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Abstract

Natural disasters represent a major challenge for human welfare across the globe. Given the prominent role of international humanitarian aid in alleviating human suffering, the investigation of its determinants is of paramount importance. While existing studies show its allocation to be influenced by donors' foreign policy considerations, domestic political factors within recipient countries have not been systematically explored. This paper addresses this important research gap by investigating whether regional favoritism shapes humanitarian aid flows. Using a rich and unique dataset derived from reports of the Office of US Foreign Disaster Assistance (OFDA), we show that substantially larger amounts of aid are disbursed when natural disasters hit the birth region of the recipient countries' political leader. While we find no evidence that US commercial or political interests affect the size of this home bias, the bias is stronger in countries with a weaker bureaucracy and governance, suggesting the absence of effective safeguards in the allocation of aid.

JEL Classification: H84

Keywords: Humanitarian Aid, Natural Disasters, Regional Favoritism, Birth Regions

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Home bias in humanitarian aid: The role of regional favoritism in the allocation of international disaster relief

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Abstract

Natural disasters represent a major challenge for human welfare across the globe. Given the prominent role of international humanitarian aid in alleviating human suffering, the investigation of its determinants is of paramount importance. While existing studies show its allocation to be influenced by donors' foreign policy considerations, domestic political factors within recipient countries have not been systematically explored. This paper addresses this important research gap by investigating whether regional favoritism shapes humanitarian aid flows. Using a rich and unique dataset derived from reports of the Office of US Foreign Disaster Assistance (OFDA), we show that substantially larger amounts of aid are disbursed when natural disasters hit the birth region of the recipient countries' political leader. While we find no evidence that US commercial or political interests affect the size of this home bias, the bias is stronger in countries with a weaker bureaucracy and governance, suggesting the absence of effective safeguards in the allocation of aid.

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1. Introduction

The principle of impartiality in the allocation of humanitarian aid is firmly established in international law (Persson 2004). In spite of this, anecdotal evidence suggesting that politically important subnational regions receive favorable treatment is easy to find. According to policy reports by the Red Cross and Red Crescent Societies (Klynman et al., 2007) and the International Dalit Solidarity Network (2013), power relations at the community level within recipient countries distort the allocation of humanitarian aid. In this paper, we provide the first systematic investigation of whether and to what extent humanitarian aid is indeed impartial with respect to recipient country politics. We focus on the birth regions of recipient country leaders and investigate whether they are more likely to receive (larger) support when being hit by exogenous rapid-onset natural disasters.

The importance of national leaders' birth regions for the allocation of funds under their control has been demonstrated in previous work, most notably in Hodler and Raschky (2014). Investigating one potential channel, Dreher et al. (2019) show that recipient leaders channel foreign aid to their birth regions to the extent that the donor does not put strings on how these funds are allocated, but not otherwise. We thus consider the focus on birth regions to be a suitable test of the impartiality principle in the allocation of humanitarian aid. Given the direct connection of such assistance with humanitarian suffering, the examination of such political economy factors is of paramount importance.[1]

We examine whether recipient country leaders can channel humanitarian aid in line with their personal interests – with the potential to influence domestic political equilibria – and whether and to what extent commercial and political relations with the donor facilitate their abuse. Specifically, we investigate the allocation of humanitarian aid from the United

[1] Natural disasters constitute a major challenge for human welfare. In the 1964-2017 period, they have reportedly killed more than five million people across the globe (Guha-Sapir et al., 2018). Furthermore, natural disasters exert a substantial negative effect on economic growth (Felbermayr and Gröschl, 2014) and on the human capital (Caruso, 2017) and income (Karbownik and Wray, 2019) of subsequent generations. As climate scientists predict a substantial increase in both the frequency and intensity of natural disasters in the near future, this type of aid is likely to further gain in importance for human welfare. What is more, climate-related risks are projected to be disproportionately concentrated in already vulnerable countries with low response capacities (IPCC, 2018).

States for 6,228 rapid-onset natural disasters that have hit 50 countries over the 1964-2017 period. We derive these rich and unique data on disaster relief from annual reports issued by the Office of US Foreign Disaster Assistance (OFDA) – the US agency responsible for providing disaster relief overseas. OFDA responds to an average of 65 disasters in more than 50 countries per year (USAID, 2018a); the United States have been by far the biggest donor of humanitarian aid. In the 1972-2017 period, they financed more than 40% of the humanitarian assistance of the countries that report to the OECD's Development Assistance Committee (DAC) (OECD, 2019).

We employ three strategies to address endogeneity and identify the causal effects of leaders' birth regions on the allocation of humanitarian aid.[2] First, we control for a range of observable characteristics of the affected subnational area. This type of specification allows us to adjust for the most obvious sources of confounding. However, unobserved omitted variables could potentially still bias estimates. In a second step, we therefore include disaster-area fixed effects, limiting the analysis to identical areas that have been hit by multiple disasters, while having experienced changes in their birth region status over time. In this restrictive setting all time-invariant unobserved heterogeneity is controlled for, allowing us to further increase the internal validity of our estimates.

As a third empirical approach, we run placebo regressions that test whether disasters hitting regions that were the birth region in the previous or subsequent year of the disaster, but not during the time of the disaster itself, receive similar treatment compared to disasters hitting contemporaneous birth regions. In comparing these points in time, any unobserved characteristics of birth regions that do not vary over a very short period are thus accounted for. We have no reason to assume that exogenous disasters should be more likely to receive funding in case they hit the birth region of a national leader, compared to disasters that hit the same region in the years directly before or after the

[2] While the timing as to when a rapid-onset natural disaster hits a particular sub-national region is random, decisions on aid allocation might be endogenous. Sub-national regions connected to the government by virtue of being the political leader's birth region might differ from other regions in ways that are correlated with the need for aid. For instance, it seems plausible that regions with political ties to the government are richer and better protected against the risks arising from natural disasters compared to areas populated by weaker groups. In such a case, our estimate of how regional favoritism affects the allocation of aid could be biased.

leader assumes power. Comparing these years thus allows us to derive a causal estimate of the importance of birth region favoritism in a scenario of urgent humanitarian need.

Our results suggest that birth region-related favoritism exerts a strong and robust influence on humanitarian aid, increasing the *amounts* of US-provided disaster relief by 45% to 85% in our main specifications. In contrast, we do not observe any systematic effects on the *probability* of receiving US-provided disaster relief, which can be plausibly explained by the structure of OFDA's decision-making process. We do not find evidence that the United States' political or commercial interests in a country hit by disaster affect the size of the home bias. To the contrary, recipient-country characteristics in which leaders should find it easier to misappropriate funds – such as clientelism prevailing in public spending or low bureaucratic quality – explain a substantial share of it. What is more, providing a novel and innovative dataset on ethnic power relations, we show that the observed favoritism is not explained by ethnic ties between political leaders and the population in disaster-affected areas, but constitutes a self-sustained, independent dimension of favoritism within recipient countries.

With this paper, we mainly contribute to two strands of literature. First, and most directly, our research connects to a number of studies that investigate the determinants of disaster aid allocation across countries. According to these studies, donor political interests influence humanitarian aid (Ball and Johnson, 1996; Drury et al., 2005; Eisensee and Strömberg, 2007; Strömberg, 2007; Fink and Redaelli, 2011; Fuchs and Klann, 2012; Raschky and Schwindt, 2012; Annen and Strickland, 2017). While some of these previous studies also used individual disasters as unit of observation (rather than country-years), the political economy within *recipient* countries has largely been ignored.[3]

[3] There are two exceptions, both focusing on individual countries rather than a broader sample. One focuses on the head of government's electoral incentives, such as re-election concerns or providing support to the governing party at the national or regional level (Jayne et al., 2002; Francken et al., 2012, Kunze and Schneider, 2019; Eichenauer et al., 2019). The second investigates discrimination against individual victims of disasters. This literature shows that the probability to receive aid depends on gender, race, income, and education, among others (Broussard et al., 2014; Bolin and Kurtz, 2018).

Second, this paper contributes to the literature investigating regional favoritism. Hodler and Raschky (2014) and Dreher et al. (2019) show that country leaders' birth regions experience higher economic growth and receive more Chinese foreign aid, respectively. This literature provides the analytic background for our study. It shows that leaders of countries around the world divert resources to their birth regions, either for altruistic purposes, or political ones. However, given that disaster relief is primarily triggered by large, exogenous shocks with potentially grave humanitarian consequences, such political-economy considerations would even be more alarming, and stand in direct contrast to international law.

More broadly, our results also relate to the aid allocation literature at large. Much of this literature investigates how donor political interests affect the allocation of aid at the country level (Alesina and Dollar, 2000; Kuziemko and Werker, 2006; Hoeffler and Outram, 2011; Vreeland and Dreher, 2014). Only recently, this literature has begun to investigate political motives in the allocation of aid at the sub-national level. However, due to limited data availability, previous work has focused on either single recipient countries, multilateral aid, or the allocation of Chinese development finance.[4] With this analysis, we are thus the first to investigate the effect of regional favoritism in the allocation of a Western bilateral donor for a large number of recipient countries and thus extend the literature in an important dimension. What is more, our focus on exogenous variation in recipient need caused by natural disasters facilitates the identification of causal effects compared to studies focusing on the allocation of aid more broadly.

Finally, we also contribute to the literature on ethnic power relations by providing novel data on the sub-national location of ethnic groups, which we use to separate favoritism towards ethnic regions from those towards birth regions. Unlike previous studies (e.g., De Luca et al. 2018, Anaxagorou et al., 2019), we identify the ethnic composition of sub-national populations based on census and survey data. This is an improvement over previous research, which is mostly based on less precise historical ethno-linguistic or expert-based maps such as *Ethnologue* data (Gordon, 2005), the *Geo-referencing of*

[4] See Francken et al. (2012), Dionne et al. (2013), Jablonski (2014), Masaki (2018), Nunnenkamp et al. (2017), Brazys et al. (2017), Anaxagorou et al. (2019) and Dreher et al. (2019).

Ethnic Regions (*GREG*) data (Weidmann et al., 2010) or *GeoEPR* – a geocoded version of the *Ethnic Power Relations* (*EPR*) dataset (Wucherpfennig et al., 2011).

We proceed as follows. Section 2 introduces our main data sources – on humanitarian aid, natural disasters, leaders' birth regions – as well as our control variables, and discusses the methods used to construct our measures. Section 3 discusses descriptive statistics, while Section 4 explains our method of estimation. We show the main results in Section 5 and our analyses of whether US-commercial and political interests drive the effect in Section 6. Section 7 presents extensions to the main analyses, while the final Section 8 discusses the policy implications of our research.

