A Test for Reverse Causality in the Democratic Peace Relationship*

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Several studies have suggested the possibility of reverse causation in the 'democratic peace' relationship: that the well-known extreme rarity of wars between democratic nations may be partially or wholly explained by a negative impact of war on democracy. Three kinds of war-on-regime effects are discussed. *Anterior* effects are regime changes that occur in preparation for wars; *concurrent* effects are those that occur during the course of a war; and *posterior* effects are regime changes that occur after a war concludes. Because studies have shown that democratic nations are rarely, if ever, on opposite sides in wars at their start, it is argued that reversed causation may affect the presence of causation from democracy to peace only if nations tend to become more autocratic as they prepare for impending wars. This proposition is examined with the observation of war events involving geographic neighbors or major powers, worldwide, from 1816 to 1992. With interrupted time-series analysis, it is found that nations are about as likely to become more institutionally autocratic as they are to become more democratic in the periods before the onset of wars. Moreover, this pattern holds even for the smaller subset of nations estimated to be democratic in the periods before major wars. These results indicate that studies of regime type and war participation have not been underspecified due to possible reverse causation before the onset of wars, and thus support the notion that the direction of causation in the democracy and war relationship is unidirectional from democracy to peace.

The Democracy–Peace Nexus

Empirical research has firmly established that democratic nations very rarely engage each other in war, and this pattern is highly

unlikely to be attributable to chance. While democracies do clearly engage in wars against other states, the absence of wars between democratic nations has attracted a great deal of attention in recent years, generating dozens of published articles and at least three special symposia in leading journals. This attention is warranted, as the 'democratic peace' offers the remarkable prospect that a world of democratic nations may be a world at interstate peace. The impact of democracy on war has become even more relevant in the wake of the Cold War and

the consequent diffusion of democratic government around the globe.

While numerous studies have engaged in introducing and examining potential explanations for the democratic peace (e.g. Bueno de Mesquita & Lalman, 1992; Dixon, 1994; Maoz & Russett, 1993), its policy implications (e.g. Ray, 1997; Russett, 1993), and wider aspects of democratic foreign behavior (e.g. Mousseau, 1997, 1998a; Simon & Gartzke, 1996), a seemingly growing number of studies have alluded to the possibility of reverse causation in the democratic peace relationship (e.g. Gates et al., 1996; James et al., 1999; Midlarsky, 1995; Thompson, 1996; Wolfson et al., 1998). The notion of endogeneity means simultaneous causation: that war has a negative impact on democracy while, at the same time, democracy has a negative impact on war. If this second aspect of causation exists in the democracy and war relationship, then previous studies of regime type and war participation have been underspecified. This means that the effect of democracy on war may be less than the current evidence suggests, and the possibility exists of full 'reverse' causality: that a negative impact of war on democracy may explain the democratic peace (Layne, 1994: 44–45).

We believe the notion of reverse causality should not be taken lightly. If the path of causation is unidirectional and runs from peace toward democracy, then all the attention given the democratic peace is unwarranted, and the prospect of interstate peace through global democratization is an idealist's dream. Given the dramatic implications of the democratic peace, it is therefore appropriate to consider all room for doubt in the relationship, including the possibility of reverse causality. However, we are also aware that if a thesis is stated often enough it may become perceived as true, with or without evidence. Particularly

disturbing is the prospect that continuing references to the possibility of reverse causality in the empirical literature may falsely render the democratic peace as inconsequential in policymaking circles. As Chan recently stated, 'it is important for the research community to take seriously the possibility of reverse causality' (Chan, 1997: 84). We agree with Chan, and undertake this study to examine the viability of the 'reverse causality' hypothesis concerning the democratic peace.

As will be discussed in this article, it is the prospect that nations tend to autocratize as they prepare for wars that poses the plausible challenge to the presence of causality from joint democracy to peace. We thus employ an interrupted time-series analysis to estimate the impact of impending war on the regime status of nations. This method allows us to assess the degree to which wars affect political changes before their onset, apart from the impact of such political changes on war involvement. Our aim is thus limited to examining the reversed arrow of causation from war to autocracy. If we find that wars do affect governing institutions before their onset, then further study will be needed to assess the degree to which this reverse direction of causation may affect the impact of democracy on war onsets. The article begins with a survey of recent challenges to the democratic peace and the prospects for reverse causality, followed by a review of our research design which is aimed at examining the presence of causality from war to autocracy.

The Reaffirmation of the Democratic Peace

The extreme rarity of joint democratic war, after a century of democratization across the globe, has made it statistically improbable that the democratic peace has occurred by

chance.[1] This pattern appears to hold after considerations of multifarious factors that may render the relationship spurious (Bremer, 1992a, 1993a; Maoz & Russett, 1993; Raknerud & Hegre, 1997). The consequence of these and earlier findings has been a resurgence of theories on how regime status may affect patterns of interstate militarized conflict, drawn mostly on ideas introduced two centuries ago by Immanuel Kant. Dixon (1994) and Maoz & Russett (1992, 1993), for example, associate democratic institutions with cultural norms that are less amenable to the use of force. Bueno de Mesquita & Lalman (1992, ch. 5) implicitly build on Kant's notion of how institutional structures may constrain democratic leaders from resorting to war. The policy implications are obvious: if democratic states are less likely than others to engage each other in militarized conflict, then a world of democracies would be a world with less interstate militarized conflict.[2]

While the reverse notion of war affecting democracy (Gates et al., 1996; Layne, 1994; Wolfson et al., 1998) raises questions regarding the importance of the democratic peace on theoretical grounds, several empirical challenges have also arisen. Primarily, empirical challenges to the democratic peace

have focused on limitations in the broad-based statistical analyses that have initially reported its significance. Since the hypothesis pertains to militarized conflict between nations, examinations of the proposition have observed interstate pairings, or dyads of nations. The number of dyads in any given year equals $[N \times (N - 1)]/2$, where 'N' equals the number of independent nations. For example, in the year 1992, the Correlates of War (COW) Project identifies 181 independent nations, yielding 16,290 $[181 \times (181 - 1)]/2$ interstate interactions. However, the COW Interstate War data, from which studies at the war level typically rely, identify only 71 wars to have occurred worldwide from 1816 to 1992. Since most interstate wars are fought among only a small number of nations, the probability of a war opposition in any dyad-year is very small. In consequence, tests of the hypothesis impose the comparison of very small latent probabilities between the test and control group, with the result that, 'narrow spatial and/or temporal domains provide us with a very weak basis for drawing conclusions about who fights whom' (Bremer, 1992a: 310).

The observation of dyad-years across time, however, brings with it the vexing problem that the likelihood of war (or peace) in any dyad-year may be dependent on the likelihood of war (or peace) in previous dyad-years. This dilemma was acknowledged in the original pathbreaking large-N studies (Bremer, 1993a: 240; Maoz & Russett, 1993: 631–632). If the probability of militarized conflict in one year is affected by the presence (or absence) of militarized conflict in previous years, then the assumption of independence is violated, rendering possibly misleading results (Beck et al., 1998).

In an insolent critique of the empirical findings, Spiro (1994) drew on this temporal dependence as a rationale for separately

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[1] Studies have confirmed that democratic nations are less likely than chance would predict to engage each other at two key stages of interstate conflict: the onset of or involvement in militarized disputes between nations, and the onset of or involvement in interstate wars (e.g. Bremer, 1993a; Gleditsch & Hegre, 1997). The former occurs when at least one state threatens, displays or uses force against at least one other state (Jones et al., 1996: 169–171). The latter are a small subset of militarized interstate disputes that escalate to over 999 battlefield-connected deaths (ibid). Numerous studies, including this one, use the term 'democratic peace' to refer to the lower than average probability of both wars and militarized disputes between democratic nations (e.g. Chan, 1997; Oneal & Ray, 1997; Oneal & Russett, 1997; Thompson & Tucker, 1997a,b). For additional clarity, we use the term 'militarized conflict' to refer to the combined set of militarized disputes and wars.

[2] Notwithstanding the possibility that the process of global democratization may be wrought with *more* militarized conflict than otherwise (Gleditsch & Hegre, 1997).

analyzing each year from 1816 to 1980. Given the rarity of interstate war, however, it is not surprising that Spiro found in most years the likelihood of war in joint democratic dyads to be not significantly different from that in other dyads. As Russett pointed out, Spiro's year-by-year tests do not solve the problem of duration dependence, but they do serve to 'make it mathematically impossible to reject the hypothesis of no relationship' (Russett, 1995: 170–171).

