Do Major-Power Interventions Encourage the Onset of Civil Conflict? A Structural Analysis

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What is the impact of major-power intervention on civil-war onset? A considerable hurdle to answering this question is gauging expectations about future intervention on the eve of conflict. We tackle this challenge by developing a unified theoretical and empirical model of civil-war onset and subsequent intervention. Our model allows for strategic interdependence among interveners, and our empirical strategy enables estimation of intervention expectations from equilibrium behavior. We fit the model to civil war and intervention data from the second half of the twentieth century and find that major-power intervention is primarily characterized by strategic complementarities—for example, cost sharing among allies or competition for control among rivals—rather than free-riding incentives. Through counterfactual experiments, we show that commitments to decreased intervention would raise the risk of civil war worldwide, whereas increased intervention would have little effect. Our results suggest that coordination among major powers is maximally deterring civil conflict.

Since the 1950s, over two in five countries have experienced some type of civil conflict.[1] These wars not only cause immediate, staggering death and destruction—more than 6 million battle deaths in sum—but they have far-reaching consequences: even moderately sized civil conflicts reduce income per capita by 20%, life expectancy by one year, and educational enrollment by 2.5% (Gates et al. 2012; Lai and Thyne 2007). Third parties frequently intervene militarily in civil wars, and interventions by major powers are especially prominent: close to half of civil conflicts involve a major power, and a third feature intervention by multiple powers (Regan 2002). Even humanitarian interventions often require coercive force to be effective.

While scholars have amassed considerable evidence that rebel groups indeed anticipate the responses of potential interveners,[2] the impact of expected intervention, or lack thereof, on the likelihood of civil war is not well understood. Existing theoretical and empirical assessments are inconclusive.[3] And historical examples abound suggesting that interventions could both encourage and discourage the onset of civil conflict. For instance, Bosnian leaders openly admit that they anticipated Serbian retaliation and military dominance on the eve of civil war, but they repeatedly broke cease-fires in hopes of provoking wartime atrocities and an expected US intervention (Kuperman 2008). In contrast, the Brezhnev doctrine asserted the right of the Soviet Union to

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1. More than two in five countries have experienced a civil war with at least 25 battle-related fatalities, and more than one in five countries, one with at least a thousand.

2. See Carroll (2018), Cetinyan (2002), Cunningham (2016), Favretto (2009), Kuperman (2008), Kydd and Straus (2013), Meirowitz et al. (2019), Rauchhaus (2009), and Thyne (2006).

3. Scholars have shown that the effect of anticipated intervention may depend on the policy goals of the third party (Favretto 2009; Kydd and Straus 2013), whether interveners also mediate peace talks (Hörner, Morelli, and Squintani 2015; Meirowitz et al. 2019), and the expected direction of outside military support (Carroll 2018; Cetinyan 2002; Cunningham 2016).

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intervene in other communist countries whenever counterrevolutionary forces threatened socialism, and, in fact, Warsaw Pact membership perfectly predicts the absence of civil conflict during the Cold War (Fearon and Laitin 2003).

At the heart of this puzzle is understanding rebels' expectations about third-party intervention. Drawing on insights from the theory of principal-agent problems, previous studies caution that even interventions with purely humanitarian motives may be misguided as they can create perverse incentives for rebel groups who otherwise would not mobilize for civil war (Kuperman 2008). Weak groups, in particular, may rebel only if they expect a third party to intervene and alleviate the costs of the eventual conflict (Narang 2015; Spaniel 2018). The Bosnian case offers a notable example of such incentives at play.

Reliably estimating intervention expectations, however, poses three conceptual and methodological challenges. First, it is difficult to proxy for these expectations, and previous attempts may suffer from reverse causality—for example, the decision to station US troops in a country (Cunningham 2016) may be informed by its low latent risk of civil unrest. Furthermore, direct measures of expectations are typically based on evidence from countries that have experienced civil war (Carroll 2018; Gent 2007; Kuperman 2008), which raises sample selection concerns. For example, the observed frequency of US intervention in foreign civil conflicts could be explained through two channels. On one hand, the United States may have a high underlying propensity for intervention due to its position on the international stage. On the other, if US interventions are on average favorable to rebel groups, then civil wars may be more likely to erupt when rebels expect the United States to intervene.

Second, even unilateral interventions do not occur in isolation. Third parties also anticipate each other's actions due to the possibility of strategic interdependence. If interventions are like public goods, with strong free-riding incentives, intervention by one state could discourage intervention by others—that is, interventions could be strategic substitutes (Olson and Zeckhauser 1966; Steinwand 2015). On the other hand, competition for control among rivals (Grossman and Helpman 1994; Hirshleifer 1989) or tactical complementarities among allies could lead intervention by one to encourage others to intervene as well—hence, interventions could be strategic complements. A major power's intervention decisions, therefore, may have not only a direct effect on civil-war onset via their influence on local actors but also an indirect effect by encouraging or discouraging intervention by other powers. Understanding the net impact of expected intervention requires disentangling these effects.

Third, if strategic spillovers are sufficiently strong, multiple intervention outcomes could be plausible. For example, with strategic complements between two potential interveners, both or neither intervening could constitute equilibrium behaviors—as in a stag-hunt game—with disparate consequences for rebel groups. Ignoring how this multiplicity factors into relevant actors' expectations may effectively result in omitted-variable bias.

In this paper, we develop a unified theoretical and empirical model of civil-war onset and major-power intervention. The model has three key features. First, interaction is sequential: rebel groups first decide whether or not to instigate a conflict, and major powers subsequently decide whether or not to intervene. When rebels choose to start a civil war, their expectations about foreign intervention are derived from (subgame perfect) equilibrium behavior. Second, the intervention (sub)game is multilateral, with potential spillovers among major powers influencing their intervention decisions. Third, rather than assuming away multiplicity of plausible outcomes with ad hoc restrictions—for example, a selection rule or specific timing of moves—we approach it as an empirical question. Our estimation strategy flexibly controls for the empirical determinants of coordination in equilibrium selection. We fit our model to civil war and major-power intervention data from the second half of the twentieth century (Regan 2002) using a relatively new maximum simulated likelihood procedure for games with multiple equilibria (Bajari, Hong, and Ryan 2010). Thus, we estimate expectations about major-power intervention from observed equilibrium behavior. Two major results emerge.

First, we examine strategic spillovers among major powers. For each major-power pair, we estimate a parameter describing whether the expectation of intervention by one encourages or discourages intervention by the other. A priori, it is not obvious which effect should dominate in the data: rivals may seek to avoid direct confrontation or to compete for control, and allies may free ride on each other's efforts or face cost-sharing incentives. We find that major powers generally have a stronger preference for intervening if they expect others to do so as well. This suggests that cost-sharing incentives dominate among allies (e.g., United States and United Kingdom) and competition dominates among rivals (e.g., United States and China). Nonetheless, we find one important exception: strong free-riding incentives exist between France and the United States. Notably, this strategic substitution effect between the two key allies is present only during the Cold War, which is consistent with France maintaining an exclusive sphere of influence in Africa during this period when French and US goals were more aligned (Schraeder 2000).

Second, we use our fitted model to quantify the impact of expected intervention on civil-war onset. The main advantage of our unified theoretical and empirical approach is that it is ideally suited for such counterfactual policy experiments as it accounts for the indirect effects of interventions due to strategic interdependence. For instance, we find that US interventions tend to directly encourage rebels to instigate conflict, as in the Bosnian case. However, as noted, US interventions encourage UK interventions, and our results indicate that the latter tend to discourage civil-war onset. Thus, the direct and indirect effects of intervention can move in countervailing directions.

To determine which effect dominates substantively, we first consider the consequences of unilateral commitments by each major power to either always or never intervene in civil conflicts. Specifically, we fix the behavior (intervene or not) of a major power, and we use our estimated model to simulate the optimal responses of the other powers and predict whether rebels would initiate a conflict. Although previous work has examined the effects of expected US and Russian interventions via proxies, it has been difficult to do so for the remaining major powers. We find that unilateral commitments to abstention would significantly increase the risk of civil war worldwide by 5–10 percentage points. On the other hand, commitments to intervention would have little effect, with one exception: if the United States could commit to always intervene, the likelihood of peace would decrease by 10 percentage points.

Our results thus confirm the logic underlying Kuperman's (2008) case studies on a wider scale: US commitments to intervention can hinder peace. In contrast, previous cross-national analyses emphasize the conflict-deterrent potential of foreign interventions—especially those from the United States or Russia (Cunningham 2016; Fearon and Laitin 2003). One reason for this is that, by proxying for Russian and US interventions using Warsaw Pact membership or importance in the US security hierarchy, respectively, these studies are likely picking up expected interventions where the two powers have the greatest incentive to maintain the status quo, which misses interventions potentially more favorable to rebels. To better highlight this, we directly compare these proxies with our model's equilibrium intervention expectations. Both proxies correlate with our model's estimates in expected directions, but they miss substantial heterogeneity. For example, we find that the United States is considerably more likely to intervene in conflicts when they occur in Iraq or North Korea than in Sri Lanka or Bangladesh, even though both groups of countries have identical US security hierarchy scores. That is, the hierarchy scores fail to capture potential US interventions to disrupt the status quo, which can encourage conflict.[4] More broadly, this exercise illustrates how structural modeling and estimation can help political scientists measure unobserved quantities of substantive interest.

Finally, our counterfactuals suggest that coordination among major powers has been maximally successful at deterring civil conflict. To explore this further, we quantify the full range of outcomes that are plausible given equilibrium behavior, and we show that, indeed, observed coordination among major powers has effectively deterred civil wars to the fullest extent possible. Overall, our results uncover little benefit—and considerable risk of unintended consequences—from new policies or international agreements aimed at discouraging civil-war onset via major-power intervention.

RELATED LITERATURE

Critics of foreign intervention in civil wars frequently point to its potential to inadvertently encourage conflict (e.g., Kuperman 2008). As noted, existing efforts at quantifying these unintended consequences rely mostly on proxies for beliefs about foreign support. The list of proxies is long and includes several dyadic measures between potential conflict countries and outside interveners (Cetinyan 2002; Cunningham 2016; Fearon and Laitin 2003; Salehyan, Gleditsch, and Cunningham 2011; Thyne 2006). What separates this paper from those referenced here is that our approach does not require measures of intervention expectations, which are instead derived from equilibrium behavior.

Alternatively, Carroll (2018) estimates intervention expectations using a machine learning approach. He trains a predictive model of intervention behavior on the subsample of countries where civil war has occurred and extrapolates to countries with no conflict. Strategic selection (Signorino 2002) makes the extrapolation effort difficult, however. With such data, empirical analyses cannot untangle, for example, whether a foreign power intervenes frequently because it has small intervention costs or because its interventions encourage rebellion.

In addition, formal models illustrate how bargaining between rebels and governments may more easily break down when the two sides anticipate an outside party (Cetinyan 2002; Kydd and Straus 2013; Meirowitz et al. 2019; Powell 2017; Spaniel 2018). Existing work mainly considers a single intervener, which rules out the possibility of spillover effects from intervention decisions. Spillovers are common, however,

4. Indeed, when we extend our analysis to account explicitly for the direction of support of interventions, we find (i) that the United States is more likely to support rebels than governments and (ii) that, conditional on either direction of support, US interventions tend to directly encourage rebellion (although the effect for government-sided interventions is not statistically significant). This suggests that, conditional on direction, the United States uses tactics or adopts bargaining positions perceived as relatively favorable by rebels.

in reduced-form empirical analyses of why foreign powers intervene in civil wars (Findley and Teo 2006), and they also appear in the context of foreign aid and investment (Lebovic 2005; Steinwand 2015). Gent (2007) conducts a structural analysis of civil-war intervention and finds evidence of spillovers using a two-player, sequential model of interventions. These effects are estimated using the subsample of countries that have experienced civil conflict, however, which again raises selection concerns. Moreover, we estimate spillovers as major-power-pair fixed effects rather than as functions of alliance or rivalry measures, which results in a more general specification.

Finally, our analysis addresses how spillover effects can create multiple plausible predictions or equilibria. Without additional structure, multiplicity can lead to ill-defined likelihood functions, mistaken inferences, and incorrect substantive effects.[5] As a workaround, some studies impose refinements or assume that covariates perfectly determine equilibrium selection.[6] Instead, we approach multiplicity as an empirical question by modeling and estimating equilibrium selection—that is, how major powers coordinate their intervention decisions—as a latent probability distribution over all possible equilibria.

A MODEL OF CIVIL WAR AND INTERVENTION

A rebel group R chooses whether to embark on a civil conflict ($a_R = 1$) or not ($a_R = 0$). If R launches a civil war, then major power $m = 1, \dots, M$ decides whether to intervene ($a_m = 1$) or not ($a_m = 0$) in the conflict. Intervention decisions are made simultaneously. Throughout, we use m to denote an arbitrary major power and $i = R, 1, \dots, M$ to denote an arbitrary player of the game.

Let $a = (a_R, a_1, \dots, a_M)$ denote an action profile. In this game, the set of feasible action profiles is $\mathcal{A} = \{a \in \{0,1\}^{M+1} : \text{if } a_R = 0, \text{ then } a_i = 0 \ \forall\, i\}$. That is, either (i) the rebels instigate war and any combination of major-power intervention is possible, or (ii) the rebels choose peace and no intervention can occur.

