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THE SELF-DEFEATING EFFECTS OF AMERICAN DRONE WARFARE IN PAKISTAN AND YEMEN

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ABSTRACT

Are drone strikes effective tools of US counterterrorism policy? Despite intense debate, including some qualitative findings that cast doubt on their effectiveness, there are relatively few quantitative examinations of this topic. The author uses regression models to test the effects of drone strikes on terrorism in Pakistan and Yemen from 2000-2014. He tests two hypotheses: that drone strikes will lead to increased numbers of general terrorist attacks, and that drone strikes lead to increased numbers of suicide terrorist attacks. He uses negative binomial regression models, with monthly leads and lags out to 12 months, to test these hypotheses. Results show significant, positive, and endogenous relationships between drone strikes and both kinds of attacks, but only in Pakistan, and with a stronger relationship for terrorist attacks than suicide attacks. This implies that drone strikes are ineffective counterterrorism tools.

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Chapter 1

Introduction and Literature Review

Due to the increasing publicity of US drone strikes on foreign targets in recent years, authors in both the academic and popular press have increasingly debated the costs and benefits of drone warfare. The proponents of drone warfare emphasize its ability to execute surgical strikes against terrorist targets while minimizing collateral damage and preventing risk to American lives. However, drone warfare's detractors argue that it generates significant collateral damage and stokes anger at America amongst the populations that experience it. My thesis seeks to make a quantitative contribution to this debate, posing the following research question: have drone strikes launched by the US against terrorist groups abroad led to significant changes in the number of terrorist attacks in those regions?

To that end, I conduct a comparative case study of Pakistan and Yemen, as the majority of the literature on this topic focused solely on Pakistan. However, Pakistan's shared border with Afghanistan enabled Taliban militants and Al-Qaeda personnel to flee into it after the American invasion of Afghanistan, while Yemen experienced no such sudden influx. This design enables me to investigate if this influx made Pakistan an *anomaly* in terms of terrorist attacks, due to the surge of experienced terrorist elements, and control for this if necessary. As Pakistan is frequently the subject of studies of drone warfare, the discovery that it is not necessarily a representative case would contribute to the debate by helping scholars to construct future studies to account for this. My thesis also contributes to the existing literature by using a larger timeframe than that seen in previous analyses as well as a different dataset, testing the generalizability of previous research.

Given the relative recency of this phenomenon, there is no clear established consensus in the field of terrorism studies specifically regarding the effects of American drone warfare, as well as no established methodological “best approach” to investigating this issue. However, the literature on targeted killings in general is more established, and provides some insights. Thus, this literature review first examines representative qualitative studies of drone warfare, before moving on to quantitative treatments of both drone warfare and targeted killing. It ends with a brief review of studies relevant to my choice of control variables.

Those authors who used qualitative research designs, such as Boyle (2013), Hudson, Owens, and Flannes (2011), and the *Living Under Drones* report (International Human Rights and Conflict Resolution Clinic [IHRCRC] & Global Justice Clinic [GJC], 2012) focused on Pakistan. Boyle (2013) justified his case selection by appealing to the target selection rules of drone strikes: strikes in Afghanistan but not in Pakistan, Yemen, or Somalia are “subject to the existing rules for targeting and oversight that other military operations employ” (Boyle, 2013, p. 3). He also conducted a limited examination of data on drone strikes, without performing a regression (Boyle, 2013, p. 5). Hudson et al. (2012) examined Pakistan from 2002 through May 2011 to uncover principles they could apply to Yemen, but were hampered by a scarcity of data on Yemen (Hudson, Owens, & Flannes, 2012, pp. 124-129). *Living Under Drones* examined only Pakistan, as it specifically sought to challenge the “dominant narrative about the use of drones in Pakistan” (IHRCRC & GJC, 2012, p. v). Despite their individual differences, the case selections of these qualitative designs were marked by a lack of substantive consideration of the possibility that Pakistan is not a representative case. These studies agreed in their conclusions, and their findings are best summarized by the *Living Under Drones* report (IHRCRC & GJC, 2012):

> In addition, it is clear that US strikes in Pakistan foster anti-American sentiment and undermine US credibility not only in Pakistan but throughout the region. There is strong evidence to suggest that US drone strikes have facilitated recruitment to violent non-state armed groups, and motivate attacks against both US military and civilian targets. (p. 125)

There was little investigation into the generalizability of these findings.

Other studies continued this focus. Plaw, Fricker, and Williams (2011) compared data from three publicly available databases of drone strikes from 2008 through September 2011, finding that civilian deaths from drone strikes in Pakistan decreased from 2008 to 2010, then increased in 2011 (Plaw, Fricker, & Williams, 2011, p. 59). They also found that civilian casualty rates dropped during this timeframe, but were not eliminated completely (Plaw et al., 2011, p. 61). This finding implies either indirect support for, or a lack of conflict with, the studies discussed above, given that civilian deaths can hardly be expected to *increase* American popularity. Williams (2010), however, contradicted this with data from a poll of Pakistan's tribal FATA regions, which are the regions most heavily targeted by drone strikes (Williams, 2010, p. 876; IHRCRC & GJC, 2012, p. 7). He reported that the poll "clearly demonstrated that those who live in areas where the Taliban have closed girls [*sic*] schools and cinemas, executed 'adulterers' and 'spies,' killed local chieftains, and enforced strict Sharia law are more inclined to support the American drone strikes" (Williams, 2010, p. 883). Taken as a whole, these qualitative studies point to the need for more research in this area.

Quantitative studies of American drone strikes similarly focused on Pakistan. Smith and Walsh (2013) justified their case selection by theorizing that drone strikes impact a terrorist organization's ability to carry out its activities, and they operationalized that variable by examining Al-Qaeda's propaganda output (Smith & Walsh, 2013, p. 311). They selected

Pakistan as a representative case, since the US drone program there is intended to degrade the operational capabilities of its targets, including Al-Qaeda (Smith & Walsh, 2013, p. 311). Their timeframe covered "January 2006 through November 2011" (Smith & Walsh, 2013, p. 312). They found "that, at best, drone strikes have little or no effect on Al Qaeda's ability to create and issue propaganda" (Smith & Walsh, 2013, p. 325). Johnston and Sarbahi (2016) selected as their cases the seven agencies that comprise the FATA region of Pakistan, because "more than any other administrative or tribal boundaries, agencies are the administrative units that correspond with the geographic distribution of militant groups across FATA" (Johnston & Sarbahi, 2016, p. 208). They found that "drone strikes negatively correlate with various measures of militant violence. This negative association holds both within individual FATA agencies and their immediate neighborhoods" (Johnston & Sarbahi, 2016, p. 215) within a timeframe of "January 2007 through September 2011" (Johnston & Sarbahi, 2016, p. 208). Thus, drone strikes may decrease occurrences of militant violence but may be less effective at inhibiting processes like propaganda production.