With a methodology similar to that of Spiro, Farber & Gowa (1995) dissected the 1816–1980 temporal domain into five separate time periods. The authors found that before the Cold War democratic nations were not significantly less likely than other states to engage each other in wars or militarized interstate disputes. From this, the authors concluded that the democratic peace is a function of common security interests among democracies during the Cold War. As Thompson & Tucker (1997a) make clear, however, the Farber & Gowa approach affords a wide array of potential explanations for their results.

One explanation is the rare-event problem that besets Spiro's (1994) analyses. It is not surprising that Farber & Gowa found the democratic peace significant only during the Cold War era, as this period provides 63% of their observations and has the highest proportion of democratic nations. The remaining 37% of their observations were divided into four separate periods in which, inconsequentially, they report the democratic peace as not significant. If Farber & Gowa (1995) wish to examine the hypothesis that the democratic peace may be a function of common interests among democratic nations during the Cold War, then the way to do it is not to dissect the temporal domain, but to model their hypothesized process directly (Thompson & Tucker, 1997b: 463).

One way to address the rare-event problem is to increase the number of events – not by having more wars, but by observing militarized interstate disputes short of war. Bremer (1993a) found joint democratic dyads to be about 30 times less likely than other dyads to originate war, and about three times less likely to originate militarized interstate disputes. Since wars are a small subset of militarized interstate disputes that escalate to sustained and prolonged violence, wars and disputes are causally related. Given that there were about 2,000 disputes from 1816 to 1992 and only 71 wars (Jones et al., 1996: 167), the observation of disputes furnishes greater inference potential degrees of freedom – than does the observation of interstate wars. Farber & Gowa (1995) notwithstanding, by taking into account militarized disputes rather than wars, several studies have affirmed the democratic peace as significant before the onset of the Cold War (Maoz, 1998; Oneal & Russett, 1999; Thompson & Tucker, 1997a).

What about duration dependence rendering inefficient estimates? Recent pioneering studies by Raknerud & Hegre (1997) and Beck and his colleagues (Beck & Jackman, 1998; Beck et al., 1998) have addressed this thorny problem by incorporating duration dependence into their statistical models. Beck and his colleagues treat temporal dependence as a latent variable in the dyad-year framework, while Raknerud & Hegre treat spatial and temporal dependence as a 'diffusion of war' effect modeled in continuous time using Cox regression. Either way, these studies report the democratic peace to be robust at the level of both militarized interstate disputes (Beck & Jackman, 1998; Beck et al., 1998) and interstate wars (Raknerud & Hegre, 1997).

Consequently, the democratic peace – both at the level of disputes and wars – appears empirically stronger than ever. Given the widening diversity of the accumulated studies the probability has grown increasingly remote

that there is not an association between democracy and peace at the dyadic level. Still, a seemingly growing challenge persists. Can we infer causation from democracy to peace and rule out the possibility of a reverse causality? The proposition that war destabilizes democratic institutions is as old as the field of international relations itself (e.g. Wright, 1964). A reversed arrow of causation suggests that previous models at the war level may be underspecified, and thus the impact from democracy to war may be less than previously understood (Gates et al., 1996; Midlarsky, 1995; Thompson, 1996). That is, if there is a simultaneous relationship between war and democracy, then the negative impact of war on democracy may serve as an omitted variable, making the actual impact of democracy on war much less then the accumulated evidence seems to suggest (James et al., 1999; Oneal & Russett, 2000). At the extreme, with simultaneity exists the specter of full reverse causality: that a negative impact of war on democracy may account for the entire impact of democracy on war (Layne, 1994: 44–45). It is to the inference of causation that we now turn.

Reverse Causality and the Democratic Peace

In his seminal study of war, Quincy Wright asserted that 'the constitution . . . of a state results from the interaction of internal and external conditions' (Wright, 1964: 156). While externally secure states can afford the luxury of decentralized government, warmaking 'encourages and often rewards more authoritarian approaches to resource mobilization and decision making' (Thompson, 1996: 144). As Layne put it, 'some states become democracies . . . because there is no imminent external threat that necessitates a powerful governmental apparatus . . .' (Layne, 1994: 44–45).

There is some evidence that external threats increase the probability of war and decrease the probability of democracy in a nation. A clear indicator of external threat facing a nation is the number of countries with which it shares borders. It is well known that geographically bordering nations are more likely than other pairings of states to engage each other in war (Bremer, 1992a, 1993a; Vasquez, 1993). At the same time, with 'minimized' external threats measured with numbers of sea borders, Midlarsky (1995) reports that as external threats increase, the probability of democracy in a nation (as measured in political rights) decreases. Taken together, these findings suggest that bivariate studies of democracy and war are underspecified; that an impact of external threats on both the independent and response variables may render the regime type and war relationship spurious. However, these findings do not impose the presence of simultaneous causation in the democracy and war relationship. If environmental, regional or systemic conditions of threat affect both regime structures and the probability of war, then the trick is to identify these conditions and model them on the right-hand side of the equation.

In this way, several studies of democratic peace have gone to great lengths to account for external threats in the relationship. Studies have controlled for such indicators of threat as geographic contiguity, relative power, power status, and the residual effects of previous wars (Bremer, 1992a, 1993a; Gleditsch, 1995; Maoz & Russett, 1992, 1993; Raknerud & Hegre, 1997). While we cannot be certain that the theoretical constructs and measures used in these studies adequately model external threats, the state of the evidence thus far indicates that a relationship among the independent variables, i.e. conditions of threat and regime status, does not affect the impact of dyadic democracy on the probability of war.

When considering how war itself may have an impact on democracy, it is useful to differentiate among three separate kinds of war-on-regime effects: what we call 'anterior', 'concurrent', and 'posterior' effects. Anterior effects are regime changes that occur before the onset of a war. Concurrent effects are institutional changes that occur during a war, and posterior effects occur after a war is over, in the long-term constitutional make-up of nations.

Starting with posterior effects, if war has a negative impact on the postwar regime status of nations, then nations frequently involved in war are more likely, *ceteris paribus*, to be autocratic. In this case, democratic nations may be more peaceful than other nations by default, and this may explain (partially or wholly) the democratic peace. There is some evidence for posterior effects at the level of leadership incumbency: it seems that war involvement has a negative impact on the survival rate of political leaders, particularly if they are democratic (Bueno de Mesquita & Siverson, 1995). The evidence is less clear, however, at the level of institutional structures. Bueno de Mesquita et al. found war participation, particularly losing in war, to lead to 'forceful, irregular, domestically instigated change of its governing regime' (Bueno de Mesquita et al., 1992: 641).

We are unaware, however, of any evidence that these postwar regime changes favor autocratization over democratization. If democratic states tend to win the wars they are in (Stam, 1996), then posterior regime changes are more likely to affect autocracies than democracies. Indeed, defeat in war may just as easily lead to democratization (as it did for Germany, Japan, and Argentina), and war mobilization may even *contribute* to postwar political development (Thompson, 1996; Tilly, 1992). At the systemic level of analysis it appears that war has a *positive* impact on the development of democracy (Mitchell et al., 1999).

While the evidence is less clear at the national level of analysis where regime changes actually occur, a negative posterior impact of war on democracy is not a likely explanation for the democratic peace. In terms of war *involvement,* the democratic peace appears to be primarily a dyadic rather than monadic level phenomenon (Gleditsch & Hegre, 1997).[3] If war involvement favored posterior autocratization, and this explained the democratic peace, then democratic states should be less likely than other states to be involved in wars. While the evidence is stronger that democratic nations are less likely than other states to *originate* wars, or be involved in wars when they start (Bremer 1992a, 1993a; Gleditsch & Hegre, 1997: 293), democracies are also more likely than other states to join wars (Bremer, 1992b; Gleditsch & Hegre, 1997: 294; Raknerud & Hegre, 1997) and join militarized interstate disputes (Mousseau, 1998b, ch. 6). Given the evidence, if war involvement has a negative posterior impact on regime structures, then the proportion of democratic states in the international system should be shrinking rather than growing. Consequently, if there is a posterior impact of war on democracy, the more likely explanation seems to be that offered by Thompson (1996): war involvement *favors* democratization (Tilly, 1992), and joint democracy reduces the likelihood of dyadic war opposition – a cause and effect pattern supported by the findings of Mitchell et al. (1999).

