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Global Samaritans? Donor Election Cycles and the Allocation of Humanitarian Aid*

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Abstract

This paper finds a large causal donor election cycle effect in humanitarian aid allocations: On average, humanitarian aid increases by 54% in the year before elections. Our identification strategy consists of focusing on donors with fixed election dates, making elections clearly exogenous. Furthermore, we find large interaction effects with natural and human disasters. This evidence is consistent with our theory that incumbent governments responding to humanitarian disasters can increase voter support for their party and insure against the political fall-out of not being seen as representatives of a country with global interests and influence. However, it is important to stress that despite our findings, human and natural disasters explain a substantially larger share of the overall variation in humanitarian aid observed in the data.

Keywords: Humanitarian aid; election cycles; aid allocation

JEL Codes: F35; D72; H5; O19

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1 Introduction

In the last few years, bilateral donors have spent over 7 billion US dollars on humanitarian aid annually.[1] This represents about 12% of total net ODA making it a fairly significant component of overall bilateral aid. The existence of this form of aid shows that compassion and altruism extend across countries. When people in more or less distant parts of the world are faced with a disaster, helping out would appear to be a global norm.[2] However, in this paper we show that donor governments may not only give humanitarian aid because they are global samaritans, but also because there is an election coming up, and giving humanitarian aid to people affected by a disaster may increase support from voters. The evidence we present shows a large causal election cycle effect in donor countries: On average, humanitarian aid increases by 54% in the year before elections. Our identification strategy consists of focussing on donors with fixed election dates, making elections clearly exogenous.

We find no such election cycle effects for overall aid (net ODA). This finding fits well with our theory stating that humanitarian assistance addresses a much more visible kind of a problem than other forms of aid. There is often extended media coverage when disasters occur, and it seems likely that there is a political benefit from reacting promptly and decisively to such "spotlight" disasters, particularly then when an election is coming up. It not only shows that a government cares for people in hardship but also that a government is a relevant player when the "world" is dealing with an emergency. Thus, helping out may not only be perceived as a matter of solidarity but also competence. In addition, increasing humanitarian aid may also insure against the potential political fall-out that may occur if a government is perceived as not having done enough in an international crisis. For these reasons, increasing humanitarian aid may be valued across the political spectrum in donor countries. We further investigate these ideas by using a dyadic recipient-donor panel dataset that permits us to run regressions with interaction terms. We find large election interaction effects with human and natural disasters. We

[1] This includes only official flows as reported by the OECD between 2009 and 2012 and it excludes humanitarian aid given via multilateral agencies such as the UN.

[2] How that is done and if enough is done is of course another question. Some will argue that these efforts need to be increased substantially given the many problems humankind is facing (Global Humanitarian Assistance, 2015), whereas others are highly critical about current practices related to humanitarian aid (see for example Polman, 2010).

find that humanitarian aid to countries with a conflict increases by 315% and aid to a country with a natural disaster at the 90th percentile, in terms of affected people, increases by 175% in the year before elections. For a natural disaster of medium size, the increase is 72%.

Our paper contributes to the larger literature on foreign aid that deals with donor and recipient specific determinants of aid flows (e.g. Alesina and Dollar, 2000; Burnside and Dollar, 2000; Claessens, Cassimon, and Van Campenhout, 2009; Dollar and Levine, 2006; Neumayer, 2003a,b). More directly relevant for our paper is the literature related to humanitarian aid that shows that the level of humanitarian aid distributed by donors may not just be an expression of pure altruism but is also influenced by domestic strategic factors (see Drury, Olson, and Belle, 2005; Strömberg, 2007; Eisensee and Strömberg, 2007; Fink and Redaelli, 2011; Raschky and Schwindt, 2012). For example, Fink and Redaelli (2011) shows that on average, donor governments favor smaller, geographically closer and oil exporting countries, and display significant biases in favor of politically less aligned countries as well as toward their former colonies. Similarly, Raschky and Schwindt (2012) find that donor countries' decision on the channel and type of aid is not only affected by humanitarian need but also by strategic interests in trade and natural resources as well as the quality of institutions in the recipient country. Thus, similar to the work in this literature, our paper supports the proposition that politics plays a role in the allocation of foreign aid, which includes aid that is given for the purpose of helping others in disaster emergencies. To our knowledge, we are the first paper to investigate the impact of election cycles in donor countries on the allocation of aid. In a recent paper, Faye and Niehaus (2012) looked at political aid cycles by linking aid flows with elections in recipient countries. They show that bilateral donors use aid to influence elections in recipient countries. Here, we show that governments may use humanitarian aid to influence their own elections as well.

This paper also bridges the literature on the politics of aid giving with the literature on political budget cycles, which typically focuses on the effects of an upcoming election on the composition of a government's domestic expenditure (see Brender, 2003; Kneebone and McKenzie, 2001; Khemani, 2004; Drazen and Eslava, 2010; Hanusch and Keefer, 2014). For example, Kneebone and McKenzie (2001) find that Canadian provincial governments tend to increase spending in highly visible areas (schools, roads and hockey rinks) in the year prior to an election. Drazen and Eslava (2010) find similar effects at the municipal level in Colombia, with spending on major infastruc-

ture projects including roads, and power and water plants shown to increase prior to elections. Our paper suggests that models of political agency and the electoral process developed by Barro (1973) and Ferejohn (1986) do not need be limited to domestic expenditure but should include the money an incumbent government spends abroad as well. The media attention that often comes with human and natural disasters may make such budget decisions politically beneficial.

