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Unpacking Post-Civil War Democratization: How Wartime Institutional Capacity Conditions the Effects of Conflict Termination on Post-War Democratization

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ABSTRACT

What variables meaningfully influence post-civil war democratization? Existing literature on post-civil war democratization has reached conflicting conclusions on whether negotiated settlements or decisive victories foster democracy, a disagreement that persists even under methodological corrections. To address this conflict, this paper presents a theoretical synthesis that identifies a critical missing variable: wartime institutional capacity. By synthesizing studies on conflict termination with theoretical accounts of wartime governance, the paper proposes a conditioning framework in which war termination type does not directly cause postwar regime outcomes, but instead presents a set of political tasks to the postwar state. This paper argues that whether a state can execute those tasks depends on the administrative machinery built during the conflict. The paper is primarily a literature review that compares four leading studies of conflict termination and a recent account of wartime governance. It then uses the gaps identified in that review to propose a theoretical framework centered on a missing variable: wartime institutional capacity. While purely theoretical, this framework offers a possible conceptual resolution to the field's conflicting findings and yields four testable directional hypotheses to guide future empirical research linking wartime administrative capacity to postwar democratic stability and decay.

INTRODUCTION

Scholars studying post-civil war regimes have come to different conclusions regarding what factors may cause a state to democratize post-war. One strand of scholarship argues that how the war ends meaningfully influences democratization. Toft finds settlements produce a short-lived democratic gain that reverses into authoritarianism, while rebel victory alone produces durable liberalization (Toft, 2010). Nilsson, using a different measure of democracy, finds the reverse: settlement consistently outperforms victory (Nilsson, 2012). Joshi predicts government victors should liberalize faster than rebel victors and fails to find that pattern in his own data (Joshi, 2010). Fortna and Huang go further, arguing that once measurement problems are corrected, war outcome has almost no reliable effect on democratization at all (Fortna & Huang, 2012). Four careful studies testing closely related claims reach different conclusions.

A second strand rejects the premise that termination is the right place to look at all. Justino argues that the conditions shaping a postwar state are built earlier, through the governance structures an armed group constructs while it holds territory during the war. Her account carries real theoretical force but has never been tested against data. This paper argues the two strands are not competing but complementary. None of

the four termination-type studies measures what either side had already built by the time the fighting stopped. This paper develops that variable into a formal theory of wartime institutional capacity and argues that termination type does not produce a postwar outcome on its own, it hands a state a set of political conditions, and institutional capacity determines whether that state can carry them out. Rather than conducting a new quantitative analysis, this paper contributes to the literature by building a formal theoretical framework that reinterprets existing findings and establishes four directional hypotheses for future quantitative testing.

LITERATURE REVIEW

For decades, scholars studying post-civil war governance have grappled with the puzzle of why some states emerging from civil wars successfully transition into democracies while others collapse back into war or into autocracies. Two types of answers have largely dominated this question; one answer looks to the end of the war, arguing that whether a conflict concludes in negotiated settlement or military victory sets the trajectory for what follows, another answer, looks to the middle of the war tracing how the governance structures combatants build while fighting shapes state capacity and institutions long after the war ends. However, these traditional answers have not gone unchallenged. Methodological critiques, such as the one by Fortna and Huang, have questioned whether the field's core outcome variable is even measured consistently across these studies and offer alternative variables that may affect post-war democratization. The following section will examine a range of proposed variables to assess what each contributes to and leaves unanswered about this puzzle.

Type of War Termination

A first strand of scholarship attempting to answer the question of what meaningfully predicts post-conflict democratization argues that the way the civil war terminated is a variable that can predict whether or not a state transitions to democracy post-war. Monica Toft takes up this argument directly, testing whether termination type predicts not just whether peace holds, but whether the postwar regime liberalizes at all. Using a data set of 137 civil wars terminating between 1940 and 2007 Toft associates different modes of war termination with different levels of authoritarianism post-war (Toft, 2010). Toft categorizes war termination into three main categories: negotiated settlement, victory and ceasefire/stalemate. While the stalemate variable is tested, Toft takes the findings with a grain of salt because of the low number that actually terminate in a ceasefire/stalemate. Following a negotiated settlement, states experience a short term spike in Polity score that Toft attributes to elections mandated by the signed agreement. However, that gain begins to decline after five years and eventually collapses into authoritarianism. Toft argues that negotiated agreements often do not address the underlying incentive structure present in post-war states and as a result, after the first election cycle and when third parties depart, post-negotiation repression tends to increase. This finding complicates the intuitive assumption that negotiated settlements represent the more democratic path out of a civil war simply because they involve elections. Toft's data suggests that the opposite may hold true over the long run. The termination type most closely associated with democratic processes produces the weakest democratic outcome.

