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Hit or Miss? The Effect of Assassinations on Institutions and War†

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Assassinations are a persistent feature of the political landscape. Using a new dataset of assassination attempts on all world leaders from 1875 to 2004, we exploit inherent randomness in the success or failure of assassination attempts to identify the effects of assassination. We find that, on average, successful assassinations of autocrats produce sustained moves toward democracy. We also find that assassinations affect the intensity of small-scale conflicts. The results document a contemporary source of institutional change, inform theories of conflict, and show that small sources of randomness can have a pronounced effect on history. (*JEL* D72, N40, O17)

"Assassination has never changed the history of the world."
—Benjamin Disraeli, on the death of Abraham Lincoln[1]

Assassinations of prominent political leaders have occurred throughout history. From Julius Caesar to Abraham Lincoln, from John F. Kennedy to Yitzhak Rabin, many leaders have met violent ends—and many others narrowly escaped assassination. Had Adolph Hitler lingered 13 minutes longer in a Munich beer hall in 1939, he likely would have been killed by a waiting bomb. Whether or not objectionable, or illegal,[2] assassination and assassination attempts are a persistent feature of the political landscape. In fact, as we will show, a national leader has been assassinated in nearly two out of every three years since 1950.

To understand assassination—as an influence in history, as a policy, even as a normative matter—it is important to understand whether assassinations change the course of events. On this topic there is considerable debate, primarily among

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[1] Christopher Mawson (1922).

[2] Moral and legal debates over assassination stretch through history. Dante condemned Brutus for the murder of Caesar, but Cicero and others have been more kind (Robert S. Miola 1985). An ethical basis for "tyrannicide" was promulgated by John of Salisbury in the twelfth century and further articulated by Milton in the late Renaissance (e.g., Carey J. Nederman 1988). In the United States, government-sponsored assassination was not formally outlawed until 1976, and only by executive orders that are the subject of renewed debate.

historians who have focused on individual assassinations or small collections of case studies.[3] In this paper, we assess the impact of assassination using a data-driven approach.[4] Specifically, we focus on the assassination of national leaders and examine its effects on two important outcomes: institutional change and war. The results show substantial effects of assassinations, informing our understanding of assassination and more broadly informing theories of institutional change and conflict.

Analyzing the effects of assassination is difficult. While some assassinations may be associated with historical turning points, the direction of causation is difficult to establish, especially since assassination attempts often occur (as we will show) in times of crisis, such as during war. To overcome this problem, we employ a large set of assassination attempts and use the "failures" as controls for the "successes." To focus on the cases where the success or failure of the attempt was most likely determined by chance, we consider only those attempts in which the weapon was actually used—the gun fired, the bomb exploded, etc. The identification assumption is that, although attempts on leaders' lives may be driven by historical circumstances, conditional on trying to kill a leader, the success or failure of the attempt can be treated as plausibly exogenous. For example, Hitler's early departure from the beer hall in 1939, which may have saved his life, happened only because bad weather prevented him from flying back to Berlin, forcing him to leave early for a train.

To implement this approach, we collected data on all publicly reported assassination attempts for all national leaders since 1875. This produced 298 assassination attempts, of which 59 resulted in the leader's death. We show that, conditional on an attempt taking place, whether the attack succeeds or fails in killing the leader appears uncorrelated with observable economic and political features of the national environment, suggesting that our basic identification strategy may be plausible.

We find that assassinations of autocrats produce substantial changes in the country's institutions, while assassinations of democrats do not. In particular, transitions to democracy, as measured using the Polity IV dataset (Monty G. Marshall and Keith Jaggers 2004), are 13 percentage points more likely following the assassination of an autocrat than following a failed attempt on an autocrat. Similarly, using data on leadership transitions from the Archigos dataset (H. E. Goemans, Kristian Skrede Gleditsch, and Giacomo Chiozza 2006), we find the probability that subsequent leadership transitions occur through institutional means is 19 percentage points higher following the assassination of an autocrat than following the failed assassination of an autocrat. The effects on institutions extend over significant periods, with evidence that the impacts are sustained at least 10 years later.

Looking at military conflict, the results show that assassinations affect conflict, but only in limited contexts. We examine two data sources: the Gleditsch–Correlates

[3] For example, Miles Hudson (2000) discusses a set of assassinations and argues that assassination has little effect, echoing Disraeli's view. However, the murder of Archduke Franz Ferdinand, who was heir to the thrown of the Austro-Hungarian Empire, is often described as the triggering event of World War I. More recently, the murder of President Juvénal Habyarimana may have unleashed the Rwandan genocide, and historians have argued that the Vietnam War was prolonged by the assassination of Kennedy (David Halberstam 1972; Howard Jones 2003).

[4] To the best of our knowledge, the only related paper along these lines is Asaf Zussman and Noam Zussman (2006), who find evidence that assassinations of senior members of Palestinian organizations affect Israeli stock returns.

of War dataset (Meredith R. Sarkees 2000; Gleditsch 2004) and the PRIO/Uppsala Armed Conflict Database (Nils P. Gleditsch et al. 2002). We find that successful assassinations lead to an intensification of small-scale conflicts relative to failed assassination attempts. For high-intensity conflicts, we find somewhat weaker evidence that successful assassinations may have the opposite effect, hastening the end of large-scale conflicts already in progress. These results suggest heterogeneous effects of assassinations that depend on conflict status.

All of these results tell us about the difference in outcomes following success and failure. Our approach does not distinguish whether the effects are driven by successful assassination (e.g., killing an autocrat leads to more democracy), failed assassination (e.g., trying but failing to kill an autocrat leads to increased suppression), or both. To tease these different forces apart, we provide further analysis at the end of the paper that uses propensity-score matching methods to estimate the separate effects of success and failure. While the resulting estimates are informative, they should be viewed as substantially more speculative than our main results, because the decomposition relies on comparisons between years with assassination attempts and years without such attempts, which are not randomly assigned.

Using this methodology, we find that most of the effects discussed above are driven by successful assassinations rather than failures. However, 75 percent of all assassination attempts fail, and there is some evidence that failed attempts have modest effects in the opposite direction of successful assassinations. In particular, failed attempts slightly reduce the likelihood of democratic change and may lead to reductions in existing, small-scale conflict. Since failures are much more likely than successes, the modest effects of failure and the (less likely, but larger) effects of success tend to offset each other. Therefore, from an ex ante perspective, assassination attempts produce instability in political institutions and the path of conflict—with the outcome dependent on success or failure—but, at most, modest directional shifts in democracy or war on average.

The results in this paper not only help understand assassination per se, but also help inform our understanding of institutional change and war more generally. Much of the empirical literature on institutions has explored the deep historical antecedents for modern institutional forms (Barington Moore, Jr. 1966; Douglass C. North 1990; Kenneth L. Sokoloff and Stanley L. Engerman 2000; Daron Acemoglu, Simon Johnson, and James A. Robinson 2001; Edward L. Glaeser and Andrei Shleifer 2002). Meanwhile, "modernization theory," which attempts to explain democratization through increased education or income of the nation at large (e.g., Seymour Martin Lipset 1959, Samuel P. Huntington 1991, Robert J. Barro 1999), is the subject of substantial debate (Acemoglu et al. 2008). Thus, contemporary sources of democracy remain largely within the error term of recent econometric studies, so that the important question of how countries democratize remains, to an extent, unclear. In this paper, we identify a source of contemporary change in political institutions that complements the existing literature and steps beyond the confines of distant history.

The results here also emphasize the interplay between institutions and the role of individual leaders. In particular, the primary results for institutional change are found only in autocracies. This finding is natural if autocrats are relatively unconstrained, with significant authority to alter formal institutions and policies,

as opposed to leaders in democracies whose actions may be limited through electoral recall and institutions such as independent legislatures and judiciaries (Joseph A. Schumpeter 1950; Anthony Downs 1957; George Tsebelis 2002; Benjamin F. Jones and Benjamin A. Olken 2005). Our results point to the individual autocrat as a cornerstone of institutions, which suggests mechanisms (through leader selection and leader change) that can lead to institutional change.

This paper also speaks to the literature on war. Many formal models emphasize bargaining breakdowns due to information asymmetries or commitment problems between nations, with little attention to the agency of leaders, while other models emphasize the divergence between the leader's incentives and those of the population at large (see, e.g., James D. Fearon 1995; Goemans 2000; Matthew O. Jackson and Massimo Morelli 2007; Sandeep Baliga, David O. Lucca, and Tomas Sjöström 2007). From this latter point of view, assassinations, by changing leaders, may naturally produce changes in conflict status. Our research provides support for this theoretical approach, which emphasizes the role of leaders in determining the escalation and cessation of conflict.

Finally, this paper speaks to the role of chance in history. We provide a statistically driven test of the capacity for small elements of luck to change national political systems and other outcomes, an idea seen in some broad historical assessments (John Merriman 1985, Daniel J. Boorstin 1995, Niall Ferguson 1999) that stand in contrast to Whiggish or Marxist historical interpretation. In this sense, this paper shares some similarities with literatures that emphasize historical chance in the initial shaping of institutions, whether it is the disease environment (Acemoglu, Johnson, and Robinson 2001), wind patterns (James Feyrer and Bruce Sacerdote 2009), or other features.

The remainder of the paper is organized as follows. Section I presents the data and descriptive statistics. Section II describes the "hit or miss" methodology, presents the central results regarding institutional change and military conflict, and considers a number of robustness checks. Section III presents the propensity score results to separate out the effect of success from the effect of failure. Section IV considers implications of the results for theories of institutional change and conflict. Section V concludes.

I. Data and Descriptive Statistics

A. *Data*

This paper focuses on assassinations of, and assassination attempts on, the national leader, where the "leader" is defined as the most powerful political figure in each country at each point in time: the head of state (usually the President), the head of government (usually the Prime Minister), or perhaps some third figure. To establish a baseline list of leaders, we use the Archigos dataset, version 2.5 (Goemans et al. 2006), that identifies the primary leader for each country at each point in time from 1875 to 2004. Archigos provides a dataset of 2,440 leaders from 187 different countries.

To collect the assassinations data, we consulted the archives of three major newspapers: the *New York Times*, *Washington Post*, and *Wall Street Journal*. We

used a large set of keyword searches (detailed in the Appendix) and placed several limitations on the returned results. First, we excluded coup d'états, cases in which the murder or attempted murder of the leader was conducted by an individual or group in an attempt to seize power for themselves. Second, we excluded "uncovered plots" to assassinate leaders, limiting ourselves to cases in which the would-be assassins undertook the attempt. For the main specifications in the paper, we further restrict our attention to "serious attempts," which we define as those cases in which the weapon (the gun, bomb, etc.) was discharged, as opposed to cases where the attempt was thwarted prior to the weapon being used. As shown below, our results are broadly robust to different restrictions on the nature of failed attempts.