Concurrent effects are institutional changes that occur during the course of a war. It does seem that 'societies in war have

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[3] Countervailing studies include Benoit (1996), who analyzed the relatively short period of 1960–80. At the same time, war origination and involvement are not to be confused with war *intensity*. Some studies have found that democratic states are less likely to engage in wars or militarized interstate crises that involve large numbers of battle-deaths (Hewitt & Wilkenfeld, 1996; Rummel, 1995; Siverson, 1995).

often restricted citizen rights and freedoms' (Gates et al., 1996: 5). The US government, for example, suspended the right to trial during the American Civil War, interned citizens of Japanese descent during World War II, and imposed some restrictions on free speech during the Cold War. Similarly, during World War II, the leaders of both major political parties in Great Britain agreed to form a grand coalition government and postpone elections until after the war.

Still, restrictions on civil liberties and election postponements are not necessarily the same thing as broad institutional changes toward autocracy. Studies of democratic peace rely almost exclusively on coarse, institutional definitions of democracy that encompass a wide variance among nations in the marginals (e.g. Beck & Jackman, 1998; Bremer, 1992a, 1993a; Maoz & Russett, 1992, 1993; Raknerud & Hegre, 1997). As recorded in the commonly used Polity III dataset, neither the US wartime restrictions on civil liberties nor the British postponement of the elections affected the institutional democratic status of these nations. As recorded in Polity III, throughout World War II these states maintained a competitive political environment and relative parity between the executive and legislature – the cornerstones of most theories of democratic peace (Maoz & Russett, 1992). Therefore, while nations may tend to become more informally autocratic during wars, it is a negative impact of war on broad institutional structures – not on civil liberties or informal arrangements – that may affect the significance of the democratic peace proposition.

While we are unaware of any studies that show a negative concurrent impact of war on democratic institutions, such a pattern will not refute the presence of causality from joint democracy to peace. This is because the propositions are not mutually exclusive: war may have a concurrent negative impact on democratic institutions while at the same time joint democracy may reduce the probability of dyadic war. The prospect that the former may even partially explain the latter is not likely, given the evidence: studies have confirmed the democratic peace with the observation of war and dispute *origination* (e.g. Beck et al., 1998; Bremer, 1992a, 1993a; Gleditsch & Hegre, 1997). Since the relative peace among democratic nations occurs at the level of war origination (i.e. at the *onset* of wars), it is not plausible that this pattern can be explained by a concurrent impact of war on democracy.

Accordingly, while it remains an empirical question whether war has a negative impact on democratic institutions either during or after a war involvement, the resolution of these empirical questions is not likely to affect the inference of causality from joint democracy to peace. This is because the democratic peace appears to be a dyadic-level phenomenon at the level of war involvement, and the phenomenon occurs at the conflict stage of war origination. This is not to say that negative concurrent effects may not render some level of simultaneity in studies of regime type and war *duration* and war *intensity*. Rather, it is to say that, logically, negative concurrent and posterior effects cannot have an impact on the probability of war and dispute *onsets*.

It is the rarity of war and dispute onsets among democratic nations that is at issue. The democratic peace proposition does not state that militarized conflicts between democracies will be less intense or shorter than militarized conflicts between other kinds of regimes – it states that democracies will have significantly fewer wars (and disputes) among themselves. As Bremer points out, the duration and intensity of war is not the same as the onset of war (Bremer, 1995), and whether a war 'becomes long or short, large or small, severe or moderate depends upon the evolution of the war after its onset'

(Bremer, 1993b: 12). In the final analysis, it is the rarity of war and dispute onsets among democratic nations that serves as *prima facie* evidence of causation *from* democracy *to* peace. Therefore, what may affect the inference of causality from joint democracy to peace are anterior effects: regime changes that occur in *anticipation* of the onset of war.

The times before wars are often periods of preparation for the upcoming fight. In the period before the onset of World War II, for example, the democratic major powers of France, Great Britain, and the USA launched ambitious armaments programs. Just as nations often militarize in preparation for war, they can also autocratize. During the time that governments may impose conscription, increase the tax burden, and acquire larger arsenals, governments may also limit the power of the legislature, constrain the political opposition, and impose martial law. As Thompson states, 'even the threat of impending war can make decentralized power-sharing arrangements seem relatively inefficient and undesirable' (Thompson, 1996: 144).

If nations tend to autocratize as well as militarize when they prepare for wars, then the relative peace among democratic nations may be challenged by the specter of reverse causality: a negative impact of war on democracy. While this notion seems to make a mockery of the usual sequence of cause and effect, it also seems intuitively valid. Wars, particularly wars against neighbors and major powers, do not usually arise unexpectedly. Nations prepare for wars. If democratic nations have a tendency to adopt autocratic institutional structures as they prepare for wars, and if the size of this effect is strong enough to render it highly probable that at least one disputant in a joint democratic dispute is likely to autocratize before the onset of war, then indeed 'anterior' reverse causality has the potential to render the democratic peace relationship null and void.

Even if states tend to autocratize as they prepare for war, we cannot conclude that this explains the democratic peace relationship. This is because the relationship could be simultaneous: democracy can affect the probability of war onset while at the same time war can have an 'anterior' impact on democratic institutions (Thompson, 1996). The aim of this study is thus a limited one: we examine the presence of an anterior impact of war on democracy. If we find evidence for this reversed path of causation, then further study will be needed to examine the extent to which it may explain the democratic peace. If we find little or no negative anterior impact of war on democracy, however, then there would appear to be no simultaneity in the democratic peace relationship at the meaningful level of war onsets. In this case, we can conclude that simultaneity does not seem to be an omitted variable that explains, even partially, the presence of causation from democracy to peace. The crucial empirical question we ask is thus: *do political leaders tend to restrict democracy as they prepare for war?* The remainder of this article focuses on answering this question.

Analytic Procedures

By focusing on the anterior effect of war on democracy, we are able to employ a relatively simple research design to test the hypothesis. Because we already know that democracy has an impact on war, from the technical viewpoint anyone who wants to determine concurrent or posterior impacts of war on a nation's political system will have to take into account the impact of the political system on the nation's war behavior. Doing so is likely to require the use of a relatively more complicated research design that accounts for simultaneous causation. For our more limited focus on anterior effects, the statistical model we have chosen is the

Figure 1.   Modeling the Effects of War on Democracy

a. Level Effect

b. Shock Effect
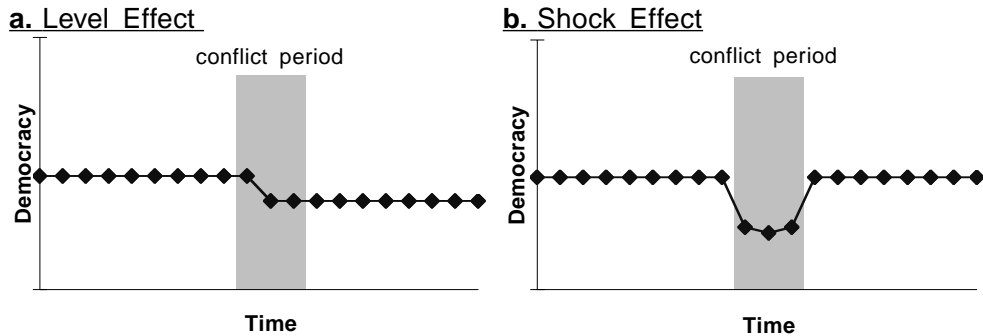
Conflict period consists of both prewar time and war time.

interrupted time-series (ITS), a regression designed to analyze the effect of a relatively discrete event on a phenomenon observed over time (Lewis-Beck, 1986). This method allows us to assess the degree to which wars affect political changes before their onset, apart from the impact of such political changes on war involvement. In applying this model, we have defined the democratic time-series at the national level as the dependent variable, and expect changes in that series to result from the efforts by political leaders to autocratize their political systems in response to impending wars.

