current levels of democratic development as well as change in *Polity* score, consistent with analysis based on Mankiw, Romer, and Weil's growth regression models (1992). Equations (1) and (2) demonstrate the basic differences between the basic level regressions and the basic growth regressions.

$$P_t = \alpha_0 + \alpha_1 \ln\left(\Sigma Troops_{t,t-30}\right) + \alpha_2 P_{t-30} + \varepsilon \quad (1)$$

$$\Delta P(1970,2010) = \alpha_0 + \alpha_1 \ln(\Sigma Troops(1970,2010)) + \alpha_2 P_{1970} + \varepsilon \quad (2)$$

Where *P* is the *Polity* score, *Troops* indicates troop-years as reported by DMDC, and $\Delta P(1970,2010)$ represents the change in *Polity* score between 1970 and 2010. In this example, I show the natural log of the sum of *Troops*, because Jones and Kane found this measure to be the most robustly correlated with economic growth; however, other

measures of troops will be also considered, including the sum of troops, a binary indicator of military presence, and threshold levels of 10, 100, and 1000 troops.

To mitigate concerns of endogeneity, omitted-variable bias, and reverse causality, an additional vector of independent variables is also included in the analysis: national conflict, economic growth, and additional sources of foreign aid.

The second portion of analysis uses the same panel of data and examines transitions between categories of political regime type. I construct two additional variables, one identifying the path from year to year of each country's regime type, and one identifying transitions resulting in at least three consecutive years within any of the five categories.

To construct the first variable, countries are grouped into the five categories previously mentioned. Negative ten to negative six are *Autocracies* (A), negative five to zero are *Closed Anocracies* (C), one to five are *Open Anocracies* (P), six to ten are *Democracies* (D), and anything else (-66, -77, -88) are *Others* (O). Transitions between years are represented by listing the previous year's category as the first letter, and the following year's category at the second letter. For example, starting in *Autocracy*, transitions include staying in *Autocracy* (A to A, or AA), or moving to *Closed Anocracy* (A to C, or AC), *Open Anocracy* (A to P, or AP), *Democracy* (A to D, or AD), or Other (A to O, or AO). A Markov matrix is constructed using the population of all annual transitions. The probability of any transition between categories is calculated by dividing the number of specific transitions from a category (e.g. AD) by the total number of transitions starting in a given category (AA + AC + AP + AD + AO). Samples of

transitions are then drawn using filters for various troop levels, and the equivalence of distributions is tested using chi-squared goodness-of-fit tests.

In subsequent analysis, I use multinomial logistic regressions to I explore the impact of the various explanatory variables described above on the relative likelihood of observing each of the transition types depending on which *Polity* category a country starts in.

Independent Variables

In the previous chapter of this dissertation, I found a strong negative correlation between internal conflict and the institutional development score generated by the Economic Freedom of the World Index. This correlation is robust to the inclusion of fixed effects, indicating that individual country's scores are lower during years in which the UCDP/PRIO Armed Conflict Dataset report a conflict. As expected, when the dependent variable is the annual Polity score there are also strong correlations with conflict, with one crucial caveat. The sign and magnitude of the correlation depends on what type of regime is being considered. Incidents of conflict increase as countries "improve" their *Polity* scores within the autocracy range of the index but decrease as countries improve within the democracy range of the index. That is, conflict is positively correlated with increasing scores within the -10 to -6 range of the index and negatively correlated with increasing scores within the 6 to 10 range of the index.

Table 1 presents the results of four OLS regressions with robust standard errors, controlling for country fixed-effects. This table demonstrates the correlations between annual incidents of conflict and annual *Polity* scores within the four categories previously

mentioned. These results indicate that during years with conflict each autocracy's *Polity* score is about a third of a point higher, and each democracy's *Polity* score is about a third of a point lower.

Table 3-1. Polity Scores and Conflict - By Regime Type - OLS Regressions with Fixed Effects

	Estimated Coefficients - Robust Standard Errors *(Standard Error)*			
	Autocracy	*Closed Anocracy*	*Open Anocracy*	*Democracy*
Conflict	0.314**	-0.231	-0.19	-0.33**
	(0.15)	*(0.29)*	*(0.26)*	*(0.132)*

*=90% confidence, **=95% confidence, ***=99% confidence

Because troop deployments are frequently correlated with conflict, failing to control for or consider incidents of conflict could produce spurious conclusions regarding the effects of troops.

Table 3-2. Polity Scores and Income Per Capita - By Regime Type - OLS Regressions with Fixed Effects

	Estimated Coefficients - Robust Standard Errors *(Standard Error)*			
	Autocracy	*Closed Anocracy*	*Open Anocracy*	*Democracy*
Income Per Capita	0.000025	0.0000027	0.000061	0.000011**
	(0.00004)	*(0.000004)*	*(0.0002)*	*(0.0000056)*

*=90% confidence, **=95% confidence, ***=99% confidence

Given the literature supporting the robustly positive impact of economic growth on democracy (Helliwell 1994, Alesina 1996, Barro 1996), I also include income per

capita as an independent variable. Table 2 verifies a positive correlation between *Polity* score and income per capita, including country fixed-effects, but only for countries already counted as democratic. This finding confirms the conclusions of Hegre, et al, that increasing GDP/capita is correlated with stabilization of democracy, but not democratization (forthcoming). Table 3 displays the mean income per capita for each regime type in 2010. These tables provide additional justification for careful consideration of movement to democracy rather than positive movement along the Polity Index as the dependent variable of interest.

