

The Politicization of Crime: Elections and Maritime Piracy¹

Ursula E. Daxecker
University of Amsterdam

Brandon Prins
University of Tennessee

Electoral competition in weakly institutionalized settings has been shown to increase the incidence of violence. While much of the literature focuses on political violence, recent research links electoral processes to criminal violence, including homicides and drug violence. Theoretical explanations suggest that greater electoral competition creates crime as a consequence of more general political instability (Hoelscher 2015; Villarreal 2002). Electoral competitiveness, by virtue of threatening the ability of incumbents to maintain informal corruption agreements with criminal networks, is argued to undermine existing patronage networks and increase the incidence of crime. We draw on this work to link elections to maritime piracy, a form of criminal behavior that has increased substantially since the end of the Cold War and for which cross-national, temporally and spatially disaggregated data are available. We argue that elections can be piracy-inducing through three mechanisms: (1) elections threaten to disrupt collusion agreements with government officials, (2) elections provide elites with incentives to provoke piracy because its incidence is expected to influence electoral outcomes, (3) elections create a need for gains from piracy because elites require funds for electoral campaigns. These explanations suggest that elections - especially if competitive - should increase maritime piracy. We test our hypotheses on elections, competitiveness, and maritime piracy with cross-national data and subnational data for Indonesia.

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Introduction

Elections held in unconsolidated regimes are often accompanied by significant violence. The literature has linked elections to various types of political violence, including low-level violence, government repression, ethnic conflict, civil war and terrorism, yet comparatively little work has explored whether elections influence criminal violence. Important contributions by Villarreal (2002) and Hoelscher (2015) identify a *competition* effect, arguing that the prospect of electoral competition induces violent bargaining between criminal groups over future influence. Evidence from Mexico and Brazil shows that elections, especially if competitive, increase the incidence of homicides (Villarreal 2002; Hoelscher 2015). Yet we do not know whether these findings hold for other regional contexts and other types of crime, or whether alternative causal mechanisms might also influence the relationship between elections and crime.

Our paper makes three contributions to the literature on elections and violence. First, we hypothesize a link between elections and maritime piracy, a form of crime that has not yet been connected to electoral processes. Piracy reemerged with the end of the Cold War and is a form of criminal violence for which spatially and temporally disaggregated data are available cross-nationally, allowing for both global and subnational assessments. Some observers note an association between elections and piracy but do not explore it systematically. Amirell (in Chong 2008), for example, observes that piracy increased in the run-up to Indonesia's first democratic elections in 1999 but does not explore it further.² While some of our results are relatively weak and remain preliminary, both cross-national and subnational findings for Indonesia suggest that elections affect the incidence of piracy, which helps broaden the empirical relevance of the election-crime link. Second, we expand on the competition effect identified in existing work on elections and crime (Villarreal 2002; Hoelscher 2015) and develop three theoretical mechanisms. Elections can induce piracy (1) because they threaten to disrupt existing patronage networks between pirates and politically relevant actors, (2) because they give political elites incentives to manipulate elections by inciting violence, and (3) because they create a need for income from piracy to help fund electoral campaigns.

² Using data from MPELD, Indonesia experienced 140 incidents from 1995-1997 compared to 304 from 1998-2000, which includes the 1999 elections.

While we cannot neatly distinguish among the three explanations, we find evidence for an elections-piracy link. Third, an advantage of data on piracy is that they are based on reports from crew, ship owners, and government authorities, which should make them less susceptible to over-reporting during election periods than media-based data commonly used in studies of political violence.

We proceed as follows. We first review the literature on elections and political and criminal violence. We then outline three mechanisms connecting elections to increased piracy, concluding with two hypotheses. Our empirical section consists of two parts; a cross-national analysis of elections and piracy covering the 1993-2010 period, and a subnational analysis of piracy in Indonesia connected to first democratic elections in 1999.

Elections and Violence

Elections offer peaceful means for contestation yet are often accompanied by significant violence.³ A growing literature has linked elections to various forms of political violence, including low-level violence, government repression, ethnic conflict, civil war and terrorism (Goldsmith 2014; Cederman, Gleditsch, and Hug 2013; Salehyan and Linebarger 2014; Hafner-Burton, Hyde, and Jablonski 2014; Newman 2012; Fjelde and Höglund 2014). Research suggests that electoral processes in weakly institutionalized settings (including post-conflict environments) produce political violence because increased stakes in poorly constrained environments generate incentives for state and nonstate violence (Salehyan and Linebarger 2014; Hafner-Burton, Hyde, and Jablonski 2014). Similarly focusing on elections as high-stakes contests, Fjelde and Höglund (2014) demonstrate that the greater stakes introduced by majoritarian electoral rules are linked to higher incidence of political violence. Focusing on government violence, Hafner-Burton, Hyde, and Jablonski (2014) argue that incumbents use repression against voters and opposition if they are concerned about losing. Consistent with this emphasis on high stakes and incumbent concerns about performance, Cederman, Gleditsch and

³ Evidence linking elections to violence, however, does not imply that politics in countries with elections are more violent than in those without. Violence could occur around elections precisely because they are important, but elections could consequently decrease the potential for violence in non-election periods (Harish and Little 2013; Goldsmith 2015).

Hug (2013) show that ethnic civil wars are more likely to occur after competitive elections. While the literature discussed so far focuses on actors using violence for seeking power within the political system, work on terrorism and non-state actor conflict emphasizes the use of violence to disrupt electoral processes or even overthrow the status quo (Staniland 2014; Aksoy 2014). Newman (2012) finds that terrorist incidents peak around election time, and this pattern is particularly pronounced for dictatorships and wealthy democracies. In her analysis of democracies, Aksoy (2014) also finds that terrorism increases around elections, but only in democracies with low levels of electoral permissiveness. In sum, while theoretical arguments differ in the actors emphasized, these actors' goals, and the type of political violence involved, they agree that elections, especially if the stakes are high, are linked to various types of political violence. Empirical evidence ranging from low-intensity electoral violence, repression, civil war, to terrorism confirms a link between elections and violence.

In comparison, the literature on elections and criminal violence, i.e. violence committed by actors lacking clear political goals, is much less developed.⁴ This lack of interest is surprising considering that arguments on elections and political violence have implications for other forms of violence. If electoral competition fosters violence because it threatens elites concerned about their electoral performance in weak institutional environments, it should similarly threaten criminal networks in poorly institutionalized regimes. Collusion with political actors is crucial for the long-term viability of criminal organizations (Bailey and Taylor 2009), and the impending upheaval of electoral competition could thus lead to increased criminal contestation and violence. In an important contribution, Villareal (2002) argues that electoral competition and its potential to disrupt patron-client relations leads to increased homicidal violence in Mexico. Consistent with this expectation, he shows that rural municipalities where support for the incumbent was lower experience higher homicide rates. Similarly, Hoelscher (2015) hypothesizes that political competition threatens to disrupt coercive institutions used to protect political advantage and is thus linked to greater violence. Municipal-level results from Brazil confirm the link between elections, competitiveness, and homicidal violence (Hoelscher 2015). This evidence is also consistent with arguments linking

⁴ Aside from elections, the literature on criminal violence more generally neglects political variables .

democratization to criminal violence more generally, especially in the Latin American context (Grillo 2012; Kalyvas 2015). Yet in contrast, other research hypothesizes a political business cycle and argues that elections can be crime-reducing since political elites have incentives to reduce crime in the run-up to elections (Meloni 2012; Ghosh 2006). While this argument hinges on the assumption that concerns over crime rank highly for voters, subnational analyses of Argentina and India in Meloni (2012) and Ghosh (2006) show a reduction of crime before elections, although this effect is absent for violent crime. In sum, while sparse, existing work on elections and crime suggests that the potential disruption caused by electoral processes can be linked to increasing criminal violence. The effect of elections on non-violent crime, in comparison, is more uncertain.