Before moving on, the differences in data sources between these studies are worth considering. Johnston and Sarbahi (2016), Smith and Walsh (2013), and Boyle (2013) all used a drone strike database compiled by the New America Foundation (Johnston & Sarbahi, 2016, p. 209; Smith & Walsh, 2013, p. 326; Boyle, 2013, p. 5). The *Living Under Drones* report, which used the same Bureau of Investigative Journalism dataset (IHRCRC & GJC, 2012, p. vi) I use, sharply criticized the New America Foundation's dataset. It demonstrates, using several examples, that the Foundation's dataset is neither complete nor regularly updated (IHRCRC & GJC, 2012, p. 48). Furthermore, it is unreliable, with some corroborated strikes missing from the dataset for no stated reason (IHRCRC & GJC, 2012, p. 49). Given how *Living Under Drones* is cited in the literature on strikes, including in Boyle (Boyle, 2013, p. 6) and Johnston and Sarbahi (Johnston & Sarbahi, 2016, p. 203) it is surprising that these criticisms appear to have gone unheeded.

Literature on targeted killings, of which American use of drone strikes is one example, is more plentiful. One such study was that of Hafez and Hatfield (2004), who examined the use of targeted killings by Israeli forces against "Palestinian military commanders and political leaders" from "November 2000 to June 2004" (Hafez & Hatfield, 2004, p. 362). These targeted killings employed several different methods, including airstrikes, special forces actions, and explosive traps (Hafez & Hatfield, 2004, p. 362). They found, however, that such attacks had no significant effect on the levels of violence carried out by Palestinian insurgent organizations (Hafez & Hatfield, 2004, pp. 377-378). Another analysis of this phase of the conflict by Jaeger and Paserman (2009) examined the use of targeted killings by Israeli security forces against Palestinian insurgents from September 2000 through mid-January 2005 (Jaeger & Paserman, 2009, p. 322). They found that such a policy might result in "a short-term incapacitation or deterrent effect" (Jaeger & Paserman, 2009, p. 339) but that this decrease in violence is not due to a decrease in motivation to *commit* violence, as measured by "Palestinian efforts to respond with suicide attacks" (Jaeger & Paserman, 2009, p. 340).

Other studies found that a policy of targeted killing may actually be detrimental to the counterterrorism goals of the states implementing it. Jordan's (2009) examination of targeted killings of terrorist leaders, with the goal of leadership decapitation, used a timeframe of 1945-2004 (Jordan, 2009, p. 733). With this larger timeframe, she found that a strategy of targeted killing is "more likely to have counterproductive effects in larger, older, religious, and separatist organizations" and that decapitating strikes actually had a negative marginal value against such organizations (Jordan, 2009, pp. 753-754). Similarly, Mannes (2008) "found that the decapitation strategy appears to have little effect on the reduction of terrorist activity. The most notable trend from the statistical analysis was that decapitation strikes on religious terrorist groups tended to be followed by sharp increases in fatalities" (Mannes, 2008, p. 40). Taken together, these findings indicate that targeted killing is at best a stopgap measure that fails to deal with the underlying motivations of terrorism, and at worst exacerbates the problem.

However, there are some notable dissents from this apparent consensus. Price (2012) "analyzed the effects of leadership decapitation on the survival rate of 207 terrorist groups from 1970 to 2008" (Price, 2012, p. 11). He found that decapitation strikes against terrorist groups significantly increased their mortality rate as compared to groups that did not suffer such strikes (Price, 2012, p. 43), and that "religious terrorist groups were less resilient and easier to destroy than nationalist groups following leadership decapitation" (Price, 2012, p. 44). Joining this dissent is Johnston (2012), whose analysis of 90 counterinsurgency campaigns, beginning in 1975 and ending at 2003 (Johnston, 2012, p. 56), produced the following results:

> After correcting for the endogeneity and measurement issues that have hindered previous studies, I found that neutralizing insurgent leaders has a substantively large and statistically significant effect on numerous metrics of countermilitancy effectiveness. Specifically, the results showed that removing insurgent leaders increases governments' chances of defeating insurgencies, reduces insurgent attacks, and diminishes overall levels of violence. (p. 77)

Finally, as a caution against drawing hasty conclusions, Carvin's (2012) review of studies of targeted killing found that differences in definitions, data shortages, measurement difficulties, and differences between selected cases preclude any firm conclusions about the effectiveness of this strategy (Carvin, 2012, p. 529). As this review shows, the effectiveness of targeted killing is by no means established.

In the broader field of terrorism studies, scholars have examined the impact of economic conditions and regime types on terrorism, and these studies influenced my choice of control variables. Blomberg, Hess, and Weerapana (2004) developed a model of terrorism which theorized that groups seeking a greater share of the economic pie, but without the ability to directly influence their country's elites, choose between two methods of signaling desire for status quo change: rebellion or terrorism (Blomberg, Hess, & Weerapana, 2004, p. 469). This decision is influenced by the institutional conditions of the state: rich states have stronger institutions and militaries, which "raise the cost of rebellion to the point that dissident groups prefer to resort to terrorism" and vice versa (Blomberg et al., 2004, p. 467). They found in their analysis of terrorism incidents in 127 countries from 1968-1991 that "high income and democratic countries appear to have a higher incidence of terrorism" (Blomberg et al., 2004, p. 477). They also find that "periods of economic weakness increase the likelihood of terrorist activities" (Blomberg et al., 2004, p. 477). Weinberg and Eubank (1998) found similar results, concluding that "international terrorist events in the years 1994-95 occurred most commonly in countries externally judged to be democratic and free" (Weinberg & Eubank, 1998, p. 117). To ensure robustness, they used both a "Rand-St. Andrews 1994 chronology" of terrorist events and "the US State Department's chronology of international terrorist acts for 1995" for data for their dependent variable, and both Wesson's democracy classifications and Freedom House scores for their independent variable of government type (Weinberg & Eubank, 1998, p. 109).