For each assassination or attempted assassination found, we recorded the date and location of the attack, the weapons used, and the result for the leader, as well as information, when available, on other casualties and whether the attack was carried out by a group or solo attacker.[5] The data includes 298 assassination attempts, of which 251 are "serious attempts" and 59 resulted in the leader's death. A list of the successful assassinations is presented in Table 1.

To ensure that the data collection methodology captured all relevant assassinations, once the newspaper searches were complete, we cross referenced the assassinations found by the searches with all assassinations listed in John V. da Graca (2000), Jones and Olken (2005), and the Archigos data. This exercise showed that our keyword searches produced all relevant assassinations.[6, 7]

To investigate the effect of assassination on political institutions, we consider two measures of institutions.[8] The first measure is a dummy variable for political institutions, where one indicates democracy and zero indicates autocracy. This variable is a binary version of the POLITY2 variable from the Polity IV dataset.[9] The second measure, which is derived independently from the Archigos dataset, records the percentage of leader transitions over the following 20 years (excluding the leader

[5] While this measure serves as a proxy for the number of people involved in the attempt, it is imperfect, as even solo assassins may be supported by other actors who remain unseen.

[6] It is more difficult to assess conclusively our effectiveness in capturing assassination attempts. There are, however, several reasons to believe that our method was effective. First, we ran the keyword searches sequentially. The *New York Times* produced 263. The *Washington Post* produced an additional 33 attempts. And the *Wall Street Journal* produced only two additional attempts. The rapidly diminishing returns to further searches suggest that we are accurately capturing publicly-known assassination attempts. Second, as we will show, the number of attempts produced by these searches turns out to track the number of successful assassinations through time. Third, we focus our results on "serious attempts," where the attack was carried out. These attempts are more likely to be reported and thus harder to miss.

[7] Goemans (personal correspondence) notes that two cases, Zia in Pakistan and Boris III in Bulgaria, could be construed as natural deaths whereas our searching algorithm classified them as assassinations. We have verified that our results are not meaningfully changed by dropping these two observations.

[8] We limit our analysis to institutional changes and conflict primarily for reasons of data availability. There are few reliable annual time series on policy variables that cover the entire range of our data, from 1875–present.

[9] Specifically, we define autocracy as cases where the POLITY2 variable is less than or equal to zero and democracy as cases where the POLITY2 variable is greater than zero. The POLITY2 variable has 21 categories, ranging from −10 (most autocratic) to +10 (most democratic), but the meaning of finer distinctions in this index is less clear, especially since the POLITY2 index is a nonlinear summation of sub-indices intended to capture aspects of regime type. For this reason, we focus on the clearer binary distinction, where autocracy is defined as any POLITY2 score less than zero, for our main results. We discuss alternate transformations of the POLITY2 variable in Section IID.

TABLE 1—ASSASSINATIONS OF PRIMARY NATIONAL LEADERS SINCE 1875

Country of leader	Year of assassination	Name of leader	Weapon used
Afghanistan	1919	Habibullah	gun
Afghanistan	1933	Nadir Shah	gun
Algeria	1992	Boudiaf	gun
Austria	1934	Dollfuss	gun
Bulgaria	1943	Boris III	gun
Burundi	1994	Ntaryamira	other
Congo (Brazzaville)	1977	Ngouabi	gun
Congo (Kinshasa)	2001	Kabila	gun
Dominican Republic	1899	Heureaux	gun
Dominican Republic	1911	Caceres	gun
Dominican Republic	1961	Trujillo	gun
Ecuador	1875	Moreno	other
Egypt	1981	Sadat	gun
Greece	1913	George I	gun
Guatemala	1898	Reina Barrios	unknown
Guatemala	1957	Castillo Armas	gun
Haiti	1912	Leconte	explosive device
India	1984	Indira Gandhi	gun
Iran	1896	Nasir Ad-Din	gun
Ireland	1922	Collins	gun
Israel	1995	Rabin	gun
Japan	1921	Hara	knife
Japan	1932	Inukai	gun
Jordan	1951	Abdullah	gun
Korea	1979	Park	gun
Lebanon	1989	Moawad	explosive device
Madagascar	1975	Ratsimandrava	unknown
Mexico	1920	Carranza	unknown
Nepal	2001	Birendra	gun
Nicaragua	1956	Somoza	gun
Pakistan	1951	Khan	gun
Pakistan	1988	Zia	other
Panama	1955	Remon	gun
Paraguay	1877	Gill	unknown
Peru	1933	Sanchez Cerro	gun
Poland	1922	Narutowicz	gun
Portugal	1908	Carlos I	gun
Portugal	1918	Paes	gun
Russia	1881	Alexander II	explosive device
Rwanda	1994	Habyarimana	other
Salvador	1913	Araujo	gun
Saudi Arabia	1975	Faisal	gun
Somalia	1969	Shermarke	gun
South Africa	1966	Verwoerd	knife
Spain	1897	Canovas	gun
Spain	1912	Canalejas	gun
Spain	1921	Dato	gun
Sri Lanka	1959	Bandaranaike	gun
Sri Lanka	1993	Premadasa	explosive device
Sweden	1986	Palme	gun
Togo	1963	Olympio	gun
United States	1881	Garfield	gun
United States	1901	McKinley	gun
United States	1963	Kennedy	gun
Uruguay	1897	Idiarte Borda	gun
Venezuela	1950	Delgado	gun
North Yemen	1977	Al-Hamdi	gun
North Yemen	1978	Al-Ghashmi	explosive device
Yugoslavia	1934	Alexander	gun

in power at the time of the attempt) that are "regular" (i.e., proceed lawfully) as opposed to irregular transitions such as coups.[10]

To investigate the effect of assassinations on war, we use two datasets on conflict: the Kristian S. Gleditsch (2004) revision of the Correlates of War dataset (Meredith R. Sarkees 2000) and the PRIO/Uppsala Armed Conflict dataset, version 4 (Nils P. Gleditsch et al. 2002, PRIO 2006). The Gleditsch–COW dataset contains data on all armed conflicts with over 1,000 battle deaths from 1816–2002, and to the best of our knowledge is the only dataset with worldwide coverage on conflicts for the entire time period we consider. The data indicate whether a country is at war in a given year and the type of any such war (civil, interstate, etc.). The PRIO dataset contains more information. It contains data on all armed conflicts with over 25 battle deaths per year and further describes conflict intensity, indicating whether a conflict had 25–999 battle deaths or 1,000 plus battle deaths in a given year.[11] The coverage of the PRIO dataset, however, only begins in 1946, which is why we examine both datasets.[12]

B. *Summary Statistics*

Table 2 provides summary statistics about assassination attempts. With regard to weapons, guns have been the most common instrument, used in 55 percent of attempts, and explosive devices the second most common, used in 31 percent of attempts. Guns have kill rates of about 30 percent, while explosive devices are much less likely to kill the leader, with success in only 7 percent of cases where the device was actually engaged. At the same time, explosive devices produce the greatest number of casualties among bystanders, with the mean number of dead and wounded six and eight times larger than for gun attacks. Explosive devices thus appear to be a particularly violent and particularly ineffective tool.[13]

Table 2 also shows that the vast majority of assassination attempts occur in the leader's home country, with only 4 percent occurring outside the national borders. Attempts are slightly more likely to be carried out by solo attackers than by groups of attackers (59 percent versus 41 percent) although, as discussed previously, solo attackers may have behind the scenes support we do not observe. Both solo and group attacks show a similar propensity to kill the leader, although group attacks tend to be far bloodier for bystanders.

Figure 1 shows how the frequency of assassination events has evolved over time, plotting the frequency of attempts and successful assassinations in each decade. Panel A indicates that the annual rate of assassinations increased in the late nineteenth and

[10] Archigos defines a regular leader transition as one that occurs "according to the prevailing rules, provisions, conventions, and norms of the country" (Goemans et al. forthcoming). Following Archigos, we exclude cases in which leader transitions occurred following deaths in office due to natural causes or accidents, though including them as either "regular" or "irregular" does not substantively change the results in Section II below.

[11] We define the PRIO variable to be 0.5 if a small conflict is taking place, 1 if a large conflict is taking place, and 0 otherwise.

[12] Although, in theory, Gleditsch–COW and PRIO should agree on conflicts with over 1,000 battle deaths, Gleditsch (2004) notes that they do not. Although he makes some changes to the COW data to clarify the coding, the two datasets are still not identical.

[13] Yet we also find (in results not reported) that explosive devices are used with increasing regularity through time. This may reflect the fact that bombs can be triggered remotely so that, although less effective as a weapon, bombs put the assassin(s) at lower risk of being caught.

TABLE 2—ASSASSINATION ATTEMPTS: SUMMARY STATISTICS

	Observations	Percentage	Probability leader killed		Bystander casualties	
			All attempts	Serious attempts	Mean killed	Mean wounded
Type of weapon						
Gun	161	55%	28%	31%	1.0	2.2
Explosive device	91	31%	5%	7%	5.8	18.2
Knife	23	8%	13%	21%	0.3	0.4
Other	19	6%	16%	18%	1.1	0.3
Unknown	10	3%	40%	44%	2.0	1.3
Location						
Abroad	12	4%	25%	30%	3.6	6.5
At home	286	96%	20%	23%	2.4	6.7
Number of attackers						
Solo	132	59%	24%	29%	0.4	2.5
Group	92	41%	22%	26%	5.6	11.0
Total attempts	298	n/a	20%	24%	2.4	6.7

Notes: There are 298 total assassination attempts observed and 251 serious attempts. Serious attempts are defined as cases where the weapon was actually used. Note that the location of the attack is observed in every case, but the type of weapon is observed in 288 cases and the number of attackers observed in 224 cases. For some attempts, multiple types of weapons were used, so that the weapon observation counts sum to 304. Attacks with weapons classified as "other" include arson, rocket attacks, stoning, and automobile crashes, among others. Also note that casualties among bystanders are skewed distributions so that the means are much larger than medians.

early twentieth centuries, decreased substantially during the 1940s (perhaps as a result of heightened security during World War II), and has been at relatively high levels since 1950. Currently, the world witnesses the assassination of a national leader in one out of every two years. Interestingly, the frequencies of attempts and successes closely track one another. In fact, the conditional probability of killing a leader given a serious attempt is not trending, remaining at about 25 percent through time.

Panel B of Figure 1 presents these frequency patterns again, but normalizes by the number of countries (and hence the number of national leaders) that exist in a given year. The rate of attempts and successes now appear to fall after 1930, an effect driven by the increasing number of independent countries in the world. This means that, although the annual rate of assassinations is currently at historically high levels, the probability that a given leader is killed in any given year has fallen during the twentieth century. At the peak, in the 1910s, a given leader had a nearly 1 percent chance of being assassinated in a given year. Today, the probability is below 0.3 percent.