However, victory's relation to post-war democratization does not mirror negotiated settlements in reverse. Averaged across all cases, victory has a negligible impact on post-war governance. It is only when the categories of victory are disaggregated that a pattern emerges. The two sides that victory is separated into: rebel victory and government victory move in completely opposite directions post-civil war. Government victories increase repression by one to two Polity points over subsequent decades, while rebel victories produce a decline in autocracy exceeding one point within ten years and more than doubling by twenty (Toft, 2010). Toft argues that the identity of the victor is an important distinction because a victorious government and a victorious rebel group have different incentives post-war. What this argument reveals is that the meaningful predictor of post-war democratization is not the binary of settlement vs. victory, but the incentive structure a given termination type leaves with each.

Nilsson takes up the same question of what termination type does to postwar democratization, but where rebel victory outperforms settlement over the long run, Nilsson's evidence points the other way. Nilsson departs from Toft in his first methodological choice: how democratization itself is measured. Rather than using the Polity IV's regime score, Nilsson adopts Coppedge et al. 's (2008) two-dimensional Polyarchy measure. This measure tackles both contestation and inclusiveness separately. Contestation tracks the extent to which citizens and rival political organizations can freely compete for elected positions of power; inclusiveness measures how citizens participate in and benefit from political systems and institutions. Nilsson justifies using this method of measuring democracy by explaining that separating the way democracy is measured into contestation and inclusiveness gives much broader implications to the research relative to the more common measures of democracy which rely on either contestation or inclusiveness (Nilsson, 2012). This distinction offers far more nuanced findings for each mode of termination. For example, truces significantly outperform victories on inclusiveness but not on contestation; a pattern a single composite index would not have been able to detect at all.

Nilsson's theory starts with a reframing of the independent variable itself. Rather than focusing only on outcome, like many studies such as the one conducted by Toft, Nilsson chooses to look at processes. One of his central arguments is that negotiation itself, as a sustained activity requiring former enemies to assess each other's strength, make mutual concessions, build a working relationship over time, and produces habits of cooperation that persist into the postwar period (Nilsson, 2012). Drawing on bargaining theory, Nilsson argues that negotiators only reach an agreement when they can find gains from cooperation that go beyond simply escaping a stalemate, and that doing so usually requires both sides to move past the hostile assumptions they built up about each other during the fighting. This trust-building isn't limited to the elites at the table; it has to extend to their broader constituencies who need to accept former enemies as legitimate partners for a settlement to hold. Nilsson also argues this process does not stop once an agreement is signed: implementation and renegotiation continue afterward, and it's this ongoing relationship-building, not the settlement document itself, that he credits with producing democratization (Nilsson, 2012).

Furthermore, victories and truces are theorized much differently. Nilsson argues that a decisive victor, facing no immediate threat of renewed conflict, has little incentive to risk its position through democratic

competition, and instead tends to garner legitimacy through their military primacy rather than popular support. Truces operate as a midpoint in this framework. Truces share the aspect of not having one clear winner with negotiated settlements which should reduce the incentive and capability of a government to repress a population but they lack the sustained cooperative process that negotiated settlements require. All together, Nilsson's theory yields three hypotheses: negotiated settlements should outperform truce, which should in turn outperform victory.

Nilsson tests these hypotheses using Ordinary Least Squares (OLS) regression on 90 civil war terminations between 1951 and 1995 which is a smaller and later-starting than the data set that Toft analyzes. Nilsson's dependent variables are studied at two points: two years post-conflict termination and five years post-conflict termination; considerably shorter horizons than Toft's analysis, which tracks Polity trajectories out to twenty years. The results of Nilsson's regression strongly support his first hypothesis. Negotiated settlements show significant, positive coefficients for both contestation and inclusiveness at both two and five years out, clearly outperforming both truces and victories. Support for the second and third hypotheses on truces is more mixed. Truces significantly outperform victories on inclusiveness, consistent with his prediction, but the coefficient for contestation, while positive, does not reach significance (Nilsson, 2012). Nilsson also checks whether his settlement finding is just an artifact of which cases hold up over time. To control for this variable, Nilsson reruns his models using only cases where peace lasted at least two years. The pattern still holds: negotiated settlements outperformed both truces and victories significantly. Nilsson names his disagreement with Toft in his paper, stating that his findings run contrary to her findings that victories, rebel victories specifically, are associated with higher levels of post-war democracy. Nilsson notes that his findings may not be a complete contradiction to Toft's because Toft's analysis stretches much further into the future. Nilsson's case list does distinguish government from rebel victories but his regressions collapse both into a single victory category. This means his results can compare settlement to victory overall, but can't test the government-versus-rebel split that's central to Toft's argument. So his finding that victory underperforms settlement does not really contradict Toft's more specific claim. They're answering different questions, which limits how much his results alone can push back against hers.