More recently, Piazza (2006) conducted a "regression analysis of the incidence and casualty rates of terrorism in ninety-six countries between 1986 and 2002" (Piazza, 2006, p. 165). He also used US State Department data on "incidence and casualty rates of terrorism"

(Piazza, 2006, p. 165) and Freedom House scores to measure “the degree of political and civil freedom” in those countries (Piazza, 2006, p. 167). Piazza (2006) found the following:

> The statistical models above demonstrate that there is no empirical evidence to support the crux of the “rooted-in-poverty” thesis—popularized by world leaders, the media, and some scholars—that poor economic development, measured as low levels of per capita income, literacy, life expectancy, more equal distribution of wealth, growth of GDP, stable prices, employment opportunities, and food security, is related to increased levels of terrorism. (p. 170)

He argued that his findings provide support for social cleavage theory, reporting that “More diverse societies, in terms of ethnic and religious demography, and political systems with large, complex, multiparty systems were more likely to experience terrorism” than states lacking those traits (Piazza, 2006, p. 171). Similarly, Abadie (2006) found no “significant association between terrorism and economic variables such as income once the effect of other country characteristics is taken into account” (Abadie, 2006, p. 55). His economic variables included “the HDI and the Gini Index” as well as “log per capita GDP” (Abadie, 2006, p. 54). His “other country characteristics” included “the level of political rights, fractionalization, and geography” (Abadie, 2006, p. 54). He measured political rights using the Freedom House’s Political Rights Index (Abadie, 2006, pp. 51-52). Similarly, Walsh and Piazza (2010) found that infringing on the physical integrity rights of a population leads to increases in terrorism (Walsh & Piazza, 2010, p. 570) and that “relationships between institutional measures of democratic rule and terrorism largely disappear when one considers government behavior in the form of respect for physical integrity rights” (Walsh & Piazza, 2010, p. 571).

One additional area of research is also important for the development of my theory: the psychological and social science literature on terrorist radicalization. Moghaddam’s (2005) psychological model of terrorist radicalization has six stages, each a prerequisite for the next: individuals experience perceived deprivation, they see no possible legal solutions or personal means of escape from that deprivation, they learn to displace aggression physically, they begin to accept terrorist ideology, they begin active involvement with terrorist contacts, and finally commit an act of terrorism (Moghaddam, 2005, pp. 163-166). Speckhard's (2012) extensive field interviews with prospective and current terrorists supported Moghaddam's model by finding this pattern amongst interviewees in Chechnya (Speckhard, 2012, p. 25) and Palestine (Speckhard, 2012, pp. 100-101): traumatic experiences of violence combine with systemic feelings of powerlessness to produce individuals ripe for radicalization. She concluded from her fieldwork that warzone traumas can create psychosocial weaknesses that render individuals more susceptible to terrorist radicalization (Speckhard, 2012, pp. 771-773). It is important to note that drone strikes create such psychological traumas and feelings of powerlessness in those who experience them (IHRCRC & GJC, 2012, pp. 80-88), and thus are conducive to radicalization.

Given these findings, I theorize that drone strikes and terrorism are linked due to the inflammatory nature of drone strikes on the populations that experience them. This theory implies that drone strikes create a traumatized psychological state of powerlessness, fear, and anger, as those who experience them have no control over their occurrence and no recourse when strikes cause collateral damage. This psychological trauma both motivates further attacks by current terrorists and facilitates the radicalization of civilians. This leads to the hypothesis that drone strikes have a positive influence on terrorist attacks in a given region by increasing the pool of radicalized terrorist recruits available to execute them, while also inspiring retaliatory attacks due to anger at the strikes.

> *H1: drone strikes will have a positive influence on the incidence of terrorist attacks in the region in which the strikes occur.*

This theory also implies that drone strikes should increase the pool of available suicide bombers ready to be recruited. Thus:

H2: drone strikes will have a positive influence on the incidence of suicide attacks in the region in which the strikes occur.

Chapter 2

Method

I conduct a quantitative case study of Pakistan and Yemen during the years 2000-2014. I selected these two cases for their variance in terrorist and militant populations, as Pakistan saw an influx of Taliban and Al-Qaeda elements following the US invasion of Afghanistan in 2001, while Yemen saw no such influx. As noted above, the majority of studies that focused specifically on drone warfare examine only Pakistan. However, if this inflow of Taliban and Al-Qaeda elements affected the dependent variable, such as by providing the skill sets of experienced terrorists and so making terrorist attacks much easier within Pakistan, then research designs that only selected Pakistan as the case functionally selected on the dependent variable. My design uses the 2000-2014 timeframe because, while the first drone strikes in Pakistan and Yemen occurred in 2004 and 2002 respectively (Serle & Purkiss, 2017), starting in the year 2000 produces pre-drone-strike and pre-9/11 baseline values for terrorist attacks in both countries. I end in 2014, as both the civil war in Yemen and the rise of Islamic State would exert significant confounding effects if included in my timeframe.

Each model has one primary independent variable, three control independent variables, and one dependent variable. The primary independent variable is a measure of how many drone strikes occurred in a given month in a country, lagged and led n months on the dependent variable, allowing me to perform a time-lag analysis of the effects of strikes on attacks. My first control independent variable is a measure of whether conflict was occurring in the country during the strike, as active conflict in a country is a confounding variable due to the additional opportunities it creates for collateral damage and traumatization. My second control variable is a yearly measure of GDP, in order to control for the possible effects of economic conditions on terrorism. My third control variable is a measure of population. My first dependent variable measures terrorist attacks per month within each country in my sample, while my second measures suicide terrorist attacks per month within each country in the sample. The unit of analysis is country-month, and each dependent variable is both led and lagged out to twelve months to account for the possible delay between a drone strike and any attacks it inspired, and to address endogeneity concerns. Thus, this will create a total of 48 models for each state, with one model for each unit of country-month lag and one model for each unit of country-month lead with terrorist attacks as the dependent variable, and then one model for each unit of country-month lag and one model for each unit of country-month lead with suicide attacks as the dependent variable.

The data source for my primary independent variable is a dataset of drone strikes collected by The Bureau of Investigative Journalism (Serle & Purkiss, 2017). The Bureau of Investigative Journalism (The Bureau of Investigative Journalism [TBIJ], 2017a) collects its drone strike data from a range of international and local news sources (TBIJ, 2017a). Their dataset includes data on all known drone strikes in both Pakistan and Yemen from the first known strike in each country, which occurred in 2004 and 2002 respectively (Serle & Purkiss, 2017), through the present with ongoing updates (TBIJ, 2017a). The Pakistan dataset has an n of 424 strikes, with 408 of those falling in my timeframe for a final n of 408 (Serle & Purkiss, 2017), ranging from 0 to 23 strikes per month. The Yemen dataset has an n of 107-127 total confirmed strikes with an additional 81-97 possible strikes (Serle & Purkiss, 2017). In my analysis, I have included only confirmed drone strikes in Yemen for a total n of 78. A strike is added to these datasets if it is reported by one or more credible sources, but the Yemen data is further broken down into “confirmed” or “possible” US actions (TBIJ, 2017a) of various kinds (i.e. drone strike, cruise missile strike, ground operation) (Serle & Purkiss, 2017). These distinctions, unique to the Yemen data, are due to Yemen’s situational complexity as “The CIA, US special forces, and Yemeni air force all carry out strikes” while local media routinely report *any* air strike as a drone strike (TBIJ, 2017a). TBIJ (2017a) coding rules define “strike” as “a missile or set of missiles fired in at [*sic*] a single location in a short time window” (TBIJ, 2017a), coding missile hits that occur with more than an hour’s time difference, as well as simultaneous hits on targets a “couple of miles apart,” as separate strikes (TBIJ, 2017a). I have adopted their definitions and usage.