Finally, Table 3 presents summary statistics for the key dependent variables we examine in this paper. Panel A presents the transitions probabilities between autocratic and democratic states, using the dichotomous version of the POLITY2 variable. We consider a two-year interval when calculating transition rates (i.e., comparing the regime in year $t + 1$ with year $t - 1$) because our main analysis of assassinations will consider changes comparing one year after the attempt to one year before the attempt. Regime shifts are seen to be reasonably rare historically, with

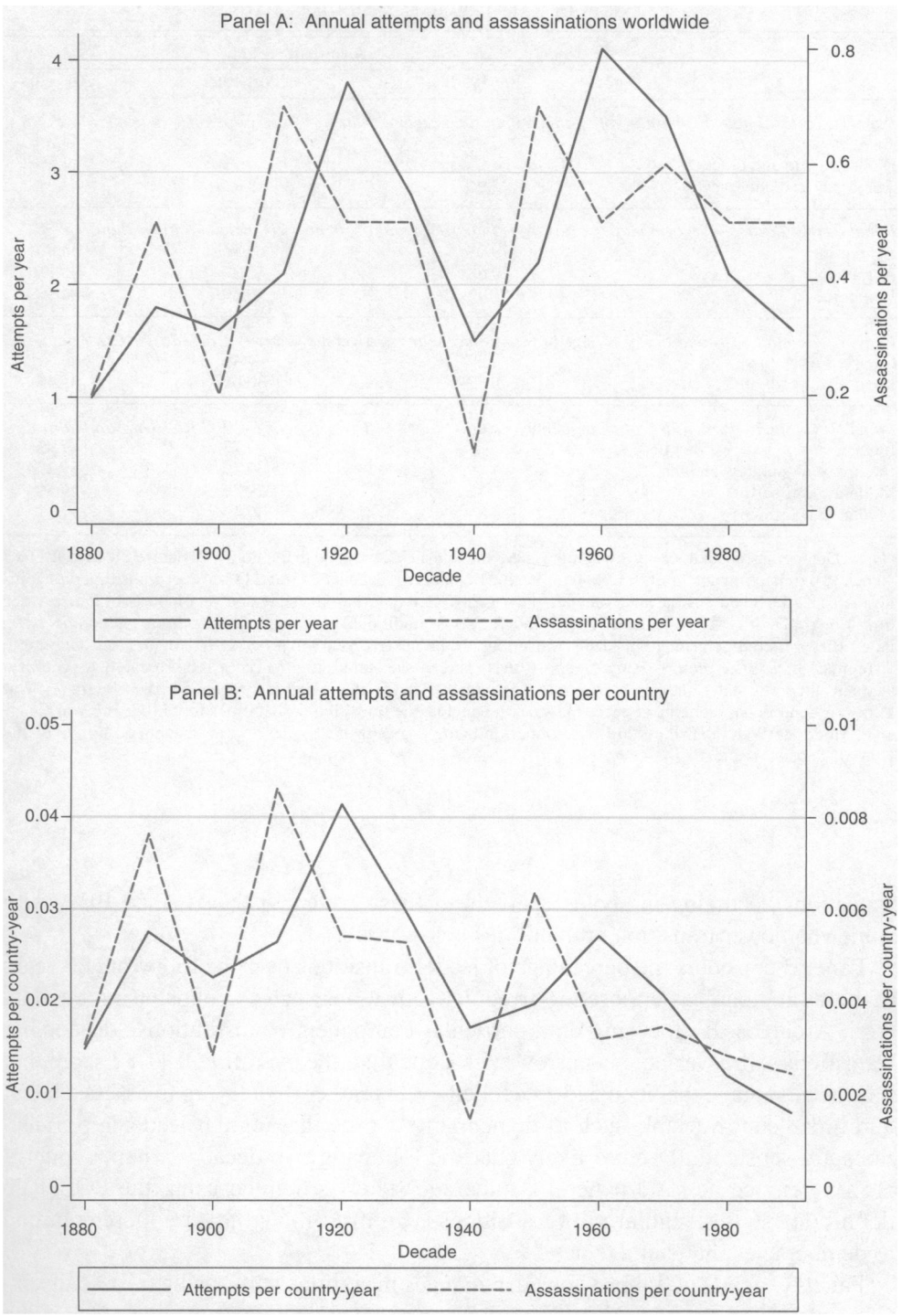

FIGURE 1. TRENDS IN THE FREQUENCY OF ASSASSINATIONS AND ASSASSINATION ATTEMPTS

Notes: Data binned by decade, with averages presented at initial year. For example, 1900 represents the average over the years 1900–1909.

TABLE 3—KEY DEPENDENT VARIABLES: SUMMARY STATISTICS

	Historical period		
	Pre 1950	Post 1950	All years
Panel A: Institutions—Probability of change in political regime, year t + 1 versus year t − 1 (Polity IV data)			
Any change	3.4%	4.9%	4.3%
Democracy to autocracy	4.0%	4.7%	4.5%
Autocracy to democracy	2.9%	5.0%	4.2%
Panel B: Institutions—Percent leader transitions that are "regular" in next 20 years (Archigos data)			
All regimes	70.2%	67.3%	68.8%
Autocracy	59.6%	51.0%	55.1%
Democracy	81.9%	86.6%	84.1%
Panel C: Conflict—Probability of change in war status, year t + 1 versus year t − 1 (Gleditsch-COW data)			
Intense war begins	7.2%	4.4%	5.5%
Intense war ends	44.1%	30.0%	35.4%
Panel D: Conflict—Probability of change in war status, year t + 1 versus year t − 1 (PRIO/Uppsala data)			
Intense or moderate war begins	—	7.7%	7.9%
Intense or moderate war ends	—	37.4%	37.3%
Moderate war ends	—	29.2%	29.5%
Moderate war intensifies	—	17.4%	17.3%

Notes: Democracy and autocracy are defined using the POLITY2 variable in the Polity IV dataset, with POLITY2 values ≤ 0 indicating autocracy and > 0 indicating democracy. Panels A, C, and D consider transition probabilities. A transition occurs if the state one year later is different that than the state one year before. In Panel A, the transition probability "to Autocracy" ("to Democracy") is conditional on being in a democratic (autocratic) state. Panel B reports the percentage of leader transitions in the next 20 years that occur by "regular" as opposed to "irregular" means (i.e., coups). Panel B reports these percentages for all regime types and separately for country-years in autocratic states and democratic states. In panels C and D, the transition probability "War Begins" ("War Ends") is conditional on being at peace (at war). In panel D, the transition probabilities for "Moderate War Ends" and "Moderate War Intensifies" are conditional on being engaged in a moderate war, as defined by the PRIO/Uppsala dataset (see text).

transitions occurring in about 5 percent of these country periods since 1950, with somewhat lower transition probabilities before 1950.

Panel B considers the percentage of leader transitions over the following 20 years that are "regular," as opposed to irregular transitions such as coups, using the data from Archigos. By focusing on a particular component of institutions—leadership transitions—this variable is narrower in scope than the overall POLITY2 score, but has the advantage that it considers explicit events rather than trying to assess a latent and broad state variable such as democracy. We see that regular leadership transitions are substantially more likely over the following two decades when a country is in a democratic state (where "democratic state" is defined using the POLITY2 definition), so that regularity of transitions is an institutional feature more common to democracies than autocracies.

Panels C and D of Table 3 consider changes in conflict status using the Gleditsch–COW data and the PRIO/Uppsala data, respectively. Comparing one year in the future to one year in the past, we see that intense conflicts (those with at least 1,000 battle deaths) begin in 4.4 percent of cases since 1950 using the Gleditsch–COW data. In the PRIO/Uppsala data, which includes additional, moderate-level wars (those with 25–999 battle deaths) that the Gleditsch–COW data does not, conflicts

begin in 7.7 percent of cases. For conflicts in progress, war ends with a probability of 30–44 percent, depending on intensity of the conflict and time period. Using the Gleditsch–COW data, war status appears more volatile, with starts and stops occurring more frequently, before 1950.

II. Hit or Miss: Identifying the Effect of Assassination

A. *Empirical Approach*

In this section, we investigate the causative effect of assassination. To identify this effect we employ the inherent randomness in whether an attack is successful or not. For example, Kennedy did not escape the bullet that killed him, even though it was fired from 265 feet away and the president was in a moving car (Earl Warren et al. 1964). But Idi Amin, President of Uganda from 1971–1979, did survive an attack in 1976, when a thrown grenade bounced off of his chest and killed several bystanders.

In our main specifications, we examine OLS regressions of the form

$$y_i = \beta SUCCESS_i + \boldsymbol{\gamma}\mathbf{X}_i + \varepsilon_i, \tag{1}$$

where i indexes a country-year in which there is an assassination attempt, y_i is an outcome of interest (institutional change or change in war status), $SUCCESS_i$ is a dummy equal to one if a leader is killed in that country and year and zero if the leader survives any attempts, and $\mathbf{X}_i$ is a vector of other regressors. The key identifying assumption is that we can treat *SUCCESS* as exogenous conditional on observables. Then $E[\varepsilon \mid SUCCESS, \mathbf{X}] = 0$, and we can write the average treatment effect as

$$\beta = E[y \mid SUCCESS = 1, \mathbf{X}] - E[y \mid SUCCESS = 0, \mathbf{X}]. \tag{2}$$

This expression makes clear that estimates of (1) identify the difference between successful assassinations and failed assassination attempts. Thus, we answer the question: what is the effect of killing versus failing to kill the leader? If hypothesis tests reject that β is zero, then the outcome of the attempt matters, and more broadly, we can reject the idea that assassinations do not change the course of events. Note, however, that we cannot tell whether the effect of assassinations we identify comes from the effect of killing the leader, failing to kill the leader, or both. In Section III, we use propensity-score matching methods to tease out whether β is driven primarily by successful or failed assassinations, but since assassination attempts are nonrandom, that analysis is necessarily more speculative than the analysis presented here.[14] Therefore, we focus first on the better-identified question of whether national outcomes differ depending on the success or failure of assassination attempts.

[14] As a preview, we find that it is primarily the killing of leaders that appears to drive change, rather than failure.

B. *Is Success Exogenous Conditional on Attempts?*

The key identification assumption for the main analysis is that, conditional on a serious attempt taking place, the success of the attempt (i.e., where the bullet hits, where the target is standing when the bomb explodes, etc.) is uncorrelated with the error term in (1). To investigate this assumption, we first ask whether observable variables that might be related to the error term in (1) predict *SUCCESS* conditional on attempt.[15]

As previously discussed, one variable that we know predicts success is the type of weapon used in the attack. In particular, attempts that use explosive devices are much less likely to result in a leader's death than attempts that use other weapons. For this reason, all specifications in the analysis below will include weapon fixed effects, although it turns out that the inclusion or exclusion of weapon fixed effects does not affect the results.