Madhav Joshi takes up the same question as Toft and Nilsson, but grounds it in a different theoretical premise: a sufficient precondition for democracy is when a stable balance of power exists between the government and rebel groups (Joshi, 2010). His termination categories resemble Toft's and Nilsson's, distinguishing negotiated settlement from government victory and rebel victory, with one notable omission: unlike either scholar, Joshi does not code truce or stalemate as its own category, instead folding those cases into negotiated settlement or government victory depending on how they eventually resolved. Additionally, Joshi doesn't only analyze termination types in his framework. He also builds in three more variables: whether the conflict was fought along ethnic lines, whether a biased third party intervened during the war and whether an unbiased UN mission was deployed. The first and last variables do show up in Nilsson's regression but neither reach significance.

Joshi's answer to the question of what influences post-conflict democratization is that democratization post-war rests on the balance of power between former combatants meaning each side has the ability to pursue their economic and political goals without needing the other side's cooperation (Joshi, 2010). Joshi defines "balance of power" as "the capability of former protagonists to pursue their political and economic interests unilaterally" (Joshi, 2010, p. 831). Within Joshi's framework, an actor is able to act unilaterally post-war when two conditions are met: when the war ends in a decisive military victory for one side and when that actor has little to no fear that its opposition can resume armed attack. Because unilateral capability is defined in relative terms in the paper, it is best understood as a spectrum rather than a binary. According to Joshi, at one end sits a complete military victory which favours one side over the other and at the other end there sits a negotiated settlement which is normally born out of a military stalemate. Joshi's framing leaves an open question: whether the same balance of power looks different depending on what each side actually built while the war was still underway.

Unlike both Toft and Nilsson, instead of treating negotiated settlements as a cause of democratization, Joshi treats negotiated settlements as indicators that a balance of power exists between two opposing parties in a civil war. Joshi is careful to note that settlement alone does not guarantee democratization follows. Former combatants remain wary of each other, and each side fears the other might defect from the agreement and attack. Joshi argues this "credible commitment problem" is resolved through external guarantees and power-sharing arrangements spanning political, military, economic, and territorial dimensions, giving each side a check on the other's unilateral use of power (Joshi, 2010, p. 832). This is where Joshi's answer continues to diverge from Nilsson's. Where Nilsson locates the source of democratization in the negotiation process itself, the trust built through sustained bargaining and implementation, Joshi locates it in external, institutional guarantees against defection.

When these commitment problems are resolved and the balance of power is supported by institutional arrangements and external power sharing agreements, Joshi theorizes that former protagonists become more willing to negotiate a transition toward democracy. Because elections let former government and rebel elites contest power repeatedly rather than in a single winner-take-all military confrontation, a lost election ceases to be a permanent verdict. The losing side retains a real chance of returning to power in a future cycle, which lowers the stakes of any single contest. This logic underlies Joshi's first hypothesis: that a transition toward democracy is more likely in post-civil war states following a negotiated settlement (Joshi, 2010).

Joshi tests H1 by doing a survival analysis, a statistical method that tracks how much time passes before an event happens; in this case, before a country transitions to democracy. This method can show not just whether a settlement leads to democracy, but how quickly, which is something neither Toft or Nilsson analyzes. Joshi found that civil wars ending in a negotiated settlement cut the expected survival time (the time a country spends without democratizing) by 167% compared to a government-victory case (Joshi, 2010). This is directionally consistent with Nilsson's finding that settlement outperforms both truce and victory, though the two measure different things: Joshi tracks speed to a single Polity threshold, while Nilsson tracks the magnitude of change on two distinct Polyarchy dimensions. Joshi's dependent variable

is a one-time threshold that states pass or do not pass. In Joshi's analysis a state transitions when it has changed its Polity score by 3 points or more. Toft, by contrast, tracks the full Polity trajectory over five, ten, and twenty years, and finds that settlement's initial gain reverses into authoritarianism before the twenty-year mark. Because Joshi's design does not analyze after the first threshold crossing, his finding that settlement accelerates democratization and Toft's finding that settlement's democratic gains don't hold are not strictly incompatible. They may be looking at the same underlying process, one capturing the initial shift Joshi's model is built to detect, the other capturing the reversal his model does not look at.