2. Data sources and variables

Disaster Aid

Three main data sources have previously been used to study the effect of natural disasters on aid. One group of papers relies on data that donor governments report to the OECD's DAC, which includes entries for emergency relief and food aid, among others. While these data have the advantage of being easily available for the major Western donors organized in the DAC, using them comes at a cost. As DAC data do not exclusively focus on disaster relief, but also on crisis prevention, and emergencies other than disasters, such aid cannot directly be attributed to individual disasters (Fink and Redaelli, 2011). Given that it is thus not possible to attribute the aid flows to subnational regions in aid-receiving countries, DAC data are not suitable to address our research questions.

A second set of papers relies on data provided by the Financial Tracking Service (FTS) managed by the UN Office for the Coordination of Humanitarian Affairs (OCHA). One of its main advantages consists of its wider coverage of donor countries. These data cover nearly all donor countries in the world rather than just the set of mainly Western donors that report to the DAC. Contrary to the DAC data – which are at the recipient-year level – the FTS provides aid information for disaster appeals. These data come, however, with the disadvantage that reporting to FTS is voluntary, potentially giving rise to underreporting (Harmer and Cotterrell, 2005; Raschky and Schwindt, 2012). Moreover, parts of the aid reported there cannot be attributed to specific disasters. While previous work making use of FTS data was restricted to the 1992-2004 period (Fink and Radaelli, 2011), the share of contributions not assigned to a specific disaster is substantially higher in more recent years, creating the risk of measurement error for our analysis.[5] Finally, the lack of a common standard by which individual donors report their aid contributions makes it difficult to compare aid across donors. For example, overvalued in-kind contributions could lead to over-reporting of aid (Harmer and Cotterrell, 2005).

The third group of papers focuses on the United States as donor exclusively, using data from the US Office for Foreign Disaster Assistance (OFDA). OFDA is part of the US Agency for International Development (USAID) and is the lead agency responsible for international disaster relief (Drury et al., 2005; Eisensee and Strömberg, 2007; Fink and Radaelli, 2011; Margesson, 2013; Kevlihan et al., 2014).[6] The United States are by far the largest provider of disaster relief, both in terms of the number of supported disasters and regarding the amounts of aid provided. For the 1972-2017 period, the United States financed more than 40% of the humanitarian assistance provided by countries of the DAC. (OECD, 2019). The key advantage of OFDA data compared to the previously mentioned data sources consists of the agency providing detailed and complete annual reports for each fiscal year since 1964, allowing us to match individual contributions to specific disasters without ambiguity. This rather long time frame combined with the reliability and completeness of aid data makes OFDA the most suitable data source for the research question at hand.

We extract aid flows from OFDA's annual reports for the fiscal years 1990 to 2017 and transform them to constant 2017 US Dollars (OFDA, 1990-2017). For the 1964-1989 period, we use data provided to us by Cooper Drury who previously conducted research

[5] According to personal correspondence with FTS staff, one reason for the increase in unassigned contributions is a lack of resources to properly categorize all disasters (email from August 10th, 2017).

[6] The USAID (2005: 10) defines disaster aid as "[i]mmediate, life sustaining assistance provided to disaster victims." It is given based "upon the written determination that a disaster exists in the host country which meets three criteria: it is of a magnitude with which the affected community cannot cope; recognized representatives of the affected population desire the assistance; and it is in the USG's [United States Government's] interests to respond" (USAID, 2005: 5).

based on data from OFDA's annual reports (Drury et al., 2005).[7] Besides OFDA, there are other US agencies that allocate disaster assistance, most notably the Office of Food for Peace (also part of USAID) and the US Department of Defense (Margesson, 2013). Thanks to its chief role for US disaster assistance, OFDA collaborates closely with these agencies and provides approximate numbers for their aid contributions for the sub-period 1964 to 2004.[8] While our main analysis focuses on the full period (1964-2017), we show in a test for robustness that our key results extend to the aid provided by the full range of US agencies using the 1964 to 2004 sub-period.

Natural Disasters

We take data on natural disasters from the EM-DAT international disaster database (Guha-Sapir et al., 2018), which was assembled by the University of Louvain's *Centre of Research on the Epidemiology of Disasters* (*CRED*). This is the most comprehensive list of disasters available, comprising more than 22,000 disasters from the year 1900 to the present. EM-DAT includes disasters that meet at least one of the following criteria: At least ten people are reported as killed, at least one hundred people are reported to be affected, a state of emergency has been declared, or a call for international assistance has been issued. They provide disaster-specific information on the type of disaster, the number of people killed, missing and presumed dead, and the number of people affected (EM-DAT, 2018b).[9] Conveniently, EM-DAT also includes the sub-national location of the disaster for the first administrative levels or lower.[10]

For our analysis, we include the 50 countries that are most frequently affected by rapid-onset disasters (floods, storms, earthquakes, epidemics, landslides, extreme temperatures, volcanic activity, wildfires, dry mass movements and insect infestations)

[7] All annual reports (with the exception of the 1974-1982 period) are available for download from the USAID Development Experience Clearinghouse (USAID, 2018b).
[8] In this time period, OFDA aid flows represent on average 76% of all US disaster relief.
[9] EM-DAT draws from a number of sources, including UN agencies, non-governmental organizations, insurance companies, research institutes and press agencies (EM-DAT, 2018a).
[10] Subnational regions at the first administrative level (ADM1) typically are departments, provinces or states; regions at the second administrative level (ADM2) include districts or municipalities, among others. Whenever a change in the admin boundaries occurred over time, we consider the modern counterparts of reported administrative areas. Note that out of 7,318 disasters, 880 disasters (of which only 60 received OFDA aid) are not considered in the analysis due to imprecise location information. Moreover, disasters in Vietnam during the Vietnam War are excluded.

over the 1964-2017 period.[11] We follow Fink and Redaelli (2011) in limiting the scope of the analysis to rapid-onset disasters, whose timing is rather random and therefore occur unexpectedly. By contrast, the timing and particularly the duration of slowly evolving catastrophes such as droughts, famines or complex emergencies might introduce endogeneity concerns.[12] We merge the EM-DAT list of disasters with disaster-specific aid data from OFDA reports based on the timing and location of disasters. EM-DAT disasters not mentioned in the OFDA reports are assumed to not have received aid.[13]

Leaders' birth regions

To identify birth regions of the countries' political leaders, we make use of the latest version of the Archigos database of leaders such as presidents, prime ministers, or religious leaders, depending on the political system (Goemans et al., 2009; Archigos, 2018). We complement these data with birthplace information acquired through online search. While leader birth regions are often also available at the ADM2 level, we focus on ADM1 birth regions, as EM-DAT does not always provide a comprehensive list of affected ADM2 areas. Our explanatory variable of interest is then constructed as a binary indicator equal to one if any of the disaster-specific locations listed by EM-DAT was the birth region of the political leader at the time of the disaster, and zero otherwise.[14]

[11] Given that we focus on within-country variation in disaster locations, increasing the sample beyond the 50 most frequently affected countries would hardly increase the degrees of freedom. Moreover, the following advanced economies with a high number of natural disasters, which rarely appear in OFDA reports, were not included in the sample: Australia, Canada, France, Hong Kong, Italy, Japan, New Zealand, Russia, South Korea, Spain, Taiwan, and United Kingdom. We exclude Algeria due to a lack of variation in the birth region status of disaster-affected areas and Somalia due to the uncertain governmental control over national territory.

[12] Including disasters that are driven by such dynamics would substantially increase the complexity of the analysis and its interpretation as the United States may adjust its aid depending on how a disaster evolved. For the same reason, we do not include technical disasters.

[13] We exclude observations for which matching was not possible due to ambiguous information on timing, and location and due to ambiguous information on aid flows. As a consequence, 79 disasters remain unmatched and hence drop out from the sample.

[14] For most disasters reported by EM-DAT, start dates and end dates are identical or very close to each other. In the uncommon event where start and end dates differ and leaders change in between, we code the binary indicator as equal to one if any of the leaders' birth regions was hit. We use the same approach for the rare case that only the month and year of the disaster are reported and a leader change happens to occur in the respective month.

Control variables

To address the concern that regions affected by natural disasters may differ from other regions in ways that are correlated with birth region status and aid flows, we construct a range of control variables reflecting local area characteristics. In particular, we suspect that birth regions are richer, more populated (affecting the economic damage and number of casualties created by disasters) and easier to access (facilitating disaster relief). For the construction of control variables for population density, nighttime light intensity, barren land and ruggedness, we match the disaster locations indicated by EM-DAT with grid maps and calculate zonal statistics using a Geographic Information Systems software. For the case of multi-location disasters, we weight locations by their area.[15]

As a measure for population concentration, we use the above procedure to construct a continuous variable for average population density per square kilometer in the year 2015.[16] Moreover, we control for the population size in major cities of affected areas for the entire time period of analysis, based on data from the UN World Urbanization Prospects (UN, 2018). We lag values by one year, rescaled to ten million inhabitants.[17]

For economic activity, we use average annual cloud-free night time light intensity maps in combination with population figures to construct a measure of average night time light intensity per capita. This variable aims to capture economic activity conditional on population density. For that purpose, we use data from the National Oceanic and Atmospheric Administration (NOAA, n.d.). As night time lights are highly volatile across years, we exploit the availability of maps for multiple years (1992 to 2013) and take averages across time before taking averages across administrative regions. This approach brings the advantage of reducing the influence of very cloudy years, in which satellites can only measure night time light emissions during a limited number of days.[18]

[15] To increase precision, we exploit information at the ADM2-level whenever available.
[16] We rescale the variable to 1,000 people per square kilometer. For the vast majority of countries, we use data from the WorldPop project (Tatem, 2017). Where these are unavailable, we use SEDAC (CIESIN, 2017).
[17] The cities included in the dataset are those with more than 300,000 inhabitants in the year 2018.
[18] The fact that we take averages across time brings the disadvantage that we cannot use lagged night time light intensity which would limit endogeneity concerns. However, the required satellite data are unavailable for years before 1992 and we consider measurement error a more severe problem. Moreover, results

Further variables that control for the economic or strategic relevance of the affected areas are the percentage of barren land, the number of ports, the number of nuclear plants and a binary indicator for disasters hitting the capital city. The barren land variable is based on the GlobCover 2009 grid map, which captures the share of land classified as bare land in the disaster-affected areas (Arino et al., 2012). Data for the number of large ports and nuclear plants comes from the World Port Index of the US National Geospatial Intelligence Agency (NGIA, 2017) and the World Nuclear Association (WNA, 2017), respectively.[19]

To proxy for accessibility, we measure ruggedness with the so-called Terrain Ruggedness Index, which was initially developed by Riley et al. (1999) and constitutes a fine-grained measure for average differences in elevation per 30 arc seconds grid cell. We use pre-constructed grid cell-level data by Nunn and Puga (2012) and take averages across grid cells within disaster areas. Similar to them, we scale the index such that it represents average elevation differences in hundreds of meters.