We test our hypothesis by examining the effect of elections in 18 donor countries between 1995 and 2012. Our main result focusses on the 8 donors with fixed election dates as these are exogenous to the decision-making process of the government. When governments can choose the election date, econometric issues may arise as for example a better budgetary situation makes it more likely that an election is called, which also may affect the level of humanitarian aid. In addition, these governments can use election timing as an additional tool to increase chances for re-election. Also, governments may break by a confidence vote in parliament, making elections less predictable. All these may suggest that the mechanism uncovered here may be less important for countries that do not have a fixed election date. Our estimate suggests that humanitarian aid, on average, increases by 54% in the year before elections. Our result is robust: First, the result remains statistically significant (p-value 0.36) when using the "Wild Cluster Bootstrap" proposed by Cameron, Gelbach, and Miller (2008) because our clustered standard errors may be downward biased as our regressions have only a small number of clusters. Second, our election dummy remains statistically significant when running the regression without the US, which is by far the largest donor of humanitarian aid. Third, our result is robust to the inclusion of per capita income changes and central government budgetary surplus lagged by one year. When including these controls, we also find an election cycle effect for donors without fixed election dates. The increase in this case is 20% (significant at the 10% level).

When we repeat the same analysis using ODA instead of humanitarian aid, our election dummy coefficient is substantially reduced and never significant. Thus, we do not find an election cycle effect for overall aid. We believe this has to do with the fact that human and natural disasters often come with substantial media attention which brings aid efforts related to these incidences into the spotlight. It is this attention that may translate into increased voter support for an upcoming election. For example, Eisensee and Strömberg (2007) have shown that increased media attention leads to

higher levels of humanitarian aid by the US. It is certainly plausible that our election cycle effect is partially driven by such "spotlight" disasters.

By using a dyadic donor-recipient panel data set we can expand our analysis to test whether our election cycle effect is conditional on recipient specific and dyad specific factors. In this analysis, we include covariates such as natural disasters, conflicts, and recipient income levels, which have been shown to be correlated with increased levels of humanitarian aid in the pervious literature (Fink and Redaelli, 2011; Raschky and Schwindt, 2012; Bermeo, 2010). We also include other political motives that have been analyzed in the literature such as UN voting affinity and democracy, as well as a new dyadic variable that to our knowledge has not been studied before: the share of recipient immigrants in a donor country. The idea behind this variable is as immigrants are also part of the electorate in donor countries, there may be some political benefits from focusing aid efforts to recipient countries with a large immigrant share. In support of the existing literature, we find that the incidence of a civil conflict or a natural disaster leads to a substantial increase in humanitarian aid. We also find that there are significant interaction effects between the incidence of natural disasters and conflicts and the election variable. In fact, our study shows that humanitarian aid to countries experiencing a conflict increases by 315% in the year before an election. We show similarly strong interaction effects for natural disasters: for a natural disaster of medium size in terms of the total of people who are affected by the disaster, humanitarian aid increases by 72% in the year before elections. For a disaster at the 90th percentile in terms of affected people, humanitarian aid increases by 175% in the year before elections. Related to our immigrant share variable, we show that there is a positive and significant relationship between immigrant share and humanitarian aid. We, however, do not find any evidence of humanitarian aid increasing more to recipient countries that have a large share of immigrants in the year before elections.

We want to emphasize that even though we find a strong and robust election cycle effects in the data, these effects only explain a minor part of the total variation in humanitarian aid observed in the data. Other reasons, such as conflict or natural disaster explain a substantially larger share of the total variation. When running univariate regressions for each of these variables, we show that the explanatory power of these traditional variables is substantially larger than the ones related to "politics." For example, the R-squared in a univariate regression for the incidence of a natural disaster is about 10 times higher than the corresponding R-squared for the incidence of

an election in a donor country. The explanatory power of conflicts is almost 6 times higher than the explanatory power of election cycles.

The rest of the paper is organized as follows. Section 2 describes the data sources. Section 3 presents the donor panel results and Section 4 the dyadic panel results. Section 5 compares donor motives using univariate regression comparisons. Section 6 concludes.

2 Data Sources

We obtain our humanitarian aid from the OECD's DAC database (Table 2a). In DAC reporting, aid is considered humanitarian if it is used for the purposes of "disaster prevention and preparedness, reconstruction relief, relief coordination, protection and support services, emergency food aid and other emergency/distress relief." This strict definition of humanitarian aid, which is governed by the principles of neutrality and impartiality, marks it out from development aid, which can be subject to some conditionality. We use all available data, which is for the years 1995 to 2012. We focus on bilateral donors that are well established within the global aid community by including 18 donor countries all of whom have been members of the DAC by 1990. For 146 recipients in our sample of total 189 recipients, we have data for every year between 1995 and 2012. 60% of these recipients received humanitarian aid every year. There are only 15% of these recipients that received humanitarian aid in less than half the years for the period we studied. Thus, we can conclude receiving humanitarian aid seems to be a rather permanent condition for the large majority of the countries for which we have data over the entire period of our study.

