War Abroad and Homicides at Home: Evidence from the United States*

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We analyze why domestic homicide rates in a country sending troops into war increase with some international wars, but not others. Drawing from research on the brutalization effect, we first explain how war can have an effect on homicides through individuals learning acceptable behavior from the state. Second, we explain why we observe the brutalization effect with some wars, but not others. We argue that illegitimate wars are associated with increased homicide rates, while state participation in legitimate wars should not affect homicide rates. Pursuing an illegitimate war may serve as a signal to society that norms and morals have been suspended, leading to a period of moral deregulation in the form of anomie. To test our theory, we conduct time-series analyses of data for the United States between 1928 and 2014. After examining the characteristics of eleven international wars pursued by the United States, we find that a brutalization effect occurs when the country engages in illegitimate, but not legitimate, conflicts. We also examine the validity of several potential alternative explanations and provide directions for future research.

We analyze whether wars *fought abroad* affect post-war homicide rates *in the country sending soldiers overseas*. There is a reason to believe that such a connection exists: During the Vietnam War, the homicide rate in the United States more than doubled, from 4.5 murders per 100,000 inhabitants in 1963 to 9.3 in 1973. However, homicide rates in the United States increased in connection with some wars, but not others. For example, while homicides proliferated with the Vietnam War, they did not increase with the Korean War. Similarly, the U.S. military engagement in the Dominican Republic resulted in higher homicide rates, while operations in Haiti did not. Therefore, our research question is

This paper benefited from comments by Jeff Dumas, Clint Peinhardt, Todd Sandler, Richard Scotch, and Sheryl Skaggs. We thank the conference participants at the American Society of Criminology in New Orleans, LA, and the International Society for Research on Aggression in Sydney, New South Wales, Australia for helpful comments. Nina Barbieri, Leslie Stanaland, and Anh Pham Thi Cam provided excellent research assistance. All errors are ours.
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Social Forces 1–25
doi: 10.1093/sf/soy100

straightforward: Why do homicide rates in the United States increase with some wars, but not others?

Our theory suggests a conditional effect of international war on domestic homicide rates. In a first step, we explain why international wars can have an effect on domestic homicide rates by drawing on research examining the brutalization effect (Cochran and Chamlin 2000, 2005; Shepherd 2005). This effect suggests that individuals "learn" from the state about acceptable behaviors (Bowers and Pierce 1980; Cochran and Chamlin 2000). As with capital punishment, we argue that the declaration of war can, under certain conditions, suspend cultural norms against violence. As wars carry the full authority of the state, they may lead to a process of the legitimization of violence, which allows individuals to infer that violence is acceptable behavior.

In a second step, we explain why homicide rates increase only with some international wars. We draw on concepts such as social currents and anomie to explain under which conditions the state can no longer provide moral guidance to individuals, with crime and violence as a potential consequence (Bernburg 2002; Dicristina 2016; Messner and Rosenfeld 2012). We argue that the brutalization effect—and consequently, higher homicide rates—occur only with illegitimate wars: In contrast, homicide rates do not increase with legitimate wars.[1] Thus, it is not government-sponsored violence per se that might inspire domestic homicide, but the observation of unjustified violence by the state.

We test our argument by analyzing a long time-series (1928–2014) for a single country (the United States). The United States has participated in several wars, resulting in a comparatively large sample. The characteristics of these wars differ, which allows us to analyze if the effect of war on homicide rates differs across wars. Our analysis of eleven international wars provides support for our argument that only illegitimate wars increase domestic homicide rates, while participation in legitimate wars does not.

In addition, we examine the validity of three alternative hypotheses. Homicides may increase with wars involving many veterans, while those with few returning soldiers do not have an effect. Similarly, homicides could increase if soldiers are exposed to violence abroad, but may remain unaffected by wars with comparatively few casualties. Finally, it might only be unpopular wars that lead to higher homicide rates, while popular wars do not show such an effect. However, our analysis suggests that these alternative explanations do not explain the patterns in homicides observed in the United States.

International War and Domestic Homicide Rates

A key component of modern states is their monopolization over the legitimate use of physical force in order to protect citizens from internal and external threats. Upholding law and order is a significant task. In some cases, states utilize the death penalty in hopes of deterring potential future offenders. Early research supported this notion that death penalty lowers homicide rates (Ehrlich 1975, 1977). More recently, economists have identified deterrent effects from

capital punishment in a subset of US states (Shepherd 2005) and for certain types of homicides (Shepherd 2004).

However, other researchers have pointed out that several states applying capital punishment have experienced an *increase* in homicide rates (Cochran and Chamlin 2000; Cochran, Chamlin, and Seth 2005; Neumayer 2003; Shepherd 2005). In search for a causal mechanism explaining an increase in the homicide rates, scholars have identified the *brutalization effect:* An execution may send a signal to potential offenders that lethal violence is a legitimate option (Bowers and Pierce 1980), thus negating any potential deterrent effect. Some empirical evidence supports the existence of a brutalization effect. Researchers analyzing within-country variation (Cochran and Chamlin 2000; Cochran, Chamlin, and Seth 2005; Shepherd 2005) as well as cross-national variation (see Neumayer 2003) conclude that abolishing the death penalty should lead to lower homicide rates. While general consensus about the deterrence effects of capital punishment remains inconclusive (Nagin and Pepper 2012), scholars have begun to examine these divergent findings more carefully.

As Neumayer points out, the mixed results might be a result of endogeneity issues: "the positive association between the two variables [death penalty and homicide rates] might be because high homicide rates prompt policymakers to introduce the death penalty in order to fight violent crime" (Neumayer 2003, p. 635). Thus, the causal arrow might not point from punishment to homicide rates, but from homicide rates to punishment. Endogeneity may thus explain the mixed evidence concerning an effect of capital punishment on homicide rates.

