Online Appendix for "Bypass Aid and Unrest in Autocracies"

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A Formal Model

Though the central hypotheses in the main text are relatively straightforward and intuitive, formalizing the argument is a helpful additional exercise for a few reasons. First, Altincekic and Bearce (2014) draw on Acemoglu and Robinson's (2001) formal model in arguing that non-fungible aid should not benefit autocratic regimes. Since I am using the same theoretical framework to predict the opposite outcome, the model helps show that I am not introducing any new or unstated assumptions to the argument. Second, using the formal model allows me to derive a clear comparative static on one of the variables of interest: the proportion of aid delivered through bypass channels. This is important since including measures of total aid along with a proportion that is calculated using total aid may raise concerns about bias in the statistical tests. Lastly, incorporating aid into this framework provides an important baseline for future extensions. In particular, this model should prove useful in endogenizing donor choices over aid channels with an explicit model of domestic politics in the recipient country.

The Model

Consider a dynamic game played between a group of elites and a population.[2] At the start of the game, the elites control the government. In any round, the elites can choose to allot a portion x of the available resources, R, to the population. I assume that prior to receiving any international aid, the stock of resources is 1. In every round the country receives an amount of aid denoted by $a > 0$. For simplicity I assume that a is exogenous and constant across rounds. Let $\beta \in [0, 1]$ (also exogenous) be the proportion of aid that goes directly to the population. In every round, the population derives utility from any foreign aid it receives directly (βa), plus whatever the government allocates to it (x). (The total amount of bypass aid is therefore equal to βa.) The government derives utility from the portion of aid that it receives ($(1 - \beta)a$) as well any remaining state resources ($1 - x$).

The state of the world in time period t is defined by the pair (G^t, c^t) where $G^t \in \{A, R\}$ and $c^t \in \{c_L, c_H\}$. If $G^t = A$, the country is in a state of autocracy, while if $G^t = R$ the country is in the state of a revolutionary regime. The parameter c indicates the cost to the population of removing the incumbent regime from power in a given state, which corresponds to the extent to which national wealth would decrease in the aftermath of a revolution. Thus, the government can be either in one of two autocratic states – (A, c_L) or (A, c_H) – or one of two states of a revolutionary (i.e., post-autocratic) regime – (R, c_L) or (R, c_H). I assume that $c_L \sim U[0, 1]$ is drawn at the beginning of the game by Nature and applies in all future rounds. As a simplifying assumption, I follow Gehlbach (2013) in assuming that c_H is high enough such that the population never chooses to initiate a revolution in the state (A, c_H) even when the regime transfers no resources to the population.[3] In the state (A, c_L), however,

[2]The game closely follows a simplified version of Acemoglu and Robinson's (2001) model presented in Gehlbach (2013). The solution concept is Markov perfect equilibrium. Similar to others (e.g., Meirowitz and Tucker 2013), I abstract away from problems of collective action in treating the elites and the population as unified actors, acknowledging that these problems are not trivial.

[3]This amounts to assuming that $c_H \geq 1 + (1 - \beta)a$. Alternatively, one could assume that the opposition does not get to move if $c = c_H$ and derive the same result.

the population may want to revolt. In either state of autocracy, the population can decide whether or not to oust the elites from office and take over the government. If the population initiates a revolution, it takes over the government and inherits the government's resources, minus the costs of revolution.

In any given round, the probability that the next state is (A, c_L) (if there is no revolution) is $q \in (0, 1)$ and the probability that the state is (A, c_H) is $1 - q$. Let $\delta \in (0, 1)$ be a common discount factor. Denote the value to the population of state (A, c_H) as $V(A, c_H) = \beta a + \delta[qV(A, c_L) + (1 - q)V(A, c_H)]$. Here x is zero because an autocrat has no incentive to make concessions when revolution is not credible. The above expression can be rewritten in terms of $V(A, c_H)$ as

$$V(A, c_H) = \frac{a\beta + \delta q V(A, c_L)}{\delta(q - 1) + 1}. \tag{3}$$

When costs are low enough so that revolution is politically possible, the elites may want to transfer some resources to the population to prevent a revolution. I assume that the elites receive nothing under a revolutionary government. The value to the population of living in state (A, c_L) is now a function of the resources transferred to them by the elites, the amount of foreign aid they receive, and their expectations about the likelihood of being in the other of the autocratic states in the future, which can be expressed as

$$V(A, c_L) = x + \beta a + \delta[qV(A, c_L) + (1 - q)V(A, c_H)]. \tag{4}$$

Substituting (3) into (4) and simplifying yields $V(A, c_L) = \frac{a\beta + \delta(q-1)x + x}{1-\delta}$. The population will be content under autocracy if the amount of resources that the elites transfer to the population makes them at least as well off as they would be under a revolutionary regime.[4] Since a revolution succeeds with certainty, the value of a revolutionary regime is $V(R, c_L) = \frac{1+(1-\beta)a-c_L}{1-\delta}$. To avoid revolution, the elites' offer must satisfy $V(A, c_L) \geq V(R, c_L)$, or

$$\frac{a\beta + \delta(q - 1)x + x}{1 - \delta} \geq \frac{1 + (1 - \beta)a - c_L}{1 - \delta}. \tag{5}$$

The government's utility is decreasing in x, so it sets x such that Equation 5 holds with equality. Solving for this optimal offer and taking its first derivative with respect to β shows that the government's equilibrium transfer is decreasing in the proportion of bypass aid ($\frac{\partial x^*}{\partial \beta} < 0$). As the proportion of aid delivered through bypass channels increases, smaller offers from the government deter unrest. So long as aid provides some direct benefit to the population, governments can more easily stave off revolution. This is independent of any effect that government aid would have on allowing the government to make bigger offers. The intuition behind the result for the proportion itself is that as more of the total "pie"

