WAR AND THE FATE OF REGIMES: A COMPARATIVE ANALYSIS

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Governments are likely to be held accountable for the success or failure of their foreign policies. Consequently, we claim that international wars can, under specified conditions, have domestically instigated consequences for violent regime change in the political systems of the participants. Drawing upon all international war participation between 1816 and 1975, we seek to answer the question, Do wars lead to violent changes of regime and if so, under what conditions? Three hypotheses set out the expected associations of a nation's initiator or target role in a war, the war outcome, and the costs of the war with domestically instigated violent changes of regime. Direct relationships are found for all three and hold even against possible threats to their validity and robustness. The results suggest that domestic politics play a larger role in national security policy than is generally believed by realist or neorealist theorists.

Today those of us living in democratic states take for granted the right to hold governments accountable that fail to advance and protect the well-being of their citizens. The acknowledgment of such a right is not universal, nor is it of such long standing. The right to punish a regime by removing it from power—with force if necessary—was hotly debated among social contract theorists throughout the seventeenth and eighteenth centuries. It remains a contentious issue in China, much of Africa and the Middle East, and elsewhere around the globe even today. Beyond the philosophical question of the "natural right" to punish regimes for failed policies is the empirical issue of accountability. Whatever position one may take on the philosophical issue, it is important for leaders and researchers alike to know whether regimes are held responsible for failed national security policies.

If, as a matter of fact, regimes (encompassing leaders and their policies) are held responsible for jeopardizing the well-being of citizens, then we should expect such accountability to dampen any enthusiasm for risky political adventures. In particular, if the fate of a regime turns on how its national security policies affect its citizens, then those policies must be shaped and constrained by expectations about the domestic political consequences if they fail. According to this perspective, every foreign policy maker must look over his or her shoulder and contemplate the prospective domestic political risks associated with each foreign policy action (Lamborn 1991).

We propose to investigate whether the fate of regimes has been dependent on domestic responses to war experiences over the past century-and-a-half. War-related actions are a good starting point, because war is the costliest policy in which a regime can engage. As such, it is the governmental action most likely to jeopardize citizen welfare. Because war has the potential to be extremely costly, experiences in war are particularly salient for an investigation of the political accountability of regimes. Our principal concern, then, is with the relationship between a government's war experience and the likelihood of a forceful removal of the regime by its domestic opponents.[1]

If a relationship between domestic responses to war performance and regime stability is validated empirically, then the realist or neorealist emphasis on external, structural features of the international system probably understates the importance of domestic affairs in shaping national security policy. Regime leaders, according to a perspective that takes accountability seriously, must consider how their actions will play at home even if an adventurous foreign policy ultimately is expected to prove successful militarily. The quest for enhanced international power and security would be expected also to accommodate the desire to satisfy domestic constituencies that can threaten a regime's survival at home (Bueno de Mesquita and Lalman 1992).

Does international war involvement increase the probability that a nation's political regime will be removed from power through violent means? Historians, sociologists, and political scientists have speculated that it does; but broad, systematic data on the relationship are lacking. No research directly addresses the possible linkage of war to revolutionary domestic upheaval in an explicit manner using a rigorous research design. We attempt to correct this lacuna and in doing so to ascertain how war experiences influence the stability of regimes.

The lack of research on this important possible consequence of war is puzzling. Social scientists seem generally agreed that wars are likely to produce an increase in the probability of a violent regime change, but they differ with regard to the factors believed to explain such changes. Some argue that the resource extraction necessary to fight a war will produce instability (Tilly 1975, 1990). Others argue that rebel-

lion will follow war participation when a regime is unable to extract the resources necessary to meet the demands placed upon it by the population. This leads to a consequent decrease in legitimacy and an increase in dissatisfaction with the product of the state (DeNardo 1985; Lamborn 1991; Organski and Kugler 1980). Still others argue that wars bring about a redistribution of power among interest groups and coalitions "and thus prime society for conflict over values and resources, including revolutionary conflict" (Starr 1991, 26; see also Goldstone 1980).

Despite these assertions, prior empirical research provides little evidence relating war experiences to domestic political upheaval and revolution. To be sure, there is research on the effects of international conflict on all levels of internal political conflict; but the results of this research are either mixed (Wilkenfeld and Zinnes 1973), based upon only casual inspection of the data (Sorokin 1957), or limited to a single society (Rasler and Thompson 1989; Stohl 1980).