3. Data description

Table 1 shows descriptive statistics by disaster type. The 50 countries included in our sample were hit by 6,228 natural disasters, 13.4% of which received disaster relief from OFDA. On average, 360 persons were reported dead and the average disaster with positive aid flows received approximately 1.78 million 2017 US dollars. Funding shows high variation as indicated by the large standard deviations in parentheses. In rare cases, OFDA donated aid to multiple disasters at once. We treat these cases as one event and assign the disaster type “multiple” if these disasters were of different types.

(available on request) remain close to identical when night light intensity and/or population density are dropped from the analysis.

[19] We use one-year lags for the number of nuclear plants. The number of ports does not vary over time in our sample period.

Table 1. Disaster impact and emergency relief, 50 countries, 1964-2017

Disaster type	Frequency	Average number of casualties (SD)	Percentage funded	Average funding in 1,000s, 2017 US dollars (SD)
Earthquake	761	1,350.1 (13,058.8)	18.4	4,850.8 (28,916.2)
Epidemic	608	194.0 (600.6)	5.9	865.8 (3,666.2)
Extreme temperature	186	134.0 (315.5)	1.6	1,732.6 (2,798.0)
Flood	2554	79.7 (654.5)	13.9	1,047.5 (12,695.0)
Insect infestation	16	0.0 (0.0)	37.5	537.3 (551.8)
Landslide	452	70.6 (185.3)	7.1	137.1 (215.3)
Mass movement (dry)	24	77.3 (86.8)	12.5	46.7 (7.0)
Storm	1371	583.9 (9,749.8)	13.8	1,665.4 (5,220.5)
Volcanic activity	134	183.2 (1,883.0)	22.4	905.7 (1,947.9)
Wildfire	98	8.0 (22.6)	18.4	485.2 (996.6)
Multiple	24	367.3 (397.9)	100.0	1,641.8 (3,048.3)
All disasters	6,228	360.0 (6,495.9)	13.4	1,776.5 (14,720.2)

Notes: Standard deviations in parentheses. Average funding includes only aid provided by OFDA (rather than all US government agencies). We exclude disasters without any funding from this average.

Analyzing the table by disaster type, important differences become visible. First, certain disasters are substantially more frequent than others. For instance, more than half of the sample comprises of storms and flood events, while insect infestations are rare. Disasters belonging to the categories of insect infestation, volcanic activity, earthquake and wildfire are more likely to receive funding. Among those disasters with funding, earthquakes, extreme temperatures and storms receive the larger aid amounts on average. It should be noted however that there is a large variation in the granted aid amounts within most disaster types.

Table 2. Summary statistics by country

Country	Disasters	Average number of casualties	Percentage hit leader's birth region	Percentage funded	Average funding in 1,000s (2017 USD)
Afghanistan	150	137.7	7.3	5.3	509.9
Angola	55	109.3	41.8	9.1	293.6
Argentina	93	11.3	31.2	12.9	169.8
Bangladesh	223	802.4	29.1	4.5	2,413.4
Bolivia	67	25.1	38.8	34.3	232.2
Brazil	175	60.5	17.1	13.7	147.3
Chile	77	36.8	35.1	16.9	1,453.0
China	748	586.0	9.5	4.1	311.9
Colombia	143	212.3	19.6	14.0	771.2
Costa Rica	50	9.3	24.0	52.0	392.3
Cuba	49	4.3	34.7	8.2	519.3
DR Congo	118	89.8	16.1	8.5	923.3
Dom. Rep.	57	31.5	33.3	21.1	816.6
Ecuador	77	108.3	28.6	28.6	621.4
El Salvador	37	94.2	45.9	32.4	2,196.3
Ethiopia	59	44.5	27.1	10.2	173.7
Greece	69	9.0	24.6	13.0	504.9
Guatemala	77	339.2	35.1	16.9	634.7
Haiti	93	2,575.5	47.3	23.7	19,524.0
Honduras	52	232.0	26.9	28.8	687.3
India	535	326.8	8.2	7.9	1,081.4
Indonesia	415	72.3	10.6	12.3	474.6
Iran	159	744.1	6.9	5.0	2,076.1
Kenya	81	56.5	21.0	4.9	1,914.1
Madagascar	56	80.8	44.6	42.9	580.6
Malawi	40	32.5	22.5	22.5	366.4
Malaysia	60	15.6	20.0	11.7	415.8
Mexico	208	77.8	4.8	11.1	746.4
Mozambique	70	59.7	38.6	22.9	1,922.3
Myanmar	55	2,569.9	20.0	21.8	3,638.3
Nepal	83	231.3	38.6	16.9	2,676.2
Nicaragua	52	19.5	23.1	40.4	304.0
Niger	61	149.6	39.3	18.0	156.4
Nigeria	108	249.3	10.2	4.6	147.2
Pakistan	185	2,418.6	17.8	16.2	12,196.3
Panama	46	7.2	34.8	28.3	113.0
Papua N.G.	54	60.5	5.6	18.5	160.5
Peru	131	595.9	21.4	22.1	1,248.6
Philippines	450	123.7	22.0	14.7	1,291.7
Romania	53	48.1	34.0	20.8	2,383.4
South Africa	84	24.0	27.4	7.1	85.6

Table 2. Summary statistics by country (continued)

Country	Disasters	Average number of casualties	Percentage hit leader's birth region	Percentage funded	Average funding in 1,000s (2017 USD)
Sri Lanka	73	55.3	41.1	23.3	456.9
Sudan	75	118.7	18.7	22.7	1,063.0
Tajikistan	57	9.7	43.9	28.1	543.0
Tanzania	70	64.8	8.6	10.0	64.2
Thailand	99	41.6	12.1	15.2	223.0
Turkey	121	277.8	14.0	12.4	3,733.1
Uganda	61	30.6	4.9	11.5	122.4
Venezuela	39	794.7	10.3	17.9	564.5
Vietnam	178	87.6	11.2	14.6	283.4
All disasters	6,228	360.0	18.9	13.4	1,776.5

Notes: Average funding includes only aid provided by OFDA (rather than all US government agencies). We exclude disasters without any funding from this average.

We further provide an overview of descriptive statistics for all countries in Table 2.[20] Overall, there is substantial heterogeneity in the indicators. The number of disasters included in the sample ranges from 37 in El Salvador to 748 in China, while the percentage of funded disasters varies between 4.1% (China) and 52% (Costa Rica).[21] There are further significant differences in the percentages of disasters hitting the leader's birth region, with values ranging from 4.8% (Mexico) to 47.3% (Haiti). Finally, the included disasters are on average deadliest in Myanmar, with a mean of 2,569.9 casualties.

4. Empirical strategy

We estimate a range of regression models to test for birth region-related favoritism in the provision of disaster relief. We follow Fink and Redaelli (2011) and Raschky and Schwindt (2012) by estimating this relationship as a two-step process. The first stage of the aid allocation model captures whether or not a disaster is funded; our dependent variable Y_{ect} is thus binary and takes the value of one if any aid is granted for disaster *e* that took place

[20] We provide descriptive statistics for the control variables in Table A1.

[21] These figures do not involve funding practices of the United States for disaster types that are not included in this analysis. Especially for African countries, complex emergencies and droughts are salient and receive generous support from the United States.

in year *t* in recipient country *c*. In the second stage, the outcome is the amount of OFDA aid that was disbursed for a certain disaster, in logged 2017 US Dollars. Given the structure of the dataset, we estimate regressions at the disaster level rather than at the regional level. As a starting point, our empirical models are thus of the following form:

$$Y_{ect} = \beta_1 birth_{ect} + \beta_2 X_{ect} + \gamma_c + \theta_e + \delta_t + \varepsilon_{ect}. \tag{1}$$

In all specifications, $birth_{ect}$ is a binary indicator that is equal to one if any ADM1 area hit by a disaster is the birth region of the leader governing the country at the time of the disaster, and zero otherwise. The vector of control variables X_{ect} captures local area characteristics as discussed in Section 2. Note that while some of the controls are constructed with time-invariant input data, they still vary between country-specific disasters to the extent that disasters hit different areas. Moreover, we control for fixed effects on the country (γ_c), disaster-type (θ_e) and year (δ_t) levels. In a further specification, we add the number of casualties reported by EM-DAT as a proxy for disaster magnitude. This variable should be interpreted with caution, as even though we are limiting the focus to rapid-onset disasters, the number of casualties might arguably be endogenous to disaster aid.[22] We cluster standard errors at the country level.

In a more restrictive model, we then replace all control variables by disaster-area fixed effects, thus restricting the analysis to disasters that hit the same ADM1 areas with alternating birth region status over time. By comparing disasters hitting the exact same ADM1 areas, this specification brings the major advantage of allowing us to control for all factors that do not vary at the ADM1-level over time. That being said, the inclusion of restrictive fixed effects can reduce the signal-to-noise ratio, making results more sensitive to individual observations.

[22] Given the potential endogeneity of disaster magnitude, we do not use this variable in Sections 6 and 7. Results are however highly similar and are available on request.

As a third identification strategy, we therefore introduce a placebo model with an alternative exposure variable (henceforth "placebo") taking the value of one for disasters that hit regions that were birth regions in the previous or subsequent period but not during the disaster, and zero otherwise.[23] We thereby exploit a discontinuity introduced by leader changes. While new leaders may have been born in a different region than their predecessors, introducing a sudden change in the birth region status of affected regions, changes in unobserved confounding variables are unlikely to simultaneously exhibit discontinuities even if they vary over time. Regions that are the birthplace of a leader who just lost power or will gain power in the very near future should therefore exhibit the same underlying traits as the contemporaneous leader's birth region. The model is hence able to control for both time-invariant and time-variant unobserved heterogeneity.[24] A statistically significant coefficient for the placebo would thus indicate that our identification strategy might suffer from omitted variables bias. Differences between our variable of interest and the placebo can be understood as the bias-corrected net effect of disasters hitting the birth region of the current leader.[25]

5. Main results

We begin by examining whether disasters hitting the birth region of the country's leader have a higher probability of receiving humanitarian aid than disasters hitting non-birth regions. In Table 3, the dependent variable in all columns takes the value of one if the disaster received any aid and zero otherwise.

[23] We define previous and subsequent periods as one year before the start date and one year after the end date of disasters, respectively. For observations with missing days or days and months, we assumed the 15th of each month and the 1st of July as start and end dates, respectively.
[24] This placebo model is particularly powerful in the context of rapid-onset natural disasters, given their random timing.
[25] This interpretation abstracts from the possibility that leaders who just lost or are about to gain de jure power may already/still hold a certain degree of de facto power. Therefore, the placebo model, strictly speaking, provides a falsification test, as, with sufficient power, a lack of significance for the placebo coefficient rules out the discussed form of omitted variable bias but a significant coefficient does not prove with full certainty that omitted variable bias is present.