[4]Under the assumption that the value of a revolutionary regime also includes the full amount of future bypass and government aid ($V(R, c_L) = \frac{1-c_L+a}{1-\delta}$), the offer that buys off the population is still decreasing in β ($\frac{\partial x^*}{\partial \beta} = -\frac{a}{\delta(q-1)+1} \leq 0$).

of aid goes directly to the population now, there is less to be gained from taking over the government (and, by extension, the aid it receives). In sum, in periods that are unfavorable for revolution, bypass aid acts as a humanitarian boon – without it the population would get nothing. In periods where revolution is possible, bypass aid acts a political subsidy to the government by making it easier to buy off the opposition.[5]

The probability of revolution in the model is the probability that the optimal transfer x^* is less than the resources available to the government, which can be written as

$$\Pr(x^* < 1 + (1 - \beta)a), \tag{6}$$

or

$$\Pr(c_L < \delta(q - 1)(a(\beta - 1) - 1) - a\beta). \tag{7}$$

Since $c_L \sim U[0, 1]$, this is equivalent to $\delta(q - 1)(a(\beta - 1) - 1) - a\beta$, which is decreasing in β.

B Extensions to Model

What If Bypass Aid Makes Revolution Easier?

As noted in the main text, an important stylized fact to account for is that bypass aid may actually help sustain rebel groups. The baseline model assumes that an attempted revolution succeeds with certainty. I now relax this assumption to account for the possibility of a failed revolution, which also allows me to account for the fact foreign aid may affect the probability of a revolution succeeding. Let $\rho(\beta) \in (0, 1)$ denote the probability that a revolution is successful, and let $1 - \rho(\beta)$ denote the probability of failure. Let $\rho(\beta)$ be an increasing, concave function of the proportion of bypass aid.[6] For now I assume that in the case of success the resources of the state are available to be evenly divided across the population forever, minus the costs of revolution. If revolution fails, the population gets nothing forever. I also assume that the government can decide how much aid to allow into the country up to some exogenously determined threshold, $\hat{a}$. Think of this as the maximum amount of aid that the international community would be willing to contribute.

Now the population chooses not to revolt if

[5]It might also be the case that bypass aid could stabilize a regime if the regime intercepts aid or coopts bypass actors to invest in repressive capacity. In this case, the prediction would essentially be the same as models that assume government control of aid, but through a different mechanism. While the results here are similar to those in Smith (2008), the model here shows that government control of aid is not a necessary condition for aid to deter unrest. In Section I of the Supplementary Files I find no evidence for a relationship between bypass aid and changes in military expenditures in the sample.

[6]Note that the model implicitly accounts for the possibility that government aid increases the odds that the government wins in a revolutionary contest, since as the proportion of bypass aid decreases, the probability that the government wins $(1 - \rho(\beta))$ increases.

$$\frac{a\beta + \delta(q-1)x + x}{1-\delta} = \frac{\rho(\beta)\left(a(1-\beta) - c_L + 1\right)}{1-\delta}. \tag{8}$$

The value of x that will buy the population off is now

$$\hat{x} = \frac{\rho(\beta)\left(a(-\beta) + a - c_L + 1\right) - a\beta}{\delta(q-1)+1}. \tag{9}$$

The derivative of this optimal $\hat{x}$ with respect to β is

$$\frac{\partial \hat{x}}{\partial \beta} = -\frac{a\rho(\beta) + \rho'(\beta)\left(a(\beta-1) + c_L - 1\right) + a}{\delta(q-1)+1}, \tag{10}$$

where $\rho'(\beta)$ denotes the first derivative of the probability that the revolution succeeds with respect to β. Now the optimal $\hat{x}$ is decreasing in β only if

$$\rho'(\beta) \leq \rho'(\beta)^* \equiv -\frac{a(\rho(\beta)+1)}{a(\beta-1) + c_L - 1}, \tag{11}$$

which is always positive. This threshold is less than 1 (i.e., it is possible that the relationship between bypass aid and the optimal offer may be negative or positive) if the condition $a(\beta + \rho(\beta)) + c_L - 1 > 0$. This highlights that governments may allow aid to bypass them even when it potentially increases the likelihood of a successful revolution because of the effects that it has on undermining the incentive for revolution.

C Countries Included in Analysis

Table 5 reports the country names (sometimes abbreviated) used in the analysis as they appear in the Geddes, Wright and Frantz (2014) data.