Our ITS model has four independent variables, two of which are intended to capture an autocratizing action taken by a nation preparing for war. To understand how they work, let us assume that we observe a warring nation for twenty years. Suppose that the nation engaged in a war in the tenth year, and the fight lasted for two years. If the leaders had moved towards autocratic governing structures at some point before the onset of the war, a negative shift would take place in the nation's democratic time-series at that point. In the ITS framework, the simplest way to model such a shift is to create a dummy variable that has the value of 0 for observations before that point and 1 for observations after that point. A negative regression coefficient

associated with this variable, called a step function, will confirm the occurrence of the shift. But this is not enough, for this variable assumes not only that leaders take autocratic measures at some point before the onset of a war, but that these institutional changes are kept in place until the end of our observation period. In other words, the political shift triggered by the prospect of war is assumed to have a continuous effect, as visualized in Figure 1a.

Since leaders are also likely to return to structures that are more democratic once a war is over, we added a second variable that assumes the political impact of a war to be relatively short-lived. We code this variable 0 for observations before the shift point, 1 for observations between the shift point and the end of the war, and 0 again for observations after the war. As illustrated in Figure 1b, this variable, an impulse function, will work if the leaders of the warring nation repealed those prewar autocratic measures once the war was over, and the democracy score returned to what it was before the shift point.[4] We call the first variable a 'level' effect, and the second, a 'shock' effect. It must be cautioned that our assumptions

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[4] There is a possibility that a warring nation might repeal only part of those autocratic measures after a war ends. In this case, both variables will work and are necessary.

about the length of time for which a warring nation may keep prewar-induced autocratic measures has somewhat simplified how an interstate war may influence the political system. Because we have chosen only two endpoints, one at the end of a war (Shock) and the other at the end of an observation (Level), we might have risked measurement error. This is because a warring nation can return to democracy at any other point – say, two years after a war is over. Our aim at this stage, however, is to identify a pattern of prewar autocratization among nations; we thus assume that such measurement error is random.

We design a third variable in our model to control for concurrent and posterior impacts of war on democracy. As discussed, to capture a prewar autocratizing action by a warring nation, Level and Shock will be assigned a value of 1 between the shift point and the end of the war (Shock) or observation period (Level). A problem with this approach is the possibility that either Shock or Level may pick up some of the changes that take place in the democracy score during (concurrent) or after (posterior) a war. This being the case, we may reach a wrong conclusion as to the anterior effect of a particular war event. To address this problem, we include a third variable, 'control', which we code 0 for observations

before the onset of wars, and 1 for observations afterward.

A fourth variable in our ITS model is aimed at controlling for the trend of political change across time. To determine if nations tend to autocratize while preparing for war, we have to consider the possibility that a nation may be undergoing a long-term political transition for a host of reasons other than war. If we simply compare a nation's democracy score across time via the ITS model – disregarding the trend of political change – we may reach a wrong conclusion as to the political impact of a particular war. Suppose that Figure 2 is what we see in observing two nations' democracy time-series (the lines with solid diamonds). For the nation in Figure 2a, the average democracy level is higher after the shift point than before. However, it is wrong to conclude that the war had no negative impact on democracy. This is because without the war, the level of democracy after the shift point would have been higher (the line with hollow diamond) than what is observed. Figure 2b offers an opposite scenario. Here the average level of democracy is lower after the shift point than before. However, the drop is caused by the long-term political change rather than by the war. In the ITS framework, the most common way to control for the impact of long-term trends is

Figure 2.   Modeling the Effects of War on Democracy, Controlling for Trend
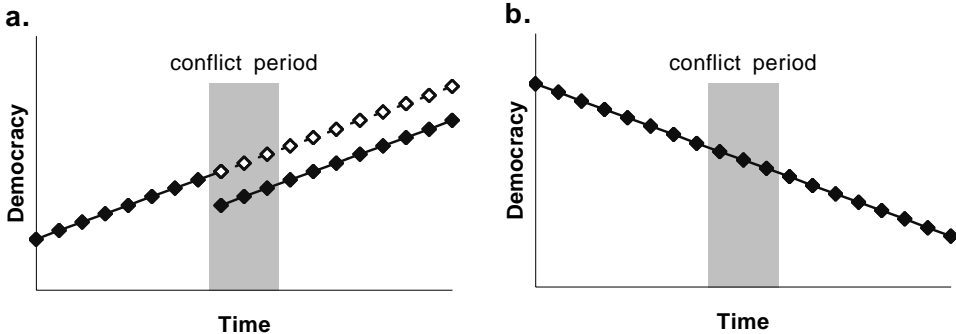
Table I.   Independent Variables in the ITS Model

Year	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Level	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1
Shock	0	0	0	0	0	0	0	0	1	1	1	0	0	0	0	0	0	0	0	0
Time	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Control	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1

to include a count variable, which we call 'time'.

The following is a more formal expression of the model we estimate:

$$Y_t = \beta_0 + \beta_1 Level_t + \beta_2 Shock_t + \beta_3 Control_t + \beta_4 Time, \; t = 1, 2, \ldots \; (1)$$

where Y is the democracy score at time t. To make things clearer, we can revisit the example used above. Suppose that nation A took some autocratic measures in the ninth year of observation, one year before the onset of its war. According to our definitions, we code all the independent variables as shown in Table I.

If nation A kept those autocratic measures until the end of the war, the estimation of Equation 1 will generate a negative β_1. This is because since year 9, Level = 1. So ceteris paribus, a negative β_1 will pull down the democracy level. If nation A returned to the prewar level of democracy once the war was over, we will see a negative β_2. This is because from year 9 to the end of the war (year 11), Shock = 1; after the war was over, Shock returns to 0. Again, if everything else remained constant, a negative β_2 will reduce the level of democracy for a short time-period. We emphasize that we designed our Level and Shock variables to capture any shift engineered by the prospect of war. If our goal was to determine the impact of an ongoing war – concurrent and posterior impacts of a war – we would have to start assigning both variables a '1' some time during the war.

Three technical notes are in order. First, since our focus is on the anterior impact of wars, in operating the ITS model we must choose a shift point before a war begins. A dilemma is that while the timing of a war is recorded, it is hard to know when leaders decide to impose autocratic measures in preparation for war. Therefore, in the subsequent analyses we have set three such shift points, one, three, and five years before a war, and we estimate our ITS model with each of these selections. While not perfect, this experimental approach helps us minimize the possibility of underestimating the anterior impact of war due to an erroneous choice of a prewar time threshold.

Second, the ITS model represented in Equation 1 tests the impact of interstate wars on democracy with the experience of one nation at a time. If, while preparing for war, the leadership of a nation takes a measure that renders the state less democratic, then we should be able to see the regression coefficients associated with either the Level or Shock variable as negative. However, because the experience of one nation may not be representative, our judgement on the presence of reverse causality will depend on the number of warring nations that have had similar experiences. If nations do tend to autocratize as they prepare for wars, then we should be able to observe negative Level and Shock effects in a majority of warring nations analyzed.

Finally, the ITS model specified in Equation 1 has relied on an approach that focuses more on real-world events than on statistical properties of the dependent variable.

However, in light of the self-perpetuating capability of most political systems, we must consider the possibility that the model may violate the basic assumptions underlying the ordinary regression technique. The most acute challenge comes from autocorrelation or serial dependence, a condition that will render our statistical testing unreliable. To minimize this possibility, we rely on three diagnostic tests for time-series: Durbin–Watson tests for first-order autocorrelation, Dickey–Fuller tests for unit root, and white noise tests for auto-correlation beyond the first order. Without going into details, it is enough to say that in most of the cases we analyzed, the democracy score demonstrated a certain level of non-stationarity, and the error terms from the regressions tended to be related across time segments, mostly in the first order. This suggests not only that a time variable is necessary, but also that the model must include a dependent variable lagged by a certain order. For this reason, we have performed our estimation with a procedure that contains the capability of autoregressive error correction. Before explaining this procedure, we address our data sources and measures.