Our data on the national elections in donor countries comes from the National Elections Across Democracy and Autocracy (NELDA) database. For a detailed description of this data set see Hyde and Marinov (2012). We focus on government elections, where the leader of the country's government is elected. These consist of executive elections in presidential democracies and parliamentary elections in parliamentary democracies. In the full sample, there occurred 123 elections with 91 occurring in countries with non-fixed election dates and 32 in countries with fixed election dates. The average donor in a non-fixed election date country experienced 5.06 elections during the sample period and the average donor in a fixed election date country experienced 4 elections. The following countries are included in our fixed

election date sample: Austria, Finland, France, Germany, Norway, Sweden, Switzerland, and the United States.

Our data on conflicts was taken from the UCDP/PRIO Armed Conflict Dataset (Gleditsch, Wallensteen, Eriksson, Sollenberg, and Håvard, 2002). For the purposes of this analysis only those conflicts with at least 1000 battle-related deaths during a calendar year were included. For example, the conflicts between the state of Israel and Hezbollah during the 1990s and 2000s are not included whereas the conflict between the state of Pakistan and the Tehrik-i-Taliban Pakistan from 2008 onwards is included because the latter crossed the battle-related deaths threshold. We include all types of conflicts reported in the data base, which includes intra-state (civil wars) and inter-state conflicts. This specification resulted in 147 conflict-year observations being included. The average length of these conflicts was 4.8 years, and in any given year there were at least 4 conflicts with the maximal number of conflicts in a single year being 13, which occurred in 1998. There were also 12 conflicts in 1999 and 2000. On average, there were 8.25 conflicts in a given year. These discouraging numbers imply there was a continual need for humanitarian assistance over the evaluated time period independent of natural disasters.

The data on natural disasters was taken from the Centre for Research on the Epidemiology of Disasters (CRED) Emergency Events Database (Guha-Sapir, Below, and Hoyois, 2015). In order to assure the exogeneity of the events, this analysis focused on natural rapid onset disasters such as droughts, earthquakes, extreme temperature, floods, storms, etc. similar as in Strömberg (2007). This specification resulted in 1530 disaster events being included in the analysis with the average recipient experiencing a disaster in 43% of the years included in the sample. Given the number of disasters included, there was a wide variation in the total number of people affected by each event. A disaster in the 50th percentile affected 31,890 people while a disaster in the 90th percentile affected 1,821,495.

Additional recipient variables such as population, income per capita, and central government budget surplus were taken from the World Development Indicators Database (World Development Indicators).[3] For our democracy measure, we use the measure for Civil Liberty (CL) by Freedom House.

[3]We complemented data on central government budget surplus by using the Government Finance Statistics from the IMF as WDI has many missing values, particularly for earlier years.

We invert that measure to make the interpretation of the regression results easier.[4] The UN voting data we obtained from Voeten, Strezhnev, and Bailey (2009). Affinity is measured by the average difference of votes in roll-call votes in the UN general assembly between 1995 and 2012. If a donor and recipient vote identically, this difference is zero, and it is 2 for yes-no or no-yeas pairs and 1 of yes-abstain or no-abstain pairs. Thus, a lower value measures higher affinity. The immigration data we obtained from United Nations (2015). This data is available for every 5 years only. We used the *ipolate* command in Stata to fill in data for the missing years. This command linearly interpolates values. Finally, the data for donor-recipient distance and donor-recipient colonial history we obtained from CEPII (see Mayer and Zignago, 2011). We use the direct distance between the most populated cities in kilometres as our distance measure.

3 Aid Election Cycles: Donor Panel Analysis

There are several reasons why donors might increase the level of humanitarian aid prior to an election. The existence of such a phenomenon would support the models of political agency and the electoral process first developed by Barro (1973) and later refined by Ferejohn (1986) that suggested the existence of political budget cycles. These models stated that politicians, once in government, cannot necessarily be relied upon to keep the promises made during an election campaign. As such, there is an incentive for voters to base their vote on the actions of the politicians while they are in office, and therefore incumbent politicians will make their expenditure choices in anticipation of this behaviour giving rise to cyclical behaviour in budget spending around elections. The theoretical support for our hypothesis that humanitarian assistance would be one of the types of expenditure that governments would increase prior to elections stems from the work of Rogoff (1990) and Rogoff and Sibert (1988). These authors write that because voters do not observe all government expenditure there is an incentive for politicians to increase spending in areas that voters can observe. Critically, both the problems presented by humanitarian emergencies, and the solutions that humanitarian assistance is designed to fund, bare distinct similarities to the high visibility types of domestic expenditure that have been shown to increase prior to

[4]In the Freedom House index, CL is measured between a number of 1 and 7, where a larger number means less freedom.

