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Abstract

International humanitarian aid is pivotal in the response to natural disasters suffered by low-and middle-income countries. While its allocation has been shown to be influenced by donors' foreign policy considerations, power relations within recipient countries have not been addressed. This paper is the first to investigate the role of regional and ethnic favoritism in the formation of humanitarian aid flows. We construct a novel dataset combining information on birth regions of political leaders and the geographic distribution of ethnic groups within countries with high numbers of natural disasters building on census (IPUMS) and Demographic and Health Surveys (DHS) data. Our results suggest that the Office of US Foreign Disaster Assistance (OFDA) disburses larger amounts of aid when natural disasters affect the birth region of the countries' leader. We find some evidence that OFDA disburses aid more frequently to leaders' birth regions as well as when regions hit by disasters are populated by politically powerful or discriminated ethnicities. Our findings imply that humanitarian aid is not given for humanitarian reasons alone, but also serves elite interests within recipient countries.

JEL-Codes: F350.

Keywords: humanitarian aid, disasters, ethnic favoritism, regional favoritism.

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1. Introduction

Natural disasters are a key developmental challenge for a large number of low- and middle-income countries across the globe. While the often substantial human and economic loss following earthquakes, floods, landslides or similar events requires a swift and organized response, poor management capacities and a lack of resources make low-income countries frequently unable to provide adequate relief on their own (Mirza, 2003). Given the combination of high vulnerability and low response capacities in many countries, international humanitarian aid becomes a crucial means to alleviate suffering in disaster-affected regions. In this paper, we investigate whether such aid is exclusively allocated based on need, or whether (ethno-)political motives influence the allocation of aid.

Previous research shows the importance of donor political interests in the allocation of humanitarian aid. For instance, disasters hitting allies of the United States (US) have a substantially higher probability to evoke US support, though they are comparable to other countries in terms of the number of people killed or losing their home (Drury et al., 2005). Humanitarian aid has also been shown to be shaped by donors' domestic political motives, such as the media attention a disaster receives, the donors' budgetary situation, or their own exposure to disasters (Drury et al., 2005; Eisensee and Strömberg, 2007). To the contrary, the political economy within *recipient* countries has largely been ignored.[1] While Drury et al. (2005) raise the possibility that domestic politics within the recipient country shape the allocation of humanitarian aid in important ways, they neither provide specific hypotheses nor a test.

Despite this lack of systematic research, anecdotal evidence regarding the potential importance of the political economy within aid-receiving countries are 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), the allocation of humanitarian aid is distorted due to power relations at the community level within recipient countries. In this paper, we provide the first systematic investigation of the role of recipient country political factors as determinants of humanitarian aid. More specifically, we investigate the extent to which the power relations between disaster victims and the affected country's central government shape the international aid response. We focus our analysis on the allocation of US humanitarian aid using information on disaster relief decisions derived from annual reports issued by the Office

[1] 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; Franken et al., 2012, Schneider, 2018). The second investigates discrimination against individual victims of disaster. 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).

of US Foreign Disaster Assistance (OFDA) – the US agency responsible for providing disaster relief overseas. The United States are by far the biggest donor of humanitarian aid. OFDA responds to an average of 65 disasters in more than 50 countries per year.[2]

We measure power relations within aid-receiving countries in two ways. First, we test if disasters occurring in birth regions of the political leader receive more aid than other comparable disasters. Second, we investigate whether the power status of ethnic groups hit by natural disasters matters for disaster relief.[3] To this end, we introduce a novel dataset that codes the prevalence of ethnic groups in sub-national regions with the help of census data from the Integrated Public Use Microdata Series (IPUMS) as well as Demographic and Health Surveys (DHS) (Minnesota Population Center, 2017; MEASURE DHS, 2017).[4]

Using data on 5,189 rapid-onset natural disasters that have hit 38 countries over the 1964-2015 period, we investigate how ethnic and regional favoritism shape the allocation of aid at the disaster level in two stages. First, we focus on the decision on whether or not to extend any aid. Second, we investigate whether favoritism affects the amount of aid among disasters with positive aid flows.

While the timing as to when a 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 or hosting the ruling ethnicity) 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 power dynamics affect the probability of receiving aid as well as the amounts of aid could be biased. We follow two strategies to test the causal effects of power relations measures on the allocation of aid: First, we control for a range of local area characteristics derived from census and satellite data. Second, we test whether disasters hitting regions that are not powerful at the time of the disaster but will become powerful subsequently receive similar treatment compared to disasters hitting contemporaneously powerful regions. Rather than comparing catastrophes happening in politically important regions with those affecting unimportant regions, we thereby compare disasters in areas that are unimportant at the time of the disaster, but will become important soon. In comparing these two types of disasters, any unobserved characteristics of powerful

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[2] See https://www.usaid.gov/who-we-are/organization/bureaus/bureau-democracy-conflict-and-humanitarian-assistance/office-us (last accessed January 5th, 2018).