In reviewing a number of these studies, Stohl begins with the observation that although it is widely thought that war leads to domestic political conflict, "only a few of the studies actually confront this problem with systematic data" (1980, 316). Stohl's further evaluation of these few studies points out their mixed findings and limitations of research design and interpretation. He concludes that a considerable amount of work needs to be done on the possible effects of international conflict on political instability.[2]

Since Stohl's review there has been relatively little new empirical research on war and violent regime change. Two studies published in the same year as Stohl's essay but probably not available to him offer examinations of war participation and regime change; but their analyses are brief and limited in range and do not distinguish among various forms of regime change (Siverson 1980; Stein and Russett 1980). Rasler and Thompson (1989) trace out the effects of war involvement on domestic political conflict; but their analysis, while theoretically rich and methodologically sophisticated, is limited to the United States in the twentieth century. As part of his analysis of the relationship between national economic decline and dispute escalation, Russett examines the effects of international conflict participation on rebellion, which he measures by a combination of the numbers of armed attacks on political authorities and the resulting deaths; but no direct relationship is found (1990, 136).

Still, the idea that there is a link between international war and internal political change persists—and perhaps for good reason. Tilly, for instance, asserts, "All of Europe's great revolutions, and many of its lesser ones, began with the strains imposed by war" (1990, 186). Tilly recounts the effects of wars in producing the English Revolution of 1688 and the French Revolution and concludes with the following description of the Russian Revolution: "Russian losses in World War I discredited tsarist rule, encouraged military defections, and made the state's vulnerability patent" (ibid.). Tilly's assertion is accurate; but it addresses only major revolutions, the largest, most visible part of violent regime change. Our interest is in probing the overall linkage between war participation and violent domestic political changes of all kinds—not just the relatively few highly visible cases. A few cases, no matter how widely recognized as historically important, are not the basis for a sustainable generalization (Riker 1957).

There is, consequently, much we do not know about the relationship that might exist between war participation and rebellion or revolution. For example, at what rate do the states that enter wars experience violent changes of regime? Does it make a difference in that rate if the state does well in its performance against its opponents (i.e., wins, rather than loses)? Does it make a difference whether the state is an initiator of the war or the target of some other state's policies? Do differences in the cost of war have a consequence for the likelihood of violent regime changes?

Answers to questions such as these are critically important if we are to understand not only the aftermath of war but *also the judgments and expectations of decision makers that promote its inception.* War is not only about gains in national power or security within the context of the international system. We believe that the causes and consequences of wars extend well beyond the systemic, structural impediments that are the focus of realist and neorealist theories of international affairs (Morgenthau 1973; Waltz 1979). War is also a powerful force shaped by, and giving shape to, domestic political affairs and for making *or breaking* the political fortune of national governments. As such, an understanding of the conditions under which war enhances or diminishes the fate of a regime is important both from the perspective of an interest in reducing the danger of violent conflicts and from the perspective of understanding linkages between domestic politics and foreign affairs.

INTERNATIONAL CONFLICT AND VIOLENT REGIME CHANGE

The fundamental question we wish to explore is whether states experience significant violent, internally motivated political changes following participation in international war and, if so, under what war-related conditions. Despite the dearth of broad-based empirical research, there are reasons to believe that a close connection exists between war and the fate of regimes. The ideas supporting this relationship (as mentioned) relate war involvement to regimes' accountability to the citizens they serve. Our consideration of this question takes place in several stages. First, we focus on the outcome of the war and the role of the participants in shaping this outcome. Then we move to an examination of the effects of war costs in influencing violent regime changes. We conclude with an investigation of the relationship be-

tween prior and current state experiences with violent regime change to ascertain whether the effects we observe are spurious or robust.

At the outset, we reason that a failed policy in the domain of international war will render a regime vulnerable to removal. A failed conflict policy can arise in several ways. We view all defeats in war as instances of failed policies. After all, war is extremely costly in lost life, property, and opportunities foregone. Consider that defeat in war almost always alters the loser's freedom of action, reducing the nation's autonomy over its own foreign policy or depriving the vanquished state of sovereignty over some portion of its citizens, territory, or national product (Morrow 1987). Evidence of such losses is likely to be more dramatic and more apparent to the domestic population than is evidence of the failure of economic policies, an area known to influence the survivability of regimes. Moreover, in nations in which an electoral system is not present, or is nonfunctional, war losses are abundantly obvious to members of the elite, who themselves may have both the opportunity and motive for replacing leaders.

