

Heterogeneity in the Militarized Interstate Disputes (MIDs), 1816–2001: What Fatal MIDs Cannot Fix*

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We examine a major source of heterogeneity across cases in the Correlates of War Militarized Interstate Dispute Dataset, 1816–2001, and demonstrate that this variation across cases biases most analyses of conflict. Disputes are coded using two logics—the familiar state-to-state militarized action represents one case while the second relies on sponsor governments to protest state targeting of private citizens. We show that the latter introduces additional measurement bias and does not match well the original conceptualization of what constituted a dispute. The protest-dependent cases are caused by different processes, and omitting them from analyses provides truer estimates of the effects of most conflict predictors. We find that previous controls for heterogeneity in the dispute data—such as using fatal militarized interstate disputes only—substantially underestimates the dangerous effects of contiguity and the pacifying effects of regime similarity. We also show that governments are seldom willing to risk militarized conflict for private citizens during these unique cases. We provide a list of the protest-dependent cases for future conflict analyses.

For some time we have known that our principle measure of international conflict—the militarized interstate dispute (MID)—constitutes a grab bag of multiple types of disputes and even non-conflict interactions between states in the international system. This is why scholars often appeal to analyses of disputes with fatalities to isolate those cases that are the “serious disputes” in the data. This decision is problematic for many reasons, but, most importantly, this type of escalation-based selection prevents us from understanding how potentially dangerous disputes can be de-escalated before fatalities ever occur. We know that heterogeneity in the dependent variable exists, but this simple selection rule ultimately decreases our ability to understand the causes of peace and conflict.

After a review of the MID data coding procedures, we have noted that there are two principle logics by which an historical event is considered a militarized dispute. First is the most common path: one state threatens, displays, or uses force against another state. The second type of dispute is what we term the “protest-dependent” MIDs. These are dispute cases in which the militarized incident that defines the dispute is not between two states, rather, the dispute begins with a militarized action against a private citizen of another state, and then that person’s government protests the action. By coding rule, these protest-dependent cases are included alongside the majority of disputes in which one state targets the government of another state with its military, even though the actions in these cases are actually matters of international and domestic law.

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The protest-dependent cases are numerous—319 in all or more than 13 percent of all disputes in the MID data set—and include attacks on private shipping, seizures of fishing boats and other private crafts, as well as hot-pursuit chases of rebels and accidental border crossings. Significant portions of these cases are isolated temporally during the world wars and the Tanker War between Iraq and Iran in the 1980s, but the entire set of cases is large enough to affect inferences across the time span of most conflict analyses. We demonstrate this with analyses of non-protest cases that differ substantially from analyses using all disputes—differences that are even starker when compared with fatal-MID-only analyses.

THE TWO LOGICS OF CODING AN MID

Jones, Bremer and Singer (1996, 169–70) developed specific coding rules for militarized incidents that would otherwise be questionable or unclear, and these again guided subsequent iterations of the data set. Beginning with the militarized incident, and then aggregating these incidents to the militarized dispute, inclusion in the data set was based on whether the actions were codeable using seven specific criteria. First among these criteria is that the “militarized incident must occur among, and be explicitly directed towards, one or more interstate system members...” However, the rest of that criterion is altered by the following phrase: “if another system member undertakes a militarized action or diplomatically protests actions taken by another state against a non-system member within its boundaries, a militarized incident between the two system members is said to exist.” Further, the sixth criterion explicitly mentions private citizens—the only coding rule to do so:

Actions taken by the official forces of one state against private citizens of another state are generally not coded as militarized incidents. Exceptions include seizures (of personnel or material) within the confines of disputed territory, attacks on international shipping, and the pursuit (by air, land or sea) of rebel forces across international boundaries. Further, such incidents are included only when the targeted state responded militarily or protested diplomatically (Jones, Bremer and Singer 1996, 170).