To investigate whether other variables predict successful assassinations, we present in panel A of Table 4 the mean values of a number of variables in the year prior to successful and failed assassination attempts, as well as the result from two-sided *t*-tests for the equality of these means. The table shows that the sample of successful and failed assassination attempts is balanced across a wide variety of variables: a dummy for whether the country was democratic or not (defined using the POLITY2 variable from Polity IV) and recent changes therein, the status of war and recent changes therein (from the Gleditsch–COW war data), the age of the leader, the tenure of the leader, and log per-capita energy consumption, which serves as a proxy measure for per capita income.[16, 17] The only result in Table 4 where the difference between successes and failures is statistically significant is the log of national population (p-value 0.05). Given that we have examined eight variables, however, it is natural that one be statistically significant at this level.

In panel B of table 4, we present the results from probit specifications that consider all of these variables simultaneously. Specifically, we estimate the following equation:

$$P(SUCCESS_a) = \Phi(\gamma_1 + \boldsymbol{\gamma}_2 \mathbf{X}_a), \tag{3}$$

where a is a serious assassination attempt, and $\mathbf{X}$ represent the same variables considered in panel A. We present specifications with and without weapon fixed effects, and also with and without fixed effects for the region of the world where the attack takes place. When considering all of the variables in Table 4 jointly, their joint

[15] Of course, the limitation of this type of analysis is that unobservable factors that predict success and predict changes in institutions or conflict status cannot be assessed. For example, attacker effort might predict success and be associated with underlying imminent changes.

[16] Recent changes in political institutions and war status compare values in the year prior to the attempt to values three years prior to the attempt. These are lagged versions of the dependent variables used below, which compare institutional or conflict status one year after the attempt with status one year before.

[17] The energy consumption measure comes from the Correlates of War National Material Capabilities dataset version 3.02 (J. David Singer et al. 1972, 1987). We use such a proxy measure because data on per capita income is not available for the world sample prior to 1950.

TABLE 4—ARE SUCCESSFUL AND FAILED ATTEMPTS SIMILAR?

Variable	Success	Failure	Difference	p-val on difference
Panel A: Pairwise t-tests of sample balance				
Democracy dummy	0.362	0.344	0.018	0.80
	(0.064)	(0.035)	(0.072)	
Change in democracy dummy	−0.036	−0.022	−0.013	0.67
	(0.025)	(0.019)	(0.032)	
War dummy	0.263	0.318	−0.055	0.42
	(0.059)	(0.034)	(0.068)	
Change in war	0.036	0.011	0.025	0.71
	(0.058)	(0.034)	(0.067)	
Log energy use per capita	−1.589	−1.740	0.152	0.69
	(0.338)	(0.180)	(0.383)	
Log population	9.034	9.526	−0.492	0.05*
	(0.219)	(0.117)	(0.248)	
Age of leader	55.172	52.777	2.395	0.14
	(1.351)	(0.866)	(1.604)	
Tenure of leader	9.328	7.619	1.709	0.27
	(1.440)	(0.544)	(1.539)	
Observations	59	194		
	(1)	(2)	(3)	(4)
Panel B: Multivariate regressions				
Democracy dummy	0.068	0.063	0.071	0.070
	(0.068)	(0.066)	(0.070)	(0.067)
Change in democracy dummy	−0.039	−0.050	−0.033	−0.036
	(0.100)	(0.103)	(0.104)	(0.109)
War dummy	0.057	0.063	0.061	0.067
	(0.069)	(0.065)	(0.070)	(0.065)
Change in war	−0.024	−0.017	−0.025	−0.013
	(0.077)	(0.083)	(0.076)	(0.083)
Log energy use per capita	0.002	0.001	0.008	0.009
	(0.014)	(0.014)	(0.015)	(0.015)
Log population	−0.027	−0.025	−0.028	−0.032
	(0.021)	(0.021)	(0.021)	(0.020)
Age of leader	0.003	0.003	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Tenure of leader	0.004	0.004	0.005	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Weapon FE	N	Y	N	Y
Region FE	N	N	Y	Y
Observations	208	208	208	208
p-value of F-test on all listed variables	0.46	0.49	0.46	0.40
p-value of F-test on all listed variables and fixed effects	0.46	0.06*	0.59	0.01***

Notes: Panel A reports the means of each listed variable for successes and failures, where each observation is a serious attempt. Standard errors are in parentheses. p-values on differences in the mean are from two-sided unpaired t-tests. All variables are examined in the year before the attempt took place. Change variables represent the change from three years before the attempt occurred to one year before the attempt occurred. Panel B reports marginal effects from a probit regression, where each observation is a serious attempt and the dependent variable equals one for successful assassinations. Robust standard errors are in parentheses, adjusted for clustering by country. Weapon FE refers to dummies for each weapon type (gun, knife, explosive, poison, other, unknown), and region FE refers to dummies for each region of the world (Africa, Asia, Middle East/North Africa, Latin America, Eastern Europe, Western Europe/OECD).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

p-value ranges from 0.40 to 0.49, depending on which fixed effects are included.[18] In the robustness analysis (see Section IID), we show that the inclusion or exclusion of all of these variables as controls has little effect on the results. Combined, the relative lack of predictability of *SUCCESS*, and the invariance of the results to adding controls for *SUCCESS*, suggests that the identification assumption is plausible.

C. *Main Results*

In this section, we present our main results. To test hypotheses, we consider parametric and nonparametric specifications. First, we estimate (1) using OLS with robust standard errors, adjusted for clustering at the country level. Adjusting for clustering at the country level helps account for potential serial correlation of the error term in the event that there are multiple attempts in the same country. In the OLS specifications, we include fixed effects for the weapon used to take into account the differential success probabilities of different weapons, as discussed above. We also include fixed effects for the number of attempts in a given country-year. We do this because, even if the success or failure of a given attempt is exogenous, as the evidence above suggests, the likelihood of success on an annual basis is increasing in the number of attempts, so that the probability of success in a given year is only exogenous if we condition on the number of attempts that took place.[19]

Second, we report the results of nonparametric tests. For cases where the dependent variable takes a small number of potential outcomes, we report the results of the Fisher exact test (Ronald A. Fisher 1935; Marcello Pagano and Katherine T. Halvorsen 1981), which has exact small sample properties. This test takes the marginal distribution of each variable as given and calculates the probability that the actual association found, or a tighter association, could be produced by chance. This test is exact because it calculates the exact probability of each permutation of the variables, which is a finite set.[20] For variables that take a large number of values, we calculate nonparametric p-values from the Frank Wilcoxon (1945) rank-sum test. In this test, the outcomes from successful and failed assassinations are pooled and jointly ranked. The test statistic is the sum of the ranks for the successes. Wilcoxon shows that the sum of the ranks is normally distributed, and gives formulas for the mean and variance of the sum of the ranks under the null hypothesis that the two samples are drawn from identical distributions.

Political Institutions.—Table 5 presents the main results for the effects of assassination on political institutions. In column 1, we examine whether there are changes in

[18] If we use the linear POLITY2 variable instead of the democracy dummy, the joint p-values range from 0.09 to 0.31, with the linear variable significant in some specifications. The results in the paper are also robust to including this linear POLITY2 variable as a control instead of the dummy version.

[19] In any case, the inclusion or exclusion of weapon fixed effects or number-of-attempt fixed effects has no material effect on the results.

[20] For example, if success occurs in 59 of 251 cases and an outcome variable changes in 25 of 251 cases, one can calculate the probability for each possible permutation of these two variables in a 2×2 matrix (e.g., the probability that 22 of the successes correspond to 12 of the outcome changes). By considering every possible permutation of success and the outcome, one can calculate the cumulative probability that the actual association witnessed, or some even tighter association, was produced purely by chance. This is the reported p-value.

TABLE 5—ASSASSINATIONS AND INSTITUTIONAL CHANGE

	Absolute change in POLITY2 dummy (1)	Directional change in POLITY2 dummy (2)	Percentage of "regular" leader transitions in next 20 years (3)
Panel A: Average effects			
Success	0.091	0.079	0.111
	(0.047)	(0.051)	(0.057)
Parm. p-value	0.06*	0.12	0.06*
Nonparm. p-value	0.03**	0.02**	0.18
Observations	221	221	138
Data source	Polity IV	Polity IV	Archigos
Panel B: Split by regime type in year before attempt			
Success × autocracy		0.131	0.191
		(0.055)	(0.085)
Success × democracy		−0.012	0.034
		(0.083)	(0.043)
Autocracy—parm. p		0.02**	0.03**
Autocracy—nonparm. p		0.01***	0.05**
Democracy—parm. p		0.89	0.43
Democracy—nonparm. p		0.13	0.96
Observations		221	133
Data source	Polity IV	Polity IV	Archigos

Notes: Results from estimating equation (1). Success is a dummy for whether the assassination attempt succeeded. The dependent variable in column 1 is a dummy for whether there was a change from autocracy to democracy or vice versa (change = 1, no change = 0). The dependent variable in column 2 indicates the direction of any change (change to democracy = 1, no change = 0, change to autocracy = −1). The dependent variable in column 3 is the percentage of future leader transitions that are "regular" as opposed to "irregular" (i.e., coups). This measure excludes the transition of the leader in power during the attempt. The sample in all columns is limited to serious attempts. Standard errors and parametric p-values are computed using robust standard errors, adjusted for clustering at the country level; these specifications all include dummies for weapon type and the number of attempts in that year. Nonparametric p-values are computed using Fisher's exact (1935) p-values in columns 1 and 2 and using a Wilcoxon (1945) rank-sum test in column 3. In panel B, autocracy/democracy is defined by the POLITY2 dummy in the year before the attempt. The main effect for the lagged autocracy variable is also included in the panel B regressions. Absolute change in POLITY2 dummy is not shown in panel B as it is mechanically identical to the directional change in POLITY2 dummy once we split by lagged POLITY2 dummy status.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

institutions (in one direction or another), comparing changes in the binary POLITY2 variable from the year before the assassination attempt to the year after. The dependent variable in column 1 takes a value of one if the regime switched democracy/autocracy status and zero otherwise. We see that changes between regimes are 9 percentage points more likely when the leader is killed than when the leader survives the attack. These results are statistically significant using both the parametric and nonparametric hypothesis tests. In column 2, we consider whether assassinations lead systematically in the direction of democracy or autocracy. Here, the dependent variable takes the value 1 if a regime switched from autocracy to democracy, −1 if the regime switched from democracy to autocracy, and 0 if no change occurred. The results show that, on average, successful assassinations lead to democracy. This result is not quite statistically significant with the parametric test and significant with

a p-value of 0.02 with the nonparametric test. Lastly, using the alternative, Archigos-based measure characterizing future leader transitions, column 3 shows that successful assassinations raise the probability that future leader transitions occur lawfully by 11 percentage points.[21] While analysis of leader transitions captures a somewhat different phenomenon than the Polity measure, this independent source helps validate the broader structural changes that the Polity IV data describe.[22,23]

Panel B of Table 5 presents the effect of assassination conditional on the initial nature of the regime. Importantly, we find that the effects are limited to autocracies. The successful assassination of an autocrat creates a highly significant 13 percentage point increase in the probability of democratic transition, compared to the case where the assassination attempt failed. Meanwhile, the successful assassination of democrats produces no change in institutions using the Polity IV measure. Democratic institutions thus appear robust to the assassination of leaders, while autocratic regimes do not. Similar results are obtained using the percentage of regular future leadership transitions from Archigos as the criterion—successful assassination of autocrats creates a 19 percentage point increase in the probability that future leadership transitions occur by regular means, whereas there is no change in the probability that future leadership transitions occur by regular means following a successful assassination of a democrat.