[3] Throughout this paper, we refer to regions in which a country's leader is born or that are populated by ethnic groups with representation in national government as "powerful."

[4] Some of the DHS data were retrieved from the IPUMS website (Heger Boyle et al., 2017).

regions that do not vary over a very short time frame will thus be controlled for. In the same spirit, we also test how disasters in regions that are not powerful at the time they are hit by disasters – but have just been powerful in the years before – compare to disasters in powerful regions. In tandem with the fact that we limit the scope of our analysis to disasters that are plausibly exogenous in their timing, we interpret our results as causal.

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 the results of these studies, donor political interests feature prominently among the motives for granting food and other humanitarian aid (Ball and Johnson, 1996; 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) none of them focused on variation within recipient countries. Consequently, no previous study has investigated the importance of ethnic and regional favoritism for the allocation of disaster aid.

Second, our paper contributes to the literature investigating regional and ethnic favoritism. A number of recent studies have shown that populations living in the ethnic homeland of the countries' leaders benefit from better education and health (Franck and Rainer, 2012; Kramon and Posner, 2016), as well as infrastructure (Burgess et al., 2015). Using a worldwide sample, Hodler and Raschky (2014) and De Luca et al. (2018) show that ethnic homelands of a country's leader and their birth regions experience higher economic growth. This literature provides the analytic background for our study. It shows that leaders of countries around the world divert resources to their birth regions and ethnic homelands out of altruism for their own ethnicity but also out of electoral and other strategic concerns (Padró i Miquel, 2007; De Luca et al., 2018).[5]

We also extend the literature by providing novel data on the sub-national location of ethnic groups. For each country in our sample, we identify the ethnic composition of sub-national populations based on census or survey data. This is an improvement over previous research, which is mostly based on *Ethnologue* data (Gordon, 2005), the *Geo-referencing of Ethnic Regions* (*GREG*) data (Weidmann et al., 2010) or *GeoEPR* – a geocoded version of the *Ethnic Power Relations* (*EPR*) dataset (Wucherpfennig et al., 2011). First, our data define ethnicity based on census and survey responses rather than being restricted to differences in language exclusively (as do *Ethnologue* or *GREG*). Second, unlike *GREG* data that are based

[5] Alesina and La Ferrara (2005) discuss different rationales for treating members of the same ethnic groups differently from members of other ethnic groups.

on the *Soviet Atlas Narodov Mira,* we take account of changes in the composition of ethnic populations over the more than 50 years that have passed since these data have been compiled in the 1960s. And third, compared to *GeoEPR* which is based on the assessment of a small number of country experts determining ethnic homelands, our data are considerably more fine-grained in terms of geographic precision.

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. While recipient need is an important determinant of the amount of aid a country receives, donors' geostrategic considerations are equally key (Alesina and Dollar, 2000; Kuziemko and Werker, 2006; Hoeffler and Outram, 2011; Vreeland and Dreher, 2014). Only recently has this literature begun to investigate donor and recipient motives in the allocation of aid at the sub-national level. Most closely related to this paper is Dreher et al. (2016) who show that birth regions of a country's national leader and sub-national regions populated by the same ethnicity as the leader receive more foreign aid from China. They identify the causal effect of aid by comparing regions that have just ceased to be powerful or will be powerful subsequently, but are not politically connected at a certain point in time. This paper follows their identification strategy while focusing on disaster aid rather than general foreign aid.

Dreher et al. (2016) benchmark their results on the World Bank, pointing to the absence of project-level data for Western bilateral donors that could be used for sub-national analysis. With this paper, we are thus the first to investigate the effect of regional and ethnic favoritism in the allocation of a Western bilateral donor and thus extend the literature in an important dimension. What is more, we exploit exogenous variation in recipient need caused by natural disasters, facilitating the identification of causal effects compared to studies focusing on the allocation of aid more broadly.

Finally, our study relates to the literature that investigates political influences in the allocation of central government support to sub-national regions that are affected by disasters. This literature has shown that political motives shape the allocation of resources. For the United States, plenty of evidence reports that disaster declarations become more likely, and disaster expenditures increase, when the region hit by the disaster is politically aligned with the President, in particular at election time or when electoral competitiveness is high (Garrett and Sobel, 2003; Reeves, 2011; Gasper, 2015). Garrett and Sobel (2003: 496) conclude that "nearly half of all disaster relief is motivated politically rather than by need." To the extent that recipient governments decide on where disaster aid is allocated, we would expect similar mechanisms to be at play in the allocation of international disaster aid.

We proceed as follows. Section 2 introduces our main data sources – on humanitarian aid, natural disasters, leaders' birth regions, ethnic groups, 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 provide various extensions and tests for robustness in section 6. We discuss the policy implications of our research in the concluding section 7.

2. Data sources and variables

Disaster Aid

Three main data sources have been previously 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 Development Assistance Committee (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 the research questions of this paper.