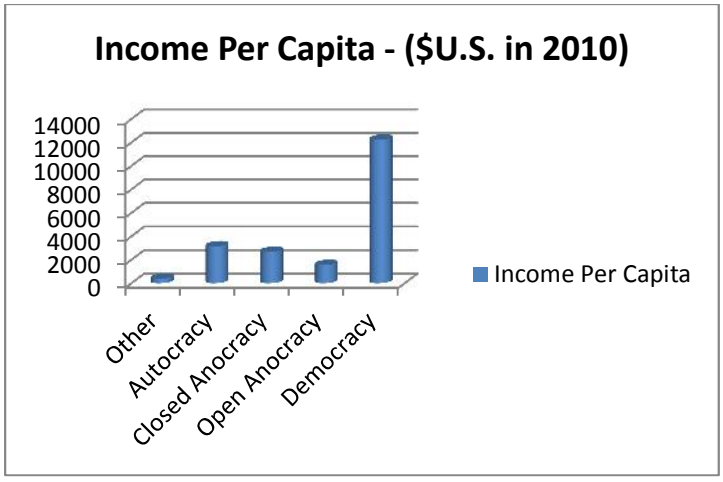

Figure 3-2. Income Per Capita - U.S. Dollars in 2010 by Regime Type

A number of studies have found significant economic impacts of foreign aid, though frequently disagreeing with each other. Many claim that foreign aid significantly hampers economic growth (Bauer and Sen 2004, Djankov, Montalvo and Reynal-Querol 2008, Easterly 2001), while others claim that the key to more growth is more aid (Sachs

2014). Given the strong correlations between economic growth and democracy and between aid and troops, the significance of previous findings indicate that ignoring other foreign aid in this analysis may lead to omitted-variable bias and spurious conclusions. I use data from the World Development Index (WDI) and include Official Development Assistance (ODA) as a percentage of Gross National Income (GNI) as an independent variable. To identify if troops and aid work synergistically, I also create an interaction term by taking the natural log of the sum of annual products of the annual ODA/GNI and troop years variables over the length of the assessment window.

The last independent variable I include is a binary indicator for years post-1989. This coincides with the fall of the Berlin Wall and the rapid transitions to democracy which followed, as identified in Figure 1.

Results and Discussion

Results: OLS Regressions

Table 3 presents the results of five OLS regressions with fixed-effects and robust standard errors using only two independent variables, the Polity score for each country at the beginning of the assessment window and the natural log of the sum of troops deployed to each country during each time period. The negative sign on the lagged Polity score demonstrates convergence, as countries with higher Polity scores in the past have less room to move upward on the scale, and more countries with low Polity scores have transitioned to democracy. Each of the regressions returns a positive sign on the coefficient for troops; however, only coefficients from the ten- and fifteen-year

regressions are statistically significant. This could be due to decreasing sample size as we expand the length of the assessment window and decrease the number of windows available. The positive and increasing trend from five to fifteen years supports the idea that influence may increase with time rather than in operate in the short run. However, this is countered by the decreasing significance observed in the 20- and 30-year regressions.

Table 3-3. Change in Polity Score With Increasing Assessment Window Length - Non-Overlapping Windows

	Estimated Coefficients - Country Fixed Effects, Robust Standard Errors *(Standard Error)*				
	5-Year	*10-Year*	*15-Year*	*20-Year*	*30-Year*
Lagged Polity Score	-0.357***	-0.568***	-0.783***	-0.781***	-1.546***
	(0.037)	(0.059)	(0.058)	(0.079)	(0.17)
ln(Sum of TroopYrs)	0.062	0.290*	0.511**	0.210	0.133
	(0.085)	(0.155)	(0.20)	(0.31)	(0.38)
Total Observations	1178	627	431	303	192
Number of Countries	150	148	149	127	126
Ave Obs/Country	7.9	4.2	2.9	2.4	1.5

* = 90% confidence, **=95% confidence, ***=99%confidence

Table 3-4. Relationship between Troops and Change in Polity Score, Increasing Assessment Window Length - Non-Overlapping Windows, By Starting Regime Type. Coefficients of Lagged Polity Scores are not Displayed.

Troops' Coefficient when the country starts in:	Estimated Coefficients for ln(sum of troops) - Country Fixed Effects, Robust Standard Errors *(Standard Error)*				
	5-Year	*10-Year*	*15-Year*	*20-Year*	*30-Year*
Autocracy	0.351**	0.621***	0.888***	Insufficient Observations	
	(0.15)	(0.22)	(0.27)		
Closed Anocracy	0.024	-0.934	0.488		
	(0.68)	(1.05)	(0.49)		
Open Anocracy	-0.260	-0.063	-0.711		
	(0.155)	(0.42)	(1.43)		
Democracy	-0.141	-0.160	-0.313		
	(0.13)	(0.186)	(0.22)		

*=90% confidence, **=95% confidence, ***=99% confidence

Tables 1 and 2 and Figure 2 demonstrate that impacts of various independent variables may differ depending on the institutional development of the target country. Table 4 explores this idea by repeating the regressions in Table 3 according to the regime type of each country at the beginning of assessment windows. To prevent overlapping assessment windows, regressions are filtered to report on five-, ten-, and fifteen-year segments, e.g. 1950, 1955, 1960, 1965, etc., for the five year regressions. Analyzing each regime type separately confirms that military interventions can increase the Polity scores of non-democracies, as identified by Pickering and Kisangani (2006). In each of the assessment windows, increases in the number of troops deployed to a country are positively correlated with increasing Polity scores as long as that country starts in the autocratic range of the Polity scale. The differences in magnitude from year to year demonstrate a relatively linear effect over time. A one hundred percent increase in the number of troops is correlated with about three tenths of a point increase over five years, about six tenths over ten years, and about nine tenths over fifteen years.

Including additional independent variables confirms the positive correlation between troops and positive movement in Polity scores under autocracy and provides some additional insights. Focusing on a ten-year assessment window and only those countries which are autocratic at the beginning of each assessment window, Table 5 presents results of regressions as I include additional explanatory variables.

