Supplemental Material (not copyedited or formatted) for: Michael Gibilisco, Sergio Montero. 2022. "Do Major-Power Interventions Encourage the Onset of Civil Conflict? A Structural Analysis." The Journal of Politics 84(2). DOI: https://doi.org/10.1086/715602.

Online Appendix for *Do Major-Power Interventions Encourage the Onset of Civil Conflict? A Structural Analysis*[*]

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[*]This version: December 21, 2020.

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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

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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).[1] 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 $(\theta^\dagger, \lambda^\dagger)$.[2] 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.[3] Our implementation relies on Knitro's Interior/Direct algorithm, to which we provide ex-

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

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

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

act first and second derivatives of the log-likelihood.[4] 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

[4] 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.

Supplemental Material (not copyedited or formatted) for: Michael Gibilisco, Sergio Montero. 2022. "Do Major-Power Interventions Encourage the Onset of Civil Conflict? A Structural Analysis." The Journal of Politics 84(2). DOI: https://doi.org/10.1086/715602.

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 $m = 1, \dots, M$ 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:

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

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 $\delta^S_{m,m'}$. If m' intervenes on the opposite as m, then m receives the payoff $\delta^O_{m,m'}$. 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.[5] 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.

[5]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.[6] 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

[6] 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)

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