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 data for each appeal, such that they can be matched to individual disasters. They include information about non-governmental organizations, allow identifying the original bilateral donor of multilateral aid, and separately report whether the aid is given as cash or in kind. They come, however, at 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 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,[6] creating the risk of measurement error for our analysis. Finally, the lack of a common standard

[6] 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).

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 is an office within 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; Kevlihan et al., 2014).[7] 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, typically accounting for a third of total emergency aid donated by OECD countries (Eisensee and Strömberg, 2007; Fink and Redaelli, 2011).[8] 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 make 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 2015 and transform them to constant 2015 US Dollars. For the 1964-1989 period, we use data provided to us by Cooper Drury who previously conducted research based on data from OFDA's annual reports (Drury et al., 2005).[9] A potential drawback of relying on OFDA data is that while OFDA is usually the first and principal US agency reacting to a disaster, it is not the only US agency giving humanitarian aid (Strömberg, 2007). According to Strömberg (2007), OFDA does however steer the response of other US agencies, like the Office for Food for Peace, which requires OFDA declarations to take action. What is more, until fiscal year 2004, OFDA reports contain figures on humanitarian aid provided by these agencies such that we are able to assess the robustness of our results in this sub-period. Moreover, in line with previous research (Fink and Redaelli, 2011), we limit the scope of the analysis to rapid-onset disasters, which often

[7] 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). Interestingly, OFDA (2010: 13) also explains that a response must be "in the interest of the US Government" (p.13).

[8] Similarly, in 2016, US humanitarian aid constituted nearly one third of all disaster relief reported to FTS. See https://fts.unocha.org/global-funding/overview/2016 (accessed February 23rd, 2018).

[9] All annual reports (with the exception of the 1974-1982 period) are available for download from the USAID Development Experience Clearinghouse. See https://dec.usaid.gov/dec/search/Sear-chResults.aspx?q=ZG9jdW1lbnRzLndlYl9jb2xsZWN0aW9uPSJvZmRhIGFubnVhbCByZXBvcnQi (accessed January 15th, 2018).

occur unexpectedly and, in comparisons to slowly evolving catastrophes such as famines or complex emergencies, are less likely to be the consequence of intentional acts of governments or the inactivity of international donors.[10] This latter group of disasters is more likely to require the involvement of several government agencies (such as the Office for Food for Peace), so that excluding them increases the representativeness of OFDA aid data.

The annual reports also describe how OFDA allocates aid: The local US Ambassador or Chief of the US Mission (in case there is no mission: the appropriate US Assistant Secretary of State) declares a disaster when the magnitude is beyond the country's capacity to respond and the affected country asks for aid or at least agrees to it (OFDA, 2010). They can afterwards directly allocate a limited amount of aid (US$ 50,000 since 2003, US$ 25,000 before). OFDA officials then decide on whether to grant additional aid (Kevlihan et al., 2014). Depending on the amounts granted, this can involve officials from the Department of State, the National Security Council, or even the President. Among our sample, the amount of aid given to the average disaster with positive aid flows (measured in terms of 2015 US-$) amounts to US-$ 1.98 million, with a minimum of US-$ 202.60 and a maximum of US-$ 319.07 million.

Favoritism in the allocation of this aid may appear on both levels. On the one hand, the ambassador's geographic proximity to local politicians could make collusion at the initial aid decision stage particularly likely. On the other hand, while ambassadors often need to make quick decisions on whether or not to grant initial relief, total humanitarian aid amounts are decided with some delay, such that more time for lobbying is available.

Natural Disasters

We take data on natural disasters from the EM-DAT international disaster database (Guha-Sapir, 2018), which is 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, and the number of people

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

affected.[11] Conveniently, EM-DAT also includes the sub-national location of the disaster for the first administrative levels or lower.[12]

For the analysis, we include 38 countries affected by a high number of rapid-onset emergencies (earthquakes, dry mass movements, floods, landslides, storms, volcanos, cold/heat waves, epidemics, insect infestations, and wildfires) over the 1964-2015 period. For each country, we merge the list of disasters with aid data from OFDA, using information on the timing and location of disasters provided in both the EM-DAT data and the OFDA reports. In doing so, we face the challenge that aid contributions per disaster are not disaggregated by affected administrative regions. Moreover, EM-DAT does not report the distributions of victims and damage within the disaster-affected subnational areas. Aid given for disasters that hit more than one sub-national region is thus attributed to all of the affected regions. We hence analyze our research question on the disaster-level rather than in the form of a regional panel regression approach, as explained further below.