In the following section, we elaborate theoretical mechanisms linking elections to maritime piracy, a form of criminal behavior that has increased substantially since the 1990s.

Electoral Competition and Maritime Piracy

Maritime piracy reemerged as a common form of criminal violence with the end of the Cold War. Compared to other crime such as robberies, rape, or homicides, piracy is geographically more limited since it occurs at sea or in ports. Yet piracy nevertheless affects many coastal states. Between 1993 and 2014, 97 of 177 states with coastlines experienced at least one piracy incident, 47 coastal states experienced more than 10 incidents, and 13 coastal states experienced over 100 pirate attacks.⁵ Conceptually, many piracy incidents (particularly those occurring in ports) may be closest to armed robbery, although piracy incidents can involve significant violence against crewmembers (or the threat thereof) and may also involve the hijacking of crewmembers and/or ships and bargaining for ransom with foreign ship owners. As with other crime, maritime piracy encompasses individuals engaging in sporadic, isolated acts but also small and medium-sized groups with substantial organization and hierarchy (Hastings 2012).

⁵ Information on states and coastlines comes from the Correlates of War project and the CIA factbook, respectively. Piracy data come from the Maritime Piracy Event and Location Data, version 5.0, see <http://brandonprins.weebly.com/minervaresearch.html>.

Scholarship on piracy has not yet examined whether electoral processes influence pirate organizations and the incidence of piracy. Existing research has focused on the role of weak institutions and lack of legal employment opportunities in creating permissive conditions for piracy (Hastings 2009; Jablonski and Oliver 2013; Daxecker and Prins 2013; Daxecker and Prins 2015). We theorize three potential mechanisms through which elections induce piracy: a disruption, a manipulation, and a revenue mechanism.

The disruption mechanism draws on existing literature on elections and criminal violence. Building on this work, we argue that elections threaten to disrupt existing agreements between piracy groups and local elites. Collusion between pirates and local authorities is often noted as critical for flourishing pirate organizations (Hastings 2012; Shortland and Varese 2014; Hastings and Phillips 2015). Hastings (2012: 689), for example, observes that incidents in the South China Sea essentially disappeared once the government cracked down on pirate-government collusion. Empirical studies of piracy in Africa and Southeast Asia have also highlighted the presence of pirate group connections with government actors (Hastings 2012; Hastings and Phillips 2015; Shortland and Varese 2014; Pérouse de Montclos 2012). Pirate organizations might thus be concerned about potential disruption imposed by elections, in particular if those elections are expected to be competitive in the regions where they are active.⁶ These electoral concerns could translate into increasing numbers of attacks because pirate groups are unsure about their future ability to continue operating. Different from existing work on elections and crime, we thus do not anticipate increases in crime because pirate groups violently compete with each other over future influence. Rather, pirates fear a disruption of pirate businesses after elections and increase the rate of attacks in anticipation.

The second mechanism views piracy as an electoral manipulation strategy used by political elites. A rapidly growing literature is exploring the varieties, causes, and consequences of electoral manipulation in weakly institutionalized settings (for a review, see Mares and Young 2016). As mentioned in the discussion of elections and political violence above, elites use violence as a form of manipulation to influence electoral

⁶ While our cross-national analysis focuses on national elections, local elections may be equally if not more relevant for this mechanism. Unfortunately, election dates for local elections are not available for a global sample. Our subnational analysis, however, covers both national and subnational elections.

outcomes, such as deterring voters or weakening opponents. Criminal acts could similarly be used to manipulate elections. With regard to piracy, elites could contract local pirates to carry out incidents in an effort to undermine their opponents and make them appear incompetent. For example, Perouse de Montclos (2012: 536) describes how politicians in the Niger Delta hired pirates to show the inability of the federal government and regional governor to maintain order in the run-up to the 2011 general elections in Nigeria. The incitement of maritime piracy could thus be a strategy to undermine opponents, leading to an increase in piracy incidents before elections.⁷

In the third mechanism, piracy is a strategy to raise income for electoral campaigns. This mechanism should be most prevalent in the most corrupt piracy prone states, such as Somalia or Nigeria, since significant financing of electoral campaigns from piracy is likely problematic in more stable states with substantial piracy, such as Indonesia. Shortland and Varese (2014) identify a link between funds for electoral competition and piracy in Somalia. Citing documents from the UN Security Council, they note that pirate money “was used to fund the election campaign of Abdirahman Farole for the presidency of Puntland” (Shortland and Varese: 16). While their evidence remains limited and descriptive, we consider the use of pirate money for election campaigns a third plausible mechanism.

All three mechanisms suggest that piracy increases in the run-up to elections, whether it is because of anticipated disruption, to manipulate outcomes, or to raise funds for campaigns. It is however challenging to strictly distinguish among the empirical implications of the above mechanisms. Arguably, there may be some differences in the expected timing of piracy, where piracy as manipulation should occur close to the polling, whereas piracy as fundraising would likely happen several months or longer before elections since it will take time to convert loot in black markets or because ransom negotiations tend to be lengthy. For the disruption mechanism, its implications for the

⁷ One concern regarding this mechanism stems from work on elections and the political business cycle. Incumbents might have incentives to do precisely the opposite, that is, to pay off local thugs to ensure that piracy subsides before elections in order to improve voters’ evaluations (Meloni 2012; Ghosh 2006). In the absence of systematic empirical assessments, it is difficult to know which effect dominates although presumably it is conditional on whether incumbents or the opposition control coastal areas favorable for piracy.

timing of attacks vis-à-vis elections are ambiguous. In sum, we do not think that these differences in timing will be pronounced enough to specify separate hypotheses. Similarly, the competitiveness of elections at the aggregate level should matter for all three mechanisms. Whether elections are piracy-inducing because they threaten to disrupt, because they incentivize manipulation, or because they create a need for revenue, all mechanisms should become more prevalent the more competitive elections are. We thus only formulate preliminary empirical implications on the piracy-increasing effect of elections and their competitiveness, but do not (yet) distinguish among the mechanisms.

Hypothesis 1: The holding of elections increases the incidence of piracy before and during the polling.