We address this problem of endogeneity by examining an alternative measure of government force that might also cause homicide rates to increase: International war.[2] War is frequently undertaken to protect one's own citizenry from external threats. As such, participation in war might have the same effects as governments' use of force to deter internal threats. However, in contrast to the death penalty, it is unlikely that participation in international war is affected by endogeneity concerns: Wars are not declared in reaction to domestic crime problems.[3]

A small literature examines the effect of wars fought abroad on homicide rates at home. Some scholars find that war increases homicides: Gurr (1989) shows that violent crime rates in the United States surged with both World Wars and the Vietnam War. Bebber (1994) draws similar conclusions after analyzing four military engagements by the United States in the 1980s. However, others dispute that wars increase homicides. For example, Reckless (1942, p. 328) notes that "it should not be expected that war has had or will have a marked direct effect on a problem such as crime." A third set of scholars details that wars increase homicide rates in some instances, but not others. For example, Archer and Gartner (1987) find that U.S. participation in World War I increased domestic homicide rates while participation in World War II did not. The mixed findings in previous research may be the result of the differing methodological approaches: Gurr based his conclusions on the raw homicide rate numbers over time. In contrast, Archer and Gartner compare the mean homicide rates five years prior World War II to the mean homicide rates five years after World War

II. However, they could also be the result of heterogeneous patterns within the data suggesting a conditional effect of wars.

Against this background, our task is three-fold: First, we analyze multiple wars fought by a single country with an identical methodological approach using data whose variable definitions have not changed over time. This approach ensures that findings suggesting a conditional effect are due to heterogeneous patterns in the data, while ruling out mixed findings due to changes in variable definitions or inconsistent methodology. Second, we introduce an improved conceptual understanding of why wars fought abroad might affect domestic homicide rates. Our starting point is the brutalization effect introduced above: As with capital punishment, we argue that individuals "learn" about acceptable behavior from a state's involvement in international war. We argue that international war leads to increased homicide rates only if the war was illegitimate; in contrast, homicide rates are unaffected by legitimate wars. Third, we use a research design that allows us to adequately test our hypotheses. Here, we analyze long time-series data available for the United States as this approach can account for issues of endogeneity, spatial spillovers, and temporal dynamics in the data.

Why Would International War Affect Domestic Homicide Rates?

As noted above, scholars have explained the increase in homicide rates associated with executions by the brutalization effect. We argue that this effect also applies to international war: In both instances, states use violence, which can signal acceptable behavior to individuals. However, we argue that the brutalization effect occurs only with some wars, but not others.

Durkheim's concept of anomie has been influential in understanding how social institutions, including the government, can impact criminal behavior (Bernburg 2002; Dicristina 2016; Messner and Rosenfeld 2012). The starting point is the idea that culture is held together by a set of shared norms and values. In situations when those shared values break down, social institutions may no longer be capable of providing moral guidance to individuals. The resulting process of moral deregulation, known as anomie, can then create an environment more permissive to crime and violence.

We argue that wars waged abroad can also lead to situations where society provides little moral guidance to individuals. In times of peace, states condemn violence as immoral and impose severe punishment on citizens who commit murder. However, these customary judgments of violence are reversed during times of war when murder—in the form of soldiers killing enemy fighters—is viewed as necessary. Oftentimes states intentionally honor these acts of violence. The killing of enemy soldiers is treated not as a regrettable measure, but instead as praiseworthy. Archer and Gartner (1987, p. 66) note that "Wars reward killing in the sense that some war "heroes" are decorated and lionized, often in direct proportion to the number of homicides they have committed." This

change is also reflected in the language used by states. "The governments in whose name wartime killings are done are likely to refer to these deaths using neutral terms like "casualties," "body counts," or simply "losses." [. . .] These euphemisms for violent death are preferred by authorities because the terms killing or murder connote the kinds of illegal violence for which punishment is generally administered" (Archer and Gartner 1987, p. 65).

This change in states' behavior shapes individuals' perceptions of legitimate actions. We see two possible transmission mechanisms. First, citizens' view of the legitimacy of governmental institutions might be affected. War waged by states might signal a legitimization of force to citizens since wars carry the full authority of the state. Thus, violent acts legitimized by the state "can provide a model or script for the postwar acts of individuals, increasing the likelihood of imitative violence" (Archer and Gartner 1987, p. 66). In other words, individuals "learn" from the state about acceptable behaviors (Bowers and Pierce 1980; Cochran and Chamlin 2000). In this sense, governmental institutions that typically regulate societal life are de-legitimized and thus cannot control individual behavior as tightly.

Second, observing war might affect individuals' perceptions of societal norms that typically govern inter-individual behavior. Post-war homicide rates might increase because the legitimatization of violence reduces inhibitions against taking another's life. Mannheim (1955) argues that changes in social norms and social breakdown contributed to increased violence in Great Britain after World War II. Lunden (1963) also reports crime increases post-World War II, blaming increased social disorganization. In response to homicide increases with the Vietnam War, Tanter (1969) suggested that war facilitates "a state of 'normless-ness' in which traditional structures against criminal acts lose their effectiveness" (Tanter 1969, p. 436).

However, we do not suggest that international wars always lead to the breakdown of societal norms or the de-legitimization of institutions. Rather, we propose a conditional effect of participation in international war on domestic homicide rates. We argue that the brutalization effect—and consequently, higher homicide rates—occur only with illegitimate wars. In contrast, homicide rates do not increase in connection with legitimate wars because norms of legitimate violence are not affected. Thus, it is not government-sponsored violence per se that might inspire domestic homicide, but the observation of illegitimate violence.

This relates to Durkheim's concept of social currents (Durkheim 1938). Such currents describe certain transitory states of the collective mind. He argues that these social currents can have a powerful influence over individuals' actions: "The great movements of enthusiasm, indignation, and pity in a crowd do not originate in any one of the particular consciousnesses. They come to each one of us from without and can carry us away in spite of ourselves" (Durkheim 1938, p. 4). We argue that the characteristics of the war will affect the type of social current that surrounds times of war. In particular, we suggest that illegitimate wars produce a social current conducive to the brutalization effect. In contrast, the social current surrounding legitimate wars does not legitimize violence in the

private realm. As a result, citizens do not perceive killing in these just wars as a signal that "anything goes."

> Hypothesis 1 *Domestic homicide rates should increase only with illegitimate wars, while homicide rates should not be affected by legitimate wars.*

The argument can be illustrated by contrasting World War II and the Vietnam War. The former was viewed as a legitimate war effort with a worthy goal of restoring peace and stopping genocide, while the latter was the target of much political dissent and viewed as unjust. Therefore, we would expect homicide rates to increase with the Vietnam War, but not World War II.