Table 3. Birth region effect on the probability to receive humanitarian aid

	(1) *Any funding*	(2) *Any funding*	(3) *Any funding*	(4) *Any funding*	(5) *Any funding*
Birth region	0.050***	0.022	0.026*	0.027**	-0.031
	(0.015)	(0.014)	(0.013)	(0.013)	(0.030)
Economic/strategic importance					
% barren land		0.000	0.001*	0.001*	
		(0.000)	(0.000)	(0.000)	
# ports		0.032**	0.022*	0.022*	
		(0.013)	(0.011)	(0.011)	
# nuclear plants		-0.006***	-0.003*	-0.003*	
		(0.002)	(0.002)	(0.001)	
Capital city		0.044***	0.048***	0.046***	
		(0.013)	(0.014)	(0.014)	
Nightlight p.c.		-0.070*	-0.029	-0.028	
		(0.041)	(0.047)	(0.047)	
Population					
Pop. density		-0.005**	-0.007***	-0.006***	
		(0.002)	(0.002)	(0.002)	
City population		0.005***	0.011***	0.011***	
		(0.002)	(0.002)	(0.002)	
Accessibility					
Ruggedness		-0.008*	-0.003	-0.002	
		(0.004)	(0.004)	(0.004)	
Magnitude					
# deaths (in 1,000s)				0.004***	
				(0.001)	
Fixed effects					
Country	X	X	X	X	
Disaster type		X	X	X	
Year			X	X	
Disaster area					X
Observations	6,228	6,228	6,228	6,228	6,228
R^2	0.067	0.114	0.183	0.188	0.618

Notes: ***, **, * denote statistical significance at the 1, 5 and 10% level respectively, with clustered standard errors in parentheses. The sample consists of 50 countries over the 1964-2017 period. The dependent variable is a binary indicator taking the value of one if the disaster was granted any funding and zero otherwise. The variable birth region takes the value of one for disasters that hit the birth region of the country leader and zero otherwise.

In column 1, which adjusts for country-level fixed effects only, we find that a disaster hitting the leader's birth region significantly increases the probability to receive aid by five percentage points. However, this association turns insignificant after controlling for local area characteristics related to economic/strategic importance, population, and accessibility as well as for disaster-type fixed effects (column 2).[26] The birth region coefficient regains statistical significance once year fixed effects (column 3) and disaster magnitude (column 4) are accounted for, and reaches a magnitude of 2.6 and 2.7 percentage points, respectively. Finally, in column 5, we apply our most conservative model using disaster-area fixed effects.[27] In this specification, the coefficient is not statistically significant and turns negative. In summary, our estimates show that there is no robust evidence for systematic favoritism in the probability to receive any aid.

We next turn to the birth region effect on aid amounts in logged 2017 US-Dollars. This analysis draws upon the reduced sample of disasters with positive aid flows, which covers all 50 countries and 13.4% of all disasters in our dataset. Following the same structure as Table 3, Table 4 shows that disasters hitting leaders' birth regions receive substantially higher aid amounts than other disasters. Estimates are robust to controlling for local area characteristics, disaster-type fixed effects, year fixed effects, and disaster magnitude (columns 2 to 4). Across these specifications, the statistically significant increase in funding is of approximately 45%-85%, on average.[28] For the average disaster's funding in our sample, this corresponds to an increase by US$849,984 to US$1,506,135.

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[26] The size of the population residing in cities, the number of ports and the fact that a disaster hit a nation's capital city are positively associated with the probability to receive disaster assistance across specifications. In contrast, countries are less likely to receive humanitarian aid the higher the population density and the more nuclear plants are located in disaster-affected areas. While the control variables condition on each other and may not represent causal estimates, these results suggest that nuclear plants may introduce a different geopolitical dynamic and it may not always be in the interest of the United States to extend aid if these are located in disaster areas.

[27] Given that this model reduces the variation of interest to a much narrower set of disasters, we do not include year fixed effects or other controls in order to maintain a sufficient number of degrees of freedom.

[28] Given that the outcome is logged US Dollars, the marginal effect for the binary birth region indicator expressed as percentage change in funding can be calculated as $\frac{Y(1)-Y(0)}{Y(0)} = e^{\beta_1} - 1$, where $Y(1)$ and $Y(0)$ represent the outcome with the indicator switched on and off, respectively, and β_1 being the estimated coefficient. A potential concern relates to the second stage of the aid allocation decision being subject to selection bias given that the aid amounts regression is estimated on a subsample of regions hit by disasters. As we lack a clear candidate for an excludable instrument for the first stage, the performance of the conventionally used Heckman Selection Model is limited. When we estimate a Heckman Selection Model without exclusion restriction most results are however unchanged (results available on request).

Table 4. Birth region effect on amount of received aid

	(1) *Log funding*	(2) *Log funding*	(3) *Log funding*	(4) *Log funding*	(5) *Log funding*
Birth region	0.614***	0.501***	0.368**	0.391**	0.941[*]
	(0.156)	(0.155)	(0.149)	(0.147)	(1.104)
					[0.057]
Economic/strategic importance					
% barren land		0.010**	0.005	0.009***	
		(0.004)	(0.004)	(0.003)	
# ports		-0.353**	-0.162	-0.146	
		(0.175)	(0.151)	(0.155)	
# nuclear plants		-0.190***	-0.195***	-0.164***	
		(0.057)	(0.054)	(0.060)	
Capital city		0.169	0.017	-0.003	
		(0.155)	(0.152)	(0.149)	
Nightlight p.c.		0.143	1.307	1.162	
		(3.924)	(3.716)	(3.607)	
Population					
Pop. density		0.006	0.006	0.006	
		(0.032)	(0.044)	(0.044)	
City population		0.314***	0.267***	0.235***	
		(0.049)	(0.046)	(0.049)	
Accessibility					
Ruggedness		-0.083	-0.116**	-0.101*	
		(0.055)	(0.053)	(0.053)	
Magnitude					
# deaths (in 1,000s)				0.024***	
				(0.004)	
Fixed effects					
Country	X	X	X	X	
Disaster type		X	X	X	
Year			X	X	
Disaster area					X
Observations	836	836	836	836	836
R^2	0.151	0.227	0.343	0.381	0.881

Notes: ***, **, * denote statistical significance at the 1, 5 and 10% level respectively. Stars in brackets denote statistical significance based on clustered wild bootstrapping. Clustered standard errors in parentheses. Column 5 displays p-value based on clustered wild bootstrapping in brackets. The sample consists of 50 countries over the time period 1964-2017. The dependent variable is the disaster-specific log funding in 2017 US dollars, conditioning on having received any funding. The variable birth region takes the value of one for disasters that hit the birth region of the country leader and zero otherwise.

While these specifications already acknowledge important confounders, substantially reducing the scope of alternative interpretations for the birth region coefficient, they control for the heterogeneity of disaster-affected areas imperfectly.[29] In Column 5, we therefore introduce disaster-area fixed effects, which fully absorb all time-invariant heterogeneity

[29] As before, we find – consistently across specifications – that the number of nuclear plants is negatively associated with funding, whereas the size of the population residing in cities increases aid.

between disaster-affected areas. Note that in this restrictive model, the number of countries contributing to the point estimate is reduced to a subset of 14 countries that were affected by disasters hitting the exact same ADM1 areas multiple times with alternating birth region status. As can be seen, the coefficient is not statistically significant due to a substantial loss in precision. However, clustered standard errors are not appropriate in regressions with such a small number of clusters. For this reason, we also report a p-value based on clustered wild bootstrapping, which are more suitable for this setting (see Roodman et al., 2019). As can be seen, the birth region effect is significant at the ten percent level and increases to 156%. Interestingly, countries exhibiting this type of variation score worse in terms of governance indicators that are arguably associated with higher degrees of favoritism, likely driving the observed increase in effect size.[30] Taken together, Table 4 lends strong support for the existence of substantial favoritism.

Tables 3 and 4 suggest the absence of birth region favoritism at the extensive margin but a strong presence of it at the intensive margin. This combined result may be rooted in the decision-making process within OFDA. As discussed in more detail in Section 6, US ambassadors have the authority to grant small amounts of aid on behalf of OFDA as an initial response to disasters. This type of decision is made quickly after disaster onset, when the full geographic spread of a disaster may not yet be fully understood. Hence, it may not leave much room for complex strategic considerations that would be reflected in regional favoritism. In cases where the ambassador's rapid-response assistance is deemed insufficient, more detailed impact assessment and coordination take place to determine further steps, creating substantial room for the formation of favoritism.

We next perform a series of placebo tests in Table 5 that exploit discontinuities of birth region status over time and the random timing of rapid-onset disasters to further ease endogeneity concerns. We reproduce columns 1 and 2 from Tables 3 and 4 with the addition of the placebo indicator.[31] Results confirm the absence of systematic favoritism

[30] The indicators we investigated are extent of clientelism in public spending, the quality of the local bureaucracy, and government accountability. A more detailed discussion of these indicators is provided in Section 6.
[31] Since this analysis exploits variation in the timing of disasters, we do not include year fixed effects to ensure sufficient power.

at the extensive margin. The placebo indicator is not only positive and significant but also exceeds the birth region indicator in size in both specifications. In stark contrast to this, the placebo estimates in the aid amounts regressions are negative and insignificant, while the birth region coefficients themselves remain positive and highly significant. This is further evidence that birth region effects are attributed to the political power of affected areas, rather than being driven by unobserved inherent characteristics of birth regions.

Table 5. Placebo models

	(1) *Any funding*	(2) *Any funding*	(3) *Log funding*	(4) *Log funding*
Birth region	0.053***	0.024*	0.608***	0.480***
	(0.015)	(0.014)	(0.158)	(0.155)
Placebo	0.077***	0.063**	-0.073	-0.157
	(0.028)	(0.029)	(0.250)	(0.278)
Controls		X		X
Fixed effects				
Country	X	X	X	X
Disaster type		X		X
Bias-corrected p-value	n/a	n/a	0.0001	0.0002
Direct test (p-value)	n/a	n/a	0.0159	0.0373
Observations	6,161	6,161	836	836
R^2	0.068	0.115	0.151	0.227

Notes: ***, **, * denote statistical significance at the 1, 5 and 10% level respectively, with clustered standard errors in parentheses. The table replicates columns 1 and 2 from Tables 3 and 4 with the addition of the placebo indicator. The underlying test statistics for the bias-corrected p-values are calculated by subtracting the placebo coefficient from the birth region coefficient and dividing by the standard error of the birth region coefficient. The direct tests consider the Null hypothesis that the birth region and placebo coefficients are equal.