This coding rule was not mentioned in the original MID data release (Gochman and Maoz 1984), but the rule has been followed in later iterations of the MID data set. The effect of this criterion has been to create dispute cases—319 MIDs or over 13 percent of the data set, 1816–2001—that involve no actual state-versus-state militarized actions. These cases include attacks on civilian shipping interests, attacks on/seizures of fishing vessels and other civilian crafts that violate territorial waters, hot-pursuit chases of rebels across a border, attacks on non-maritime civilian targets, and seizures of non-maritime persons and property. In these cases, coded as *interstate* militarized disputes, one state’s seizure or attack on non-state actors or their property is protested by the civilians’ country of origin on their behalf, and this is then coded as a militarized dispute *between the two states*. This coding rule change has loosened the concept of a militarized dispute significantly and added unnecessary heterogeneity to the data.

Using Fatal MIDs as a Corrective

Our field acknowledged long ago that not all MIDs are the same. Perhaps the first to document this was Hegre (2000, 13), who wrote: “Disputes with battle-deaths are more clearcut examples of militarized disputes than those not involving fatalities.” This brilliantly simple correction of using only fatal MIDs in analyses has been incredibly influential over the last 15 years. Using

Google Scholar, we estimate that well over 400 articles, including some of our own, have used fatal MIDs as a dependent variable in at least one analysis. The implication in all of these studies is, again, that these “serious disputes” comprise the cases where conflict escalation is most likely.

Nevertheless, this corrective will overwhelmingly select the already-escalated disputes in the data while also missing, by definition, those dangerous cases that were resolved peacefully, before fatalities broke out. This will have consequences for those studies that examine the causes of peace and de-escalation and will introduce bias into the effects of variables that are influential both before and after disputes cause fatalities. As Hegre (2000, 13) pointed out “In a trial run with Oneal & Russett’s (1997) dataset... [the use of fatal MIDs] resulted in a higher level of significance for their interdependence variable, despite the loss of cases.” If a selection effect is present, though, interdependence among “serious disputes” could have a weaker or stronger effect depending upon how those no-fatality cases behave—we do not know and, more importantly, cannot know.

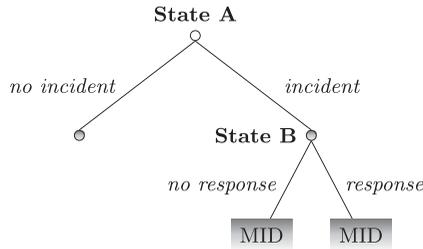
There are also 272 disputes between 1816 and 2001 in which the fatality measure in the MID data is missing, and we note that few scholars have investigated these disputes to determine whether the missing variable denotes the coder’s inability to determine fatalities or the inability to specify the range of fatalities. Reporting how these cases are treated is also uncommon. This is important because missingness does not necessarily indicate a lack of severity, or even a lack of fatalities, and bias will be introduced if the missingness is correlated with independent variables of interest.

The Logic of Conflict in Protest-Dependent Disputes

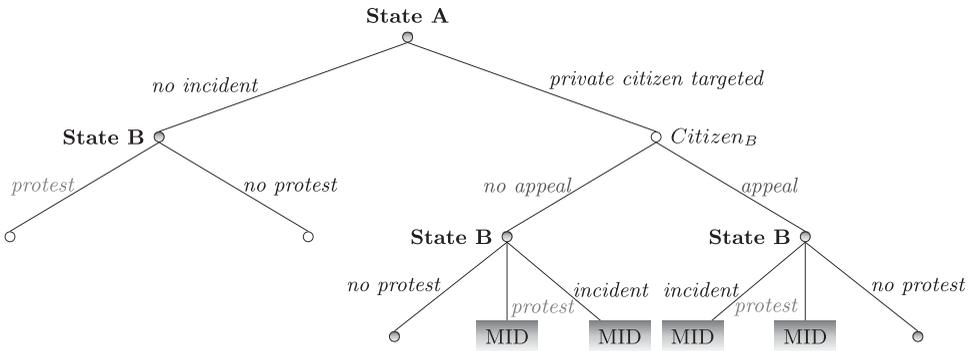
We have outlined how MID coding rules introduced the targeting of citizens into the data set. These are based on the inclusion of some form of state-sponsored protest by the private entity’s state, and we label these as protest-dependent MIDs. Theoretically, the protest coding rule was probably meant to aid reliability and reduce uncertainties regarding the coding of violent state actions outside national borders. Practically, though, the coding rule creates a set of cases in which no actual state-to-state militarized action takes place. State A may target a private citizen, without any intention of involving State B, and then State B responds with a diplomatic protest. There is no threat, display, or use of force against State B in this case, except by virtue of the coding rule.