Table 1: Election Cycles and Humanitarian Aid: Donor Panel

Dependent Variable: Humanitarian Aid (Log, in const. USD)	(I)	(II)	(III)	(IV)	(V)	(VI)
=1 one year before elections	0.43**	0.26***	0.45**	0.09	0.18*	0.24**
	(0.18)	(0.07)	(0.18)	(0.12)	(0.09)	(0.10)
Change in GNI per capita			-1.73		10.85	2.85
			(4.98)		(7.44)	(4.55)
Budget Surplus (lagged)			0.03		-0.11	0.01
			(0.08)		(0.08)	(0.06)
Constant	3.96***	3.64***	3.85***	4.14***	3.28***	3.83***
	(0.88)	(0.95)	(0.98)	(0.52)	(0.84)	(0.68)
N	143	125	142	169	148	290
R-squared	0.07	0.08	0.09	0.16	0.18	0.07

All regressions include year fixed effects, which are not reported. Columns (I)–(III) includes donors with fixed election dates and Columns (IV) and (V) includes donors without fixed election dates. Column (II) excludes the US. Cluster-robust standard errors at the donor level are reported in parenthesis. Significance levels : $*$: 10 $**$: 5 percent $***$: 1 percent.

elections (see Brender, 2003; Kneebone and McKenzie, 2001; Khemani, 2004; Drazen and Eslava, 2010). The visible nature of humanitarian emergencies, whether man-made or naturally occurring, is self evident given their definition as the loss of human life or suffering on a massive scale. Similarly, the solutions that humanitarian aid funds, as a response to these highly visible problems, are by nature highly visible. Their visibility is derived from the ease with which a voter may understand the altruistic intent of the provision of food, water, shelter, medicine and the other most basic necessities for human survival that are funded by humanitarian aid. From the perspective of the voter, the ease of understanding the positive effects of humanitarian aid directly contrasts the substantial and highly public debate around the effects of traditional forms of aid.[5] The significant difference in the level of debate around the effects of each type of aid allows humanitarian aid to trigger a more spontaneous and intrinsic sense of altruism for the voter and alleviates concerns that don't allow increases in traditional aid to secure voter sup-

[5]See for example Sachs (2005) on the one side and Easterly (2006) and Moyo (2009) on the other for such a debate.

port.[6] Additionally, voters may view the capacity to provide humanitarian assistance as an indicator of the incumbent government's ability to influence the world globally. It may not just be about showing compassion and altruism but also about showing that a government is a relevant player when the "world" is confronted with an emergency. Thus, it may not only be about showing solidarity but also about showing competence. These characteristics stand in contrast to traditional forms of aid, which address more complicated issues with more complex solutions, and whose effects are subject to considerably more debate, which would therefore imply there should be no effect of elections on ODA.

One statistical concern is that donor governments may choose the date of their elections to correspond with high levels of voter support generated by social issues or the strength of the country's economy thereby creating an endogeneity problem in the regression. For example, a better budgetary situation makes it more likely that an election is called, which also may affect the level of humanitarian aid. We address this problem by using two samples, our sample consisting of donors with fixed election dates and the one consisting of donors without fixed election dates. Elections with constitutionally fixed dates are clearly exogenous. Thus, the regression

$$\text{aid}_{i,t} = \alpha * \text{election}_{i,t} + \gamma_t + \epsilon_{i,t}, \tag{1}$$

where i indexes a donor will be able to identify the election cycle effect α when we use our fixed election date sample. We also include a year fixed effect, γ_t. Notice though that whether we include this effect or not barely changes our results, which supports our claim of our election dummy being exogenous. Table 1 shows our regression results. In all regressions we obtain a positive coefficient for our election dummy variable, which is consistent with our theory of election cycles related to humanitarian aid. Columns (I) – (III) show the effect for donors with fixed election dates. Column (I) shows the first result. We find that spending in humanitarian aid increases by 53.7% in the year before an election. In all the regressions we use cluster-robust standard errors. Since we only have 8 clusters, the reported standard errors may be downward biased. In order to correct for such a potential bias, we use the "Wild Cluster Bootstrap" procedure proposed by Cameron, Gelbach,

[6]This is not to say there is no debate around the effects of humanitarian aid, for example see Polman (2010), but that there is substantially less debate around the effects of humanitarian aid compared to traditional forms of aid.

and Miller (2008) to generate p-values. This procedure is appropriate when the number of clusters is low. Our result remains statistically significant at the 5% level with a p-value of 0.36. In Column (II) we remove the US from our sample as a robustness check. We see that we still find a statistically significant election cycle effect. Thus, our result is not driven by the fact that our sample includes the largest donor of humanitarian aid.[7] In Column (III) and (V) we include the change in GNI per capita and central government budget surplus lagged by one year as additional controls. We pointed out that a comfortable budgetary situation and increasing income levels may make elections more likely and also increase aid spending when governments can choose the election date. In Column (III) we use the sample of donors with fixed election dates and we find that our coefficient hardly changes. These controls, however, make a difference in our sample of donors that do not have fixed election dates. When comparing regressions (IV) and (V) we see that the coefficient increases and now is statistically significant at the 10% level. Column (VI) reports the result for our full sample for completeness. We find a statistically significant election cycle effect overall. Noteworthy is that our coefficient of interest is lower in all the regressions that use the sample of donors without fixed election dates as compared to the sample of donors with fixed election dates. This may have to do with the possibility that a government without fixed election dates can use the timing of the election as an alternative or additional instrument to influence elections to their benefit. As a result, they may have less of a need to try to increase voter support by using disaster relief.