To further substantiate this point, we conduct two formal tests. First, we divide the difference between the birth region and placebo coefficients by the standard error of the birth region estimate in order to test whether the bias-adjusted birth region coefficient remains statistically significant. As shown by the bias-corrected p-values at the bottom of Table 5, this is clearly the case (with p-values < 1%). A potential problem with this approach is that the placebo estimates may suffer from a lack of power and only happen to be close to zero. We therefore conduct a second more conservative test, in which we directly test whether the birth region and placebo estimates are significantly different from each other. The results are reassuring. This more conservative test also yields statistically significant results (with p-values <5%). Therefore, the placebo analysis suggests that our results cannot be explained by characteristics inherent to potential birth regions.

Taken together, the previous analyses thus demonstrate that there is strong evidence in line with regional favoritism at the intensive margin while we do not find systematic evidence for the decision of whether or not to grant aid in the first place. Before discussing further extensions to the main regression models, we proceed with analyzing potential factors facilitating the observed relationship.

6. Do US-interests facilitate the home bias?

In this section we test whether the birth region-effect we have uncovered is actively supported by the United States or is exclusively the result of recipient country politics, in tandem with the absence of sufficient safeguards to prevent such favoritism. To discern how regional favoritism can enter the decision process on humanitarian aid amounts, it is useful to first review how the OFDA response to disasters unfolds. As mentioned earlier, the disaster response usually begins with a US diplomat, either a US Ambassador, Chief of Mission or US Assistant Secretary of State, responding to an assistance request from the recipient country's government.[32] This occurs when the disaster magnitude is declared to exceed the response capacity of affected countries. Afterwards, usually within 24 hours, the US diplomat can allocate a limited amount of up to US$ 50,000 (OFDA, 2010; Margesson, 2013, Kevlihan et al., 2014).[33]

OFDA officials coordinate with the government of the affected countries to determine whether and how much additional aid should be granted. Importantly, while the level of coordination can vary, the governments of affected countries are usually involved in the needs assessments by OFDA, which serve as a basis for determining additional aid amounts. The involvement of recipient countries can be either passive, by providing information on the disaster damage, or active, by participating in joint assessments with

[32] Alternatively, recipient countries can also accept an offer from US representatives.
[33] Before 2003, the limited amount was US$ 25,000 (Kevlihan et al., 2014).

OFDA teams. The OFDA teams deployed to the affected areas to carry out these assessments vary in size. For instance, a five-person team was sent to affected areas by typhoon Megi in the Philippines in 2010. The team's objective was to identify humanitarian needs together with officials of the Government of the Philippines, among others. The assessment was later used as a basis to allocate additional funds amounting to US$ 1.1 million (OFDA, 2011). For the case of large-scale disasters, bigger elite missions of humanitarian experts and technical advisors are deployed to the affected areas.[34] For example, the team for Hurricane Matthew affecting Haiti, Jamaica and the Bahamas in 2016 surpassed 70 members at its peak. Moreover, OFDA can grant additional aid without the deployment of teams to the affected areas, in which case information provided by local governments further gains in importance (OFDA, 2008-2017; Margesson, 2013, Kevlihan et al., 2014).

The involvement of the government in damage assessments, as well as the difficulties in verifying all governmental information, could enable country leaders to favor disasters hitting their birth regions in multiple ways. First, assessments might become more accurate for such disasters, improving their quality and increasing the likelihood of higher aid amounts, while no such care might be taken when non-birth regions are hit. Second, both the number of casualties and physical damage could be intentionally magnified for disasters hitting birth regions. Third, if non-birth regions are more likely to be politically misaligned with the country leader, disasters affecting those regions could be intentionally underplayed, or governments could hinder the entry of humanitarian aid to the country.[35]

As our setting puts the relationship between the population in disaster-affected areas and the country leader at the center of the analysis, it is natural to argue that leader's interests are at work. Following this logic, leaders might favor their birth regions for two reasons. They might simply derive utility from supporting a community they identify with, or attempt

[34] In the US fiscal year of 2017, a record of 6 out of 53 disasters with OFDA funding required such missions, referred to as disaster assistance response teams (DARTs, see OFDA, 2017).
[35] For instance, the government of Burma was criticized for blocking and delaying the entry of humanitarian aid for the damages caused by cyclone Nargis in 2008 (Martin and Margesson, 2008; OFDA, 2008).

to ensure electoral and political support from their stronghold (Hodler and Raschky, 2014; Do et al., 2017; Anaxagorou et al., 2019; Dreher et al., 2019).

Nevertheless, the political and economic interests of the United States might also be at play and facilitate additional funding for disasters affecting the birth region of leaders. In this section, we focus on two dimensions that we expect to increase the US government's stakes in a recipient country. The first is geopolitical alignment, which is often proxied with a country's voting behavior in the United Nations General Assembly (e.g., Fuchs and Klann, 2013). Using this proxy, Faye and Niehaus (2012) show that the political interests of donor governments distort the allocation of aid towards their allies in election years, with the aim to help such allies stay in power.[36] Requests from leaders aligned with the United States might then be more likely to be approved. In order to test whether the United States selectively allocates its humanitarian aid towards its allies, we interact our birth region-indicator with a (lagged) measure for the share of votes in which the disaster-affected countries were in agreement with the United States in the UNGA.[37]

The second dimension we focus on is a recipient country's commercial relations to the United States. A substantial literature explores how trade can be used to punish or reward countries (Berger et al., 2013; Fuchs and Klann, 2013), potentially with the aim to harm or support incumbent governments. We capture economic ties by using dyadic trade data from the Correlates of War project (Barbieri et al., 2009; Barbieri and Keshk, 2016) and calculate the sum of (lagged) US imports from and exports to disaster-affected countries both as shares of US GDP and in absolute terms. Again, to the extent that the US government grants humanitarian aid with the aim to support the incumbent of the recipient country in mind, more aid should be granted to birth regions if the recipient's trade ties with the US are stronger.

[36] Also see Rommel and Schaudt (2017).

[37] We take UN voting data from Voeten (2013), relying on the fraction of votes for which the United States was in agreement with the respective recipient country (counting abstentions as half-agreement).

Table 6. Birth region effect and US interests

	(1) *Log funding*	(2) *Log funding*	(3) *Log funding*
Birth region	0.220	0.419**	0.424**
	(0.364)	(0.171)	(0.168)
Birth region X UNGA overlap	0.516		
	(0.988)		
Birth region X trade/GDP		-1.059	
		(0.690)	
Birth region X trade (absolute)			-0.009
			(0.005)
Controls	X	X	X
Fixed effects			
Country	X	X	X
Disaster type	X	X	X
Year	X	X	X
Observations	806	823	823
R^2	0.339	0.335	0.335

Notes: ***, **, * denote statistical significance at the 1, 5 and 10% level respectively, with clustered standard errors in parentheses. The dependent variable is the disaster-specific log funding in 2017 US dollars, conditioning on having received any funding. The variable birth region takes the value of one for disasters that hit the birth region of the country leader and zero otherwise. Absolute trade values are scaled to 2017 Billion US dollars. The main effects of the interacted variables are included in the regressions.

We show the results in Table 6. As can be seen, neither political nor commercial motives seem to be at play. Column 1 shows no evidence to support the idea of political interests exacerbating or ameliorating regional favoritism. According to columns 2 and 3, trade with the United States does not significantly affect the degree of favoritism in humanitarian aid, suggesting that US economic interests do not influence the observed effect. Overall, the results thus show no evidence in support of the hypothesis that the United States uses its humanitarian aid to facility requests from closer allies over those from more distant countries, in terms of geo-political and commercial relations.

In order to further test the potential role of the United States in the birth region-effect, we focus on a number of variables that we expect to affect how the US evaluates appeals. According to Eisensee and Strömberg (2007), the US government is particularly keen to support countries hit by disasters when they receive more media attention in the US, arguably because it pleases their own support base. We thus focus on US media attention to natural disasters, exploiting information stored in the Vanderbilt Television News Archive (n.d.), and create a binary indicator capturing whether a disaster was featured at least once during the evening news of the ABC news network.[38] When media attention is larger the US government will find it harder to support its allies. At the same time, media attention might increase US scrutiny for favoritism-based requests from the recipient, which would also reduce the home bias-effect.

We similarly expect the US government to be more skeptical of politically charged requests for help in countries with pronounced clientelism, low bureaucratic quality, and weak governance, as these are arguably those environments where the abuse of funds is most likely. We also test whether the US is more attentive at (recipient country) election time, as it is in such years that recipient country leaders might be most keen to misrepresent need and channel additional funds to their birth region (Anaxagorou, et al. 2019; Dreher et al., 2019).

For this exercise, we use a continuous index from the Variety of Democracy (V-Dem) project, in which higher scores stand for a lower degree of clientelism in government spending (i.e., a larger fraction of social and infrastructure expenditures are public goods rather than favoring particularistic interests). This indicator is based on country expert assessments and covers all countries and years in our sample (Coppedge et al., 2018). Second, we make use of two measures for quality of bureaucracy and democratic accountability obtained from the International Country Risk Guide/ICRG (ICRG, 2013). The former captures the institutional strength and autonomy of local bureaucracies on a scale from 0 to 4, with higher values indicating that the bureaucracy is able to govern without drastic policy changes or service interruptions (for instance, thanks to established staff recruiting systems and administrative procedures). The latter measure ranks countries on a scale from 0 to 6 taking into consideration the degree of political competition as well as checks and balances. Again, higher values indicate higher quality. Unlike the V-Dem data, the ICRG measures are not based on expert judgement but follow

[38] Vanderbilt Television News Archive only began recording on August 5th, 1968, leading to a small loss in the number of observations. We focus on ABC, as other major news networks such as CNN or Fox News started operating at a later date and would hence be unavailable for a large number of disasters. Matching of news reports and disasters is done on the basis of disaster dates as well as news content summaries reported in the Vanderbilt Television News Archive.

predetermined coding rules (see ICRG, 2013 for details).[39] Finally, we make use of election data from Cruz et al. (2018) to estimate our models interacted with election indicators. We construct election indicators that take the value of one if an executive, legislative or any of the two type of elections took place within five months after the disaster month.[40]

Table 7. Birth region effect and factors influencing US evaluation of appeals

	(1) *Log funding*	(2) *Log funding*	(3) *Log funding*	(4) *Log funding*	(5) *Log funding*	(6) *Log funding*	(7) *Log funding*
Birth region	0.357**	0.412***	1.402***	1.204**	0.350**	0.390**	0.365**
	(0.159)	(0.146)	(0.290)	(0.532)	(0.149)	(0.147)	(0.148)
Birth region X Media	-0.121						
	(0.256)						
Birth region X Particularistic		-0.261*					
		(0.144)					
Birth region X Bureaucracy			-0.619***				
			(0.161)				
Birth region X Accountability				-0.215*			
				(0.118)			
Birth region X Ex. elections					0.668		
					(0.610)		
Birth region X Leg. elections						-0.288	
						(0.451)	
Birth region X Elections							0.146
							(0.475)
Controls	X	X	X	X	X	X	X
Fixed effects							
Country	X	X	X	X	X	X	X
Disaster type	X	X	X	X	X	X	X
Year	X	X	X	X	X	X	X
Observations	761	836	551	551	835	835	835
R^2	0.402	0.348	0.389	0.378	0.345	0.344	0.344

Notes: ***, **, * denote statistical significance at the 1, 5 and 10% level respectively, with clustered standard errors in parentheses. The dependent variable is the disaster-specific log funding in 2017 US dollars, conditioning on having received any funding. The variable birth region takes the value of one for disasters that hit the birth region of the country leader and zero otherwise. The main effects of the interacted variables are included in the regressions.

