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Helping hand or heavy hand? Foreign aid, regime type and domestic unrest

Daniel Yuichi Kono
University of California, Davis, USA

Gabriella R. Montinola
University of California, Davis, USA

Nicholas Verbon
University of California, Davis, USA

Abstract
Does foreign aid affect domestic political unrest? 'Selectorate' models of political survival predict that foreign aid should lead autocratic governments, but not democratic ones, to restrict civil liberties. This requires investment in repressive capacity, which should in turn deter unrest. We thus argue that foreign aid should reduce unrest in autocracies but not in democracies. We find strong support for this hypothesis in a sample of 84 countries from 1970 through 2007, as well as evidence for our causal mechanism. Our results add to the mounting evidence that foreign aid has more desirable effects when targeted at democratic regimes.

Keywords
Autocracy, civil unrest, democracy, foreign aid, repression

Introduction

Is foreign aid good for recipient countries? This question has historically divided 'aid optimists' from 'aid pessimists'.[1] Optimists argue that foreign aid has great potential to promote economic development and would be more effective if provided more generously (Sachs, 2005; Sachs et al., 2004). Pessimists, in contrast, argue that foreign aid is useless and possibly even harmful, as it can

Corresponding author:
Gabriella R. Montinola, Department of Political Science, University of California, Davis, One Shields Avenue, Davis, California 95616, USA.
Email: grmontinola@ucdavis.edu

lead to rent-seeking and corruption (Hodler, 2007; Moyo, 2009; Svensson, 2000), moral hazard and bad policy incentives (Moyo, 2009; Svensson, 2000), and reduced demand for democratic accountability (Djankov et al., 2008; Morrison, 2009). In recent years, however, many scholars have moved towards a middle ground, arguing that aid can be beneficial under certain conditions (Bearce and Tirone, 2010; Burnside and Dollar, 2000; Isham et al., 1997; Kosack, 2003; Svensson, 1999). In their view, the important task is to identify the conditions under which aid is helpful and harmful so that donors can target aid effectively.

One potentially important, but largely unexplored, consequence of aid is domestic political unrest. Although scholars have examined aid's impact on the likelihood of civil war (Collier and Hoeffler, 2002; De Ree and Nillesen, 2009; Grossman 1992), they have not asked how aid affects lower-intensity – but far more common – forms of unrest, such as strikes, demonstrations and riots. There is, however, good reason to expect aid to affect unrest. It could do so by promoting economic growth, by helping governments to buy off potential protesters, or by strengthening the government's capacity to repress dissent. Moreover, for good or ill, unrest may have important political and economic effects. It could allow citizens to influence their governments, but it could also wreak economic harm (Alesina et al., 1996; Barro, 1991). Thus, it is important to understand how aid affects unrest, as the results may shed light on the desirability of foreign aid.

We argue that the effects of aid on unrest depend on domestic regime type. We build on Bueno de Mesquita and Smith (2009) and Smith (2008), who argue that foreign aid both increases citizens' incentives to overthrow autocratic regimes and leads the latter to restrict civil liberties in an effort to offset such threats. Foreign aid could thus either increase or decrease unrest in autocracies, depending on the relative magnitude of these effects. In democracies, in contrast, citizens have no incentive to seek regime change. Moreover, although foreign aid leads to increased citizen demands for public goods, democratic leaders provide these as a matter of course. Foreign aid should thus have no effect on unrest in democracies. We thus predict that foreign aid will not affect unrest in democracies, while it may have either a positive or a negative effect in autocracies.

We analyse the relationship between aid and unrest in a sample of 84 countries from 1970 through 2007. We find that foreign aid reduces unrest in autocracies but has no effect in democracies. To demonstrate that the aid–unrest relationship in autocracies reflects hypothesised causal mechanisms, we also show that: (1) aid strengthens repressive capacity in autocracies but not in democracies; and (2) greater repressive capacity reduces unrest in autocracies but not in democracies. Together, these results strongly suggest that aid reduces unrest in autocracies by helping autocratic recipients to more effectively deter dissent.

Our results contribute to the debate on the effects of foreign aid, and have implications for the effectiveness of foreign aid. Isham et al. (1997) show that World Bank-funded projects perform better when citizens are able to engage in protest because this provides a feedback mechanism. Given this, our results suggest that foreign aid can – by facilitating repression – actually undermine the conditions under which it might be economically effective. In this sense, we provide more ammunition for the aid pessimist camp.

On the other hand, our results point to ways in which foreign aid might be used more effectively. A number of studies already show that aid promotes economic growth and development in democracies but not autocracies (Dollar and Burnside, 2004; Dollar and Levin, 2005; Isham et al., 1997; Kosack, 2003; Svensson, 1999). There are thus good economic reasons to target aid more selectively towards democracies. Our results reinforce this policy prescription by showing that there are also political reasons to withhold aid from autocracies. We thus add to the mounting evidence that foreign aid might have more desirable economic and political effects if restricted to democratic regimes.

Foreign aid and domestic unrest

Most work on foreign aid and domestic conflict has focused on civil wars (Collier and Hoeffler, 2002; De Ree and Nillesen, 2009; Grossman, 1992). This work suggests that foreign aid affects the likelihood of civil war in various, potentially offsetting, ways. For example, Grossman (1992) argues that by increasing government revenues, aid raises the incentives to take over the state by force. Foreign aid thus encourages civil war by raising the value of the spoils. In contrast, Collier and Hoeffler (2002) argue that aid could reduce the risk of civil war by strengthening government forces, by promoting economic growth and by reducing dependence on primary commodity production. These competing causal pathways have two implications for research on aid and civil war: first, the impact of aid on civil war is necessarily an empirical question; and, second, it is difficult to interpret an aid–civil war relationship without additional research on the intervening causal mechanisms.

Empirically, foreign aid reduces the likelihood of civil war (Collier and Hoeffler, 2002; De Ree and Nillesen, 2009). This does not mean that Grossman's (1992) causal path is inoperative, but it does mean, at least, that it is overwhelmed by competing effects of aid. As to what these competing effects are, Collier and Hoeffler (2002) find that aid reduces the risk of civil war not by strengthening government forces, but by promoting economic growth and diversification.