In order to test whether there are election cycle effects for overall aid, we run the same set of regressions for net ODA instead of humanitarian aid. Net ODA is a comprehensive aid measure which includes many forms of aid, including humanitarian aid. It is the most commonly used measure in studies related to foreign aid. Table 2 shows the results. The coefficients on the election dummy variable is substantially lower in all the regressions and it is not significant in any of them. Thus, we find no evidence of an election cycle effect related to overall aid. We have argued that this may happen because of the higher visibility of humanitarian aid, which is important if aid policies should translate into electoral support.

[7] In average over our sample period, the US contributed 32% to total humanitarian aid in a given year. This is the largest share by far. No other donor contributed more than 10% in average.

Table 2: Election Cycles and ODA: Donor Panel

Dependent Variable: ODA (Log, in const. USD)	(I)	(II)	(III)	(IV)	(V)	(VI)
=1 one year before elections	0.11	-0.00	0.07	0.03	0.15	0.06
	(0.14)	(0.10)	(0.12)	(0.10)	(0.09)	(0.08)
Change in GNI per capita			-8.52		4.04	-3.23
			(9.29)		(8.04)	(5.05)
Budget Surplus (lagged)			-0.06		-0.17*	-0.07
			(0.06)		(0.08)	(0.06)
Constant	7.49***	7.33***	7.52***	7.11***	6.11***	7.18***
	(0.48)	(0.52)	(0.54)	(0.35)	(0.54)	(0.40)
N	144	126	143	180	157	300
R-squared	0.02	0.01	0.10	0.03	0.23	0.09

All regressions include year fixed effects, which are not reported. Columns (I)–(III) includes donors with fixed election dates and Columns (IV) and (V) includes donors without fixed election dates. Column (II) excludes the US. Cluster-robust standard errors at the donor level are reported in parenthesis. Significance levels : $*$: 10 $**$: 5 percent $***$: 1 percent.

4 Aid Election Cycles: Dyadic Panel Analysis

Donors allocate humanitarian aid for many reasons. We have pointed out earlier that many recipient countries receive humanitarian aid every year. We expect that the election cycle effect of humanitarian aid may mostly work through natural or human disasters, as such disasters typically receive substantial media attention ("Spotlight disaster"). Such attention is crucial if increased spending there should translate into higher voter support. Eisensee and Strömberg (2007) and Strömberg (2007) show for US disaster and relief efforts that there is a causal link between media attention and the level of these efforts. It is plausible that this media attention is a crucial channel for the election cycle that we find in the data. Unfortunately, it is beyond the scope of this paper to produce a "news" variable for our cross-country context in order to test this proposition more directly. However, we can do so indirectly. We run dyadic panel regressions that will include human and natural disaster variables that we then interact with our election dummy variable. In addition, we will be able to test for a wide range of donor motives

related to humanitarian aid allocations. We estimate the following model:

$$\text{aid}_{i,j,t} = \alpha * \text{election}_{i,t} + X_{i,j,t} + Z_{j,t} + \gamma_t + \epsilon_{i,j,t}, \tag{2}$$

where i indexes donors, j recipients, and t years. $X_{i,j,t}$ is a vector of dyadic specific controls such as colonial past, immigrant share in donor countries, UN voting affinity, and geographical distance. $Z_{j,t}$ is a vector of recipient specific controls such as income and population, and measures for natural and human disasters. As before, γ_t is a year fixed effect. Again, our main interest is the election cycle coefficient α and this coefficient conditional on other covariates when we interact our election dummy with these other covariates.

Table 3 shows the regression results. Column (I) confirms our result from Table 1 showing a large election cycle effect. Column (II) adds human and natural disasters occurring in recipient countries into the regression. We see that our coefficient of interest stays unchanged and that conflicts and natural disasters all are significantly positively related to humanitarian aid. In fact, the effect is very large. Having a conflict increase humanitarian aid 800 fold. We find similarly large effects for natural disasters. Here, we confirm results from previous studies by Fink and Redaelli (2011), Raschky and Schwindt (2012) and Bermeo (2010). Fink and Redaelli (2011) and Raschky and Schwindt (2012) focus on the determinants of the flows of humanitarian aid but exclude civil conflicts. While both use humanitarian aid data from UN's Office for the Coordination of Humanitarian Affairs (OCHA) Financial Tracking System (FTS), Fink and Redaelli (2011) evaluated the difference between the FTS data and the OECD data this paper uses, and concluded that when focusing on rapid onset disasters the differences are relatively minor. However both find that the incidence of a natural disaster, and the number of people affected, increases the probability of a country receiving aid and the amount of aid receieved. A comparison with the coefficients from our results is limited by the econometric methods used in these papers, as both use a two stage analysis involving a gate-keeping equation in the first stage and a probit or logit estimator in the second stage. However, Bermeo (2010) examines the effect of civil conflicts and natural disasters on the log of humantiarian assistance, using all of the same data sources as this paper, and using OLS regressions but uses aid commitments rather than disbursement data and only examines the period from 2002 to 2007. Yet, the coefficient of the author's natural disaster variable is of similar magnitude to the results in this paper. While the coefficient of the conflict variable in the author's paper is approximately a quarter of the coefficient of the conflict variable in our results this