Data and Method

We analyzed a long time-series for a single country (the United States) over an extended period (1928–2014) to test our hypothesis. This approach had a number of advantages. First, homicide victimization rate data were available for the United States for the entire period. The definition of these data has not changed over time, making them useful for dynamic analyses. Second, control variables were available over the same period, allowing us to account for observable changes in the U.S. economy, polity, and society. In addition, focusing on a single country over time allowed us to control for unobservable time-invariant characteristics. Further, the fact that the United States is geographically isolated and did not experience war within its own borders in the past century helps avoid spatial spillovers that may bias the analysis. In addition, studying a country on whose territory no wars were fought during the time period under consideration avoids the need to differentiate between deaths classified as homicides and deaths in the context of war. Fourth, the United States has engaged in multiple wars over this period of time. Importantly, these wars have had different characteristics, where some were viewed as illegitimate while others were not. This variation allowed for examining how different types of war might have a diverging impact on homicide rates.

Data

Dependent Variable: Homicides: We chose homicides as our measure of violence in a society. We do not suggest that it is the only violent crime that may be affected by anomie. However, given the severity of this type of crime, we obtain a more conservative estimate of our theory than if we were to focus on lesser crimes. Consequently, we analyzed the total homicide victimization rate: The number of people murdered per 100,000 inhabitants in a given year.

We used data from the National Vital Statistics System (NVSS), available from 1928 to 2014.[4] These data are likely accurate as deaths count as murder following an investigation by a coroner or medical examiner (Cantor and Cohen 1980).[5] Further, as the data are drawn directly from death certificates, there is little room for ambiguity with respect to separating out a homicide from an

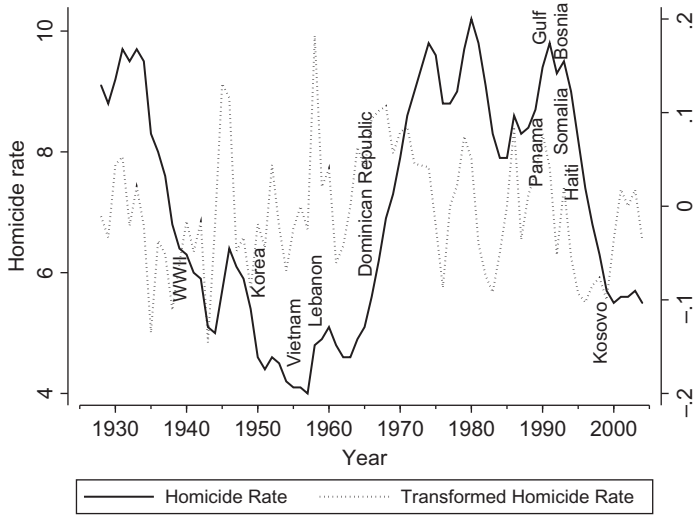
attempted homicide. Lastly, the data were available for a long time period without definitional changes.

Figure 1 shows homicide victimization rates from 1928 to 2014. We calculate the first difference to account for prior trends in the data. Augmented Dickey-Fuller, KPSS, and Phillips–Perron tests confirmed that the first difference was sufficient to correct for non-stationarity. In addition, we took the natural logarithm of each series to stabilize the variance and to facilitate the interpretation of elasticities.

Independent Variable: International Wars: We included every major act of war abroad in which U.S. troops were involved. This definition excludes some conflicts for two reasons. First, the focus on U.S. troops excluded events such as the Bays of Pigs invasion of Cuba in 1961, as it was conducted by Cuban exiles and not U.S. forces. While the group was supported by the CIA, the attempted military overthrow of the Cuban government occurred without the participation of U.S. troops.[6] Second, methodological reasons prevented the inclusion of more recent wars. Time series models require a sufficient number of observations to be available after the intervention. Our data end in 2014. For this reason, we could not analyze ongoing interventions (see Afghanistan) or recently concluded interventions (see Iraq, which ended in 2011).

We coded intervention variables to analyze the effect of international wars on domestic homicide rates. In this process, we made three modeling decisions. First, the interventions were coded to begin with U.S. entry in a war, rather than the official beginning of the war. We expect international wars to affect domestic

Figure 1. Homicide rates and International Wars.



Note: Figure displays the untransformed homicide victimization rate in the United States as well as the start dates of international wars pursued by the United States. In addition, the figure compares the untransformed homicide victimization rate to the same data after transformation. The graph shows that after differencing and logging the variable, the data appear stationary.

homicides only if the U.S. government is directly involved. While the dates of the official beginning of a war and the U.S. entry into the war were typically identical, this was not always the case. For example, World War II began in 1939, but the U.S. did not officially enter the war until 1941. Our intervention variable, therefore, codes 1941 as the start of U.S. engagement.[7] Second, we applied the same reasoning to the coding of the end of the intervention period. Lastly, we coded the intervention variables as linearly increasing functions as we expect the effect of wars on homicide rates to increase with the duration of the wars.[8]

Differentiating Between Legitimate and Illegitimate Wars: It is impossible to directly measure a war's degree of legitimacy. Instead, we used three alternative, albeit imperfect, measures to *proxy* whether a war was legitimate. Our first approach focused on judgments by an external, but internationally respected, authority. A vote by the United Nations (UN) Security Council or General Assembly can serve as a strong signal of legitimacy as it provides social cues by an actor with moral authority. Existing research shows that UN votes exert a strong effect on the opinions and behavior of U.S. citizens. For example, Kreps and Wallace (2016) found that references by United Nations to drone strikes violating the sovereignty of target states are associated with a drop of between 6 percent and 8 percent in approval. Both Wallace (2013) and Tomz (2008) showed that respondents are far more likely to oppose policies that would violate international law than to oppose otherwise identical policies that do not violate rulings by the UN. However, this approach has the disadvantage that the UN was founded only after World War II. Further, despite the evidence by Kreps and Wallace (2016), Wallace (2013), and Tomz (2008), questions remain how strongly U.S. citizens are aware of UN votes.[9]

We addressed these shortcomings with our second measure. We examined how U.S. media reported on the legitimacy of a particular war. Editorials in major newspapers are likely to capture the knowledge, perception, and judgments of citizens. For each war in our sample, we coded editorials referencing the war published in two newspapers—the left leaning *New York Times* and the right-leaning *Wall Street Journal*. In total, we analyzed 722 opinion articles. We used the proportion of editorials that deem a war legitimate as our second measure of a war's legitimacy. Insofar as newspapers reflect and/or influence the opinions of their readers, coding the two newspapers with the largest readership is likely to capture the population's view on the legitimacy of wars.[10] Our third approach to proxy whether a war was legitimate or not examined the share of foreign aid the United States provides to the enemy country after the war. The international community is likely to share the costs of rebuilding a nation if it views a war as legitimate. In contrast, other donors are unlikely willing to pay for the destruction caused by an illegitimate war, leaving the United States to foot the majority of the bill. Following this reasoning, we argue that a lower share of foreign aid provided by the United States, relative to the contributions of other western donors, is a proxy for a war's legitimacy.[11]