To understand the magnitude of these effects, one can compare the results in Table 5 with the background means as summarized in Table 3. The assassination effect in autocracies, raising the probability of democratization by 13.1 percentage points, is more than triple the 4.2 percent background probability of autocratic to democratic transitions—a large effect. Meanwhile, the 19 point increase in the percentage of "regular" leader transitions over the ensuing 20 years covers two-thirds of the 29- percentage-point difference between democracies and autocracies in this measure—once again, a large effect.

Table 6 breaks down the effects by the tenure of the leader at the time of the assassination attempt and by the duration of the effect. Each cell reports the coefficient on *SUCCESS* from a separate regression, where the sample is shown in the column and the duration of the change used to calculate the dependent variable is shown in the row. The top panel indicates that the short-run move to democracy is particularly large following the assassination of long-tenured autocrats, for whom a successful assassination increases the probability of democratic transition in the next year by

[21] The Archigos analysis of subsequent leader transitions excludes the leader in power at the time of attempt. Calculating the percentage of regular transitions over all transitions 1–20 years after the attempt (as opposed to excluding the target of the attempt) produces stronger results than those reported in the table.

[22] One potential critique of the Polity IV measures is that the Polity analysts may have used changes in leadership to demarcate underlying changes in institutions. This concern, however, does not apply to the percent of regular transitions variable from Archigos, which examines only leadership changes for subsequent leaders. The fact that we obtain substantial effects using the percent of regular transitions variable suggests that coding decisions are not driving the results.

[23] We have also considered the sub-indices of the POLITY2 variable (in results not reported but available from the authors). These indices include the XCONST, which measures the degree of executive constraints on the leader, and POLCOMP, which measures political competition—the extent to which alternative political preferences can be both expressed and pursued. POLCOMP is intended to refer to aspects of the political regime other than the power of the executive (which is captured by XCONST). We find that these measures produce broadly similar results to those presented in the tables.

TABLE 6—TENURE OF LEADER AND DURATION OF EFFECTS

	All leaders			Autocrats only		
	All (1)	Tenure ≤ 10 (2)	Tenure > 10 (3)	All (4)	Tenure ≤ 10 (5)	Tenure > 10 (6)
Panel A: Directional change in POLITY2 dummy						
1 year out	0.079	0.058	0.129	0.130	0.088	0.214
	(0.051)	(0.051)	(0.125)	(0.057)	(0.069)	(0.110)
Parm. *p*-value	0.12	0.26	0.31	0.03**	0.21	0.06*
Nonparm. *p*-value	0.02**	0.31	0.01***	0.01***	0.13	0.02**
10 years out	0.046	0.013	0.092	0.190	0.226	0.169
	(0.062)	(0.075)	(0.146)	(0.079)	(0.108)	(0.132)
Parm. *p*-value	0.46	0.86	0.53	0.02**	0.04**	0.21
Nonparm. *p*-value	0.01**	0.12	0.03**	0.05**	0.22	0.08*
20 years out	−0.003	−0.006	0.001	0.023	0.091	0.013
	(0.091)	(0.116)	(0.154)	(0.090)	(0.117)	(0.157)
Parm. *p*-value	0.98	0.96	0.99	0.80	0.44	0.94
Nonparm. *p*-value	0.86	0.78	0.72	0.59	0.75	0.60
Panel B: Percentage of transitions by "regular" means						
1–10 years out	0.099	0.126	0.087	0.186	0.197	0.102
	(0.077)	(0.089)	(0.243)	(0.113)	(0.145)	(0.255)
Parm. *p*-value	0.21	0.16	0.73	0.11	0.18	0.70
Nonparm. *p*-value	0.35	0.18	0.53	0.16	0.25	0.28
1–20 years out	0.111	0.116	0.274	0.165	0.147	0.306
	(0.057)	(0.063)	(0.181)	(0.095)	(0.113)	(0.227)
Parm. *p*-value	0.06*	0.07*	0.15	0.09*	0.20	0.20
Nonparm. *p*-value	0.18	0.23	0.03	0.05**	0.15	0.03**
11–20 years out	0.119	0.1	0.368	0.208	0.181	0.422
	(0.068)	(0.072)	(0.246)	(0.107)	(0.110)	(0.275)
Parm. *p*-value	0.09*	0.17	0.16	0.06*	0.11	0.15
Nonparm. *p*-value	0.25	0.59	0.04	0.03**	0.16	0.05**

Notes: Each cell reports the coefficient and *p*-values on "success" from a separate regression of equation (1). Columns 1 and 4 report results for all leaders, columns 2 and 5 report results for those with tenure ≤ 10 years in year before assassination, and columns 3 and 6 report results for those with tenure > 10 years in year before the year of attempt. For the POLITY2 dummy, one year out compares the change in polity score one year after attempt to one year before attempt; five years out compares the change in polity score five years after attempt to one year before attempt, etc. For regular transitions, 1–10 years out calculates the average percentage of leadership transitions that are regular in years 1–10 after the attempt, etc. Standard errors and *p*-values are as in Table 4.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

21 percentage points relative to a failed assassination. The distinction with respect to tenure is less clear with time, however. The most interesting result in this table appears in column 4 of panel A, which shows that democratic transitions following assassinations of autocrats appear to be sustained 10 years later. The point estimate suggests that initially autocratic regimes are 19 percentage points more likely to be democracies 10 years after the attempt if the assassination succeeded. Twenty years into the future, however, the results are substantially attenuated using the binary Polity IV measure.

Panel B of Table 6 considers the probability that future leader transitions are regular. Of particular note is the last row, where we limit ourselves to leadership transitions that occur between 11 and 20 years after the assassination attempt. These results show that, following the successful assassination of an autocrat, leadership transitions 11–20 years after the attempt are 21 percentage points more likely to be regular. Following the successful assassination of a long-tenured autocrat, leadership transitions 11–20 years after the attempt are 42 percentage points more likely to be regular, though this result is only statistically significant in the nonparametric specification. Combined, these results suggest that assassinations have substantial, and at least somewhat prolonged, effects on institutions.

War.—Table 7 examines the effect of assassination on war status. The dependent variable is the difference in war status of a country one year after assassination attempts compared to one year before.[24] The first column presents the results for the full sample, using all attempts from 1875–2002 and the Gleditsch–COW data. The second column presents the results again but restricting the Gleditsch–COW data to the postwar period (1946–2002), and the final column presents the results using the PRIO data for the same sample (1946–2002).[25] Panel A presents the average effect of successful assassination relative to failed attempts. Panel B splits the sample by war status in the year prior to the attempt.

Looking at Table 7, we see three primary results. First, there is weak evidence that successful assassination attempts, compared to failed assassination attempts, tend to hasten the end of intense wars (i.e., wars with greater than 1,000 battle deaths). This effect appears in panel B, column 1, and suggests that successful assassination lowers the probability of continued, intense conflict by 25 percentage points, which represents about a 70 percent increase above the 35-percentage-point background probability that an intense war ends (see Table 3). Although the effect is quite large in magnitude, it is only marginally significant (p-value of 0.08 parametrically and 0.13 non-parametrically) and is not significant when we restrict to the post–World War II period. The post-war results are difficult to interpret, however, because there are few observations of intense wars after 1946. Overall, we conclude that there is some evidence, but only weak evidence, for an effect on intense wars.

Second, there is evidence that successful assassination attempts, compared to failed attempts, lead to increased intensity of existing moderate-level conflicts. This is seen in panel B, column 3, where we see a 33 percentage point increased probability that a war intensifies when the leader is killed, which triples the 17-percentage-point background probability that a moderate war intensifies (see Table 3). This large point estimate shows some significance (p-value of 0.05 parametrically, and 0.13 nonparametrically) even though the sample size is substantially smaller given that the PRIO data exists only for the post-1945 era.

[24] We group all types of war, which are mainly interstate wars or civil wars. In results not reported, we analyze civil wars separately and find no substantial difference in the results.

[25] We define the PRIO variable to be 0.5 if a small conflict is taking place, 1 if a large conflict is taking place, and 0 otherwise.

TABLE 7—ASSASSINATIONS AND CONFLICT: CHANGE ONE YEAR AFTER ATTEMPT

	Gleditsch-COW dataset 1875–2002 (1)	Gleditsch-COW dataset 1946–2002 (2)	PRIO/Uppsala dataset 1946–2002 (3)
Panel A: Average effects			
Success	−0.072	0.041	0.162
	(0.068)	(0.093)	(0.071)
Parm. *p*-value	0.29	0.66	0.02**
Nonparm. *p*-value	0.57	0.83	0.03**
Observations	223	116	116
Data source	Gleditsch	Gleditsch	PRIO
Panel B: Split by war status in year before attempt			
Success × intense war	−0.255	−0.103	−0.110
	(0.144)	(0.257)	(0.294)
Success × moderate war			0.334
			(0.163)
Success × not at war	−0.024	0.020	0.070
	(0.068)	(0.086)	(0.057)
Intense war—parm. *p*-value	0.08*	0.69	0.71
Intense war—nonparm. *p*-value	0.13	1.00	0.69
Moderate war—parm. *p*-value	N/A	N/A	0.05**
Moderate war—nonparm. *p*-value	N/A	N/A	0.13
Not at war—parm. *p*-value	0.73	0.82	0.22
Not at war—nonparm. *p*-value	0.62	0.71	0.21
Observations	222	116	116
Data source	Gleditsch	Gleditsch	PRIO

Notes: See notes to Table 5. Nonparametric *p*-values are computed using Fisher's exact tests. In panel B, at war/not at war is defined by whether the relevant war concept (i.e., the concept used in the dependent variable) is positive in the year before the attempt. The main effect for the lagged war variable is also included in the regression in panel B.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Third, we find that the outcomes of assassination attempts have no statistically significant effect on the start of new wars. This is seen in panel B, across both datasets we examine. For example, taken literally this suggests that World War I might have begun regardless of whether or not the 1914 attempt on the life of Archduke Ferdinand had succeeded or failed.[26]

In sum, these results suggest heterogeneity in the effect of assassination, depending on the level of conflict at the time of the attempt. The success or failure of an assassination does not matter for the start of conflicts, as least as we can measure them in our data. However, successful assassinations, compared to failed assassinations, appear to intensify moderate-level conflicts but hasten the end of high-intensity conflicts. These are somewhat subtle results, suggesting an important role of

[26] Note, however, that this event itself is not in our data, as Archduke Ferdinand was the crown prince of Austria-Hungary, rather than the leader.