Joshi's logic behind his second hypothesis (H2) mirrors Toft's own explanation for the reverse pattern in her data: both argue that a defeated side's lingering capacity to resume fighting shapes the winner's incentive to share power. However, Joshi holds that rebel victors eliminate the old elite so thoroughly, through exile, execution, or asset confiscation, that they face little residual threat and correspondingly little reason to democratize, while government victors can't fully eliminate defeated rebels, who can blend into the population and regroup, giving weakened governments a stronger incentive to court former rebel constituencies (Joshi, 2010). Joshi's H2 predicts government victors should democratize faster than rebel victors; Toft's data show the reverse, with rebel victory the only termination type tied to a significant decline in autocracy (Toft, 2010). Tested on 55 government victories and 28 rebel victories, with government victory as the reference category, Joshi's H2 fails: the rebel-victory coefficient is negative in every model suggesting, if anything, a faster transition after rebel victory rather than a slower one, but it never reaches statistical significance (Joshi, 2010).

These three studies all agree that war termination type matters in some capacity for determining post-conflict prospects for democratization but they do not fully agree on how or why. Toft finds that rebel group victories produce the conditions for democratization long-term after civil wars end while settlements end up causing a long-term increase in authoritarianism. Nilsson finds that settlements outperforms victories more broadly. Joshi’s findings strengthen Nilsson’s conclusion but only measures how fast an initial shift happens, not whether it lasts, so it does not fully
resolve the disagreement with Toft.

Third Party Intervention

Joshi treats third-party involvement as separate from the balance of power itself, according to his theory it's what determines whether that balance holds once the war ends. He splits this into two variables. Biased intervention means an outside state militarily backs one side during the fighting; Joshi predicts this destabilizes any postwar balance because it hands one side an advantage rooted in outside help rather than its own domestic strength (H4) (Joshi, 2010). UN peacekeeping, by contrast, is unbiased and only enters after the war ends. Joshi argues it works by resolving the credible commitment problem directly: an outside enforcer makes defection costly for both sides, which lets former rivals trust the settlement enough to compete through elections instead of force (H5) (Joshi, 2010.)

The results split cleanly along that same line. Biased intervention shows no significant effect on democratization in any of his models, which Joshi takes as evidence that a balance of power propped up

by an outside patron does not function the same way as one the combatants built themselves (Joshi, 2010). UN missions, however, show a strong and significant effect across every model, cutting expected time to democratization by roughly the same margin as negotiated settlements (Joshi, 2010). The distinction Joshi draws here is that an intervention that shapes who wins the war does nothing for democratization, but a neutral presence that stabilizes the peace afterward does a great deal.

Wartime Governance and the Type of Conflict

Toft, Nilsson, and Joshi essentially all argue, in different forms, that what determines whether a state will democratize is how the war terminated, and Joshi extends this with the variables of ethnic conflict and third-party intervention. A different strand of scholarship rejects this premise entirely, arguing that this framing ignores what happened during the war itself Patricia Justino critiques the answer forwarded by Toft, Nilsson and Joshi because she argues that it treats civil wars as a blank slate when they are not (Justino, 2022). Justino argues that the conditions that determine what path a state will take post-civil war are created during the war itself and not negotiated into existence after the war ends. Importantly, Justino's framework does not claim to explain why a state ends up democratic, autocratic, or fragmented outright. Rather, it explains how variation in wartime governance shifts the probability that a state moves toward one of these outcomes over another (Justino, 2022).

Justino's framework identifies two conditions that jointly determine which of six post-war trajectories a country follows: stable democracy, weak democracy, stable autocracy, fragmented rule, contested autocracy, and durable disorder. The first condition, wartime institutional capacity, is shaped by territorial control and institutional reach. Groups that hold segmented territorial control, meaning they govern a defined territory without serious contestation for a meaningful length of time, are able to build lasting institutions: courts, taxation systems, local administration. Groups operating under fragmented, contested control may attempt to govern but rarely sustain it long enough for it to matter (Justino, 2022). This produces two types of armed groups: those with strong wartime institutional capacity, and those confined to disconnected territorial control, with limited institutional scope (Justino, 2022). Institutional capacity does not, by itself, determine whether a country becomes democratic or autocratic, both stable democracy and stable autocracy require strong wartime institutional capacity in Justino's framework; what separates them is a second condition.

That second condition is wartime civilian rule, defined by two variables: how armed groups distribute public goods, and how they manage violence against civilians (Justino, 2022). Justino sorts this into three categories. Under inclusive rule, armed groups extend governance and public goods to the broader population, not just to civilians already tied to the group's own networks, and generally keep violence against civilians low. This tends to produce legitimacy and draw civilians into voluntarily supporting the group. Under exclusive rule, goods and services are limited to the group's own kinship and patronage networks, and violence against civilians outside that network is targeted rather than widespread. Legitimacy exists only within the patronage network, and pockets of civilian resistance tend to persist outside it. Under violent rule, governance is maintained mainly through the threat or use of force, public

goods beyond occasional security are minimal or absent, and violence against civilians is indiscriminate rather than targeted (Justino, 2022).