Hypothesis 2: During election periods, more competitive contests increase the incidence of piracy.

Research Design

We test the empirical implications of our arguments with cross-national and subnational data. While a cross-national comparison provides us with leverage for the generalizability of our results, it loses important information about subnational effects of elections and the spatial variation in the location of piracy incidents. We first present cross-national analyses, which provide modest support for a general effect of elections on piracy (hypothesis 1), but do not find evidence for an effect of competitiveness (hypothesis 2). We then proceed to subnational analyses of elections and piracy in Indonesia, which are better suited to examine the second hypothesis because of better measures and significant spatial variation in competitiveness and piracy. Findings for Indonesia show a link between the competitiveness of elections and piracy.

Cross-National Analysis: Data and Variables

The unit of analysis in our cross-national analyses is the country-month. Our sample consists of all 97 coastal states experiencing at least one piracy incident between 1993-

2010.⁸ Our theoretical mechanisms presuppose that piracy is a viable option available to elites during electoral periods, which would make it problematic to include states where piracy never occurs. We start in 1993 because piracy data for earlier years are limited, and end in 2010 because the data used for elections were not available for later years. For hypothesis 1, our sample includes country-months with and without election periods, whereas we limit the sample to election-months for hypothesis 2.

Data for maritime piracy, the main dependent variable, come from the Maritime Piracy Event and Location Data (MPELD). These data (when complete) will combine data from the International Maritime Bureau (IMB), the International Maritime Organization (IMO), and the Anti-Shipping Activity Messages (ASAM). At the time of writing, MPELD contains only IMB data but all incidents have been examined for errors and geocoded.⁹ The data contain 6,330 incidents covering the period from 1993-2014. Our dependent variable is a count of piracy incidents attributed to each country per month.¹⁰ The variable ranges from 0 to 21 (Indonesia in April 2003). We use negative binomial estimation because our dependent variable is an event count with overdispersion.

Our main independent variables focus on the presence of elections (hypothesis 1) and the competitiveness of elections (hypothesis 2), respectively. Data for elections come from the National Elections in Democracies and Autocracies dataset (NELDA, (Hyde and Marinov 2012). We include executive and legislative national elections.¹¹ To assess hypothesis 1, we create several dummy variables for elections. First, we create a dummy variable coded 1 during an election month, 0 otherwise. Second, we create a dummy

⁸ Of 177 states with coastlines, 97 experienced at least one incident. Restricting the sample to states experiencing some piracy likely also captures states with weak institutional environments, where arguments on elections and crime should be most relevant.

⁹ In line with the IMB definition, incidents are included if they meet UNCLOS definition of piracy or the IMO's definition of armed robbery.

¹⁰ We assigned pirate incidents to states if they occurred within 12 nautical miles of coastlines, to the country from which the pirates originate (if indicated in the IMB report), or to the coastal country closest to the pirate incident for incidents outside of 12 nautical miles. ArcGIS was used to assign incidents to individual countries.

¹¹ As mentioned above, dates for local elections are difficult to find for a global sample, but we hope to include subnational elections in future versions. We further plan to distinguish among election types in the future.

variable coded 1 in the month before, during, and after the election, and 0 otherwise. Third, we create a series of lead variables to assess the effect of elections on piracy in the months before elections.¹² These dummy variables are coded 1 in the 12 months leading up to an election, 0 otherwise. To assess hypothesis 2 on the competitiveness of elections, we limit the analysis to election periods since the expectation expects a difference between elections that are expected to be competitive compared to elections that are not (rather than comparing them to non-election periods). We thus limit the sample to election periods (393 observations). We use two proxy variables from the NELDA data to capture competitiveness. The first one is a dummy variable coded 1 if incumbents are confident that they will win elections, 0 otherwise. The second variable is a dummy coded 1 if a country is holding first competitive, multi-party elections, which we should plausibly expect to be more competitive. More direct measures of competitiveness, such as margin of victory, are not available in NELDA or other global datasets on elections.

We control for a variety of factors that likely also affect piracy, including state capacity, GDP per capita, population size, the occurrence of civil war, and temporal dependence. We use data on government effectiveness from the World Bank Governance Indicators to control for state capacity (Kaufmann, Kraay, and Mastruzzi 2009). Data on per capita GDP and population size also come from the World Bank. The civil war variable is a dummy coded 1 if a country experienced ongoing civil war, 0 otherwise. Data come from the UCDP Dyadic Dataset, version 1-2014 (Gleditsch et al. 2002). We control for temporal dependence with a three-month moving average of the dependent variable.

Cross-National Analysis: Descriptives

We begin with a descriptive assessment of the incidence of piracy around elections. Using data for all countries experiencing at least one piracy incident in 1993-2010, we plot the incidence of piracy across countries for 365 days before and after elections. Centered on the date of the election (0 on the x-axis), figure 1 shows the number of piracy incidents on each day across the 97 countries with one or more piracy incident.

¹² These specifications are most consistent with our argument on how the period before and during elections should increase the risk of piracy.

The figure shows no clear peak on election day and a rather ambiguous pattern for the year before and after elections. There appears to be somewhat of an increase in the three months before elections, followed by a decline in the month after, but overall the figure does not indicate a strong relationship between elections and piracy.

FIGURES 1&2 HERE

For comparison purposes, we create a similar figure for terrorism and elections. The literature has indicated an effect of elections on the incidence of various forms of political violence, including terrorism. We use data from the Global Terrorism Database (GTD, (LaFree, Dugan, and Miller 2014) to create the identical figure as for piracy. Unlike for piracy, there is a clear peak on election day, which shows by far the highest number of terrorist events. Aside from the peak on election day, terrorism also increases 1-2 months before elections, and drops off immediately after, but for earlier and later months, the pattern is also ambiguous. A caveat regarding the peak of terrorism on election day arises from the GTD's reliance on media reports for data collection. Elections as high-stakes events are likely seen as more "newsworthy" by reporters, raising the possibility that terrorist events occurring outside of the electoral cycle are not reported (Earl et al. 2004). In contrast, data on piracy collected by the IMB are based on self-reporting of events from crews, ship owners, and/or government authorities, which should make them less prone to over-reporting during electoral periods. From this comparative descriptive exercise, we infer that compared to terrorism, there appears to be only a weak temporal pattern linking elections and piracy.

Cross-National Analysis: Results

We first present and discuss results for hypothesis 1. The coefficient plot (figure 3) shows coefficients for election variables and controls in three models.¹³ In model 1, we include the dummy coded 1 for election-months, 0 otherwise. We see a weakly significant positive effect of elections on piracy for this variable. In model 2, we include a variable coded 1 in the month before, during, and after elections. This variable is positive and significant, again indicating modest support for the first hypothesis. Model 3 includes lead dummy variables for up to 12 months leading up to elections, capturing the more

¹³ Table 1 in the appendix presents these same results.

long-term anticipation of elections and its effect on piracy. These results are fairly disappointing, showing that no lead except the election-month dummy significantly affects piracy. This finding appears most consequential for the revenue mechanism, because income from piracy occurring in election-months could not be used to fund campaigns.

