## Democracy, Economic Inequality, and State Conflict Behavior

David Kinsella (Portland State University)

Bruce Russett (Yale University)

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## Why Examine Inequality and Interstate Behavior?

It has long been evident that the standard Polity scores, while the best-accepted single measure we have for regime type, are incomplete. They largely measure the structures of political institutions, and are less adequate for measuring values and norms. Work by Bueno de Mesquita and associates is a step forward with its emphasis on the size of the selectorate and the winning coalition, but it too is concerned mainly with institutions, and indeed an important component of their measurement strategy depends on Polity scores (e.g., Bueno de Mesquita et al. 2003). However, Stein (2015) reviews a number of normative explanations that could modify the emphasis on structures, and can be measured. Among them is the degree of income/wealth inequality that reduces the size of the real winning coalition. She also provides a useful review of the existing literature using this and other proposed modifiers. One is the concept of revenge-oriented societies, as measured by retention of the capital punishment. Only 10 established democracies do so, including the United States, but none in Europe. Another is work by Dafoe and Caughey (2015) on a culture of honor and reputation for resolve in the U.S. South.

Here we focus on economic inequality. The causes and effects of inequality within states (especially democracies) have been studied extensively, but only recently emerged as a scientific topic in international relations. We are hardly the first, as Stein shows. Caverley (2014) is probably the most prominent effort, but it is hardly alone. Others, especially Solt (2009, 2011), have made good progress with a new data set on inequality (see also Solt and Lai 2013). But the biggest data innovation is that by Piketty and his group in Luxembourg, which produced and continually revises and expands the data over space and time (Alvaredo et al. 2015; Piketty 2013). It has a lot of holes in it, but we have managed to produce some datasets suitable for analysis.

Our premise here is that the size of the real winning coalition depends on the distribution of economic resources, with the rich and especially the super-rich able to buy (or at least rent) far more influence than those below them on the income scale. In the United States, superPACs, blessed by the Supreme Court, carry this to an extreme. Its effects may be manifested in particular policies, and also in efforts to reduce the selectorate still more by effectively denying the franchise to ethnic and other minorities.

We originally had two empirical goals for this paper. The first was to explore this as a dyadic phenomenon in the usual mode of much of the democratic peace literature. But existing inequality data are insufficient for the comprehensive analysis we intended. So we confine the analysis to our second goal, a monadic analysis. Doing so allows us not only to generalize about many states' behavior, but as you might guess from the previous paragraph, to examine the United States as an exceptional state in international politics. Over many decades it has engaged in more militarized conflicts than any other state. That can be partially explained by good realist variables—highest GDP of any other state, the biggest and most effective military with global interests, alliance commitments both economic and political. But we think that explanation, while correct, may not be sufficient. In terms of income and wealth inequality, the United States leads the 20 most economically developed countries, and virtually all democracies regardless of development level (Credit Suisse Research Institute 2014; World Bank 2015). So we are exploring a case of "exceptionalism" that also has roots in the culture and distribution of economic resources as modifiers of U.S. democratic institutions, with consequences for its international behavior in initiating or participating in militarized disputes. So we now turn to the nittygritty of analysis.

## **DATA ANALYSIS**

## Measuring Income Inequality

Income inequality is the main predictor of conflict that interests us here, and the availability of these data limits the scope of our analysis. The Gini index is the most familiar measure of inequality in society; it is the area between a society's Lorenz curve, a plot of the cumulative income earned by the poorest through the richest portions of the population, and the hypothetical 45-degree line representing perfect income equality. Until recently, data necessary to compute the Gini index had not been collected on an annual basis for most countries, but work by Solt (2009) has filled in many gaps through the use of Monte Carlo simulations. As we discussed above, Solt and Lai (2013) assembled sufficiently comprehensive Gini data to model the incidence of militarized disputes among politically relevant dyads as a function of income inequality and other factors, annually from 1961 to 2001.