Table 5: Temporal and Spatial Domain

	Country	Year(s) in Data		Country	Year(s) in Data
1	Afghanistan	2010	32	Madagascar	2010
2	Algeria	2005 - 2010	33	Malaysia	2005 - 2010
3	Angola	2005 - 2010	34	Mauritania	2005 - 2010
4	Armenia	2005 - 2010	35	Morocco	2005 - 2010
5	Azerbaijan	2005 - 2010	36	Mozambique	2005 - 2010
6	Bangladesh	2008	37	Myanmar	2005 - 2010
7	Belarus	2005 - 2010	38	Namibia	2005 - 2010
8	Botswana	2005 - 2010	39	Nepal	2005 - 2006
9	Burkina Faso	2005 - 2010	40	Oman	2005 - 2010
10	Cambodia	2005 - 2010	41	Pakistan	2005 - 2008
11	Cameroon	2005 - 2010	42	Rwanda	2005 - 2010
12	Cen African Rep	2005 - 2010	43	Saudi Arabia	2005 - 2010
13	Chad	2005 - 2010	44	Singapore	2005 - 2010
14	China	2005 - 2010	45	Sudan	2005 - 2010
15	Congo-Brz	2005 - 2010	46	Swaziland	2005 - 2010
16	Congo/Zaire	2005 - 2010	47	Syria	2005 - 2010
17	Cuba	2005 - 2010	48	Tajikistan	2005 - 2010
18	Egypt	2005 - 2010	49	Tanzania	2005 - 2010
19	Eritrea	2005 - 2010	50	Thailand	2007
20	Ethiopia	2005 - 2010	51	Togo	2005 - 2010
21	Gabon	2005 - 2010	52	Tunisia	2005 - 2010
22	Gambia	2005 - 2010	53	Turkmenistan	2005 - 2010
23	Guinea	2005 - 2010	54	Uganda	2005 - 2010
24	Iran	2005 - 2010	55	United Arab Emirates	2005 - 2010
25	Ivory Coast	2005 - 2010	56	Uzbekistan	2005 - 2010
26	Jordan	2005 - 2010	57	Venezuela	2006 - 2010
27	Kazakhstan	2005 - 2010	58	Vietnam	2005 - 2010
28	Kuwait	2005 - 2010	59	Yemen	2005 - 2010
29	Kyrgyzstan	2005 - 2010	60	Zambia	2005 - 2010
30	Laos	2005 - 2010	61	Zimbabwe	2005 - 2010
31	Libya	2005 - 2010			

D Measure of Unrest Excluding Riots

Following Kono, Montinola and Verbon (2015), the dependent variable used in the main text is a count of strikes involving more than 1,000 people directed at the government or national policies, riots involving more than 100 people, and anti-government demonstrations involving more than 100 people from Banks (2011). Since the theory emphasizes actions that would challenge a government specifically and the count of riots does not specify that the riots need to have anything to do with government policies, I want to ensure the main result does not depend unduly on the inclusion of riots in the dependent variable. (Additionally, from a normative perspective, it might be desirable for bypass aid to reduce unrest if the unrest events it affects are riots.) Table 6 reports the results of rerunning the models from Table 2 in the main text while excluding riots from the count variable. Though the results are weaker in Models 5, 6, and 7, they are in the same direction as those reported in the main text (In Model 6, the coefficient on bypass aid is negative and statistically significant at the 90% confidence level in a one-tailed test). Still, including riots seems reasonable in that it may capture the willingness of citizens to take action in the face of the failure of government policies (for example, food riots).

Table 6: Bypass Aid and Domestic Unrest (Excluding Riots), 2005-2010

	Unrest Events						
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*	*Model 7*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bypass Aid / Capita	-0.64^{***}		-0.89^{**}	-0.87^{**}		-0.05	
	(0.16)		(0.39)	(0.41)		(0.03)	
Bypass Ratio		-0.03^{**}			-0.02		-0.002
		(0.01)			(0.01)		(0.002)
Governance Index	-1.21^{***}	-1.15^{***}	-1.16	-0.82	-1.61	-0.30	-0.32
	(0.25)	(0.24)	(1.46)	(1.36)	(1.41)	(0.22)	(0.22)
Civil Conflicts	0.40	0.42	0.11	0.04	0.29	0.06	0.06
	(0.33)	(0.27)	(0.37)	(0.42)	(0.35)	(0.07)	(0.07)
Natural Disasters	0.10^{***}	0.13^{***}	0.04	0.05	0.02	0.03^{*}	0.03^{*}
	(0.02)	(0.02)	(0.07)	(0.07)	(0.07)	(0.02)	(0.02)
Government Aid / Capita				-0.21		0.01	-0.0000
				(0.50)		(0.03)	(0.03)
Country-fixed Effects	N	N	Y	Y	Y	Y	Y
Year-fixed Effects	N	N	Y	Y	Y	Y	Y
Observations	326	338	326	326	338	326	326
R^2						0.52	0.52
Adjusted R^2						0.39	0.39
Log Likelihood	-208.92	-219.22	-156.97	-156.78	-163.45		
θ	0.28^{***} (0.07)	0.23^{***} (0.05)	1.33^{***} (0.50)	1.37^{***} (0.52)	1.10^{***} (0.37)		
Akaike Inf. Crit.	427.83	448.44	449.94	451.56	466.90		
Residual Std. Error (df = 257)						0.32	0.32
F Statistic (df = 69; 257)						4.03^{***}	4.02^{***}

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. (White's (1980) HC1 standard errors)

Negative binomial regression (Models 1-5) and OLS (Models 6 and 7). Outcome in Models 6 and 7 is dichotomous indicator of at least one unrest event.

E Logit Models for Dichotomous Unrest Variable

To ensure that the results of the count models are not driven by outlying cases with many unrest events, Table 7 reports the results of the key models from Table 2 in the main text where the dependent variable is a dummy variable that takes on a value of "1" when there is at least one unrest event in the data. In both models, the relationship between bypass aid and unrest is negative and statistically significant.

Table 7: Bypass Aid and Domestic Unrest (Logit Models), 2005-2010

	Unrest Events				
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*
	(1)	(2)	(3)	(4)	(5)
Bypass Aid / Capita	-0.43^{***}		-1.38^{**}	-1.38^{**}	
	(0.15)		(0.64)	(0.64)	
Bypass Ratio		-0.03^{***}			-0.06^{***}
		(0.01)			(0.02)
Governance Index	-1.12^{***}	-1.12^{***}	-4.53^{**}	-4.70^{**}	-5.42^{**}
	(0.29)	(0.30)	(2.07)	(2.09)	(2.14)
Civil Conflicts	-0.01	0.02	0.58	0.65	0.77
	(0.32)	(0.26)	(0.76)	(0.78)	(0.63)
Natural Disasters	0.17^{***}	0.18^{***}	0.34^{**}	0.33^{**}	0.28^{**}
	(0.05)	(0.05)	(0.15)	(0.15)	(0.14)
Government Aid / Capita				0.27	
				(0.49)	
Country-fixed Effects	N	N	Y	Y	Y
Year-fixed Effects	N	N	Y	Y	Y
Observations	326	338	326	326	338
Log Likelihood	-142.01	-147.89	-83.40	-83.25	-85.64
Akaike Inf. Crit.	294.02	305.77	302.81	304.50	311.28

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

Logit regression (Models 1-5). Outcome is dichotomous indicator of at least one unrest event.