Data Sources and Operationalization

Our aim is to examine the impact of inter-state war on democratic institutions before the onset of such wars. Since we observe each country separately, construction of our data sample begins with the identification of nations that have experienced the traumatic event of interstate war. The COW project defines 'interstate war' as a militarized conflict characterized by sustained and prolonged engagement between adversaries. Operationally, wars are identified as militarized interstate conflicts that experience at least 1,000 battlefield-connected fatalities. By this definition, the COW Interstate War dataset identifies 83 independent nations involved in 71 interstate wars from 1816 to

1992. Many of these nations fought multiple wars, yielding 261 separate war events.[5]

As discussed above, impending war participation is thought to affect the democratic status of a regime when the survival of the nation is at stake. However, some wars are more threatening than others. Great Britain's engagement in the Boer War, for example, was probably less likely to affect this nation's ruling institutions than its war against Germany in World War II. In the former case, the British were engaged in a war of imperialism far from their shores; in the latter case, the survival of the nation was at stake. It is when a nation's survival is at issue when the need for autocratizing measures is most compelling. Therefore, by including wars in the analysis where national security was less a direct issue, we may be biasing the sample in favor of a null finding (against reverse causality). For this reason, we sought to include for observation only those cases of war participation that most evidently endanger the security of the nation. We identify such wars as those fought against major powers or bordering nations. In this way we observe, for example, changes in the regime status of the Democratic Republic of Vietnam that may be due to its war against the USA (a major power), but we do not observe the impact of this war on the institutional structures of the USA – which we expect to be negligible. We do observe, however, changes in the regime status of the USA during its wars against Mexico (a neighbor) and Germany (a major power).

By including in the analysis only cases of war against neighboring states or major

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[5] Actually, the COW Interstate War dataset identifies 265 cases of war participation over the time-period studied. However, eight cases of war participation involve four nations that switched sides during World War II: Bulgaria, France, Italy, and Romania. For reasons that will become clear, we consider as war participation only the first side with which these nations entered the war, resulting in 265 minus 4 equals 261 cases of war participation.

powers, we remain consistent with a large number of studies in the democratic peace literature that have observed only dyads that are geographically contiguous or have at least one major power (e.g. Maoz & Russett, 1992, 1993; Oneal & Ray, 1997; Oneal & Russett, 1997). We identify major powers with COW rules and neighboring states as those with shared borders or separated by less than 150 miles of open water.[6] By these standards, 218 of the 261 war events, involving 77 countries in 69 wars, are identified as national war events in which at least one state on the opposite side of the conflict was a neighbor or major power.

An additional sampling consideration involves the intensity of war events. A large number of war events involve nations, often joiners to ongoing wars, who may have been involved only peripherally. For example, a large number of nations fought in the Korean War against the major power China, such as Ethiopia and Colombia, but suffered few fatalities. It seems less likely that such war participations were a direct matter of national security for these nations, and therefore it seems less likely that such war participations would affect the polity status of these regimes. As Siverson suggests, 'policy-makers have expectations about the outcomes and costs of wars' (Siverson, 1995: 483).

If the reverse causation hypothesis is correct, then the higher the costs that leaders expect of a war, the higher the probability that they will become more autocratic as they prepare for that war. This means that the inclusion of low-intensity war events in the analysis might bias the sample in favor of a null finding (against reverse causality). To rectify this bias, we opted

to exclude war events in which a nation suffered fewer than 1,000 battlefield-connected fatalities. The impact of interstate war on the democratic status of regimes will thus be estimated with the observation of war events in which a nation's national security appears to have been directly at issue: war events that involved at least 1,000 battlefield-connected fatalities and were fought against neighbors or major powers. Imposition of the fatality threshold reduced the number of war events to 159, involving 59 countries that fought in 59 interstate wars.

Recall that the interrupted time-series analysis requires us to perform a separate regression for each war fought by each country. This means that we need to observe the democratic status of each nation's governing institutions for a period of time before, during, and after each of its war participations. We thus extend our period of observation up to 20 years before and after each war for each participating regime and – consistent with theoretical expectations – assign dates of war participation as beginning when a country enters a war, and ending when the war (not the country's participation in it) ends. The COW War dataset records the first war since the Napoleonic era (within the above thresholds) to have begun in the year 1828 between Russia and Turkey, with the last war ending in 1991 (Gulf War). Yet not every nation was in existence for 20 years before its first war involvement. Czechoslovakia, for example, became an independent nation only in 1918, and its single war (against neighboring Hungary) was fought only one year later. Since we obviously can not gauge prewar polity trends under such circumstances, we omit from the analysis all war events begun within the first ten years after a country's establishment as an independent nation. We assume that one decade provides a reasonable window of time for gauging prewar levels of democracy.

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[6] The Correlates of War project identifies major powers as Great Britain 1816– ; France 1816–1940 and 1945– ; Russia 1816–1917 and 1922– ; Germany 1816–1918, 1925–1945 and 1991– ; Austria–Hungary 1816–1918; USA 1899– ; Italy 1860–1943; China 1950– ; and Japan 1895–1945 and 1991– .

This consideration reduced our sample to 147 war events involving 56 countries.[7] Data on governing institutions (to be discussed shortly) are available from the years 1800 to 1994. Since we are primarily interested in the impact of war on governing institutions before war involvement, we did not impose the ten-year rule as a criterion after the last year of a country's war participation.

Turning to our measure of democracy, we must consider that the democratic peace hypothesis is drawn not from observations of civil liberties directly, but from observations of institutional structures (e.g. Bremer, 1992a, 1993a; Gleditsch & Hegre, 1997; Maoz & Russett, 1993; Raknerud & Hegre, 1997). Since the purpose of our analysis is to test for the potential of reverse causality in the democratic peace relationship, we follow the overwhelming majority of studies of democratic peace and obtained data on governing institutions from the Polity III dataset (Jaggers & Gurr, 1995). In line with the theoretical proposition that war participation fosters concentrations in political power, we gauge democracy by subtracting a state's 11-point autocracy score from its 11-point democracy score, creating a 21-point index of democracy ranging from -10 to $+10$. We obtained data on the actual dates of regime changes from the Polity IIId dataset (McLaughlin et al., 1998), and assigned polity changes in the annually aggregated data to reflect the actual sequence of events in war starting and

war ending years.[8] Because of minor mismatches between Polity III and Polity IIId, incorporation of the two reduced our sample to 54 countries involved in 137 war events.[9]

Design-Related Problems

Before we report the results, let us first describe the analytical problems we encountered. The first problem has to do with nations involved in either two or more wars simultaneously (e.g. the Democratic Republic of Vietnam, 1965–88), or one war followed close behind another (e.g. Ethiopia, 1935–45). Since nations already in war are presumably already prepared for war, separate regressions of these war events will not provide any new information and will not serve to test the hypothesis of prewar autocratization. The best way to deal with this problem is to treat multiple or consecutive war events as single war events. Because of

[7] For each year, we identify independent nations in accordance with Correlates of War, whose list of nations can be obtained from the web page of the Peace Science Society (International) at http://pss.la.psu.edu. Countries excluded on the ten-year rule basis are Czechoslovakia (Hungarian–Allies War) and both North and South Korea (Korean War). Excluded war events are: Bulgaria (First and Second Balkan Wars), China (Korean War); Hungary (Hungarian–Allies War), Israel (Palestine War), Italy (Seven Weeks War), Jordan (Palestine War), Poland (Russo–Polish War) and Syria (Palestine War).

[8] For example, in the case of Spain's war against Morocco, Polity IIId records Spain's regime as changing from -5 to -6 on 1 July 1859 – almost four months before its war began on 22 October. Consequently, we assigned Spain a score of -6 for the years 1858 and 1859. Had the regime change occurred on or after 22 October 1859, we would have kept the -5 score for the year 1858 and begun assigning -6 from 1859. Antithetically, if a nation's regime changed in the last year of a war participation, and this change occurred before the war ended, we assigned the new polity status for the warring year.

[9] We coded cases of institutional transitions, interruptions, or interregnums as missing values. Polity IIId does not provide data on regime changes for Austria–Hungary (1816–1918), Italy (1816–60) and Serbia (1878–1918), resulting in the loss of one, three and three war events, respectively. Germany is recorded as experiencing an institutional transition before and throughout the Franco–Prussian War (1870–71), the Papal States is recorded as experiencing an institutional transition before and throughout the War of the Roman Republic (1849), and Kuwait is recorded as undergoing an institutional interruption before and throughout the Gulf War (1990–91). These cases resulted in the loss of one war event (for Germany) and two countries (the Papal States and Kuwait). The Polity III and IIId data are availalable at: http://www.colorado.edu/IBS/GAD/spacetime/data/Polity.html.

this, the total number of war events in our analysis was reduced from 137 to 113.[10]

The second problem has to do with the number of observations in each regression. As discussed above, we observe a warring nation's political system 20 years before and after a war event. This should give us more than 40 observations in each regression. But only 36 war events meet this requirement. Excluding 22 cases in which the democracy score does not vary over the observation period, there are 55 cases where missing values occur. In approximately ten such cases, the number of missing values is over five.[11] A major reason is that the Polity III and IIId datasets, while quite extensive, have a large number of missing values for certain nations (e.g. Morocco, Italy, and France). Another reason is right-censoring – the democracy score ends in 1994, which means that for any nation that fought a war after 1974 (e.g. China, Iran, and Iraq), we cannot observe its postwar political status for the full 20 years. The third reason is that a number of warring nations, such as Baden, Bavaria, Germany, Vietnam, and Wuerttemburg, disappeared or were split

once the wars were over. The last reason is that a few warring nations, such as Israel, came into existence less than 20 years before they fought a war. Regardless of the causes, the reduced number of observations requires us to be cautious in interpreting the results of diagnostic tests and regression results.