Whether foreign aid has similar effects on civil unrest is not clear, as research on aid and this lower-level conflict is scant. There are reasons to believe that it should. In both revolutionary and lower-intensity cases, dissenters seek access to government resources but may be deterred by government repression (Lichbach, 1995; Moore, 1995). On the other hand, there are also important differences between the two forms of civil strife. Revolutionary actors seek to control the state, whereas non-revolutionary protesters often simply seek change in government policy. Given these differences, the relationship between aid and lower-level unrest remains unknown.

Why might aid affect domestic unrest? To answer this question, we build on models developed by Bueno de Mesquita and Smith (2009) and Smith (2008). These models examine the policy consequences of 'free resources' such as natural resources and foreign aid. Because these models provide the foundation for our argument, we briefly review their main features before discussing our extensions.

In these models, policies are made by an incumbent leader who needs the support of a winning coalition (W) to stay in power. W can be small or large and is a function of regime type – autocracies have small winning coalitions, while democracies have large ones – so we henceforth refer to small-W and large-W regimes as autocracies and democracies, respectively. Both autocratic and democratic leaders maintain power by offering coalition members a mix of private and public goods. However, the mix varies across regime types: autocrats rely more heavily on private goods, while democrats rely more heavily on public goods.

The incumbent can be deposed by a challenger from within the winning coalition or by a 'revolutionary activist' who seeks to expand W to include half the population: that is, the activist seeks full democratisation. The leader can thus be deposed by members of W or by the currently disenfranchised masses. It is the latter form of deposition that most interests us, as it has clearer implications for public unrest.

The activist's likelihood of revolutionary success depends on the supply of 'core public goods' such as freedom of speech and assembly. As Bueno de Mesquita and Smith (2009: 6) note:

> some public goods, especially freedom of assembly, free speech, free press, and transparent government, improve the ability of citizens … to organize and coordinate in the event that they are displeased with their government. We assume that, should the citizens decide to rebel, the probability that they will succeed is an increasing function of these core public goods.

In other words, core public goods help citizens coordinate their activities against an unpopular regime. Suppose, for example, that an autocrat allows freedom of assembly and speech. Freedom of assembly would allow potential revolutionaries to meet and plot against the ruler, while a free press would inform other citizens of revolutionary activity, perhaps emboldening them to join the cause or to take revolutionary actions of their own. Because such freedoms facilitate revolutionary efforts, autocrats can minimise revolutionary threats by constraining the supply of core public goods.

Building on these foundations, Bueno de Mesquita and Smith (2009) and Smith (2008) explore the impact of 'free resources' on government policy. Free resources are non-tax sources of government revenue, whose key characteristic is that 'they do not require the citizens to work to provide them' (Smith, 2008: 780). Free resources are fungible in the sense that leaders can use them as they see fit. Bueno de Mesquita and Smith (2009) identify mineral deposits and foreign aid as prominent examples. One might reasonably ask whether foreign aid meets the fungibility criterion, given that it is often earmarked for specific purposes. However, numerous studies show that aid is indeed fungible: foreign aid given for one purpose allows recipient governments to reduce domestic resources allocated for that purpose, and to redirect those resources towards other uses (Collier and Hoeffler, 2007; Feyzioglu et al., 1998; Khilji and Zampelli, 1994; Lu et al., 2010). Lu et al. (2010: 1382) find, for example, that aid recipient governments reduced their health expenditures by at least US$0.43 for every US$1 of aid targeted towards the health sector that they received. Feyzioglu et al. (1998) show that governments that received concessionary loans for agriculture, education and energy reduced their own resources to these sectors and used them elsewhere. It thus seems reasonable to assume, as Bueno de Mesquita and Smith (2009) do, that foreign aid qualifies as a free resource.

The models predict that democrats and autocrats spend foreign aid differently: democrats spend aid on their large coalition via public goods, while autocrats spend aid on their small coalition via private goods. In addition, autocrats contract core public goods because the increase in government resources boosts the public's incentive to seek full democratisation. By restricting core public goods, autocrats counter this increased revolutionary threat.

Although Bueno de Mesquita and Smith (2009) and Smith (2008) focus on the provision of public and private goods, their models also have implications for domestic unrest. Specifically, they suggest that foreign aid may have different effects on unrest in democracies and autocracies. Under all regime types, foreign aid should increase mass demand for public goods. In democracies, where leaders spend aid on public goods, this demand is granted as a matter of course. Foreign aid should thus neither encourage nor discourage unrest in democracies, as it essentially creates demands that are immediately met. Moreover, because fully democratic regimes cannot be further democratised, an increase in aid also gives citizens no reason to seek revolutionary change. For these reasons, foreign aid should not affect unrest in democracies.

The situation in autocracies is more complex. On the one hand, aid leads to mass demand for public goods that autocratic leaders do not meet. Moreover, the government's increased resources imply larger gains from full democratisation, giving activists stronger incentives to seek revolutionary change. For these reasons, aid could increase unrest in autocracies. On the other hand, autocrats understand these incentives and contract core public goods to discourage revolutionary activity. In practice, this implies that protesters are more likely to be arrested and punished. This increased threat of punishment should deter would-be protesters from protesting in the first place, in which case foreign aid would reduce unrest in autocracies.

Foreign aid could thus increase unrest in autocracies by increasing the potential benefits from democratisation, or decrease unrest by increasing the potential costs of protesting. The net impact of aid on unrest in autocracies is thus an empirical question, to which we now turn.

Analysis

Does foreign aid affect unrest?

We examine the impact of aid on unrest using all country-years for which data are available: 84 countries from 1970 through 2007. Our dependent variable, $Unrest_{i,t}$, is the degree of civil unrest in country i in year t. It is based on three measures from Banks's (2011) Cross National Time Series (CNTS) data. The first measures the number of peaceful, public demonstrations involving 100 or more people with the express goal of voicing opposition to the domestic government. The second measures the number of general strikes directed at government policies involving 1000 or more industrial or service workers. The third captures any violent demonstration or clash of 100 or more citizens. Data for these events are based on reports in the *New York Times* (Banks, 2011). Our measure of unrest is the sum of these indicators: for example, a value of 5 indicates that a country experienced five demonstrations, strikes or riots in a given year.