Payoffs are common knowledge and take the following form:

$$v_i(a; w, \theta, \epsilon_i) = u_i(a; w, \theta) + \epsilon_i(a). \tag{1}$$

Thus, i's payoff from action profile $a \in \mathcal{A}$, $v_i(a; w, \theta, \epsilon_i)$, is composed of two terms. The first is a function $u_i(a; w, \theta)$ that captures the systematic features of i's payoff. This function is parameterized using covariates w and coefficients θ to be estimated. We write $w = (w^R, w^I)$ to refer to covariates that are relevant to the rebels (w^R) and interveners (w^I). The second component of i's payoff is a random profile-specific shock $\epsilon_i(a)$. This term captures idiosyncratic features of the game that are common knowledge to the players but unobserved by the analyst. We assume that $\epsilon_i(a)$ is drawn from the standard normal distribution and is independent across profiles and players, a common assumption in empirical analyses of this class of games.[7]

We endow the rebels' systematic payoff with the functional form:

$$u_R(a; w^R, \theta) = a_R\left(x^R \cdot \beta + \sum_{m=1}^{M} a_m[\gamma_m + z_m^R \cdot \gamma_0]\right). \tag{2}$$

This payoff structure has a natural interpretation. First, we normalize the payoff from not launching a civil conflict to zero. Thus, $x^R \cdot \beta$ represents a baseline expected relative benefit of initiating conflict, which we parameterize using standard country-level covariates x^R from the civil war literature (e.g., mountainous terrain, democracy, GDP per capita). The term $\gamma_m + z_m^R \cdot \gamma_0$ describes how m's intervention decision affects R's expected war payoff. Here, γ_m is a baseline effect—specific to power m—that is adjusted by $z_m^R \cdot \gamma_0$. Thus, the model incorporates standard dyadic measures z_m^R of intervener characteristics (e.g., colonial history, alliance relationship) that describe whether an intervention is likely to support or undermine rebels' efforts. For example, rebels may expect substantial resistance from m's intervention if m has a security alliance with the host government. Likewise, rebels may have baseline expectations concerning major powers' attitudes toward insurrection and the amount of resources they are likely to expend, which are captured by the major-power fixed effects, γ_m.

We pursue a similar strategy with the systematic payoffs of the major powers:

$$u_m(a; w^I, \theta) = a_m\left(x^I \cdot \phi_m + z_m^I \cdot \chi + \sum_{m' \neq m} a_{m'} \delta_{m',m}\right). \tag{3}$$

The sum $x^I \cdot \phi_m + z_m^I \cdot \chi$ represents major power m's expected net benefit of intervening in the civil war, absent intervention by other major powers. This payoff is parameterized using standard country-level covariates x^I as well as dyadic

5. For discussions in the econometrics literature, see Bajari et al. (2010) and Tamer (2003).

6. See, e.g., Crisman-Cox and Gibilisco (2021) and Jo (2011). In the political methodology literature, other approaches include tweaking the underlying data-generating process so that simultaneity operates through latent variables (or errors) or lagged outcomes—see Franzese, Hays, and Cook (2016) for a thorough summary.

7. Another typical choice is the type I extreme value distribution. However, due to its symmetry and numerical performance in simulations, the standard normal distribution is better suited to our estimation strategy.

covariates z_m^I from the intervention literature. For example, major powers may have a greater incentive to intervene in democracies or in countries that are closer to their own borders. The final component of m's systematic payoff from intervention captures the strategic nature of major-power interactions. For each pair of major powers, $\delta_{m',m}$ measures the extent to which, on average, intervention efforts by m and m' are strategic complements or substitutes. If $\delta_{m',m} > 0$, then m has a stronger preference for intervention if it expects m' to intervene as well. In this case, intervention efforts are complementary, as when rivals compete for control or allies face cost-sharing incentives. If $\delta_{m',m} < 0$, then m has a weaker preference for intervention if it expects m' to intervene, as in the case of free riding among allies or avoidance of direct confrontation among rivals.

For the sake of parsimony, we impose two common restrictions on the coefficients in equation (3).[8] First, we allow for major-power fixed effects in the baseline payoff from intervening in a civil war, but we pool coefficients associated with nonconstant covariates.[9] Second, we impose symmetry on the spillover effects—that is, we assume that $\delta_{m,m'} = \delta_{m',m}$ for all pairs of major powers m and m'. Previous empirical analyses of intervention decisions do not consider intervener fixed effects, and they impose more restrictive forms of symmetry on spillovers by letting them depend only on covariates such as rivalry or alliance measures (e.g., Findley and Teo 2006; Gent 2007; Regan 1998). Our specification is therefore more general than the typical approach. Indeed, as discussed below, our model, while parsimonious, can accommodate remarkable heterogeneity in the net substantive effects of intervention decisions on both rebels' and major powers' incentives.

A strategy for the rebels is a decision $\sigma_R \in \{0, 1\}$ to start a civil war or not. A strategy for major power m is a probability $\sigma_m \in [0, 1]$ of intervention in the event of a conflict.[10] Let $\sigma = (\sigma_R, \sigma_1, \ldots, \sigma_M)$ denote a generic strategy profile and $\sigma(a)$ denote the probability that action profile $a \in \mathcal{A}$ is played under σ. Because the game is partly sequential with complete information, we focus on subgame perfect Nash equilibria—equilibria hereafter. Collect player i's payoffs in the vector $v_i = (v_i(a))_{a\in\mathcal{A}}$, and let $v = (v_R, v_1, \ldots, v_M)$. We denote by $\mathcal{E}(v)$ the set of all equilibria given payoffs v.

As noted, there may be multiple equilibria of the game—that is, given payoffs v, the set $\mathcal{E}(v)$ may have more than one element. Unaddressed, multiplicity poses serious challenges for estimation due essentially to omitted-variable bias: while the players know which equilibrium they are playing, this is unobserved by the analyst. Though we could endow our model with additional structure or refinements to guarantee uniqueness, there is little empirical justification for such ad hoc restrictions. For example, assuming that major powers move sequentially in the intervention subgame would remedy the problem, but it would require imposing a specific order of play among potential interveners. Furthermore, there is little hope that the data would help identify such order: we cannot observe the timing of interventions in countries with no civil war, and in observations with conflict the power with the first opportunity to intervene may be different from the power that ultimately intervenes first.

In contrast, we take a more flexible approach by treating multiplicity as an empirical question. When faced with multiple plausible outcomes, as Schelling (1960) describes, players rely on features of their environment or history to form expectations about which equilibrium is likely to attract the most attention, thereby becoming focal and leading them to rationally play it. We model this selection process simply as a probability distribution over the set of equilibria, which we estimate. Note that a key advantage is that this accommodates the possibility that our actors play different equilibria across observationally equivalent scenarios. Specifically, given payoffs v, $F(\sigma; v, \lambda)$ denotes the probability that equilibrium $\sigma \in \mathcal{E}(v)$ is played. The parameter vector λ is to be estimated, and it captures the relative importance of different factors determining equilibrium selection. These factors may depend on payoffs v and equilibrium behavior σ—for example, the expected number of interveners or the preferences of major powers. We discuss in detail our specification of $F(\sigma; v, \lambda)$ in appendix B.1 (apps. A–F are available online).

It is now straightforward to compute the likelihood of observing action profile $a \in \mathcal{A}$ given covariates w, payoff parameters $\theta = (\beta, \gamma, \phi, \chi, \delta)$, and equilibrium selection parameters λ:

$$P(a; w, \theta, \lambda) = \int \left[\sum_{\sigma \in \mathcal{E}(v(w,\theta,\epsilon))} F(\sigma; v(w, \theta, \epsilon), \lambda)\sigma(a) \right] g(\epsilon) d\epsilon, \tag{4}$$

where g is the joint probability density function of $\epsilon = (\epsilon_R, \epsilon_1, \ldots, \epsilon_M)$ described above. The likelihood in equation (4) has a natural interpretation. First, the covariates, payoff parameters, and realized payoff shocks determine the players' payoff vector $v(w, \theta, \epsilon)$, which in turn determines the set of

8. These restrictions help reduce the dimensionality of the model but are not necessary for identification.

9. The first coordinate of x^I is a constant—that is, $x^I = (1, x_2^I, \ldots, x_K^I)$, where K denotes the total number of covariates. Given $\phi_m = (\phi_{m,1}, \ldots \phi_{m,K})$, we set $\phi_{m,k} = \phi_{m',k}$ for all $k \geq 2$ and all m and m'.

10. Our assumption regarding the distribution of payoff shocks implies that equilibria with proper mixing by the rebels occur with probability zero.

equilibria $\mathcal{E}(\nu(w, \theta, \epsilon))$. Then $\sigma(a)$ is the probability of observing action profile $a \in \mathcal{A}$ conditional on equilibrium $\sigma \in \mathcal{E}(\nu(w, \theta, \epsilon))$ being played. Second, the selection parameters determine the probability $F(\sigma; \nu(w, \theta, \epsilon), \lambda)$ that equilibrium σ is selected from $\mathcal{E}(\nu(w, \theta, \epsilon))$. Finally, equation (4) integrates out the unobservables (from the researcher's perspective): the realized equilibrium and payoff shocks.[11]

Before describing our empirical strategy, we remark on several key features of our model. First, we do not explicitly model war outcomes in players' payoffs, in large part due to data availability regarding conflict evolution, resources expended, and terms of settlement, which limits our ability to reliably estimate these outcomes as a function of intervention decisions. Nevertheless, we interpret equations (2) and (3) in standard discrete-choice fashion as capturing players' expected net benefit from conflict participation. This enables us to organize within a coherent and familiar theoretical framework covariates that previous studies have shown are robustly associated with civil-war onset and intervention. For instance, we find that major powers' net benefit from intervention decreases with distance to conflict, which is consistent with, for example, increasing deployment costs and reduced domestic interest.

Second, for our empirical analysis, we use Regan's (2002) data, where interventions are ascertained using news reports, as in other event-data studies, which raises concerns about potential measurement error. In particular, states may have strategic incentives to hide the timing, scope, and scale of their interventions. To mitigate this, we take a coarser look at the data and focus on whether participation at all by an intervener can be discovered. Accordingly, rather than explicitly modeling the scale or timing of intervention efforts, we model only the high-level strategic decision by a major power to participate or not in an ongoing civil conflict.[12] This allows us to adopt a more flexible model specification that accounts for important and often-overlooked unobserved heterogeneity in both major powers' propensity for intervention and their influence on rebels' expected benefits from war. Nonetheless, to examine the sensitivity of our results to this key modeling choice, we conduct in appendix E a series of robustness exercises concerning alternative potential codings of the data. As discussed below, our main findings are virtually unchanged.

Third, we do not model the direction of support of an intervention. Again, this is motivated by data limitations: some states appear to intervene on multiple sides of a conflict—for example, Regan's (2002) data record Russia as supporting both sides of the Congolese civil war—and others overwhelmingly favor a particular side—for example, France intervenes in 14 civil conflicts but supports the rebels only once.[13] As noted, to account for this important feature of a major power's strategy in our baseline analysis, we estimate the expected characteristics of an intervention using fixed effects and observable dyadic covariates such as alliance relationships. Indeed, we find that intervention by a major power that, for example, has a security alliance with the rebels' home government tends to reduce rebels' expected benefits from civil war. Likewise, we find that expected interventions by France and the United Kingdom tend to decrease rebels' war payoffs more than other major powers, and table A2 illustrates that French and British interventions are generally in support of governments over rebels. Thus, while not explicitly modeled, our baseline analysis successfully accommodates observed patterns of major-power support in civil conflicts. Furthermore, in appendix F, we estimate an extension of our model that endogenizes the direction of support of interventions, and our main results remain.

Fourth, we do not model the actions of the host government. Empirical studies of civil-war onset have overwhelmingly focused on the incentives for rebels to launch insurgencies (Blattman and Miguel 2010; Fearon and Laitin 2003; Lacina 2014). We similarly prioritize the rebels' decision problem and approximate the host government's behavior through covariates. For example, democratic governments may be more likely to share power with rebels, and high-income countries may more easily repress the local population. We estimate net effects of such country characteristics on rebels' incentives to initiate a conflict.

Finally, our approach to multiplicity is novel in political science and deserves further discussion. The likelihood in equation (4) shares features of more familiar selection and mixture models. The term $F(\sigma; \nu(w, \theta, \epsilon), \lambda)\sigma(a)$ resembles the likelihood of a selection model, where the within-sample likelihood, $\sigma(a)$, is weighted by the probability of selection into the sample, $F(\sigma; \nu(w, \theta, \epsilon), \lambda)$. And the sum $\sum_{\sigma \in \mathcal{E}(\nu(w,\theta,\epsilon))} F(\sigma; \nu(w, \theta, \epsilon), \lambda)\sigma(a)$ can be viewed as a finite mixture. This mixture arises endogenously from the set of equilibria, $\mathcal{E}(\nu(w, \theta, \epsilon))$, which depends on residual unobserved heterogeneity, ϵ, explicitly accounted for in equation (4).

11. Because $\epsilon_i(a)$ is independent and identically distributed standard normal, all payoff vectors ν have positive density, which implies that $P(a; w, \theta, \lambda) > 0$ for all profiles $a \in \mathcal{A}$ and all (θ, λ).

12. This is similar to work by, e.g., Cetinyan (2002), Gent (2007), Grigoryan (2010), and Regan (1998, 2002).

13. See table A2 (tables A1, A2, C1, D1–D4, E1–E12, and F1–F4 are available online).

As noted above, scholars have long argued that equilibrium selection is likely determined by shared history, norms, or environmental factors on which players rely to refine their expectations. While from this perspective multiplicity of equilibria could be viewed as reflecting an incomplete account of the strategic situation under consideration, multiplicity often remains even in rich extensive-form games under strong equilibrium refinements. Admittedly, we focus in our analysis on the high-level strategic form of what is surely a much more intricate setting for civil-war onset and intervention. This likely omits, for example, important aspects of communication and coordination among major powers concerning intervention efforts. However, without additional information about the details of such interactions, rather than ignoring multiplicity, which would severely undermine our analysis, we parsimoniously estimate via $F(\sigma; \nu, \lambda)$ the key factors driving equilibrium selection in the data. In appendix D, we show that our main results are robust to alternative specifications of the equilibrium selection mechanism.

An alternative approach, common in economics, would be to remain agnostic about which equilibrium is played in the data and to estimate instead the set of parameters consistent with the equilibria of the model.[14] While this set (or partial) identification approach has many advantages—including possibly relaxing what information is available to players (Magnolfi and Roncoroni 2019)—it has two significant drawbacks. Inference typically relies on subsampling methods that are very computationally expensive and sensitive to tuning parameters for which there is often little practical guidance.[15] More importantly, set identification is by nature not guaranteed to deliver useful or precise information about substantive effects of interest. As noted by Bajari et al. (2010), estimation of the equilibrium selection mechanism, on the other hand, enables the researcher to simulate the model, which is central to performing the type of counterfactual experiments at the heart of this paper.