Although fighting a war is sometimes warranted by beliefs about the prospects of success or even for purposes of building a reputation for toughness or steadfastness, still mounting *anticipated* costs heighten the impetus a regime faces to settle without fighting (Bueno de Mesquita and Lalman 1992; Wittman 1979). Fighting a losing war reflects upon a government's failure to anticipate adequately the costs being imposed on the nation. Such a failure is, we believe, likely to be punished; and this punishment is expected to increase with the costs of the war. This should be true whether the regime's leaders initiated the fighting or were the target of attack. A losing initiator regime is likely to be held accountable for its aggressiveness. A defeated victim who survives the wrath of the victor still confronts the prospect of punishment by its own citizenry for failing to cut losses and settle up earlier.

Victory in war is not as straightforward as defeat. An initiator who wins has generally gained some benefits for the state that apparently were not obtainable through other means, although the gains often come at a high price. Such an accomplishment is likely to prompt indifference or reward; it is unlikely to provoke punishment of the regime. But a victorious target faces a rather different circumstance. When the target of attack emerges as the winner, there is clear ex post evidence that the government's ex ante policy was a failure. Since an initiator is unlikely to attack unless it expects to do better by fighting than by not fighting, it is likely (though not inevitable) that the leaders of a defeated initiator state misjudged the nation's prospects. Such misjudgments are, to some extent, the responsibility of the loser—hence our hypothesis that defeat heightens the likelihood of punishment. But such misjudgments may also reflect a failure by the target to communicate adequately about its motivation and/or capabilities. Such failure to communicate must at least partially offset any domestic political benefits to be had as a result of ultimate success. Consequently, we hypothesize that victorious initiator governments gain the most benefits—victorious targets of aggression not doing as well as victorious initiators in terms of regime survival. Still, winning is better than losing; and this is expected to be true whether one is an initiator or the target.

We propose, then, the following hypotheses which, if supported, provide an empirical foundation for the claim that the political welfare of a regime is intimately tied to the foreign policy welfare of the state. Data supportive of these hypotheses provides evidence of an important linkage between domestic affairs and foreign policy choices, providing an important contrast to the classical, realist perspective:

HYPOTHESIS 1. *The chances of a domestically instigated, violent regime change increase with defeat in war.*

HYPOTHESIS 2. *Violent regime change is least likely for winning initiators; the likelihood of violent regime overthrow increases for winning targets, losing targets, and losing initiators in that order.*

HYPOTHESIS 3. *The chances of a violent regime change increase with the costs of war, irrespective of the nation's war outcome or initial conditions.*

We examine whether during a war or after its conclusion the leader *and* his or her *entire regime* is removed from office through violent means. By violent means we intend to cover revolutions, coups d'état, and the like but not (as noted) removal by the opponent state. Removal of an entire regime is a very drastic act that is not always easily accomplished and that when carried out successfully, often requires appreciable political mobilization. If regime change is a regular feature of major failures in national security policy, then the *expectation* of such a consequence is probably an important, domestically induced, pacifying element in international affairs, an element that helps account for the frequency with which states tolerate one another, rather than wage war (Keohane 1984).

For removal from office through violent means to be reasonably related to war experience, it should occur within a period from after the war's onset to a relatively short time after the end of the war. We define two alternative time frames as the relevant periods for consideration: (1) the period from the beginning of the war until three years after its end, and (2) the period from one year before the end of the war until one year after its end.

For our purposes, the first period, extending up to three years beyond the end of a war is taken as the germane, *extended* time frame for a war-related violent regime change to occur. We choose a three-year interval because it offers the opportunity for war costs and outcomes to be recognized by the population and for an opposition to form and act. Taking it further than three years, however, could possibly let nonwar phenomena enter the picture more strongly. If a leader's regime is removed during the war or within

three years of its termination, this seems to us as a reasonable case of war-related removal.

Two questions arise regarding this extended time frame. First, why did we use the entire period of the war? It is reasonable to suspect that policy failure will not necessarily be evident during the entire period of the war and that therefore we should look only at the period around the end of the war. For example, policy failure would not have been evident to Germans in 1940 but had become manifest by late July 1944. The problem with this is that we know of the end of the war post hoc. Those who forcefully overthrow a regime during wartime to change governmental policy have no way of knowing when the war will end. Second, why do we choose to extend our data into the third year after the end of a war? Do we not run the risk as the years pass beyond the end of the war of including violent regime changes having less and less to do with war experience and more to do with other possible variables? The answer to this is, of course, *yes*. However, any decision on a time frame will necessarily be arbitrary. We believe that an ample enough amount of time needs to pass so that the costs of a war can become apparent. After the end of the war but before fours years have passed, these costs are most likely to affect the victors. Thus, the time period will likely make the test of some of the hypotheses more stringent. The extended time frame has the virtue that it is unlikely to exclude relevant violent regime changes, but it has the limitation that it may include changes that were not related to the proximate war experience.