FIGURES 3&4 HERE

Figure 4 plots the coefficients for model 4, presenting results for hypothesis 2. We limit the sample to election periods and include dummies for incumbent confidence and first elections. Neither coefficient is statistically significant. In addition to using weak proxies, measuring competitiveness at the election-level also means losing lots of spatial variation, which is why we proceed to subnational analyses of competitiveness and piracy.

Sub-National Analysis of Indonesia: Case Selection, Data and Variables

We choose Indonesia as the case for subnational analysis because it offers lots of variation in electoral competition and piracy. We purposely choose a case with pervasive piracy since the implications of our arguments should be most apparent in countries where pirates are historically active and where organized piracy is present. In addition, Indonesia is a useful case with regard to independent variables. Before the fall of Suharto in 1998, elections were not competitive and ought to not have affected Indonesian pirates' considerations. The June 1999 elections were the first elections with multi-party competition and this prospect should have impacted pirate organizations as hypothesized above. As first democratic elections, they are thus well suited for testing the argument (we hope to add future Indonesian elections later). These analyses focus on hypothesis 2 and do not examine hypothesis 1 because the presence or absence of elections does not offer much interesting spatial variation (national elections would affect all regions simultaneously, and local elections could be held at different points in time, spatial variation would still be sparse).

Our unit of analysis for Indonesia consists of PRIO grids (55x55 kilometer cells) within Indonesia's Exclusive Economic Zone (EEZs) (Tollefsen, Strand, and Buhaug

2012).¹⁴ 2,364 grids fall within Indonesia's EEZ, which extends (up to) 200 nautical miles from its coastline. We choose EEZs rather than territorial waters since most accounts suggest that piracy in this area is committed by Indonesian pirates (Nautilus Institute 2007).

Our dependent variable measures the number of piracy incidents for the years 1998-2000 in each grid. Data come from MPELD and we calculate the sum of piracy incidents for each EEZ grid. Our data thus link elections to piracy in the year before, the year of, and the year after the 1999 elections. The variable ranges from 0 to 20, and 3.3% of all grids experience one or more incident. Our estimation method is again negative binomial regression.

Our key independent variable measures electoral competition. We use province-level election results from the Global Elections Data (Brancati 2014) to calculate the margin of victory in each of the 34 provinces for the 1999 elections.¹⁵ Since citizens voted in both national legislative (Lower House) and provincial elections, we calculate the margin of victory for both. Margin of victory is calculated by subtracting the second-place party's votes from the winner and then dividing this number by total votes. Smaller values thus indicate more competitive elections. Because election happen on land and piracy at sea (or in ports), we subsequently create buffers of the two margin variables that extend to all EEZ grids. We then calculate average values for margin buffer variables for all grids. Margin ranges from 0.03 to 0.48 in legislative elections, and 0.03 to 0.47 in provincial elections. Since this procedure could overestimate the effect of victory margins in grids far away from the coastlines, we also interact our measures with the distance grid-coast control variable described below (see also figures 7&8).

The map below shows Indonesia's provinces with victory margins (with lighter orange shades representing smaller margins) and Indonesian EEZ grids. The black dots show piracy incidents from 1998-2000.

¹⁴ PRIO GRID is available at <http://grid.prio.org/#/download>. EEZ shapefiles come from <http://www.marineregions.org/downloads.php>. ArcGIS 10.3 was used to create the dataset and calculate distance controls.

¹⁵ Provinces are the first-order administrative units. Electoral districts would be preferable but no shapefile of the 77 districts exists (although we plan to create our own).

FIGURE 5 HERE

For controls, we try to keep them comparable to the cross-national data as much as possible. Most controls come from data available in the PRIO GRID. To control for economic development, we use data on nighttime light emissions for all grids in Indonesia. For nighttime lights, we use a similar procedure with buffers as described for elections above.¹⁶ Data for population size are unfortunately missing for Indonesia. We include data for rainfall in grids because excessive rains could reduce the potential for piracy (Schneider et al. 2011). Another control variable measures the kilometer distance between each grid and Indonesia's coastline. The variable ranges from 0 (for grids adjacent to Indonesia's coast) to 422 kilometers. The statement that the origins of piracy are land-based has become a cliché in the piracy literature, but suggests that we should expect that grids at greater distances experience less piracy. We account for temporal dependence by calculating the average number of piracy incidents in MPELD in each grid from 1995-1997. The distance (in kilometers) between the Malacca Straits - a major shipping chokepoint creating opportunity for piracy - and each grid is included as a control in models 7 and 8.

Subnational Analysis of Indonesia - Results

Figure 6 presents a coefficient plot with four models on competitiveness and piracy in the 1999 elections.¹⁷ Model 5 includes the measure for victory margins in national legislative elections. The coefficient is negative and significant, indicating that larger margins (=less competitive elections) reduce the incidence of piracy. Model 6 includes the margin variable for provincial elections. Consistent with model 1, larger margins lower the expected incidence of piracy. Both models thus support the second hypothesis. Grids in the proximity of less competitive elections experience less piracy.

FIGURE 6 HERE

Models 7 and 8 present the same measures for competitiveness in national and provincial elections but include a control for distance of each grid to the Malacca Strait. Figure 5 illustrates the heavy clustering of piracy close to the Straits, a major shipping

¹⁶ DMSP OLS Nighttime Lights, data from <http://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html>

¹⁷ Table 2 in the appendix presents these same results.

chokepoint. In these models, results remain consistent for margins in provincial elections, whereas the coefficient for national margins is no longer significant.

For controls, results are mostly in line with expectations. Grids further away from the coast are less likely to experience piracy. Earlier piracy incidents in years 1995-1997 increase the risk of piracy in 1998-2000. Distance to the Malacca Straights reduces piracy incidence. Somewhat contrary to expectations, proximity to land grids with higher nighttime lights emissions increases the risk of piracy, although this measure might also capture grids with greater economic activity and thus more opportunity for piracy.

A final model (results not shown) interacts victory margins in legislative elections and the distance between grids and the coast. Our results on winning margins and piracy are more plausible if they have stronger effects on grids closer to coastlines, since it is more difficult to establish from which land areas piracy hundreds of kilometers away from the coast originates. Figures 7&8 show a predicted probabilities and marginal effects plots of this interaction. Figure 7 shows the expected number of piracy events for grids adjacent to the coast (distance=0) and grids at one standard deviation above the mean of distance (distance=189). The results fit our expectations: Greater margins reduce piracy in grids on to the coast (blue line), whereas the effect is absent for grids far away (red line). The overlaid kernel density plot of victory margins also shows that the significant effect cover empirically relevant cases. Figure 8, the marginal effects plot, shows that the conditional effect of distance is statistically significant for ranges of margins from 0 to 0.35.