Our key independent variable, $Aid/GNI_{i,t-1}$, is country i's net inflows of official development assistance as a percentage of gross national income (GNI) in year $t - 1$.[2] We deflate aid by GNI because the impact of an additional aid dollar on the capacity to deter unrest should be smaller both in larger countries (there are more people to repress) and in richer countries (aid is small relative to domestic resources). We lag *aid/GNI* to ensure that aid observations are realised before our dependent variable is observed. For similar reasons, we also lag all other independent variables. We log *aid/GNI* to normalise the distribution of aid, which is right-skewed, and because aid may have diminishing marginal effects on unrest.

Because we hypothesise that the impact of aid depends on regime type, we include measures of regime type ($Democracy_{i,t-1}$) and interactions between regime type and aid ($Aid/GNI_{i,t-1}$*$Democracy_{i,t-1}$). Our first regime-type measure, *W*, is Bueno de Mesquita and Smith's (2010) measure of winning coalition size. This measure ranges from 0 to 1 in intervals of .25, with higher values indicating larger coalitions. Our second measure, *Polity*, is the 21-point Polity index, which ranges from −10 for full autocracies to +10 for full democracies. For ease of interpretation, we recode the Polity index to range from 0 to 1.[3] We interact aid with each regime type measure. The coefficient on aid thus gives the impact of aid in autocracies, while the coefficient on the interaction term shows how this impact changes as aid recipients become more democratic.

We hypothesise that aid should not affect unrest in democracies, while it could have either a positive or a negative effect in autocracies. The expected signs on both aid and the interaction term are thus theoretically indeterminate. If aid increases unrest in autocracies but has no effect in democracies, then aid will be positively signed while the interaction term will be negatively signed. If aid reduces unrest in autocracies but has no effect in democracies, then aid will be negatively signed while the interaction term will be positively signed.

We include a number of controls that prior work has shown to influence political unrest. We include the log of $GDP\ per\ capita_{i,t-1}$ because richer countries experience less political violence (Goldstone et al., 2010). We also include $Economic\ Growth_{i,t-1}$, the annual change in the log of per capita gross domestic product (GDP), because a growing economy gives citizens fewer economic grievances and more to lose from unrest-induced work stoppages. We include $Inflation_{i,t-1}$, the annual percentage change in the consumer price index, and its square, $Inflation^2_{i,t-1}$, because research shows that inflation has a curvilinear relationship with unrest: both extremely low and extremely high inflation increase unrest (Gurr, 1968; Hibbs, 1973; Muller and Seligson, 1987). We include $Urbanisation_{i,t-1}$, the percentage of individuals living in urban areas, because higher levels of urbanisation facilitate collective action and promote unrest (Auvinen, 1997; Hibbs, 1973). We

Table 1. Foreign aid, regime type and domestic unrest.

	Model 1 (W)			Model 2 (Polity)		
	Coeff.	Std. Err.	IRR	Coeff.	Std. Err.	IRR
$Aid/GNI_{i,t-1}$	–.643**	.260	.525	–.397*	.213	.671
$Aid/GNI_{i,t-1}$*$Democracy_{i,t-1}$	.819**	.344	2.26	.444*	.253	1.56
$Democracy_{i,t-1}$	–1.28**	.654	.275	–.496	.482	.608
Per capita $GDP_{i,t-1}$	.013	.137	1.01	–.038	.128	.961
Economic $Growth_{i,t-1}$	–.171**	.047	.842	–.204**	.044	.815
$Inflation_{i,t-1}$	–0004**	.0002	.999	–.0003**	.0001	.999
$Inflation^2_{i,t-1}$	.0000	.0000	1.00	.0000**	.0000	1.00
$Urbanisation_{i,t-1}$	.016**	.005	1.01	.014**	.005	1.01
Trade $Openness_{i,t-1}$	–.511**	.114	.599	–.478**	.109	.619
Debt Service/$GNI_{i,t-1}$	.065	.054	1.06	.052	.049	1.05
$Unrest_{i,t-1}$	.070**	.006	1.07	.069	.006	1.07
Constant	.960	1.11		1.00	1.04	
Wald χ^2 ($P > \chi^2$)	292.64			311.73		
Observations	2289			2418		

Notes: Dependent variable: $Unrest_{i,t}$. Values are coefficients, standard errors and IRRs.* $p < 0.10$; ** $p < 0.05$.

include *Debt Service/*$GNI_{i,t-1}$, the log of public and publicly guaranteed external debt service as a percentage of GNI, because countries with large external debts experience more strikes, mass demonstrations and riots (Auvinen, 1997; Walton and Ragin, 1990).[4] Finally, we include *Trade* $Openness_{i,t-1}$, the log of trade as a percentage of GNI, because studies have found that external trade affects levels of domestic unrest (Arce and Bellinger, 2007; Kurtz, 2004).

In addition to these controls, we include a lagged dependent variable because unrest in one year often spills over to the next, and it is important to address this serial correlation. We also include year fixed effects to control for unobserved variables that might affect the reporting of unrest incidents over time. Year dummy variables also control for the possibility that foreign aid, democracy and protest are all upward-trending variables.

We employ negative binomial regressions, a procedure best suited for analysing event count data (Hausman et al., 1984). To account for heterogeneity across countries, we apply the conditional fixed-effects negative binomial regression, where the joint probability of the counts for each country is conditioned on the sum of the counts for the country.

Table 1 presents our results. Model 1 uses W as the regime type measure, while Model 2 uses the Polity score. In addition to coefficients and standard errors, we present incidence rate ratios (IRRs), which show the percentage change in incidence of unrest for each unit increase in the independent variables.

The results show that the impact of aid on unrest is conditional on regime type. *Aid/GNI* is negatively signed and significant in both models, indicating that aid significantly reduces unrest in autocracies. The interaction terms are positive and significant, indicating that this effect diminishes as recipients become more democratic. To obtain a more complete picture of these effects, we calculate the marginal effects of aid at different degrees of democracy (Brambor et al., 2006). These effects are presented in Figure 1.