discrepancy can be easily explained as Bermeo (2010) includes all civil conflicts with a minimum of 25 battle related deaths in a year. As this minimum is considerably lower than the level used in our paper, it follows our theory that our coefficient would be larger. These papers therefore support our results that both conflict and natural disasters are significant determinants of the flows of humanitarian aid. Notice also that the R-squared increases quite a bit when we move from Column (I) to (II), indicating that our human and natural disaster variables explain much more of the total variation in humanitarian aid observed in the data. Column (III) adds additional controls that are typically included in aid allocation papers such as total population, recipient income levels and geographic distance. GDP per capita measures are typically included to assess the poverty selectivity of aid allocations (see Knack, Rogers, and Eubank, 2011; Annen and Knack, 2015). Population is often included as empirical studies have shown that the typical donor has a biased towards smaller countries (i.e. Alesina and Dollar, 2000). We find significant relationships between these variables and humanitarian aid, all with the expected sign: Humanitarian aid is poverty selective and the elasticity related to population is less than one, which confirms the bias towards smaller recipients. In addition, recipients located further away from a donor receive significantly less aid. Poverty selectivity can be expected as richer countries are better able to cope with a disaster on their own (Strömberg, 2007). Column (IV) uses the same specification than in Column (III) but uses the full sample. We observe that all the coefficients are quite similar to the ones in the fixed date election sample with the exception of the election dummy, which is substantially smaller and not significant. Column (V) adds covariates to the regression that relate to "politics" as they have been studied previously in the aid allocation literature. These variables include UN voting affinity, democracy, and colonial past. In particular, Alesina and Dollar (2000) have shown that aid allocations of overall aid are strongly affected by these variables.[8] The findings related to these coefficients are different from the findings on aid allocation studies of overall aid. For example, the finding related to Democracy is the opposite than what found in Alesina and Dollar (2000). The reason for this may be that democracy is positively related to government efficacy (Isham, Kaufmann, and Pritchett, 1997) and it is reasonable that higher efficacy means governments may be better able to cope

[8]However, a discussion of how much of the total variation in aid is explained by these variables as opposed to other ones is missing in their analysis.

Table 3: Election Cycles and Humanitarian Aid: Dyadic Panel

Dependent Variable: Humanitarian Aid (Log, in constant USD)	(I)	(II)	(III)	(IV)	(V)	(VI)
Election Dummy	0.44**	0.44**	0.49**	0.10	0.56***	0.19
	(0.15)	(0.15)	(0.19)	(0.16)	(0.13)	(0.15)
Conflict Dummy		6.69***	4.78***	4.52***	4.27***	4.12***
		(0.53)	(0.49)	(0.39)	(0.40)	(0.34)
Natural Disaster		0.15**	-0.01	-0.06	0.03	-0.00
		(0.06)	(0.04)	(0.03)	(0.03)	(0.03)
Disaster Squared		0.02***	0.01***	0.01***	0.01***	0.01***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Total Population			0.81***	0.56***	0.61***	0.43***
			(0.11)	(0.09)	(0.10)	(0.07)
GDP per capita			-1.50***	-1.27***	-1.45***	-1.20***
			(0.17)	(0.14)	(0.17)	(0.14)
Donor-Recipient Distance			-1.07***	-1.31***	-1.23***	-1.43***
			(0.26)	(0.38)	(0.24)	(0.33)
UN Voting Affinity					1.77*	2.46***
					(0.76)	(0.70)
Democracy					-0.35**	-0.26***
					(0.11)	(0.07)
Colony Dummy					-0.17	1.44**
					(0.54)	(0.66)
Immigrants					0.15**	0.08*
					(0.06)	(0.04)
Fixed Election Date Sample	yes	yes	yes	no	yes	no
N	25207	25207	21353	45214	19547	42517
R-squared	0.01	0.17	0.29	0.23	0.30	0.25

All regressions include year fixed effects that are not reported. Cluster-robust standard errors at the donor level are reported in parenthesis. Election dummy equals 1 in the year before an election; Conflict Dummy equals 1 for conflicts with more than 1000 battleground death; Natural Disaster is measured by the log of total people affected by the disaster; Total Population and GDP per capita (measured in constant international dollars) are both in logs; Donor-Recipient Distance measures direct distance between largest cities; UN Voting Affinity is measured by the share of time a recipients votes the same as a donor in the UN General Assembly; Democracy is measured by the inverted 'Civil Liberty' measure from Freedom House; Colony Dummy equals 1 if a recipient-donor pari ever had a colonial relationship; Immigrants is measured by the share of recipient immigrants donor countries. Significance levels : $*$: 10 $**$: 5 percent $***$: 1 percent.

with emergencies on their own making humanitarian aid less necessary. In

Table 4: Election Cycles Channels: Interaction Effects

Election interacted with:	Election Dummy	Interaction-term	R^2 (%)
Conflict Dummy	.52***	.904	30.3
	(.14)	(.615)	
Natural Disaster	.24*	.058***	30.3
	(.13)	(.015)	
Immigrants	.53***	.054	30.3
	(.13)	(.043)	

All regressions use the same specification than the one reported in Column (V) in Table 3 with the exception of the added interaction terms. Cluster-robust standard errors at the donor level are reported in parenthesis. Significance levels : $*$: 10 $**$: 5 percent $***$: 1 percent.