It is important for future research to extend our modeling framework, improve the quality of available data, and revisit the above features of our analysis with greater detail. For the sake of transparency and robustness, we take a more restrained first step. The key benefit is simultaneously addressing—for the first time, to our knowledge—selection into civil war and strategic interdependence among interveners. This enables us to flexibly and reliably estimate intervention expectations and to quantify their impact on the likelihood of conflict onset.

14. See, e.g., Ciliberto and Tamer (2009) and Iaryczower, Shi, and Shum (2018).

15. Kalandrakis (2019) discusses and exploits recent advancements in this literature.

EMPIRICAL STRATEGY

Data

We define major powers as the five permanent members of the UN Security Council.[16] Because our model is static, we construct a cross-sectional, country-level sample, where all countries, besides the major powers themselves, represent individual observations associated with an outcome (action profile $a \in \mathcal{A}$) of the game. Following the literature, we use Regan's (2002) data to identify the occurrence of civil wars and military interventions therein.[17] For each country, we code the rebels as starting a civil war if at any time between 1950 and 1999 the country appears as a civil war in Regan's (2002) data. Similarly, we code a major power as intervening in the country if it is recorded as a third-party intervener at any time between 1950 and 1999.[18]

Recall that our covariates include country-specific as well as country-major-power-specific data. As country-specific covariates we include a measure of terrain ruggedness from Shaver, Carter, and Shawa (2019), the polity2 measure of democracy from the Polity IV database, and population and GDP per capita from Maddison (2010). These covariates repeatedly appear as the most robust predictors of civil-war onset (Blattman and Miguel 2010; Hegre and Sambanis 2006). As such, they likely approximate relevant costs and benefits for rebels—for example, insurgencies may be more successful on mountainous terrain (Fearon and Laitin 2003). As dyadic covariates we include alliances from Gibler (2008), colonial history from the Correlates of War (COW) project, and the distance between each country and major power's capitals. We also include an indicator of whether the major power and the country have fought an interstate war, as defined by COW. While some of the covariates do not vary over time—for example, colonial history, terrain ruggedness, and distance—the remaining do, and we take the mean value for each country or each country-major-power pair between 1950 and 1999 as our observation. Finally, for interpretability, we standardize continuous variables.

16. This definition excludes Germany and Japan, but the two combined account for less than 2% of observed major-power interventions and less than 1% of all interventions. In contrast, the next least likely intervener is China, accounting for 5% of major-power interventions.

17. In the data, civil wars are internal conflicts with at least 200 battlefield fatalities. Interventions are military or economic (but not diplomatic) activities that break with past practices or conventions to influence the balance of power in a civil war. Military interventions include the deployment of forces, giving of equipment or intelligence, and military sanctions. Economic interventions include loans, debt relief, and economic sanctions.

18. In a robustness exercise, we focus specifically on military—excluding economic—interventions. As shown in app. E, our main results remain.

By constructing a cross-sectional sample, we depart from previous structural endeavors in international relations, which generally use panel data and repeat a static game in every group-year (e.g., Carter 2010; Gent 2007; Kurizaki and Whang 2015). We avoid a panel setup in our baseline analysis because it would introduce time dependence. If a country experiences civil-war onset in one year, the conflict is likely to continue in the next. If a major power intervenes in a civil war in one country-year, this may alter the costs of subsequent interventions. Scholars typically use functions of past actions to account for this serial correlation in reduced-form regressions (e.g., Beck, Katz, and Tucker 1998). In our structural framework, however, such an approach would treat past endogenous actions as exogenous covariates and would imply that our actors do not take into account how their actions in one period affect future payoffs, an unappealing assumption given our view on the strategic nature of major-power interventions. Put simply, a panel analysis would require a dynamic model and, as discussed above, reliable data about the timing of intervention efforts. While we acknowledge the importance of exploring dynamic incentives in future work, it is beyond the scope of this paper.

Nevertheless, to address justifiable concerns that our cross-sectional sample may gloss over important temporal heterogeneity, we engage in four robustness checks in appendix E. First, we vary the sample period—during and after the Cold War—and separately reestimate our model in each time frame. Second, we drop countries with multiple civil wars.[19] Third, we consider country-decade observations following the same coding rules as above.[20] Fourth, we consider country-decade observations coding covariates based on their initial values in the decade rather than averaging over time. As discussed below, our main conclusions persist in all four exercises, assuaging concerns about our baseline cross-sectional analysis.

Estimation

We estimate the parameters of our model via maximum simulated likelihood.[21] As described, for a sample of $N =$ 150 countries and $M = 5$ major powers, our data consist of observed civil-war onset and intervention decisions as well as various country-specific and dyadic (relative to each major power) covariates:

$$
\begin{aligned}
D &= \{(a_n, w_n)\}_{n=1}^{N} \\
&= \{(a_{nR}, a_{n1}, \dots, a_{nM}, x_n^R, z_{n1}^R, \dots, z_{nM}^R, x_n^I, z_{n1}^I, \dots, z_{nM}^I)\}_{n=1}^{N},
\end{aligned}
$$

where subscript $n = 1, \dots, N$ indexes observations (countries). Using equation (4), the likelihood of the data can be written as

$$
\mathcal{L}(D; \theta, \lambda) = \prod_{n=1}^{N} \int \left[\sum_{\sigma \in \mathcal{E}(v(w_n, \theta, \epsilon_n))} F(\sigma; v(w_n, \theta, \epsilon_n), \lambda)\sigma(a_n) \right] g(\epsilon_n) d\epsilon_n. \tag{5}
$$

Directly maximizing (the log of) $\mathcal{L}(D; \theta, \lambda)$ poses two computational challenges. First, the integrals in equation (5) do not admit closed-form solutions. Moreover, note that the set of equilibria $\mathcal{E}(v(w_n, \theta, \epsilon_n))$ depends on the payoff parameters θ, which implies that costly equilibrium calculations would be required at every step of the optimization search process. Following Bajari et al. (2010), we address these challenges with a threefold approach: we employ a change-of-variables transformation, importance sampling, and Monte Carlo integration. We then estimate θ and λ by maximizing the resulting simulated likelihood $\hat{\mathcal{L}}(D; \theta, \lambda)$.[22] This estimator is consistent and asymptotically normal by standard arguments from the theory of importance sampling and maximum simulated likelihood (Hajivassiliou and Ruud 1994). We provide a detailed description of our estimation algorithm in appendix B.2.

ESTIMATION RESULTS

Tables 1 and 2 present our coefficient estimates. Table 1 reports the payoff parameters besides the spillover effects δ, which are in table 2.[23]

The first column of table 1 shows the rebels' payoff parameters. The first five rows correspond to country-specific variables that shape rebels' baseline utility from launching a civil war, and we find effects that agree with the existing literature. The remaining rows correspond to rebels' preferences over major-power interveners. Thus, the positive coefficient in the US row indicates that expected US interventions tend to increase rebel war payoffs, all else equal. In contrast, the negative coefficient in the UK row implies that,

19. Aggregating interventions over multiple conflicts may overstate strategic complementarities if major powers frequently change their intervention decisions across conflicts. Reassuringly, this is not the case: at the high end, the United States and Russia switch intervention decisions in 17% of countries with civil wars, and France and China do so in approximately 6% at the low end. Furthermore, our results are robust to excluding these countries from our sample.

20. Crisman-Cox and Gibilisco (2021) and Kurizaki and Whang (2015) also use country-decade observations.

21. Identification is guaranteed by our model specification—see app. B.3.

22. Standard errors are computed using an estimate of the information matrix—see app. B.2 for details.

23. See app. C for model fit and app. D for discussion of the equilibrium selection parameters λ.

Table 1. Estimates of Payoff Parameters θ

	Rebels' Payoffs	Interveners' Payoffs
Constant	.44	
	(.07)	
Terrain	.11	
	(.03)	
GDP per capita	−.08	.03
	(.02)	(.01)
Democracy	−.03	.00
	(.04)	(.02)
Population	−.02	−.04
	(.02)	(.01)
Distance		−.07
		(.02)
Allies	−.14	.18
	(.06)	(.06)
Colony	.18	.06
	(.08)	(.07)
Previous war	−.61	5.74
	(1.07)	(1.23)
US	.39	−.44
	(.06)	(.08)
UK	−.40	−.62
	(.07)	(.08)
France	−.35	−.30
	(.06)	(.08)
Russia	−.03	−.28
	(.07)	(.07)
China	−.27	−.67
	(.06)	(.08)
N	150	
$\log \hat{\mathcal{L}}$	−219.98	

Note. Standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

Table 2. Estimates of Intervention Spillover Effects δ

	UK	France	Russia	China
US	.42	−.27	.05	.37
	(.07)	(.06)	(.07)	(.06)
UK		.29	.16	.12
		(.07)	(.06)	(.06)
France			.03	.39
			(.06)	(.06)
Russia				−.05
				(.07)

Note. Standard errors in parentheses.

on average, rebels expect a substantial cost from UK interventions. Notice that the only major powers without negative, significant coefficient estimates are the United States and Russia, indicating that the two Cold War rivals tend to be less hostile to rebels. As previewed, we also find that major powers that are allied with the host government reduce rebels' payoffs to a greater extent when they intervene in a civil war than major powers without such alliances. Intuitively, interveners who are allied with the government are more likely to support the anti-rebel cause. In addition, major powers that intervene in civil wars in former colonies tend to increase rebel war payoffs.

It may be difficult to interpret whether interventions from any specific major power directly increase or decrease rebels' incentives for conflict because these effects are composed of several terms. For major power m, the sample average effect of an intervention on rebels' expected war payoff is

$$\bar{\gamma}_m = \hat{\gamma}_m + \frac{1}{N}\sum_{n=1}^{N} z^R_{nm} \cdot \hat{\gamma}_0.$$

These average effects are $\bar{\gamma}_{\text{US}} = 0.35$, $\bar{\gamma}_{\text{UK}} = -0.37$, $\bar{\gamma}_{\text{FRN}} = -0.34$, $\bar{\gamma}_{\text{RUS}} = -0.04$, and $\bar{\gamma}_{\text{CHN}} = -0.28$.[24] Notably, the model illustrates that expected interventions have remarkably heterogeneous direct effects on rebels' incentives to instigate conflict, a result that is masked by studies focusing on one major power or pooling effects across interveners.[25] In addition, the positive effect $\bar{\gamma}_{\text{US}}$ provides evidence that US interventions tend to directly encourage rebels to initiate wars, confirming the logic underlying the case studies in Kuperman (2008) on a wider scale.

Although our baseline analysis does not include the direction of interventions, these effects help illustrate how the model approximates such considerations. For example, the negative effects $\bar{\gamma}_{\text{FRN}}$ and $\bar{\gamma}_{\text{UK}}$ suggest that French and UK interventions generally reduce rebels' benefits from war. This is consistent with the two powers' tendency to intervene on behalf of governments in the postwar era. In contrast, the United States, Russia, and China are generally more open to pro-rebel interventions. Likewise, an intervention becomes less beneficial to rebels when it involves a major power allied with the host government. Examining the heterogeneity of these effects across countries, we find that in Latin America, for example, the average effect of US interventions on rebels' war payoffs is smaller (more negative) than the corresponding

24. Corresponding standard errors are 0.06, 0.09, 0.08, 0.05, and 0.02.

25. Note, however, that our results do not disentangle specific channels for these effects—e.g., via changes in the cost of war, probabilities of victory, or expected concessions.

effect outside the region.[26] This result comports with the narrative of US support for sympathetic governments in the region—particularly during the Cold War—and it emerges even though we do not explicitly model the direction of interventions.

The second column of table 1 reports the major powers' payoff parameters. Positive (negative) coefficients indicate that the variables are associated with greater benefits (larger costs) from intervention for the major power. For the country-specific variables, we find that major powers have weaker incentives to intervene in countries with lower per capita GDP and larger populations, which may present substantial development challenges. For the dyadic variables, greater distance between the major powers and the civil war implies greater net costs of intervention, hence the negative coefficient estimate. This mirrors previous results that use distance, contiguity, or same-region indicators to predict interventions (Findley and Teo 2006; Gent 2007). Likewise, major powers have greater incentives to intervene in civil wars if they occur in countries that are their allies or with which they have been engaged in an interstate war.[27] Finally, the major-power rows in the second column correspond to fixed effects on the intervention payoffs. Thus, we find that, all else equal, China is generally the least inclined to intervene, whereas Russia is the most prone.[28]

Finally, table 2 reports the intervention spillover effects. Recall that, if $\delta_{m,m'} > 0$, then intervention efforts between major powers m and m' are strategic complements. If $\delta_{m,m'} < 0$, intervention efforts are substitutes. Thus, major-power interventions are largely characterized by strong strategic complementarities, which are suggestive of cost-sharing incentives for allies—for example, the United States and United Kingdom—and competition for control among rivals—for example, the United States and China. This explains the observed prevalence of intervening coalitions among allies and opposing interventions among rivals.

Two notable exceptions exist, however. First, we find strong strategic substitution between the United States and France. As discussed below, the result plausibly stems from France's sphere of influence in Africa during the Cold War and the need for Western allies to efficiently coordinate interventions against the Soviet Union. Second, we find that, on average, there are little strategic spillovers between the United States and Russia. Although this may seem surprising at first glance, it is likely driven by two countervailing forces. On one hand, the two rivals have attempted to avoid overt confrontation (Laitin 1999), and the Soviet Union would temper its intervention intensity when expecting a risk of conflict with the United States (Kaw 1989). This suggests negative spillovers. On the other, the two powers regularly engage in proxy wars indicative of positive spillovers (Kalyvas and Balcells 2010). Our estimates reveal that these countervailing effects offset each other in the aggregate.[29]

Because we flexibly specify spillover effects while accounting for civil-war onset, our findings add nuance to previous studies. For example, Gent (2007) finds that joint interventions are more prominent when major-power pairs have opposing policy preferences, and he argues that this indicates competition among rivals and free riding among allies.[30] Our analysis partially supports this as we find strategic complementarities between the West and East major powers, but strategic substitution—and, thus, free riding among allies—is not a dominant effect in table 2. As noted, the only exception is free riding between the United States and France. Our estimates of positive spillovers among allies better match Findley and Teo (2006), who find, perhaps surprisingly, that allies encourage each other to launch both joint and opposing interventions using a hazard analysis.