[39] A drawback of the ICRG data is that they are only available to us for the sub-period 1985 to 2012 and fully unavailable for Afghanistan, Nepal and Tajikistan.

[40] Results are unchanged when we focus on 11 months after the disaster occurs.

Table 7 reports the additional results. According to column 1, US media attention does not significantly moderate the degree of regional favoritism. As shown in column 2, countries with a higher fraction of public relative to particularistic spending behavior exhibit less regional favoritism. Moreover, both a better quality of the local bureaucracy (column 3) as well as higher government accountability (column 4) are significantly associated with less regional favoritism. Columns 5 to 7 show that none of the interactions with election indicators is statistically significant. Taken together, the evidence is thus not consistent with a storyline in which the United States carefully polices its funds with the goal to prevent potential abuse for domestic political purposes. Quite the contrary, it seems that recipient country leaders prone to clientelism and with weak bureaucracy and governance channel higher amounts of aid to disasters hitting their birth regions.

Our results also speak to potential motives of recipient country leaders. Conceptually, altruistic, electoral or political reasons more broadly might drive the home bias that we detect. A detailed test of these different channels is beyond the means of this paper. However, our results show that short-term electoral motives do not seem to dominate, as favoritism should then become stronger in the run-up for national elections in disaster-affected countries. As we do not find evidence for electoral motives behind the observed birth region effect, favoritism is more likely to reflect leaders' intrinsic desire to assist their own birth regions.[41] We find the home bias to be stronger rather than weaker in countries with stronger clientelism and weaker bureaucracy as well as governance, which are arguably environments that give domestic political leaders more leeway in pursuing their own interests. To the contrary, we have no reason to assume that the US government would want to channel more aid to the birth regions of the leaders of such countries. In summary, it thus seems that OFDA either lacks the motive or the means to prevent recipient governments from favoring birth regions, but does not actively support the misallocation of funds.

[41] This interpretation matches evidence from Vietnam according to which politicians channel public resources to their hometowns due to social preferences rather than expected political gains (Do et al., 2017).

7. Extensions

Total US disaster assistance

As previously discussed in Section 2, OFDA is the chief agency for US disaster assistance but there are other USAID offices (e.g., Office of Food for Peace) or ministries that also contribute humanitarian aid for certain disasters. Given that OFDA provides approximate numbers for these contributions for the 1964 to 2004 sub-period, we are able to formally test whether the previously obtained results are representative for the United States at large, or pertain to OFDA only.[42] In Table 8, we replicate columns 1 to 3 of Table 4, using total US government contributions as reported by OFDA. To facilitate comparison, we also show regressions with OFDA funding restricted to the same sub-period. While the results for OFDA remain similar to those obtained with the full sample, point estimates increase in magnitude when we include all contributions, suggesting that favoritism might become more salient when multiple US agencies are involved.[43] Given these estimates, we interpret the key findings of this study to be representative for US humanitarian aid at large.

Table 8. Total US government contributions, Log funding

	(1) *All*	(2) *All*	(3) *All*	(4) *OFDA*	(5) *OFDA*	(6) *OFDA*
Birth region	0.755***	0.758***	0.713***	0.459***	0.489***	0.419**
	(0.198)	(0.230)	(0.243)	(0.141)	(0.164)	(0.170)
Controls (local area characteristics)		X	X		X	X
Fixed effects						
Country	X	X	X	X	X	X
Disaster type		X	X		X	X
Year			X			X
Observations	633	633	633	587	587	587
R^2	0.171	0.241	0.318	0.149	0.235	0.313

Notes: ***, **, * denote statistical significance at the 1, 5 and 10% level respectively, with clustered standard errors in parentheses. The table replicates columns 1 to 3 from Table 4 using all US funding in columns 1 to 3 and OFDA only in columns 4 to 6.

[42] Due to the shorter time period, within-country variation is reduced to the extent that we refrain from estimating disaster-area fixed effects models using total US government contributions.

[43] A potential reason for this could be that procedures become more complex, making it easier for a recipient country's political leaders to favor their regions.

Ethnic favoritism

A potential explanation for our main results relates to the overlap of birth regions with ethnic homelands of country leaders. As such, the estimated birth region effects might be driven by ethnicity rather than birth region. To test this possibility, we exploit variation in the ethnic identity of the population in disaster-affected areas and generate an alternative measure of favoritism based on the ethnic identity of the political leader.[44]

To determine the geographic spread of ethnic settlement patterns, the political-economy literature on ethnicity heavily relies on ethno-linguistic or expert-based maps, such as *GREG* (Weidmann et al., 2010), Ethnologue (Gordon, 2005), and *GeoEPR* (Wucherpfennig et al., 2011).[45] In addition to being based on potentially outdated, one-dimensional, or incomplete information, these approaches come with the major disadvantage of failing to capture the nuanced realities of actual settlement patterns.[46] Therefore, this paper follows a novel approach of using IPUMS census data and DHS data.[47] IPUMS-International provides microdata for 98 countries. It currently covers more than one billion people in 443 censuses. The data are consistent across countries and over time and represent the largest available archive of publicly available census data (MEASURES DHS, 2017; Minnesota Population Center, 2017, 2019).

[44] For the identification of leader ethnicities, we augment the Archigos database with leader ethnicity information acquired from Fearon et al. (2007), Parks (2014) and through online search.
[45] See, for instance, Alesina et al. (2016), Guariso and Rogall (2017), and Anaxagorou et al. (2019).
[46] For instance, according to Weidmann et al. (2010), details on sources, definitions, and coding conventions of the *Atlas Narodov Mira* which serves as a base for GREG are not documented. Weidmann et al. (2010) however infer the coding criteria by comparing sub-samples with data on ethnicities from other sources. They conclude that the distinction between groups within countries is mainly based on language. This ignores important differences between ethnic groups. For example, the Sunni-Shi'ite division in Iraq is ignored, as is those between the Hutus and Tutsis in Rwanda, even though these are among the most important cleavages in their countries (Wucherpfennig et al., 2011).
[47] Weidmann et al. (2010: 492) mention the possibility to "infer the location of ethnic groups from survey or census data." According to Weidmann et al. (2010: 492) "providing spatially referenced census data for a larger set of cases is not possible." We disagree and do exactly this (see Gershman and Rivera, 2018, for a similar approach to coding the ethno*linguistic* composition of sub-national regions in Sub-Saharan Africa).

Table 9. Birth region and ethnic homelands

	(1) *Log funding*	(2) *Log funding*	(3) *Log funding*	(4) *Log funding*	(5) *Log funding*	(6) *Log funding*
Birth region	0.492***	0.410**	0.362*	0.539***	0.431***	0.373**
	(0.159)	(0.161)	(0.180)	(0.149)	(0.150)	(0.163)
Ethnic homeland	0.117	0.085	0.069			
	(0.225)	(0.244)	(0.256)			
Ethnic share				-0.046	0.062	0.238
				(0.303)	(0.341)	(0.387)
Controls		X	X		X	X
Fixed effects						
Country	X	X	X	X	X	X
Disaster type		X	X		X	X
Year			X			X
Observations	673	673	673	673	673	673
R^2	0.137	0.224	0.339	0.136	0.223	0.339

Notes: Table replicates columns 1 to 3 from Table 4. ***, **, * denote statistical significance at the 1, 5 and 10% level respectively, with clustered standard errors in parentheses.

We exploit the precise estimates provided by the census data to define ADM1-level ethnic homelands satisfying at least one of the three following requirements: (i) the ethnic share of the region's population is of at least 90%, (ii) the region is among the top decile of ethnic shares, (iii) the ethnic share of the region's population is between the highest ethnic share observed in other regions and 10% below. Moreover, in cases where (ii) holds but the ethnic share of the region's population is less than 50% of the highest ethnic share observed in other regions, we do not consider the region an ethnic homeland.[48] Once all ethnic homelands are determined, we match them to the ethnicities of country leaders. We then code a binary indicator that equals one if a disaster hit the ethnic homeland of the political leader.[49] To make sure that our results do not depend on specific choices made when defining ethnic homelands, we further create a continuous measure capturing

[48] Rule (i) describes ethnic majority regions. Rule (ii) is useful to identify homelands of ethnic minorities who never reach a population share of 90%. We introduce rule (iii) as it may be the case that regions that are covered by rule (ii) do not necessarily exhibit a substantially higher population share than those closely below this cutoff. It is further possible that regions covered by rule (ii) comprise both areas with very high and very low population shares if an ethnicity is mainly centered in one or two regions of a country. The final requirement therefore imposes a restriction on the allowed divergence from the most important region from the perspective of the ethnicity.

[49] Note that given the definition of ethnic homelands applied in this paper, it is possible that a leader has multiple ethnic homelands. In these cases, the binary indicator equals one if a disaster hit at least one of these homelands.

the share of the population in disaster-affected areas belonging to the same ethnicity as the political leader ("ethnic share").[50]

In Table 9, we replicate columns 1 to 3 of Table 4 adding either the binary ethnic homeland indicator or the ethnic share. While the sample is reduced to 37 countries for which ethnic data are available, the birth region effect remains both economically and statistically significant in all specifications. By contrast, the influence of both ethnic homelands and ethnic share never reach statistical significance. Consequently, ethnic power relations do not seem to explain the observed birth region effect.[51]

8. Conclusion

With poor response capacities and vulnerable economies, low- and middle-income countries are particularly exposed to the impacts of natural disasters. Over the 1964 to 2017 period, each of such calamities in our sample resulted in the death of 360 people, on average. In addition to high casualty numbers, natural disasters can potentially lead to a range of adverse outcomes such as weaker economic growth (Felbermayr and Gröschl, 2014) or educational deficits of future generations (Caruso, 2017). International humanitarian aid therefore potentially fulfills a crucial role by providing affected countries with much-needed relief goods, financial means, organizational support and expertise, and thereby reducing the macro- and micro-costs of natural disasters.