Figure 1 plots the aid coefficients, on the y-axis, against the degree of democracy, on the x-axis. The solid lines represent conditional aid coefficients, while the dashed lines depict 90% confidence intervals. The aid coefficients present the effect of a one-standard-deviation increase in the log of *aid/*

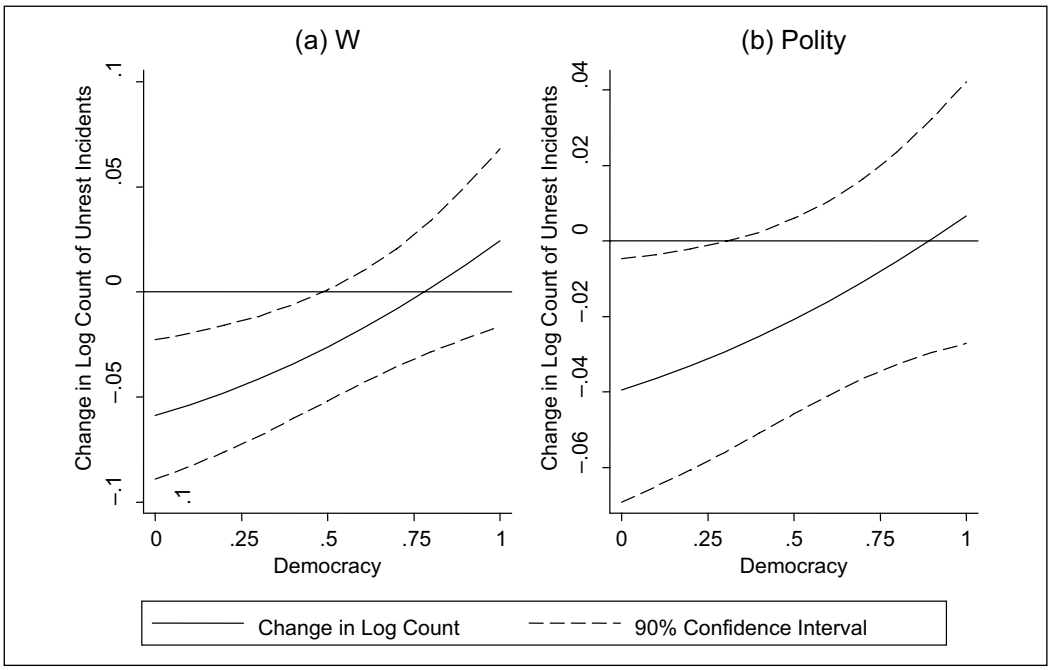

Figure 1. Conditional effects of development assistance on domestic unrest.

GNI from its mean. The mean of aid for both analyses is 2.10. The standard deviation is .629 for the analysis using W and .636 for the analysis using Polity. All other variables are held at their means.

Figure 1(a) presents results based on W, while Figure 1(b) presents estimates based on the Polity score. Both models yield similar results. When democracy equals zero, aid significantly decreases unrest. The effects of aid continue to be negative and significant until W equals .5 and the rescaled Polity score equals .3. This represents 28% of the sample for W and 34% for Polity. Beyond these thresholds, the impact of aid on unrest becomes insignificant, indicating that aid has no effect. Our results thus indicate that aid reduces unrest in autocracies but not in democracies.

For a substantive interpretation of these effects, we turn to the IRRs presented in Table 1. The IRRs show that a one-unit increase in *aid/GNI* decreases the incidence of unrest in full autocracies by 47% and 33% in the W and Polity models, respectively. In partial autocracies (W = .25, rescaled Polity = .25), the reductions in unrest are 35% and 25%, respectively. In full democracies, a one-unit increase in *aid/GNI* actually increases unrest by 19% and 5% for W and Polity, respectively. However, as noted earlier, this effect is not significantly different from zero.

These substantive effects are comparable to those of other variables that have been posited to reduce unrest. For example, the unrest-reducing effect of aid in autocracies is similar to that of trade openness: a one-unit increase in logged trade openness reduces unrest by 38–40%. The effect of aid in autocracies is also more important than that of economic growth: a one-unit increase in growth reduces unrest by 16–19%.[5] Our results thus indicate that foreign aid is one of the more important determinants of domestic unrest.

Some readers may be concerned that aid is endogenous to recipient characteristics: for example, aid donors may be more (less) likely to give aid to leaders facing domestic unrest, which would produce a positive (negative) relationship between aid and unrest even if the former does not affect the latter. To address this concern, we repeated our analysis using two-stage least-squares

regression.[6] The results are qualitatively the same as our earlier ones, implying that our results are not driven by endogeneity. They are available from the authors upon request.

Why does foreign aid affect unrest?

The previous analysis shows that foreign aid reduces unrest in autocracies but not in democracies. However, it does not tell us why. Our theory provides one explanation: aid causes autocrats to crack down on civil liberties, deterring potential protesters by increasing the threat of punishment. We now investigate whether this causal explanation is true.

In answering this question, it is important to recognise that we do not expect autocrats to increase actual repression. If autocrats can credibly threaten to crack down on dissent, then this threat of punishment should deter unrest without the deterrent ever being used. Moreover, if this deterrent is successful, then we might expect actual repression to fall. As Davenport (2007) notes, unrest tends to provoke government retaliation. Given this 'Law of Coercive Responsiveness', autocrats who successfully deter unrest should engage in fewer repressive acts. We thus cannot investigate our causal argument by examining repression *per se*.[7]

Instead, we examine the impact of aid on repressive capacity. An autocrat who wishes to deter dissent should hire additional security forces, station them in key locations, increase the number of patrols on duty, and so on. In other words, the autocrat should invest in repressive capacity. Such investments improve the autocrat's ability to detect, punish and, hence, deter dissent. If our explanation of the aid–unrest results is correct, we should find that foreign aid leads to increased repressive capacity in autocracies but not in democracies.