As a check on this extended time period we also evaluate our hypotheses in terms of a more *constrained*, second time interval encompassing violent regime changes that took place in the period from one year before the end of the war to one year after its end. In this way we can better evaluate the robustness of the evidence we uncover and assess whether it is consistent or inconsistent with our hypotheses. With this narrower time frame we mitigate the risk of including "irrelevant" regime changes; but, of course, we increase the chances of excluding violent regime changes that should have been included. Thus, each of our time frames offsets a limitation of the other, providing a more general basis for evaluating the reliability and robustness of the evidence we report.

THE DATA

Our data are relatively straightforward. The war data are taken from the well-known collection of the Correlates of War project reported in Small and Singer's (1982) *Resort to Arms*.[3] The data set not only reports on national involvement in all international wars between 1816 and 1975 with at least one thousand battle-related fatalities but also identifies the nations that initiated the wars and the eventual winners and losers.

We eliminate two categories of cases. First, we drop the cases in which the regime change is forced upon a state by its external enemies during the war or at its conclusion. This is consistent with our interest in domestically instigated violent regime change. For example, we do not include the new German governments that emerged in the years following the end of World War II or the removal of the Dutch government by the Germans during the war. Second, we exclude the cases in which there was no clear-cut winner (e.g., the Korean War). After removing these, our data set consists of 177 cases.

The data on violent changes of regime were derived from two basic sources. First, Banks's *Cross-Polity Time-Series Data* lists, by year, national attributes with respect to type of regime and number of successful forceful changes of government between 1815 and 1965 (1971, 3–53). These data were checked against the historical chronology given in Langer's (1952) *Encyclopedia of World History* and were also compared to information contained in Spuler's *Rulers and Governments of the World* (1977). Post-1965 data were gathered from *Facts on File* and Bienen and van de Walle's (1991) collection of data on political leaders.[4] The central question we asked was, Did there occur during a war or within the first three years after the end of a nation's involvement in an international war a forceful, irregular, domestically instigated change of its governing regime that resulted in the replacement of one elite by another? The identities of the cases of violent regime change we record are given in the Appendix.

In order to test Hypothesis 3, we need data on the costs of war. Measuring the costs of war directly is not a simple matter. The price of a war includes not only the immediate financial costs but needs to reflect opportunity costs as well. Although Organski and Kugler (1980) have been able to calculate some important dimensions of war costs for a few nations, we know of no data set that provides us a readily available measure for the number of nations with which we are dealing. However, an alternative measure is accessible in the war lethality data contained in the Small and Singer compendium (1982, 82–95). Small and Singer list for each nation's war participation the number of months it participated in the war, its number of battle deaths, its total population, its battle deaths per nation-month and its battle deaths per ten thousand population. This last measure is particularly attractive, since it is consistent across time and is insensitive to the size of a nation's population. Using battle deaths per ten thousand population permits us to make comparisons across nations and ask whether on average the winning targets who experienced violent regime changes suffered more deaths relative to their population than did the losing targets. Because these data are highly skewed at 11.98, we used a logarithmic transformation to reduce skewness considerably, to .95. In addition, it makes substantive sense to use the transformation because increasing battle deaths probably have a decreasing marginal impact that would otherwise be exaggerated (Jackman 1991).

DATA ANALYSIS

At the outset it is worthwhile to note that of the 177 war-participating nations in our data set, 32 (18.8%) experienced a violent change in their regime during the war or within three years of the war's end. It is difficult, however, to know how to evaluate this figure without some base against which to compare it. Is this 18.8% rate higher or lower than that experienced in nations that did not participate in war?

In order to estimate the effect of war on the rate of violent regime change, we drew a random sample of nation-years between 1815 and 1975, excluded any cases already in our war involvement data set and then determined whether the remaining nations experienced a violent regime change in the year drawn or in any of the three subsequent years.[5] We performed this procedure three times. The percentage rates of violent change in these samples were, from lowest to highest, 8.91%, 9.92%, and 10.21%.[6] The rate shown in the data of 18.8% indicates that war participation itself approximately doubles the chances of the regime's being violently overthrown by domestic opponents as a consequence of war participation.[7]

Our hypotheses, to which we now turn, predict significant variation across this base rate. With respect to hypothesis 1, what is the effect of winning or losing a war on the fate of the regime? A cross-tabulation of the data shows that there is a relatively strong relationship between losing a war and drastic changes in political leadership, with 29.5% (23/78) of the losers, and only 9.1% (9/99) of the winners, experiencing an violent regime change ($x^2 = 12.25$, $p < .01$). This is consistent with our first hypothesis and with the notion that failed national security policies provoke domestic political retribution.