Data for the first control variable, which accounts for civil conflict, comes from the Political Instability Task Force State Failure Problem Set, which has data from 1995-2015 (Center for Systemic Peace, 2014). This dataset has five different kinds of state failure: consolidated cases, regime change, ethnic war, revolutionary war, and genocide/politicide (Center for Systemic Peace, 2014). However, the project’s codebook cautions that “When state failure events overlap or when five years or less separate the end of one event and the onset of the next, they have been combined,” to produce the consolidated cases (Marshall, Gurr, & Harff, 2016, p. 2), such that the Political Instability Task Force (abbreviated PITF) data has four distinct measures and one combined measure. This data is in case-year format, but contains starting and ending months for all conflicts that I manually coded into my dataset (Marshall et al., 2016, p. 2). I coded each country-month with a dichotomous 0-1 variable for each of the five different case types from PITF, where 1 indicates the occurrence of any number of conflicts of that particular case type during that month. I then summed these dichotomous measures to create an monthly index ranging from 0-5, where higher numbers indicate greater civil conflict. I also created an index that repeated this process but excluded all consolidated cases, with a range of 0-4. This was necessary because it is unclear if including the consolidated cases in the index is double-counting: the consolidated entry for Yemen specifically mentions all the revolutionary wars in the timeframe, but does *not* mention a regime change conflict that also occurred, while the Pakistan entry includes all ethnic wars in the timeframe. The codebook does not clearly state whether the consolidated cases should be counted as their own category or not, though its phrasing seems to imply that (Marshall et al., 2016, p. 2). As both indexes produced similar results in initial testing, I use the one without the consolidated category for all the regressions reported here.

For the second control variable, GDP, I use the International Monetary Fund's (2014) data for "Gross domestic product based on purchasing-power-parity (PPP) per capita GDP" in terms of current international dollars (International Monetary Fund, 2014). For the third control variable, population, I use the World Bank's (2017) online annual datasets for both Pakistan and Yemen (World Bank Group, 2017). Data for the dependent variable come from the Global Terrorism Database (National Consortium for the Study of Terrorism and Responses to Terrorism [NCSTRT], 2016c), which has recorded the number of terrorist attacks per month in both Pakistan and Yemen from 1970 to 2014, with annual updates (National Consortium for the Study of Terrorism and Responses to Terrorism [NCSTRT], 2016b). These data were gathered from "publicly available, open-source materials" such as news archives, data sets, and journal articles (National Consortium for the Study of Terrorism and Responses to Terrorism [NCSTRT], 2016a). This dataset uses a complex definition of terrorism, with an incident first required to be "an *intentional act of violence or threat of violence by a non-state actor*"

(NCSTRT, 2016a). After fulfilling that requirement, an incident had to meet two of the three following criteria to be included in the raw data: “The violent act was aimed at attaining a political, economic, religious, or social goal;” or “The violent act included evidence of an intention to coerce, intimidate, or convey some other message to a larger audience (or audiences) other than the immediate victims;” or “The violent act was outside the precepts of International Humanitarian Law” (NCSTRT, 2016a). When collecting the terrorist attacks variable data for my analysis using the Database’s online search tools, I required that all three terrorism criteria be met for an incident to be included in my dataset, excluded ambiguous cases, included unsuccessful attacks, showed any number of casualties with both injuries and fatalities in the casualty count, and left all other fields blank. This search’s results included the suicide attacks that were coded separately in the suicide attacks variable. When collecting the suicide attacks variable for my analysis using the same online search function, I again required all three criteria, excluded ambiguous cases, included unsuccessful attacks, showed any number of casualties with both injuries and fatalities in the casualty count, and checked “suicide attacks” in the Attack Type field, with all other fields left blank. For Pakistan, these searches yielded a terrorist attacks n=8605 and a suicide attacks n=371, with the number of non-suicide attacks being 8605-371=8234. For Yemen, this yielded a terrorist attacks n=1279 and a suicide attacks n=69, with the number of non-suicide attacks being 1279-69=1210. To be clear, the terrorist attacks variable’s count contains the suicide attacks as well, such that the suicide attacks variable data is strictly a subset of the terrorist attacks data.

When conducting my regressions, I use a negative binomial regression rather than an OLS model, because my dependent variables are count variables that do not conform to a normal distribution. This is a standard method for dealing with dependent variables that cannot be assumed to have a normal distribution (Cameron & Trivedi, 1986; Hilbe, 2011). There is also dependency between observations, which is an added reason to use a negative binomial model (Cameron & Trivedi, 1986). However, this means that the raw coefficients cannot be directly interpreted to find effect size, as in an OLS model. To interpret them, I ran marginal effects simulations. As noted above, each dependent variable is both led and lagged out to twelve months to account for delays and endogeneity concerns. This creates a total of 48 models for each state, with one model for each unit of country-month lag for each dependent variable, and one model for each unit of country-month lead for each dependent variable. For the sake of thoroughness, I conducted the same lead and lag regressions on the aggregate dataset, which was simply the data from each state combined together. That also produced 24 models for each dependent variable, for a total of 48 aggregate models, which were treated identically to the country-level models in all respects.

Chapter 3

Results

In the Pakistan models with terrorist attacks as the dependent variable, the drone strikes independent variable was significant to $p<.05$ and positive in almost all lead and lag models, with the only non-significant results occurring in the 11-month lead model and the 12-month lead model. Tables 1 and 2 show selected, representative regression tables for the lag and lead models. In those tables, the dependent variable is terrorist attacks per month, lagged/led n months on the primary independent variable, drone strikes per month. The lag/lead process caused a loss of 1 month of observations for each month of lag/lead, leading to the declining number of observations across models in both tables. However, the drone strikes variable was almost never significant for Yemen, with its only significant results appearing in the 12-month lead model and 8-month lag model, and both of those results being negative. In aggregated models, which combined data from both countries, the drone strikes variable was always positive and significant to $p<.05$. The country-level models, however, indicate that the Pakistan data were driving the relationship in the aggregate models. All models not shown in this section are available upon request, and all significant results in this section are robust to the controls.