We employ a different measure of inequality, the *share of income earned by the richest one percent of the population*. Of course, this is but one point along a society's Lorenz curve, so it provides a less complete summary of income inequality than the Gini index. However, our argument connecting income inequality to a greater probability of conflict, even by and among democracies, focuses on the influence that the richest in society can bring to bear on the policymaking apparatus of government, so this measure suits our purpose.

As a measure of inequality, our indicator is less demanding than the Gini index, which requires estimates of cumulative income along the whole range of poor to rich for each state at each time, but the information is still quite sporadic and places substantial limits on the sample of countries and time periods we can examine. The data come from the World Top Incomes Database (WTID), a data collection based on personal income tax statistics, growing out of the work of Piketty and associates (Alvaredo et al. 2015; see also Piketty 2013; Atkinson, Piketty, and Saez 2011). After filling in missing values, following the procedure described in the appendix, we are able to assemble three reasonably complete time-series cross-sections. As shown in Table 1, the longest series, 1921-2006, includes 13 countries; the 1951-2006 series includes 17 countries; and the shortest series, 1981-2006, includes 26 countries. Four countries with too few data to be included even in the shortest series are included in an "unbalanced" time-series cross-section, which consists of all observations for which there are nonmissing data.

## [Table 1 about here]

Dyadic analysis, even limited to "politically relevant dyads," is not very meaningful when confined to such a small subset of countries. Although many of the countries included in our cross-sections are democracies, especially in the most recent time period, they are not really a network that would justify focusing on bilateral relationships only within the collective. Thus, the availability of inequality data limits us to a monadic analysis: does income inequality help to predict involvement in militarized disputes with other states generally, as opposed to disputes with particular other states (i.e., democracies or nondemocracies). As we will see below, our monadic findings are interesting not only for what the reveal about the effects of inequality on the likelihood of militarized disputes, but also because they suggest that previous monadic analyses of the democratic peace have missed a piece of the puzzle.

## Other Variables in the Model

In the dyadic democratic peace literature, we find a fairly standard "benchmark" model for predicting militarized disputes, against which other models that include new explanatory variables, operational measures, controls, and estimation techniques are evaluated (e.g., Russett and Oneal 2001; Oneal and Russett 1999). The monadic proposition that democracies are more peaceful in their foreign relations with all states, rather than just other democratic states, is not well established and consequently there is no benchmark model specification.<sup>1</sup> Our specification is as follows:

*Dependent variable*. We want to know whether income inequality, along with democratic governance and other factors explain the likelihood of a state's involvement in militarized disputes. We operationalize the dispute variable two different ways, both using Militarized Interstate Dispute Data from the Correlates of War Project (Kenwick et al. 2013). We only count MIDs in which the state was an "originator" of the dispute and the dispute resulted in

<sup>&</sup>lt;sup>1</sup> Here we are referring to the monadic *proposition* that democracies are more peaceful, and the monadic *level of analysis* in which the dependent variable is a state's behavior toward any and all other states. The monadic proposition has also been tested using dyadic data. In that case, evidence for or against the proposition rests on the estimated effects of *both* jointly democratic dyads and democratic-nondemocratic dyads (e.g., Quackenbush and Rudy 2009; Huth and Allee 2002).

fatalities.<sup>2</sup> The first measure of the dependent variable is binary: whether or not the state was involved in a new dispute during a particular year. The second measure is a count: the number of disputes the state was involved in during the year, either new ones or disputes ongoing from the previous years.

*Explanatory variables*. Alongside any effect of income inequality, we are interested in whether democratic governance dampens the likelihood of a state's involvement in military disputes. As is most common in the democratic peace literature, our indicator is the Polity Project's composite measure of institutionalized democracy, which ranges from 0 to 10 (Marshall, Gurr, and Jaggers 2013). Because ours is a monadic analysis, the "joint democracy" measure typically used in dyadic democratic peace research is not pertinent. But we do include in the model a variable for the extent to which the state resides in a "democratic neighborhood." This is operationalized as the average level of democracy characteristic of neighboring states that are contiguous by land or separated by less than 400 miles of water.<sup>3</sup> The indicator combines democracy data from Polity and contiguity data from the Correlates of War (on the latter, see Hensel 2007; Stinnett et al. 2002).