Intervention expectations

A key feature of our structural approach is that expectations about major-power intervention are derived from equilibrium behavior. In this section, we explore how well standard proxies for anticipated interventions correlate with the expectations implied by our model. We use Warsaw Pact membership and Lake's (2009) US security hierarchy as proxies for Russian and US intervention propensities, respectively (Cunningham 2016; Fearon and Laitin 2003). Given our previously reported payoff estimates, and using the estimated selection mechanism to compute intervention probabilities, we compare these proxies with our model's predictions.

Figure 1 presents the comparisons. Figure 1*A* is a box plot of predicted Russian intervention probabilities in civil wars occurring in countries that are nonmembers (left) and

26. In Central and South America, the average effect is 0.25. Outside of the Americas, the average effect is 0.38. The difference is statistically significant at the 1% level in a two-sample difference-of-means test with unknown variances.

27. The estimate associated with the colony variable is not significant at conventional levels. See Chacha and Stojek (2019) for a discussion of how control variables may mask the effects of colonial ties on interventions.

28. These fixed effects match intervention frequencies in table A2.

29. In our extension modeling the direction of interventions, we find greater evidence that the United States and Russia avoid supporting interventions but seek opposing interventions—see app. F.

30. Gent (2008) also finds that an intervention on behalf of one side of a civil war increases the likelihood of another intervention on the opposite side, indicating positive spillovers among rivals.

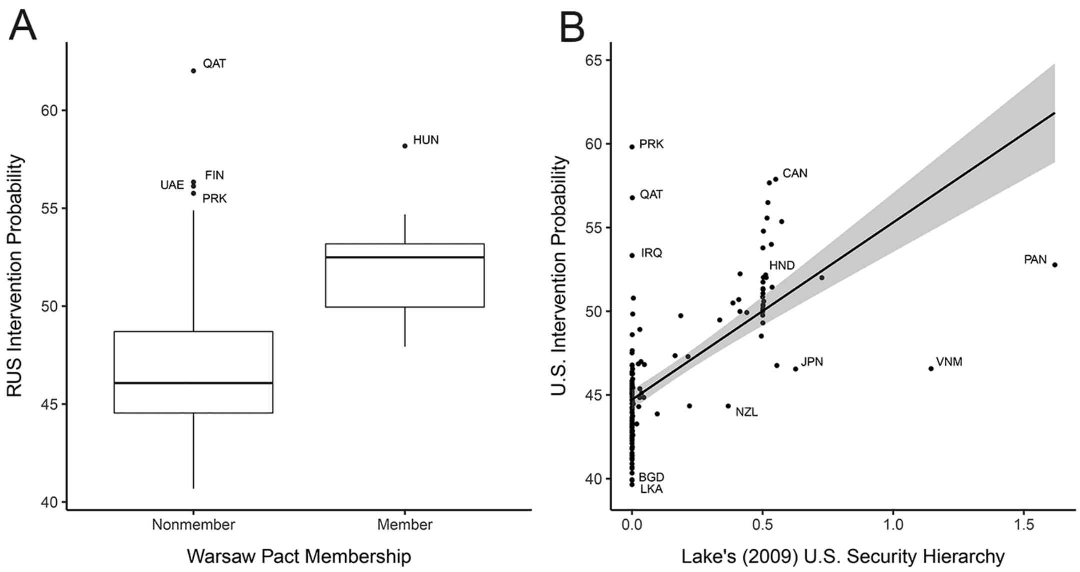

Figure 1. Comparing proxies and estimated intervention expectations. Panel *A* is a box plot of the estimated probability of Russian intervention (conditional on civil war) by Warsaw Pact membership. Panel *B* is a scatter plot comparing the estimated probability of US intervention (conditional on civil war) with Lake's (2009) US security hierarchy measure.

members (right) of the Warsaw Pact. Figure 1*B* is a scatter plot of countries where the horizontal axis corresponds to the US security hierarchy score and the vertical axis measures the predicted probability of US intervention. All predicted intervention probabilities are computed conditional on civil-war onset—that is, in the intervention subgame.

Two takeaways emerge. First, both proxies correlate with the estimated intervention probabilities in expected directions. We find that, on average, Russia is more likely to intervene in civil wars when they occur in Warsaw Pact member states than in nonmember states. In member states, Russia intervenes with average probability 52%, whereas, in nonmember states, Russia intervenes with average probability 46%. The difference is statistically significant at the 1% level. Furthermore, we find that importance in the US security hierarchy is positively correlated with expectations of US intervention in civil wars. The correlation coefficient is $\rho = 0.66$ and is also significant at the 1% level.

However, the two proxies miss important heterogeneity. In particular, they likely capture potential interventions on behalf of the government and overlook those that are more encouraging (or at least less discouraging) for rebels. In the Russian case, the model plausibly predicts that Russia is quite likely to intervene in civil wars if they occur in Qatar, Finland, and the United Arab Emirates, even though these countries were not members of the Warsaw Pact. Similarly, we find that the United States is more likely to intervene in civil wars if they occur in North Korea and Iraq than in Bangladesh and Sri Lanka, yet all four countries have identical (zero) hierarchy scores. Notably, in our sample period, the United States has stronger incentives to disrupt the status quo in the former countries than the latter.

Robustness and direction-of-intervention extension

We evaluate the robustness of our results to several key features of our analysis. To examine the consequences of aggregating over time when creating a cross-sectional sample, we consider four exercises. First, we split the sample into two periods, during and after the Cold War, and separately reestimate our model. Second, we drop countries with multiple civil wars from the original sample. Finally, we construct panel versions of the data with country-decade observations, both averaging observed covariates and taking initial values as the corresponding observation for each decade. In addition, to explore the sensitivity of our results to the intervention measure, we consider an alternative coding that focuses on military—excluding economic—interventions.[31]

Appendix E presents our results in detail. All of our main conclusions persist. Coefficient estimates associated with key covariates have similar magnitudes and significance levels across robustness checks, although standard errors are generally smaller with country-decade observations. We consistently find that US and Russian interventions are expected to be the most favorable to rebels, while UK and French interventions are the least favorable. And spillovers among

31. We also examine the robustness of our results to alternative specifications of the equilibrium selection mechanism—see app. D.1.

major powers are largely characterized by strong strategic complementarities rather than substitution effects. In fact, the robustness exercises suggest that strategic complementarities might be even more predominant given that we only find free-riding incentives between the United States and France during the Cold War. This is consistent with accounts describing how the need to efficiently coordinate responses to the Soviet block led Western allies to rely on France to handle interventions in its sphere of influence, *Françafrique*, but, since the fall of the Berlin Wall, the United States has adopted a more competitive military role in Africa as French and American interests have become less aligned (Schraeder 2000).

Furthermore, we extend our analysis to explicitly model the direction of intervention efforts in appendix F. That is, if rebels launch a civil war, the major powers simultaneously decide whether to stay out, intervene to support the government, or intervene to support the rebels. We maintain a flexible specification of systematic utilities and use Regan's (2002) target of intervention variable to identify pro-government and pro-rebel interventions. This exercise yields three conclusions.

First, as previewed above, our baseline analysis accommodates well the expected direction of interventions through observed covariates and major-power fixed effects. For example, we find that security alliances encourage pro-government interventions and discourage pro-rebel interventions. Moreover, conditional on either direction, interventions by major powers allied with the home government decrease rebels' war payoffs. These results are consistent with the corresponding coefficient estimates in table 1.

Second, we find that the United States is generally the major power that is most favorable to rebels, all else equal. Even when intervening on behalf of the government, a US intervention increases rebels' expected war payoff, although the effect is not significant at conventional levels. This suggests that, conditional on either direction of intervention, the United States uses tactics or adopts bargaining positions that are more rebel friendly than other major powers. Similarly, conditional on either direction, French and UK interventions are still the least sympathetic to rebels, whereas Russia is the most favorable after the United States. These results match the fixed effects in table 1 and the sample average effects $\bar{\gamma}_m$ reported above.

Third, spillover effects among major powers are largely characterized by strategic complementarities, although two additional nuances emerge. The extended analysis suggests that strategic substitution between France and the United States may be driven by a preference to avoid opposing interventions rather than by free riding on joint or supporting interventions. This is also consistent with diverging French interests in Africa—for example, France intervenes on behalf of the rebels in the Nigerian (Biafran) civil war, opposing the United Kingdom. We also find that the United States and Russia face strategic substitution for joint or supporting interventions, whereas they have stronger incentives to engage in opposing interventions. While this suggests that their incentives to compete via proxy wars may outweigh those to avoid overt competition, disentangling these effects with statistical precision is difficult with the data at hand.

INTERVENTION COUNTERFACTUALS

What is the impact of expected major-power intervention on civil-war onset? The estimates in table 1 provide only a partial answer. Specifically, the value $\hat{\gamma}_m + z^R_{nm} \cdot \hat{\gamma}_0$ measures the effect of an intervention from major power m on rebels' expected payoff from starting a civil war in country n while fixing the intervention decisions of all other major powers $m' \neq m$. The choices of the other major powers are endogenous, however. As such, the intervention decision of power m has not only a direct effect on rebels' incentives to instigate a conflict but also an indirect effect through its influence on the decisions of the remaining powers.

These direct and indirect effects obscure the total impact of interventions on civil-war onset. For instance, consider the United States. As $\bar{\gamma}_{\mathrm{US}} > 0$, US interventions should discourage peace because they directly increase rebels' war payoffs on average. However, US and UK interventions are strategic complements, which implies that the former encourage the latter. The United Kingdom, in turn, directly decreases rebels' war payoffs on average as $\bar{\gamma}_{\mathrm{UK}} < 0$. Indirectly, then, US interventions can deter civil-war onset via the United Kingdom's entry decision.[32] As a result, US interventions have countervailing effects on peace.

We conduct a series of counterfactual experiments to explicitly quantify these substantive effects. First, for each major power m and country n, we fix m's intervention decision to entering a civil war if it is launched by the rebels in that country, $a_{nm} = 1$. In other words, m commits to intervening in every civil war that arises. Anticipating this commitment, the remaining actors—that is, major powers $m' \neq m$ and the rebels—still make decisions optimally, with

32. We use average effects for illustration, but these vary across countries. Moreover, this does not necessarily imply that the two powers would be on opposing sides of the same civil war. Even when supporting the same side, the United States and United Kingdom may adopt different tactics or bargaining positions, with the United Kingdom expected to be less favorable to rebels. Indeed, we find evidence of this in app. F. Nonetheless, Western allies do occasionally intervene on opposite sides—e.g., in the Nigerian civil war.

payoffs given by equation (1) and parameters θ previously estimated, and their behavior satisfies subgame perfection. As before, multiple equilibria may exist, even when fixing the action of intervener m, so we select equilibria with probabilities $F(\sigma; \nu, \lambda)$ and the selection parameters λ previously estimated. We then compute the probability of peace. For each country, we compare these expectations with those generated by the original model where m chooses freely whether or not to intervene. We then repeat the same exercise but fix each major power's decision to staying out of any civil war, $a_{nm} = 0$. Finally, although our focus is on the net impact of unilateral commitments by major powers on the onset of civil war, we also consider counterfactual scenarios where either every major power intervenes in every civil war or no major power intervenes in any civil war.

Figure 2 summarizes our analysis. The top panel graphs the change in the probability of peace under the six counterfactuals where major powers commit to intervening in every civil war. The bottom panel graphs the corresponding change under the counterfactuals where the powers commit to staying out of any civil war. In both panels, the dashed horizontal line highlights a null effect, and estimates above (below) the line correspond to peace enhancing (diminishing) commitment policies. Three major takeaways emerge.

First, we find that, at least for the United States, the road to hell is paved with good intentions. Greater US commitment to civil-war intervention perversely increases the probability of war onset. Specifically, if the United States could commit to always intervene, the probability of peace would decrease by 10 percentage points, which is the effect largest in magnitude under the commit-to-intervene treatment. As discussed above, this confirms the logic of Kuperman's (2008) Balkans case studies on a wider scale and contrasts with studies that proxy for US interventions using security hierarchy, which likely misses cases where the United States is less invested in the status quo.

Second, the story is different when the powers commit to abstaining from civil wars. When rebels do not expect a major power to intervene, the probability of war almost always increases. The effect is largest when the United Kingdom and France avoid intervention, decreasing the probability of peace by 10 percentage points.

Third, the counterfactual commitments have heterogeneous effects on the likelihood of civil war, varying systematically across the major powers. In contrast to the United States, United Kingdom and Chinese commitments to intervene generally increase the probability of peace—by about 3 and 5 percentage points, respectively. When the United Kingdom and France do not intervene, peace decreases considerably, whereas there is no effect for the United States. This demonstrates the limits of studying the effects of potential interventions focusing on a single major power.

To some degree, figure 2 masks the heterogeneity that our unified theoretical and empirical model is able to uncover because it reports averages over all countries.[33] For example, the United States and United Kingdom intervened in the 1958 crisis in Lebanon to support the Chamoun government. No other power intervened in the conflict, including France, the colonial power governing Lebanon after World War I. Our counterfactual experiments suggest that, had France committed to intervene a priori, the probability of peace would have decreased by one percentage point, and the probability of US intervention conditional on conflict would have decreased by a similar magnitude.[34] Compare this with the civil war in South Africa, a former British colony, during the late 1980s and early 1990s, which had the United States and United Kingdom intervening to support the rebels. In this case, a French commitment to intervention would have increased the probability of peace by 5 percentage points and, conditional on conflict, would have increased the likelihood of a US intervention by one percentage point.