Table 1 summarizes the three measures. The classification of most wars is uncontroversial. For instance, in the case of World War II, Korea, Gulf War, Somalia, Bosnia, and Kosovo: (a) the UN resolutions suggest they were

Table 1. Differentiating between legitimate and illegitimate wars using three different measures

	UN Resolutions by judgment		Count of Newspaper Articles			% of total aid provided by U.S.	Classification
	Legitimate	Illegitimate	Legitimate	Neutral	Illegitimate		
World War II	28, 95		60	60	4	No data	Legitimate
Korea	82, 83, 84		68	51	10	No data	Legitimate
Lebanon		129	0	12	22	No data	Not legitimate
Vietnam		189	6	80	70	84.2%	Not legitimate
Dom. Rep.		156, 203	4	8	0	99.6%	Not legitimate
Panama		44–240	11	10	3	93.5%	Not legitimate
Gulf War	660, 661, 662, 664, 665, 666, 667, 670		26	33	1	38.8%	Legitimate
Somalia	751, 767, 775, 794, 47–160		26	23	13	28.9%	Legitimate
Haiti	841, 875, 917, 940, 49–27, 49–201		16	13	17	59.7%	Legitimate
Bosnia	816, 819, 942, 958, 47–121, 48–88		21	15	3	14.7%	Legitimate
Kosovo	1239, 1244, 54–183		20	11	5	11.7%	Legitimate

legitimate wars; (b) number of articles classifying these wars as legitimate clearly outnumbers the number of articles suggesting that these wars are illegitimate; and (c) the share of total aid provided by the United States to rebuild the country is low, indicating that other countries viewed this war as legitimate. Conversely, UN votes, newspaper articles, and foreign aid data offer a consistent classification of Vietnam and Lebanon as illegitimate wars. For three wars, however, the three measures do not perfectly align: In each of these cases, two measures agree while the third does not. For instance, based on our measures using UN votes and foreign aid data, we would expect that the military operations in Dominican Republic and Panama would be illegitimate, while Haiti would be a legitimate war. However, the newspaper counts do not mirror these judgments.[12] For example, eleven articles suggest that Panama was a legitimate war while only three argue it was not. Similarly, seventeen articles suggest that Haiti was an illegitimate war while only sixteen suggest it was legitimate. In these three cases, our classification follows the two measures agreeing with each other.[13] As a result, U.S. interventions in Vietnam, Lebanon, Panama, and the Dominican Republic are consistently classified as illegitimate wars, while all other military campaigns are judged legitimate.

Control variables: Existing scholarship points to a large number of factors with the potential to explain trends in U.S. domestic homicide rates. While we would like to control for all of these factors, our methodological approach—analyzing the time series of a single country between 1928 and 2014—only offers eight-seven observations. Therefore, including too many control variables would result in an overfitted model with possibly inaccurate estimates. We reviewed the existing literature and identified the relevant control variables. Based on our reading of the literature, we controlled for the most prominent factors affecting homicide rates.[14]

First, we accounted for economic factors. We controlled for the real income per capita, as existing research suggests that individuals with better economic prospects should commit fewer homicides (Piliavin et al. 1986). Further, we accounted for the unemployment rate, as scholars have argued that the lack of legitimate jobs may cause individuals to resort to illegitimate means for survival (Fajnzylber, Lederman, and Loayza 2002a). We also controlled for inflation, as rising prices might increase societal discontent, resulting in higher homicides. Second, we accounted for demographic characteristics, as existing literature points out that younger individuals are overrepresented among offenders (Neumayer 2003). We consequently included the proportion of individuals aged 15–24 in the total population. Third, we accounted for issues of governance. We controlled for the welfare assistance per capita provided by the U.S. government as individuals' incentive to engage in violent crimes might be moderated if a social safety net is available (Messner and Rosenfeld 1997). In addition, the external security environment of the United States changed over time. Citizens' perception of a war's legitimacy might be shaped if it occurred in the context of the Cold War. We accounted for this by including a dummy set to one between 1947 and 1991, and zero in other years (Mitchell and Moore 2002). Lastly, as

each of the models studies the effect of a single war in isolation, we controlled for the possible effect of the ten remaining wars on homicide rates.

Method

When analyzing the relationship between international war and homicide rates in the United States, some studies merely report descriptive statistics of homicide rates over time without statistical analysis. For example, Bebber (1994) simply points to year-to-year changes in murder rates. However, these approaches did not account for pre-existing trends in homicide rates. Furthermore, they were unable to incorporate control variables accounting for factors besides war that could also affect homicide rates. Other work analyzed homicide rates for multiple countries using panel fixed effects regressions to account for unobserved heterogeneity across countries. However, these studies suffered from the comparatively short-time periods for which cross-nationally comparable data are available. For example, the data analyzed by Pampel and Gartner (1995) ranges from 1951 to 1986; Bennett (1991a, 1991b) from 1960 to 1984; Fajnzylber, Lederman, and Loayza (2002b) from 1965 to 1995; Fajnzylber, Lederman, and Loayza (2002a) from 1970 to 1994; and Neumayer (2003) from 1980 to 1997.

In contrast, we conducted time-series analyses. This methodological approach had three advantages: First, it allowed for incorporating information from prior homicide trends into the analysis. After all, this year's homicide rates are not completely independent from last year's homicide rates. Auto-Regressive Integrated Moving Average (ARIMA) models can account for such prior trends: When estimating the value of y_t, ARIMA models can incorporate the effect of its previous values, y_{t-1}, y_{t-2}, etc., also known as "autoregressive" terms ρ. For example, if the estimated ρ for y_{t-1} was 0.30, then the current value of the series would be related to 30 percent of its value 1 period ago. Incorporating various combinations of these lags significantly improves model precision. In addition, moving average parameters θ relate what happens in period t to the errors involved in estimating the effect for past time periods, i.e., E_{t-1}, E_{t-2}, etc. In other words, ARIMA models incorporate information on how well the model explains past periods into the estimation of current periods, thereby accounting for the fact that imprecise estimates for yesterday's value should affect the precision of today's estimates. Accounting for these effects significantly improves the accuracy of results in comparison to cross-country regressions. Furthermore, this approach focuses on within-country variation in homicide rates, which allows holding country characteristics constant while analyzing the variation in the characteristics of wars. These considerations suggested that a time-series approach is conceptually and methodologically more appropriate for our research question than cross-country regressions.