Models testing the democratic peace proposition generally include control variables for state power, because powerful states tend to have expansive spheres of interest and influence, which may lead to clashes with other states. As a proxy for state power, we use gross domestic product (logged). Our longest time series dates back to 1921, so we use data from the Maddison Project Database, which collects information from historical national accounts (Bolt and van Zanden 2014). We also include a binary variable for the presence of warfare within the state. Solt and Lai (2013: 18) note the importance of including this control because economic inequality may contribute to civil wars, and civil wars to interstate conflict. With this variable in the model, we can be more confident that the estimated effect of income inequality on interstate disputes is the direct effect that interests us here, and not the indirect effect due to internal wars spilling over into international ones. We include intrastate war as a binary variable, using data from the Correlates of War: 1 if the state was experiencing a civil, regional, or intercommunal war during the year; 0 if not (see Sarkees 2010; Sarkees and Wayman 2010: chap. 2).

## Statistical Estimation

We estimate eight (nearly) identically specified statistical models, four for each of our two measures of military dispute involvement. The first of the four uses all valid observations for the thirty countries for which we have annual income inequality data (see Table 1) any

<sup>&</sup>lt;sup>2</sup> "Originator" means the state was involved at the outset, not necessarily that the state was an initiator of the dispute against some target. That is, the category of participants we exclude are "joiners," states that become involved in already ongoing disputes.

<sup>&</sup>lt;sup>3</sup> One country in our sample, New Zealand, is about 1,200 miles away from its nearest neighbor, Australia. Setting the value of this variable to 0 would misleadingly suggest that New Zealand is located in a very nondemocratic neighborhood. To avoid this problem, we count Australia as contiguous to New Zealand and vice versa.

time during the time period from 1870 to 2006. (This end year is determined by the availability of the contiguity data.) These are unbalanced time-series cross-sections, given that the time periods vary widely by country and are sometimes punctuated by missing observations. These models may have convoluted statistical properties that we should attend to before we can be fully confident in our inferences. But we also estimate three balanced time-series cross-sections, which we construct by selecting countries and fixed time periods so as to minimize the number of missing observations. Quantitative researchers have a better handle on the properties of these data structures and have developed estimation procedures designed to avoid common errors in inference. We return to this issue below.

We employ logistic regression analysis to estimate the effects on income inequality on whether a state becomes involved in any new militarized dispute, our binary dependent variable (no = 0, yes = 1). We use negative binomial regression to estimate the effects of inequality on involvement in any and all disputes, our dependent variable measured as an event count. Poisson regression is perhaps more commonly used to estimate event count models, but negative binomial regression is a generalization of the Poisson model with less restrictive assumptions about the variation in the data approximating variation in the theoretical population (King 1989a, 1989b). "Overdispersion" in the data can be a function of a large number of zero event counts, something to be expected in datasets like ours built to predict rare events like militarized interstate disputes. When over dispersion is absent, the negative binomial model reduces to the Poisson model.

## Findings

Table 2 shows the logistic regression estimates for four models predicting a state's involvement in a new military dispute. We hypothesize that income inequality increases the likelihood of military disputes and this is supported by estimates derived from the unbalanced TSCS and two of the balanced TSCSs: 1921-2006 and 1951-2006. The estimated effect of inequality variable is positive and statistically significant in these three models. The effect of democracy is negative and significant, which supports the monadic democratic peace proposition. This suggests that, within democratic states, democratic institutions and income inequality work at cross purposes, which is consistent with the argument we have developed in this paper.

## [Table 2 about here]

When states reside in more democratic neighborhoods, they are also less likely to become involved in military disputes, a result that is also consistent with the theory behind the democratic peace. State power has the opposite effect; higher levels of GDP are associated with a higher probability of military disputes with other states. Although this result more closely aligns with realist theory, it reinforces a common finding in democratic peace research, which has emphasized the importance of controlling for various explanatory factors in order to isolate the pacific effects of democracy—and, we should now add, income inequality as a countervailing force. The estimated effect of intrastate war on

interstate disputes is statistically significant only 1981-2006 TSCS, and the variable drops out of the 1921-2006 model altogether because none of those 13 states experienced intrastate conflicts during that period.