Hypotheses 2 and 3 may be tested much more rigorously, since their predictions lead us to anticipate some relatively sharp differences in the data. Hypothesis 2 indicates that victorious initiators experience the lowest risk of violent regime change (i.e., in the probit analyses the relevant coefficient, b_1, is expected to be negative). The signs of the coefficients associated with the remaining variables from Hypothesis 2 (i.e., b_2, b_3, and b_4) can be positive or negative. In either case, b_2 and b_3 are expected to have magnitudes that fall between b_1 and b_4. This indicates that victorious initiators are less likely to suffer from violent changes in regime than are any other type of nation with a recent war experience, while victorious targets are less likely to suffer violent regime transfers than are defeated targets; and both are less likely to have such an experience than defeated initiators. Hypothesis 3 states that the likelihood of a violent change in regime increases as the costs of war increase, thereby leading to the prediction that b_5 will be positive.

In order to ascertain the overall relationship of these variables to violent regime change and to each other, we construct two probit models containing these ordered inequalities. The first model contains the effects postulated in Hypothesis 2 on the likelihood of a violent change in regime, and the second adds the costs of war to these variables. The expectation from the probit analysis is that violent regime change equals

$$-b_1\text{init. wins} \pm b_2\text{targ. wins} \pm b_3\text{targ. loses} + b_4\text{init. loses.}$$

Adding costs, it equals

$$-b_1\text{init. wins} \pm b_2\text{targ. wins} \pm b_3\text{targ. loses} \pm b_4\text{init. loses} + b_5\log(\text{battle deaths/population}),$$

where $b_1 < b_2 < b_3 < b_4$.

Table 1 displays the results of these probit models. The first column of the table shows the results without the inclusion of war costs. Here, everything is consistent with our expectations as to whether a regime endures more than three years beyond the end of a war or is violently removed in response to the nation's performance in war. The coefficients are all as predicted, with $b_1 < b_2 < b_3 < b_4$ in each probit analysis.[8]

How much of a difference do these factors make in the violent removal of a regime? Table 2 reports the cumulative normal distribution of the coefficients shown in Table 1, column 1; these are the translation of the probit coefficients into statement of probability of regime overthrow, given the contingent condition identified.[9] Thus, for losing initiators, the increment in the probability of violent regime change is .44. The other relevant incremental probabilities show losing targets half (.22) as likely to be violently removed as losing initiators and winning targets half again (.11). For winning initiators the probability is exceedingly small (.008).

As shown in Table 1, column 2 when the costs of war (in terms of the log of the battle deaths per ten thousand population) are added to the probit, the coefficients maintain the same order shown in column 1 and are fairly close to their original values. (However, note that the coefficients for the winners move closer together.)[10] It is tempting to look at interactions between battle deaths and the other variables, but there is a more revealing way of getting at the question.

Drawing upon the same method used to derive the results in Table 1, Figure 1 is a graph of the effect of levels of battle deaths on the probability of violent regime change given the four contingent conditions of interest. For the winners, as battle deaths increase, the chances of being violently overthrown nearly double. For losing targets, the chances of removal increase from slightly over .40 to just under .60; for losing initiators, the probability moves from just over .60 to slightly under .80. When compared to the effects of the contingent conditions themselves, it is evident that the effects of battle deaths are far from insignificant and increase the probability of removal by substantial amounts. For example (and most sig-

TABLE 1

Violent Changes of Regime: The Effects of Winning and Losing, Initiation, and Battle Deaths for the Extended and Constrained Models

	EXTENDED MODELS (N = 177)		CONSTRAINED MODEL (N = 171)
INDEPENDENT VARIABLE	(1)	(2)	(3)
Initiator wins	−1.26**	−1.16**	−1.27**
	(.351)	(.364)	(.393)
Target wins	−1.09**	−1.13**	−1.15**
	(.345)	(.349)	(.395)
Target loses	−.64*	−.57*	−.43
	(.311)	(.320)	(.333)
Log(battle deaths/population)	—	.14*	.19**
		(.072)	(.078)
Constant[a]	−.13	−.51*	−.73*
	(.242)	(.306)	(.328)
$\tilde{x}^2$ ($p < .001$)	17.03	21.26	23.66