TABLE 8—ALTERNATIVE SPECIFICATIONS

	Absolute change in POLITY2 dummy one year out	Directional change in POLITY2 dummy one year out		Percentage regular leader transitions 1–20 years out	
	(1)	(2)	(3)	(4)	(5)
	All	All	Autocrats only	All	Autocrats only
Baseline specification (serious attempts)	0.091 (0.047)	0.079 (0.051)	0.131 (0.055)	0.111 (0.057)	0.191 (0.085)
Parm. *p*-value	0.06*	0.12	0.02**	0.06*	0.03**
Nonparm. *p*-value	0.03**	0.02**	0.01***	0.18	0.05**
Observations	221	221	142	138	74
Control group: bystanders or target wounded	0.078 (0.049)	0.076 (0.052)	0.130 (0.055)	0.151 (0.074)	0.255 (0.097)
Parm. *p*-value	0.11	0.15	0.02**	0.05**	0.01***
Nonparm. *p*-value	0.07*	0.06*	0.02**	0.13	0.01***
Observations	157	157	103	97	54
Control group: target wounded	0.081 (0.050)	0.057 (0.053)	0.120 (0.055)	0.182 (0.095)	0.264 (0.126)
Parm. *p*-value	0.11	0.28	0.03**	0.06*	0.04**
Nonparm. *p*-value	0.11	0.25	0.12	0.35	0.04**
Observations	104	104	66	68	38
Control group: any attempt	0.090 (0.047)	0.068 (0.051)	0.132 (0.056)	0.116 (0.054)	0.172 (0.081)
Parm. *p*-value	0.06*	0.18	0.02**	0.04**	0.04**
Nonparm. *p*-value	0.02**	0.01***	0.01***	0.37	0.10*
Observations	260	260	166	173	94
Solo attempts only	0.073 (0.063)	0.027 (0.066)	0.095 (0.066)	0.144 (0.060)	0.258 (0.115)
Parm. *p*-value	0.25	0.68	0.15	0.02**	0.03**
Nonparm. *p*-value	0.26	0.41	0.21	0.41	0.11
Observations	100	100	53	65	30
First attempt on leader (serious attempts only)	0.080 (0.060)	0.048 (0.066)	0.099 (0.067)	0.111 (0.061)	0.206 (0.093)
Parm. *p*-value	0.18	0.47	0.14	0.07*	0.03**
Nonparm. *p*-value	0.12	0.11	0.07*	0.51	0.11
Observations	172	172	102	108	52
Adding all Table 4 controls quarter-century FE, and region FE (serious attempts)	0.081 (0.056)	0.088 (0.057)	0.176 (0.084)	0.192 (0.063)	0.237 (0.110)
Parm. *p*-value	0.15	0.13	0.04**	0.00***	0.04**
Observations	189	189	115	112	57

Notes: See Section IID.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

assassination for conflict, but with effects depending on the circumstances. We will consider further interpretation of the conflict results in Section IVB below.

D. *Robustness Checks and Additional Specifications*

Our main results feature parametric and nonparametric tests, and thus confront alternative specifications of the error process. In this section, we further consider a number of robustness checks based on alternative specifications of assassination events and the inclusion of observable variables.

Table 8 reconsiders the main results for institutional change. For comparison, the top panel summarizes the baseline results from Table 4, where we compared successful assassinations against failed "serious attempts," defined as those attempts in which the weapon was actually used in the attempt to kill the leader. The next four panels consider different ways of defining the comparison group of failed assassination attempts. We see that further limiting the set of serious attempts to cases where there were casualties (i.e., where the target or a bystander sustained wounds) produces similar results as the main specification. Further limiting the control group to cases where the leader was wounded (but not killed) reduces the set of failed attempts by 70 percent, leaving only 40 failures. When we focus on this more limited sample, the results tend to lose some significance, though some results remain statistically significant and overall the point estimates do not change substantially. The next panel uses the full set of attempts, as opposed to serious attempts, and produces similar results to the main specification. Finally, we try limiting the observations to attempts by solo attackers, and the first attempt on a given leader. While these restrictions cut the sample size down, so that the standard errors increase, most point estimates change modestly.

The last panel of Table 8 tries a somewhat different specification. We return to the baseline specification but add, as controls, all of the variables in Table 3, as well as time (quarter-century) and region fixed effects. Including the full set of controls reduces the sample size, but the results are similar to the baseline, with typically somewhat larger coefficients and somewhat larger standard errors.[27]

In results not reported in the table (but available from the authors upon request), we have also conducted the same set of robustness checks on the war results. As with the results on institutional change, we find that the war results are essentially similar to the results in the main specifications if we consider alternate control groups (bystanders wounded, target wounded, or all attempts), consider only solo attempts or first attempts, or add the full set of controls.

Finally, in results not reported in the table (but available upon request), we consider alternative transformations of the POLITY2 variable (as opposed to the binary democracy/autocracy variable we use in the main analysis). Specifically, we examine the impact of assassinations on changes in the untransformed linear POLITY2 variable, as well as on a three-part variable where POLITY2 scores from -10 to -7

[27] In a different style of analysis, we have also considered whether natural or accidental leader deaths produce institutional change. We find that natural and accidental deaths of autocrats increase the probability of a change in institutions, but these changes are much smaller in magnitude and limited to extreme autocrats.

are coded as 0 (autocratic), from −6 to +6 are coded as 0.5 (transition), and from +7 to +10 as 1 (democratic). We find, essentially, the same results, strong moves to democracy associated with successful assassinations relative to failures, using either of these alternative variables.

III. Distinguishing between Success and Failure

The results thus far suggest that assassinations have important effects. These effects are identified using inherent randomness in whether an attack is successful, showing significant differences in outcomes comparing successes and failures. It may be natural to presume that the "successes," where the leader dies, are more important drivers of change than the "failures," since success automatically produces changes in leadership while failure does not. However, it is also possible that failed attempts change outcomes. For example, an autocrat who survives an assassination attempt may impose crackdowns on opposition groups, leading a country further from democracy.

In this section, we consider the separate effects of success and failure. Identifying these effects separately is necessarily more speculative than identifying the difference between them. The challenge is that, while the path of a bullet may be driven largely by chance, attempts themselves do not occur randomly. As a result, the absolute effect of successes and the absolute effect of failures may be conflated with changes that would have occurred anyway, and that are correlated with the probability an attempt took place. For example, if attempts on autocrats are more likely in autocracies that are in the process of liberalizing, one might erroneously attribute a subsequent democratization that would have happened anyway to the effect of a successful or failed assassination.

That said, one can make some headway on this issue by employing a propensity-score matching approach. We use observable features of the national context to predict when assassination attempts will occur and then stratify the sample according to these features. We are therefore making comparisons between years with assassination events and years without such events within comparable contexts. While this approach is not perfect, and does not solve the problem if assassination attempts are correlated with unobservable variables that also predict subsequent outcomes, it does provide a flexible approach to dealing with selection on observables.

To implement this approach, for all countries c and years t, we first estimate equations of the form

$$P(ATTEMPT_{ct}) = \Phi(\boldsymbol{\rho}\mathbf{X}_{ct}) \tag{4}$$

which allow us to predict attempts conditional on observables. Based on the predicted probabilities from (4), we form six blocks, denoted by b, for different levels of the propensity score, and check that the covariates are all balanced between treatments and controls within each block. We then estimate regressions of the form

$$y_{ib} = \alpha_b + \beta_s SUCCESS_{ib} + \beta_f FAILURE_{ib} + \boldsymbol{\gamma}\mathbf{X}_{ib} + \varepsilon_{ib}, \tag{5}$$

where α_b indicates fixed effects for each propensity score block.

TABLE 9—WHAT PREDICTS ATTEMPTS?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy dummy	−0.007*							−0.0002
	(0.004)							(0.0034)
Change in democracy dummy		−0.012*						−0.009
		(0.007)						(0.007)
War dummy			0.028***					0.025***
			(0.006)					(0.007)
Change in war				0.004				−0.007
				(0.006)				(0.005)
Log energy use per capita					−0.003***			−0.002***
					(0.001)			(0.001)
Log population						0.005***		0.004***
						(0.001)		(0.001)
Age of leader							−0.00022*	−0.0003**
							(0.00012)	(0.0001)
Tenure of leader								−0.0001
								(0.0002)
Observations	11,171	10,763	11,671	11,258	9,664	10,607	12,019	8,904
p-value of regression	0.08*	0.07*	0.00***	0.47	0.00***	0.00***	0.08*	0.00***

Notes: Results are marginal effects from a probit specification. Robust standard errors are in parentheses, adjusted for clustering at the country level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

A. *Predicting Assassination Attempts*

We start by considering whether assassination attempts are predictable and find that they are—and in interesting ways. Table 9 shows the results of estimating (4) using the same set of variables we considered in Table 4. The annual rate of assassination attempts is 0.7 percentage points higher in autocracies than in democracies. The baseline probability of an attempt in a given country-year is 2.4 percent, so this implies that autocrats are approximately 30 percent more likely to be the target of attacks in a given year. This can also be seen in Figure 2, which shows histograms of the full 21-point POLITY2 score in years without assassination attempts (left panel) and years with assassination attempts (right panel). The figure shows that assassination attempts are more likely among extreme autocracies (scores of −10) and less likely among extreme democracies (scores of +10).

Table 9 also reveals that assassination attempts are 1.2 percentage points less likely immediately following a democratic transition. Attacks are 2.8 percentage points more likely during wartime (more than doubling the background probability), which makes war a particularly powerful predictor of assassination attempts.