Notice that the substantive effects of a French commitment to intervention can run counter to the directions suggested by the parameters $\bar{\gamma}_{\mathrm{FRN}}$ and $\delta_{\mathrm{US,FRN}}$. For example, in the Lebanese case, a French intervention encourages civil-war onset even though $\bar{\gamma}_{\mathrm{FRN}} < 0$. In the South African case, the United States is more likely to intervene given a French intervention, yet $\delta_{\mathrm{U.S.,FRN}} < 0$. These countervailing forces arise from the indirect effects of French interventions via the equilibrium behavior of other major powers and the equilibrium selection mechanism. Examples like these demonstrate the importance of conducting counterfactual analyses in addition to examining parameter estimates.

More broadly, the counterfactuals in figure 2 carry an important policy implication. If the goal is to minimize the onset of civil conflict, foreign policy practitioners should avoid committing major powers a priori to either intervening in or staying out of civil wars. Our results suggest that coordination among major powers has been maximally successful at deterring civil conflict. Next, to explore this further, we

33. This heterogeneity emerges through three avenues. First, intervention by major power m has a fixed effect γ_m on rebels' war payoffs. Second, this effect is adjusted according to the dyadic covariates z^R_{nm}. Third, major-power-pair-specific strategic spillovers, $\delta_{m,m'}$, encourage (or discourage) other major powers to intervene as well, which in turn affects rebels' payoffs.

34. This prediction does not necessarily imply that France would have supported the rebels and opposed the United States and United Kingdom. The commitment may have changed the direction or quality of US and UK interventions. See n. 32 for a similar discussion.

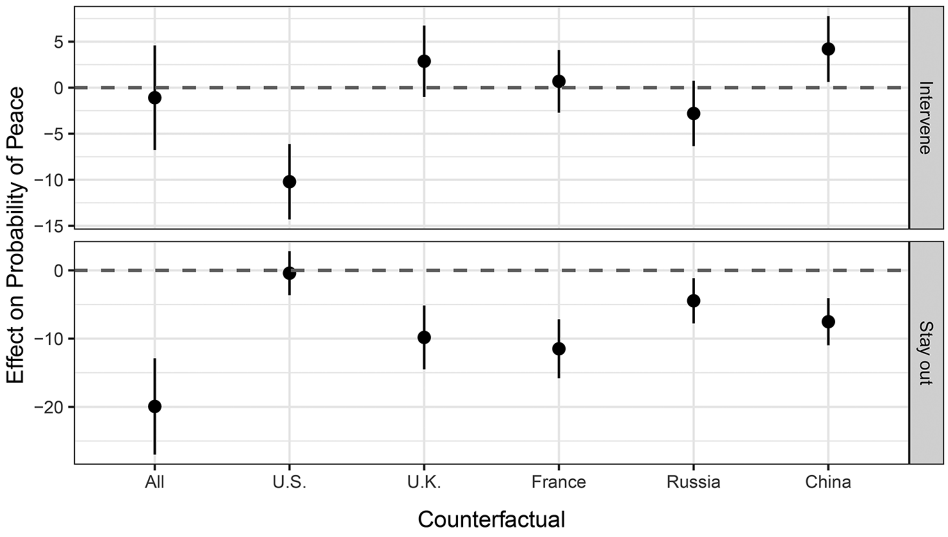

Figure 2. Intervention and the likelihood of peace. The *x*-axis corresponds to counterfactual scenarios fixing the intervention decision of the respective actor(s). The *y*-axis measures the change in the predicted probability of peace. The top (bottom) panel corresponds to a commitment to always intervene in (stay out of) civil wars. We plot sample average effects, and vertical lines cover 95% confidence intervals.

quantify the full range of outcomes consistent with equilibrium behavior.

EQUILIBRIUM SELECTION COUNTERFACTUALS

How important is major-power coordination for conflict outcomes? Could the major powers select more peaceful or more conflictual equilibria? In this section, we answer these questions by illustrating how the equilibrium selection mechanism, $F(\sigma; \nu, \lambda)$, affects the model's predictions. Such an exercise is important because coordinating on different equilibria is a feasible policy counterfactual. That is, changing what equilibrium actors play does not require politically untenable measures—such as binding commitments to intervention—nor the resources required to change country characteristics—for example, changing democracy or development levels.

We consider two polar, deterministic equilibrium selection mechanisms. One criterion is the most peaceful equilibrium, which is computed as follows. First, select the equilibrium such that rebels play peace. If multiple equilibria exist in which rebels play peace or if no such equilibrium exists, then select the equilibrium that minimizes the expected number of interveners in the intervention subgame. Analogously, we consider the most conflictual equilibrium, where we select the equilibrium such that rebels start a civil war. If multiple such equilibria exist or if none exists, then we select the equilibrium that maximizes the expected number of interveners in the intervention subgame.

Figure 3 summarizes our results. The horizontal axis depicts the two selection mechanisms, and the vertical axis measures the predicted probability of peace. The dashed horizontal line highlights our baseline model prediction, with the shaded area covering the 95% confidence interval. An estimate above (below) the line indicates that the proposed selection mechanism has a peace enhancing (diminishing) effect relative to the data. Two major lessons emerge.

First, equilibrium selection has nontrivial effects on peace. The most peaceful selection increases the probability of peace by 10 percentage points, though not in a statistically meaningful way. In contrast, the most conflictual selection decreases the probability of peace by almost 40 percentage points, twice the effect of committing all major powers to abstaining from interventions.

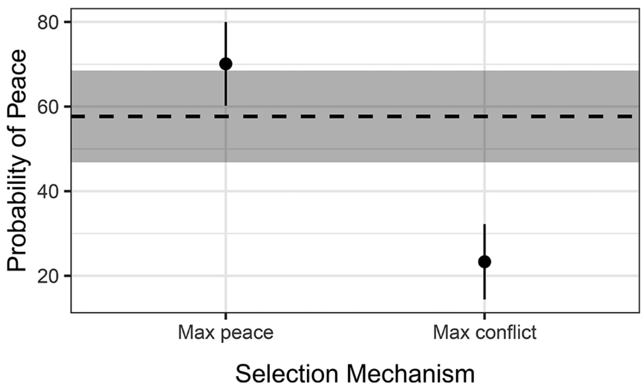

Figure 3. The effects of equilibrium selection. The *x*-axis corresponds to counterfactual equilibrium selection criteria. The *y*-axis measures the predicted probability of peace. Vertical lines cover 95% confidence intervals. The dashed line shows our baseline model prediction given the selection mechanism in the data, with the shaded area covering the 95% confidence interval.

Second, the effects identified in figure 3 demonstrate that the major powers tend to coordinate on peaceful equilibria. Although there exists a selection criterion that produces more peace than the one identified in the data, the effect is relatively small. And the potential for a greater probability of peace from coordinating on the most peaceful equilibrium is dwarfed by the potential for additional wars from coordinating on more conflictual equilibria. This reveals that coordination among major powers has effectively maximized deterrence of civil conflict. As a prescription for policy, therefore, practitioners should avoid courses of action that could potentially shift intervention focal points.

DISCUSSION

Besides quantifying the net impact of expected major-power intervention on civil-war onset, our findings have substantive and methodological implications for ongoing research in international relations. A sizable body of literature examines the effects of military interventions and humanitarian aid on conflict outcomes. These studies generally rely on a sample of countries with a known history of conflict for their analyses. In addition to raising methodological questions about potential selection bias, this overlooks the possibility that aid and interventions might encourage the onset of new conflicts not previously considered, offsetting any estimated benefits for ongoing conflict outcomes. Our results validate this concern. While we do not model other potential benefits of intervention—for example, shortening the length of war—future work could expand our framework in order to explicitly compare additional channels through which interventions may affect the nature of conflict.

Although our analysis is largely empirical, our results have implications for future theoretical work. When flexibly estimating strategic spillovers among major powers, we find substantial evidence of strategic complementarities, which may have grown even stronger after the Cold War. Future theories of third-party intervention should therefore focus on disentangling mechanisms that explain the emergence of and variation in strategic complementaries rather than on strategic substitution.

Finally, our analysis demonstrates nonconventional avenues by which formal models can be useful for empirical research in political science. Typically, the interaction between models and data is limited to statistically testing hypotheses derived from a theoretical model. In contrast, we use a game-theoretic model to help measure unobserved quantities of substantive interest—namely, strategic spillovers among major powers and endogenous expectations about third-party intervention. As our results illustrate, spillovers can generate crucial indirect equilibrium effects that should not be glossed over when probing empirical relationships of interest. We hope our efforts highlight the usefulness of the structural enterprise and encourage future interaction between formal models and empirical research.

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ONLINE APPENDIX

A Additional Tables

Table A1: Countries in sample.

Country	Years	Notes	Country	Years	Notes
Canada	1950-1999		Cuba	1950-1999	
Haiti	1950-1999		Dominican Republic	1950-1999	
Jamaica	1962-1999		Trinidad and Tobago	1962-1999	
Mexico	1950-1999		Guatemala	1950-1999	
Honduras	1950-1999		El Salvador	1950-1999	
Nicaragua	1950-1999		Costa Rica	1950-1999	
Panama	1950-1999		Colombia	1950-1999	
Venezuela	1950-1999		Ecuador	1950-1999	
Peru	1950-1999		Brazil	1950-1999	
Bolivia	1950-1999		Paraguay	1950-1999	
Chile	1950-1999		Argentina	1950-1999	
Uruguay	1950-1999		Ireland	1950-1999	
Netherlands	1950-1999		Belgium	1950-1999	
Luxembourg	1950-1999		Switzerland	1950-1999	
Spain	1950-1999		Portugal	1950-1999	
Germany	1950-1999	Federal Republic of Germany from 1950-1990.	Poland	1950-1999	
Austria	1955-1999		Hungary	1950-1999	
Czechoslovakia	1950-1992		Czech Republic	1993-1999	
Slovakia	1993-1999		Italy	1950-1999	
Albania	1950-1999		S Macedonia	1993-1999	
Croatia	1992-1999		Yugoslavia	1950-1991	
Bosnia and Herzegovina	1992-1999		Slovenia	1992-1999	
Greece	1950-1999		Cyprus	1960-1999	
Bulgaria	1950-1999		Moldova	1991-1999	
Romania	1950-1999		Estonia	1991-1999	
Latvia	1991-1999		Lithuania	1991-1999	
Ukraine	1991-1999		Belarus	1991-1999	
Armenia	1991-1999		Georgia	1991-1999	
Azerbaijan	1991-1999		Finland	1950-1999	
Sweden	1950-1999		Norway	1950-1999	
Denmark	1950-1999		Cabo Verde	1975-1999	
Guinea-Bissau	1974-1999		Equatorial Guinea	1968-1999	
Gambia	1965-1999		Mali	1960-1999	
Senegal	1960-1999		Benin	1960-1999	
Mauritania	1960-1999		Niger	1960-1999	
Côte d'Ivoire	1960-1999		Guinea	1958-1999	
Burkina Faso	1960-1999		Liberia	1950-1999	
Sierra Leone	1961-1999		Ghana	1957-1999	
Togo	1960-1999		Cameroon	1960-1999	
Nigeria	1960-1999		Gabon	1960-1999	
Central African Republic	1960-1999		Chad	1960-1999	
Congo	1960-1999		Democratic Republic of the Congo	1960-1999	
Uganda	1962-1999		Kenya	1963-1999	
Tanzania	1961-1999		Burundi	1962-1999	
Rwanda	1962-1999		Djibouti	1977-1999	
Ethiopia	1950-1999		Angola	1975-1999	
Mozambique	1975-1999		Zambia	1964-1999	
Zimbabwe	1965-1999		Malawi	1964-1999	
South Africa	1950-1999		Namibia	1990-1999	
Lesotho	1966-1999		Botswana	1966-1999	
Swaziland	1968-1999		Madagascar	1960-1999	
Comoros	1975-1999		Mauritius	1968-1999	
Morocco	1956-1999		Algeria	1962-1999	
Tunisia	1956-1999		Libya	1951-1999	
Sudan	1956-1999		Iran	1950-1999	
Turkey	1950-1999		Iraq	1950-1999	
Egypt	1950-1999		Syria	1950-1999	
Lebanon	1950-1999		Jordan	1950-1999	
Israel	1950-1999		Saudi Arabia	1950-1999	
Yemen	1990-1999		Kuwait	1961-1999	
Bahrain	1971-1999		Qatar	1971-1999	
United Arab Emirates	1971-1999		Oman	1971-1999	
Afghanistan	1950-1999		Turkmenistan	1991-1999	
Tajikistan	1991-1999		Kyrgyzstan	1991-1999	
Uzbekistan	1991-1999		Kazakhstan	1991-1999	
Mongolia	1950-1999		North Korea	1950-1999	
South Korea	1950-1999		Japan	1952-1999	
India	1950-1999		Pakistan	1950-1999	
Bangladesh	1971-1999		Myanmar	1950-1999	
Sri Lanka	1950-1999		Nepal	1950-1999	
Thailand	1950-1999		Cambodia	1953-1999	
Laos	1953-1999		Democratic Republic of Vietnam	1950-1999	Socialist Republic of Vietnam from 1976-1999.
Malaysia	1957-1999		Singapore	1965-1999	
Philippines	1950-1999		Indonesia	1950-1999	
Australia	1950-1999		New Zealand	1950-1999	

Table A2: Major-power interventions and direction of support.

	Government	Rebels	Neutral
China	2	5	0
France	12	1	1
Russia	15	6	0
U.K.	9	3	1
U.S.	25	11	1

Notes. Data from Regan (2002). Totals do not include repeated interventions in the same direction in a single conflict.

B Estimation Details

B.1 Equilibrium selection

We adopt a relatively parsimonious specification of the equilibrium selection mechanism, which takes the form:

$$F(\sigma; v, \lambda) = \frac{\exp\{y(\sigma, v) \cdot \lambda\}}{\sum_{\sigma' \in \mathcal{E}(v)} \exp\{y(\sigma', v) \cdot \lambda\}}. \tag{B1}$$

Given payoffs v, the probability $F(\sigma; v, \lambda)$ that equilibrium $\sigma \in \mathcal{E}(v)$ is played is thus a logit function parameterized using a vector $y(\sigma, v)$ of properties of the equilibrium and coefficients λ to be estimated. The choice of $y(\sigma, v)$ is somewhat arbitrary—subject to an identification restriction discussed in Appendix B.3—as there is no previous applied theoretical or empirical work to guide our specification.