This raises the question of how to measure repressive capacity. We employ *Military Expenditure/* $GNI_{i,t}$, the log of country *i*'s military spending as a percentage of GNI, as a proxy for repressive capacity.[8] This seems reasonable given that military resources are often used to repress domestic dissent: for example, in the 'Arab Spring' of 2011, governments across the Middle East and North Africa used their armed forces to intimidate pro-democracy protesters. Nonetheless, this measure has two potential shortcomings. On the one hand, military spending is also used to protect the homeland from foreign threats. Our measure may thus include some spending that has nothing to do with domestic repression. On the other hand, domestic repression is sometimes carried out by non-military forces such as domestic police. Our measure may thus omit some spending that is used for domestic repression.

These concerns are less troubling than they may seem. First, as we discuss later, our empirical analysis controls for national-security determinants of military spending, such as interstate conflicts, neighbours' military spending and so on. This should help 'purge' the dependent variable of spending targeted to national-security ends, so that the unexplained remainder is more likely to serve repressive purposes. Second, our military spending measure probably includes a significant amount of spending on internal police. Although the Correlates of War (COW) project has tried to exclude internal policing from its military spending measure, its documentation states that 'it was often difficult to identify and exclude civil expenditures from reported [military] budgets of less developed nations' (COW, 2010: 19). Because our sample includes only developing countries, our measure probably includes spending on internal policing as well as on national defence forces.

Finally, it is worth noting that these measurement errors should make it harder to obtain supportive results. If our measure is dominated by national-security spending or omits most spending on domestic repressive forces, then there is no theoretical reason to expect aid to increase spending in autocracies but not in democracies. In fact, Bueno de Mesquita and Smith (2009) and Smith (2008) predict that foreign aid should increase the provision of public goods – such as national security – in democracies but not in autocracies. Hence, if we are really measuring

national-security rather than repressive capacity, we might expect aid to increase military spending in democracies but not in autocracies. Conversely, if we find the opposite relationship, we can have more confidence that our measure in fact captures repressive capacity.

Our key independent variable is again *Aid/GNI*. Because we expect aid's impact on military spending to depend on recipient regime type, we interact aid with the two measures of *Democracy* that we used in the earlier analysis. If we have correctly explained the aid–unrest relationship, then *aid/GNI* should be positively signed, indicating that aid increases military spending in autocracies. The coefficient on the interaction term should be negative, indicating that this effect is smaller in more democratic recipients.

As noted earlier, we need to control for external security threats to ensure that our proxy for repressive capacity is *not* motivated by legitimate security concerns. We thus include several controls relating to national security. First, because domestic military spending is likely to respond to military spending in neighbouring countries (Collier and Hoeffler, 2007), we include *Neighbour Military Expenditure/GNI*, the log of neighbour military expenditures as a percentage of neighbour GNI. States are considered neighbours if they are separated by a land border or no more than 24 miles of water. We focus on neighbour military expenditures because most developing countries are not great powers and are thus threatened primarily by their neighbours (Collier and Hoeffler, 2007).[9] If neighbour military spending provokes a domestic response, this variable will be positively signed.

Security threats are most pronounced during periods of active warfare. We thus also include *Interstate Conflict* and *Domestic Conflict* to control for participation in both types of conflict. Both variables are ordinal and are coded 0 for no armed conflict, 1 for minor armed conflict, 2 for intermediate conflict and 3 for outright war.[10] Because active conflicts require higher military spending, both variables should be positively signed. Finally, we include the log of *GDP Per Capita* because scholars have argued that the level of economic development may influence military spending, albeit in potentially contradictory ways (Collier and Hoeffler, 2007; Sandler and Hartley, 1995). We also employ country fixed effects to control for unobservable country-specific influences.

We estimate an error-correction model of the following form:

$$\begin{aligned}\Delta Military\ Expenditures/GNI_{i,t} = \beta_0 + \beta_1 Military\ Expenditures/GNI_{i,t-1} \\ + \gamma \Delta \mathbf{X}_{i,t} + \lambda \mathbf{X}_{i,t-1} + \varepsilon_{i,t},\end{aligned}$$

where $Military\ Expenditures/GNI_{i,t-1}$ is the one-year lag of military spending, $\Delta \mathbf{X}_{i,t}$ is a vector of annual changes in all right-hand-side variables, $\mathbf{X}_{i,t-1}$ is a vector of one-year lags in all right-hand-side variables, and $\boldsymbol{\gamma}$ and $\boldsymbol{\lambda}$ are vectors of coefficients for the first-differenced and lagged variables, respectively.

We use an error-correction model because it imposes fewer assumptions than other time-series estimators regarding the timing of the independent variables' effects (De Boef and Keele, 2008). A government that receives sector-specific aid might transfer resources from the targeted sector into other sectors, but it might not do so immediately. Error-correction models are useful because they allow us to estimate both the immediate and the lagged effects of right-hand-side variables. The immediate effects are given by $\boldsymbol{\gamma}$, the coefficients on the first-differenced variables. The lagged effects are given by $\boldsymbol{\lambda}$, the coefficients on the lagged variables. The total effects – that is, the sum of all immediate and lagged effects – are given by the long-run multipliers (LRMs), which are calculated by dividing each independent variable's coefficient by the coefficient on the lagged dependent variable.[11] Our results are reported in Table 2.

Table 2. Foreign aid, regime type and military spending.