Figure 1 displays the estimated impact of income inequality—the one percent's income share—on dispute involvement. For two of the models reported in Table 1, the 1921-2006 and 1951-2006 TSCSs, we use variable means to generate the predicted probability of state involvement in a dispute. That predicted probability is the height on the vertical axis corresponding to the square (0.028 in the upper chart, 0.029 in the lower chart), which is located at the mean of income inequality variable on the horizontal axis (9.74 percent of total income in the upper chart, 8.36 percent in the lower chart). The actual means of the binary dependent variables are 0.10 and 0.11, respectively, so the models under-predict the base probability of these rare events. In any event, the predicted effect of a standard deviation increase and decrease in income inequality are shown by the adjacent circles along the curve. In the upper chart, a standard deviation increase in the one percent's income share (from 9.74 percent of total income to 13.41 percent) would predict an increase in the probability of a state's dispute involvement from 0.028 to 0.044. That is more that a 50 percent increase in the predicted probability, but of course that probability is low to begin with. The lower chart can be interpreted similarly.

[Figure 1 about here]

We can also view the predicted effects of changes in income inequality in conjunction with changes in states' levels of democracy. For example, in the upper chart of Figure 2, the probability of dispute involvement is highest (0.50) where the plane peaks in the right-front corner of the space: where the democracy score is zero and the one percent's income share is 20 percent (the highest share we observe in the sample is 19.9). It is lowest in left back corner where democracy is high and income inequality is at its lowest (3.5 percent of income in our samples). Other points on the plane correspond to different combinations of institutionalized democracy and income inequality. The lower chart in Figure 2 provides the same visualization for the 1951-2006 TSCS, but the space is truncated at democracy levels below 6 because none of the 17 states in that sample had scores that low during the period.

[Figure 2 about here]

Table 3 shows the negative binomial regression estimates for the four models when the dependent variable is operationalized as event counts. The results are very similar to the logistic regression results in Table 2 and our inferences reaffirmed. The only noteworthy difference is that the effect of intrastate war on number of new and ongoing disputes is statistically significant in the analyses of the 1870-2006 and 1951-2006 TSCSs, in addition to the 1981-2006 TSCS; the logistic regressions produced a significant parameter estimate only from the latter sample. Charts like those shown in Figures 1 and 2, but using estimates reported in Table 3, also suggest very similar interpretations of the effects of income inequality and democracy on states' dispute involvement.

## [Table 3 about here]

Models of the democratic peace at the dyadic level customarily include parameters to account for time dependence in the binary data. The most common corrective is to use natural cubic splines, as proposed by Beck, Katz, and Tucker (1998), computed using a variable that counts the number of years since the state was last involved in a military dispute. The complexity of interpreting these time parameter estimates, and the often arbitrary choice of knot locations, have been discussed by Carter and Signorino (2010), who propose the simpler alternative of including in the model a cubic polynomial approximation (that is: t,  $t^2$ , and  $t^3$ , where t is the number of years since the last dispute).

As a robustness check on the results in Table 2, we implemented both procedures (results to be included in a web appendix). The two techniques produce very similar sets of estimates for the substantive variables of interest to us, although some inconsistent estimates for the time parameters. There are two noteworthy deviations from the findings we present above. Both techniques yield a statistically insignificant estimates, at odds with those in Table 2: for the effect of democracy in the unbalanced 1870-2006 TSCS (p = 0.20 with the splines, p = 0.21 with the polynomial), and for the effect of income inequality in the 1951-2006 TSCS (p = 0.12 with the splines, p = 0.13 with the polynomial).