F Zero-inflated Negative Binomial Regression

To account for the possibility of excessive zeroes in the dependent variable, Table 8 reports the results of two negative zero-inflated binomial regression models where the inflation variable is a factored version of the country code variable. (I omit the coefficients on the model of the probability of observing a zero count.)

Table 8: Zero-inflated Negative Binomial Regression Models

	Unrest Events	
	Model 1	*Model 2*
	(1)	(2)
Bypass Aid / Capita	-0.01^{***}	
	(0.002)	
Bypass Ratio		-0.03^{**}
		(0.01)
Governance Index	-0.66^{*}	-0.76^{*}
	(0.36)	(0.42)
Civil Conflicts	0.39	0.52^{**}
	(0.24)	(0.26)
Natural Disasters	0.03	0.04^{**}
	(0.02)	(0.02)
Year-fixed Effects	Y	Y
Observations	326	326
Log Likelihood	−213.69	−215.43

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. Estimated standard errors in parentheses.
Zero-inflated negative binomial regression.

G Aid Scandal Cases

Table 9 reports the basic case information for the aid scandals used in the construction of the instrumental variable. A complete version of the data with full citation information will be available on the author's website. I conducted the data collection in three rounds as follows: On the first round, I used *Lexis Nexis Academic* to search for "corruption AND foreign aid" in the category of newspapers from 1990 to 2014, sorted the results by relevance, and then manually sifted through the results to identify and record possible scandals. In the second round, I repeated this process but with the search phrase "aid AND misus* AND government." In the third round of data collection, I again searched for "aid AND misus* AND government," but did a separate search for each individual year from 1990 to 2014. Note that the observations are scandals (or minor scandals), not news stories about scandals, even while they are based on news stories.

Table 9: Cases from Aid Scandal Data, 2003-2008

Number	Year	Month	Day	Country	Major?
1	2003	10	18	Philippines	0
2	2003	10	19	Zimbabwe	0
3	2004	2	24	Iraq	1
4	2004	7	14	Haiti	0
5	2005	1	13	Indonesia	0
6	2005	2	13	Sri Lanka	1
7	2005	7	6	Nigeria	0
8	2005	12	7	Malawi	0
9	2006	1	22	Cambodia	0
10	2006	5	7	Vietnam	0
11	2006	6	1	Uganda	1
12	2006	10	4	Macedonia	1
13	2007	2	20	Colombia	1
14	2007	7	31	Iraq	0
15	2007	8	20	Zimbabwe	1
16	2007	12	27	Pakistan	0
17	2008	2	6	Afghanistan	1
18	2008	2	11	Indonesia	1
19	2008	3	26	Afghanistan	0
20	2008	5	8	Myanmar	1
21	2008	6	30	Pakistan	1
22	2008	7	4	Pakistan	1
23	2008	7	19	Bulgaria	1
24	2008	7	27	Costa Rica	1
25	2008	9	23	Iraq	1
26	2008	10	15	Tajikistan	0
27	2008	11	3	Zimbabwe	1

H Evaluating Exclusion Restriction

While the requirement that the instrumental variable be strongly correlated with the endogenous variable of interest is straightforward to evaluate, it is more difficult to ensure that the aid scandals do not affect unrest through alternate channels. As discussed above, there are a few potential mechanisms through which outside aid scandals could potentially affect domestic unrest independently of their effects on bypass aid. In the main text I argued that these threats should not be serious theoretically given that one of the key instrumental variables is a count of aid scandals that occurred outside country i's continent in year $t-2$. Here I perform some statistical tests to evaluate those potential violations of the exclusion restriction.

Table 10 presents the results of a series of bivariate models. One concern is that aid scandals might affect overall aid amounts, which could affect a government's repressive capacity. However, there are equally plausible arguments for both a positive and negative effect here. On one hand, aid scandals might lead donors to reduce overall aid flows in light of evidence of misuse. On the other hand, donors may only reduce aid to the country experiencing an aid scandal, reallocating fixed aid budgets to other countries and regions. In any case, while aid scandals are strongly correlated with the channel distribution of aid in a given country, Models 1 and 2 in Table 10 shows that they do not appear to affect total aid amounts.

Models 3 and 4 shows that there appears to be no direct effect of aid scandals on unrest, and Model 5 shows (perhaps surprisingly) that there is no effect of aid scandals in country i on unrest in country i. Model 6 shows that outside unrest is weakly correlated with less unrest in country i, and Model 7 suggests that unrest outside a given country's continent is not associated with domestic unrest. A possible explanation for the result in Model 6 is that autocrats increase repression when outside countries experience unrest to deter unrest, but since there is no evidence that scandals in country i affect unrest in that country (Model 5), this concern should not apply to aid scandals.

Implementing Conley, Hansen and Rossi's (2012) "Plausibly Exogenous" Test

The tests in the previous section seek to identify potential theoretical mechanisms through which aid scandals might affect unrest outside of their effects on the distribution of bypass aid. On balance, it seems reasonable that aid scandals would serve as an exogenous source of variation in aid channel distribution.