Explanatory variable	Democracy measure			
	W		Polity	
	(1)	(2)	(3)	(4)
	Coefficient	LRM	Coefficient	LRM
First differences				
$\Delta Aid/GNI_{i,t-1}$	.122 (.042)**		.085 (.037)**	
$\Delta Aid/GNI_{i,t}$*$Democracy_{i,t-1}$	–.159 (.073)**		–.070 (.062)	
$\Delta Democracy_{i,t-1}$	.222 (.143)		.031 (.121)	
$\Delta Domestic\ Conflict_{i,t-1}$	.037 (.015)**		.037 (.015)**	
$\Delta Interstate\ Conflict_{i,t-1}$	.036 (.014)**		.023 (.014)	
$\Delta Neighbours'\ Mil.\ Exp./GNI_{i,t-1}$	.097 (.024)**		.102 (.023)**	
$\Delta GDP\ per\ capita_{i,t-1}$	–.278 (.077)**		–.280 (.078)**	
One-year lags				
$Aid/GNI_{i,t-1}$	.051 (.025)**	.191 (.089)**	.050 (.019)**	.183 (.067)**
$Aid/GNI_{i,t}$*$Democracy_{i,t-1}$	–.061 (.038)	–.226 (.146)	–.044 (.028)	–.162 (.108)
$Democracy_{i,t-1}$	.030 (.086)	.114 (.320)	–.012 (.062)	–.045 (.229)
$Domestic\ Conflict_{i,t-1}$	.035 (.010)**	.131 (.037)**	.033 (.011)**	.123 (.040)**
$Interstate\ Conflict_{i,t-1}$	.046 (.015)**	.171 (.059)**	.034 (.015)**	.125 (.057)**
$Neighbours'\ Mil.\ Exp./GNI_{i,t-1}$	.025 (.009)**	.093 (.032)**	.027 (.008)**	.100 (.030)**
$GDP\ per\ capita_{i,t-1}$	.010 (.027)	.039 (.102)	.015 (.027)	.055 (.100)
$Mil.\ Exp./GNI_{i,t-1}$	–.269 (.022)**	–2.70 (.304)**	–.273 (.022)**	–2.65 (.296)**
Observations	3200		3308	
F (P > F)	16.31 (.000)		15.63 (.000)	

Notes: Dependent variable: $\Delta Military\ Expenditure/GNI_{i,t}$. Robust clustered standard errors in parentheses.
*$p < 0.10$; **$p < 0.05$.

The results show that the impact of aid on military spending depends on recipient regime type. As expected, the aid LRMs are positive and significant, indicating that aid significantly increases military spending in the most autocratic countries. The interaction term LRMs are negatively signed, indicating that the impact of aid on military spending declines as recipients become more democratic. To explore these effects more fully, we again calculate the marginal effects of aid at different degrees of democracy. These effects – specifically, conditional long-run multipliers – are presented in Figure 2.

Figure 2 plots the aid LRMs, on the y-axis, against the degree of democracy, on the x-axis. Figures 2(a) and 2(b) present results for W and Polity, respectively. Both models yield similar results. When democracy equals zero, aid significantly increases military spending. Specifically, a one-percent increase in *aid/GNI* increases *military spending/GNI* by .19% when W = 0 and by .18% when Polity = 0.[12] The effects of aid continue to be positive and significant until democracy exceeds .25 and .30 for W and Polity, respectively: this represents 32% of the W sample and 48% of the Polity sample. Beyond this threshold, the impact of aid on military spending quickly declines, becoming statistically insignificant from zero. The results thus support our explanation for the aid–unrest relationship: development aid increases repressive capacity in autocracies but not in democracies.[13]

Although the preceding results are encouraging, they do not in themselves fully explain the aid–unrest relationship. To do this, we also have to show that increased repressive capacity reduces

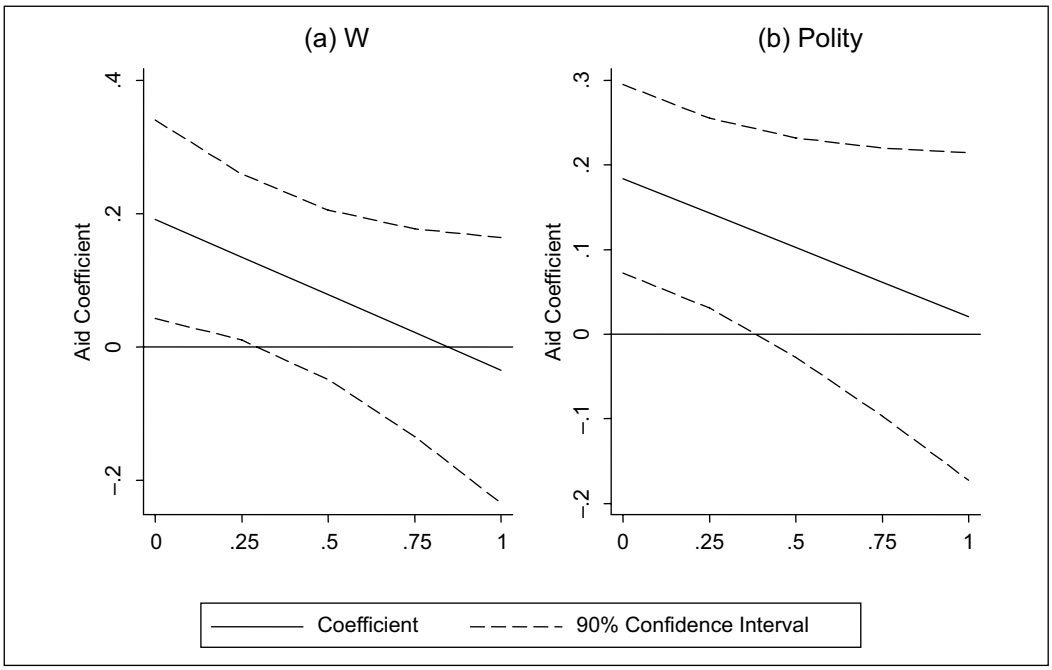

Figure 2. Conditional effects of development assistance on military spending.

unrest in autocracies. Fortunately, this is straightforward to do: we simply repeat our earlier analysis of unrest, including military spending and its interaction with regime type on the right-hand side. If military spending indeed reduces unrest in autocracies, then military spending will be negatively signed. If democracies are less inclined to use their militaries to repress unrest, then the interaction term will be positively signed. We report the results of this analysis, using both measures of democracy, in Table 3. As in Table 1, we present coefficient estimates, standard errors and IRRs.

Military spending is negatively signed and significant in both models, indicating that an increase in spending reduces unrest in autocracies. In both models, the interaction with democracy is positive and significant, indicating that the unrest-reducing effects of military spending decline as countries become more democratic. Closer examination of conditional effects, shown in Figure 3, reveals that military spending significantly reduces unrest until W and Polity exceed .5 and .4, respectively. Beyond these thresholds, the effects of military spending become insignificant. The results thus indicate that military spending reduces unrest in autocracies but not in democracies.