However, in order to ensure that aid is granted to those who are most in need, it is essential to understand political and economic rationales that could distort the allocation

[50] Given that the geographic distribution of disaster victims is not reported, disasters affecting multiple regions are handled by calculating an area-weighted average of the ethnic composition of all affected ADM1 regions (or ADM2 areas if available).

[51] As Ahlerup and Isaksson (2015) point out, ethnic and regional favoritism are conceptually distinct from each other. Our results match those of Dreher et al. (2019) for Chinese development finance. However, unlike Dreher et al., who analyze ethnicity effects at the level of traditionally defined ethnic homelands based on *GREG* data, we are able to analyze both birth region and ethnicity effects at the level of ADM1 regions for aid flows directed to the same regions, ruling out the possibility that the differential effects are driven by varying geographic coverage. This feature of our data further allows us to estimate interaction effects between ethnic homelands and birth regions. These are, however, statistically insignificant in all specifications (results available on request).

of aid and thus reduce the effectiveness of disaster relief.[52] Our study contributes to the literature by taking a unique and new perspective that puts a *domestic political factor within a large number of recipient countries* at its center. In the first sub-national analysis of its kind, we have uncovered the importance of regional favoritism for the allocation of disaster aid. According to our results, disasters hitting birth regions of political leaders receive substantially higher amounts of humanitarian aid than other comparable disasters. This result is robust to netting out effects of local area characteristics and disaster magnitude.

Moreover, both disaster-area fixed effects and placebo models indicate that the effect is not related to the inherent unobserved characteristics of the affected areas, but attributed to the importance they hold when being hit by disasters. Furthermore, we do not find evidence that ethnic power relations govern the observed relationship. Nor is there evidence that the United States government actively promotes the birth region effect in order to please political leaders of countries with close political or commercial ties. To the contrary, regional favoritism seems to be weaker in countries where social and infrastructure spending tends to follow public interests, where bureaucracies are more able to resist political influence, and where governments are held accountable by reliable institutions and political competition, suggesting that the birth-region effect is driven by recipient country politics, facilitated by US policies that seem to neglect the internal political dynamics in the countries receiving its aid.

Our results have important policy implications. First, we identify the existence of favoritism in an extreme scenario of humanitarian need. While similar forms of favoritism have previously been observed in the contexts of Chinese development aid (Dreher et al., 2019) and economic growth (Hodler and Raschky, 2014), the fact that discriminatory politics are applied even when human lives are directly at stake is alarming. Our analysis thus calls for further research on this topic and the exploration of realistic possibilities of engaging in corrective action. In particular, even though we do not find evidence that the observed effects are driven by donor's political and economic interests, it is imperative to revise

[52] On the importance of political motives for the effectiveness of aid, see Dreher et al. (2018).

established processes related to disaster damage assessment and aid allocation in order to reduce the scope for manipulation by recipient country leaders.

This policy implication should be prioritized for two reasons. First, given that the results appear to be driven by recipient countries, it is likely that aid from other donor countries is similarly affected to the extent that these donors apply decision-making processes comparable to those of the United States. Second, as with accelerating climate change, natural disasters are projected to significantly increase in both frequency and intensity (IPCC, 2018), the relevance of effective humanitarian aid will likely increase in the near future.

A further policy implication of our findings relates to the long-term consequences of natural disasters. As leaders' birth regions tend to already be among the richer regions of their country (Hodler and Raschky, 2014; Dreher et al., 2019), regional favoritism in humanitarian aid could further exacerbate within-country inequalities. This is especially problematic given the potential of natural disasters to cause adverse long-run social and economic consequences. In order to ensure that disaster relief activities do not counteract international efforts to reduce inequality within poor countries, donors need to increase their efforts to incorporate concerns of regional equity in their aid allocation decisions.

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Appendix

Table A1. Control variables by country

Country	Barren land (%)	# ports	# nuclear plants	Capital city (%)	Night light per 1000 people[a]	Population densitiy[b]	City population[b]	Ruggedness[c]
Afghanistan	35.4 (15.6) [0.2,70.6]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	10.0	1.3 (1.8) [0.0,8.3]	0.3 (1.0) [0.0,6.1]	360.6 (923.0) [0.0,4720.9]	3.7 (2.0) [0.1,8.6]
Angola	0.2 (0.4) [0.0,2.2]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	41.8	3.9 (2.6) [0.2,11.8]	0.5 (1.3) [0.0,6.5]	2516.7 (2939.2) [0.0,10612.5]	0.8 (0.5) [0.1,3.0]
Argentina	5.1 (7.4) [0.0,36.6]	0.2 (0.4) [0.0,1.0]	0.6 (0.8) [0.0,3.0]	15.1	86.0 (48.6) [19.9,267.2]	0.1 (0.4) [0.0,3.6]	3740.0 (5361.0) [0.0,23860.4]	0.6 (0.9) [0.1,5.9]
Bangladesh	0.3 (0.5) [0.0,2.9]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	38.1	2.4 (0.8) [0.3,4.6]	1.3 (0.9) [0.1,8.2]	6643.3 (6882.6) [108.0,27684.1]	0.1 (0.2) [0.0,1.3]
Bolivia	4.4 (8.0) [0.0,55.6]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	56.7	18.6 (8.0) [5.4,52.5]	0.0 (0.1) [0.0,0.5]	1811.9 (1495.0) [0.0,4599.8]	1.2 (1.0) [0.1,4.7]
Brazil	0.2 (0.3) [0.0,2.7]	0.6 (0.7) [0.0,3.0]	0.3 (0.7) [0.0,2.0]	0.6	43.5 (17.8) [7.7,75.2]	1.0 (2.1) [0.0,8.4]	10548.3 (12518.2) [89.4,59021.8]	0.5 (0.4) [0.0,2.9]
Chile	16.4 (21.7) [0.0,78.6]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	36.4	69.3 (71.8) [14.4,429.9]	0.2 (0.5) [0.0,2.0]	2628.6 (3112.3) [0.0,9115.4]	2.8 (1.0) [1.0,7.1]
China	8.1 (19.4) [0.0,88.1]	0.1 (0.4) [0.0,4.0]	1.4 (3.4) [0.0,22.0]	2.4	13.0 (9.4) [2.2,70.5]	0.3 (0.4) [0.0,5.9]	32861.2 (50198.6) [0.0,481157.6]	2.3 (1.2) [0.0,7.7]
Colombia	0.3 (0.5) [0.0,4.7]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	16.1	24.5 (13.1) [0.9,74.7]	0.6 (1.6) [0.0,7.4]	3754.1 (6210.2) [0.0,23161.7]	1.7 (1.0) [0.0,5.5]
Costa Rica	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	40.0	59.5 (11.2) [39.4,84.8]	0.1 (0.1) [0.0,0.3]	467.8 (540.2) [0.0,1760.0]	2.4 (0.8) [1.0,4.3]
Cuba	0.0 (0.0) [0.0,0.3]	1.8 (1.6) [0.0,6.0]	0.0 (0.0) [0.0,0.0]	22.4	22.0 (10.3) [3.7,72.7]	0.4 (1.3) [0.0,8.7]	707.1 (957.4) [0.0,2913.0]	0.8 (0.5) [0.0,2.3]
DR Congo	0.0 (0.0) [0.0,0.1]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	12.7	1.2 (3.0) [0.0,29.6]	1.5 (4.3) [0.0,21.5]	1493.2 (2910.7) [0.0,16086.6]	0.7 (0.6) [0.0,2.2]
Dominican Rep.	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	31.6	21.8 (4.9) [11.4,46.0]	0.5 (0.6) [0.1,2.8]	1024.7 (1244.1) [0.0,3491.1]	1.6 (0.6) [0.2,3.6]
Ecuador	2.2 (10.1) [0.0,56.9]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	23.4	40.9 (56.5) [1.5,495.8]	0.2 (0.2) [0.0,0.6]	931.7 (1312.0) [0.0,4520.7]	1.7 (1.1) [0.1,4.4]
El Salvador	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	59.5	21.2 (3.4) [12.2,26.9]	0.9 (1.4) [0.2,5.9]	602.9 (520.8) [0.0,1096.5]	1.8 (0.4) [1.2,3.7]
Ethiopia	8.0 (15.5) [0.0,57.1]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	10.2	0.7 (1.2) [0.0,6.3]	0.3 (1.3) [0.0,7.2]	400.9 (820.0) [0.0,3317.9]	1.5 (1.1) [0.1,6.2]
Greece	0.2 (1.2) [0.0,9.7]	0.3 (0.4) [0.0,1.0]	0.0 (0.0) [0.0,0.0]	18.8	136.9 (58.0) [5.4,245.8]	1.4 (4.1) [0.0,16.6]	960.2 (1446.1) [0.0,3981.7]	3.0 (1.3) [0.6,6.0]
Guatemala	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	33.8	13.4 (6.9) [0.9,41.3]	0.7 (1.5) [0.0,7.1]	737.2 (1078.4) [0.0,2738.3]	2.1 (1.1) [0.1,5.0]
Haiti	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	51.6	1.2 (0.9) [0.0,3.8]	0.8 (1.0) [0.1,3.7]	982.3 (1062.7) [0.0,2642.8]	2.5 (0.6) [1.1,4.4]
Honduras	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	33.3	17.5 (8.4) [2.1,51.6]	0.2 (0.3) [0.0,0.8]	551.5 (613.0) [0.0,2069.0]	2.1 (0.9) [0.4,4.8]
India	2.6 (5.3) [0.0,34.2]	0.3 (0.5) [0.0,3.0]	1.2 (2.3) [0.0,17.0]	6.0	9.8 (18.2) [0.5,414.0]	1.3 (4.0) [0.0,50.3]	17358.1 (21380.6) [0.0,159538.5]	1.3 (1.8) [0.0,8.3]
Indonesia	0.0 (0.0) [0.0,0.0]	0.3 (0.5) [0.0,2.0]	0.0 (0.0) [0.0,0.0]	9.2	6.5 (5.7) [0.0,84.5]	1.0 (2.7) [0.0,16.7]	3451.8 (5762.2) [0.0,34596.4]	1.3 (0.7) [0.1,3.3]
Iran	50.7 (37.5) [0.0,100.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	4.4	114.6 (245.5) [20.4,3071.1]	0.1 (0.1) [0.0,0.8]	1069.2 (1782.3) [0.0,14678.0]	3.1 (1.8) [0.1,9.6]
Kenya	6.7 (10.6) [0.0,38.7]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	15.9	1.3 (1.2) [0.1,7.2]	0.3 (0.8) [0.0,5.8]	1140.6 (1517.9) [0.0,6205.0]	0.8 (0.8) [0.1,3.6]
Madagascar	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	39.3	0.6 (0.3)	0.0 (0.0)	819.1 (1036.3)	1.2 (0.2)