FIGURES 7&8 HERE

Conclusion

An emerging literature links elections to criminal violence, in particular homicides. Our paper is the first to examine whether elections similarly create incentives for maritime piracy. We theorize three theoretical mechanisms suggesting that elections increase piracy because of disruption, incentives for manipulation, or the need to fund electoral campaigns. We find a modest effect of elections on piracy in a sample of all states with at least one piracy incident. Cross-national analyses, however, show no effect of competitiveness when we restrict the sample to electoral periods. Since dummy variables

of electoral competition in these analyses are poor proxies and over-aggregate significant subnational variation in competitiveness and piracy, we examine the effect of competitiveness on piracy in a disaggregated analysis of piracy for first democratic elections in Indonesia. Results show that close elections (whether legislative or provincial) increase the risk of piracy, particularly in grids close to the coast. While cross-national results are relatively weak, and while there are some inconsistencies between cross-national and subnational findings, the evidence on balance supports a piracy-increasing effect of elections rather than political business cycle arguments.

Figures

Figure 1: Daily incidence of piracy between 1993-2010, centered on election day

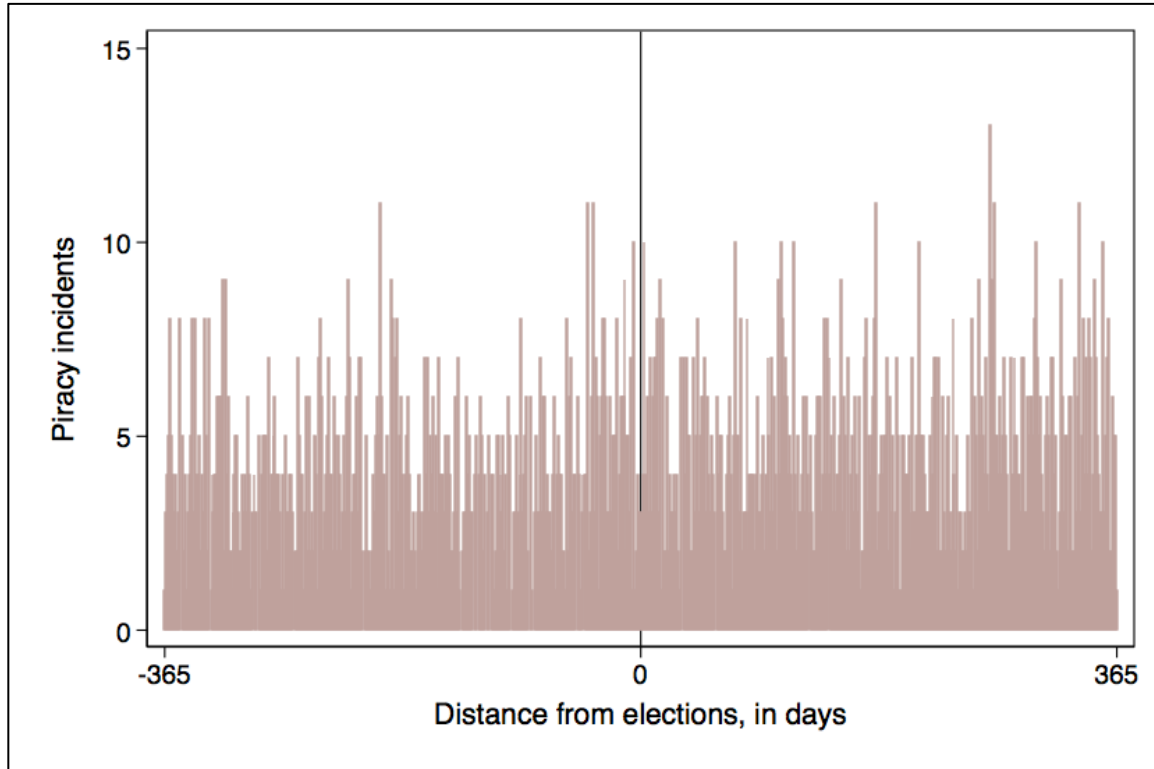


Figure 2: Daily incidence of terrorism between 1993-2010, centered on election day