An analysis of these two conditions together is what Justino uses to ground her theoretical conclusions. Strong institutional capacity combined with inclusive rule produces stable democracy; the same capacity combined with exclusive rule produces stable autocracy; combined with violent rule, it produces contested autocracy, a state with a capable center that nonetheless relies on repression to hold a periphery it has not won over. Limited institutional capacity combined with inclusive rule produces weak democracy; combined with exclusive rule, it produces fragmented rule, where authority persists at a local or subnational level without ever consolidating nationally; combined with violent rule, it produces durable disorder, where no lasting form of governance ever takes hold, alliances between local power holders break down quickly, and civilians face widespread, untargeted violence (Justino, 2022).

Toft, Nilsson, and Joshi treat termination type as producing a given regime outcome through the incentive structure it leaves the victor or negotiating parties; however, Justino's model implies that by the time a war ends, the institutional capacity that will shape the post-war state has typically already been built. A group that spent years governing a stable territory arrives at a negotiated settlement or a military victory with a different institutional endowment than a group that survived through raiding alone, even where the termination type is identical. Read this way, termination type may be less a cause of the post-war regime than a marker of the wartime governance capacity a group had already built. Justino's contribution is theoretical rather than causal-empirical. She illustrates the framework through historical cases but does not test it against other established variables in the literature. This leaves a clear division between the two strands of literature reviewed here: Toft, Nilsson, and Joshi offer large-N, cross-national tests of termination type with no measure of wartime governance built in; Justino offers a theoretically developed account of wartime governance with no comparable empirical test.

Critiques of Methodologies and Economic Variables

Virginia Page Fortna and Reyko Huang approach the same puzzle from a different angle; they argue that the field's contradictory findings are themselves a product of flawed measurement and model specification (Fortna & Huang, 2012). Their contribution is empirical rather than theoretical. They set out to correct the existing literature's methods and retest the same basic question using Polity scores.

Fortna and Huang identify four specific sources of the field's inconsistency. First, several studies measure democratization as the raw difference between a country's prewar and postwar Polity score. This conflates whatever changes occurred during the fighting with whatever changes occurred afterward, which is a problem for isolating the effect of the war's outcome specifically, since part of the change in the dependent variable happens before the independent variable, how the war ends, is even determined. Second, they note that Toft tracks democracy levels before, during, and after the war but does not run multivariate analysis on her postwar findings, meaning her results are not adjusted for confounding variables like development or prewar regime type.

Third, studies covering different time periods reach different conclusions, since findings drawn from post-1989 wars diverge from findings drawn from the broader post–World War II period, suggesting some results may simply reflect which decades a dataset happens to cover rather than a stable underlying relationship. Fourth, they raise a measurement critique borrowed from Vreeland (2008): Polity itself incorporates measures of political instability and violence into its scoring, which risks making any finding that "war affects democracy" partly circular, since a country's wartime instability can lower its own Polity score independent of any real change in its political regime. To correct for this, Fortna and Huang build a dataset of 128 civil wars ending between 1945 and 1999, measure democratization as the change in Polity score from the year the war ends to 2, 5, 10, and 20 years afterward, and run multivariate OLS regressions controlling for prewar Polity, economic development, oil exports, military size, peacekeeping, identity war, and a post–Cold War dummy variable. They test their results against both standard Polity and Vreeland's X-Polity index, which strips out the participation and instability components that make ordinary Polity's use in this context potentially tautological.

Their findings on termination type land squarely inside the disagreement Toft, Nilsson, and Joshi have already staked out. They find a short-term positive effect for negotiated settlement, a roughly 1.7-point increase in Polity score at five years out, which is directionally consistent with Nilsson's and Joshi's settlement findings. But that effect is not robust at ten or twenty years,
where the coefficient turns negative and loses significance, echoing Toft's own reversal pattern even though Fortna and Huang reach it through an entirely different design (Fortna & Huang, 2012).