Others have outlined procedures for formally relaxing the exclusion restriction (Conley, Hansen and Rossi 2012) in the context of two-stage least squares estimation (2SLS). Implementing those procedures here is messy for a number of reasons. First, the models in the main article use different estimators in each stage of the analysis to accommodate the count structure of the outcome variable (unrest events). I use OLS to estimate the relationship between the various measures of aid scandals and the logged bypass ratio, and then negative binomial regression to estimate the relationship between bypass aid and a count of unrest events. To adjust for the incorrect standard errors in the second stage of this procedure, I rely on bootstrapped coefficients from the second stage to draw inferences. Second, the

Table 10: Evaluating Exclusion Restriction

	Dependent variable:						
	DV: Total Aid		DV: Unrest Events				
	OLS				*Negative binomial*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Major Aid Scandals	16.195		−0.024				
	(19.961)		(0.054)				
Major Aid Scandals Outside Continent		31.847		−0.008			
		(26.259)		(0.070)			
Scandals in Country					−1.864		
					(1.295)		
Outside Unrest						−0.031*	
						(0.018)	
Unrest Outside Continent							−0.005
							(0.019)
Constant	734.114***	721.415***	−0.353*	−0.398**	−0.386**	0.678	−0.310
	(77.673)	(73.893)	(0.206)	(0.197)	(0.159)	(0.660)	(0.443)
Observations	344	344	344	344	344	344	344
R^2	0.002	0.004					
Adjusted R^2	−0.001	0.001					
Log Likelihood			−323.800	−323.892	−322.810	−322.211	−323.865
θ			0.140*** (0.024)	0.140*** (0.024)	0.143*** (0.024)	0.146*** (0.025)	0.140*** (0.024)
Akaike Inf. Crit.			651.599	651.784	649.620	648.421	651.729
Residual Std. Error (df = 342)	1,104.885	1,103.578					
F Statistic (df = 1; 342)	0.658	1.471					
Note:							*p<0.1; **p<0.05; ***p<0.01

analysis in the main article transforms the predictions for the first stage dependent variable (logged bypass ratio) before including it as a covariate in the second stage. The transformation of the dependent variable is intended to make the dependent variable continuous following Dietrich (2013). When transforming the predictions from these models back into the [0,100] interval, the models generate predictions that lie in that interval. This is not the case when using the raw percentage variable, which in the prediction stage can result in negative values or values above 100. For these reasons, the main analysis does not implement 2SLS.

Nonetheless, I implemented the procedures in Conley, Hansen and Rossi (2012) using modified versions of two of the models in the main text (Models 8 and 9). I use the percentage of bypass aid as the endogenous regressor in the first stage in all three models. The second stage outcome variable is a dummy variable indicating whether or not at least one unrest event occurred. I estimated robust standard errors using Stata's `vce(robust)` command. The procedures outlined in Conley, Hansen and Rossi (2012) allow for researchers to relax the exclusion restriction in an instrumental variables model by introducing a parameter γ that represents any direct effect that the excluded instrument may have on the second-stage outcome variable (in this case, any direct effect that aid scandals could have on the likelihood of unrest.) The user specifies the minimum and maximum bounds for γ, call these γ_L and γ_H. I set γ_L=0 for all three models, and set γ_H equal to the estimated coefficient from a regression of the dichotomous unrest variable on each of the aid scandals measures. This value is -.007 for both Model 8 and Model 9.

Figure 3 reports the results of this procedure including the instruments used in Models 8 and 9 from the main text along with the control variables included in those models. In

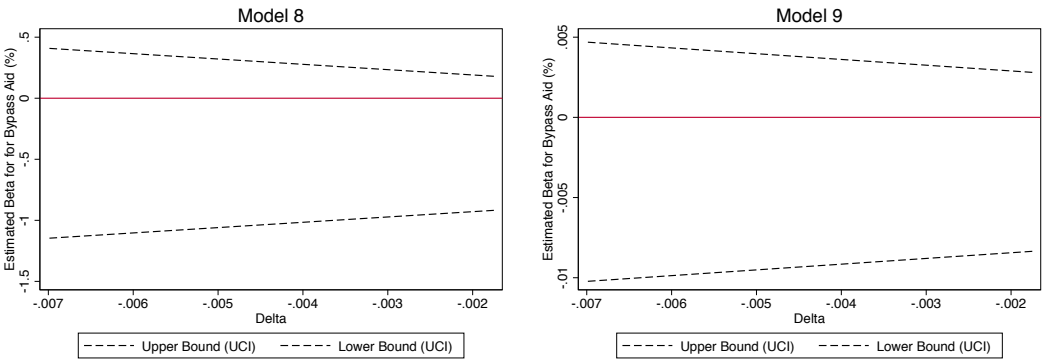

Figure 3: Union of Confidence Intervals Approach to Relaxing Exclusion Restriction.

all three cases, the mean of the estimated coefficient on the logged bypass ratio is negative, though the 80% union of confidence intervals includes zero in all three cases. As the assumption of no direct effect on the outcome is relaxed, the confidence intervals widen slightly. Again, a direct comparison with the results in the article is not possible given the different methods employed in each case. That is, the models used in the "plausibly exogenous" test are not the same as those reported in the main article, and are less appropriate for evaluating the key hypothesis given the nature of the data. Although the results are weaker when using this alternative framework, it is at least encouraging that these different design choices produce produce results that point substantively to the conclusions in the main analysis.

Ultimately the exogeneity of the excluded instrument needs to be justified on theoretical grounds, and the previous section identifies some potential threats to inference and shows that they are likely unwarranted. Further, the results in the main text show that the association between bypass aid and unrest is robust to a variety of alternate research design choices in ways that are consistent with the theory. Indeed, the main analysis explicitly seeks to control for confounders, and, as noted in the main text, the main results are also robust to controlling for military expenditures.