Table 3-5 OLS Regression w/Country Fixed-Effects: Ten-Year Change in Polity Score; Various Independent Variables

	Estimated Coefficients - Robust Standard Errors *(Standard Error)*				
	Combined	*Autocracy*	*Closed Anocracy*	*Open Anocracy*	*Democracy*
Lagged Polity Score	-0.915***	-0.914		-1.631***	1.126
	(0.069)	*(1.24)*		*(0.28)*	*(0.80)*
ln(Sum of TroopYrs)	0.0093	0.877*		8.203	0.023
	(0.286)	*(0.52)*	*Insufficient*	*(15.4)*	*(0.22)*
10 Yr Ave: Aid/GNI	0.348**	1.300**	*Observations*	31.98	0.273
	(0.14)	*(0.57)*		*(62.17)*	*(0.22)*
Troops*Aid	-0.035	-0.213**		-6.49	0.0026
	(0.032)	*(0.106)*		*(13.46)*	*(0.029)*
Lagged Conflict	1.516	1.548		20.89	-1.465
	(1.01)	*(1.801)*		*(30.79)*	*(1.69)*
10 Yr Sum of Conflict	-0.471***	-0.420		-2.91	-0.344
	(0.15)	*(0.301)*		*(3.06)*	*(0.59)*
10 Yr Ave: Income/Capita	-0.00016***	-0.0034		-0.00068	-0.000087*
	(0.00005)	*(0.0037)*		*(0.0025)*	*(0.00005)*
Post-1989	*4.379****	*3.959****		*0.248*	*1.73**
	(0.86)	*(1.18)*		*(1.05)*	*(1.01)*
Countries	*115*	*52*		*14*	*70*
Ave Obs/Country	*2.8*	*1.7*		*1.6*	*2.4*
R-squared	0.194	0.0226		0.04	0.0244

*=90% confidence, **=95% confidence, ***=99% confidence

The statistically significant negative coefficients on lagged Polity score are consistent with convergence to higher Polity scores. It is noteworthy that the regressions only return a statistically significant coefficient for troops in the construction examining countries which start in the autocratic portion of the scale. This demonstrates that after controlling for a number of potentially confounding variables, the presence of U.S. troops continues to have a positive correlation with increasing Polity scores, but only for those countries which begin the assessment periods as autocracies. Again, this is consistent with the findings of Pickering and Kisangani and the less specified model presented in Tables 3 and 4, that troops appear to increase Polity scores only in countries starting as non-democracies (2006).

The positive, significant coefficients on the ten-year average of aid/GNI seem to indicate that aid may correlated with increasing Polity scores in autocracies, contrary to findings of Dutta, Leeson, and Williamson. Their 2013 paper claims to find an amplification effect of aid for the regime in power when aid is received. That is, if countries are autocratic, additional aid appears to strengthen the autocracy. If countries are beginning to be democratic, aid strengthens the democracy. Their exploration is specifically devoted to the impacts of aid on democracy. Thus, their panel of data involves different independent variables than this study. They do not, however, consider troop deployments in their analysis, and the inclusion of this data provides some additional insight. While the regression for *Autocracy* in Table 5 returns a positive coefficient for aid and for troop deployments, the interaction between the two is correlated with smaller changes in Polity scores. That is, the greater the average percentage of national income provided by foreign aid together with the presence of U.S. troops over the course of time, the smaller the change in Polity scores. If the reception of aid signals economic turmoil, and the presence of troops signals political turmoil, the combined effect may be difficult to overcome.

The two controls for conflict return the expected signs in three of the four regressions, and the number of years with conflicts reported generates a statistically significant negative coefficient at above the 99 percent level of confidence when the regression is run over the entire data set. The effects of conflict to not appear to change significantly between the different categories of regimes. In each case, the sign of the coefficient indicates that Polity scores increase less when more cases of conflict are

reported, but none of the coefficients is statistically significant when evaluated by regime category.

Finally, the dummy variable indicating years after 1989 delivers a positive and statistically significant result for regressions with data filtered for *Autocracy, Democracy,* and the combined data set. This coincides with expectations given the monumental shift toward democracy following the fall of the Berlin Wall and the rapid global changes which ensued over the following few years.

The following section builds on the findings above by exploring transitions between regime types, rather than just looking at changes in *Polity* score.

Results: Polity as a Categorical Dependent Variable

While the objective of this study is to see if increasing numbers of U.S. troops are correlated with increasing rates of transition to democracy, it has been noted that the route to democracy can be a long and complex process. If previous analysis inappropriately assumes an ordinal development of democracy along the *Polity* scale, the purpose of this section is to explore whether conclusions change when transitions are analyzed, rather than just raw change in *Polity* score.