Figure 1 describes the coding processes for militarized disputes and the protest-dependent cases. The first logic observes that a Correlates of War (CoW)-defined state in the international system initiates a threat, display, or use of force against another state in the international system. The second state—State B in the figure—need not respond because the militarized incident creates the dispute in the data set (Gochman and Maoz 1984; Jones, Bremer and Singer 1996).

Contrast that straightforward coding rule with the second logic described in Figure 1. In the second scenario, the initiating state targets a private citizen or entity. The action of the initiating state is not (yet) a militarized incident because the target is not a state in the international system, which is a requirement of incident coding rule #1 (Jones, Bremer and Singer 1996, 169). The private citizen may appeal to its government to launch a diplomatic protest. If the citizen does appeal, a host of factors may influence government officials’ decision on whether to launch a protest. Perhaps the citizen or company is politically influential or wealthy, perhaps the action is the fifth such similar event initiated by the other state, perhaps regime type matters, or perhaps there are elections coming up and the government in power wants to make an issue.



Logic #1: Dispute occurrence based solely on state-sponsored, militarized actions



Logic #2: Dispute occurrence after state action, domestic politics, and diplomacy

Fig. 1. Two logics of coding militarized interstate disputes

That uncertainty is the problem—we do not know the motivations of launching a protest and have never modeled the variables that affect inclusion into the data set.

Even without a citizen appeal, it could be the case that the government officials launch a diplomatic protest because of some conflict directed at the initiating state rather than anything related to the target of the original event. Government officials use the rival’s actions against private citizens to their advantage and launch a diplomatic protest. Again, even in this case, we do not understand or even consider the processes that make these types of protests likely, and this is yet another process by which cases enter the data set.

Modeling the causes of protest, which would be necessary to avoid introducing bias into most analyses, would require data on at least a random sample of all possible state-targeting-of-civilian instances that could invoke this coding rule. Of course, we have no such data. There is no data set of all the seizures of and/or attacks on private citizens since 1816. There is also no data on all of the accidents and mistaken border crossings that could have been protested. There is no way of knowing the composition of the cases that may have been disputes, which are labeled as “no protest” in the third level of the decision tree.

The conditionality of this coding rule can be expressed in yet another way as well. Other actions against private citizens not included in the MID coding rules as well as non-militarized actions by rival states can all be protested by the government. This is the left-hand side of the second logic in Figure 1, and there are many such instances of protest. For example, the CoW Diplomatic Exchange Dataset includes over 5000 cases in which a charge d’affaires, counselor, or ambassador was expelled, recalled, or withdrawn (Bayer 2006). These are high-level protests and not necessarily based on the cases represented in the MID data set; in fact, we found few cases in which there was more than a temporal/rival connection between the data sets of the two

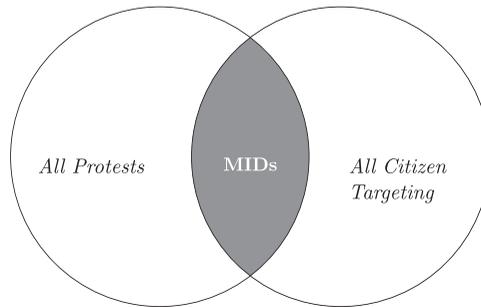


Fig. 2. Protest-dependent cases and data availability

Note: Militarized interstate disputes: union of two data sets with unknown sample size and defined by intersection of multiple empirical observations that are not always well represented in the media.

events. This means we know that missing data is likely to be substantial in the processes that code these cases.

In Figure 2, there are three types of international events: all protests by State B, all citizen targeting by State A, and the union of these that represent MIDs. We know that high-level protests at the state-to-state level are over twice as numerous as the MID data itself, and these do not include all of the lower-level cases of action-specific protests by states which are numerous from year to year. In other words, we do not have the universe of protest cases to investigate the possible union with citizen targeting. Worse still is that we have no idea how many times governments have targeted foreign citizens. Our preliminary searches for data of this type suggest that these cases occur frequently, to the point of being commonplace in any given year. Together, the existence of a dispute is the union of two sets of observations—citizen targeting and state protest—whose composition remains unknown.