Following Harsanyi and Selten (1992), we allow $y(\sigma, v)$ to depend only on endogenous features of the game and equilibria, and we view two aspects of our model as key potential drivers of equilibrium selection. First, because multiplicity arises in the intervention stage, it fundamentally poses a coordination problem for major powers to solve. To concisely summarize each major power's evaluation of equilibria, we use a normalized ordinal ranking. Say major power m strictly prefers equilibrium σ to equilibrium σ' if m has a larger expected utility under equilibrium σ than σ'—i.e., $\sum_{a \in \mathcal{A}} \sigma(a) v_i(a) > \sum_{a \in \mathcal{A}} \sigma'(a) v_i(a)$. Let $r_m(\sigma, v)$ denote the ordinal (ascending) rank of equilibrium σ in major power m's preference ordering over $\mathcal{E}(v)$, and let $\bar{r}_m(\sigma, v)$ denote the normalized ordinal rank—i.e., $\bar{r}_m(\sigma, v) = r_m(\sigma, v)/\#\mathcal{E}(v)$. Intuitively, $\bar{r}_m$ is a variable with range between 0 and 1 such that $\bar{r}_m(\sigma, v) = 1$ if σ is m's most preferred equilibrium in $\mathcal{E}(v)$. Second, we use $\sigma_R \sum_m \sigma_m$, the (on-path) expected number of interveners under σ, to summarize the extent of international involvement in civil wars. We then set $y(\sigma, v) = \big(\sigma_R \sum_m \sigma_m, \bar{r}_1(\sigma, v), \ldots, \bar{r}_M(\sigma, v)\big)$.

The coefficients in $\lambda = (\lambda_R, \lambda_1, \ldots, \lambda_M)$ determine the weights with which these considerations drive equilibrium selection. For example, if $\lambda_m > 0 = \lambda_{m'}$ for all $m' \neq m$, then equilibria that give major power m relatively larger expected payoffs are more likely to be played. Similarly, if $\lambda_R > 0$, equilibria with multiple interveners are more likely.

Addressing multiplicity in this manner has two advantages. First, equilibrium selection is probabilistic, so we accommodate the possibility that our actors play different equilibria across observationally equivalent scenarios. Second, when two scenarios are not observationally equivalent, their distributions over equilibria may differ because the preferences of major powers (which vary with covariates) are included in the factors determining selection.

B.2 Estimation algorithm

For a sample of $N = 150$ countries and $M = 5$ major powers, our data consist of observed civil-war onset and intervention decisions as well as various country-specific and dyadic (relative to each major power) covariates:

$$D = \{(a_n, w_n)\}_{n=1}^{N} = \{(a_{nR}, a_{n1}, \ldots, a_{nM}, x_n^R, z_{n1}^R, \ldots, z_{nM}^R, x_n^I, z_{n1}^I, \ldots, z_{nM}^I)\}_{n=1}^{N},$$

where subscript $n = 1, \ldots, N$ indexes observations (countries). Using Equation 4, the (conditional) likelihood of the data can be written as

$$\begin{aligned} \mathcal{L}(D; \theta, \lambda) &= \prod_{n=1}^{N} P(a_n; w_n, \theta, \lambda) \\ &= \prod_{n=1}^{N} \int \left[\sum_{\sigma \in \mathcal{E}(v(w_n, \theta, \epsilon_n))} F(\sigma; v(w_n, \theta, \epsilon_n), \lambda) \sigma(a_n) \right] g(\epsilon_n) d\epsilon_n. \end{aligned} \tag{B2}$$

Directly maximizing (the log of) $\mathcal{L}(D; \theta, \lambda)$ presents two significant computational challenges. First, the integrals in Equation 5 do not admit closed-form analytical solutions. Moreover, note that the set of equilibria $\mathcal{E}(v(w_n, \theta, \epsilon_n))$ depends on the payoff parameters θ, which implies that costly equilibrium calculations would be required at every step of the optimization search process. Following Bajari, Hong and Ryan (2010), we address these challenges with a threefold approach: we employ a change-of-variables transformation, importance sampling, and Monte Carlo integration.

By changing the variables of integration from the payoff shocks ϵ_n to the final payoffs v_n, the likelihood of the data can be rewritten as

$$\mathcal{L}(D; \theta, \lambda) = \prod_{n=1}^{N} \int \left[\sum_{\sigma \in \mathcal{E}(v_n)} F(\sigma; v_n, \lambda) \sigma(a_n) \right] g(v_n - u(w_n, \theta)) dv_n. \tag{B3}$$

Using importance sampling, the integral in Equation B3 can be approximated via Monte Carlo integration as follows. Given any probability density function $h(\cdot; w_n)$ with full support, notice that

$$\int \left[\sum_{\sigma \in \mathcal{E}(v_n)} F(\sigma; v_n, \lambda)\sigma(a_n) \right] g(v_n - u(w_n, \theta)) dv_n$$
$$= \int \left[\sum_{\sigma \in \mathcal{E}(v_n)} F(\sigma; v_n, \lambda)\sigma(a_n) \right] \frac{g(v_n - u(w_n, \theta))}{h(v_n; w_n)} h(v_n; w_n) dv_n.$$

Thus, if $\{v_n^s\}_{s=1}^S$ is a random sample from $h(\cdot; w_n)$, $\mathcal{L}(D; \theta, \lambda)$ can be approximated by

$$\widehat{\mathcal{L}}(D; \theta, \lambda) = \prod_{n=1}^{N} \frac{1}{S} \sum_{s=1}^{S} \left[\sum_{\sigma \in \mathcal{E}(v_n^s)} F(\sigma; v_n^s, \lambda)\sigma(a_n) \right] \frac{g(v_n^s - u(w_n, \theta))}{h(v_n^s; w_n)}. \tag{B4}$$

To prevent simulation error from propagating across observations, we draw independent random samples $\{v_n^s\}_{s=1}^S$ of size $S = 2{,}000$ for each country n in our data.

We estimate θ and λ by maximizing the simulated likelihood $\widehat{\mathcal{L}}(D; \theta, \lambda)$. This estimator is consistent and asymptotically normal by standard arguments from the theory of importance sampling and maximum simulated likelihood (Hajivassiliou and Ruud 1994).[35] The key advantage is that, as the importance distribution $h(\cdot; w_n)$ is independent of (θ, λ), the simulated payoffs v_n^s and corresponding equilibria $\mathcal{E}(v_n^s)$ in Equation B4 can be drawn prior to optimization and remain fixed throughout the search process. This substantially lowers the computational cost of estimation. We rely on the open-source software Gambit to compute equilibria, using their polynomial support-enumeration algorithm (McKelvey, McLennan and Turocy 2016).

To mitigate potential finite-sample bias from the choice of importance distribution, we employ an iterative approach. In a first round, we draw $\{v_n^{\dagger s}\}_{s=1}^{1000}$ i.i.d. from the standard normal distribution and compute preliminary (consistent) estimates $(\theta^\dagger, \lambda^\dagger)$.[36] We then draw $\{v_n^s\}_{s=1}^S$ independently from the normal distribution with mean $u(w_n, \theta^\dagger)$ and unit standard deviation. This ensures importance draws closer to the true distribution of final payoffs, which we use to compute our reported estimates $(\hat{\theta}, \hat{\lambda})$.

For accuracy and efficiency, we use the industry-leading optimization software Knitro.[37] Our implementation relies on Knitro's Interior/Direct algorithm, to which we provide ex-

[35] A sufficient condition for asymptotic normality is that $S/\sqrt{N} \to \infty$.

[36] In this first round, for computational simplicity, we constrain the coefficients of all non-constant covariates except terrain and distance to zero.

[37] `https://www.artelys.com/solvers/knitro/`.

act first and second derivatives of the log-likelihood.[38] Standard errors for our benchmark model are calculated using the Hessian of the log-likelihood to compute an estimate of the information matrix.

Finally, to mitigate concerns about potential local maxima, we repeatedly draw random starting values for the optimization algorithm. Specifically, for our first-round preliminary estimates, we draw 3,000 i.i.d. starting values from the $N(0, 0.1)$ distribution and select the solution $(\theta^\dagger, \lambda^\dagger)$ that achieves the highest log-likelihood value. For our reported estimates, we independently draw 3,000 starting values from the same distribution but centered at $(\theta^\dagger, \lambda^\dagger)$, and we again select the solution $(\hat{\theta}, \hat{\lambda})$ that achieves the highest log-likelihood.

B.3 Identification

We briefly discuss identification of our model. A model is said to be identified if its primitives can be recovered from the observed distribution of the data. In other words, hypothetically, if sample size were not a limitation and the analyst could observe the population distribution of the data, would she be able to back out the exact configuration of the model that generated the data? Could the data have been generated by distinct instances (parameter values) of the model?

Unfortunately, it is well known that discrete games such as ours are not identified nonparametrically (Pesendorfer and Schmidt-Dengler 2008). Consequently, our model parameterizes players' utilities with specific functional forms. By itself, however, a parametric specification is not sufficient for identification. Bajari, Hong and Ryan (2010) conduct a formal identification analysis of the general class of models to which ours belongs. Here, we provide only an intuitive discussion of the features of our model that ensure identification of our parameters of interest, θ and λ. These sufficient identifying conditions are the following:

(I1) Normalization of the systematic payoff from staying out of conflict.

(I2) Known distribution of payoff shocks.

(I3) Exclusion restrictions.

(I4) Equilibrium selection mechanism is payoff-scale-invariant.

Conditions (I1) and (I2) are standard in the literature on discrete-choice models given that observed choices are determined only by ordinal utility comparisons. But, unlike discrete-choice data resulting from individual decisions driven by individual preferences, observations from discrete games constitute equilibrium behavior: they are determined by simultaneous utility

[38] Knitro offers a derivative-check option—which our implementation passes—to test the code for exact derivatives against finite-difference approximations.

comparisons by multiple players. As a result, while variation in observed choices in the single-agent context can be directly attributed to changes in utility, it is not as straightforward to recover individual preferences from variation in equilibrium behavior, even with access to a rich set of covariates. Just as in instrumental-variables regressions or other simultaneous-equations models, exclusion restrictions, (I3), prove crucial to isolating the individual components of the data generating process. Our model specification, which closely follows the civil war and intervention literature, automatically satisfies the required exclusion restrictions: continuous variables in z_m^I (e.g., geographic distance) do not enter other major powers' payoffs. This makes it possible to trace individual utilities by shifting covariates along paths on which other players' actions become dominant strategies, thereby reducing variation in observed outcomes to a single-agent decision problem.

Together, conditions (I1)-(I3) ensure identification of the payoff parameters θ, and condition (I4) is then sufficient to identify the equilibrium selection parameters λ. Intuitively, once θ is known, one can restrict attention to a region of the covariate space where the influence of the payoff shocks ϵ is relatively small so that final payoffs $v(w, \theta, \epsilon)$ are known. In this region, observed probabilities over action profiles are determined solely by the remaining unknowns, the selection parameters λ. The scale-invariance property in (I4) is simply a technical requirement for this identification argument.

C Model Fit

Table C1 presents in-sample model fit. The Civil war column corresponds to the probability of observing a civil war. The remaining columns report the probabilities of observing intervention by the five major powers. The two rows compare the observed frequency in the data with that predicted by our estimated model. Overall, in-sample model fit is strong (with the exception of French interventions).

Table C1: In-sample model fit.

	Civil war	U.S.	U.K.	France	Russia	China
Data	0.433	0.153	0.073	0.067	0.113	0.040
Model	0.423	0.153	0.119	0.147	0.141	0.089

D Equilibrium Selection Parameters

Table D1 reports estimates of the equilibrium selection parameters λ for our benchmark model. The large standard errors in the second column are a potential concern. From Equation 4, the likelihood of observing profile $a \in \mathcal{A}$ is a mixture distribution, where λ parameterizes the mixing weights over the component distributions σ (equilibrium profiles). As such, it is well known that λ may be difficult to identify, which could result in large standard errors. To investigate the extent of these issues, we employ a parametric bootstrap with 500 simulated samples to reestimate the standard errors associated with λ. The results are presented in the third column of Table D1. Note that the bootstrapped standard errors are smaller than those relying on the Hessian, which should alleviate concerns about separation and identification. Furthermore, using the bootstrapped standard errors, we reject the null hypothesis that the coefficients associated with the expected number of interveners and France's preferences over equilibria are equal to zero at the 5% level. More substantively, the results suggest that rebels and major powers are coordinating on equilibria that disadvantage France and minimize the expected number of interveners.

Table D1: Estimates of equilibrium selection parameters λ.

	Estimate	SE Hessian	SE Bootstrap
Exp. Interveners	−118.40	145.15	37.89
U.S.	−117.73	142.82	71.75
U.K.	−10.66	10.56	23.77
France	−312.58	279.12	156.85
Russia	12.44	14.89	21.50
China	275.38	335.62	168.00

D.1 Alternative specification

To examine the sensitivity of our results to the specification of the equilibrium selection mechanism, we consider three modifications. First, along with the major powers' normalized ordinal preference ranking over equilibria $\bar{r}_m$, we include in $y(\sigma, v)$ the rebels' normalized ordinal ranking, $\bar{r}_R(\sigma, v)$, similarly computed. Second, we include in $y(\sigma, v)$ a binary indicator of whether σ is an equilibrium in pure strategies. Third, we also include in $y(\sigma, v)$ a binary indicator of whether σ is Pareto dominated in $\mathcal{E}(v)$. Given this alternative specification of y and our baseline specification of players' payoffs, we reestimate our model using our baseline sample.

Tables D2-D4 present our results. The estimated payoff coefficients in Table D2 and spillover effects in Table D3 are virtually identical to their baseline counterparts. And the first six rows of Table D4 agree perfectly with Table D1. In addition, we find that equilibria that favor the rebels, are in mixed strategies, and are Pareto undominated are more likely to be played in the data.