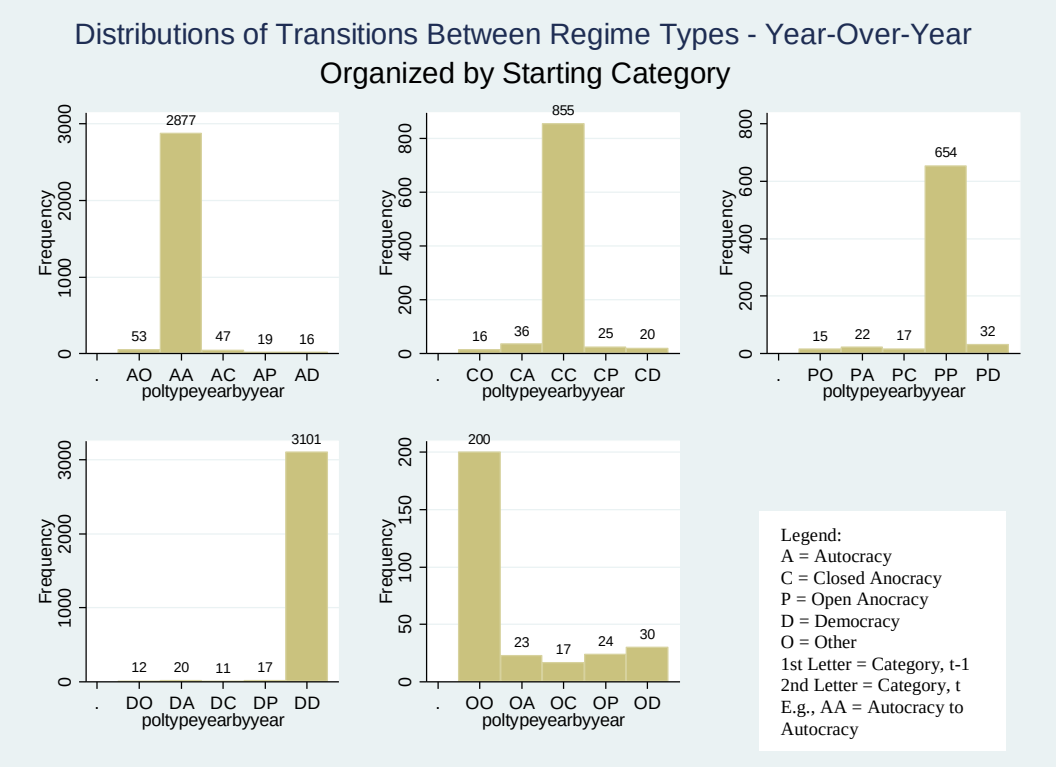

Figure 3-3. Graphs of Year-to-Year Regime Transitions

Figure 3 presents the number of occurrences observed for each of the year-to-year transitions in our dataset between 1950 and 2012. From year to year, overwhelmingly countries stay in the same category as the previous year (95 percent A to A, 90 percent C to C, 88 percent P to P, 98 percent D to D, and 70 percent O to O). As countries in the *Other* category are by definition in transition, it is not unexpected that countries in the *Other* category transition more frequently than others. I note here that the probability of transitioning directly to *Democracy* is greater for *Closed Anocracy* (0.02) and *Open Anocracy* (0.04) than for *Autocracy* (0.005). As such, it could be argued that upward movement on the Polity Scale is indeed progress toward democracy. However, from the

Anocracy categories, it is appears to be roughly equally likely that countries will move back down the scale as up toward democracy.

Figure 4 presents the same transitions identified in Figure 3, but divides the data into pre- and post-1989. Not surprisingly, the probabilities of transitioning between categories changes considerably after 1989, with large shifts away from autocracy and toward democracy. Chi-squared goodness-of-fit tests indicate that pre- and post-1989 distributions are significantly different from the overall distribution across all categories evaluated together, as well as for each category evaluated independently. This is an important consideration, as overseas troop deployments have shifted significantly since the end of the Cold War.

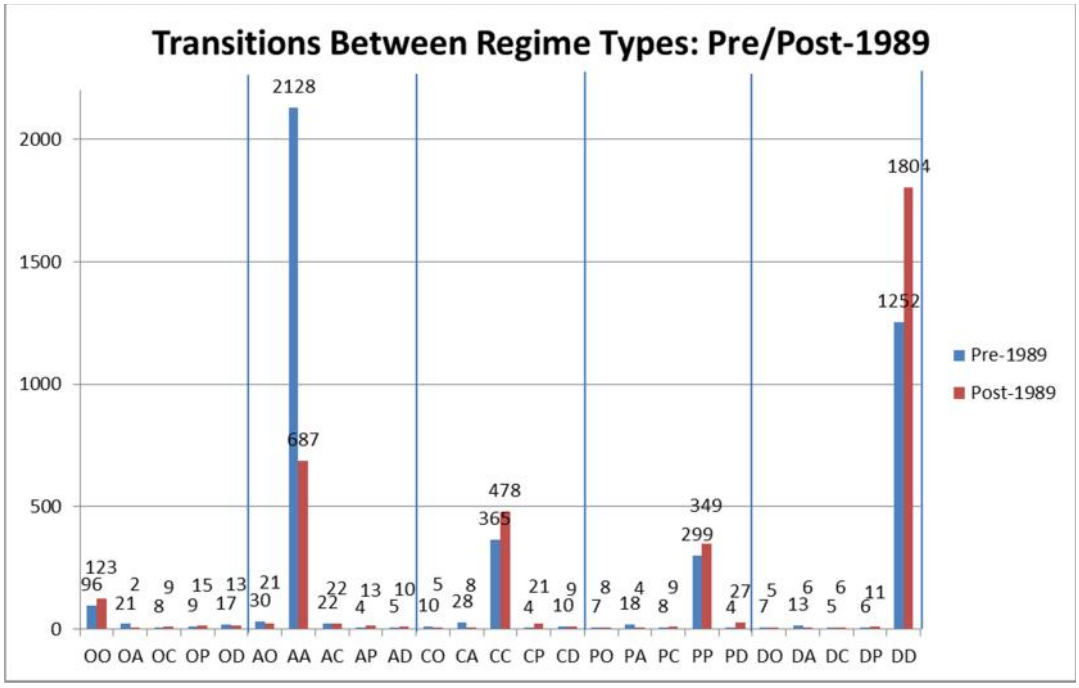

Figure 3-4. Comparing Annual Regime Transitions Pre- and Post-1989

Using the pre- and post-1989 distributions, I filtered the regime transitions according to three troop deployment thresholds, 10 troop-years, 100 troop-years, and 1000 troop-years, and conducted chi-squared goodness-of-fit tests for the overall set of data. Table 7 presents the results of these tests.