The third problem is about how to estimate the ITS model. By inspecting the democracy scores of all the warring nations in our sample, we can see that political systems often change in the form of a step or impulse function. This suggests that the data-generating process underlying the democracy score may have followed a pattern known as random walk with drift, and that an ordinary least squares (OLS) estimation of the model may be inefficient. In some cases, a long-term trend in the time-series can be observed, a condition that warrants the inclusion of the Time variable. However, this may not be a perfect solution, for we may still risk the danger of violating some basic assumptions of OLS in cases where a trend effect is absent.

Since both random walk with drift and model misspecification will yield serial dependence, we have carried out a series of tests to identify the nature of every democracy time-series. Durbin–Watson tests show that in most cases, we cannot reject the null hypothesis of no first-order autocorrelation. Augmented Dickey–Fuller tests further show that in a number of cases there exists a certain level of nonstationarity in the democracy score, even with the presence of the Time variable.[12] To solve this problem, we relied on a procedure that estimates the regression coefficients and the autoregressive error parameters simultaneously. This is known as autoregressive error correction. Let us now turn to the results.

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[10] Nations where multiple wars are treated as a single war events are Ethiopia (Italo–Ethiopian War of 1935–36 and World War II of 1939–45); Greece (Second Balkan War of 1913–17, World War I of 1917–18 and Greco–Turkish War of 1919–22); Finland (Russo–Finnish War of 1939–40 and World War II of 1941–45); China (Sino–Soviet War of 1929–29 and Manchurian War of 1931–33); China (Sino–Japanese War of 1937–41 and World War II of 1941–45); D.R. Vietnam (Vietnamese War of 1965–75, Vietnamese–Cambodian War of 1975–88, Sino–Vietnamese War of 1979–79 and Second Sino–Vietnamese War of 1985–87); Egypt (Six Day War of 1967–67 and Israeli–Egyptian War of 1969–70); Germany (Second Schleswig–Holstein War of 1864–64, Seven Weeks War of 1866–66 and Franco–Prussian War of 1870–71); Iraq (Iranian–Iraqi War of 1980–88 and Gulf War of 1990–91); Japan (Sino–Japanese War of 1937–41 and World War II of 1941–45); Romania (World War I of 1916–17 and Hungarian Allies of 1919–19); Russia (Changkufeng War of 1938–38 and World War II of 1939–45); and Turkey (First Balkan War of 1912–13, Second Balkan War of 1913–13, World War I of 1914–18 and Greco–Turkish War of 1919–22).

[11] Details available in the Appendix (Table 1) which is available at: http://personal.ecu.edu/shiy/peace/peace.htm.

[12] Details available in the Appendix (Table 4) which is availalable at: http://personal.ecu.edu/shiy/peace/peace.htm.

Analysis and Results

As specified in Equation 1, two coefficients in our ITS model, Shock and Level, are of particular importance, for they are designed to measure the political effects of preparing for war. However, when we set prewar to three years, 87 of the 113 regressions have generated a statistically insignificant estimate for both Shock and Level. These include 22 cases of no change in a warring nation's democracy score. The remaining 26 regressions, listed in Table II, have at least one of the coefficients statistically significant. As those not reported here, we estimated all these cases with maximum likelihood. This

Table II.   Maximum Likelihood Estimates of Interstate Wars on Democracy

State	War	Degrees of Freedom	Intercept	Time	Control	Shock	Level	Shock + Level
Panel 1								
Belgium	World War I 1914–18	38	5.62*	0.05*	0.09	−2.00*	1.97*	−0.03
Bulgaria	World War II 1941–45	31	−3.00*	−0.23*	−0.08	−5.20*	4.56+	−0.64
Russia	Russo–Japanese War 1904–05	35	−10.00*	0.03	0.02	−4.50+	4.30	−0.20
Panel 2								
Egypt	Sinai War 1956	35	0.92*	0.01+	−0.02	0.05	−8.10*	−8.05
Ethiopia	Multiple Wars 1935–45	38	3.49*	−0.19*	−0.47	3.31*	−4.40*	−1.09
France	Crimean War 1854–56	27	−2.70*	0.37*	−0.37	0.68	−14.00*	−13.32
Germany	First Schleswig–Holstein 1848–49	33	−11.00*	0.09*	0.59*	0.55+	−0.60+	−0.05
P.R. China	Second Sino–Viet 1985–87	25	−9.20*	0.14*	−0.41+	0.68*	−1.00*	−0.32
Romania	World War II 1941–45	35	−4.00*	0.00	−0.67*	−0.02	−2.40*	−2.42
Turkey	Turko–Cypriot War 1974	33	3.30+	0.51*	−0.77	3.65	−13.00*	−9.35
Panel 3								
India	Bangladesh War 1971	34	9.09*	−0.01	−0.33	0.65*	−0.42	0.23
Iran	Iranian–Iraqi War 1980–88	30	−10.00*	0.01	2.62*	1.06*	0.19	1.25
Paraguay	Chaco War 1932–35	37	−2.70*	−0.04	2.24	5.00*	−4.70	0.30
Yugoslavia	World War II 1941–45	31	−2.40	−0.26	0.26	6.62+	4.48	11.10
Panel 4								
Austria	World War I 1914–18	34	−0.38	−0.31	0.35	−13.00*	13.50*	0.50
Brazil	World War II 1944–45	31	−2.30*	−0.32*	0.63	−15.00*	16.30*	1.30
Denmark	First Schleswig–Holstein 1848–49	37	−8.90*	−0.05	5.63*	−5.50*	6.38*	0.88
France	Franco–Prussian 1870–71	27	−5.50*	−0.09	2.18	−11.00*	13.40*	2.40
Italy	World War II 1940–45	29	−2.00	−0.21*	0.22	−20.00*	20.60*	0.60
Paraguay	Lopez War 1864–70	40	−8.80*	−0.02	1.28	−4.36*	4.55*	0.19
Pakistan	Second Kashmir War 1965	33	3.69	−0.51*	−1.70	−0.38	11.90*	11.52
Romania	Second Balkan War 1913	34	−6.70*	0.03+	−0.04	0.05	1.93*	1.98
Russia	World War I 1914–18	36	−7.40*	−0.25+	0.19	−10.00*	10.40*	0.40
Turkey	Russo–Turkish 1877–78	36	−9.90*	−0.01	−2.00*	−0.06	2.11+	2.05
Turkey	Multiple Wars 1912–22	43	−9.60*	−0.04*	1.23*	4.70*	3.72*	8.42
Uganda	Ugandan–Tanzanian War 1978–79	22	4.39+	−0.85*	1.14	−13.38*	13.86*	0.48

The entries are regression coefficients estimated using SAS Autoreg procedure with Maximum Likelihood and backstep option, which includes autoregressive coefficients up to the fourth order when they are significant at the 0.05 level. *Indicates a statistical significance at the 0.05 level, and +, at the 0.10 level. In several instances, multiple wars were treated as a single war event. Ethiopia and Turkey are two such instances. For details see Appendix Table 6, which is available at: http://personal.ecu.edu/shiy/peace/peace.htm.

was necessary because the model became nonlinear after it took into account the autoregressive error correction. Another benefit of maximum likelihood is its capacity to deal with missing values.