Table D2: Alternative equilibrium selection mechanism payoff estimates θ.

	Rebels' payoffs	Interveners' payoffs
Constant	0.44	
	(0.07)	
Terrain	0.10	
	(0.03)	
GDP pc	−0.08	0.03
	(0.02)	(0.01)
Democracy	−0.04	−0.01
	(0.04)	(0.02)
Population	−0.02	−0.04
	(0.02)	(0.01)
Distance		−0.07
		(0.02)
Allies	−0.13	0.18
	(0.06)	(0.07)
Colony	0.20	0.06
	(0.08)	(0.07)
War	−0.68	5.78
	(1.08)	(1.22)
U.S.	0.39	−0.45
	(0.06)	(0.08)
U.K.	−0.40	−0.61
	(0.07)	(0.08)
France	−0.35	−0.29
	(0.06)	(0.08)
Russia	−0.05	−0.25
	(0.07)	(0.07)
China	−0.26	−0.67
	(0.06)	(0.08)
N	150	
$\log \hat{\mathcal{L}}$	-218.46	

Notes. Hessian standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

Table D3: Alternative equilibrium selection mechanism spillover effects δ.

	U.K.	France	Russia	China
U.S.	0.43	−0.28	0.05	0.37
	(0.07)	(0.06)	(0.06)	(0.06)
U.K.		0.31	0.16	0.11
		(0.07)	(0.06)	(0.07)
France			0.03	0.40
			(0.06)	(0.06)
Russia				−0.06
				(0.07)

Notes. Hessian standard errors in parentheses.

Table D4: Alternative equilibrium selection mechanism parameters λ.

	Estimate	SE Hessian	SE Bootstrap
Exp. Interveners	−116.16	209.71	21.47
U.S.	−133.16	230.66	51.25
U.K.	−10.85	18.07	13.31
France	−310.03	549.53	89.77
Russia	13.37	38.75	18.07
China	261.30	447.17	104.61
Rebels	24.60	49.03	20.90
Pure strategy	−9.57	21.12	12.01
Pareto dominated	−9.65	18.85	11.94

E Robustness

E.1 Cold War

It could be the case that the end of the Cold War fundamentally changed the strategic incentives underlying interventions in civil wars. To explore this, we reestimate our model using two subsamples of data demarcated by the end of the Cold War. We report our results in Tables E1 and E2 for the Cold War (1950–1989) subsample and Tables E3 and E4 for the post-Cold War (1990–1999) subsample. Our main results are robust to this exercise, although the Cold War estimates are more similar to the baseline estimates in Tables 1 and 2, which is unsurprising given that the Cold War dominates our time frame.

E.2 Countries with multiple civil wars

Some countries experience more than one civil war between 1950-1999. For example, the data detail two civil wars in Lebanon during our time frame. The first occurs in 1958 and involves

Table E1: Cold War payoff estimates θ.

	Rebels' payoffs	Interveners' payoffs
Constant	0.38	
	(0.08)	
Terrain	0.14	
	(0.04)	
GDP pc	-0.05	0.03
	(0.02)	(0.01)
Democracy	-0.02	-0.01
	(0.04)	(0.02)
Population	0.03	-0.02
	(0.02)	(0.01)
Distance		-0.07
		(0.03)
Allies	-0.17	0.21
	(0.06)	(0.07)
Colony	0.22	0.05
	(0.09)	(0.08)
War	-0.73	4.65
	(0.87)	(1.08)
U.S.	0.46	-0.41
	(0.06)	(0.09)
U.K.	-0.43	-0.57
	(0.07)	(0.09)
France	-0.28	-0.31
	(0.07)	(0.10)
Russia	-0.04	-0.22
	(0.07)	(0.08)
China	-0.23	-0.63
	(0.06)	(0.09)
N	127	
$\log \hat{\mathcal{L}}$	-172.50	

Notes. Standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

Table E2: Cold War spillover effects δ.

	U.K.	France	Russia	China
U.S.	0.44	−0.27	0.04	0.31
	(0.08)	(0.07)	(0.07)	(0.07)
U.K.		0.20	0.15	0.11
		(0.07)	(0.06)	(0.08)
France			0.02	0.42
			(0.07)	(0.07)
Russia				−0.09
				(0.07)

Notes. Standard errors in parentheses.

interventions from both the U.S. and the U.K., whereas the second ranges between 1975-1990 and involves no interventions. As such, there are two possible codings for the U.S. and U.K. intervention decisions. Our current rule codes both of their actions as interventions.

In general, examples like these are rare. The modal number of civil wars per country is zero, and the median is one. Nevertheless, there is a possibility that we are overstating strategic complementaries in the data if two major powers intervene in a country but do so in different civil wars. As a robustness check, we reestimate our model excluding the 15 countries with more than one civil war from the sample. The results in Tables E5 and E6 should alleviate concerns. The spillover effects indicate strong strategic complementarities even after dropping countries with multiple civil wars. Furthermore, as in Table 2, we only find evidence of strategic substitution between the U.S. and France and between Russia and China.

E.3 Country-decade observations

As discussed, our baseline analysis relies on a cross-sectional sample because the model is static. To explore the robustness of our results to a panel version of the data, we reestimate our model using country-decade observations. For each country-decade, we code the rebels as starting a civil war if the country-decade appears as a civil war in Regan's (2002) data. Similarly, we code a major power as intervening in the country-decade if it is recorded as a third-party intervener in that country at any time during that decade. As in the original sample, we average country-level and dyadic covariates within the relevant decade.

Tables E7 and E8 show that our main conclusions generally remain intact using the country-decade sample. The U.S. and Russia are the major powers most favorable to rebels on average, and the U.K. and France are the least favorable. The rebels' war payoffs decrease when a power that is allied with the host government enters the war. The country-level and dyadic covariates have similar signs, although standard errors are generally smaller with 592 rather

Table E3: Post-Cold War payoff estimates θ.

	Rebels' payoffs	Interveners' payoffs
Constant	0.13	
	(0.08)	
Terrain	0.03	
	(0.03)	
GDP pc	−0.08	0.07
	(0.03)	(0.02)
Democracy	−0.01	0.01
	(0.03)	(0.02)
Population	0.01	0.00
	(0.01)	(0.01)
Distance		−0.06
		(0.02)
Allies	0.03	0.14
	(0.05)	(0.07)
Colony	0.01	−0.08
	(0.08)	(0.07)
War	−0.90	0.64
	(0.54)	(0.75)
U.S.	0.36	−0.47
	(0.06)	(0.08)
U.K.	−0.33	−0.78
	(0.06)	(0.08)
France	−0.19	−0.53
	(0.06)	(0.07)
Russia	−0.12	−0.14
	(0.07)	(0.07)
China	0.03	−0.41
	(0.07)	(0.08)
N	148	
$\log \hat{\mathcal{L}}$	-151.65	

Notes. Standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

Table E4: Post-Cold War spillover effects δ.

	U.K.	France	Russia	China
U.S.	0.41	0.06	0.06	−0.01
	(0.06)	(0.06)	(0.07)	(0.06)
U.K.		0.16	0.24	0.35
		(0.06)	(0.06)	(0.06)
France			0.17	0.51
			(0.06)	(0.06)
Russia				−0.39
				(0.06)

Notes. Standard errors in parentheses.

than 150 observations. In addition, spillovers among major powers are still characterized by strategic complementarities. The most substantive change is that we now find less strategic substitution between the U.S. and France, indicating that strategic complementarities among Western powers might be even stronger than suggested by our baseline analysis.

E.4 Country-decade observations with initial-valued covariates

In our baseline sample, we average observed covariates over time. On one hand, averaging over the time frame minimizes measurement error. On the other, averaging may introduce post-treatment bias. For example, one reason countries may have smaller GDPs during the sample period is because they experienced a civil war. We build on the country-decade analysis in Section E.3 to gain some leverage on the extent to which the analysis may be subjected to post-treatment bias. Specifically, we use the same country-decade sample as described above but now code our exogenous covariates based on the first observed value in the decade. For instance, Iraq 1960–9 is a country-decade observation for which we use Iraq's 1960 value of GDP per capita.

Tables E9 and E10 present the results. They should be explicitly compared with Tables E7 and E8, which report coefficient estimates when using the country-decade sample but covariates are averaged over the decade. Overall, the estimated payoff parameters using the two different codings of covariates are nearly identical, suggesting that post-treatment bias is not a substantial issue in the analysis.

E.5 Military interventions

It could be the case that our choice of intervention measure biases our results. In our benchmark model, we code a major power as having intervened in a civil war if it enters Regan's (2002) data as an intervener by contributing either military or economic aid. Tables E11 and E12 present results from coding interventions only if a major power commits to military aid in

Table E5: Payoff estimates without countries with multiple civil wars.

	Rebels' payoffs	Interveners' payoffs
Constant	0.57	
	(0.09)	
Terrain	0.10	
	(0.03)	
GDP pc	−0.08	0.04
	(0.02)	(0.01)
Democracy	−0.03	−0.01
	(0.04)	(0.02)
Population	−0.03	−0.03
	(0.02)	(0.01)
Distance		−0.08
		(0.02)
Allies	−0.06	0.20
	(0.06)	(0.07)
Colony	0.31	−0.12
	(0.09)	(0.08)
War	−0.66	4.64
	(1.24)	(1.35)
U.S.	0.19	−0.52
	(0.07)	(0.09)
U.K.	−0.50	−0.76
	(0.07)	(0.08)
France	−0.35	−0.57
	(0.07)	(0.09)
Russia	−0.09	−0.37
	(0.07)	(0.09)
China	−0.23	−0.63
	(0.07)	(0.08)
N	135	
$\log \hat{\mathcal{L}}$	-153.73	

Notes. Standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

a civil war. Our conclusions remain intact even after using this more stringent coding of interventions. This suggests that rebels and major powers face the same strategic tradeoffs regardless of the type of intervention under consideration.

Table E6: Spillover effects without countries with multiple wars.

	U.K.	France	Russia	China
U.S.	0.51	−0.14	0.13	0.32
	(0.09)	(0.07)	(0.07)	(0.07)
U.K.		0.26	0.22	0.14
		(0.08)	(0.06)	(0.08)
France			0.15	0.41
			(0.07)	(0.06)
Russia				−0.14
				(0.10)

Notes. Standard errors in parentheses.

Table E7: Country-decade payoff estimates θ.

	Rebels' payoffs	Interveners' payoffs
Constant	0.18	
	(0.07)	
Terrain	0.07	
	(0.02)	
GDP pc	−0.02	0.01
	(0.01)	(0.01)
Democracy	−0.02	−0.04
	(0.02)	(0.01)
Population	0.01	−0.02
	(0.01)	(0.01)
Distance		−0.06
		(0.02)
Allies	−0.10	0.06
	(0.03)	(0.05)
Colony	0.03	0.05
	(0.07)	(0.06)
War	−0.40	−0.33
	(0.27)	(0.27)
U.S.	0.12	−0.41
	(0.07)	(0.06)
U.K.	−0.21	−0.77
	(0.06)	(0.07)
France	−0.22	−0.33
	(0.06)	(0.08)
Russia	−0.02	−0.10
	(0.05)	(0.06)
China	−0.06	−0.64
	(0.07)	(0.07)
N	592	
$\log \hat{\mathcal{L}}$	-641.64	

Notes. Standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

Table E8: Country-decade spillover effects δ.

	U.K.	France	Russia	China
U.S.	0.41	0.02	−0.11	0.57
	(0.07)	(0.06)	(0.07)	(0.06)
U.K.		0.28	0.26	−0.03
		(0.06)	(0.05)	(0.05)
France			0.13	0.30
			(0.05)	(0.05)
Russia				−0.01
				(0.05)

Notes. Standard errors in parentheses.

Table E9: Country-decade with initial-valued covariates payoff estimates θ.

	Rebels' payoffs	Interveners' payoffs
Constant	0.20	
	(0.06)	
Terrain	0.09	
	(0.02)	
GDP pc	−0.02	0.01
	(0.01)	(0.01)
Democracy	−0.03	−0.04
	(0.02)	(0.01)
Population	0.01	−0.02
	(0.01)	(0.01)
Distance		−0.06
		(0.02)
Allies	−0.09	0.03
	(0.03)	(0.04)
Colony	0.03	0.01
	(0.07)	(0.06)
War	−0.13	−0.11
	(0.1)	(0.12)
U.S.	0.10	−0.37
	(0.06)	(0.06)
U.K.	−0.22	−0.72
	(0.06)	(0.07)
France	−0.22	−0.25
	(0.06)	(0.07)
Russia	0.02	−0.10
	(0.05)	(0.06)
China	−0.12	−0.60
	(0.06)	(0.07)
N	592	
$\log \hat{\mathcal{L}}$	-644.11	

Notes. Standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

Table E10: Country-decade with initial-valued covariates spillover effects δ.

	U.K.	France	Russia	China
U.S.	0.41	−0.01	−0.07	0.55
	(0.06)	(0.07)	(0.06)	(0.06)
U.K.		0.26	0.25	−0.07
		(0.05)	(0.05)	(0.05)
France			0.13	0.30
			(0.05)	(0.05)
Russia				−0.01
				(0.05)

Notes. Standard errors in parentheses.

Table E11: Payoff estimates with more stringent intervention measure.

	Rebels' payoffs	Interveners' payoffs
Constant	0.19	
	(0.08)	
Terrain	0.12	
	(0.03)	
GDP pc	−0.05	0.04
	(0.02)	(0.01)
Democracy	−0.10	−0.03
	(0.04)	(0.02)
Population	0.00	−0.02
	(0.02)	(0.01)
Distance		−0.05
		(0.02)
Allies	0.11	0.27
	(0.06)	(0.08)
Colony	−0.09	0.11
	(0.08)	(0.07)
War	−2.68	7.43
	(1.07)	(1.29)
U.S.	0.25	−0.61
	(0.07)	(0.09)
U.K.	0.00	−0.82
	(0.08)	(0.10)
France	−0.17	−0.57
	(0.08)	(0.08)
Russia	−0.22	−0.21
	(0.07)	(0.09)
China	−0.15	−0.86
	(0.07)	(0.08)
N	150	
$\log \hat{\mathcal{L}}$	-212.39	

Notes. Standard errors in parentheses. Major-power rows correspond to major-power fixed effects on rebels' war payoffs (first column) and intervention benefits (second column).