Leaders' birth regions

To identify our first measure of favoritism in the allocation of humanitarian aid, we match regions affected by disasters to birth regions of the countries' political leaders. The latest version of the Archigos database[13] lists the political leaders (President, Prime Minister, or religious leader, depending on the political system) for a large number of countries. We complement these data with birthplace information acquired through online search.[14] While leader birth regions are also available on the ADM2 level, we focus on ADM1 birth regions, as

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[11] EM-DAT draws from a number of sources, including UN agencies, non-governmental organizations, insurance companies, research institutes and press agencies. See EM-DAT's Frequently Asked Questions Page at http://www.emdat.be/frequently-asked-questions (accessed on January 12th, 2018). The number of persons killed is defined as those confirmed dead or missing and presumed dead; the total number of people affected is the sum of people injured, homeless, and affected (with affected implying that they require immediate assistance during an emergency situation). See http://www.emdat.be/source-entry (accessed on January 12th, 2018). Kevlihan et al. (2014) highlight the drawbacks of the EM-DAT data: The number of persons killed are reported by governments and are thus potentially biased, the definition of "number of persons affected" might be applied differently across countries and time, and figures are not given per year when countries are hit by multi-year disasters. In spite of these potential criticisms, EM-DAT is the most widely-used and most reliable database on disasters. Note that a potential correlation of the number of disasters with development at the country-level (Felbermayr and Gröschl, 2014) would not threaten identification in our disaster-level regressions, given the set of fixed effects that we include.

[12] Subnational regions at the first administrative level (ADM1) typically are departments, provinces, states, and governorates; regions at the second administrative level (ADM2) include districts or municipalities, among others.

[13] See http://www.rochester.edu/college/faculty/hgoemans/data.htm (accessed on January 22, 2018) and Goemans et al. (2009).

[14] The main sources we used to identify birth regions were the leader biography database of the Barcelona Centre for International Affairs (CIDOB, n.d.), the online version of Encyclopaedia Britannica (n.d.), as well as the online persons database of Munzinger (n.d.). Further sources were Amstutz (1986), Lenz (2013), Casa Rosada (n.d.), Banglapedia (2012), Vicepresidencia (n.d.), the Brazilian Presidential Library (n.d.), Obregón Quesada (2002), the Encyclopedia of Latin American History and Culture (2008), the Encyclopedia of World Biography (2004a,b), Harding (2003), Dorcé and Tremblay (2015), HNS (2009), and the Supreme Court of Nepal (n.d.).

more than 60% of EM-DAT disasters either provide only ADM1-level locations or a combination of ADM1 and 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 the disaster hit the country.[15]

Ethnic Groups

Our second measure of favoritism refers to the ethnic identity of disaster victims. We identify the sub-national location of ethnic groups relying on data from IPUMS (Minnesota Population Center, 2017) and the DHS (MEASURE DHS, 2017).[16] IPUMS-International provides census microdata from around the world for 85 countries. It currently covers 672 million people in 301 censuses, some of which go back to the 1960s. The data are consistent across countries and over time and represent the largest available archive of publicly available census data. The data are recorded at the ADM2 level or lower for most countries in our sample. For the remaining set of countries, ADM1-level information is available. Where we could not obtain census data, we made use of survey data instead. Given that the number of disaster victims is small relative to the total populations living in affected areas, it is unlikely that disasters significantly alter the local ethnic composition over time. What is more, natural disasters have historically not led to large long-term displacements of populations in most cases (Noji, 1997: 80), and there is evidence that large-scale disasters do not substantially affect economic development (Cavallo et al., 2013). Nevertheless, to minimize any potential endogeneity bias, we make use of the latest census/survey collected before a disaster happened, where possible.[17]

Using these data sources, we generate continuous measures reflecting the ethnic identity of disaster victims. The core assumption that allows us to generate this indicator is that within any disaster-affected administrative area, all inhabitants have the same probability to fall victim to a disaster. Moreover, given that EM-DAT often reports multiple administrative areas as disaster locations, it is necessary to calculate an average distribution of ethnicities. To this end, we take population-weighted means for the share of each ethnicity across the affected

[15] For most disasters reported by EM-DAT, start dates and end dates are identical or very close to each other. In the rare event where start and end dates differ and leaders change in between, we coded the binary indicator equal to one if any of the leaders' birth regions was hit.

[16] 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 that. See Gershman and Rivera (2018) for a similar approach to coding the ethno*linguistic* composition of sub-national regions. For one country (Turkey), neither IPUMS nor DHS data were available. We used KONDA (2014) survey results as an alternative.

[17] For our analysis in general as well as for the construction of geo-coded control variables, it is important to hold geographic units constant over time. We therefore use recent Geographic Information Systems (GIS) maps as benchmarks for the definition of administrative areas and match census/survey data from various waves to these regions, taking into account boundary changes over time.

administrative regions.[18] While EM-DAT does not provide ADM2-level location information for all disasters, we calculate ethnicity shares at the ADM2-level where possible to maximize precision. Assuming that the probability of death is uniformly distributed within disaster-affected regions, population weighting produces a good approximation of the true ethnicity distribution among disaster victims.