In sum, we find that foreign aid reduces unrest in autocracies but not in democracies. We also find that aid increases military spending in autocracies but not in democracies. Finally, we find that military spending reduces unrest in autocracies but not in democracies. Together, these results provide strong support for a causal argument in which aid strengthens repressive capacity in autocracies and thereby deters dissent.

Before concluding, it is worth asking whether our results could reflect some alternative causal channel. The most obvious alternative is economic growth: if aid promotes growth, this could reduce economic grievances and, hence, unrest. Note, however, that we control for economic growth rates in our regressions. This means that growth cannot be mediating the impact of aid on unrest. Because we control for this intervening variable, we can dismiss this alternative argument.

Table 3. Military spending, regime type and domestic unrest.

	Model 1 (W)			Model 2 (Polity)		
	Coeff.	Std. Err.	IRR	Coeff.	Std. Err.	IRR
Military Spending/GNI$_{i,t-1}$	−.552**	.228	.575	−.449**	.184	.637
Military Spending/GNI$_{i,t-1}$**Democracy*$_{i,t-1}$	.722**	.343	2.05	.572**	.260	1.77
Democracy$_{i,t-1}$	−.706	.450	.493	−.390	.344	.676
Per capita GDP$_{i,t-1}$	.040	.131	1.04	.006	.123	1.00
Economic Growth$_{i,t-1}$	−.163**	.048	.848	−.185**	.047	.830
Inflation$_{i,t-1}$	−.0004*	.0002	.999	−.0004*	.0002	.999
Inflation$^2_{i,t-1}$	.0000	.0000	1.00	.0000	.0000	1.00
Urbanisation$_{i,t-1}$	.016**	.005	1.01	.014**	.005	1.01
Trade Openness$_{i,t-1}$	−.543**	.113	.580	−.521**	.109	.593
Debt Service/GNI$_{i,t-1}$	.071	.056	1.07	.061	.052	1.06
Unrest$_{i,t-1}$	.071**	.006	1.07	.069**	.006	1.07
Constant	.436	.979		.633	.927	
Wald χ^2 (P > χ^2)	288.61 (.000)			304.81 (.000)		
Observations	2222			2334		

Notes: Dependent variable: *Unrest*$_{i,t}$. Values are coefficients, standard errors and IRRs.
*p < 0.10; **p < 0.05.

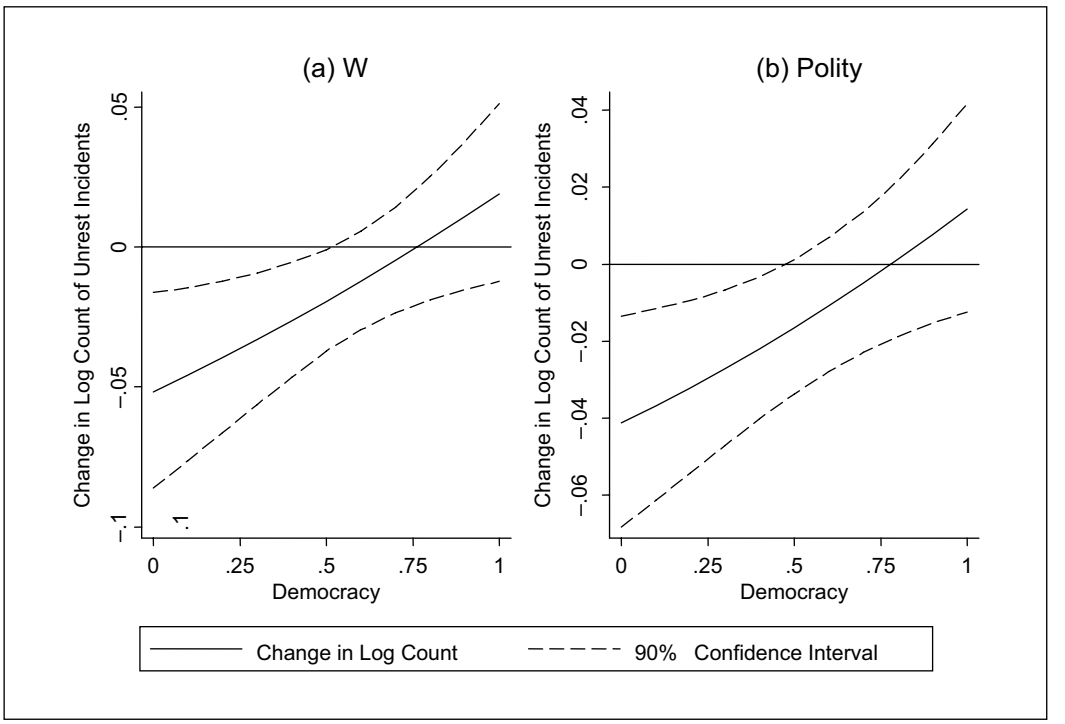

Figure 3. Conditional effects of military spending on domestic unrest.

It is also worth noting that research on aid and growth points away from a growth explanation. Most studies conclude that aid does not affect growth rates at all,[14] and studies that find conditional effects generally show that aid promotes growth in democracies but not in autocracies (Dollar and Burnside, 2004; Dollar and Levin, 2005; Isham et al., 1997; Kosack, 2003; Svensson, 1999). This implies that aid should reduce unrest in democracies but not in autocracies – at least if it operates through a growth channel – which is the opposite of what we find. Hence, even if we did not control for growth in our regressions, it seems unlikely that a growth channel could explain our results.

Foreign aid could also reduce unrest by helping governments buy off potential dissenters. Non-revolutionary protests often have material ends: strikers seek higher wages, rioters seek lower food prices and so on. By providing extra resources, foreign aid could help governments meet such demands, thus reducing unrest. Of course, for this 'co-optation' channel to explain our results, foreign aid would have to promote co-optation in autocracies but not in democracies. We find this implausible on both theoretical and empirical grounds.