Table 3-6. Chi-Squared Goodness-of-Fit Tests of Annual Regime Transititions For Countries Receiving Troops, Pre- and Post-1989

	Chi-Squared Test Statistic (p-value)		
	Troop-Year Threshold		
Control Distribution:	10 Troops	100 Troops	1000 Troops
Pre-1989: All	17.243	22.76	31.95
	(0.838)	(0.534)	(0.128)
Post-1989	21.76	21.857	15.155
	(0.594)	(0.59)	(0.92)

The statistically insignificant p-values presented in Table 6 indicate no significant distributional differences between the types of transitions observed as troop presence increases, either pre- or post-1989. I repeated the chi-squared goodness-of-fit tests separately for transitions beginning in each of the regime types, including all data, and separated according to pre- and post-1989. The only significant chi-squared statistic was generated by filtering for transitions originating in the *Other* category pre-1989 with greater than 1000 troop years(chi-square: 18.015, p-value: 0.0012). One potential weakness of this construction arises due to the overwhelming probability of remaining in the same regime category year-over-year. As such, the principal finding associated with Table 6 is that increasing U.S. troop presence does not appear to increase the likelihood

of a political transition. However, the significant chi-squared statistic identified for transitions from the *Other* category indicates some potential that countries in transition respond differently from the overall population. To evaluate this possibility, I repeated the chi-squared tests on the distribution of annual transitions by including only movements between regime types. All tests generated insignificant results, though filters reduced the frequency of observations and expectations in some cells below levels which are generally considered necessary for reliable chi-squared goodness-of-fit tests.

Using the year-over-year transitions above, the next stage of analysis utilizes multinomial regressions to evaluate correlations between the various independent variables explored in the first section and the likelihood of observing each of the types of transitions from the five different *Polity* categories. Table 7 presents the results of a multinomial logistic regression evaluating the likelihood of transition from *Autocracy* to other categories.

The coefficients from multinomial logit regressions are equal to the log of the likelihood ratio between the target category and the base category. In this case, the base category is the transition from *Autocracy* to *Autocracy* (AA). Thus, the coefficients presented indicate correlations between the explanatory variables and the likelihood of observing the target transition rather than remaining in *Autocracy.*

The *Autocracy* category covers *Polity* scores from -10 to -6. The positive coefficients on lagged *Polity* scores indicate that increasing along that range increases the likelihood of transitioning out of *Autocracy* to *Closed Anocracy* or *Open Anocracy*, but not directly to *Democracy*.

Table 3-7. Likelihood of Transition From Autocracy - Multinomial Logit Regression with Robust Standard Errors

Likelihood of Transition	Estimated Coefficients *(Standard Error)*			
From *Autocracy* to:	*Other*	*Closed Anocracy*	*Open Anocracy*	*Democracy*
Lagged Polity Score	0.326	0.449*	1.326*	-0.0523
	(0.30)	*(0.25)*	*(0.74)*	*(0.31)*
Lagged TroopYrs	0.000103***	-0.0000353	-0.151	0.000074
	(0.00003)	(0.000091)	*(0.11)*	*(0.000055)*
Aid/GNI	0.0041	*-0.0163*	-0.0136	0.0560
	(0.025)	*(0.023)*	*(0.031)*	*(0.039)*
Troops*Aid	0.00300	0.00755	0.0177	-0.018
	(0.0136)	*(0.00094)*	*(0.016)*	*(0.014)*
Lagged Conflict	1.214**	0.791*	0.0484	0.677
	(0.544)	*(048)*	*(1.135)*	*(1.07)*
Income per Capita	0.000186	0.000293*	-0.000699	0.00062***
	(0.0002)	*(0.00017)*	*(0.00089)*	*(0.0002)*
Post-1989	*0.392*	*0.317*	*18.225****	*-0.124**
	(0.69)	*(0.44)*	*(0.48)*	*(0.89)*

Total Obs = 660. Pseudo R-squared= 0.10. *=90% confidence, **=95% confidence, ***=99% confidence

The presence of U.S. troops significantly increases the likelihood of transitioning out of *Autocracy* to *Other*, but not to *Anocracy* or *Democracy*. The *Other* category is specifically for countries in political transition, civil war, or other interruption in government, so this correlation is unsurprising if U.S. troops deploy to assist countries in turmoil. However, previous regressions found a positive correlation between increases in *Polity* scores and U.S. troops for countries under *Autocracy*. If increasing *Polity* scores are correlated with troop presence, but transitions to other categories are not, the results in Table 8 may suggest that the countries with increasing *Polity* scores are not

transitioning out of *Autocracy,* and conclusions that troops improve democracy in non-democratic countries (Pickering and Kisangani 2006), may need to be reconsidered.

While Hegre, et al, find that economic growth does not have a democratizing effect (forthcoming), the statistically significant coefficient on income-per-capita in Table 8 is more consistent with Helliwell's findings that economic growth strongly influences democratic development (1994). The magnitude of this effect appears small, a one dollar increase in income-per-capita is correlated with a 0.06 percent increase in likelihood of transition from *Autocracy* to *Democracy*. However, if the relationship is linear, a one hundred dollar increase in income-per-capita corresponds to a six percent increase in likelihood of transition to *Democracy*. Whereas the average income-per-capita among autocracies in 2010 was roughly $3000, income-per-capita would have to increase by just over ten percent to increase the probability of annual transition to *Democracy* to around 20 percent.

Tables 8 through 10 present similar regressions for transitions from *Other*, *Closed Anocracy*, and *Open Anocracy*.