Formally, we estimated a series of ARIMAX models to analyze the U.S. homicide rate in terms of a linear combination of independent variables, as well as an ARMA disturbance process. This methodology accounted for the dynamics of

our dependent variables' own history while statistically controlling for our chosen covariates. For example, an AR(1) MA(1) process would be represented by

$$\begin{aligned} y_t &= x_t\beta + \mu_t \\ \mu_t &= \rho\mu_{t-1} + \theta\varepsilon_{t-1} + \varepsilon_t \end{aligned} \tag{1}$$

where ρ is the first-order autocorrelation parameter, θ is the first-order moving-average parameter, and ε_t is a white-noise disturbance.

Combining these equations yields the general form of the ARMA model:

$$\begin{aligned} y_t = x_t\beta + \rho_1(y_{t-1} - x_{t-1}\beta) + \rho_2(y_{t-2} - x_{t-2}\beta) + \cdots + \rho_p(y_{t-p} - x_{t-p}\beta) \\ + \theta_1\varepsilon_{t-1} + \theta_2\varepsilon_{t-2} + \cdots + \theta_q\varepsilon_{t-q} + \varepsilon_t \end{aligned} \tag{2}$$

We estimated the models with robust standard errors to account for the possibility of heteroskedastic or non-Gaussian errors.

Selecting an appropriate model was an iterative process. Consequently, for each war, we estimated all combinations of AR processes with zero to three lags as well as MA processes with one to three lags. We report only the model with the best fit, as judged by two criteria: First, and most importantly, we examine model fit by presenting those models that minimize Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) statistics. Second, if models are still comparable, we chose more parsimonious lag structures over more complicated models. We analyzed the final models to ensure that the residuals are white-noise.

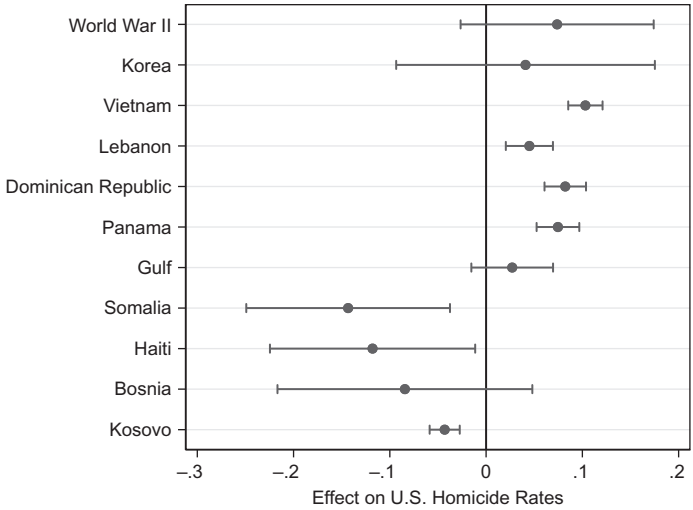
Findings

Figure 2 displays the effect of different international wars on the domestic homicide rate in the United States. The figure summarizes the substantive findings derived from eleven *separate* models, one for each war.[15] The figure illustrates that some models had a statistically significant and positive coefficient associated with the military intervention while others did not. For instance, the Vietnam War and the U.S. invasion of Panama were associated with an increase in homicide rates in the United States. In contrast, World War II and the Korean War did not have a statistically significant effect on homicide rates. Additionally, the U.S. interventions in Haiti, Somalia, and Kosovo corresponded to reduced homicide rates.

This heterogeneity supports the notion that international wars do not always increase domestic homicide rates, and thus points to a conditional effect: International wars increase homicide rates under some conditions, but not others. Hypothesis 1 suggests that domestic homicide rates should increase only in connection with illegitimate wars, while legitimate wars should not create the conditions that lead to increased homicide rates.[16]

To examine whether the results presented in figure 2 support our theory, table 2 reorganizes the information by classifying wars as either legitimate or illegitimate following the discussion above. The table illustrates that the effect of

Figure 2. Effect of International Wars on U.S. Homicide Rate.



Note: Effects of different international military engagements conducted by the U.S. government abroad on the domestic homicide rate. This figure summarizes the findings of eleven separate models, one for each war. While the point estimates and confidence intervals are not directly comparable, the figure illustrates that some models have a statistically significant and positive coefficient for the intervention while others have an insignificant (or even negative) coefficient. This indicates that some international wars are associated with an increase in the domestic homicide rate, while others are not.

Table 2. Effect of international wars on domestic homicide rates, classified by legitimate versus illegitimate wars. Results indicate that homicide rates increase only in response to illegitimate wars, not legitimate wars

	Effect on homicides
Illegitimate Wars	
Vietnam	⇑
Lebanon	⇑
Dominican Republic	⇑
Panama	⇑
Legitimate Wars	
World War II	...
Korea	...
Gulf War	...
Somalia	⇓
Haiti	⇓
Bosnia	...
Kosovo	⇓

illegitimate wars on domestic homicide rates differs markedly from that of legitimate wars: All wars classified as illegitimate—Vietnam, Lebanon, Dominican Republic, and Panama—were associated with statistically significant increases in the domestic homicide rates. In contrast, the seven wars classified as legitimate did not increase homicide rates. The evidence provides strong support for our theory that the brutalization effect occurs only in response to illegitimate, but not legitimate, wars.

Interestingly, three of these seven legitimate wars—Somalia, Haiti, and Kosovo—were associated with a slight decrease in homicide rates. We can speculate as to why this is the case. One possibility pertains to the fact that they were explicitly multilateral efforts backed by international organizations. For example, in Somalia, the United States fought under the umbrella of the U.N. peace efforts, while the war in Kosovo was a joint effort by all NATO partners. In contrast, other legitimate wars such as Korea and the Gulf War in the 1990s were largely undertaken by the United States alone.