It is worth noting that our approach of measuring ethnicities brings substantial advantages relative to traditionally used datasets. Being a digitized version of the *Soviet Atlas Narodov Mira*, the previously mentioned *GREG* data restrict the list of ethnic groups to those determined in the 1960s.[19] According to Weidmann et al. (2010), details on sources, definitions, and coding conventions of the *Atlas* 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. Compared to our approach 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).[20] What is more, as data for the *Atlas* were collected in the 1960s, they do not accurately reflect the current location of ethnic groups. *GeoEPR* – a further often-used dataset – is the geocoded version of the Ethnic Power Relations (*EPR*) dataset (Wucherpfennig et al., 2011) and avoids some of these problems. It provides a list of 733 politically relevant ethnic groups, based on online expert surveys, and takes account of changes over time. It however suffers from the disadvantage that regional groups with an exclusively urban base, geographically dispersed groups, or migrants are excluded. As a consequence, using *GeoEPR* maps to locate ethnicities would require dropping all disasters which affected urban areas, considerably narrowing the scope of analysis. We hence consider the use of IPUMS census data and complementary survey data a superior way of approaching the research question at hand.

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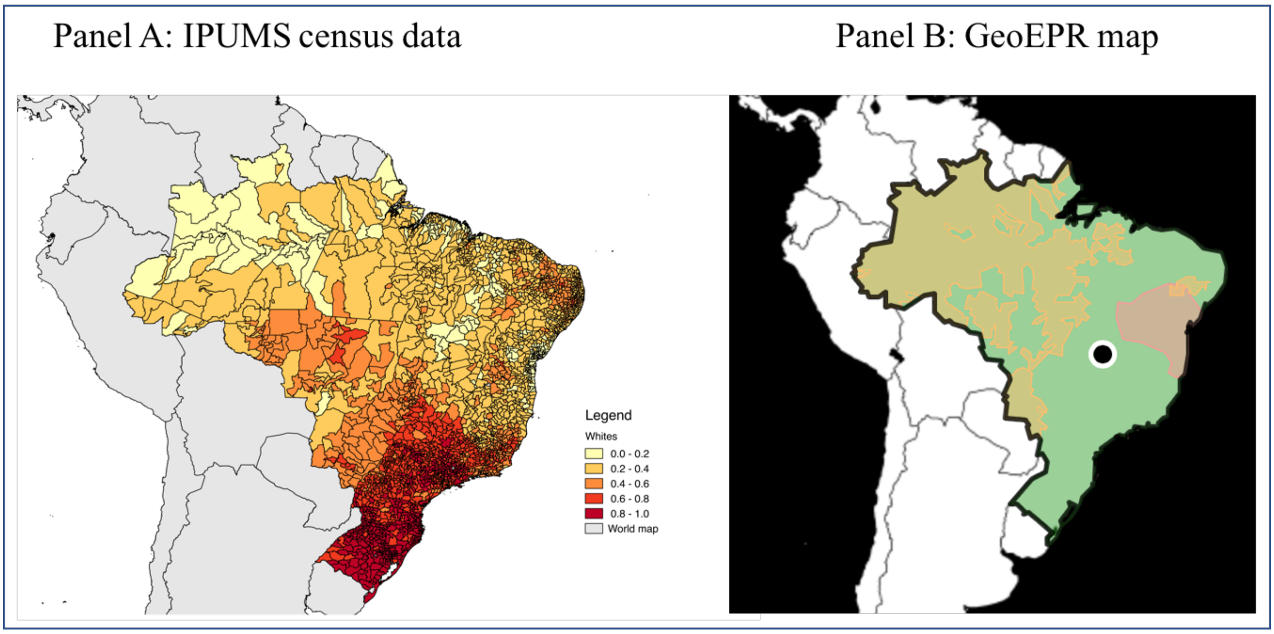
[18] We describe the way we measure population in more detail below. Population data are only available from the year 2000 onwards in quinquennial intervals. We hence use 2000 population data for all years before the year 2000 and always the year that is closest to the census date after 2000 to construct ethnicity weights.

[19] The *Atlas* reports different levels of accuracy across world regions (Wucherpfennig et al., 2011). For example, they are more detailed in Eastern Europe and the former Soviet Union than in Africa.

[20] Another dataset available to study ethnic favoritism is the *World Language Mapping System*, which builds on the languages described in *Ethnologue* (Gordon, 2005). *Ethnologue* includes a comprehensive list of languages (De Luca et al., 2018) and portrays the homelands of ethnic groups around the world. Given its focus on homelands, *Ethnologue* does not take account of recent migration, to the urban centers, or otherwise (Alesina et al., 2016). Like *GREG*, *Ethnologue* narrowly focuses on linguistic traits.

Figure 1: Settlement patterns of Whites according to IPUMS census data and *GeoEPR*



Panel A shows the fraction of Whites by Brazilian municipality based on IPUMS census data. Panel B shows ethnic homelands as postulated by *GeoEPR* data (green area: Whites; yellow area: indigenous people; red area: Afrobrazilians. Sources: Global Administrative Areas (available from: http://gadm.org, accessed March 1st, 2018) for shapefile used in panel A; panel B adapted from GROWup – Geographical Research on War (available from: https://growup.ethz.ch/pfe/Brazil, accessed March 1st, 2018).