There are two guidelines for our interpretations. The first is the statistical significance and direction of Shock and Level. If one of the coefficients is statistically significant and has a negative sign, we may very likely have a case in which the war event triggered a prewar downturn for the nation's political institutions. Still, a negative sign and statistical significance are only a necessary condition for a conclusive determination. This is because every time we started assigning 1 to Shock we also began coding Level as 1. Hence, any change in the democracy score during the prewar time is determined by Shock and Level together. If the coefficient for one variable is negative while the coefficient for the other is positive, the magnitude of change in democracy will be more complicated than it appears with either coefficient alone. For this reason, we look at the combination of Shock and Level as our second guideline. If a war event had caused the democracy score to drop, the combination of Shock and Level must be negative.

Following these two guidelines, we divided the war events in Table II into four categories. For each of the three cases in Panel 1, Shock is statistically significant and negative ($p < 0.10$, two-tailed test), while Level is either positive or not significantly different from zero. The message is that the warring nations in this panel had experienced a drop in the level of democracy while preparing for the noted wars. We can take Belgium as an example. Disregarding the estimates for autoregressive errors, the estimation gives the following equation: *Democracy score* = 5.62 + 0.05 *Time* − 0.09 *Control* − 2.0 *Shock* + 1.97 *Level*. Because the war spanned 1914–18, we set 1911 as the shift point, three years before the war.

Before 1911, both Shock and Level are equal to 0 and the democracy score is determined by the intercept and Time. From 1911 to the onset of the war, both Shock and Level are 1 and the democracy score is determined by the intercept and Time, plus the combination of Shock and Level in the last column. The negative combination suggests that if everything else were held constant, the democracy score would drop by 0.3 units in 1911 to the onset of this war. After the war began, the Control variable accounts for any concurrent and posterior impacts of war on democracy.

Panel 2 consists of seven cases in which Level is statistically significant and negative while Shock is either positive or not significantly different from zero. Similar to those in Panel 1, the warring nations in Panel 2 appear to have changed their political systems toward less democracy while preparing for the noted wars. The difference is that in Panel 1 a warring nation had either repealed those anti-democratic measures or become more democratic once the war was over, while in Panel 2 the warring nations kept the anti-democratic measures in place until the end of the observation period. We can take Egypt as an example. The ITS regression yields the following estimation of the impact of the Sinai War on Egypt: *Democracy score* = 0.92 + 0.01 *Time* − 0.02 *Control* + 0.05 *Shock* − 8.1 *Level*. Before 1953, the democracy score is determined by the intercept and Time. From 1953 to the onset of the war, it is determined by the intercept, Time, Shock, and Level. Because the combination of Shock and Level is −8.05, the democracy score is estimated to be 8.05 units lower from 1953 to the onset of the war than it was before 1953, a clear indication of a negative anterior impact of war on democracy.

Panel 3 in Table II is for cases where Shock is statistically significant and positive while Level is not significantly different from

zero. These four cases are interesting because the wars had actually been accompanied by an anterior increase of democracy for the nations involved, ranging from 0.65 units in India before the Bangladesh War, to 6.62 units in Yugoslavia before World War II. We do not argue that the prospect of war had caused these nations to become more democratic, but at least in these four cases, impending wars had not exerted a negative impact, as the reverse causality hypothesis might lead us to expect. This observation applies also to the cases in Panel 4. In most of those twelve cases, Shock is statistically significant and negative, but because Level is greater than Shock in terms of absolute value, the combination of Shock and Level has come out positive. Therefore, the net effect of the war events is in the positive direction.

What then is the overall picture? The coefficients in Table II seem to provide little support for the notion that nations tend to become autocratic as they prepare for wars. If autocratization during preparation for wars is the reason for the absence of wars among democratic nations, then we should be able to observe a large number of cases in the first two panels of the table. But there are only three confirmed cases where preparation for war seems to have lowered the level of democracy in the short term (Panel 1), and only seven confirmed cases in which such preparation appears to have lowered the level of democracy in the long run (Panel 2). Taken together, this makes a total of ten cases out of 113 where preparation for interstate war appears to be significantly associated with a political change toward less democracy, a ratio of less than one in eleven. This suggests that, by and large, preparations for war have left no negative impacts, short-term or long-term, on the governing structures of nations.

This conclusion, however, needs some qualifications. One has to do with the possibility that the findings reported in Table II

may change if we alter the length of prewar time. To find if this is the case, we recoded Shock and Level with prewar times equal to one and five years, respectively, and re-estimated their regression coefficients. As we had done with the three-year estimation, we then identified cases like those in Panels 1 and 2. Table III summarizes the overall picture with each specification for prewar time.[13] Each entry in the table represents the number of cases in which at least one regression coefficient, either for Shock or Level, is statistically significant and negative, and their combination is negative. The figures under Shock are for cases like those in Panel 1 of Table II, the figures under Level are for cases like those in Panel 2 of Table II, and the figures under Total are the sum of cases under Shock and Level. Comparing across the table, we can see that, whether we estimate with prewar times of one, three, or five years, the total number of significant negative cases remains small, though increasing a little as the prewar period is extended. For the vast majority of the 113 war events we estimated, the regression coefficients are either positive (alone or in combination), or not significantly different from zero.

Supplementing Table III is Figure 3, which reports the sum of the Shock and Level coefficients for each of the 113 cases for each specification of prewar time. The vertical axes represent the sum of Shock and Level for the

Table III.   Number of Regressions with Significant and Negative Estimates

	Coefficients		
Prewar Time	Shock	Level	Total
1 year	2	7	9
3 year	3	7	10
5 year	3	9	12

[13] Detailed regression outputs are availalable at: http://personal.ecu.edu/shiy/peace/peace.htm.

Figure 3.   Histograms of Combined Impacts of Shock and Level for Prewar Times of 1, 3, and 5 Years
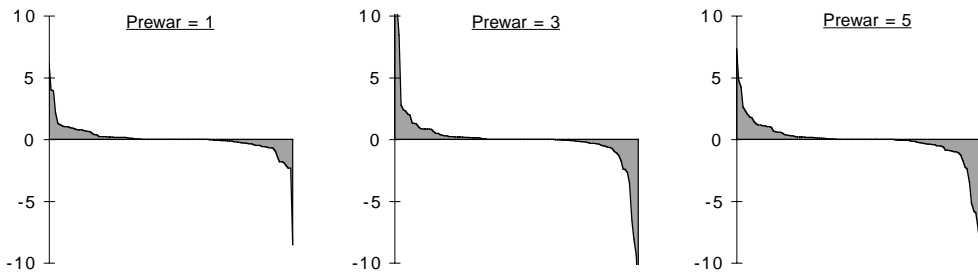
Vertical axes represent the coefficient values for the 113 war events, placed in descending order along the horizontal axes. Coefficient values have been capped at values less than −10 or greater than 10 for the purpose of presentation.

113 records, placed in descending order along the horizontal axes. If nations do not tend to autocratize while preparing for major wars, then the sum of Shock and Level across the 113 records will have a normal distribution – with negative coefficient values canceled out by positive ones. As can be seen in Figure 3, the sum of Shock and Level appear to be roughly normally distributed for each specification of prewar time, as the top half of each histogram approximately mirrors the bottom half. The histograms in Figure 3 thus illustrate the estimated result that nations are about as likely to become more democratic as they are to become more autocratic in the periods before they fight major wars.

Another qualification concerns the prewar political system as a constraint on political change. So far we have ignored the fact that the degree to which a nation can move its system toward autocracy is limited by the nature of its system. A highly autocratic regime, for example, has little room to become more autocratic. This means that if nations alter their governing institutions towards autocracy as they prepare for wars, the phenomenon is more likely to be associated with democratic nations. Consequently, we reconfigured Table III by dividing all the records at each prewar time into three groups, depending on the average value of democracy over the first 15 years of each observation period. Following Jaggers & Gurr (1995: 474), we identified autocracies as those with average values less than or equal to −7, anocracies as those with average scores greater than −7 but less than 7, and democracies as those records with average values greater than or equal to 7.