As these models are negative binomial regressions, Figures 1 and 2 show the calculated marginal effect size of a 1-unit increase in drone strikes on terrorist attacks for these models. In both figures, all variables were held to their means except for the civil conflict variable, which was held to its median of 2. All effects shown in both figures were significant to $p<.05$. Notably, both lead and lag models show a similar pattern in the magnitude of their effects, with the greatest increase in effect size occurring in the transition between a 1-month and 3-month model, followed by a decrease in effect size. Figure 3 is a simulation of the effects of a 1-unit increase in drone strikes per month on subsequent counts of terrorist attacks per month, using the 1-month lag regression for Pakistan shown in Table 1. Predictions were significant at $p<.05$ for strikes 0 through 20, while strikes 21 through 23 were not significant. Figure 4 shows the outcomes of the same simulation, but using the 1-month lead regression model for Pakistan shown in Table 2. All predictions there were significant to $p<.05$ except for the 23-strike prediction. In both simulations, all other values were held to their means, excepting the conflict index, which was held to its median of 2, and the number of simulated strikes covered the full range of strike values in the data. Taken together, Figures 3 and 4 show the results of manipulations of the independent variable for the 1-month marginal effects lag and lead models for Pakistan, indicating that while drone strikes lead to increases in the number of terrorist attacks per month, approximately 12 and 10 terrorist attacks (respectively for the lag and lead models) would occur even in the complete absence of drone strikes.

Table 1. Representative Terrorist Attack Lag Regressions for Pakistan

	1 month	3 months	6 months	9 months
Drone strikes/month	0.026**	0.034***	0.038***	0.039***
	(-0.009)	(-0.01)	(-0.01)	(-0.01)
GDP	0.002*	0.001	0.001	0.001
	(-0.001)	(-0.001)	(-0.001)	(-0.001)
Population	0.053*	0.073***	0.082***	0.091***
	(-0.021)	(-0.017)	(-0.018)	(-0.019)
Civil Conflict	-0.361	-0.334	-0.509	-0.684**
	(-0.251)	(-0.242)	(-0.26)	(-0.254)
Constant	-9.835***	-11.169***	-12.132***	-13.039***
	(-1.427)	(-1.111)	(-1.203)	(-1.253)
Wald Chi^2	1040.88***	1087.76***	1027.88***	1165.54***
Pseudo R^2	0.2096	0.2077	0.2074	0.2159
Observations	179	177	174	171

Notes:
Dependent variable: terrorist attacks per month, lagged n months.
Negative binomial regressions.
Robust standard errors in parentheses.
*** p<.001, ** p<.01, * p<.05

Table 2. Representative Terrorist Attack Lead Regressions for Pakistan

	1 month	3 months	6 months	9 months
Drone strikes/month	0.038***	0.051***	0.038***	0.027*
	(-0.011)	(-0.012)	(-0.008)	(-0.011)
GDP	0.003***	0.003***	0.004***	0.004***
	(-0.001)	(-0.001)	(-0.001)	(-0.001)
Population	0.022	0.014	-0.008	0
	(-0.02)	(-0.017)	(-0.018)	(-0.016)
Civil Conflict	-0.501*	-0.46	-0.626*	-0.443
	(-0.24)	(-0.245)	(-0.258)	(-0.236)
Constant	-7.681***	-6.899***	-5.580***	-6.050***
	(-1.392)	(-1.161)	(-1.165)	(-1.118)
Wald Chi^2	1048.39***	981.73***	900.58***	918.83***
Pseudo R^2	0.2154	0.2161	0.2124	0.2146
Observations	179	177	174	171

Notes:
Dependent variable: terrorist attacks per month, led n months.
Negative binomial regressions.
Robust standard errors in parentheses.
*** p<0.001, ** p<0.01, * p<0.05