I Bypass Aid and Investment in Repressive Capacity

The theory suggests that government control of aid is not a necessary condition for aid to depress the incentive to revolt. Still, it is possible that bypass aid would influence unrest not through benefitting citizens, but rather because recipient regimes intercept or tax bypass aid and use these resources to invest in greater repressive capacity. In Table 11 I regress (logged) changes in military expenditures as a percent of government spending from the World Bank from year t to $t-1$ on bypass aid. Model 1 uses the proportion of bypass aid as the key independent variable. Model 2 uses the logged total of bypass aid in millions of US dollars, controlling for logged government aid. Since interstate crises and civil conflicts might affect both bypass aid and military expenditures, I control for a count of militarized interstate disputes and civil conflicts in the previous year. The count of the number of militarized interstate disputes (MIDs) that a country was involved in during the previous year is based on data from the Correlates of War project (Palmer et al. 2015). (The results do not depend on the inclusion of these variables.) To account for unobserved heterogeneity, I include year-fixed effects. In both models, the relationship between bypass aid and and changes in military expenditures is negative, though not statistically significant. This is consistent with the theory. If bypass aid provides a service that benefits people, then autocratic regimes need not invest in greater repressive capacity.

Table 11: Bypass Aid and Change in Military Expenditures (% Gov. Spending)

	Dependent variable: Change in Military Expenditures	
	Model 1	*Model 2*
	(1)	(2)
Bypass Aid / Capita	0.09	
	(0.23)	
Bypass Ratio		−0.02
		(0.02)
International Disputes	0.22	0.21
	(0.25)	(0.25)
Civil Conflicts	−0.74	−0.53
	(0.62)	(0.64)
Year-fixed Effects	Y	Y

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. Estimated standard errors in parentheses.
OLS estimates.

J Alternative *AidData* Measure

One potential concern in using the OECD data on aid channels is that the data could be subject to measurement bias due to underreporting as donors were adjusting to new reporting requirements that the OECD implemented in 2004.[7] Unfortunately, no other source provides comprehensive data on aid channels. However, the *AidData* project (Tierney et al. 2011), which records aid data at the project level, does contain information about the stated purpose for which donors commit aid. To create an alternative indicator of bypass aid, I search for patterns in the "purpose" variable that might indicate that aid was given through bypass channels. The string of patterns I search for in *R* (R Core Team 2014) is "`ngos|NGOs|NGO's|civil society|food|Food|Material relief`." I create a measure of the proportion of aid that was committed to projects that included one or more of these terms for each recipient year.[8] This measure is moderately correlated with the measure constructed from OECD data ($\rho = 0.54$).

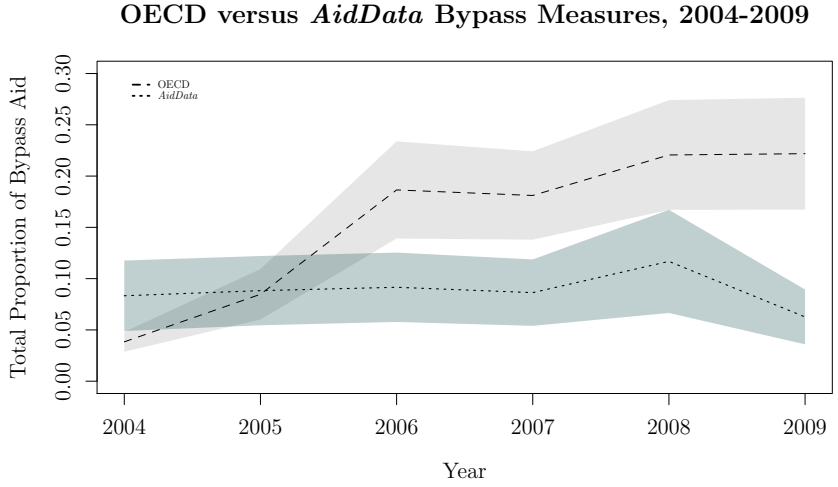

Figure 4: *Comparing Aid Measures*. The dashed line is the mean level of OECD bypass aid for all countries in the sample (i.e., all autocracies), and the dotted line is the mean level of bypass aid using my measure constructed from *AidData*. The shaded areas mark 95% confidence intervals.

Figure 4 plots the means of each measure ± 1.96 standard deviations for every year from 2004 to 2009. The mean level of bypass aid in the OECD measure is closely clustered around about four percent in 2004, while the *AidData* measure is more varied, centered around eight percent. The *AidData* measure is relatively consistent across time and may be undercounting bypass aid. Whether this is the case is unclear, though the measure is arguably less susceptible to concerns about underreporting during the early years of OECD reporting requirements.

[7] Dietrich (2013, 702) also acknowledges this issue in footnote 13 in her article.

[8] Though disbursements would be more appropriate, this data is not available from *AidData*. See Section J in the Supplementary Files for a description of a validity check for this coding procedure.

Table 12 reports the results of the basic model specification of Model 2 from Table 2 substituting in the alternative measure of bypass aid. Again, there is clear support for the theory in that the measures of bypass aid is negatively associated with unrest events at the 95% confidence level or greater. Because the results are similar across measures and there is no clear benchmark to compare *AidData* measure in the pre-2004 period (and Figure 4 suggests that undercounting may not actually be much of a concern), I use the OECD measure in the main analysis. Additionally, the *AidData* measure captures only commitments, not disbursements. Because donors often withhold or delay the disbursement of aid commitments, using the OECD data on actual disbursements likely does a better job capturing the political effects of bypass aid in recipient countries. Still, the congruity of the results from using this alternative measure with the OECD measure is encouraging.