## CONCLUSION

We continue the emerging research program that focuses on the effect of extreme income inequality on states' international behavior. In doing so, we present a monadic analysis rather than a dyadic one that has for good theoretical ones been more common. The dyadic focus has much to recommend it because it focuses on the interactions between states derived from their similar or different political and economic ties. But monadic analysis has taken a back seat, in part due to its inability to explore the interactions, giving conflicting results that have not been sufficiently explained. This paper makes a case for returning to monadic analysis to identify the overall conflict behavior of states, and the effect of great economic inequality. It throws some light on the behavior of particular states, and gives a new perspective on a form of United States exceptionalism. Its implications for research on the democratic peace emphasize the need to move beyond conventional institutional measures of democracy to economic and cultural conditions that modify the effect of institutions.

## **APPENDIX: MISSING INEQUALITY DATA**

There are many gaps in the inequality indicator we employ in this analysis, the income share of the top one percent of the population. WTID reports data based on different income generating unit: (i) households, (ii) married couples and single adults, and (iii) adults. If an observation is missing for the household-level indicator, we use the indicator for married couples and single adults; if that is missing, we use the indicator for adults. However, that still leaves many gaps in the data. Any gaps in the time series of 1, 2, 3, or 4 years, we fill by means of linear interpolation, but we also interpolate to fill select longer gaps. All interpolated gaps in the data are shown in Table A1.

Depending on the time period analyzed, this procedure results in the use of interpolated inequality data for between 12 and 15 percent of all observations, so we must acknowledge having taken some liberties here. On the other hand, because these are linear estimates, we do not believe that our data analysis inflates the explained variance in MIDs.

[Table A1 about here]

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	Balanced TSCSs			
	1921-2006	1951-2006	1981-2006	
Argentina				
Australia	х	x	x	
Canada	х	х	х	
China				
Colombia				
Denmark	х	х	х	
Finland	х	х	х	
France	х	х	х	
Germany	х	х	х	
India		х	х	
Indonesia			x	
Ireland		х	х	
Italy			x	
Japan	х	х	х	
Korea			х	
Malaysia			х	
Mauritius			х	
Netherlands	х	х	х	
New Zealand	х	х	х	
Norway	х	х	х	
Portugal			х	
Singapore			х	
South Africa		x	x	
Spain			х	
Sweden	х	х	х	
Switzerland		х	х	
Taiwan			х	
Tanzania				
United Kingdom	х	х	х	
United States	х	х	х	

# **TABLE 1: Three Time-Series Cross-Sections**

	Unbalanced TSCS	Balanced TSCS		
Predictor	1870-2006	1921-2006	1951-2006	1981-2006
Democracy Score	-0.052*	-0.236***	-0.626***	0.033
	(0.030)	(0.050)	(0.122)	(0.057)
One Percent's Income Share	0.106***	0.123***	0.121**	0.057
	(0.021)	(0.033)	(0.053)	(0.085)
Contiguous States' Democracy	-0.212***	-0.108***	-0.165***	-0.265**
	(0.040)	(0.041)	(0.046)	(0.121)
GDP (natural log)	0.598***	0.604***	0.627***	0.784***
	(0.067)	(0.094)	(0.112)	(0.225)
Intrastate War	0.566		0.893	1.631***
	(0.429)		(0.593)	(0.571)
Constant	-9.730***	-8.904***	-4.934***	-12.922***
	(0.953)	(1.347)	(1.431)	(2.689)
Observations	1771	1083	937	612
Number of States	30	13	17	26
Log-likelihood	-353.1	-195.0	-184.6	-88.8

# **TABLE 2: Regression Results for Predictors of New Militarized Disputes**

Binary logistic regression estimates with robust standard errors in parentheses. Dependent variable is 1 if state was an originator of a new fatal MID in a given year, and 0 otherwise. Statistical significance: \*\*\* 0.01 level, \*\* 0.05 level, \* 0.10 level