Interestingly, these results are consistent with the results in Section II, which showed that assassinations of autocrats had an impact on institutional change, and that assassinations had an impact on wars that were in progress. Here, the results suggest that potential assassins may understand that assassinations against autocrats

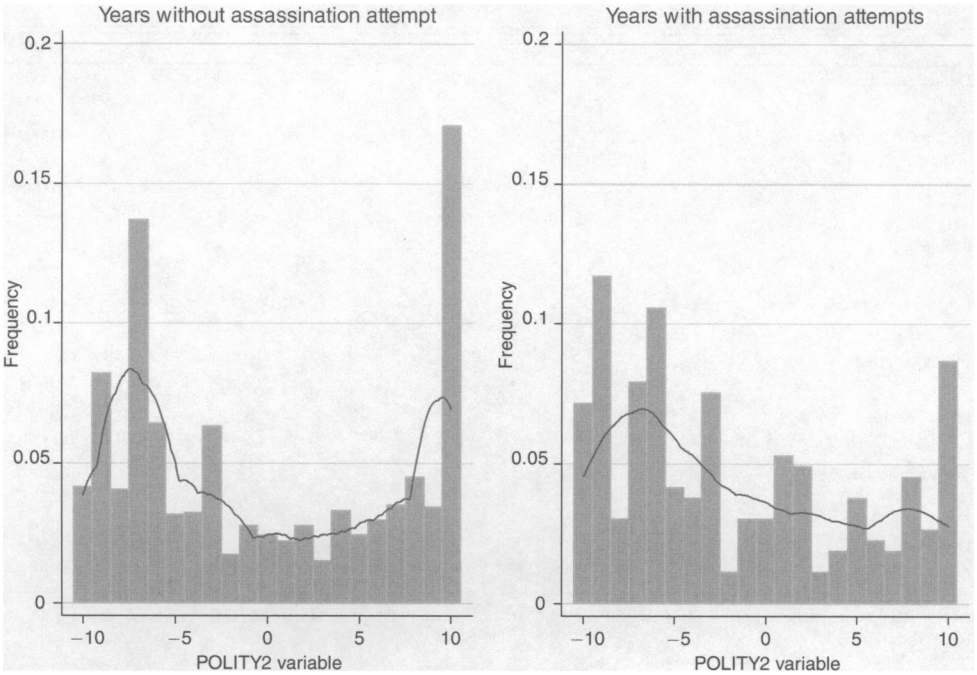

FIGURE 2. THE DISTRIBUTION OF ASSASSINATION ATTEMPTS BY TYPE OF POLITICAL REGIME

Notes: These plots display histograms for the frequency of different political regimes according to the 21-point scale of the POLITY2 index, where −10 represents the most autocratic regimes and +10 represents most democratic regimes. The line in each plot provides the kernel density estimate of the distributions. The left panel considers all country-years since 1875 in which there were no assassination attempts on the national leader, while the right panel considers only those country-years with assassination attempts. The primary result seen in these graphs is a substantially lower rate of assassination attempts in full democracies (POLITY2 = +10). These plots consider years with and without any assassination attempt in the data; plots using only serious assassination attempts are extremely similar.

Source: Authors' calculations using POLITY IV data.

or wartime leaders are more likely to have an effect, and hence are more likely to attempt to kill those leaders where it would make a difference.[28]

Another interesting result that emerges in Table 9 is that attempts are more common in countries with larger populations. Doubling the population increases the probability of an assassination attempt each year by 0.35 percentage points. Though this may seem like a small effect, this implies that the leader of a country the size of the United States (population 300 million) is 1.8 percentage points, or about 75 percent, more likely to be assassinated each year than the leader of a country the size of

[28] These results are broadly consistent with the findings of Ivo K. Feierabend et al. (1971) and Zaryab Iqbal and Christopher Zorn (2006). Feierabend et al. consider the correlates of assassination attempts from 1948–1967, and, consistent with our findings, find that assassination attempts are more common in poorer countries, more autocratic (or, in their terminology, more coercive) countries, and in countries involved in war. Iqbal and Zorn consider predictors of successful assassinations since World War II and find, as we do, that political institutions and war predict assassination. Both studies are limited to the question of predicting assassinations rather than assessing the consequences of assassination.

Switzerland (population 7.5 million). This population effect is sustained in a multivariate context, so that it does not appear to proxy for per capita income, institutions, or war status. One natural interpretation is that the number of would-be assassins rises with a country's population, whereas there is only one leader in each country. Therefore, the ratio of would-be assassins to leaders, and hence the probability of an attempt, increases with population. The results in Table 9 also indicate that assassination attempts are somewhat less likely in richer countries, as measured by energy intensity. Note that, in results not reported in the table, both the population and the energy intensity results are unchanged when we include decade fixed effects, so that these results are not being driven by growth in population or income over time.

B. *The Roles of Success and Failure*

Given these predictors of assassination attempts, Table 10 presents separate estimates for the effects of success and failure, relative to comparable years in which there was no assassination attempt, using equation (5). For each dependent variable, we present two specifications. In the first column, we present the regression with no controls. In the second column, we include all of the controls in Table 9, which we have seen have substantial predictive power for assassination attempts, and stratify the sample using propensity score matching.[29] As is evident in the table, adding the controls and the propensity score matching has a negligible effect on the estimates.[30]

We find several interesting results. Keeping in mind the caveats about identification in this section, we see that most of the effects identified in Section II appear to be driven by successful assassinations, though there are some cases in which failures may have effects. The first two columns in Table 10 investigate the absolute value of changes in the POLITY2 dummy. The evidence suggests that it is successful assassinations that are driving the results. Similar insight is provided by the second set of columns, which consider moves toward democracy. Examining autocrats, successful assassination increases the probability of democratic transition in the next year by 13 percentage points compared to years without assassination events, while failed assassinations suggest a modest and statistically insignificant 1 percentage point fall in the probability of democratic transition. The effects of failure are amplified when we consider the percentage of "regular" leader changes in the ensuing 20 years, where successful assassinations of autocrats are associated with 16–22 percentage point increases in the percentage of regular future leader transitions while failure is associated with 5–7 percentage point declines in the percentage of such leader transitions (though the latter is not statistically significant). If these estimates of failure represent the true causal effect of a failed assassination (as opposed, perhaps, to selection effects not controlled for perfectly with the propensity score methodology), then this would suggest that autocrats may tighten their grip on power slightly after failed assassination attempts.

[29] In results not reported in the table, we find that alternate methods of propensity score estimation, such as kernel density matching and nearest-neighbor matching, produce qualitatively similar results in almost all cases.

[30] In results not reported in the table, we also find that including additional variables, such as dummies for all 21 possible POLITY values in the year before the election and the number of leader changes in the previous 5 years, as controls or in the propensity score, also does not affect the results.

TABLE 10—SEPARATING IMPACTS OF SUCCESSES AND FAILURES ON INSTITUTIONAL CHANGE

	Absolute change in POLITY2 dummy		Directional change in POLITY2 dummy		Percent regular leader transitions 1–20 years out	
	No controls (1)	Adding controls and propensity score stratification (2)	No controls (3)	Adding controls and propensity score stratification (4)	No controls (5)	Adding controls and propensity score stratification (6)
Panel A: Average effects						
Success	0.098	0.100	0.066	0.060	0.071	0.112
	(0.042)	(0.042)	(0.047)	(0.045)	(0.040)	(0.042)
Failure	0.006	0.005	−0.017	−0.021	−0.071	−0.040
	(0.018)	(0.017)	(0.019)	(0.019)	(0.041)	(0.024)
Success *p*-value	0.02**	0.02**	0.17	0.18	0.08*	0.01***
Failure *p*-value	0.72	0.76	0.39	0.33	0.08*	0.10*
Observations	10,932	10,932	10,932	10,932	5,979	5,979
Data source	Polity IV	Polity IV	Polity IV	Polity IV	Archigos	Archigos
Panel B: Split by regime type in year before attempt						
Success × autocracy	—	—	0.125	0.125	0.155	0.212
	—	—	(0.057)	(0.056)	(0.059)	(0.054)
Failure × autocracy	—	—	−0.013	−0.009	−0.074	−0.052
	—	—	(0.016)	(0.016)	(0.052)	(0.040)
Success × democracy	—	—	−0.051	−0.054	0.023	0.007
	—	—	(0.066)	(0.063)	(0.034)	(0.042)
Failure × democracy	—	—	−0.042	−0.039	−0.025	−0.028
	—	—	(0.042)	(0.042)	(0.038)	(0.032)
Autocracy *p*-value—success	—	—	0.03**	0.03**	0.01**	0.00***
Autocracy *p*-value—failure	—	—	0.42	0.59	0.16	0.20
Democracy *p*-value—success	—	—	0.44	0.39	0.50	0.87
Democracy *p*-value—failure	—	—	0.32	0.36	0.51	0.38
Observations			10,932	10,932	5,573	5,573
Data source			Polity IV	Polity IV	Archigos	Archigos

Note: Controls include all variables shown in Table 9; quarter-century fixed effects; and region fixed effects.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

In sum, the institutional changes identified in Section II appear to decompose into substantial roles for success and smaller, and typically statistically insignificant, roles for failure in the opposite direction. The results are quite consistent across specifications, so that controlling for observables and propensity score matching do not appear critical to the results. This suggests that, to the extent that the observable variables used in the propensity score form an important part of the selection of when attempts take place, these selection effects are not driving the results. Of course, it is impossible to know whether the effects of failures we pick up are driven by selection on unobservables, but the fact that controlling flexibly for the observable predictors of attempts makes no substantive difference provides at least suggestive evidence that the estimates are, in fact, identifying the effect of failures rather than a pure selection effect.

Given that only 25 percent of assassination attempts are successful, if we take the point estimates in Table 10 literally, they imply that the average effect of assassination attempts on democracy is only modestly positive ex ante, if positive at all. Overall,

the results imply that one would expect a 6 percentage point move toward democracy if the assassination succeeds (approximately 25 percent of the time), and a 2 percentage point move toward autocracy with failure (approximately 75 percent of the time), suggesting an approximately zero net effect on average. Focusing on autocrats, meanwhile, suggests a modest, positive move to democracy in expectation, with the point estimates implying a 2–3 percent ex ante increased probability of democratization from assassination using the POLITY2 measure and essentially no mean shift ex ante using the Archigos measure of future leader transitions—far smaller than the 15–20 percent average move to democracy comparing success with failure. Thus, a policy of assassination attempts creates risk—it increases the probability that there will be a change in a country's institutions—but if the probability of an attempt succeeding is 25 percent, there are, at most, modest gains in democracy on average.

The results on war, presented in Table 11, are similar to the results for institutional change in that they decompose into substantial roles for success and smaller roles for failure. Focusing on panel B, where we split by war status, columns 1 and 2 indicate that if a country is already involved in a serious conflict, a successful assassination can hasten the end of that conflict, with failed assassination attempts having little effect. Specifically, the coefficients on success, suggesting a 25 percentage point fall in the probability that the war continues, are similar to what we found in Table 7 and are now significant at the 95 percent level. Meanwhile, failure to kill the leader during an intense war has no apparent effect on the conflict. As with Table 7, however, these effects are substantially weaker in columns 3–6 where we consider post-1946 data. As previosuly noted, there are few relevant observations of intense conflicts in this later period, so decisive interpretation of the post-war difference is difficult.

Second, focusing on moderate-level conflicts, in panel B, columns 5 and 6, we see that most of the intensification effect found in Table 7 is driven by successful assassinations, although failed assassinations do suggest a decline in conflict intensity. Taken literally, this latter result might suggest that failed assassination attempts scare leaders enough to lead to a cessation of conflict. Given the opposing effects of success and failure, and the greater propensity for failed attempts in the data, these results share a similarity with the results for institutional change: assassination attempts increase the variance of outcomes, but produce approximately neutral effects on moderate-level conflicts on average.