[a]The constant is the baseline established by the cases of defeat for the initiator.
*$p < .05$, one-tailed.
**$p < .01$, one-tailed.

nificantly), battle deaths increase the probability of winning initiators being deposed violently from .008 to just over .20. However, it would be a serious mistake to focus only on the differences between this figure and the data in Table 2, because there is another aspect of the data in Figure 1 that merits discussion. Note that even when battle deaths are included, the contingent conditions remain of overwhelming importance. Thus, although the regimes of losing targets experience an increase in the probability of violent removal from about .40 to just under .60, the latter figure is *still below the starting point of losing initiators*. The same pattern holds in comparing the losing targets to the winning initiators and winning targets. In short, increasing battle deaths raise the probability of violent regime change; but the conditions specified in Hypothesis 2 are even more important.

The results of this analysis provide strong support for the hypothesis we originally offered about the effects of war outcomes on the political fortunes of the regimes that are the initiators or targets of international war. Clearly, regimes that initiate wars and do not prevail are at the highest risk of being replaced, while those who initiate and win are at relatively little risk. Our results with respect to the targets of international war are also as expected. All of this is true whether the costs of the war are included or not.

TABLE 2

Cumulative Normal Probabilities of Violent Regime Change

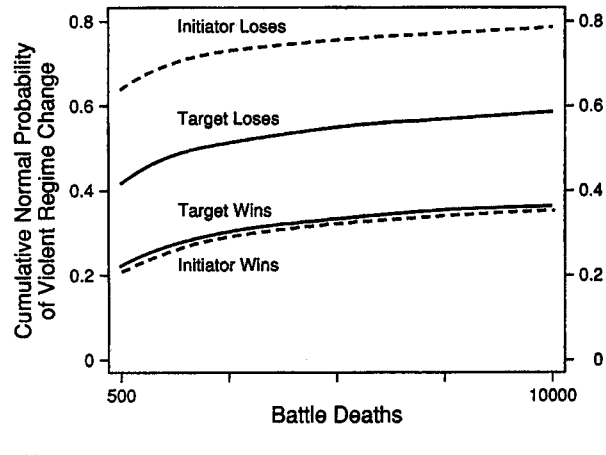
COEFFICIENT	CUMULATIVE NORMAL PROBABILITY
Initiator wins	.008
Target wins	.11
Target loses	.22
Initiator loses	.44

THREATS TO VALIDITY

Although our hypotheses are well supported, we pause to explore several threats to the robustness and validity of the results. Recall that the period over which we collected data on violent regime change extended from the onset of the war to three years beyond its end. As noted, it is reasonable to be

FIGURE 1

War Performance and the Probability of Regime Change



curious about the extent to which our findings would continue to obtain if this period were constrained to a much shorter one. For this purpose we remove from the data set all cases in which the violent change of regime took place more than a year before the end of the war and more than a year after the end of the war. This more limited time frame removes six cases from the data. The probit results from this more constrained data set are reported in Table 1, column 3. In terms of their sign, relative order, and statistical significance, the results shown here are extremely close to those obtained earlier, the main difference being that the effect on violent regime change of being a losing target is just beyond conventional standards of statistical significance (p = .095), although the odds are still better than 10 to 1 that the effect is not due to chance.

Another potential threat to the validity of our analysis derives from the fact that regimes and governments are always changing at some rate and that some nations are more prone to change than others. It is possible that to one degree or another, the changes we observe simply reflect a continuation of past governmental instability. After all, if a nation has experienced political instability in some period before its involvement in war, it is not unreasonable to think that irrespective of war involvement, some of that instability may be sufficiently embedded in the political system to account for the violent regime changes we observe better than does war involvement. To assess this possibility that the results are spurious, we constructed an index of prewar political stability for each of the 177 cases in our data. This was done by enumerating the number of successful coups d'état each case experienced in the 10 years prior to its war participation.[11] Interestingly, the results of this probit reveal that a relationship does exist between prewar political instability and violent regime change among the states participating in war (coefficient = .22, p = .027).

Given this, we need to assess the extent to which this estimated relationship is independent of the state's initial position in the war, its outcome, and its level of cost. It is not, after all, unreasonable to suspect that the regimes of unstable polities may be more sensitive to, for example, the costs of a war than regimes existing in generally stable environments. If so, then including our measure of prewar instability into our previous model could cause some of the already reported effects to be altered.