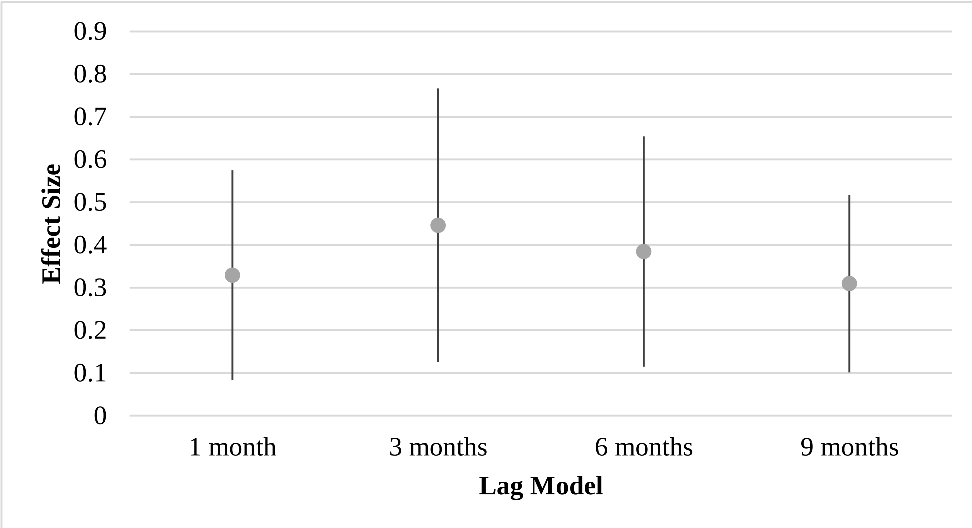

Figure 1. Marginal Effect Size Calculations for Table 1

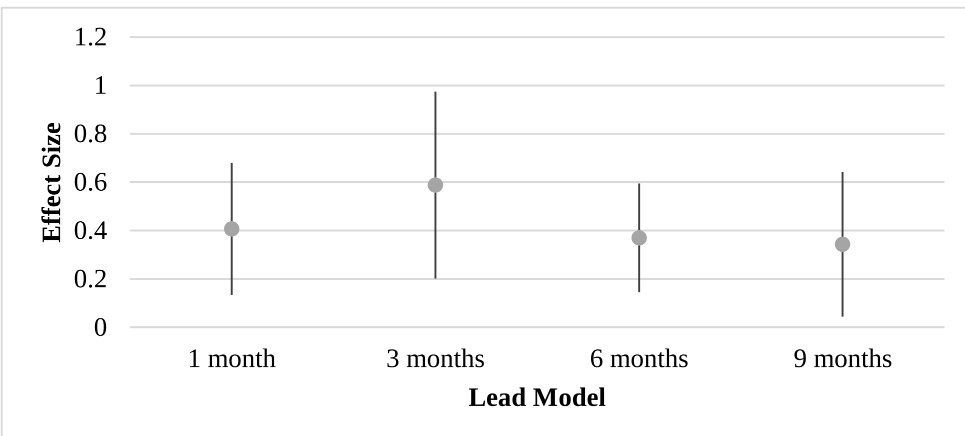

Figure 2. Marginal Effect Size Calculations for Table 2

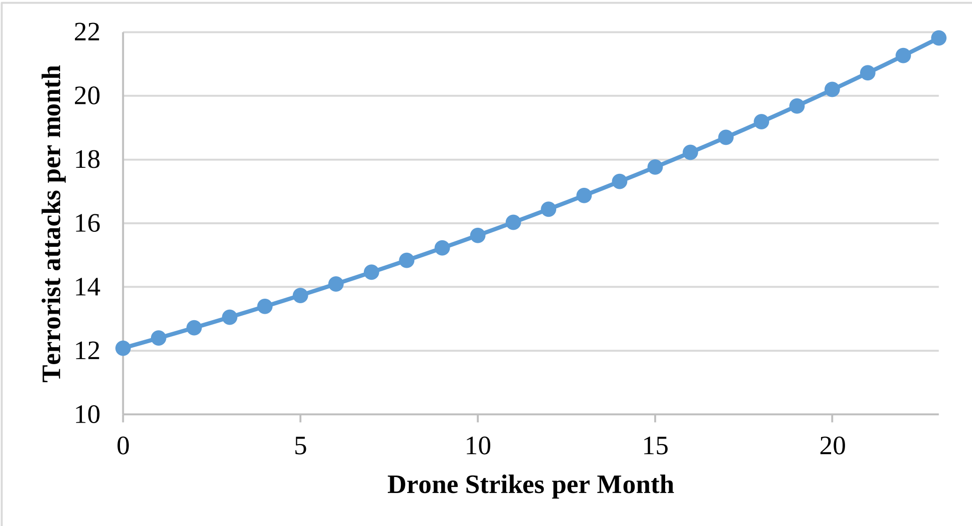

Figure 3. Marginal Effects Simulation for the 1-Month Lag Pakistan Model

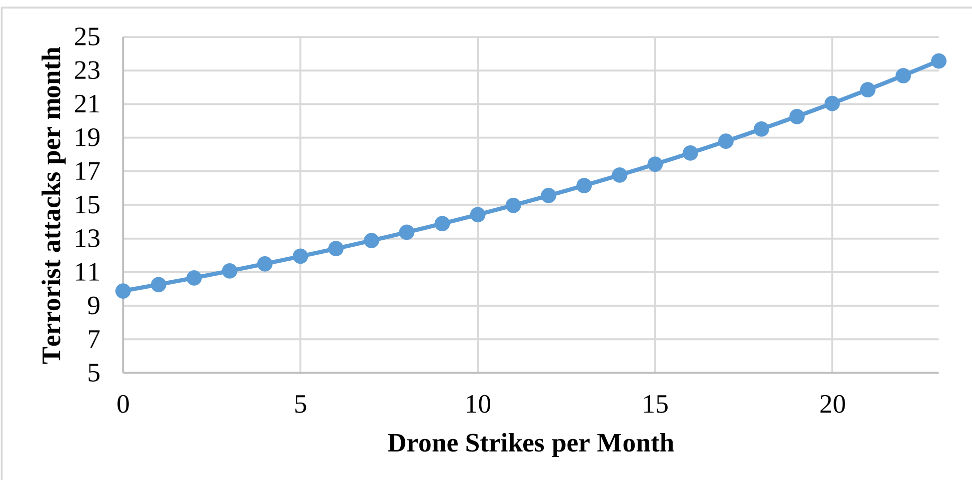

Figure 4. Marginal Effects Simulation for the 1-Month Lead Pakistan Model

Meanwhile, the suicide attacks variable was significant in fewer instances than the general terrorist attacks variable. In the aggregate model, it was significant ($p<.05$) and positive for 3-, 6-, 7-, 8-, and 9-month leads. It was also significant ($p<.05$) and positive for 2-, 5-, 9-, 10-, 11-, and 12-month lags. These results indicate that the relationship between drone strikes and suicide attacks is also endogenous, which is unsurprising given that the suicide attacks data is strictly a subset of the terrorist attacks data. When disaggregated by country, it becomes clear that the relationship between these two variables in Pakistan is, again, driving the relationship. Tables 3 and 4 show representative regression models for Pakistan from both lag and lead versions of the suicide attacks models. There, the dependent variable is suicide terrorist attacks per month, lagged/led n months on the primary independent variable, drone strikes per month. As in Tables 1 and 2, the lag/lead process caused a loss of 1 month of observations for each month of lag/lead, leading to the declining number of observations across models in both Table 3 and Table 4. While the disaggregated data for Pakistan showed significant ($p<.05$) and positive results for 3-, 4-, 6-, 7-, 8-, and 9-month leads and 2-, 5-, 6-, 9-, 10-, 11-, and 12-month lags, the only significant result for Yemen was for the 1-month lead. That single result was also positive.

As with the terrorist attacks models, these models are negative binomial regressions and thus require calculation of marginal effects. Figures 5 and 6 show the marginal effect size of a 1-unit increase in drone strikes on suicide attacks for these models. All variables in both figures were held to their means except for the civil conflict variable, which was held to its median of 2. None of the ranges shown in those figures were significant to $p<.05$, and the only discernable pattern in either figure is the widening of confidence intervals, shown most clearly in Figure 5 but faintly visible in Figure 6. Figures 7 and 8 show the results of manipulations of the independent variable for the 6-month marginal effects lead and lag models for Pakistan. Figure 7 simulated the effects of a 1-unit increase in drone strikes on subsequent counts of suicide attacks per month, using the 6-month lag regression for Pakistan shown in Table 3. Figure 8 simulated the effects of a 1-unit increase in drone strikes on subsequent counts of suicide attacks per month, using the 6-month lead regression for Pakistan shown in Table 4. In both simulations, all other values were held to their means, aside from the conflict index, which was held to its median of 2, and the number of simulated strikes covered the full range of strike values in the data. Notably, only the first prediction in each simulation was significant to at least $p<.05$. These both indicate that while drone strikes lead to increases in the number of suicide attacks per month, at least one attack would occur each month without the occurrence of any strikes.

Table 3. Representative Suicide Attack Lag Regressions for Pakistan

	6 months	9 months	12 months
Drone strikes/month	0.034*	0.064***	0.067**
	(-0.016)	(-0.019)	(-0.023)
GDP	0.005**	0.006**	0.005*
	(-0.002)	(-0.002)	(-0.002)
Population	-0.06	-0.08	-0.046
	(-0.046)	(-0.045)	(-0.052)
Civil Conflict	0.529	0.757	0.895
	(-0.774)	(-0.869)	(-0.882)
Constant	-4.082	-3.625	-5.779
	(-2.978)	(-2.887)	(-3.36)
Wald Chi^2	136.45	108.11	104.9
Pseudo R^2	0.2054	0.2266	0.2078
Observations	174	171	168

Notes:
Dependent variable: suicide attacks per month, lagged n months.
Negative binomial regressions.
Robust standard errors in parentheses.
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Table 4. Representative Suicide Attack Lead Regressions for Pakistan

	3 months	6 months	9 months
Drone strikes/month	0.052**	0.056**	0.035*
	-0.017	-0.017	-0.015
GDP	0.007***	0.007***	0.005***
	-0.002	-0.002	-0.001
Population	-0.115**	-0.116**	-0.082*
	-0.04	-0.038	-0.034
Civil Conflict	0.187	0.225	0.525
	-0.579	-0.563	-0.519
Constant	0.978	1.586	-0.048
	-2.61	-2.556	-2.414
Wald Chi^2	123.7	120.39	111.21
Pseudo R^2	0.1967	0.1949	0.1719
Observations	177	174	171

Notes:
Dependent variable: suicide attacks per month, led n months.
Negative binomial regressions.
Robust standard errors in parentheses.