accordance with this hypothesis, we find that when we run the same regression than in (V) but without income per capita, the democracy coefficient becomes more negative (i.e. it decreases to -.52). UN voting affinity has the wrong sign but is significant only at the ten percent level. The same is the case for the colony dummy variable. These estimates seem to suggest that humanitarian aid responds somewhat differently to these political variables as compared to overall aid. We include also the dyadic variable immigrant share, which measures the share of immigrants from a given recipient country residing in a donor country. We would expect that humanitarian aid to recipients with more immigrants should be higher – particularly when a government tries to increase its political support in the home country. Our regression result confirms this prediction. We find a statistically positive relationship between immigrant share and humanitarian aid. The effect there is large as well. A 10 percentage point difference in this share is associated with a 1.5 log point difference in humanitarian aid, which corresponds to a 348% difference. Column (VI) runs the same regression specification than (V) but with the full sample. The results are unchanged except for the colony dummy which now is positive and significant at the 5% level. Thus, we find a positive relationship between colonial past and the level of humanitarian aid, similar than in studies that looked at aid allocations of overall aid.

If a government gains political support by increasing humanitarian aid, one would expect that this government would increase it for the right reasons. Also, one would expect for that rationale to work, media attention is neces-

sary. We have argued that natural and human disasters typically receive a lot of media attention. As mentioned before, the rational of giving more humanitarian aid may not just be about showing compassion and altruism, but also that a government is a relevant player when the "world" faces a crisis. We, therefore, would expect that our election cycle effect should be larger in recipient countries facing a human or natural crisis. Another reason such an effect may exist is that a pending election may amplify the positive (or negative) effect, in terms of voter support, of increasing (or deciding not to increase) humanitarian aid compared to making the same decision in a non-election year. One example of this effect is the negative political fallout that occurred as a result of the attack on the United States embassy in September 2012, two months before the presidential election. Incumbent president Barack Obama was criticized heavily for not responding appropriately to the attacks with republican presidential candidate Mitt Romney arguing that "Mr Obama has damaged ties with key U.S. allies such as Israel, while showing weakness towards Iran and Russia and mishandling the Arab Spring" (Swaine and Irvine, 2012 (accessed June 13, 2015). While such criticism may occur given similar events in a non-election year, there is nevertheless evidence that the weight of criticism is more significant for an incumbent government when an opposition party is trying to sway voters and the media is aware that their coverage of political events is being more closely followed than in non-election years. Thus, in order to insure against the political fallout of not responding adequately to a disaster situation, governments may be inclined to increase support when elections are close.

In order to test this hypothesis, we run the same regressions as before but add an interaction term between our election dummy and conflict. We do the same for natural disasters and our immigrant variable. Table 4 shows the results. We find that the interaction-terms have all the expected sign and for conflicts and natural disasters the effects are large. Humanitarian aid to countries with a conflict increases by 1.424 log points a year before elections, which corresponds to an increase of over 315%. Notice that this coefficient conditional on having an election is statistically significant at the 5% level (Standard Error: 0.56). However, the interaction term itself is not significant, which implies that we do not find a significant difference of humanitarian aid allocations to conflict places between election and non-election years. However, notice that this interaction terms is quite large. In terms of natural disasters, we find again that our regression coefficient conditional on having a disaster is large. It increases in the size of the

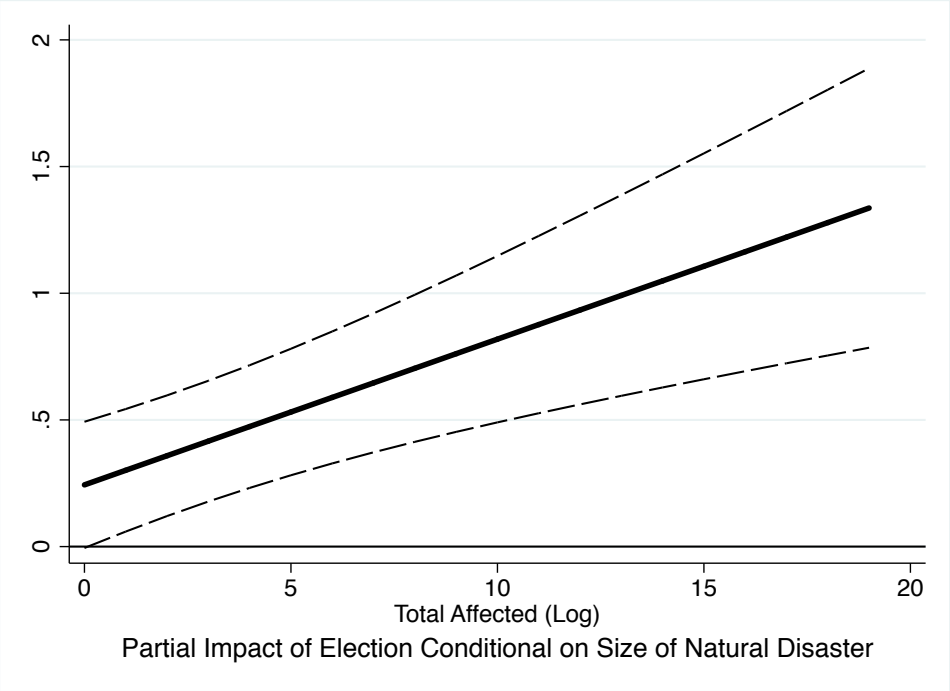

Figure 1: Dashed line shows the 95% confidence interval for the point estimate on the election dummy conditional on the Total Affected by a Natural Disaster.

disaster. Figure 1 shows a plot of the partial impact of our election dummy conditional on our disaster variable.[9] For a natural disaster with medium size in terms of the number of people affected, humanitarian aid goes up by 72% in the year before election. For a natural disaster in the 90th percentile in our data, that number increases to 175%. As Figure 1 shows, those point estimates are significant at the 5% level for any size. For our immigrant variable, we do not find an interaction effect as the interaction term is very small.