13 States, 1921-2006



17 States, 1951-2006





13 States, 1921-2006



17 States, 1951-2006



FIGURE 2: Combined Effect of Democracy and Inequality on Militarized Dispute

	Unbalanced TSCS	Balanced TSCS		
Predictor	1870-2006	1921-2006	1951-2006	1981-2006
Democracy Score	-0.071***	-0.266***	-0.407***	-0.005
	(0.024)	(0.022)	(0.083)	(0.040)
One Percent's Income Share	0.102***	0.079***	0.122***	0.066
	(0.014)	(0.024)	(0.036)	(0.047)
Contiguous States' Democracy	-0.216***	-0.149***	-0.205***	-0.268***
	(0.029)	(0.028)	(0.036)	(0.078)
GDP (natural log)	0.624***	0.698***	0.634***	0.816***
	(0.044)	(0.065)	(0.085)	(0.135)
Intrastate War	0.499*		0.754***	1.584***
	(0.260)		(0.229)	(0.251)
Constant	-9.030***	-8.721***	-6.423***	-12.710***
	(0.612)	(0.818)	(0.941)	(1.743)
Observations	1771	1083	937	612
Number of States	30	13	17	26
Log-likelihood	-573.2	-303.9	-300.4	-134.3

TABLE 3: Regression Results for Predictors of New and Ongoing Militarized Disputes

Negative binomial regression estimates with robust standard errors in parentheses. Dependent variable is number of new and ongoing fatal MIDs that a state was involved in during a given year as an originator. Statistical significance: \*\*\* 0.01 level, \*\* 0.05 level, \* 0.10 level

# Table A1: Data Interpolation

	Ba	alanced TSC	Ss	Interpolated Gaps					
	1921-2006	1951-2006	1981-2006	1 year	2 years	3 years	4 years	Longer	
Argentina				1955, 1957, 1960					
Australia	х	х	x						
Canada	х	х	х						
China									
Colombia									
Denmark	х	х	х	1916, 1969, 1973			1904-07	1909-14	
Finland	х	х	х						
France	х	х	х					1906-14	
Germany	х	x	x		1930-31, 1966-67, 1969-70, 1972-73, 1975-76, 1978-79, 1981-82, 1984-85, 1987-88, 1990-91, 1993-94, 1996-97, 1999-00, 2009-10			1920-24, 1939-49, 1951-56	
India		x	x	1942, 1946, 1963, 1969, 1972					
Indonesia			х	1920, 1997	1988-89, 1991-92, 1994-95	1935-37	1983-86		
Ireland		x	х				1939-42	1944-74	
Italy			х		1996-97				
Japan	х	x	х	1946					
Korea			х					1986-94	
Malaysia			х	1987, 2004		1952-54, 2006-08	1947-50, 1989-92, 1996-99	1976-82	
Mauritius			х	1992, 2009	1950-51, 1973-74, 1999-00				
Netherlands	х	x	x	1940, 1951, 1963, 1965, 1974, 1976	1960-61, 1968-69, 1971-72	1947-49, 1978-80, 1982-84, 1986-88	1942-45		
New Zealand	x	x	x	1961	1931-32		1941-44		
Norway	х	x	x	1956	1904-05, 1911-12	1889-1891, 1907-1909		1876-87, 1914-28, 1930-37, 1939-47	
Portugal			x					1983-88	
Singapore			x	1955, 1992					

	Balanced TSCSs			Interpolated Gaps					
	1921-2006	1951-2006	1981-2006	1 year	2 years	3 years	4 years	Longer	
South Africa		х	х	1913, 1960, 1962, 1966, 1968, 1970, 1989	1972-73, 1976-77			1994-01	
Spain			х						
Sweden	x	x	x	1942	1917-18	1904-06, 1908-10, 1913- 1915, 1931-33		1921-29, 1936-40	
Switzerland		x	x	1935, 1944, 1946, 1948, 1950, 1952, 1954, 1956, 1958, 1960, 1962, 1964, 1966, 1968, 1970, 1972, 1974, 1976, 1978, 1980, 1082, 1984, 1986, 1988, 1990, 1992, 1994					
Taiwan			х						
Tanzania									
United Kingdom	x	х	x	1950, 1961, 1980, 2008				1920-36, 1938-48	
United States	x	x	х						