*** p<0.001, ** p<0.01, * p<0.05

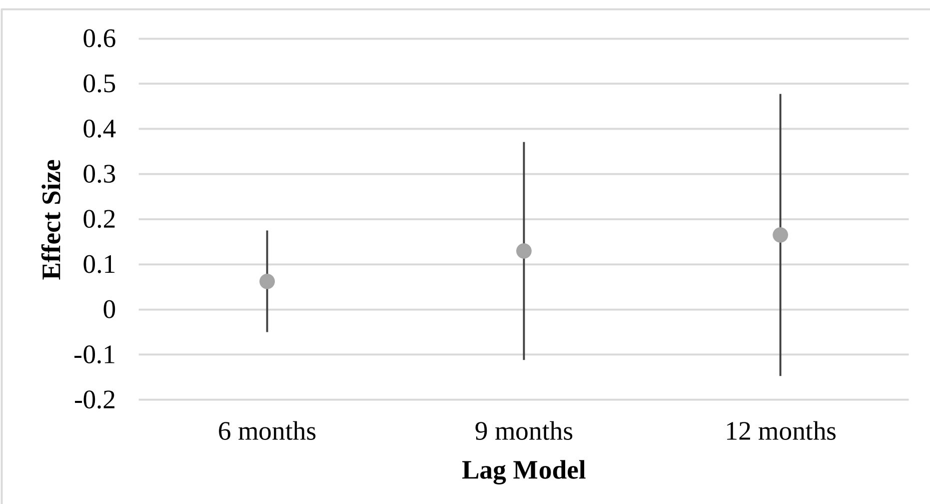

Figure 5. Marginal Effect Size Calculations for Table 3

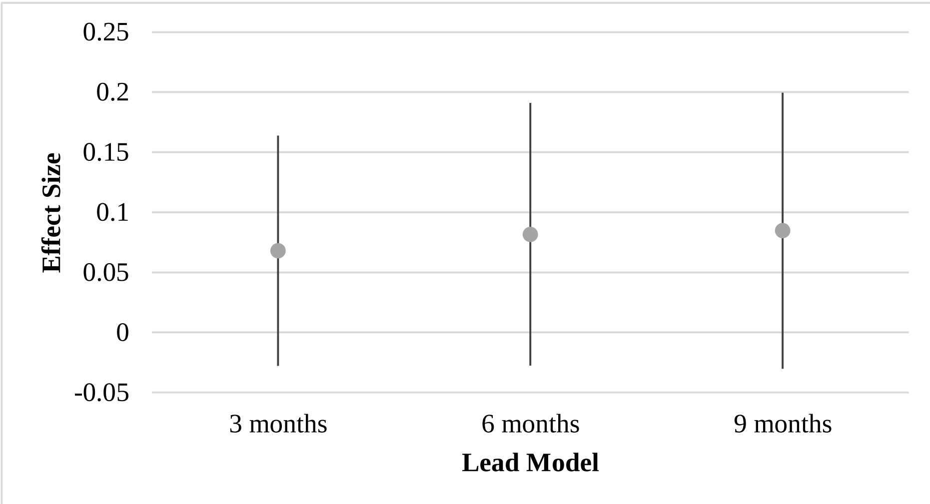

Figure 6. Marginal Effect Size Calculations for Table 4

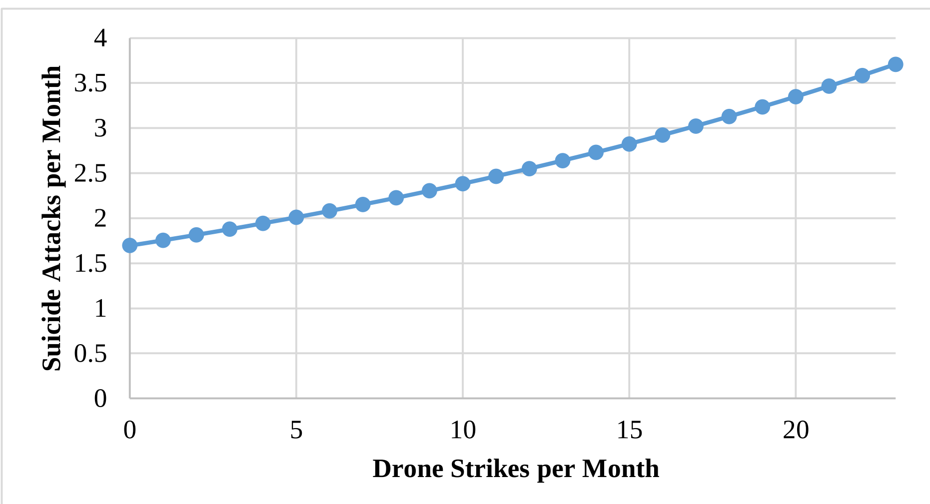

Figure 7. Marginal Effects Simulation for the 6-Month Lag Pakistan Model

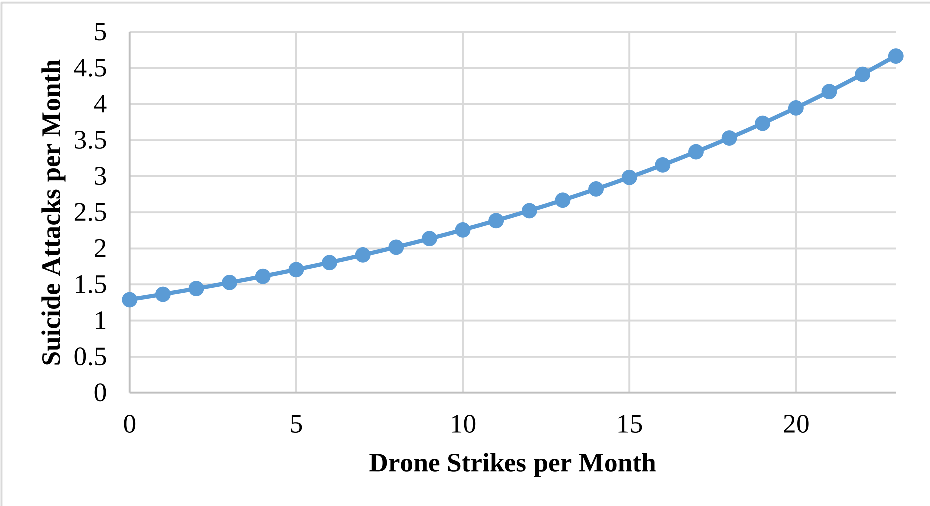

Figure 8. Marginal Effects Simulation for the 6-Month Lead Pakistan Model

Chapter 4

Discussion

These results indicate that the relationship between drone strikes and terrorist attacks is positive, complex, and endogenous in Pakistan, and that this Pakistan relationship drives the significant results of the aggregated models. Meanwhile, the comparative lack of significant results for Yemen implies that there is no real substantive relationship between the two variables in Yemen. As such, while my first hypothesis is technically supported for Pakistan, there is not enough substantive evidence to isolate the causal direction of this relationship. A very similar description pertains to the relationship between drone strikes and suicide attacks, with the primary difference being that the pattern of significance there was weaker overall. Again, while this also means that my second hypothesis is technically supported for Pakistan, I lack the ability to isolate the causal relationship.

Notably, these results clearly indicate that drone strikes do not reduce or even stabilize rates of terrorism, calling into question their efficacy as counterterrorism tools. Military drones have been touted by US government officials, including the former President Obama, as a precision weapon able to surgically eliminate terrorist targets with minimal or no collateral damage (Woodrow Wilson International Center for Scholars, 2012; The White House: Office of the Press Secretary, 2013). The counterpoint to such claims has been provided by investigations sponsored by NGOs and the United Nations. Such investigations have led to findings by the UN’s Special Rapporteur on Counter-Terrorism and Human Rights that at least 400 civilians died in US strikes in Pakistan (Office of the United Nations High Commissioner for Human

Rights, n.d.), a report by Amnesty International expressing grave concerns over whether some strikes were extrajudicial killings (Amnesty International, 2013), a conservative estimate by The Bureau of Investigative Journalism (TBIJ, 2017b) that 736 civilians had died in these strikes through early 2017 (The Bureau of Investigative Journalism [TBIJ], 2017b), and indications that those who survive drone strikes are often left with serious psychological scars (IHRCRC & GJC, 2012, pp. 80-88). While my findings cannot shed any definitive light on the questions of collateral damage or strike legality, both of which have been covered in the popular press (for example, see Walsh & Mehsud, 2013), they do contradict the portrayal of strikes as an effective tool of counterterrorism. While the baseline levels of terrorist attacks indicate that some level of terrorism would happen regardless of whether drone strikes occurred or not, the marginal effects models for Pakistan imply that drone strikes only exacerbate the problem of terrorism at the national level.

While all of the above holds for Pakistan, the lack of significant results for Yemen is a puzzle that must be explored. The first possibility is that the lack of a relationship in Yemen is due to a methodological weakness of this study, such as data collection issues. A second possibility, related to this, is the way in which my sample was heavily skewed towards Pakistan, with 78 drone strikes in Yemen but 408 strikes in Pakistan. A more advanced design, perhaps one that adjusted the weights of these strikes, might compensate for it. A third possibility is that terrorist groups in Yemen lacked the capacity or ability to engage in terrorist attacks of the kind collected by the Global Terrorism Database within my timeframe. A fourth possibility is that there is simply no relationship between these two variables in Yemen. In future research, a revised design could operationalize and test some of these possibilities.