On military victory, the coefficient's sign is inconsistent across time horizons and never reaches statistical significance. This pushes back on Toft's central finding that rebel victories produce a significant, sustained decline in autocracy, while it lines up with Joshi's own null result on his H2 rebel-versus-government distinction, despite the fact that Fortna and Huang use a different dataset, dependent variable, and specification than Joshi does entirely. Peacekeeping, a variable that is tested in Joshi's analysis of what affects the stability of balance of power, shows no significant positive effect once their fuller model is applied. This contradicts Joshi's argument that peacekeeping may be associated with a faster time to post-war democratization. What survives, with moderate to strong support, are the same variables that predict democratization in any society, war-torn or not: economic development is positively associated with democratization, and oil wealth is negatively associated with it, with oil-rich states landing roughly 2.6 points closer to autocracy than non-oil-rich states at ten years out (Fortna & Huang, 2012). Fortna and Huang's model also turns up one further significant result: identity war. Using their standard Polity measure, the effect of fighting along ethnic or identity lines is negative but mostly insignificant. Using X-Polity, the measure that strips out the components of Polity they consider potentially contaminated by wartime instability, identity wars show a significant negative effect, moving states about 1.07 points toward autocracy at five years compared to non-identity wars (Fortna & Huang, 2012, 806). Justino's framework has not been tested against many of these variables because it mainly exists as a typology of different post-war trajectories. Whether wartime governance would survive being tested against development and oil wealth as competing predictors, the way Fortna and Huang test settlement and victory against them, is a question the current literature has not yet answered.

The Gap and this Study's Contribution

The literature reviewed above does not agree on the answer to the puzzle of post-war democratization, nor does it agree on the best way to approach answering the puzzle. The current literature leaves two major elements of answering this question unresolved. First, Toft, Nilsson, Joshi, and Fortna and Huang disagree with each other on termination type's effect even where their models overlap, and no single methodological fix fully accounts for the disagreement. Second, none of the four termination-type studies, however broad or methodologically careful, includes any measure of what capacity either side in a civil war had already built before the war ended, which is precisely the variable Justino argues is missing, and which she has not tested.

Toft's, Joshi's, and Nilsson's mechanisms differ in their specifics, legitimacy-seeking, credible commitment, trust built through sustained negotiation, but each is a theory centered around incentive. Each asks why a government, a rebel movement, or a negotiating pair would want to liberalize, given the risks and costs of doing so. Fortna and Huang's controls, drawn from the broader democratization literature, work the same way: economic development and oil wealth are both understood as shaping a state's incentive to build democratic institutions, not its administrative capacity to do so. None of these studies asks whether the actor in question actually has the means to act on whatever incentive it has. A government or rebel movement could have every reason to hold elections or share power and still lack the courts, revenue systems, and local administration needed to carry that out credibly. This is the specific blind spot Justino's account of wartime governance identifies, and the one this paper takes up directly.

This paper builds a theory that does not treat wartime institutional capacity as a fifth competing variable alongside settlement, victory, ethnic conflict type, and third-party intervention, instead it treats wartime institutional capacity as a variable that conditions what the actors post-war can most reasonably accomplish. Termination type sets the political terms a post-war state is supposed to operate under; unbiased third-party intervention determines whether an outside actor can force both sides to actually stick to those terms; wartime institutional capacity determines whether the state has the administrative machinery to carry those terms out at all. Read this way, the disagreement between Toft, Nilsson, Joshi, and Fortna and Huang on what termination type does not necessarily evidence that one of them measured it wrong or even has a weakened theory. It may be evidence that termination type's effect depends on a variable none of them measured, and that averaging across cases with very different wartime institutional histories is enough on its own to produce the kind of unstable, dataset-dependent findings this literature has already produced. The following section develops this argument as a formal theory.

THEORETICAL FRAMEWORK AND HYPOTHESES

The literature reviewed above disagrees on what termination type does to post-war democratization, and none of the four termination-type studies measures what either side in a civil war built before the war ended. This section builds a theory that adds that missing piece: wartime institutional capacity. The argument is not that termination type does not matter. Toft, Nilsson, and Joshi all find real effects tied to

how a war ends, and Fortna and Huang find at least a short-term effect for settlement even after tightening the model. The argument forwarded by this theory is that termination type's effect depends on something none of them measured, and that this dependency is a possible reason the four studies keep landing on different answers. A negotiated settlement, as Joshi describes it, typically involves power-sharing arrangements across political, military, economic, and territorial dimensions, along with a path to elections. A government or rebel victory does not normally carry these same shared arrangements, but the winning side still has to establish governance without the other side's formal cooperation. Every one of these tasks requires an administrative apparatus that already exists, or has to be built from nothing. Toft's, Nilsson's, Joshi's and Fortna and Huang's models do not ask whether that apparatus exists. They ask what type of ending occurred and treat every case of a given type as functionally identical going into the post-war period. This is the specific point at which wartime institutional capacity becomes relevant: it is a measure of whether that administrative apparatus already exists by the time the war ends.

Wartime institutional capacity is defined here the way Justino defines it: the extent to which an armed group, government or rebel, held a defined territory without serious contest for a sustained period during the war, and used that control to build lasting administrative structures, courts, taxation and local government, as opposed to territorial control that was too fragmented, too contested, or too short-lived to produce anything beyond the capacity to keep fighting. This is treated as a matter of degree. Real cases will fall somewhere between a group that built extensive parallel governance across a large territory and a group that held no territory at all.