Table E12: Spillover effects with more stringent intervention measure.

	U.K.	France	Russia	China
U.S.	0.27	−0.21	0.10	0.64
	(0.07)	(0.06)	(0.06)	(0.08)
U.K.		0.39	0.08	0.08
		(0.07)	(0.06)	(0.06)
France			0.08	0.48
			(0.07)	(0.07)
Russia				−0.12
				(0.07)

Notes. Standard errors in parentheses.

F Expanded Analysis with Direction of Interventions

Finally, we generalize our analysis to include the direction of interventions.

F.1 Model

A rebel group R chooses whether to start a civil conflict ($a_R = 1$) or not ($a_R = 0$). If R launches a civil war, then major power $m = 1, \ldots, M$ decides whether to stay out ($a_m = 0$), intervene to support the government ($a_m = 1$), or intervene to support the rebels ($a_m = 2$). As before, intervention decisions are made simultaneously. In this game, the set of feasible action profiles is $\mathcal{A} = \big\{a \in \{0,1\} \times \{0,1,2\}^M : \text{if } a_R = 0, \text{ then } a_i = 0 \ \forall i\big\}$.

Payoffs are common knowledge and take the following form:

$$v_i(a; w, \theta, \epsilon_i) = u_i(a; w, \theta) + \epsilon_i(a). \tag{F1}$$

The shock $\epsilon_i(a)$ is also drawn from the standard normal distribution and is independent across profiles and players.

The rebels' systematic payoff takes the form:

$$u_R(a; w^R, \theta) = a_R \left(x^R \cdot \beta + \sum_{m=1}^{M} \sum_{d=1}^{2} \mathbb{I}\{a_m = d\} \left[\gamma_m^d + z_m^R \cdot \gamma_0^d\right] \right), \tag{F2}$$

where $\mathbb{I}$ denotes the indicator function. Here, $\gamma_m^1 + z_m^R \cdot \gamma_0^1$ is the effect of major power m's decision to intervene for the government on the rebels' payoff, and $\gamma_m^2 + z_m^R \cdot \gamma_0^2$ is the effect of m's decision to intervene on behalf of the rebels. Let K^R denote the number of variables in x^R, and let L^R denote the number of variables in z_m^R. Our baseline model has $K^R + M + L^R$ payoff parameters for the rebels. This extended version has $K^R + 2(M + L^R)$, so we are estimating $M + L^R$ additional parameters.

For the major powers, we specify their payoffs as follows:

$$u_m(a; w^I, \theta) = \begin{cases} x^I \cdot \phi_m^1 + z_m^I \cdot \chi^1 + \sum_{m' \neq m} \left[\mathbb{I}\{a_{m'} = 1\}\delta_{m,m'}^S + \mathbb{I}\{a_{m'} = 2\}\delta_{m,m'}^O\right] & \text{if } a_m = 1, \\ x^I \cdot \phi_m^2 + z_m^I \cdot \chi^2 + \sum_{m' \neq m} \left[\mathbb{I}\{a_{m'} = 2\}\delta_{m,m'}^S + \mathbb{I}\{a_{m'} = 1\}\delta_{m,m'}^O\right] & \text{if } a_m = 2, \\ 0 & \text{if } a_m = 0. \end{cases} \tag{F3}$$

In Equation F3, $x^I \cdot \phi_m^1 + z_m^I \cdot \chi^1$ is m's baseline payoff from intervening on behalf of the government, and $x^I \cdot \phi_m^2 + z_m^I \cdot \chi^2$ is m's baseline payoff from supporting the rebels. Major power m's intervention payoffs are affected by the actions of the other major powers. If m' intervenes on the same side as m, then m receives an additional payoff $\delta_{m,m'}^S$. If m' intervenes on the opposite as m, then m receives the payoff $\delta_{m,m'}^O$. As in the baseline model, we impose symmetry, so $\delta^S_{m,m'} = \delta^S_{m',m}$ and $\delta^O_{m,m'} = \delta^O_{m',m}$ for any pair of major powers m and m'. In addition, we allow for major-power fixed effects in the baseline payoffs $x^I \cdot \phi^d_m$, but we pool coefficients associated with non-constant covariates. Let K^I denote the number of non-constant variables in x^I, and let L^I denote the number of variables in z^I_m. In this version of the model, we have $2(M + K^I + L^I + \binom{M}{2})$ payoff parameters for the major powers. In the baseline model, we have $M + K^I + L^I + \binom{M}{2}$ parameters.

F.2 Data and estimation

To code the direction of intervention, we use the `target` variable from Regan (2002), which identifies the side that the intervener supports.[39] As in our baseline, to generate a cross-sectional sample, we aggregate over time. If a major power has intervened for both the rebels and the government in a country during the 1950–99 time frame, then we take the modal type of intervention as our observation (breaking ties in favor of government support). For example, the U.S. intervenes in Guatemala on behalf of anti-government forces during the 1954 coup but then supports the government in the Guatemalan civil war between 1966-1995. We code this as intervening for the government. However, examples like these are rare. This happens twice for the U.S. (in Guatemala and Cambodia) and once for Russia (in Georgia) and China (in Malaysia). It never happens for the U.K. or France.

Estimation and inference proceed analogously to our baseline analysis, with one important exception. We can no longer efficiently compute all equilibria with five major powers and three actions. It takes more than 21 days to compute all equilibria of a single game using Gambit on a computer with a 2.3 GHz 18-Core Intel Xeon W-2195 processor. This is particularly detrimental to our estimation procedure, which requires computing all equilibria of simulated games, with 2,000 simulations per observation. As a workaround, we focus on pure-strategy subgame-perfect equilibria in this extension. Let $\mathcal{E}^{\mathrm{p}}(v)$ denote the set of pure-strategy equilibria given payoffs v. As before, $F(\sigma; v, \lambda)$ denotes the probability that equilibrium $\sigma \in \mathcal{E}^{\mathrm{p}}(v)$ is played. Using Equation B1 and the specification of $y(\sigma, v)$ described in Appendix B.1, the (simulated) likelihood of observing profile $a \in \mathcal{A}$ takes a similar form as in Equation B4.

F.3 Results

Next, we present the results of this extension of our analysis. However, we note that, given the computational challenges and added complexity associated with this version of the model—particularly in light of our finding in Appendix D that mixed-strategy equilibria are more likely to be played in the data—we view these results mainly as a robustness check on our preferred baseline specification.

[39]This variable also identifies "neutral" interventions, which we code as no intervention in this extension since the goal is to analyze efforts by major powers to shift the balance of power in a conflict.

Table F1 reports the rebels' estimated payoff parameters. In this extension, we are especially interested in the bottom rows, which report how the two types of intervention (government- or rebel-sided) affect the rebels' expected war payoff. For example, the negative estimates corresponding to the allies variable indicate that, conditional on either direction of support, interventions by major powers that have security alliances with the rebels' home government reduce rebels' benefits from civil war. Both estimates are significant at conventional levels. Intuitively, conditional on a direction of intervention, interveners who are allied with the government may use tactics or adopt bargaining positions that are less favorable to the rebels. Likewise, we find that major powers who have previously fought an interstate war with the rebels' home government are generally more favorable to the rebels when intervening on behalf of the government than those major powers who have not fought an interstate war against the government.[40] Overall, these results are substantively identical to our baseline analysis that did not include the direction of interventions.

In addition, the major-power specific effects in Table F1 also confirm the results of our baseline model. Notice that these effects are smaller (more negative) for government-sided than for rebel-sided interventions. This indicates that the major-powers are more favorable to the rebels when intervening on their side than when intervening on the government's side, an important face-validity check for the model. Furthermore, we find that U.S. interventions increase rebel war payoffs regardless of the direction, although the U.S.-specific estimate for government-sided interventions is not significantly different from zero. This mirrors the findings from our baseline analysis and suggests that the U.S. generally chooses tactics and policies that are relatively more favorable to the rebel cause, even after choosing a specific side to support. Finally, we find that the U.K. and France are generally the least supportive of rebels, in line with our baseline analysis.

To better shed light on the direct effects of major-power interventions in this version of the model, we can compute the sample average effect of m's intervention in direction $d = 1, 2$ on rebels' expected war payoffs:

$$\bar{\gamma}_m^d = \hat{\gamma}_m^d + \frac{1}{N} \sum_{n=1}^{N} z_{nm}^R \cdot \hat{\gamma}_0^d,$$

where $d = 1$ corresponds to government-sided interventions and $d = 2$ to rebel-sided interventions. These average effects and their corresponding standard errors are reported in Table F2. The table illustrates the unique position of the U.S. as the major power that is the most favorable to rebels. Even when looking at the effects of government-sided interventions, U.S. interventions decrease rebels' war payoffs by the least and may actually increase them. Besides

[40] Although the coefficient associated with interstate war is negative for the effect of rebel-sided interventions, it is not significant at conventional levels.

Table F1: Rebel payoff estimates with intervention direction.

		Estimate	SE
Country-level covariates	Constant	0.07	0.03
	Terrain	0.02	0.01
	GDP pc	0.01	0.01
	Democracy	−0.02	0.01
	Population	0.01	0.01
Gov-sided intervention	Allies	−0.05	0.02
	Colony	0.08	0.03
	War	3.15	0.49
	US	0.02	0.03
	UK	−0.08	0.02
	France	−0.10	0.02
	Russia	−0.02	0.02
	China	−0.07	0.03
Reb-sided intervention	Allies	−0.10	0.02
	Colony	−0.02	0.03
	War	−0.40	0.44
	US	0.04	0.02
	UK	−0.04	0.03
	France	−0.01	0.02
	Russia	0.01	0.02
	China	−0.01	0.02

Notes. Standard errors in parentheses. $\log \hat{\mathcal{L}} = -417.40$ and $N = 150$. See Table F3 for the major powers' payoff parameters and Table F4 for major-power spillover effects.

the U.S., Russia appears as the next most favorable major power for the rebels, whereas France and the U.K. are the least favorable.

Table F2: Sample average effects of major-power interventions on rebels' civil-war benefits.

	Gov-sided		Reb-sided	
	Estimate	SE	Estimate	SE
U.S.	0.008	0.035	0.012	0.042
U.K.	−0.061	0.043	−0.055	0.028
France	−0.089	0.043	−0.029	0.030
Russia	−0.020	0.019	−0.004	0.033
China	−0.051	0.059	−0.015	0.012

Table F3 reports the major powers' payoff parameters. As in the baseline analysis, distance deters both intervention types as it likely increases the costs of intervening. In addition, having an alliance with the government encourages major powers to launch government-sided interventions but discourages rebel-sided interventions. Together with the previous table, this result illustrates how the baseline model can pick up the nuances of intervention direction via observed covariates despite not modelling it explicitly. Namely, major powers with security alliances to governments in civil wars are likely to (i) intervene in the conflict, (ii) support the government, and (iii) choose policies that reduce the rebels' payoffs conditional on either intervention direction. Turning to the major-power fixed effects, notice that, for the U.S., the fixed effect associated with rebel-sided interventions is more than two standard errors larger than the one associated with government-sided interventions, suggesting that the U.S. has a preference for intervening on behalf of rebels.

Table F3: Major-power payoff estimates with intervention direction.

	Gov-sided Intervention	Reb-sided Intervention
GDP pc	−0.01	0.01
	(0.01)	(0.01)
Democracy	−0.02	0.02
	(0.01)	(0.01)
Population	−0.01	0.00
	(0.00)	(0.00)
Distance	−0.03	−0.02
	(0.01)	0.01
Allies	0.05	−0.09
	(0.02)	0.03
Colony	−0.01	−0.03
	(0.04)	(0.03)
War	0.03	1.16
	(0.53)	(0.50)
US	−0.05	0.02
	(0.03)	(0.03)
UK	−0.04	0.00
	(0.03)	(0.03)
France	−0.16	−0.10
	(0.03)	(0.03)
Russia	−0.24	−0.18
	(0.03)	(0.03)
China	−0.16	−0.11
	(0.03)	(0.03)

Notes. Standard errors in parentheses. $\log \hat{\mathcal{L}} = -417.40$ and $N = 150$. See Table F1 for the rebels' payoff parameters and Table F4 for major-power spillover effects.

Table F4 reports the spillover effects in the extended model. Notice that, for each major-power pair m and m', there are two spillover effects depending on whether they intervene on the same side ($\delta^S_{m,m'}$) or opposing sides ($\delta^O_{m,m'}$) of the conflict. The results broadly uncover strategic complementarities: the coefficient estimates are generally positive and significant at conventional levels. This again confirms our baseline analysis. Two important nuances emerge, however. First, among the western powers (U.S., U.K., and France), opposing interventions decrease expected payoffs. Although $\delta^O_{\text{U.S.,U.K.}}$ is estimated to be positive, it is not significantly different from zero at conventional levels. This suggests that the strategic substitution between France and the U.S. found in our baseline analysis might emerge from a desire to avoid confrontation. Second, we find that the U.S. and Russia avoid intervening on the same side of a civil war, but they do face strategic complementarities in opposing interventions. Without modeling the direction of interventions, these effects offset each other in our baseline analysis. In contrast, the extended version of our model provides more direct evidence that the U.S. and Russia compete for control during our sample period.

Table F4: Spillover effects with intervention direction.

	U.K.		France		Russia		China	
	Sup.	Opp.	Sup.	Opp.	Sup.	Opp.	Sup.	Opp.
U.S.	0.04	0.01	0.03	−0.03	−0.03	0.05	0.06	0.08
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
U.K.			0.02	−0.02	−0.01	0.02	0.02	0.04
			(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
France					0.09	0.05	0.09	0.07
					(0.02)	(0.02)	(0.02)	(0.02)
Russia							0.04	0.04
							(0.02)	(0.02)