As in Table III, the entries under Shock and Level in Table IV represent the number of cases in which a regression coefficient is statistically significant and negative ($p < 0.10$, two-tailed test), and the sum of the Shock and Level coefficients is negative. The figures under Total are the sums of the cases under Shock and Level, and those in the last column show the percentage of cases for each category of political system. As we can see in Table IV, the autocracy group consists of 31 war events, with between 10% and 13% associated with significant and negative changes in governing institutions during preparation for wars. Anocracies, in contrast, have 60 cases, of which between 8% and 10% are associated with significant and negative prewar changes in the level of democracy. Of the 22 cases of democratic war participation, approximately 5–14% appear to be associated with some level of prewar institutional autocratization when prewar is set three or five years. Thus, on average, it appears that democracies are no more likely than autocracies or anocracies to experience

Table IV.   Number of Significant and Negative Estimates, by Group

| Group | Number of Cases | Prewar Time | Coefficients | | | % |
			Shock	Level	Total	
Autocracy	31	1 year	2	2	4	0.13
		3 year	1	2	3	0.10
		5 year	1	2	3	0.10
Anocracy	60	1 year	0	5	5	0.08
		3 year	2	4	6	0.10
		5 year	2	4	6	0.10
Democracy	22	1 year	0	0	0	0.00
		3 year	0	1	1	0.05
		5 year	0	3	3	0.14

Autocracies are those nations with average prewar democracy values less than or equal to −7, anocracies are those greater than −7 but less than 7, and democracies as those records with average democracy values greater than or equal to 7.

negative political changes while preparing for foreign wars. The majority of cases of democratic wars – 86% or more – do not appear in Table IV because the sums of their Shock and Level coefficients are either positive or are not significantly different from zero.

Finally, we must estimate not only the proportion of confirmed democratic cases associated with downturns in governing institutions, but also the magnitude of such downturns. If reverse causality is the reason for the absence of war among democracies, then not only should there be a large number of nations experiencing some negative changes in their political systems, but it should also be the case that such negative political changes are intense enough to turn some democracies into autocracies or (at least) anocracies. To examine whether this is the case, we again follow Jaggers and Gurr's distinction and treat a nation as a democracy if its average democracy score in the first 15 years was between 7 and 10. Next, we looked at how much the democracy scores appear to have declined during the prewar time, as indicated by the combination of the Shock and Level coefficients. Table V summarizes the findings.

The third column in Table V reports the nation's estimated prewar level of democracy (sorted by level of democracy), with the remaining columns noting the sum of the Shock and Level coefficients for each of the three estimated prewar times. Of the 22 cases of democracies in major wars from 1816 to 1992, we can see only one where the level of democracy is estimated to have dropped below the 7-point democracy threshold before the onset of the war: Turkey in 1974 (Turko–Cypriot War). Of further interest is the bottom row in the table, which reports the average value for each column. Among the 22 cases of democracies in war, the average estimated change in democratic institutions associated with preparation for war is a net decrease of not more than 0.5 units (when prewar is set three years). On a 21-point scale, such a change seems very small. Moreover, this list includes most cases where democracies have fought major wars against neighbors or major powers. Nevertheless, it appears that in less than one-fourth (5) of the democratic wars is the sum of the Shock and Level coefficients negative for all three prewar times. This tally is *smaller* than the number of cases (6) where this sum is *positive* for all three prewar times. The historical record thus indicates that, as they prepare for major

Table V. Measuring the Impact of War on Democracy

State	War	Prewar Democracy	Shock + Level		
			1-year	*3-year*	*5-year*
Turkey	Turko–Cypriot War 1974	7.14	6.13	−9.35*	−0.90*
UK	World War I 1914–18	7.47	−0.12	−0.16	−0.16
France	World War I 1914–18	7.73	−0.22	−0.33	−0.41
Finland	Multiple Wars 1939–45	8.57	−0.06	−0.21	−0.39
India	Bangladesh War 1971	9.00	−0.23	−0.23	0.18
India	Second Kashmir War 1965	9.00	−0.16	−0.24	0.29
India	Sino–India War 1962	9.00	−0.23	−0.30	0.32
USA	Mexican–American War 1846–48	9.00	1.04	0.12	0.12
France	World War II 1939–45	9.20	−0.18	−0.24	0.29
Belgium	World War II 1940–45	9.33	0.00	0.01	0.03
UK	World War II 1939–45	9.60	−0.05	−0.02	0.08
Canada	World War II 1939–45	9.87	−0.02	−0.01	0.02
Israel	Yom Kippur War 1973	9.93	−0.01	−0.07	−0.86*
Australia	World War II 1939–45	10.00	0.00	0.00	0.00
Greece	Multiple Wars 1912–13	10.00	−1.80	−1.72	−1.76
Israel	Six Day War 1967	10.00	0.00	0.00	0.00
Netherlands	World War II 1940–45	10.00	0.00	0.00	0.00
New Zealand	World War II 1939–45	10.00	0.00	0.00	0.00
Norway	World War II 1940–45	10.00	0.00	0.00	0.00
USA	Korean War 1950–53	10.00	0.00	0.00	0.00
USA	World War I 1917–18	10.00	0.00	0.00	0.00
USA	World War II 1941–45	10.00	0.00	0.00	0.00
Average Score		9.3	0.26	−0.49	−0.14

* Either Shock or Level or both are negative and significant at the 0.05 level.

wars, democratic nations are as likely to become more autocratic as they are to become even more democratic.

Implications and Conclusion

This study examined the notion that the path of causation from democracy to peace may be affected by reverse causation: that war may have a negative impact on democracy. We distinguished three kinds of war on regime effects: the anterior, concurrent, and posterior. We then showed that because the democratic peace appears to be a dyadic-only phenomenon at the level of war involvement, and because democratic nations are rarely, if ever, on opposite sides in wars at their start, reverse causality is

likely to affect the role of causation in the democratic peace only if there is an impact of war on democracy before wars begin. The essential question is thus, *do nations tend to autocratize as well as militarize when they prepare for war*? We examined this proposition with the observation of wars against geographic neighbors or major powers worldwide from 1816 to 1992.

With interrupted time-series analysis, we found little support for the hypothesis. In the clear majority of cases, we found no relationship between war involvement and negative changes in political institutions. We also found that all nations – including the subset of democratic ones – are as likely to become more institutionally autocratic as they are to become even more democratic in

the periods before the onset of wars. These results are robust in the sense that we accounted for the regime status of nations on the precise date of their war onsets, controlled for long-term political changes, corrected for autocorrelation errors, and considered institutional changes that may occur from one to five years before the onset of wars. In addition, we observed wars that were most likely to threaten a nation's security and thus affect the governing institutions of fighting nations: wars fought against neighbors and major powers and where the state suffered 1,000 or more battle-deaths. If nations tend to become more institutionally autocratic as they prepare for foreign wars, then we should see this pattern occurring during preparation for these wars. Instead, we found a remarkable institutional stability among democratic regimes, with very few cases of prewar institutional change (Table V). Consequently, the finding that nations do not appear to become autocratic before the onset of these wars yields the conclusion that reverse causality seems to be an unlikely explanation for the peace between democratic nations.

Still, the analyses tell us only a part of the democracy and war story. First, we observed broad changes in institutional structures; we did not observe changes in lower levels of law and policy that may affect civil liberties. It remains an empirical question whether nations tend to restrict civil liberties as they prepare for major wars. However, the matter of civil liberties will not affect the inference of causality from regime structure to peace. While civil liberties are no doubt a part of what we usually mean by 'democracy', and democracies probably do tend to become more centralized while preparing for war, empirical support for the democratic peace hypothesis rests mostly on the observation of broad institutional structures, not civil liberties or election postponements.

Second, there is nothing about our find-

ings that suggests that war duration and war intensity may not have negative impacts on the democratic institutions of fighting nations. It remains an empirical question whether long and intense wars tend to compel governments to centralize power. Concerning the democratic peace hypothesis, however, this question is immaterial: the hypothesis does not state that wars between democracies will be less intense or shorter than wars between other kinds of regimes – it states that democracies will have significantly fewer wars among themselves.

Third, there is nothing about our findings that yields the conclusion that regime structure and the international environment are not a two-way street (Maoz, 1996). Living in a rough neighborhood may well lower the prospects for stable democratic institutions in a nation (Midlarsky, 1995; Thompson, 1996). The question at hand, however, is whether the regional environment can explain, even partially, the relative peace among democratic nations. As discussed herein, resolving this question is not an issue of simultaneity or reverse causality in the democratic peace relationship, but rather a matter of developing theoretical constructs and consequent measures of external threat and modeling these effects in future studies of democratic peace.

The finding of a remarkable institutional stability among democratic nations even while they prepare for major wars yields a clearcut implication: it seems unlikely that studies of regime type and war onset have been underspecified due to reverse causality. When it comes to the presence or absence of war, the analyses support the conclusion that the direction of causation in the regime type and war relationship is unidirectional, from democracy to peace.

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