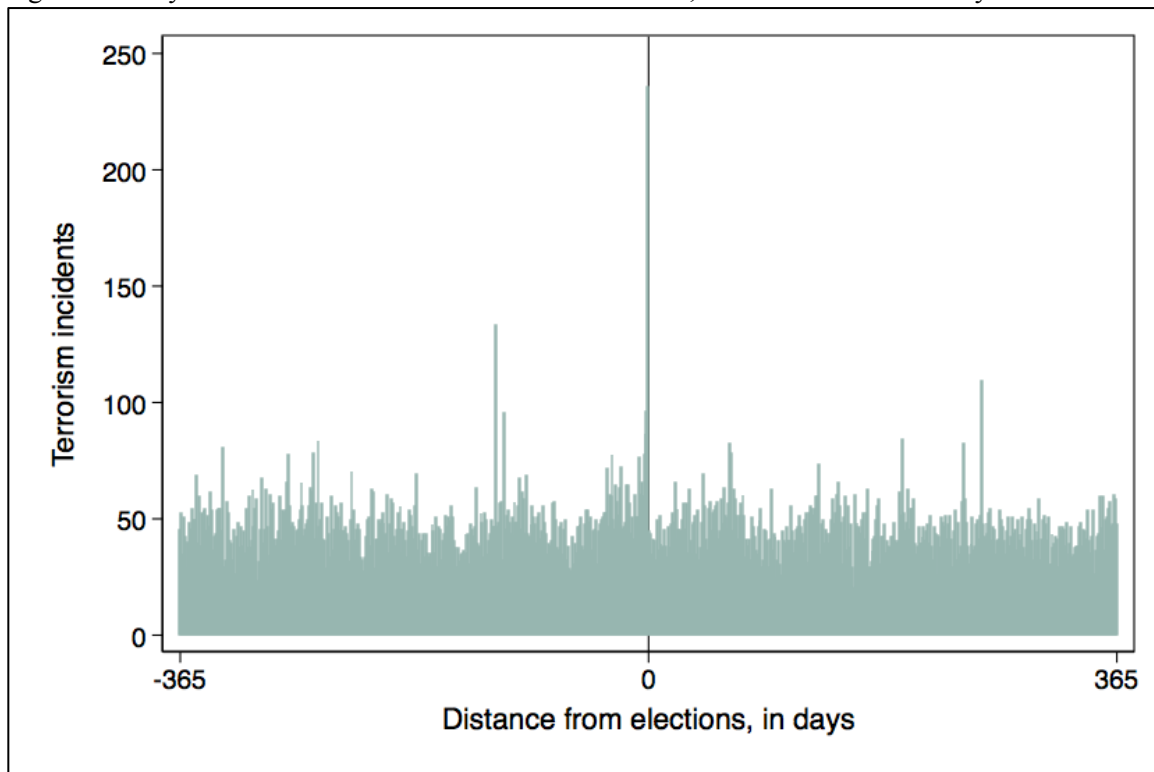


Figure 3: Coefficient plot elections and piracy 1993-2010, models 1-3

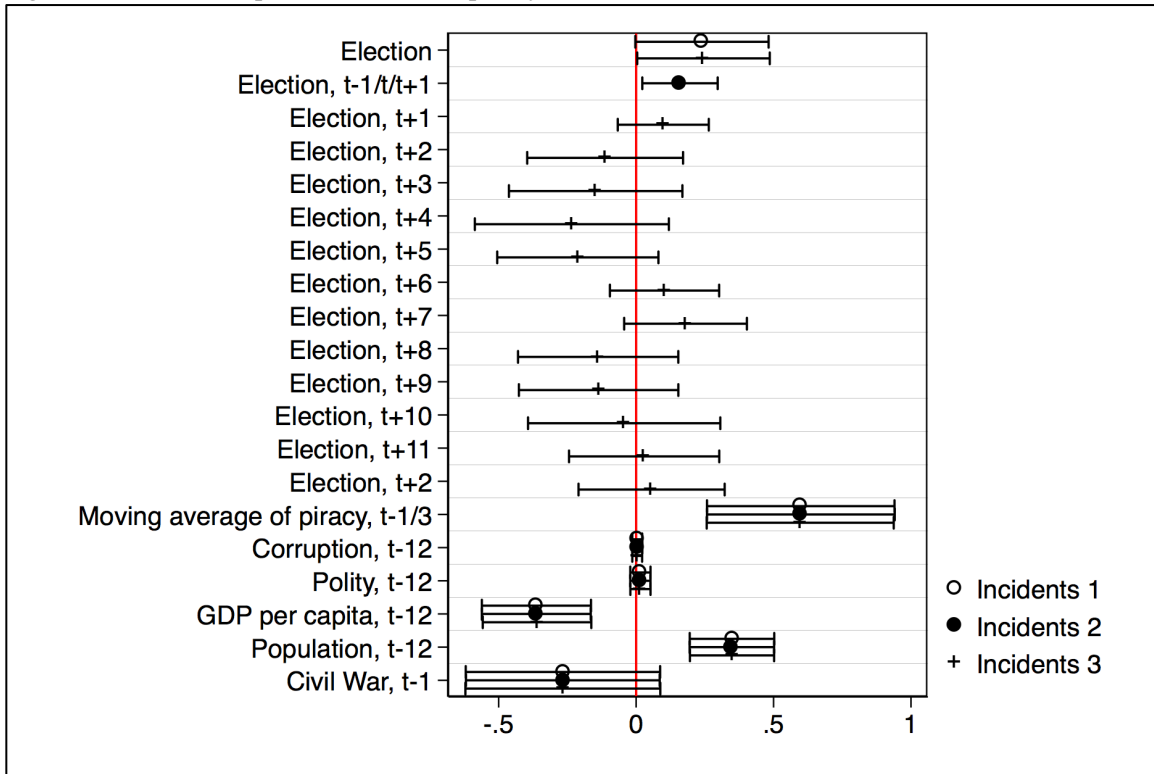


Figure 4: Coefficient plot elections and piracy 1993-2010, only election periods, model 4