Also, note that the coefficients of control variables were typically of the expected sign and significance.[17] For example, unemployment rate and inflation were both positively associated with homicide rates, suggesting that economic stress does lead to violence. As predicted, higher welfare state assistance was associated with lower homicide rates. Interestingly, welfare assistance per capita is only significant in relation to wars we considered illegitimate. This would provide further evidence in favor of differentiating between legitimate and illegitimate wars. Contrary to expectations, however, the share of the youth population was statistically insignificant.[18]

Alternative Explanations

As shown in figure 2, significant heterogeneity exists with respect to the effect of international war on domestic homicide rates. Our theory explains this heterogeneity with reference to the legitimacy of different wars. Once wars are classified as either legitimate or illegitimate, a consistent pattern of effects emerges (see table 2). However, alternative explanations could also explain the heterogeneity in effects across wars. We examine their plausibility in the following sections. These alternative explanations are evaluated using the same models as those estimated for the main analyses presented above.

Domestic Support for a War

Citizens might not differentiate between legitimate versus illegitimate wars, but between popular versus unpopular wars. Legitimacy and popularity are distinct concepts: The former refers to the moral dimension of judging wars, while the latter alludes to citizens' perception of what is in a nation's material and geopolitical interests.

For example, the U.S. invasion of Panama received a high degree of public support as citizens recognized the geopolitical importance of the Panama Canal. At the same time, citizens understood that the invasion was not morally justified:

Journalist John McWethy stated on national television "As far as international law is concerned, even sources in the U.S. government admit they are operating very near the line."[19] Using experiments, Tomz (2008) confirms that individuals differentiate between moral and material interests: He shows that in cases where the material interest for violating international law is sufficiently strong, large proportions of voters and policymakers will advocate breaking the law, even if they know that such action is morally wrong.

If citizens respond to the popularity—not legitimacy—of an international war, we would expect the brutalization effect to occur only with unpopular wars: When the government uses violence against the will of the citizens, individuals might follow this example.

> Hypothesis 2 *Domestic homicide rates should increase only with unpopular wars, while homicide rates should not be affected by popular wars.*

To test this alternative explanation, we refer to public approval of wars. Some wars were viewed extremely positively. For example, public support for U.S. involvement in World War II was strong and positive (Moskos 1971). According to a Gallup poll conducted in July 1958, approximately 54 percent of respondents approved of U.S. intervention in Lebanon (Shiraev and Zubok 2012). This conflict proved the first of several interventions with the intention to install allied leaders in foreign countries, most of which were viewed favorably by the public. For example, opinion polls showed high approval for the invasion of Panama. A LA Times poll reported that 77 percent of respondents favored the action against Panama. Gallup polls find that support for the Gulf War was high, with 78 percent of respondents in favor of the intervention in Iraq. Regarding Somalia, Baum (2004) argues public support rose significantly after the initial military engagements. Approval remained high until the end of the war, when public approval fell following the "Blackhawk Down" episode. Military action in Kosovo was supported by the moral obligation to prevent ethnic cleansing in Serbia. Over time, support for involvement became more muddled, although opinions remained positive due to fewer troop sacrifices (Larson and Savych 2005).

While classifying these wars as popularly supported is largely uncontroversial, the classification of Korea is less straightforward. According to Gallup, public support was high (78 percent) when President Harry S. Truman sent U.S. ground troops into Korea on 30 June 1950. However, support proved volatile as the war went on: Approval fell to about 40 percent in both 1951, but increased towards the end of the war, with 50 percent were in favor, and only 36 percent opposed, in 1953. Considering the favorable approval at the beginning and the end of the Korean war, we chose to classify this war as one primarily viewed favorably, though we acknowledge the variability in public opinion over time.[20]

Other wars were seen primarily in a negative light. For example, public opinion questioned the legitimacy of the Vietnam War for the majority of the conflict (Bouffard 2005). In particular, atrocities committed by soldiers against the Vietnamese people, increasing racial conflict in the military, and the inequality

of the draft resulted in negative domestic public opinion (Segal 1989). Similarly, military involvement from the United States in the Dominican Republic was criticized. It followed escalating fears of the safety of U.S. citizens in the area. Many Americans felt unsure about troop involvement at the time, with 89 percent citing either no opinion or disapproval of U.S. involvement (Shiraev and Zubok 2012). Support for military action in Haiti was low as well. While the intention of restoring democracy was consistent with U.S. foreign policy goals, a USA Today poll reported that only 40 percent of the public were in favor, with 48 percent opposed. The most difficult conflict to classify is the U.S. intervention in Bosnia. Prior to the deployment of troops to Bosnia, many Americans favored military intervention. However, public approval rates fell dramatically after deployment, with many Americans feeling little was accomplished (Larson and Savych 2005).

H2 suggests that wars viewed in a positive light will exhibit no brutalization effect. In contrast, wars without public approval should be associated with an increase in homicide rates. Table 3 examines the validity of these predictions. Differentiating between wars with high and low approval ratings does not result in consistent patterns. For example, both Panama and Kosovo were viewed in a positive light by the public; the military intervention in Panama resulted in an increase in homicides, while Kosovo did not. Conversely, both Vietnam and Haiti were viewed in a negative light by the public but the homicide rate increased with the former but not the latter. In sum, the observed patterns do not support this alternative explanation.

Table 3. Effect of international wars on domestic homicide rates, classified by popular versus unpopular wars. Results indicate no consistent patterns of effects exist across these classifications

	Effect on homicides
High Domestic Approval Rating	
World War II	...
Korea	...
Lebanon	⇑
Panama	⇑
Gulf War	...
Somalia	⇓
Kosovo	⇓
Low Domestic Approval Rating	
Vietnam	⇑
Dominican Republic	⇑
Haiti	⇓
Bosnia	...

Returning Veterans

A second alternative explanation focuses on wartime exposure to violence that may resocialize soldiers to be more accepting of violence. As a consequence, returning veterans would be more likely to commit violent crimes, resulting in a higher homicide rate. Following World War II, scholars argued that some soldiers had developed an "appetite for violence" (Hamon 1919, p. 355) during combat. Abbott (1918: 39) writes that "War does not develop the virtues of peace [. . .] It is not a school that teaches respect for the person or property of others. Men return from war with a new outlook and habits of violent and forceful acts." According to Rosenbaum (1939, p. 722), their confusion is understandable: "In war, behavior formerly called criminal, i.e., killing, is now considered good and commendable. When the war is over and man returns to the every-day civil competition of life, it is inevitable that war-ethics should have left their mark upon him."

Scholars have made similar arguments about more recent wars. Following the Vietnam War, scholars have noted the correlation between war service and higher levels of violence (Boulanger 1986). Lifton (1970, p. 32) notes that "Some are likely to seek continuing outlets to a pattern of violence to which [Vietnam Veterans] have become habituated, whether by indulging in antisocial or criminal behavior or by offering their services to the highest bidder."