Looking at cases where the country is not at war, the results, using both datasets, suggest that successes and failures lead to an increase in conflict. Taken literally, this suggests that the act of an assassination attempt provokes conflict, regardless of the success of the attempt. However, it is also possible that this result reflects the inability of the propensity score matching techniques to adequately predict assassination attempts in the context of incipient war, particularly if we view the assassination attempt as the opening shot of war.[31]

Overall, the war results make clear that the outcome of assassinations can affect the outcome of wars in progress, and that there may be substantial heterogeneity in the nature of these effects.

[31] For example, on the eve of the 2003 Iraq War, the US government actively sought to kill Saddam Hussein through targeted bombing.

TABLE 11—SEPARATING IMPACTS OF SUCCESSES AND FAILURES ON CONFLICT

	Gleditsch-COW dataset 1875–2002		Gleditsch-COW dataset 1946–2002		PRIO/Uppsala dataset 1946–2002	
	No controls (1)	Adding controls and propensity score stratification (2)	No controls (3)	Adding controls and propensity score stratification (4)	No controls (5)	Adding controls and propensity score stratification (6)
Panel A: Average effects						
Success	−0.069	−0.024	0.035	0.019	0.080	0.076
	(0.060)	(0.049)	(0.075)	(0.068)	(0.062)	(0.061)
Failure	0.001	0.054	−0.022	0.004	−0.056	−0.042
	(0.038)	(0.034)	(0.047)	(0.042)	(0.037)	(0.038)
Success *p*-value	0.25	0.63	0.64	0.79	0.20	0.21
Failure *p*-value	0.98	0.12	0.65	0.92	0.13	0.27
Observations	11,286	11,286	7,183	7,183	7,183	7,183
Data source	Gleditsch	Gleditsch	Gleditsch	Gleditsch	PRIO	PRIO
Panel B: Split by war status in year before attempt						
Success × intense war	−0.248	−0.249	−0.095	−0.106	−0.044	−0.038
	(0.125)	(0.123)	(0.219)	(0.226)	(0.272)	(0.295)
Failure × intense war	0.006	0.011	−0.042	−0.028	0.059	0.071
	(0.063)	(0.060)	(0.081)	(0.084)	(0.072)	(0.075)
Success × moderate war					0.208	0.201
					(0.137)	(0.144)
Failure × moderate war					−0.091	−0.094
					(0.074)	(0.067)
Success × not at war	0.066	0.056	0.074	0.044	0.070	0.043
	(0.051)	(0.050)	(0.066)	(0.067)	(0.055)	(0.056)
Failure × not at war	0.104	0.072	0.049	0.016	0.036	0.007
	(0.043)	(0.039)	(0.041)	(0.040)	(0.035)	(0.035)
Intense war *p*-value—success	0.05**	0.04**	0.67	0.64	0.87	0.90
Intense war *p*-value—failure	0.93	0.85	0.60	0.74	0.42	0.34
Moderate war *p*-value—success					0.13	0.16
Moderate war *p*-value—failure					0.22	0.16
No war *p*-value—success	0.20	0.27	0.27	0.52	0.21	0.44
No war *p*-value—failure	0.02**	0.07*	0.23	0.70	0.32	0.83
Observations	11,286	11,286	7,183	7,183	7,183	7,183
Data source	Gleditsch	Gleditsch	Gleditsch	Gleditsch	PRIO	PRIO

Note: Controls includes all variables shown in Table 9; quarter-century fixed effects; and region fixed effects.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

IV. Interpretations and Implications

Beyond providing an analysis of assassination per se, the facts in this paper inform theories of institutional change and conflict more broadly. We discuss several interpretations and potential implications.

A. *Theories of Institutional Change*

A long literature in economics and political science has sought to explain the determinants of democracy. One strand of this literature has focused on deep historical antecedents for modern institutional forms (Moore 1966, North 1990, Engerman and Sokoloff 2000, Acemoglu et al. 2001, Glaeser and Shleifer 2002) including democracy. In this view, many of a country's core institutions were set up hundreds of years ago in response to conditions prevailing at the time, and these institutions then persisted. While this view has found substantial empirical support, many countries do change their institutions sharply with time, leaving the important question of how countries change their institutions unanswered. Meanwhile, influential "modernization theory," which attempts to explain democratization through increased education or income (e.g., Lipset 1959, Huntington 1991, Barro 1999), has difficulty explaining within-country democratic transitions in the twentieth century (Acemoglu et al. 2008). Thus, contemporary sources of democratic change remain substantially in the error term of recent econometric work.

In this paper, we provide evidence for a source of contemporary change in institutions that complements the existing literature and steps beyond the confines of distant history—democratization may lie in the hands of individual leaders. The role of leaders, as opposed to social forces, is a contentious subject in explaining institutional change (e.g., Huntington 1996, Tato Vanhanen 2003). For example, it is debated whether or not Mikhail Gorbachev, former president of the Soviet Union, was the driving force in the transition to democracy in the former USSR (Martin Malia 1994, Archie Brown 1996). In econometric analysis, a positive relationship between leadership change and democratic transitions was established by John Benedict Londregan and Keith T. Poole (1996). However, in analyzing this relationship, Londregan and Poole argued under structural assumptions that these changes appear jointly determined by other forces.

In this paper, we focus on exogenous changes in leadership and find that the successful assassinations of autocrats lead to substantial, sustained, increased likelihood of democratic transition, while failed assassination attempts do not, so that changing the leader appears important to institutional change. Thus, our findings suggest "agency at the top," with autocratic national leaders as important forces in restraining or promoting democratic change. The potentially critical influence of autocrats is also supported by Jones and Olken (2005), which shows that autocrats, and only autocrats, have statistically significant effects on national economic growth.

B. *Theories of War*

A fundamental puzzle of war is why wars, which are so costly, occur in equilibrium. Fearon (1995), in his well-known analysis of war, articulates three broad classes of explanation: irrationality, bargaining failures between countries (e.g., due to information asymmetries or commitment problems), and differences in incentives between leaders and populations.

The results in this paper, by isolating the impact of leadership changes on conflict, provide evidence for the third of these hypotheses—i.e., that divergent

incentives between the leader and the population can create or sustain war (e.g., Goemans 2000, Jackson and Morelli 2007, Baliga et al. 2007). For example, Goemans (2000) argues that leaders may be unwilling to settle wars under unfavorable circumstances since they would suffer personally if they lost the war. In this view, replacing the leader with someone who is not responsible for the war may increase the probability that the war ends, providing one possible explanation for our finding that intense wars are more likely to end following a successful assassination.

Classic ideas in war strategy suggest even simpler explanations for the relationship between leadership change and war status. For example, Carl von Clausewitz (1989) emphasizes attacking "centers of gravity" to disrupt war operations, and Bruce A. Ross (1992) suggests that attacking a country's leader, particularly in an autocratic state, is a natural implication of applying Clausewitz's ideas. While a successful attack might end a war outright, it could also be seen as a tactical success creating disarray that raises the return to broader intensification of moderate wars in pursuit of winning the war.[32]

Given the somewhat subtler and weaker nature of our econometric results for conflict, we cannot point definitively to any single conflict theory. Nonetheless, the results support more broadly that subset of existing theoretical approaches in which individual leaders play an important role in the initiation and cessation of conflict.

V. Conclusion

This paper examines the effect of assassination on the evolution of political institutions and military conflict. Using a novel dataset of assassinations and assassination attempts against national political leaders from 1875 to 2004, we employ inherent randomness in the success and failure of assassination attempts to identify whether these events affect national outcomes. We find that the successful assassination of autocrats produces institutional change, substantially raising the probability that a country transitions to democracy. This democratization effect is sustained ten years later. The results for war are less systematic, with some evidence that assassination can exacerbate moderate-level conflict but hasten the end of intense conflict. There is little evidence that the outcome of assassination attempts matters for the instigation of new wars.

In sum, these results show that assassinations affect political institutions and conflict. Whether or not assassinations change "the history of the world" in Disraeli's words, they do appear to change the history of individual countries. Our tests provide evidence that small elements of randomness—the path of a bullet, the timing of an explosion, small shifts in a leader's schedule—can result in substantial changes in national outcomes.

The findings also inform central issues in political economy. The results inform the theory of war, lending support to those models of conflict that feature agency

[32] Following the broader insights of the economics literature on price wars (e.g., Edward J. Green and Robert H. Porter 1984), the intensification of moderate wars upon the leader's assassination might also be understood as a trigger strategy by the attacked, which is necessary to protect leaders in equilibrium.

for leaders, with divergent incentives between the leader and the population at large. The results further suggest that individual autocrats are cornerstones of national institutions, thus informing the important, policy-relevant and empirically challenging question of how societies move to democracy. Our findings complement the literature on institutional origins by showing an important component of institutional change that lies not in distant history but in contemporary hands.

APPENDIX

Data Collection—This appendix describes the method for collecting the assassinations data. For detailed information about the Archigos, Polity IV, or Correlates of War datasets, and their construction, please see the resources listed in the references.

To find assassinations and assassination attempts, we used the list of primary national leaders from 1875 to 2004 provided by Archigos and ran extensive keyword searches on the archives of major newspapers. The searches examined whether words for assassination-type events appeared in close proximity with particular leader and country identifiers. The keywords used to capture the events were:

- EVENT: {assassination, assassin, assassinated, wound, wounded, injure, injured, kill, killed, attack, attacked, attempt, attempted, bomb, bombed, murder, murdered, shot, shoot, stab, stabbed, assault, assaulted, escape, escaped, die, dies, died, perish, perishes, perished, slain}

while the country and leader identifiers were country specific. For example, for Afghanistan we used:

- LEADER: {emir, king, president, prime minister, premier, amir, leader, ruler}

- COUNTRY: {Afghanistan, Afghan}.

Specific country and title names were taken from da Graca (2000), with the keywords "leader" and "ruler" used in all searches. For some countries, where the generic LEADER keywords returned over 300 articles, we used the names of specific leaders in place of generic titles.

The search results (returned articles) were then examined to determine whether an assassination attempt or assassination had occurred. Information was then collected about the date of the event, outcome for the leader, weapon(s) used, location of the attack, extent of other casualties, and also about whether a solo assassin or group were responsible for the attack.

The searches were first run exclusively on electronic archives of the *New York Times* and then sequentially on archives of the *Washington Post* and *Wall Street Journal*. For each country, different research assistants conducted the searches on each newspaper. Distinctions between assassinations and coup d'états were determined as necessary through the newspaper articles and through historical resources, primarily Lentz (1988, 1994, 1999, 2002). Summary statistics are presented in Table 2. The codebook and detailed data are available from the authors.

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