To investigate this prospective threat to the stability of the results, we include the prewar instability variable in the original full model of Table 1. Table 3 shows that the outcome and cost variables continue to have relatively the same estimated effects found in the earlier analyses even when the index of prewar instability is included. In other words, the political instability variable does *not* have a discernible impact on the weight of war outbreak, war outcome, or costs we estimated earlier. Our results continue to appear robust even when taking into account the impact of prior political instability.[12]

TABLE 3

The Effects of War Outcome, War Costs, and Prewar Instability on Violent Changes of Regime

INDEPENDENT VARIABLE	COEFFICIENT	STANDARD ERROR
Initiator wins	−1.26**	.380
Target wins	−1.16**	.353
Target loses	−.71*	.330
Log(battle deaths/population)	.15*	.074
Prewar coups	.27*	.125
Constant[a]	−.64*	.311

Note: N = 177, χ^2 = 25.91, f = .000.
[a]The constant is the baseline established by the cases of defeat for the initiator.
*p < .05, one-tailed test.
**p < .01, one-tailed test.

CONCLUSION

Our results demonstrate quite clearly that the outcome of war has a dramatic effect on the fate of the regimes involved. Our hypotheses set out the expected associations; and all were supported by the evidence with regard to irregular, forceful changes in regime. Although this relationship has not been previously established, some may not find the results surprising. However, what we have shown is not merely a relationship, but (1) an estimate of the *size* of the effect of war on violent regime change against a baseline and (2) the *specification and testing of quite specific, detailed hypotheses* describing the relationship of war, war costs, and regime stability to violent regime change.

Although broadly based in time and ranging across all types of political systems, the results are fully consistent with the claim that the political welfare of governmental regimes is directly tied to the performance of the nation in war. This conclusion casts interesting light on a controversial part of international relations theory. The evidence presented here can be taken as an evaluation of the claim that the political welfare of foreign policy makers is intimately tied to national performance in conflicts that pose a potential threat to sovereignty and that the preferences of such leaders are constrained so as to give highest priority to maximizing the state's overall welfare (Bueno de Mesquita 1981). Having observed that regimes are ex post punished for failed foreign policies, we propose an ex ante inference. In the context of models of rational action, behavior is strongly influenced by expectations about consequences of actions "off the equilibrium path"; that is, the choice of actions is shaped by beliefs about what would happen if some other action were chosen. Leaders can anticipate that they will be held accountable for failed foreign policy adventures. Consequently, the choice of war-related behavior is likely to be dampened by the fear that the regime will be punished if things go awry. Probably, the wars we do

observe are cases where leaders generally thought the foreign policy action would turn out well from the perspective of their regime. They thought the potential benefits from fighting outweighed the associated risks to the regime. In that sense, the true effects of war on regime change are probably stronger than the observed effects reported here. The true effects include instances of wars that did not happen because of the anticipation of domestic political punishment. We believe, therefore, that the results lend considerable credence to models that treat war/peace decisions as if they are the product of rational choices by a unitary actor whose preferences are partially shaped by the existence of a threat to the state's sovereignty or autonomy and who takes into account the distribution of preferences and interests of the constituent elements that make up the domestic affairs of the state. The results show that governmental leaders must attend to the domestic political ramifications of their decisions in choosing foreign policy actions. The standard realist account, which largely ignores domestic political considerations, is difficult to sustain in light of the evidence adduced here.

APPENDIX

Our cases of violent regime change and year of war onset are derived from Banks 1971, *Facts on File* for 1966–75, and Small and Singer 1982.

Nation	Year of War Onset
Spain	1823
Mexico	1846
France	1849
Argentina	1851
Columbia	1863
Paraguay	1864
Spain	1865
Peru	1865
France	1870
Peru	1879
Guatemala	1885
El Salvador	1885
Honduras	1906
Honduras	1907
Nicaragua	1907
Turkey	1911
Turkey	1912
Germany	1914
Greece	1914
Bulgaria	1914
Turkey	1914
Austria–Hungary	1914
Russia	1914
Hungary	1919
Greece	1919
Turkey	1919
Paraguay	1932
Italy	1939
Syria	1947
United Arab Republic	1947
Pakistan	1965
Cyprus	1974

Notes

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1. We emphasize that the cases to be examined in the analysis are those in which the regime is removed by its domestic opponents. Thus, regimes that fall by virtue of foreign imposition are not included in the data.

2. Despite the time that has passed since Stohl's review was completed, it probably remains the best overall treatment of the relationship between internal and external conflict.