Evaluating this alternative argument requires data to test whether returning veterans are more likely to commit homicides than non-veterans. Unfortunately, data on the identity of perpetrators of homicides are not available for the period of our study. Therefore, we cannot directly differentiate between murders committed by veterans versus civilians. However, the number of troops deployed and the number of veterans returning might be a reasonable proxy: If this explanation is accurate, wars involving a larger number of troops and/or veterans should be followed by higher post-war homicide rates.

> Hypothesis 3 *Domestic homicide rates should increase only with wars with large troop deployments (and thus many returning veterans), while homicide rates should not be affected by wars with small troop deployments.*

Table 4 provides information on the number of troops deployed in each war as well as the number of returning veterans. If H3 is accurate, we would expect wars with high troop deployment or a large number of returning veterans to increase homicide rates. However, this is not the case: Three of the four wars with the highest number of troops and veterans—World War II, Korea, and the Gulf War—are not associated with an increase in homicides. Furthermore, when examining wars with similar levels of troop deployment or returning veterans, no consistent pattern emerges: Despite similar numbers, Lebanon and Panama saw an increase in homicide rates, while Haiti saw a decrease in homicides. A similarly contradicting pattern is visible among wars with low number of troops and returning veterans: The military intervention in the Dominican Republic was associated with a higher

Table 4. Effect of international wars on domestic homicide rates, classified by wars with high versus low troop deployment and returning veterans. Results indicate that no consistent patterns of effects exist across these classifications

	Effect on homicides	Deployment	Returning veterans
High			
World War II	···	16,112,566	15,707,167
Korea	···	1,789,000	1,752,484
Vietnam	⇑	3,403,000	3,344,791
Gulf War	···	694,550	694,022
Intermediate			
Lebanon	⇑	17,739	17,733
Panama	⇑	12,719	12,679
Haiti	⇓	17,495	17,491
Bosnia	···	15,002	14,990
Low			
Dominican Republic	⇑	2,296	2,249
Somalia	⇓	6,345	6,302
Kosovo	⇓	6,410	6,392

homicide rates, while those in Somalia and Kosovo did not. In sum, the evidence does not support this alternative explanation.

Exposure to Violence in War

Alternatively, one might suggest that it is not about the number of soldiers deployed, but the degree of violence that soldiers were exposed to. After all, soldiers might have been deployed but serve in locations far removed from battle sites. In contrast, soldiers that saw atrocities with their own eyes might reach a breaking point that changes either their own behavior, or the stories and narratives they share upon return. While we do not have data on the personal experiences of individual soldiers, the degree of violence of a war might be a reasonable proxy. If the number of soldiers or civilians killed is comparatively high, returning veterans might be more likely to commit crimes.

> Hypothesis 4 *Domestic homicide rates should increase only with highly violent wars, while homicide rates should not be affected by less violent wars.*

Table 5 provides data on the number of deaths for the wars under consideration. We emphasize that these numbers are estimates. While the data on the number of U.S. soldiers killed are comparatively reliable, the information on the number of enemy fighters and civilians killed are less so. Also, the reader should note

Table 5. Effect of international wars on domestic homicide rates, classified by wars with high versus low degrees of violence as measured by casualties. Results indicate that no consistent patterns of effects exist across these classifications

	Effect on homicides	Enemy fighters killed	Civilian deaths	U.S. fighters killed	Risk of death for U.S. soldiers (%)
High death toll					
World War II	···	8,000,000	4,000,000	405,399	2.5
Korea	···	400,000–750,000	2,500,000	36,516	2.0
Vietnam	⇑	444,000	627,000	58,209	1.7
Gulf War	···	20,000–35,000	5,100	528	0.1
Bosnia	···	57,701	38,200	12	0.1
Low death toll					
Lebanon	⇑	2,000	Unknown	6	<0.0
Dominican Republic	⇑	2,825	Unknown	47	2.0
Panama	⇑	23	200–3,000	40	0.3
Somalia	⇓	500	Unknown	43	0.7
Haiti	⇓	200	Unknown	4	<0.0
Kosovo	⇓	2,500	12,000	18	0.3

that the number of enemy fighters killed includes all kills by members of the U.S. alliance, not only those caused by U.S. soldiers. Similarly, the number of civilian casualties is the result of actions by all war participants, not only American troops. While measurement error undoubtedly affects these numbers, they nevertheless provide estimates of the relative magnitude of violence to which returning U.S. soldiers were exposed.

Conceptually, it is unclear whether enemy, United States, or civilian deaths most accurately capture the degree to which U.S. soldiers were exposed to violence. For our purposes, we present information on all three types of deaths, as they appear to be correlated. In addition, we proxy the exposure to violence by calculating the risk of a U.S. soldier being killed by dividing the number of U.S. soldiers killed by the number of troops deployed. Should H4 be accurate, we would expect wars with a high number of deaths to lead to a subsequent increase in homicide rates.

However, the data presented in table 5 do not support this explanation. World War II, Korea, and the Gulf War were among the wars with the highest casualties, yet none of these wars were associated with an increase in homicide rates. In contrast, a number of wars with comparatively few casualties (Lebanon, Dominican Republic, and Panama) result in increased homicide rates. Inconsistent findings occur even within each category: In both Vietnam and

Korea a significant number of enemy fighters were killed, yet the former resulted in an increase in homicide rates while the latter did not. Conversely, in both Lebanon and Kosovo few soldiers were killed, yet the former resulted in increased homicides while the latter did not. In sum, the data do not provide evidence in favor of this hypothesis.

Summary of Robustness Tests

We also conducted a battery of robustness tests, which are presented in their entirety in the online appendix. For example, we examine the robustness of our findings to different sets of controls variables: First, Section 3.2 of the online appendix re-estimates our model with additional controls, at the risk of estimating an overfitted model. Second, in Section 3.3, we use factor analysis to combine control variables and subsequently use the resulting factors as controls instead. Third, Section 3.4 accounts for a particular risk group: Young, unemployed men. Fourth, we account for the moral fabric of the traditional society, as measured by the prevalence of divorces (see Section 3.5). In each of these cases, our findings are robust to the inclusion of additional control variables.