Theoretically, we see no reason why autocrats would be more likely than democrats to use foreign aid to co-opt potential dissenters. If anything, we might expect the opposite if democrats find it harder to resort to repression. Empirically, we find no empirical evidence for a co-optation channel. This is admittedly hard to do, as it is not clear which budgetary categories constitute 'co-optation expenditures'. However, we examined the conditional effects of foreign aid on various types of spending – education and health care, infrastructure, and government wages – and the effects of such spending on unrest.[15] We found no evidence that aid's effects on such spending vary across regime type, nor did we find that such spending reduces unrest in autocracies but not democracies. Although these results are not conclusive, in conjunction with the results presented earlier, they strongly suggest that foreign aid affects unrest mainly by strengthening repressive forces in autocracies.

Conclusion

Policymakers and scholars continue to debate the desirability of foreign aid. Aid optimists believe that, used correctly, foreign aid can promote economic development (Burnside and Dollar, 2000; Sachs, 2005). Aid pessimists, in contrast, believe that aid is ineffective at best and pernicious at worst. Among its alleged pitfalls, foreign aid is claimed to increase rent-seeking and corruption (Hodler, 2007; Moyo, 2009; Svensson, 2000), to create moral hazard and bad policy incentives more generally (Moyo, 2009; Svensson, 2000), to reduce the demand for democratic accountability (Djankov et al., 2008; Morrison, 2009), and to prop up dictators (Bauer, 1972; Friedman, 1958). This debate has important policy implications, as rich-world citizens and the leaders who represent them typically want their aid programmes to work. Indeed, members of Congress have opposed US aid programmes on the grounds that aid tends 'to prop up bad governments that have mistreated their people' (Kiely, 2001). The effects of foreign aid thus matter not only to scholars, but also to advocates and opponents of foreign aid.

Our results support the aid pessimist camp. We find that foreign aid reduces domestic unrest in autocracies but not in democracies. In theory, aid could do this in normatively desirable ways – for example, by promoting economic growth – and undesirable ways, such as deterring dissent. In practice, our results point to the latter. Foreign aid reduces unrest only in autocracies; it increases military spending only in autocracies; and it is only in autocracies that increased military spending reduces unrest. Together, these results imply that aid reduces unrest by increasing the threat of repression in autocracies rather than through other, more benign, channels.

Our results will not surprise those who have always believed that foreign aid is the autocrat's best friend. Nonetheless, given the stakes of the policy debate, it is important to verify that the aid

pessimist charges are true. Our study is the first to demonstrate that foreign aid stifles domestic dissent not by enriching recipient citizens, but by increasing the costs of dissent. It thus strengthens the long-standing claim that foreign aid supports autocratic rule.

This does not necessarily mean that foreign aid is a bad idea, but it does suggest a need for greater selectivity in granting foreign aid. Several studies already show that aid is better for economic growth and development in democracies than in autocracies (Dollar and Burnside, 2004; Dollar and Levin, 2005; Isham et al., 1997; Kosack, 2003; Svensson, 1999). Isham et al. (1997: 219) link this result to unrest, showing that World Bank projects perform better when citizens are able to engage in protest:

> countries with the strongest civil liberties have projects with an economic rate of return 8–22 percentage points higher than countries with the weakest civil liberties…. [The] mechanism of causation is from more civil liberties to increased citizen voice to better projects.

This result, along with our own, suggests that foreign aid actually helps autocrats create conditions under which aid will be ineffective. We thus add to the mounting evidence that donors should target scarce foreign aid funds to democratic regimes.

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Notes

1. For a review of this debate, see Wright and Winters (2010).
2. Data on official development assistance and GNI are from the Organisation for Economic Co-operation and Development's (OECD's) Development Assistance Committee Online Databases.
3. Data on coalition size and Polity scores are from Bueno de Mesquita and Smith (2010) and the Polity IV project, respectively.
4. Data on per capita GDP and economic growth are from the Penn World Tables 7.0. Data on inflation, urbanisation, debt service and trade openness are from the World Bank's *World Development Indicators*.
5. The standard deviations in aid, trade openness and growth are .63, .58 and .82, respectively, so these variables are of comparable scale.
6. As instruments, we used infant mortality, infant mortality squared and the interaction of infant mortality and regime type.
7. For this reason, it seems unlikely that foreign aid-induced repression would 'radicalise' previously quiescent groups, thus increasing unrest. Although such radicalisation might occur in response to repressive acts, it seems less likely to occur in response to an increase in repressive capacity. That said, the net impact of aid on unrest via such a channel is an empirical question. Our results suggest that even if radicalisation occurs, it is outweighed by the deterrent effects of increased repressive capacity.
8. Data on military expenditures are from the Correlates of War National Material Capabilities Dataset. Data for GNI are from the World Bank's *World Development Indicators*.
9. Data on neighbour proximity and military spending are from the Correlates of War Direct Contiguity Dataset and National Material Capabilities Dataset, respectively.
10. Data on both types of conflict are from the Armed Conflict Dataset (Gleditsch et al., 2002).
11. To obtain correct standard errors for the LRMs, we employ the Bewley (1979) transformation suggested by De Boef and Keele (2008).
12. Since both variables are logged, the coefficients can be interpreted as elasticities.
13. To address potential endogeneity concerns, we performed two-stage least-squares regressions for aid and military spending. As instruments, we employed infant mortality, recipient foreign policy alignment with the US and higher-order moments of the endogenous regressors. The results are qualitatively the same as our earlier ones and are available from the authors upon request.

14. For a review of this literature, see Wright and Winters (2010).
15. We performed the analysis on other types of spending analogous to those on military spending. We first regressed the spending measure on aid, regime type, the product of aid and regime type, and controls. We then regressed unrest on the spending measure, regime type, the product of the spending measure and regime type, and the same controls used in Table 3. Neither the aid (spending) coefficients nor interaction terms were statistically significant. These results are available upon request.

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Author biographies

Daniel Yuichi Kono is an Associate Professor of Political Science at the University of California, Davis, USA. His research is on international political economy, with a focus on trade policy and foreign aid.

Gabriella R. Montinola is an Associate Professor of Political Science at the University of California, Davis, USA. Her recent work examines the impact of foreign aid across developing countries.

Nicholas Verbon received a Masters in Political Science from the University of California, Davis, USA, in June 2010.