3. We include the wars listed in the Correlates of War data set (Small and Singer 1982). The wars do *not* include events that are interventions in which a government is removed forcefully by another without sufficient resistance being present to reach the "war" threshold of one thousand battle deaths. For that reason such events as the disappearance of the Baltic Republics into the Soviet Union in 1939 and the German invasion of Denmark in 1940 are not included in the analysis.

4. Also available to us were Gurr's Polity II data. These, however, were not fully suited to our needs. Although these data are quite extensive with respect to the measurement of certain kinds of political change, they focus not on the regime (i.e., the members of the political elite who make state decisions) but on the much broader, underlying patterns of political authority and constitutional structure. Our interest is in the fate of the group of political leaders who take a nation into war and not necessarily on the overall pattern of state authority, although several such changes are recorded in the data. Thus, for example, although there have been many irregular, usually forceful, changes of regime in Mexico (Bienen and van de Walle 1991, report 28), the Polity II data record only four changes of polity. The Polity II data, it may be noted, also contain Banks's (1971) data on violent political changes on which we partially rely.

5. We chose this time period because the average length of a war in our data set was just over one year. Combining that year with the three years we allowed after the war ended yields a time frame comparable to that used in our analyses of forceful regime changes among states involved in war.

6. Excluding the nations in our data set for the years they fought and the three years after the war produces a data set of slightly more than three thousand nation-years of data. We drew 5% samples from these. While our data go to 1975, 10 years beyond the end of the data from which the samples were drawn, it is not clear that there were any marked differences in these omitted years.

7. Almost two-thirds of the violent changes of regime in the three samples are to be found in the nations of Latin America. This high rate of forceful regime change in Latin America is consistent with the findings of Bienen and van de Walle (1991, 86). It might be argued that the samples should be stratified by region to reflect the rate of change more accurately. Such a procedure would undoubtedly show that the effect of war participation on relevant regime changes in the non–Latin American nations is much *higher* than we show here. However, given the state of the data set from which the samples were drawn (i.e., no geographic code is present), it is difficult to implement this method. Moreover, it is unnecessary to our purpose, which is only to show that war participation makes a meaningful difference in the rate of violent regime change. We will approach another aspect of this question further on.

8. Because of the nature of the independent variables, it was necessary to leave one of them out of the estimation of the model. The effect of the omitted variable is then contained in the constant. However, to make sure that this procedure was not favoring our hypotheses, we computed the zero-order product moment correlation between violent change of regime and all the variables in the probit reported in Table 1, column 2. The results are perfectly consistent with the esti-

mated model and our hypotheses. The Pearson product moment correlations between violent regime change and our variables are *initiator wins* −.22, *target wins* −.06, *target loses* .01, *initiator loses* .33, and *log(battle deaths/population)* .27.

9. For a very accessible explanation of this technique, see Wolfinger and Rosenstone 1980, app. C.

10. We provide the cross-tabulation of predicted and actual outcomes based on the probit analysis shown in column 2. The model of regime turnover that we propose is not a full equilibrium model. As such, it does not take into account all of the factors that contribute to forceful, domestically instigated regime change. Consequently, the model is not expected to account for all—or even most—of such regime changes but, rather, to isolate conditions that systematically *increase the likelihood* of such change. The model's significance is best evaluated, then, in terms of the extent to which the probit coefficients are consistent with the expectations expressed in our hypotheses, rather than in how many cases were fully determined by the factors we address in our model. The cross-tabulation follows (pseudo R^2 = .11). The results are consistently robust in terms of the ordinality of the coefficients; the differences across coefficients approach but do not achieve significance.

	Predicted Outcome	
Observed Outcome	No change of regime	Change of regime
No change of regime	143	2
Change of regime	26	6

11. Data on successful coups d'état were taken from Banks 1971, segment 1, field E and (for years after 1966) from *Facts on File* for 1966–75. We would have preferred to use data on attempted coups d'état, as well as successful ones, but we were unable to find any broad data reporting such attempts.

12. Our analysis has treated all regimes as equivalent; but we know that they differ, among other things, with respect to the ease with which leaders may be replaced. Elites in some states may be forced to use violence to remove the regime because other alternatives are not readily available to them, while other states (i.e., democracies), may have well established, even institutionalized, methods for removing the political leadership. Further, the number of winning democracies in our data may distort some of our findings. To investigate this possibility, we estimated the probit contained in Table 1, column 3 after removing democracies. Although there are some modest changes in the coefficients (none larger than .15), the order of the coefficient values remains the same.

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