HOW PROTEST-DEPENDENT DISPUTES AFFECT INFERENCE

To analyze whether there are systematic differences when analyzing the dispute cases that include protest-dependent MIDs, we estimate a simple model of conflict onset using four different dependent variables over the time period 1901–2001. We focus on this time period because, as the last section demonstrated, almost 95 percent of the cases are during these 100 years. The dependent variables include, in order across columns 1 through 4, all disputes currently in the CoW MID data set, all fatal disputes in the same data set, all protest-dependent disputes, and all cases of dispute that involved militarized incidents that did not require protest.

We identify all protest-dependent disputes through examinations of the militarized incidents for each MID case (see Gibler, Miller and Little N.d.). As per Jones, Bremer and Singer (1996, 169–70), protest-dependent disputes are those cases that involve only (1) state-sanctioned targeting of a private citizen of another state (2) that was later protested by the private citizen's government. Disputes that begin with the targeting of a private citizen but then include militarized, state-to-state actions are not included. Protest-dependent disputes are unique events, absent other militarized actions, that are state protested.¹

Our independent variables remain the same for each model. We identify the land contiguous dyads with a dummy variable based on the CoW contiguity data (Stinnett et al. 2002). All dyads

¹ The protest-dependent disputes are listed and grouped by category in the Appendix. As a supplement to our paper, we also provide a Stata .do file and an R script that both identify these protest-dependent cases so that future studies can either control for or omit these cases in their analyses.

TABLE 1 *Correcting for Heterogeneity Across Militarized Interstate Disputes (MIDs), 1901–2001*

	All MIDs No Corrections	Fatal MIDs Only	Protest-Dependent MIDs Only	MIDs Without Protest Cases	Effect of MID Correction (%)	
					Fatal MIDs	Non-Protest
Contiguous	3.173*** (0.132)	4.185*** (0.273)	1.960*** (0.255)	3.352*** (0.137)	-72.84	-9.11
Allied	0.168 (0.122)	-0.145 (0.195)	0.230 (0.308)	0.163 (0.125)		
Major–minor dyad	1.881*** (0.140)	0.865** (0.310)	1.968*** (0.206)	1.812*** (0.151)	-94.04	-22.77
Capability ratio	0.982*** (0.176)	0.654* (0.312)	0.973*** (0.286)	0.950*** (0.183)	-88.13	-20.03
Joint democracy	-0.966*** (0.127)	-0.849*** (0.248)	-0.906*** (0.222)	-0.953*** (0.138)	-84.58	-18.26
Joint autocracy	-0.473*** (0.103)	-0.381 (0.201)	-0.501*** (0.194)	-0.458*** (0.109)	-85.35	-19.45
Peace years	-0.294*** (0.0176)	-0.325*** (0.0334)	-0.262*** (0.0339)	-0.293*** (0.0190)		
Spline 1	5.164*** (0.483)	5.797*** (1.007)	4.655*** (0.966)	5.106*** (0.528)		
Spline 2	-9.898*** (1.058)	-11.12*** (2.299)	-8.939*** (2.126)	-9.762*** (1.159)		
Spline 3	5.208*** (0.742)	5.821*** (1.752)	4.735*** (1.498)	5.105*** (0.817)		
Constant	-4.718*** (0.120)	-6.677*** (0.237)	-6.343*** (0.207)	-4.977*** (0.129)		
N	606,175	606,175	606,175	606,175		

Standard errors are in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

with an alliance are coded according to Gibler (2009). We measure status differences, coding a dummy variable for the presence of one major state and one minor state in the dyad (Correlates of War Project 2011). Capability ratios are computed using the weaker state's Composite Index of National Capabilities score divided by the stronger state's score (Singer, Bremer and Stuckey 1972); this produces a measure in which dyads at parity will approach a score of 1. We include two dummy variables for regime type—one measures the presence of two democracies in the dyad and one identifies two autocracies. Both use Polity IV combined democracy/autocracy indices, with democracy coded as 6–10 and autocracy coded as -6 to -10 (Marshall and Jaggers 2002). Finally, temporal corrections are included with peace years and its cubic splines (Beck, Katz and Tucker 1998). Table 1 describes the effects of these predictors on the four different dependent variables and logistic regression.