We believe that our regression results support our theory of an election cycle effect for the 'right' reasons. Our estimates for the election dummy are large when conditioned on human and natural disasters.

[9]See Brambor, Clark, and Golder (2006) for an insightful discussion on how to interpret interaction-terms.

5 Comparing Donor Motives

Many studies in the aid effectiveness literature have shown that aid is not always given for the right reasons. Aid is often politically motivated (e.g. Alesina and Dollar, 2000; Dreher, Klasen, Vreeland, and Werker, 2013). For example, Alesina and Dollar (2000) find that aid is directed "as much by political and strategic considerations, as by the economic needs and policy performance of the recipients." We want to understand the "as much" more precisely in our context as we have seen that natural and human disasters as well as "politics" are motiving humanitarian aid flows. In order to do so, we run univariate regressions for all these explanatory variables. The results of the univariate regressions give an indication of the relative magnitude of the explanatory power of each variable expressed by the R-squared values in these regressions. We are well aware that such univariate regressions come with a range of econometric concerns, but they can be nevertheless insightful. There is a difference between a marginal effect and the overall explanatory power of a given variable.[10] Table 5 shows the results. While the election variable is statistically significant, we can see that the natural disaster variable has about 10 times and the conflict variable has about 6 times the explanatory power. The same is true for our other "political" variables such as the colony dummy, the UN voting affinity, and immigrants. All these variable have a substantially lower R-squared than the one for human and natural disasters. This suggests that while an election in a donor country does effect the amount of humanitarian aid given to a recipient country, it only explains a smaller part of the overall variation in humanitarian aid observed in the data.

6 Conclusion

This paper bridges the literature on the political determinants of humanitarian aid with the literature on political budget cycles by arguing that donor countries increase the amount of humanitarian assistance to recipient countries in the years prior to an election to increase voter support.The regressions run in this analysis appear to support this theory. First, the amount of humanitarian aid from donors to all recipients increases on av-

[10]For example, Gennaioli, La Porta, Lopez-de Silanes, and Shleifer (2012) use univariate regressions to assess the explanatory power of human capital vs. institutions in regional development.

Table 5: Humanitarian Aid Allocations: Univariate Regressions

	Coefficient	Constant	N	R^2 (%)
Election Dummy	.44***	-10.84***	25207	1.33
	(.15)	(.94)		
Conflict Dummy	8.23***	-11.15***	25207	7.44
	(.58)	(.89)		
Natural Disaster	.15**	-12.18***	25207	12.79
	(.06)	(.88)		
Total Population	1.28***	-30.08***	23476	19.11
	(.14)	(1.8)		
GDP per capita	-1.9***	5.5**	21869	11.72
	(.21)	(2.37)		
UN Voting Affinity	2.29***	-12.12***	20736	3.37
	(.4)	(1.24)		
Immigrants	.48***	-11.08***	24513	3.95
	(.11)	(.85)		
Democracy	-1.28***	-5.63***	22492	12.24
	(.14)	(1.4)		
Colony Dummy	.68	-10.86***	24570	1.32
	(.75)	(.91)		

All regressions include time fixed effects. The regression on Natural Disasters also includes Natural Disasters squared, which is not reported. Cluster-robust standard errors at the donor level are reported in parenthesis. Significance levels : $*$: 10 $**$: 5 percent $***$: 1 percent.

erage by 54% in the year prior to elections. Second this result holds when using a dyadic panel dataset and accounting for the individual dynamics of donor-recipient relationships. Third, these results remain significant when only including donors with exogenous fixed election dates which implies a causal relationship between elections and humanitarian aid though this effect is relatively weak compared to the explanatory power of natural disasters and civil conflicts. Finally, we find evidence of a large interaction effect between elections, natural disasters and civil conflicts. Our results show that humanitarian aid to countries with a conflict increases by 315% and aid to a country with a natural disaster at the 90th percentile in terms of number of affected people increases by 175% with an increase of 72% for a medium size natural disaster. Additionally, we also find no election cycle effect for overall aid (net ODA) which supports our theory that that increasing humanitarian

aid increases voter support in donor countries because of the highly visible nature of humanitarian aid, unlike ODA, which is similar to the types of domestic expenditure that have been shown to increase prior to elections. These results illustrate the need for further examination of the determinats of humanitarian aid. The work of Strömberg (2007) suggests that a fruitful endeavour may be to collect data on the media coverage of natural disasters and civil conflicts from multiple donor countries to determine the effects of media coverage globally as it relates to different kinds of diasters and the flows of humanitarian aid.

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