In addition, we varied the treatment of wars other than the one under investigation. The analysis above estimates the effect of a single war, which is why we control for the possible effect of the ten remaining wars on homicide rates. Section 3.7 shows that our findings are robust even if we differentiate between legitimate and illegitimate other wars. Further, our findings do not depend on how we control for other wars, as the effects exist even if we do not control for other wars at all.

Lastly, we examine whether the findings are robust to changes in the definition of the intervention. For instance, the analysis above assumes that the intervention period stops with the withdrawal of U.S. troops. However, it is conceivable that media coverage of the war will not cease immediately after a war, and soldiers may return home sometime after the war has been officially concluded. Thus, the intervention period might need to be extended for several years after the war's end. Section 4.1 in the online appendix shows that extending the intervention period by up to five years does not significantly affect the findings. Furthermore, Vietnam was by far the longest war the United States has fought abroad. Since the American public took little notice of this war initially, it is possible that Vietnam only exerted an effect after it came to the average person's attention following memorable events. Section 4.2 shows, however, that the effect of the Vietnam War on homicides is consistently positive, irrespective of varying the start date of the Vietnam War.

Conclusion

In this paper, we examine why homicide rates increase with some international wars but not others. We develop a theory explaining under which conditions a country's participation in international war increases domestic homicide rates.

Our argument suggests that domestic homicide rates increase only with illegitimate wars, while homicide rates should not be affected by legitimate wars. Our empirical strategy relies on time-series analysis of data for the United States between 1928 and 2014. Examining the characteristics of eleven international wars in which the United States participated, we find that a brutalization effect occurs after the country engages in illegitimate, but not legitimate, conflict.

These findings have significant normative implications: Conventional wisdom suggests that engaging in illegitimate wars carries substantial moral costs. In addition, illegitimate wars also appear to have a tangible human cost in the form of increased domestic homicide rates. Even if wars are fought abroad, our results show that such wars may also kill citizens at home. These findings should provide powerful insights for governments that are concerned for their citizens' well-being, but are also contemplating the use of force abroad.

Future research could expand on our findings in several ways. For example, it would be worthwhile examining whether our findings for the United States are generalizable to other countries. One candidate for replication might be the UK given its long colonial history offers a similarly large set of wars with varying characteristics. Similarly, once more data are available, researchers could extend our analysis into the past or future, examining the effect of World War I or Iraq, respectively. Second, further work is needed to refine the conceptualization of legitimate versus illegitimate wars. While we offer three distinct approaches to measuring the degree of legitimacy—UN votes, coded newspaper editorials, and the share of post-war reconstruction expenses—we acknowledge that these are imperfect measures.

In addition, future work could investigate how increasing globalization may moderate the brutalization effect. This could operate in two ways. On the one hand, globalization may contribute to news more easily spreading from one country to another such that citizens may "learn" about acceptable behavior from a foreign state. On the other hand, globalization may increase access to news, resulting in stronger learning effects.

Lastly, scholars could apply our argument to the micro-level to explain crime rates. War is not the only way the state exerts authority and the federal government is not the only way in which state authority can be interpreted. Since 2015, several major cities have experienced an increase in homicide rates. It is possible that similar "learning" effects also operate on the city and community level, and that these effects differ depending on the type of interactions between public authority and individuals.

Notes

1. We spent considerable effort to create three variables capturing the legitimacy of a war. See pp. 12–14 for more information.
2. Note, we are not using international wars merely as an instrument. Rather, our theoretical contribution is to identify a different instance of state-sanctioned violence, other than the death penalty, that is unaffected by endogeneity concerns.

3. We recognize that Marxist approaches might disagree with us. If crime increases due to a flawed economic system that disenfranchises the working class, wars could serve as a distraction from the increasingly shaky capitalist system. However, we argue that this perspective is unlikely to apply to the United States as international wars have been fought both during "good" as well as "bad" economic times. Further, our analysis controls for the state of the economy by including real income per capita, the unemployment rate, and welfare assistance per capita, which may act as a proxy for the tensions within a capitalist system.
4. Considering the starting point of the time series, we are unable to analyze World War I, which ended in 1918. There are data available for earlier time periods, but they suffer two significant drawbacks. First, the definition of homicides differs from our time series, preventing us from combining the data without creating a structural break. Second, the quality of data for time periods prior to the establishment of government statistics offices are questionable.
5. An alternative source of data, the Uniform Crime Reports (UCR), are based solely on the responding officer's initial assessment of the crime, not based on the outcome of an investigation.
6. For similar reasons, we exclude U.S. activities in Grenada, where a total of 165 U.S. soldiers provided support for military operations conducted by domestic actors.
7. A full list of wars is available in Section 5 of the online appendix.
8. Note that some wars lasted less than a full year; yet, we are forced to code such instances with "1" for the respective year: We have only yearly, not monthly, data available for most covariates, effectively requiring the year as the unit of analysis.
9. Additional details on the views of U.S. citizens about the UN are available in Section 1.1 of the online appendix.
10. Details about the sample and coding process are available in Section 1.2 of the online appendix.
11. See Section 1.3 of the online appendix for details on data and measurement.
12. It is noteworthy, however, that very few newspaper articles were published that concern these wars. As a result, the classification is based on a small sample size, which may affect the accuracy by which they capture the general mood in the country.
13. In the case of Haiti, the U.S. aid to Haiti was inflated due to emergency assistance provided in the aftermath of Hurricane Gordon on 12 and 13 November 1994. It was responsible for killing 1,122 people, rendering 87,000 people homeless, and affecting 1.5 million people in total. The available information suggests that of the $5.4 million in total aid provided by the U.S. to Haiti in 1994, $3.5 million were humanitarian emergency aid drawn from the International Disaster Assistance Account and the Office of U.S. Foreign Disaster Assistance (OFDA) disaster travel fund.
14. See Section 3 of the online appendix for robustness tests involving alternative sets of control variables.
15. The numerical results upon which this figure is based are available in Section 2.1 of the online appendix.
16. ARIMA models must be specified for each war individually. For this reason, it is impossible to incorporate war characteristics as a control variable. To deal with this, we classify each war and then examine whether the results yield consistent patterns across types of war.
17. See Table D in the online appendix.
18. See Section 3.4 in the online appendix for additional robustness tests that define the risk group as young, male, and unemployed (as opposed to merely young). Our findings are robust to this change.

19. John McWethy, ABC World News Tonight, 5 January 1990.
20. Note, that the assessment of H2 does not depend on the classification of the Korean War.

Supplementary Material

Supplementary material is available at *Social Forces* online.

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