The sample size is large, including all non-directed dyad years over 100 years, so all but one of the predictors have small standard errors and are statistically significant at conventional levels. Only the presence of an alliance has no effect in any of the models. To demonstrate the effects of the various assumptions we place on these models, we compare the estimates for each independent variable in the all-MID model to both the fatal-MID-only model and the estimates of the model using only MIDs that are not protest dependent. We want to establish how much bias correction is forced on the model by analyzing only fatal disputes rather than using the decision rule we advocate—basing disputes on the presence of state-to-state militarized actions.

We compute these differences using marginal effects as the regressions are non-linear. To do this we first calculate the average marginal effect of each independent variable on the dependent variable in that particular model. We then compare that effect to the substantive findings of the same variable using a different dependent variable. The last two columns provide these effects—one column compares fatal MIDs to all MIDs and the other column compares the non-protest-dependent cases to all MIDs.

If we were to assume that the cases of fatal disputes were a good correction for heterogeneity in the MID data set, then the estimates of the effects of contiguity would decrease by ~73 percent, the effects of status difference would decrease by 94 percent, parity would decrease by 88 percent, and joint regime status effects would decrease by about 85 percent. These are substantial decreases in the estimated effects for each variable, especially compared with the heterogeneity corrections found by using the disputes that include at least one state-to-state militarized action. As the last column of Table 1 demonstrates, for non-protest disputes, the decreased effect of each variable ranges from one-eighth to one-fourth the size of the fatal-MID corrections.

We believe this is good evidence that our theoretically driven correction of excluding protest-dependence when analyzing MIDs is appropriate. This correction does not select cases based on processes that are correlated with the escalation of dispute fatalities, provides estimates that are approximately consistent with the full data set, and eliminates a substantial source of measurement error in the data. In short, we believe that these estimates demonstrate well the presence of coding heterogeneity in the MID data as well as how to correct for it.

CONCLUSION

Heterogeneity within the MID dataset has long been recognized as a potential problem for developing unbiased inferences regarding the escalation of conflict—not all disputes are the same. We demonstrate here that the popular correction of using fatal MIDs as a proxy for serious disputes reduces the estimated effect of several commonly used international conflict variables. Fatal-MID-only analyses also ignore the dangerous disputes that are resolved before fatalities. Most importantly for our argument, this atheoretical quick fix ignores the theoretical heterogeneity across cases in the data set.

Instead, we note that there are two logics in the MID coding rules—one that relies on state-to-state militarized actions and one that depends on government protests against private citizens. These are theoretically different types of disputes that are likely to have different processes that escalate the conflicts as well. The protest-dependent disputes also require more assumptions and more work for the coders of these cases.

We do find that protest-dependent disputes are systematically different, both logically and empirically, from the cases of state-to-state conflict that comprise the rest of the data set. These differences will add bias into analyses any time they are correlated with the independent variable(s) of interest. Thus, ignoring these MID cases, or at the very least controlling for them, presents a theoretically richer alternative to controlling for heterogeneity in the data set. We provide a list of these cases in the Appendix to this paper; we also provide statistical software that identify these cases for future analyses.

Perhaps what is most important for this investigation is that we demonstrated that current scholarship is actually *underestimating* the effects of several common indicators of dangerous dyads. We know that states that border each other are more likely to fight each other, but we also find that the effect is much larger than was first suspected. The same is true for estimates of regime type. Pairs of autocracies and pairs of democracies are both less likely to fight each other than analyses with the protest cases would have us believe.

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APPENDIX: MILITARIZED INTERSTATE DISPUTES BECAUSE OF GOVERNMENT PROTEST

Table A1 divides the 319 protest-dependent cases into five separate types of cases, which we identified using dispute narratives we have developed over time (see Gibler, Miller and Little N.d.). The table begins with the most numerous category, attacks on shipping. These are cases during an ongoing conflict in which one side starts to target the shipping of the civilian craft of non-participant states. For example, Germany during the world wars and Iran and Iraq during the Tanker War attacked merchant shipping at an extraordinary rate. Not all of their attacks are included in the MID data (because of lack of state protest), and there are numerous MID data set cases in which we could not verify a state protest; however, there still remain almost 100 incidents in which one of these states attacked a merchant ship and the state protested diplomatically on behalf of the ship's owners (again, 5 percent of the MID data set). Only five cases in this category were not coincident to the world wars or the Tanker War.