	[0.0,0.0]	[0.0,0.0]	[0.0,0.0]		[0.0,1.8]	[0.0,0.1]	[0.0,3396.6]	[0.5,1.8]
Malawi	0.0	0.0	0.0	17.5	1.4	0.2	237.0	1.1
	(0.0)	(0.0)	(0.0)		(0.4)	(0.1)	(468.5)	(0.5)
	[0.0,0.0]	[0.0,0.0]	[0.0,0.0]		[0.5,2.3]	[0.1,0.4]	[0.0,1652.5]	[0.4,3.3]
Malaysia	0.0	0.4	0.0	11.7	34.5	0.6	1183.4	0.9
	(0.0)	(0.6)	(0.0)		(14.7)	(1.1)	(1458.0)	(0.5)
	[0.0,0.0]	[0.0,2.0]	[0.0,0.0]		[3.2,57.5]	[0.0,4.3]	[0.0,8094.1]	[0.1,2.0]
Mexico	0.0	0.0	0.4	5.8	59.7	0.4	4188.2	2.0
	(0.1)	(0.0)	(0.8)		(25.0)	(1.5)	(6208.3)	(1.1)
	[0.0,0.6]	[0.0,0.0]	[0.0,2.0]		[6.9,123.5]	[0.0,11.2]	[0.0,60164.3]	[0.0,5.1]
Mozambique	0.0	0.0	0.0	34.3	2.3	0.2	1147.5	0.5
	(0.0)	(0.0)	(0.0)		(3.6)	(0.5)	(1045.3)	(0.2)
	[0.0,0.1]	[0.0,0.0]	[0.0,0.0]		[0.0,22.1]	[0.0,3.0]	[0.0,3897.9]	[0.0,1.1]
Myanmar	0.0	0.0	0.0	5.5	2.7	0.1	950.5	1.7
	(0.0)	(0.0)	(0.0)		(2.4)	(0.2)	(1677.5)	(0.8)
	[0.0,0.1]	[0.0,0.0]	[0.0,0.0]		[0.1,15.0]	[0.0,0.9]	[0.0,6119.0]	[0.1,4.5]
Nepal	1.5	0.0	0.0	59.0	1.7	0.2	549.9	5.2
	(2.1)	(0.0)	(0.0)		(0.8)	(0.1)	(524.5)	(1.2)
	[0.2,12.2]	[0.0,0.0]	[0.0,0.0]		[0.0,3.3]	[0.0,0.4]	[0.0,1572.7]	[1.8,7.7]
Nicaragua	0.0	0.0	0.0	42.3	40.0	0.1	349.4	0.9
	(0.0)	(0.0)	(0.0)		(63.9)	(0.3)	(437.0)	(0.4)
	[0.0,0.0]	[0.0,0.0]	[0.0,0.0]		[0.5,231.2]	[0.0,1.7]	[0.0,1012.6]	[0.1,2.4]
Niger	47.4	0.0	0.0	34.4	2.2	0.1	411.2	0.2
	(27.3)	(0.0)	(0.0)		(2.1)	(0.5)	(511.0)	(0.1)
	[0.2,91.9]	[0.0,0.0]	[0.0,0.0]		[0.3,7.2]	[0.0,4.0]	[0.0,1536.0]	[0.0,0.5]
Nigeria	0.2	0.1	0.0	1.9	4.7	1.1	3870.6	0.3
	(0.3)	(0.3)	(0.0)		(8.2)	(3.9)	(5924.3)	(0.2)
	[0.0,1.8]	[0.0,1.0]	[0.0,0.0]		[0.2,58.7]	[0.1,36.6]	[0.0,32142.3]	[0.0,1.1]
Pakistan	31.4	0.0	0.6	3.2	10.8	1.1	9128.8	2.6
	(26.4)	(0.0)	(0.8)		(5.9)	(2.2)	(10789.9)	(2.8)
	[0.0,98.5]	[0.0,0.0]	[0.0,4.0]		[0.0,27.5]	[0.0,8.5]	[0.0,45977.1]	[0.0,9.5]
Panama	0.4	0.0	0.0	43.5	19.6	0.1	493.1	1.6
	(0.5)	(0.0)	(0.0)		(7.6)	(0.2)	(622.8)	(0.7)
	[0.0,2.1]	[0.0,0.0]	[0.0,0.0]		[4.1,34.2]	[0.0,0.6]	[0.0,1673.1]	[0.6,3.3]
Papua N. Guinea	0.0	0.0	0.0	3.7	0.5	0.9	12.3	1.9
	(0.0)	(0.0)	(0.0)		(0.5)	(2.2)	(63.2)	(0.8)
	[0.0,0.2]	[0.0,0.0]	[0.0,0.0]		[0.0,2.1]	[0.0,11.1]	[0.0,338.1]	[0.5,3.4]
Peru	12.1	0.0	0.0	22.1	19.0	0.1	2204.8	2.1
	(15.5)	(0.0)	(0.0)		(9.0)	(0.4)	(3776.2)	(1.0)
	[0.0,63.7]	[0.0,0.0]	[0.0,0.0]		[3.1,57.1]	[0.0,3.2]	[0.0,13830.8]	[0.0,4.6]
Philippines	0.0	0.3	0.0	19.8	4.1	1.1	3684.9	1.9
	(0.0)	(0.5)	(0.0)		(1.9)	(3.8)	(5375.3)	(0.6)
	[0.0,0.0]	[0.0,2.0]	[0.0,0.0]		[0.3,10.3]	[0.0,22.8]	[0.0,25231.8]	[0.1,5.1]
Romania	0.1	0.0	0.4	43.4	85.7	0.3	1085.0	1.2
	(0.2)	(0.0)	(0.7)		(17.3)	(1.0)	(1063.3)	(0.6)
	[0.0,1.3]	[0.0,0.0]	[0.0,2.0]		[12.3,136.2]	[0.0,7.4]	[0.0,2865.0]	[0.1,3.6]
South Africa	0.3	0.7	0.4	19.0	39.1	0.4	3871.5	2.4
	(0.9)	(0.5)	(0.8)		(15.4)	(0.6)	(3889.8)	(0.9)
	[0.0,6.6]	[0.0,2.0]	[0.0,2.0]		[10.2,85.4]	[0.0,2.8]	[0.0,17145.7]	[0.3,4.6]
Sri Lanka	0.1	0.0	0.0	47.9	14.2	0.6	287.4	0.7
	(0.2)	(0.0)	(0.0)		(3.2)	(0.7)	(302.2)	(0.5)
	[0.0,1.9]	[0.0,0.0]	[0.0,0.0]		[3.1,19.8]	[0.1,3.6]	[0.0,638.9]	[0.0,2.7]
Sudan	35.3	0.0	0.0	33.3	4.6	0.0	1583.8	0.4
	(32.3)	(0.0)	(0.0)		(5.8)	(0.1)	(2200.4)	(0.3)
	[0.0,99.5]	[0.0,0.0]	[0.0,0.0]		[0.0,34.1]	[0.0,0.3]	[0.0,7696.1]	[0.0,1.5]
Tajikistan	23.0	0.0	0.0	26.3	16.3	0.5	275.1	4.1
	(17.9)	(0.0)	(0.0)		(12.2)	(1.2)	(342.6)	(2.4)
	[0.4,66.9]	[0.0,0.0]	[0.0,0.0]		[0.0,68.5]	[0.0,4.0]	[0.0,832.0]	[0.8,9.3]
Tanzania	0.0	0.0	0.0	21.4	1.6	0.4	862.7	0.6
	(0.1)	(0.0)	(0.0)		(1.8)	(0.8)	(1597.7)	(0.4)
	[0.0,0.9]	[0.0,0.0]	[0.0,0.0]		[0.0,6.8]	[0.0,3.0]	[0.0,6933.2]	[0.0,1.8]
Thailand	0.0	0.1	0.0	6.1	24.3	0.3	1154.4	1.2
	(0.0)	(0.2)	(0.0)		(10.5)	(0.7)	(1932.0)	(0.6)
	[0.0,0.1]	[0.0,1.0]	[0.0,0.0]		[4.1,81.0]	[0.0,3.7]	[0.0,9905.9]	[0.0,2.9]
Turkey	0.5	0.3	0.0	9.1	53.9	0.3	2326.3	2.8
	(0.7)	(0.6)	(0.0)		(52.3)	(0.6)	(5007.4)	(1.2)
	[0.0,3.4]	[0.0,3.0]	[0.0,0.0]		[10.1,530.0]	[0.0,3.0]	[0.0,27598.2]	[0.5,6.0]
Uganda	0.3	0.0	0.0	14.8	0.4	0.8	270.5	1.2
	(0.9)	(0.0)	(0.0)		(0.8)	(2.2)	(667.9)	(0.7)
	[0.0,5.4]	[0.0,0.0]	[0.0,0.0]		[0.0,4.1]	[0.0,11.0]	[0.0,2577.0]	[0.2,3.9]
Venezuela	0.0	0.1	0.0	43.6	57.6	0.2	2891.9	1.5
	(0.1)	(0.3)	(0.0)		(28.4)	(0.3)	(3106.4)	(1.1)
	[0.0,0.2]	[0.0,1.0]	[0.0,0.0]		[15.3,139.9]	[0.0,1.4]	[0.0,12165.8]	[0.0,4.2]
Vietnam	0.1	0.0	0.0	9.0	6.2	0.4	778.3	2.2
	(0.1)	(0.0)	(0.0)		(2.5)	(0.8)	(1696.0)	(1.2)
	[0.0,0.6]	[0.0,0.0]	[0.0,0.0]		[0.5,14.3]	[0.0,9.3]	[0.0,11118.1]	[0.0,6.1]
Full sample	6.2	0.1	0.3	16.9	21.0	0.6	7627.2	1.7
	(17.2)	(0.4)	(1.5)		(51.6)	(2.2)	(21659.8)	(1.5)
	[0.0,100.0]	[0.0,6.0]	[0.0,22.0]		[0.0,3071.1]	[0.0,50.3]	[0.0,481157.6]	[0.0,9.6]

Notes: The table displays means, (standard deviations) and [min,max] for all control variables used in the analysis. For the binary indicator "Capital city" only percentages are reported. [a] Raw night light intensity values can reach a maximum of 63. Night light intensity per 1,000 people was calculated by first aggregating night light emissions across grid cells within each ADM region and by dividing by the population (times 1,000) living in the respective area. Afterwards, for each disaster, area-weighted averages were created. [b] For readability purposes, population density is scaled in terms of 1,000 people per km^2 and city population is scaled in terms of thousands of inhabitants. [c] Ruggedness is scaled in terms of 100s of meters of elevation differences.