Table 12: Alternative AidData Measure

	Dependent variable: Unrest Events	
	Model 1	*Model 2*
	(1)	(2)
Bypass Aid / Capita	-0.37^{***}	
	(0.12)	
Bypass Ratio		-2.54^{**}
		(1.05)
Governance Index	-0.92^{***}	-0.90^{***}
	(0.24)	(0.29)
Civil Conflicts	0.49^{**}	0.41^{**}
	(0.23)	(0.18)
Natural Disasters	0.13^{***}	0.14^{***}
	(0.01)	(0.01)
Observations	633	655
Log Likelihood	−544.88	−559.60
θ	0.18^{***} (0.02)	0.18^{***} (0.02)
Akaike Inf. Crit.	1,099.76	1,129.19

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. (White's (1980) HC1 standard errors)
Negative binomial regression models.

Validity Check for *AidData* Measure

A preliminary validity check suggests that the coding procedure used to construct the alternate measure of bypass aid from the *AidData* project succeeds in identifying cases where bypass is likely. In a random sample of 50 cases where the bypass indicator takes a value of "1," all of the coded projects could plausibly bypass the government. The unique purpose codes in the sample are: `Democratic participation and civil society, Material relief`

assistance and services, Strengthening civil society, Support to international NGOs, Emergency food aid, Food crop production, Support to national ngos, Food aid/Food security programmes, Food security programmes/food aid, Support to local and regional ngos. Of course, there is still potential for measurement error. For example, it could be the case that a donor gives aid for the purpose of "Support to national ngos," but does so through the recipient government, and the aid never makes it to NGOs (or never makes it to citizens). As discussed in the main text, this is the same problem scholars face in using the OECD data. Still, relative to aid labeled as "budget support," we should be confident that a great deal of what this measure picks up is in fact bypassing the government.

K Alternative Operationalizations of Bypass Aid with OECD Data

Table 13 reports the results of models that use alternative operationalizations of bypass aid. Model 1 calculates the bypass ratio variable excluding aid delivered through public-private partnerships. Model 2 uses a measure of total bypass aid (including public-private partnerships, as in the main measure) as a percentage of real GDP output from Gleditsch (2002). As in the main text, bypass aid is statistically significant and negatively associated with unrest events.

Table 13: Alternative OECD Measures of Bypass Aid

	Unrest Events	
	Model 1	*Model 2*
	(1)	(2)
Bypass Ratio (Excluding Public-Private Partnerships)	−0.03**	
	(0.01)	
Bypass / GDP		−1.08**
		(0.52)
Governance Index	−2.17*	−1.65
	(1.20)	(1.26)
Civil Conflicts	0.41	0.41
	(0.35)	(0.44)
Natural Disasters	0.01	0.04
	(0.07)	(0.08)
Country-fixed Effects	Y	Y
Year-fixed Effects	Y	Y
Observations	338	295
Log Likelihood	−216.09	−206.07
θ	0.91*** (0.24)	0.97*** (0.26)
Akaike Inf. Crit.	572.18	536.15

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. Estimated standard errors in parentheses.
Negative binomial regression.

L Controlling for Strategic Importance of Recipient Country

To account for the possibility that the strategic importance of recipient countries explains both bypass aid and unrest in a given country, Table 14 reports the results of models controlling for variable measures of strategic importance: (1) years since independence (with the expectation that countries that achieved independence more recently may have been more valuable to colonizing states), (2) a dummy variable indicating whether the country had an alliance with the US during a given year, (3) a count of the number major-power allies that a country had during a given year (both of these measures come from the Correlates of War Formal Alliances data set version 4.1), and (4) a dummy variable that takes a value of one for countries that are members of the Organization of the Petroleum Exporting Countries (OPEC). In both models, the bypass variables remain negative and statistically significant.

Table 14: Controlling for Strategic Importance of Recipient Country

	Unrest Events	
	Model 1	*Model 2*
	(1)	(2)
Bypass / Population	-0.39^{**}	
	(0.18)	
Bypass Ratio		-0.02^{**}
		(0.01)
Governance Index	-1.25^{***}	-1.30^{***}
	(0.33)	(0.30)
Civil Conflicts	0.21	0.06
	(0.27)	(0.23)
Natural Disasters	0.02	0.02
	(0.03)	(0.03)
Years Since Independence	0.004^{***}	0.01^{***}
	(0.001)	(0.002)
US Ally	0.98^{*}	1.29^{**}
	(0.55)	(0.56)
No. Major Power Allies	0.22	0.19
	(0.25)	(0.22)
OPEC Member	-1.83^{***}	-1.59^{***}
	(0.57)	(0.60)
Observations	326	338
Log Likelihood	−269.30	−276.98
θ	0.27^{***} (0.06)	0.24^{***} (0.05)
Akaike Inf. Crit.	556.61	571.96

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. (White's (1980) HC1 standard errors)
Negative binomial regression.

M Instrumental Variables Analysis with Bypass Per Capita Measure

As I argue in the main text, the aid scandals measures are most appropriate to use as instruments for the bypass ratio – rather than the bypass per capita measure – from a theoretical standpoint. Nonetheless, here I replicate the analysis replacing the bypass measure with the bypass per capita measure. Although the second stage results are no longer statistically significant, the second stage coefficients on the predicted bypass per capita variable remain negative in two of the three models (though the standard errors are very large). Given that there is a stronger theoretical link between aid scandals and the bypass ratio rather the bypass per capita measure (i.e., aid scandals could lead to a shift in the distribution of aid across channels without leading to an increase in bypass per capita), using the bypass ratio is preferable from a research design perspective.