Our second category of cases includes fishing rights and territorial water cases. These cases constitute private vessels seized during peaceful periods. Major states instigate a large proportion of these cases—the United States, Russia, and China are responsible for 35 cases combined. Both Ecuador and South Korea seized or attacked ships in 13 separate cases, defending their territorial waters. Meanwhile, the remaining three categories have fewer dyadic clusters, but we still see a concentration of events started by the major states, with the United States, the United Kingdom, Russia, and China accounting for 24 of the remaining cases.

Overall, there is a substantial trend of mixed-regime dyads experiencing these protest-dependent disputes, whereas only 23 of the cases involve joint democracies. The categories are also more likely to be coded as policy-based or regime-oriented rather than territorial, and the prevalence of major states suggests a correlation with (non)-contiguity. If these initial observations are true, then the presence of these cases will bias our inferences on the effects of regime type, major status, issue type, and contiguity.

TABLE A 1 *Protest-dependent MIDs, 1816-2001*

Categories	State A	State B	Start Year(s)	Total	
Attacks on civilian shipping	255—Germany	Various	1914, 1915 (2), 1917, 1940 (4), 1941 (3), 1942 (6), 1943	18	
	325—Italy	365—Russia	1937, 1940 (3), 1942	5	
	600—Morocco	040—Cuba	1980	1	
	630—Iran	Various	1984 (5), 1985 (9), 1986 (4), 1987 (21), 1988 (7)	46	
	645—Iraq	Various	1982 (2), 1983 (1), 1984 (9), 1985 (11), 1987 (8), 1988 (7)	33	
	652—Syria	338—Malta	1989	1	
	713—Taiwan	Various	1955, 1956	2	
	840—Philippines	740—Japan	1982	1	
	Fishing and territorial waters seizures/attacks	002—United States	Various	1967, 1969, 1971, 1972 (2), 1976, 1977 (2), 1979, 1984, 1987, 1991, 1997	13
		020—Canada	220—France	1988	1
090—Guatemala		092—El Salvador	1970	1	
100—Colombia		093—Nicaragua	2001	1	
101—Venezuela		Various	1968 (2), 1996, 1999	4	
130—Ecuador		002—United States	1952, 1955, 1963, 1967, 1971, 1972, 1980	7	
135—Peru		002—United States	1955, 1962, 1969, 1979	4	
160—Argentina		Various	1907, 1968, 1977, 1986	4	
200—United Kingdom		Various	1939, 1996	2	
205—Ireland		230—Spain	1985	1	
220—France		230—Spain	1984	1	
265—East Germany		260—Germany	1967	1	
339—Albania		325—Italy	1955	1	
345—Yugoslavia		339—Albania	1976	1	
365—Russia		Various	1933, 1950, 1955 (2), 1958, 1959, 1960, 1961, 1962, 1967, 1968, 1998, 2000	13	
385—Norway		365—Russia	1956, 1998	2	
395—Iceland		260—West Germany	1974	1	
411—Equatorial Guinea		368—Lithuania	2000	1	
490—Democratic Republic of the Congo		551—Zambia	1990	1	
531—Eritrea		679—Yemen	1997, 1999	2	
600—Morocco		230—Spain	1979	1	
620—Libya		325—Italy	1978	1	
630—Iran		645—Iraq	1989	1	
640—Turkey		350—Greece	1978	1	
652—Syria		660—Lebanon	1976	1	
710—China		Various	1951, 1953, 1958, 1959, 1966, 1968, 1976, 1994, 1995	9	
731—North Korea		Various	1989, 1995	2	
732—South Korea		740—Japan	1955, 1958, 1966, 1977, 1982, 1999	6	
775—Myanmar		800—Thailand	1975, 1980, 1982, 1987	4	
816—Vietnam		Various	1977, 1982	2	
817—South Vietnam		710—China	1959, 1961	2	
820—Malaysia		Various	1988, 1992	2	