The issues with Yemen also point to two general weaknesses of this study. The first is the reliability of the data used here. Due to the lack of official US government datasets on drone strikes, the TBIJ dataset's coders were forced to rely on media reports, creating the potential for errors introduced by inaccurate or ambiguous media coverage (TBIJ, 2017a), as well as potentially excluding strikes that did not receive sufficient media coverage to be noticed. As such, it is almost certain that this process introduced error into my dataset. Additionally, the Global Terrorism Database's documentation warns of potential issues: "Users should note that differences in levels of attacks and casualties before and after 1997, 2008, and 2012 may be at least partially explained by differences in data collection" (National Consortium for the Study of Terrorism and Responses to Terrorism [NCSTRT], 2016d). The GTD dataset has reliability issues due to its multi-staged data collection process, which occurred over a period of four decades under the supervision of several different institutions, during which multiple different coding rubrics were used (NCSTRT, 2016d). While the current dataset has been retroactively recoded with the most recent rules to ensure consistency (NCSTRT, 2016d), its reliability remains a concern due to this history. The second issue is a potential lack of generalizability of these results. The models here only have significant results for Pakistan, limiting their explanatory power to that state. They may not generalize beyond Pakistan. However, given that American drones have apparently only been widely used in Afghanistan, Pakistan, Yemen, and Somalia (Boyle, 2013, p. 3), this issue is less pressing than it would be if the population of states experiencing drone strikes were larger.

Despite these issues, this study shows that drone strikes have three distinct potential effects on the local and international environment. First, the positive endogenous relationship between strikes and terrorist attacks suggests that drone strikes may lead to higher levels of terrorist attacks even as those attacks lead to more retaliatory strikes, creating a continuous, self-reinforcing pattern of violence. Second, drone strikes may aid terrorist groups in their recruitment efforts, as newly traumatized strike victims may be more susceptible to recruitment (IHRCRC & GJC, 2012, p. 125; Speckhard, 2012, pp. 771-773). Third, drone strikes in general and strikes with high civilian casualties in particular may provide fruitful propaganda material for jihadist groups, which could be disseminated via the Internet to an international audience, potentially aiding the radicalization of "lone wolf" terrorists in states around the world. All of these potential effects are avenues for future quantitative research into this topic, which could also address some of the potential shortcomings discussed above. Despite widespread discussion of drone strikes, such research remains to be done. This study aims to provide a starting point for such research, in hopes of eventually answering the question of whether the utility of drones as a means of eliminating terrorist targets truly outweighs their wider repercussions.

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- **B. A. in International Politics, National Security Focus, December 2017**
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- Coded a dataset of criminal cases appealed to state supreme courts in all US states.
- Coded a dataset of provisions regarding rights and freedoms in the constitutions of all US states.
- Completed numerous smaller dataset projects.

Research Assistant to **Dr. Douglas Lemke** of Penn State - State College, PA
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- Conducted research into the histories of Libya, Mali, and Niger by examining scholarly publications, news archives, and various online databases for information regarding rebel groups constituting de facto states within those countries.
- Organized, analyzed, and summarized discovered data into project-relevant formats.

Education Service Specialist at **Educere, LLC** - Ambler, PA
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- Provided general course information, basic troubleshooting, and coursework clarification in a high-stress, high-intensity call center environment to students enrolled in online summer school courses.
- Contributed to expansion and fine-tuning of the corporate customer database through detail-focused data entry and intensive review of existing customer profiles.