Table 15: Stage I - Aid Scandals and Aid Channel Distribution, 2005-2010

	Logged Bypass Aid Per Capita		
	Model 8, Stage I	*Model 9, Stage I*	*Model 10, Stage I*
	(1)	(2)	(3)
Major Aid Scandals	0.14***		
	(0.02)		
Extra-Continental Scandals		0.18***	
		(0.02)	
Affinity-weighted Scandals			0.19***
			(0.05)
Governance Index	−0.44***	−0.40***	−0.41***
	(0.09)	(0.09)	(0.08)
Civil Conflicts	0.23*	0.19	0.21
	(0.14)	(0.14)	(0.14)
Natural Disasters	−0.01	−0.01	−0.01
	(0.01)	(0.01)	(0.01)
Average Global GDP Growth	0.12	0.10	
	(0.08)	(0.08)	
Average Global Inflation	0.002*	0.002*	
	(0.001)	(0.001)	
Total Global Disasters	0.01***	0.01***	
	(0.002)	(0.002)	
Year-fixed Effects	N	N	Y
F-Statistic on Instrument	38.26	51.14	14.59
Observations	332	332	332
R^2	0.21	0.24	0.25
Adjusted R^2	0.19	0.22	0.23
Residual Std. Error	1.05 (df = 324)	1.04 (df = 324)	1.03 (df = 322)
F Statistic	12.30*** (df = 7; 324)	14.38*** (df = 7; 324)	12.09*** (df = 9; 322)

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. Estimated standard errors in parentheses.
OLS estimates.

Table 16: Stage II - Predicted Bypass Per Capita and Unrest, 2005-2010

	Unrest Events		
	Model 8, Stage II	*Model 9, Stage II*	*Model 10, Stage II*
	(1)	(2)	(3)
Predicted Bypass Per Capita	−0.17	−0.02	0.29
	(0.41)	(0.37)	(0.76)
Governance Index	−1.17***	−1.12***	−1.03**
	(0.34)	(0.32)	(0.42)
Civil Conflicts	0.44	0.43	0.37
	(0.34)	(0.33)	(0.37)
Natural Disasters	0.14***	0.14***	0.14***
	(0.03)	(0.03)	(0.03)
Average Global GDP Growth	−0.25	−0.24	
	(0.20)	(0.21)	
Average Global Inflation	0.003	0.003	
	(0.004)	(0.004)	
Total Global Disasters	−0.003	−0.004	
	(0.01)	(0.01)	
Year-fixed Effects	N	N	Y
Observations	326	326	326
Log Likelihood	−278.88	−278.96	−278.35
θ	0.20*** (0.04)	0.20*** (0.04)	0.20*** (0.04)
Akaike Inf. Crit.	573.76	573.91	576.70

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. Estimated standard errors in parentheses.
Negative binomial regression models.

N Full Output for Instrumental Variables Analysis

Table 17: Stage I - Aid Scandals and Aid Channel Distribution, 2005-2010

	Log-Transformed Bypass Ratio		
	(8)	*(9)*	*(10)*
Major Aid Scandals	0.24***		
	(0.04)		
Extra-Continental Scandals		0.29***	
		(0.04)	
Affinity-weighted Scandals			0.29***
			(0.08)
Governance Index	−1.86***	−1.81***	−1.83***
	(0.13)	(0.13)	(0.13)
Civil Conflicts	0.09	0.10	0.09
	(0.17)	(0.16)	(0.16)
Natural Disasters	0.02	0.03	0.03
	(0.02)	(0.02)	(0.02)
Average Global GDP Growth	0.35***	0.31***	
	(0.12)	(0.12)	
Average Global Inflation	0.003	0.003	
	(0.002)	(0.002)	
Total Global Disasters	0.01***	0.01***	
	(0.003)	(0.003)	
Year-fixed Effects	N	N	Y
F-Statistic on Instrument	46.52	57.99	14.83
Observations	344	344	344
R^2	0.47	0.49	0.49
Adjusted R^2	0.46	0.48	0.48
Residual Std. Error	1.62 (df = 336)	1.59 (df = 336)	1.58 (df = 334)
F Statistic	42.83*** (df = 7; 336)	45.56*** (df = 7; 336)	36.30*** (df = 9; 334)

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. Estimated standard errors in parentheses.
OLS estimates.

Table 18: Stage II - Predicted Bypass Ratio and Unrest, 2005-2010

	Unrest Events		
	(8)	*(9)*	*(10)*
Predicted Bypass Ratio	-0.06^{**}	-0.04^{*}	-0.06^{**}
	(0.02)	(0.02)	(0.03)
Governance Index	-1.73^{***}	-1.49^{***}	-1.88^{***}
	(0.43)	(0.38)	(0.48)
Civil Conflicts	0.25	0.21	0.26
	(0.27)	(0.27)	(0.27)
Natural Disasters	0.15^{***}	0.14^{***}	0.14^{***}
	(0.03)	(0.03)	(0.03)
Average Global GDP Growth	-0.25	-0.26	
	(0.20)	(0.20)	
Average Global Inflation	0.01	0.005	
	(0.004)	(0.004)	
Total Global Disasters	-0.001	-0.002	
	(0.01)	(0.01)	
Year-fixed Effects	N	N	Y
Observations	338	338	338
Log Likelihood	-284.16	-285.02	-283.93
θ	0.20^{***} (0.04)	0.20^{***} (0.04)	0.20^{***} (0.04)
Akaike Inf. Crit.	584.32	586.05	587.87

*p<0.1; **p<0.05; ***p<0.01
Two-tailed tests. Estimated standard errors in parentheses.
Negative binomial regression models.

O Online Appendix References

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