Wartime institutional capacity is not itself a single observable quantity, but its components point toward measurable proxies already used in adjacent literatures on rebel and wartime governance. Some candidates for ways to measure this variable include: duration of uncontested territorial control, presence of systematic taxation or revenue extraction and the existence of a functioning judicial or dispute-resolution apparatus, courts, tribunals, or a codified system for resolving property and criminal disputes. None of these proxies are sufficient on their own. For example, a group could tax a population without adjudicating disputes, or hold territory for years without building either. Future work could combine several of these indicators into a composite measure, for instance coding a group as high capacity only where it exhibits sustained taxation and a functioning judiciary over a multi-year period of largely uncontested territorial control, and as low capacity otherwise.

This theory does not use Justino's second variable, the distinction between inclusive, exclusive, and violent civilian rule. That variable measures whether a population saw a group's wartime governance as legitimate. Institutional capacity measures whether a group had governing capacity to begin with, independent of whether the population approved of it. The outcome this theory is built to explain, whether a state can actually carry out the terms its own termination type assigns it, is closer to the second question than the first. Civilian rule and legitimacy may matter for a related but separate question, why some capable states still face resistance from the population they govern, which is closer to what Justino's "contested autocracy" category already describes, and which this theory does not attempt to explain (Justino, 2022, p. 11). Institutional capacity, built during the war, is what a state has on hand at the

moment termination type hands it a set of political tasks to perform. If a group built strong institutions during the war, it enters the post-war period with courts, revenue collection, and local administration already staffed and functioning. When a settlement then asks it to share power, hold elections, or divide territory with a former enemy, the state has the machinery to actually carry that out, because the machinery already exists and only has to be adapted, not invented. If a group never built that capacity, the same settlement asks it to do all of this from a standing start, at the exact moment the state is also managing a fragile peace with a former enemy. This is a much harder task, and it makes the conditions of settlement much harder to fully meet. This leads to the first hypothesis:

H1: Negotiated settlements where both sides had high wartime institutional capacity are more likely to sustain democratic gains than settlements where either side had low institutional capacity.

This offers a specific account of the pattern both Toft and Fortna and Huang report: an early gain in democracy scores after settlement, followed by a decline or reversal by the ten-year mark. The early gain plausibly comes from the settlement's own terms; elections happen because the agreement mandates them. The later decline plausibly comes from the absence of institutional capacity needed to sustain those terms once the initial push, whether from the agreement's own momentum or from departing third parties, fades. Toft attributes the reversal to unaddressed incentive structures. This theory offers an additional, compatible explanation: even where the incentive to democratize exists, the capacity to administer democracy may not. If this is right, the reversal itself should be conditional on capacity, not just in its magnitude but in its timing:

H2a: The decline in democracy scores following negotiated settlements will be smaller in cases where both sides had high wartime institutional capacity than in cases where either side had low capacity.

H2b: The decline in democracy scores following negotiated settlements will occur later in cases where both sides had high wartime institutional capacity than in cases where either side had low capacity.

The same logic applies to victory. A victor that built real institutions during the war can govern the country afterward through those institutions. A victor that won mainly through force, without building comparable capacity, is likely to govern through whatever it does have, which is the demonstrated capacity for force which may explain lower democracy scores for victories. This also offers a possible account of why Toft finds government victors increasing repression while rebel victors decrease it: if rebel groups in her sample built more governing capacity during the war than the governments they defeated had left functioning, capacity rather than identity could be doing at least part of the work her incentive-structure argument assigns to the victor's identity.

H3a: Following a decisive victory, high wartime institutional capacity on the winning side is associated with governing through existing administrative institutions

H3b: Low institutional capacity is associated with governing through coercion, regardless of whether the government or the rebels won.

Joshi's theory argues that democratization depends on a stable balance of power between former combatants, meaning each side can pursue its interests without the other's cooperation, or is checked by the other's ability to resume fighting. Institutional capacity affects this balance in a way Joshi's model does not capture. A side with strong wartime institutions has options a side without them does not: it can administer territory, collect revenue, and maintain a government function independent of military strength, which gives it a form of power that does not depend on its ability to fight. A side with only fighting capacity has fewer options if it loses the military balance, since it has no separate administrative base to fall back on. This suggests institutional capacity could affect how stable a given balance of power actually is, a factor sitting alongside the third-party intervention and ethnic conflict variables Joshi already tests, without replacing either of them. This paper does not test this interaction and suggests it as a direction for future work. It also suggests that capacity should matter more where the postwar tasks are more demanding of joint execution, which settlement, with its shared power-sharing and electoral requirements, is, and victory, which requires no comparable cooperation, is not:

H4: Wartime institutional capacity has a larger effect on post-war democratization following negotiated settlements than following victories.