While the number of observations potentially calls into question the magnitude and statistical significance of some of these coefficients in Table 9, I believe the signs of the coefficients are at least insightful. Countries in the *Other* category have *Polity* scores of -66, -77, or -88. These equate with years of "interruption," "interregnum," or "transition," respectively. According to *Polity's* User Handbook, interruptions and interregna imply foreign involvement, and transition simply indicates that a country has not demonstrated the existence of a newly formed polity following the collapse of a

previous regime. Thus, the coefficients on lagged *Polity* scores in this table potentially provide the strongest argument that U.S. military involvement may promote democracy. The finding that U.S. troops are correlated with increasing transition from *Other* to *Democracy* and the positive correlation between *Polity* score and transition to *Democracy* could indicate that U.S. involvement during transition promotes the development of democracy. Additionally, the negative coefficients on lagged troops for transitions to *Autocracy* and *Closed Anocracy* indicate that countries are less likely to transition to *Autocracy* than to remain in transition as the presence of U.S. troops increases.

Table 3-8. Likelihood of Transition From *Other* - Multinomial Logit Regression with Robust Standard Errors

Likelihood of Transition From *Other* to:	Estimated Coefficients *(Standard Error)*			
	Autocracy	*Closed Anocracy*	*Open Anocracy*	*Democracy*
Lagged Polity Score	-0.857	-3.016***	0.218	0.680***
	(0.48)	*(0.65)*	*(0.28)*	*(0.20)*
Lagged TroopYrs	-0.1665*	-0.539	0.000135**	0.000135*
	(0.092)	(0.540)	*(0.000063)*	*(0.00008)*
Aid/GNI	-0.057	*-0.0278**	-0.0111	-0.0084
	(0.13)	*(0.016)*	*(0.022)*	*(0.017)*
Lagged Conflict	17.983***	-11.075***	-1.299*	-0.886
	(0.98)	*(2.74)*	*(0.79)*	*(0.682)*
Income per Capita	0.000916	0.00324*	-0.000688*	0.0000289
	(0.0014)	*(0.012)*	*(0.00040)*	*(0.00015)*
Post-1989	*-22.773****	*-3.820***	*-1.113*	*-0.598*
	(1.88)	*(1.74)*	*(0.80)*	*(0.84)*

Total Obs = 89. Pseudo R-squared= 0.40. *=90% confidence, **=95% confidence, ***=99% confidence

Interestingly, income-per-capita does not appear to be correlated with increased likelihood of transition from *Other* to *Democracy*. Finally, transitions post-1989 from

Other are considerably less likely to be to *Autocracy* or *Closed Anocracy* than back to *Other*.

Table 9 presents the results of a multinomial logit regression evaluating the likelihood of transitioning from *Closed Anocracy* to each of the other categories. Several coefficients are interesting and some have signs contrary to expectation. Notably, there appears to be little correlation between changes within the range of *Polity* scores included in *Closed Anocracy* and the likelihood of transitioning to *Democracy* or even *Open Anocracy*. The positive and significant coefficient on troops for transitions to *Other*, and not for other categories, indicates that increasing levels of U.S. troops are not correlated with higher likelihoods of direct transitions to *Democracy*, or even to transitions upward from *Closed Anocracy* on the *Polity* Scale. Interestingly, while aid appears to reduce the likelihood of transitioning to *Autocracy*, increasing income per capita seems to be correlated with an increased likelihood of moving toward *Autocracy*, instead of toward *Democracy*. Finally, post-1989 transitions from *Closed Anocracy* are considerably more likely to move up the *Polity* scale to *Open Anocracy*, but do not demonstrate an increased likelihood of transitioning directly to *Democracy*.

Table 3-9. Likelihood of Transition From *Closed Autocracy* - Multinomial Logit Regression

Likelihood of Transition From *Closed Anocracy* t	Estimated Coefficients - Robust Standard Errors *(Standard Error)*			
	Other	*Autocracy*	*Open Anocracy*	*Democracy*
Lagged Polity Score	0.271	-0.0414	0.194	0.0965
	(0.29)	*(0.18)*	*(0.28)*	*(0.20)*
Lagged TroopYrs	0.0000967**	0.0000438	-0.00153	-0.000132
	(0.0000449)	(0.000369)	*(0.0045)*	*(0.000095)*
Aid/GNI	0.0906*	*-0.220****	-0.0950	-0.0138
	(0.053)	*(0.077)*	*(0.072)*	*(0.079)*
Lagged Conflict	0.399	-0.1017	0.849	0.541
	(0.65)	*(0.63)*	*(0.70)*	*(0.61)*
Income per Capita	0.0000133	0.00103***	-0.0000457	-0.000133
	(0.000076)	*(0.00038)*	*(0.000047)*	*(0.00012)*
Post-1989	-0.678	-2.190***	14.175***	-0.784
	(0.972)	*(0.618)*	*(0.37)*	*(0.58)*

Total Obs = 537. Pseudo R-squared= 0.115. *=90% confidence, **=95% confidence, ***=99% confidence

Table 10 presents the results of a multinomial logit regression evaluating the likelihood of transitioning from *Open Anocracy* to each of the other categories. As expected, increasing *Polity* scores within the range categorized as *Open Anocracy* are significantly correlated with transitions to *Democracy*. Additionally, increasing *Polity* scores within the range categorized as *Open Anocracy* are significantly negatively correlated with transitions to *Autocracy*. However, contrary to expectations, the presence of U.S. troops is positively correlated with transitions to *Autocracy*. Conflict in *Open Anocracy* may encourage transition to *Democracy*, given the positive coefficient on lagged conflict in the *Democracy* column of Table 10. Finally, the only other marginally significant coefficient is for transitions post-1989, indicating that transitions away from *Open Autocracy* within the last 25 years favor *Democracy*.