Our approach brings a further advantage relative to previous work relying on historical ethnic maps or expert assessments: traditional data sources typically postulate that there are sharp cut-offs based on historic "ethnic homelands" which often do not reflect the nuanced reality of actual settlement patterns. To illustrate this point, figure 1 shows the distribution of people of European ancestry (in the following called "Whites") across Brazil using IPUMS census data compared to *GeoEPR*. As can be seen, settlement patterns and sharp cut-offs assumed by *GeoEPR* do not capture important nuances in the distribution of Whites across the country. Moreover, our approach does not constrain us to the assumption that ethnic groups are uniformly distributed within their homeland, as we can derive their actual distribution directly from the census data. This is especially relevant when disasters hit areas inhabited by several ethnicities. Census data allow accurate measurement of the actual ethnicity shares in affected areas. *GeoEPR* data and similar data sources could at best reflect this by displaying overlapping ethnic homelands without indicating specific ethnicity shares in the overlap region. We are thus able – in contrast to much of the previous literature – to generate a continuous measure of ethnicity rather than exclusively relying on binary measurement.

Nevertheless, testing the effect of ethnic homelands is an interesting exercise allowing us to investigate the sensitivity of our results and to achieve a higher comparability to the leaders' birth region indicator described before. As a starting point, we define any ADM1 area with an ethnicity share of at least 90% as the homeland of that ethnicity. Such a cutoff alone would however be insufficient, as ethnic homelands also need to capture the relative importance of different areas from the respective ethnicity's point of view. For instance, ethnic minorities may never surpass the 90% cutoff in any region but members of these ethnicities may still attach particular importance to regions in which their population share is particularly high. We therefore additionally code any region as homeland that is among the top 10% of ADM1 areas with the highest population share of the *ethnic group*. It may further be the case that regions which are covered by this 10% rule do not necessarily have a substantially higher population share than those below this cutoff. We therefore also code any region with a population share no more than 10% below that of the region with the highest population share as ethnic homeland. Finally, it is possible that the ranking rule described before may result in situations where the top 10% of ADM1 areas with the highest population share covers both regions with very high and very low population shares if an ethnicity is mainly centered in one or two regions of a country. As final rules, we therefore require that any region with less than 50% of the population share that the ethnic group shows in the highest ranked region or any region with a population share of exactly zero cannot be an ethnic homeland.

Once the ethnicity of disaster victims is determined, we recode ethnicities as power groups in order to incorporate the power relations of ethnic groups to the central government. To this end, we match our data on ethnic groups to the ethnic categories included in *EPR* (Vogt et al., 2015). In addition to focusing on a leaders' birth regions, this allows us to take account of broader ethnic power relations within countries. *EPR* includes politically relevant ethnic groups and indicates whether and to what extent they have access to state power ("having a power monopoly," "being the dominant group," "sharing power with other ethnic groups," "facing discrimination," "being powerless," "irrelevance," or "self-exclusion"). For our analysis, we focus on victims that have a power monopoly, are dominant, or share power (henceforth "powerful"). We separately investigate the most powerful categories – those who govern without sharing power ("powerful (no coalition)"). Finally, we investigate whether ethnic groups that face discrimination ("discriminated") receive less aid.[21] We then use these categories to translate our continuous ethnicity distribution measures into continuous measures of power relations. Similarly, to allow comparisons with the birth region analysis, we generate

[21] Monopoly power implies that a group rules alone, while a group is dominant if other ethnic groups receive token representation in the executive. Groups that share power can either be junior or senior partners; "discrimination indicates an active, intentional, and targeted discrimination by the state against group members in the domain of public politics" (Vogt et al., 2015: p.1331).

binary indicators that are equal to one if at least one of the areas affected by a disaster is the homeland of a powerful or discriminated ethnicity, respectively.[22]

Given that *EPR* focusses on politically relevant groups, not all ethnicities included in censuses or surveys are listed by *EPR*, such that the above power groups do not apply to all ethnic groups in our sample. However, we consider it safe to assume that groups which are not politically relevant are neither powerful nor discriminated as a group such that this issue does not affect our constructed power measures. Moreover, in a number of cases, *EPR* has collapsed ethnic group categories in order to form larger power groups (based on the country-expert judgment mentioned before). We therefore recoded the original ethnicities to the extent possible so that they match *EPR* categories.[23] Note that the number of observations available for the ethnicity-based analysis is lower than for leaders' birth regions, as we only included countries for which census or survey data on ethnicities were available and that feature at least two power groups in *EPR*. In contrast to the birth region analysis, the time period is shortened to 1964 to 2013 as power relations are unavailable for the two most recent years of our sample.[24]

Control variables

To address the problems that regions affected by natural disasters may differ from other regions in ways that are correlated with power status and aid flows, we construct a range of control variables reflecting local area characteristics. In particular, we suspect that areas inhabited by political minorities are poorer (affecting the economic damage and number of casualties created by disasters as well as the relevance of disasters for the national economy at large) and harder to access (making disaster relief more difficult and costly).[25] For the construction of controls, we match the disaster locations indicated by EM-DAT with grid maps on population, nighttime light intensity, and ruggedness, and calculate zonal statistics using Geographic Information Systems (GIS) software. Similar to the calculation of ethnicity shares, we handle multi-location disasters by weighting disaster locations by the total population in each affected ADM1 or ADM2 area.