TABLE A1 (Continued)

Categories	State A	State B	Start Year(s)	Total
Attacks on civilians and civilian property	840—Philippines	Various	1959, 1968, 1982	3
	850—Indonesia	Various	1963, 1964, 1965 (2)	4
	910—Papua New Guinea	740—Japan	1988	1
	986—Palau	840—Philippines	2000	1
	040—Cuba	002—United States	1996	1
	101—Venezuela	100—Colombia	1982, 2000	2
	200—United Kingdom	225—Switzerland	1940	1
	220—France	Various	1960, 1985	2
	315—Czechoslovakia	260—West Germany	1986	1
	350—Greece	339—Albania	1997	1
	355—Bulgaria	666—Israel	1955	1
	365—Russia	Various	1914, 1943, 1983, 1987	4
	517—Rwanda	516—Burundi	1972	1
	530—Ethiopia	625—Sudan	1986	1
	552—Zimbabwe	571—Botswana	1975	1
	630—Iran	Various	1981, 1999	2
	645—Iraq	640—Turkey	1965	1
	651—Egypt	002—United States	1964	1
	652—Syria	640—Turkey	1989	1
	663—Jordan	666—Israel	1959	1
	666—Israel	Various	1965, 1973, 1981	3
	700—Afghanistan	630—Iran	1934	1
740—Japan	710—China	1927	1	
770—Pakistan	775—Myanmar	1959	1	
820—Malaysia	840—Philippines	1985	1	
Seizures of civilians and civilian property	002—United States	365—Russia	1981, 2000	2
	040—Cuba	002—United States	1974	1
	070—Mexico	002—United States	1886	1
	093—Nicaragua	100—Colombia	1994	1
	100—Colombia	002—United States	1834, 1896	2
	101—Venezuela	Various	1901, 1994	2
	135—Peru	002—United States	1858, 1859	2
	140—Brazil	Various	1968, 1983	2
	155—Chile	002—United States	1857	1
	160—Argentina	255—Germany	1939	1
	200—United Kingdom	Various	1845, 1899, 1914, 1915, 1916, 1939, 1940 (5)	11
	255—Germany	Various	1939 (3)	3
	325—Italy	200—United Kingdom	1911	1
	339—Albania	200—United Kingdom	1957	1
	360—Romania	255—Germany	1914	1

	365—Russia	Various	1836, 1953, 1994, 1996	4
	484—Congo	235—Portugal	1969	1
	501—Kenya	651—Egypt	1978	1
	510—Tanzania	501—Kenya	1977	1
	541—Mozambique	560—South Africa	1987	1
	615—Algeria	666—Israel	1968	1
	625—Sudan	651—Egypt	1994	1
	630—Iran	Various	1981, 1986	2
	640—Turkey	335—Bulgaria	1986	1
	652—Syria	Various	1954, 1963	2
	666—Israel	651—Egypt	1959, 1960	2
	710—China	Various	1949, 1954, 1967, 1993	4
	713—Taiwan	Various	1951, 1953, 1954	3
	750—India	790—Nepal	1968	1
	775—Myanmar	800—Thailand	1988	1
	900—Australia	385—Norway	2001	1
Hot pursuit and attacks on rebels	002—United States	070—Mexico	1859, 1873	2
	091—Honduras	092—El Salvador	1985	1
	101—Venezuela	100—Colombia	1997	1
	115—Suriname	110—Guyana	1976	1
	305—Austria	325—Italy	1960	1
	355—Bulgaria	360—Romania	1926	1
	433—Senegal	404—Guinea-Bissau	1992	1
	490—Democratic Republic of the Congo	551—Zambia	1977	1
	552—Zimbabwe	540—Angola	1979	1
	560—South Africa	Various	1978, 1985, 1986	3
	572—Swaziland	541—Mozambique	1994	1
	640—Turkey	645—Iraq	1999	1
	704—Uzbekistan	702—Tajikistan	1999	1
	775—Myanmar	800—Thailand	1959	1
	850—Indonesia	910—Papua New Guinea	1982, 1988, 1990, 1992	4