Table 3-10. Likelihood of Transition From *Open Anocracy* - Multinomial Logit Regression

Likelihood of Transition From *Open Anocracy* to	Estimated Coefficients - Robust Standard Errors *(Standard Error)*			
	Other	*Autocracy*	*Closed Anocracy*	*Democracy*
Lagged Polity Score	-0.161	-0.400*	-0.113	0.856***
	(0.29)	*(0.22)*	*(0.34)*	*(0.32)*
Lagged TroopYrs	-0.0181	0.0000453**	-0.000163	0.000021
	(0.017)	(0.0000212)	*(0.00012)*	*(0.000017)*
Aid/GNI	-0.114	*0.0202*	-0.00979	-0.0203
	(0.10)	*(0.025)*	*(0.042)*	*(0.042)*
Lagged Conflict	0.880	-0.570	0.113	0.977*
	(0.80)	*(0.78)*	*(0.90)*	*(0.55)*
Income per Capita	-0.000582	0.00126	-0.0000308	0.000167
	(0.00064)	*(0.00083)*	*(0.00033)*	*(0.00016)*
Post-1989	0.135	-1.458	0.0759	2.018*
	(0.85)	*(0.90)*	*(0.89)*	*(1.20)*

Total Obs = 329. Pseudo R-squared= 0.128. *=90% confidence, **=95% confidence, ***=99% confidence

While examining year-over-year transitions provides unique insight and several interesting conclusions contrary to expectations, the focus of these tests concerns relatively immediate transitions. It fails to provide visibility regarding transitions in which a country passes through other categories eventually resulting in *Democracy*. For example, between 1989 and 1991, Benin transitioned from *Autocracy* to *Democracy*, and in 1990 it was appropriately categorized in transition, captured in our year-by-year analysis in the *Other* category. The year-by-year analysis above misses that this transition began in *Autocracy*, because the final transition was from *Other* to *Democracy*. Thus, in this type of analysis, transitions from *Autocracy* to *Democracy* may be underreported.

Additionally, in the year-by-year analysis, I identified that U.S. troop presence may increase the likelihood of transitions from *Autocracy* to *Other* and from *Closed Anocracy* to *Other*. Troops are not correlated with direct transitions to *Democracy* from

Autocracy, *Closed Anocracy*, or *Open Anocracy*. However, because the presence of U.S. troops increases the likelihood that countries transition from *Other* to *Democracy*, I would like to examine whether troops increase the rate of transition to *Democracy* on a path via other categories. To evaluate this potential, I constructed a variable which indicates transitions that result in regimes that endure three years or longer. The selection of three years as the critical value is arbitrary. However, extending the threshold for endurance produces several complications for data analysis, such as increased likelihood of multiple transitions before settling, and formulation of other decision criteria, such as timing of military deployments, etc. For this section of analysis, regimes (or at least classifications of government) which last two years or less are ignored. In the Benin example, this appears as a transition from *Autocracy* to *Democracy*, because the one year spent in transition is ignored. For this section of analysis, regimes lasting three years or longer are not considered to be in transition. Therefore, unlike the year-over-year discussion, I do not include transitions to the status quo. The distributions of these transitions is presented in Figure 5.

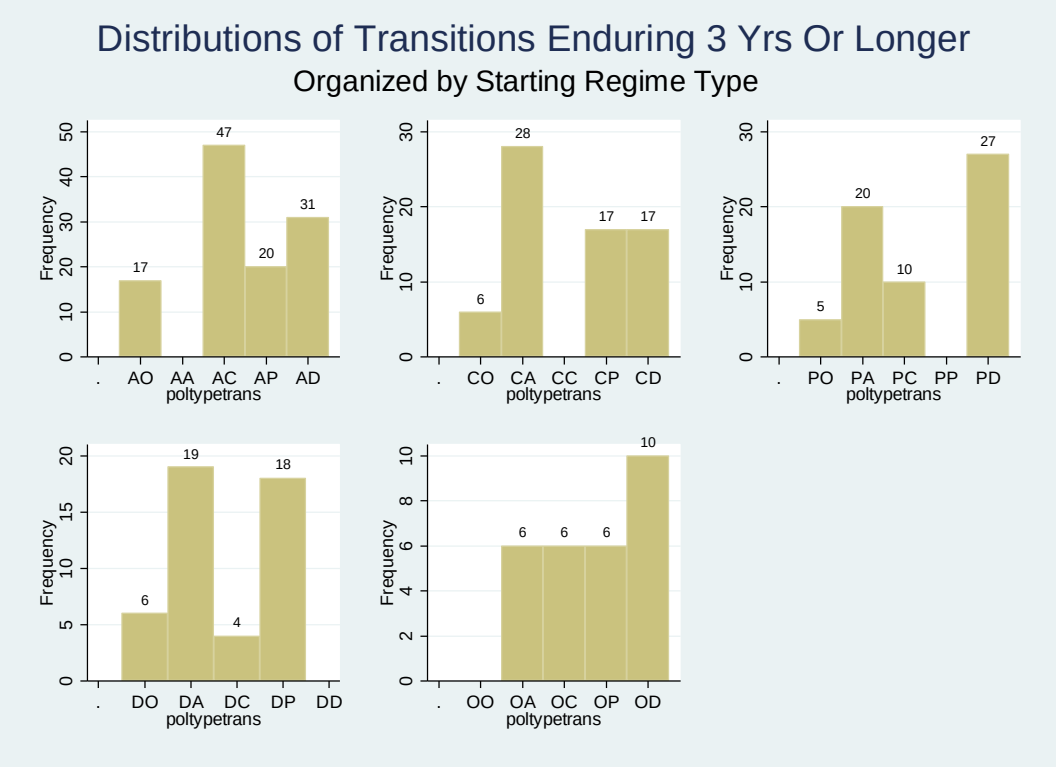

Figure 3-5. Transitions Identified by Ignoring Categorizations Lasting Less Than Three Years

The low number of observations in several of the cells makes the standard errors from multi-variate regression unreliable when all of the explanatory variables from the previous analysis are included. Therefore, I evaluated each of the explanatory variables in bivariate multinomial regressions. This increases the probability of omitted variables biasing the results, and while the low sample size still makes the results somewhat unreliable, I believe the results are still insightful. Additionally, because this analysis requires at least three years of a country remaining within a given category, instantaneous or one-year measures of our independent variables are inappropriate. Instead, I use five-year averages for income-per-capita, aid/GNI, and the log of the sum of troops received

over the last five years. Table 11 summarizes the statistically significant coefficients generated by the various bivariate multinomial regressions.