[22] It is hence possible that a disaster affects both the homeland of one or more discriminated groups and the homeland of one or more powerful groups.

[23] In rare cases, we are unable to find or recreate groups listed by *EPR*. These groups are typically small enough to not be a reason for concern.

[24] While we know exact start and end dates in the leader data, *EPR* always reports data rounded to full years. We therefore assigned power relations based on the start year of disasters.

[25] We do not make any attempts to control for the number of casualties directly as it is likely to be highly endogenous to aid disbursements. To the extent that disaster mortality is correlated with minority status, we expect poverty and population density to be the main driving force, such that it should be sufficient to control for these factors.

As a measure for economic importance as well as population concentration, we use the above procedure to construct a continuous variable for average population density per square kilometer in the year 2015.[26] We further use average annual cloud-free night time light intensity maps in combination with population figures, in order to construct a measure of average night time light intensity per capita, aiming to capture economic activity conditional on population density.[27] As night time lights are highly volatile across years, we exploit the fact that maps are available for multiple years (1992 to 2013) and take averages across time before taking averages across administrative regions. This approach brings the advantage that the influence of especially cloudy years, in which satellites can only measure night time light emissions during a limited number of days, is reduced.[28] Finally, 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 ADM1 and ADM2 areas. Similar to Nunn and Puga (2012), we scale the index such that it represents average elevation differences in hundreds of meters.[29]

3. Data description

Table 1 shows descriptive statistics by disaster type. The 38 countries included in our sample were hit by a total of 5,189 natural disasters, 12.1% of which received disaster relief from OFDA.[30] On average, 373 people died and 3.3 ADM1 regions were hit per disaster, corresponding to 13.8% of all ADM1 regions per country on average.[31] Given that disasters are geographically concentrated, we are able to identify the relationship of interest in a precise way in spite of the fact that the within-disaster distribution of aid flows and victims is not available.

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[26] We rescale the variable such that marginal effects are defined as the effects of increases in population density by 100 people per square kilometer.

[27] Our sources are the National Oceanic and Atmospheric Administration (NOAA, https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html) for night time light data and the WorldPop Project (http://www.worldpop.org.uk) for population figures.

[28] 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 hence considered measurement error a more severe problem. This is in particular the case as natural disasters have only limited impacts on population figures and economic activity, as mentioned earlier.

[29] Table A1 in the Appendix provides summary statistics as well as an overview of the sources and definitions of all variables.

[30] Disasters with missing or too vague location information where dropped from the sample. Moreover, for Vietnam which was split into two states during the Vietnam War, we excluded all years prior to 1976. Finally, for a small number of countries, time periods are reduced due to late independence.

[31] Note that zeroes in the casualty numbers indicated by EM-DAT can either mean missing or zero.

Table 1: Disaster impact and emergency relief, 38 countries, 1964-2015

Disaster type	Frequency	Percentage receiving any aid	Average number of casualties	Average percentage of regions hit
Earthquake	665	16.4	1,783.9 (15,346.4)	6.8 (9.3)
Epidemic	399	4.3	181.4 (617.8)	16.8 (21.5)
Extreme temperature	161	1.9	148.8 (335.8)	24.6 (26.5)
Flood	2,107	13.0	75.3 (283.3)	15.7 (19.8)
Insect infestation	9	11.1	0.00 (0.00)	9.2 (9.5)
Landslide	400	6.0	72.4 (194.8)	7.4 (11.9)
Mass movement (dry)	24	12.5	75.4 (87.7)	5.8 (6.6)
Storm	1,216	12.2	356.7 (5,671.1)	14.3 (17.2)
Volcanic activity	122	15.2	211.1 (2,059.6)	4.7 (3.8)
Wildfire	76	14.5	8.8 (24.6)	15.2 (19.2)
Multiple	20	100.0	306.3 (344.1)	45.6 (30.6)
Total	*5,189*	*12.1*	*373.1* *(6,175.8)*	*13.8* *(18.4)*

Numbers in parentheses are standard deviations.

In rare cases, OFDA donated aid to multiple disasters at once. In our analysis, we treated these cases as one event and assigned the disaster type "multiple" if these disasters were of different types. Going 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.[32] Interestingly, there are also important differences in the number of casualties and geographic spread of disasters. Broadly speaking, despite large variation, earthquakes are on average deadlier than floods or storms but are also spatially limited with only 6.8% of ADM1 regions in a country being affected, on average.[33]

[32] Note that this may be to some extent a consequence of EM-DAT's selection criteria described above.