This theory applies to civil wars ending in a clearly coded settlement or a clearly coded victory. It does not apply to stalemates or unresolved ceasefires, which do not produce a settled governing arrangement for institutional capacity to act on.

If wartime institutional capacity varied substantially across the cases in Toft's, Nilsson's, Joshi's, and Fortna and Huang's datasets, and none of the four models measured it, then each study's finding on settlement or victory is an average taken across cases this theory predicts should behave differently from one another. That offers one account of why studies using overlapping cases and overlapping time periods, a problem Fortna and Huang themselves raise about the field, keep producing coefficients that shift in sign and significance. This theory is not an argument that claims to fully explain the field's disagreement. Fortna and Huang's own controls, economic development and oil wealth in particular, already account for a meaningful share of the variation the field has struggled to explain, and this theory does not claim to outperform those variables, only to add one the field has not yet tested.

LIMITATIONS

This paper has several limitations that affect how its theory should be read. The most basic is that the framework is purely theoretical. Its four hypotheses are directional predictions, not findings, and the reinterpretations of Toft's, Nilsson's, Joshi's, and Fortna and Huang's results offered above are plausible readings rather than demonstrated explanations. Wartime institutional capacity has also not yet been operationalized.

A second set of limitations concerns causal identification. Institutional capacity may not be independent of termination type: groups that hold territory and govern it for long periods may be more likely to win outright, or to extract a settlement from a position of strength, in which case capacity and termination type would be correlated rather than separable. Capacity may also be correlated with the very variables Fortna and Huang find matter most, such as economic development and oil wealth, since richer or resource-rich territories may be easier to tax and administer (Fortna & Huang, 2012). Disentangling these relationships is a task for empirical work, and this paper does not claim to have done so. In addition, the theory's hypotheses characterize a settlement as favorable only where both sides had high capacity, an assumption that treats the two parties symmetrically and may obscure cases where one side's capacity matters far more than the other's.

Third, the paper's scope is deliberately narrow. It addresses only settlements and decisive victories, setting aside stalemates and ceasefires, which Toft also treats cautiously because so few wars end that way (Toft, 2010). It also sets aside Justino's second variable, wartime civilian rule, and therefore does not explain why capable states may still face resistance from the populations they govern (Justino, 2022). Its outcome measures also inherit the problems of the literature it engages: if democratization is measured with Polity, the instability and circularity concerns raised by Fortna and Huang and by Vreeland apply to any test of this theory as well (Fortna & Huang, 2012; Vreeland, 2008).

Finally, this paper does not consider several other explanations that may shape post-civil war democratization. Cultural and societal factors, such as political culture, social cohesion, and the legacies of earlier regimes, may influence whether democratic institutions take root, independent of wartime administrative capacity. Likewise, the paper treats third-party involvement only in the forms Joshi tests, biased wartime intervention and UN peacekeeping (Joshi, 2010), and does not address continued foreign intervention after the war, such as sustained aid, diplomatic pressure, or external support for particular factions, which may either supply or substitute for domestic administrative capacity. These competing explanations do not undermine the central argument of the paper but a full empirical test should account for them, and they may interact with capacity in ways this paper has not anticipated.

CONCLUSION

This paper argues that wartime institutional capacity offers one answer to why the post-civil war democratization literature keeps producing conflicting results. Termination type has been treated across the field as a self-contained cause, when it actually hands a postwar state a set of political tasks, power-sharing, elections, governing a defeated population, that the state must then carry out either by adapting administrative machinery it already built during the war, or by constructing that machinery from nothing while simultaneously managing peace. The literature supports this reframing: Toft, Nilsson, and Joshi find real but conflicting patterns tied to how wars end, disagreeing sharply on which side of a decisive victory is more likely to liberalize, while Fortna and Huang's corrections narrow this instability

without fully resolving it. Justino's account of wartime governance supplies the missing variable this paper formalizes into a testable theory.

The theory remains untested, and its four hypotheses are directional rather than confirmed. It does not claim to outperform variables like economic development that already explain real variation in this literature, only to add one the field has overlooked. The clear next step is operationalizing wartime institutional capacity and testing it against the same cases this literature has already studied.

If the theory is strengthened, it carries a practical weight beyond academic disagreement: policymakers and international actors currently draw on this literature to decide how to structure interventions, settlements, and postwar support. A field unable to agree on whether victory or settlement better predicts democratization offers unstable guidance for these decisions. If wartime institutional capacity turns out to matter as much as this theory suggests, the practical question shifts from which kind of war ending to encourage toward whether the sides fighting have built any governing capacity worth preserving once the war ends.

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