Table 3-11. Statistically Significant Coefficients from Bi-Variate Multinomial Logit Regressions - Robust Standard Errors

Transitioning From:	*Statistically Significant Variable*	*Transition to:*	*Compared with Base Outcome*	*Estimated Coefficient*	*Standard Error*	*p-Value*
Autocracy	5-Yr Ave: Income/Capita	Other	Closed Anocracy	0.000388	0.00023	0.091
		Democracy	Closed Anocracy	*0.000336*	*0.0001588*	*0.034*
	5-Yr Sum: Years w/Conflict	Other	Closed Anocracy	*0.261*	0.144	0.069
Closed Anocracy	Ln(Five-Yr Sum of Troops)	Democracy	Autocracy	0.38	0.17	0.024
	Post 1989	Other	Autocracy	2.219	1.00	0.027
		Open Anocracy	Autocracy	*3.067*	*0.81*	*0.000*
		Democracy	Autocracy	*1.408*	*0.70*	*0.044*
Open Anocracy	5-Yr Ave: Income/Capita	Other	Democracy	-0.00338	0.001	0.001
	Post 1989	*Autocracy*	Democracy	*-3.679*	*0.90*	*0.000*
		Closed Anocracy	Democracy	-1.482	0.81	0.067
Democracy	5-Yr Sum: Years w/Conflict	Closed Anocracy	Autocracy	-15.169	0.74	0.000
	5-Yr Ave: Income/Capita	Other	Open Anocracy	-0.000698	0.00031	0.026
	Post 1989	Open Anocracy	Autocracy	1.225	0.7	0.079
Other	5-Yr Sum: Years w/Conflict	Autocracy	Democracy	0.502	0.29	0.084

The estimated coefficients represent the relative difference in likelihood of observing the subject transition vs the base outcome as the explanatory variable increases. Thus, under *Autocracy*, the positive and statistically significant coefficient on income per capita indicates that as income increases the likelihood of observing a transition to *Other* increases relative to the likelihood of observing a transition to *Closed Anocracy*. The positive coefficient on income per capita for transitions from *Autocracy* to *Democracy* indicates that increasing income per capita increases the likelihood of observing a transition to *Democracy* rather than to *Closed Autocracy*. Similarly, the negative coefficient on income per capita for transitions from *Open Anocracy* to *Other* indicates that increasing income makes it less likely to observe transitions to *Other* than to *Democracy*. Thus, increasing income per capita increases the likelihood of observing

transitions to *Democracy* from both *Autocracy* and *Open Anocracy*. As noted in previous analysis, this runs contrary to the findings of Hegre, et al, and when combined with the related finding in Table 7, indicates that income per capita may be more influential than previously identified.

Related to the central questions of this paper, perhaps the most interesting finding from Table 11 is the statistically significant positive increase in the likelihood of observing a transition from *Closed Anocracy* to *Democracy* as U.S. troop deployments increase. Table 9 identified an increased likelihood of transitioning from *Other* to *Democracy* as troop deployments increase, and Table 11 identified the same correlation for transitions from *Closed Anocracy* to *Other.* The positive coefficient from Table 11 may imply that the findings in Tables 8 and 10 work together, and the presence of troops may encourage transition to *Democracy* from *Closed Anocracy,* but some transitions may require more than one year to complete and, thus, not show up in the year-by-year analysis. Three examples of this include Honduras from 1980 to 1982, Croatia from 1999 to 2000, and South Korea from 1987 to 1988.

Summary and Conclusions

The results of the OLS regressions are presented in Tables 3, 4, and 5. In general, the findings involving military presence over time do not prove to be significantly different from previous literature analyzing the effects of specific military interventions. I find that increasing troop presence is significantly positively correlated with increasing *Polity* scores when the target country starts in *Autocracy*. This finding is robust to the

inclusion of other independent variables including controls for other foreign aid, income-per-capita, national conflict, the post-1989 surge to democracy, and interaction terms between these variables and troop deployments.

The fact that previous findings are reinforced by this analysis, using additional data and over more extended assessment windows, make this finding notable by itself. However, the analysis utilizing transitions as a categorical dependent variable makes additional significant contributions. Table 6 identifies no significant chi-squared statistics as data were filtered for increasing troop presence. Thus, increasing troop presence does not appear to increase rates of transition between categories, even for countries in autocracy. If countries are transitioning, though, Tables 7 through 10 indicate that U.S. troop presence may make some year-over-year transitions more likely than others, specifically: from *Autocracy* and *Closed Anocracy* to *Other*, from *Other* to *Open Anocracy* and *Democracy*, and (contrarily) from *Open Anocracy* to *Autocracy*. Additionally, filtering transitions to include only those that endure for at least three years allows us to identify that troops may indeed promote transitions to *Democracy* via the *Other* category, especially from *Closed Anocracy*. Such transitions mask the promotion of democracy on a year-over-year basis, and may confound analysis looking at change in *Polity* scores depending on how transitions, interruptions, and interregna are treated in their analysis.

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