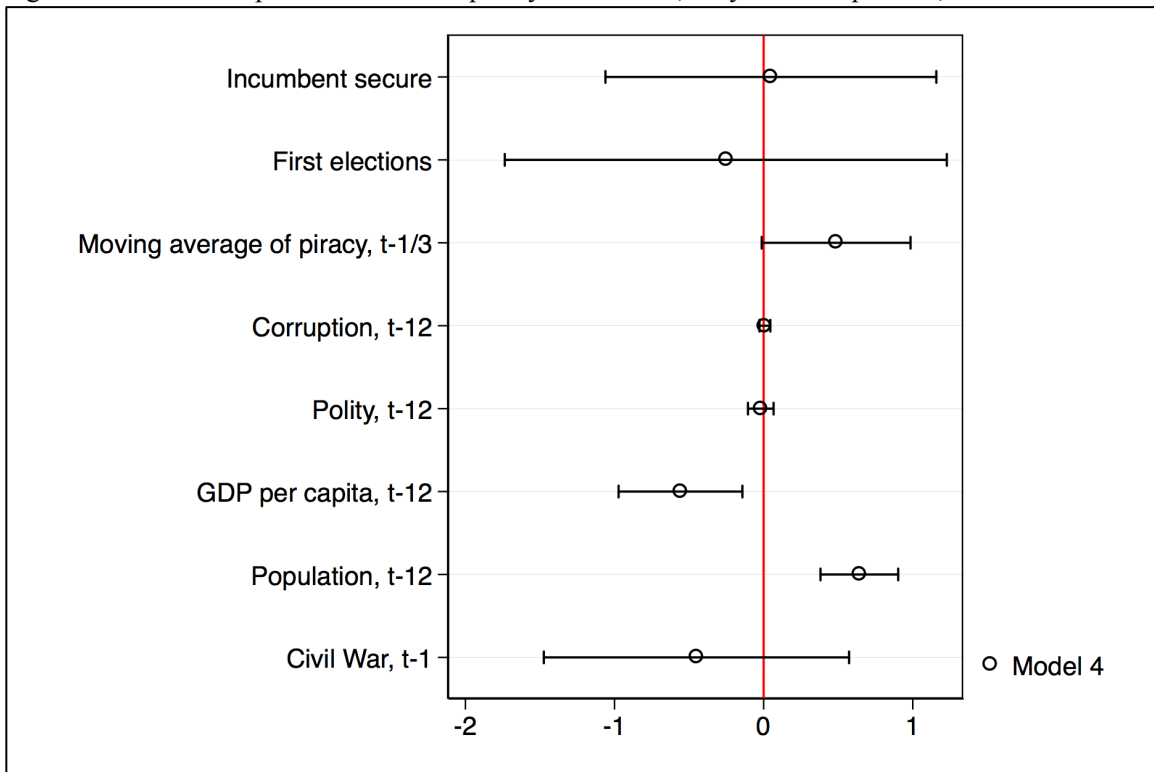


Figure 5: Indonesia EEZ PRIO grids, 1999 election margins, and piracy incidents

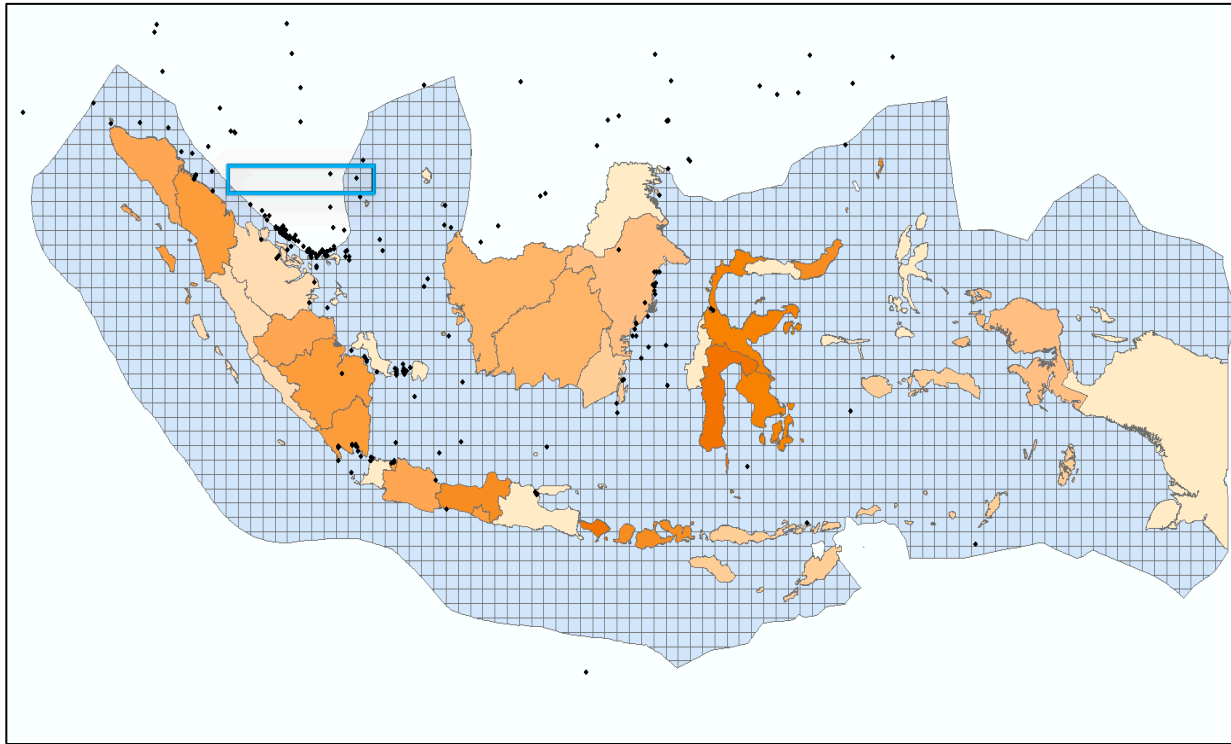


Figure 6: Coefficient plot of 1999 elections and piracy in Indonesia, models 5-8

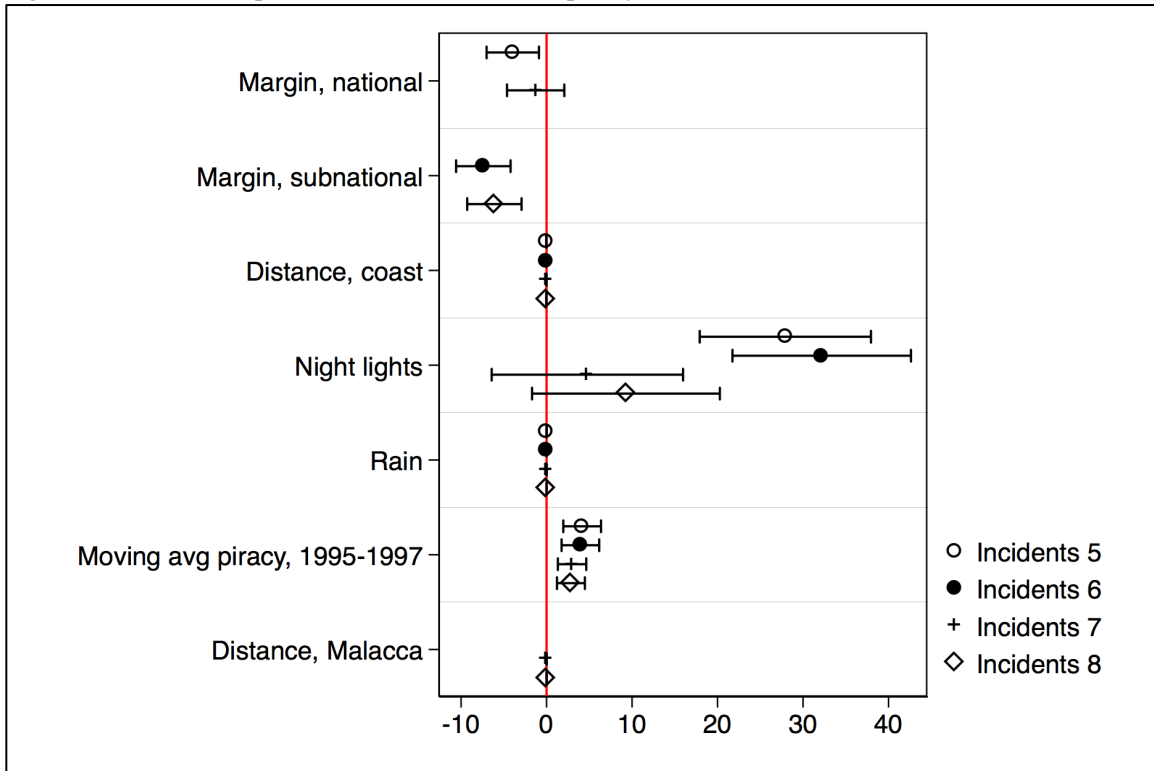


Figure 7: Predicted probabilities plot, margin of victory and distance interaction

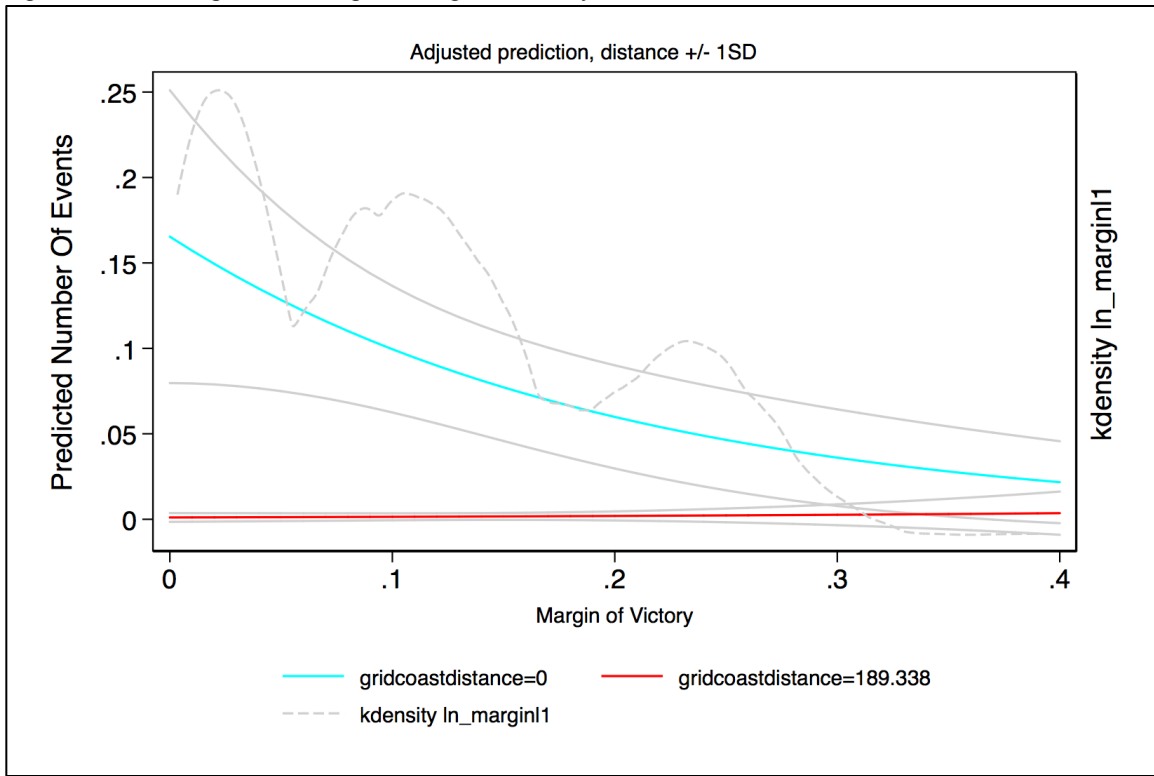
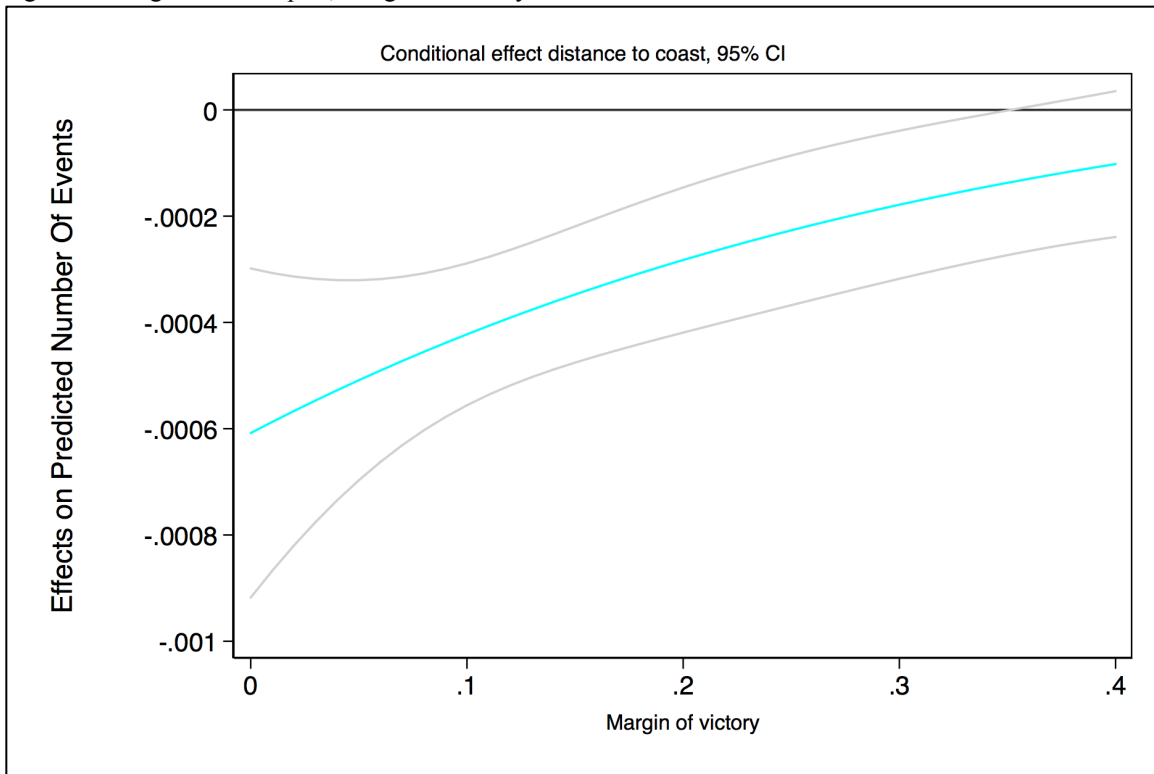


Figure 8: Marginal effects plot, margin of victory and distance interaction



Appendix: Regression Tables

Table 1: Event Count Regression of Maritime Piracy and Elections, 1993-2010

	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>	<i>Model 4 Elections only</i>
Election	0.240+ (0.147)		0.245+ (0.147)	
Election, t-1, t, t+1		0.160+ (0.083)		
Election, t+1			0.099 (0.101)	
Election, t+2			-0.113 (0.172)	
Election, t+3			-0.147 (0.192)	
Election, t+4			-0.234 (0.215)	
Election, t+5			-0.212 (0.178)	
Election, t+6			0.103 (0.121)	
Election, t+7			0.180 (0.136)	
Election, t+8			-0.138 (0.177)	
Election, t+9			-0.136 (0.176)	
Election, t+10			-0.043 (0.213)	
Election, t+11			0.029 (0.166)	
Election, t+12			0.056 (0.162)	
Incumbent confident				0.049 (0.566)
First elections				-0.254 (0.756)
Piracy, t-1/3	0.599** (0.207)	0.599** (0.208)	0.597** (0.207)	0.486 (0.255)
Gvmt effectiveness, t-12	0.004 (0.011)	0.004 (0.011)	0.004 (0.011)	0.008 (0.019)
Polity, t-12	0.016 (0.022)	0.016 (0.022)	0.016 (0.022)	-0.019 (0.044)
GDP per capita, t-12	-0.363** (0.121)	-0.363** (0.120)	-0.360** (0.120)	-0.558** (0.212)
Population, t-12	0.349*** (0.093)	0.348*** (0.093)	0.349*** (0.093)	0.642*** (0.133)
Civil war	-0.266 (0.215)	-0.267 (0.214)	-0.267 (0.216)	-0.451 (0.522)
N	14112	14112	14112	393
AIC	12348.416	12348.056	12364.320	400.796
BIC	12499.512	12499.152	12606.073	484.246

Standard errors in parentheses.

Table 2: Event Count Regression of Maritime Piracy 1998-2000 and 1999 Elections in Indonesia

	Model 5	Model 6	Model 7	Model 8
Margin, national	-3.951* (1.863)		-1.276 (2.038)	
Margin, subnational		-7.396*** (1.937)		-6.103** (1.933)
Distance grid-coast	-0.022*** (0.004)	-0.022*** (0.004)	-0.021*** (0.004)	-0.021*** (0.004)
Night lights	27.930*** (6.089)	32.187*** (6.348)	4.772 (6.805)	9.285 (6.677)
Precipitation	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.002* (0.001)
Piracy 1995-1997	4.162** (1.339)	3.956** (1.336)	2.985** (1.010)	2.848** (0.993)
Distance grid-Malacca			-0.001*** (0.000)	-0.001*** (0.000)
N	2211	2211	2211	2211
AIC	797.207	792.843	758.456	754.065
BIC	837.116	832.752	804.066	799.674

Standard errors in parentheses.

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