[33] The rationale for expressing the spatial extent of disasters in this way is to account for the fact that sub-national regions vary in size across countries. Nevertheless, the observed difference may at least in part reflect the fact that EM-DAT location descriptions differ in accuracy across countries and that certain countries are more prone to be affected by certain disaster types.

Table 2: Key summary statistics by country

Country	Disasters	Percentage funded	Average percentage of regions hit	Percentage hit leader's birth region	Share powerful in percent		Share powerful (no coalition) in percent		Share discriminated in percent	
					mean	*sd*	*mean*	*sd*	*mean*	*sd*
Afghanistan	143	5.6	8.1	7.9	80.1	30.4	6.0	18.0	11.6	28.2
Argentina	86	11.6	12.1	27.9	n/a	n/a	n/a	n/a	n/a	n/a
Bangladesh	215	4.7	30.7	30.2	88.6	5.0	88.6	5.0	10.9	4.8
Bolivia	64	32.8	38.2	39.1	62.3	28.8	25.5	27.0	0.0	0.0
Brazil	164	12.8	6.3	16.5	59.6	33.6	34.5	31.6	0.0	0.0
Chile	68	13.2	20.5	27.9	90.8	5.9	90.8	5.9	1.7	4.4
China	709	4.2	7.0	9.0	81.7	24.5	81.7	24.5	3.4	14.5
Colombia	133	13.5	13.5	13.5	79.2	21.5	79.2	21.5	1.8	6.4
Costa Rica	46	47.8	35.7	21.7	79.4	8.5	79.4	8.5	1.5	3.2
Dominican Rep.	47	14.9	34.0	34.0	n/a	n/a	n/a	n/a	n/a	n/a
Ethiopia	68	10.3	16.5	24.1	39.8	38.7	1.9	7.8	35.9	43.3
Guatemala	68	14.7	22.6	36.9	61.2	25.0	61.2	25.0	9.7	22.5
Haiti	79	24.1	29.1	46.0	n/a	n/a	n/a	n/a	n/a	n/a
Honduras	45	28.9	23.6	25.6	n/a	n/a	n/a	n/a	n/a	n/a
India	521	7.7	6.5	6.0	92.1	18.9	0.0	0.0	0.0	0.0
Indonesia	400	12.5	4.9	8.5	38.1	38.7	19.0	30.7	0.6	1.3
Iran	167	4.2	6.7	8.4	n/a	n/a	n/a	n/a	n/a	n/a
Kenya	78	5.1	29.3	19.2	59.2	36.4	0.0	0.0	23.4	36.7
Madagascar	51	37.3	44.8	42.0	n/a	n/a	n/a	n/a	n/a	n/a
Malaysia	53	9.4	14.5	17.0	85.0	22.1	0.0	0.0	0.0	0.0
Mexico	191	10.0	7.7	4.7	70.0	19.9	70.0	19.9	1.7	7.6
Mozambique	71	22.5	31.8	36.2	17.3	26.6	0.0	0.0	0.0	0.0
Myanmar	46	23.9	15.7	19.6	n/a	n/a	n/a	n/a	n/a	n/a
Nepal	79	15.2	45.8	39.0	47.4	21.1	9.1	17.9	0.0	0.0
Nicaragua	49	34.7	20.0	18.4	87.7	16.1	87.7	16.1	0.0	0.0
Nigeria	97	3.1	7.6	6.2	62.8	35.1	5.2	18.2	0.7	4.3
Pakistan	155	13.6	20.2	18.2	54.6	35.2	0.0	0.0	6.0	14.1
Panama	45	28.9	19.1	33.3	71.2	20.2	71.2	20.2	0.0	0.0
Papua New Guinea	54	16.7	7.5	4.2	n/a	n/a	n/a	n/a	n/a	n/a
Peru	122	20.5	16.5	21.3	72.1	24.5	72.1	24.5	0.0	0.0
Philippines	437	13.3	11.7	21.3	68.2	25.1	68.2	25.1	3.8	11.0
South Africa	79	7.6	20.1	26.6	80.4	37.8	1.5	3.6	12.4	30.7
Sri Lanka	68	17.7	30.2	39.7	n/a	n/a	n/a	n/a	n/a	n/a
Tajikistan	53	28.3	31.3	43.4	87.7	5.7	87.8	5.8	0.0	0.0
Tanzania	63	7.9	8.6	9.5	n/a	n/a	n/a	n/a	n/a	n/a
Thailand	94	14.9	7.6	13.0	93.9	12.2	93.9	12.3	2.9	9.1
Turkey	117	12.0	4.5	13.7	75.9	21.8	75.9	21.8	17.7	20.4
Vietnam	164	15.9	9.9	11.0	82.6	23.1	82.6	23.1	0.0	0.6

We further provide an overview of all countries included in the analysis along with key summary statistics in table 2. The number of disasters included in the sample ranges from 45 in Panama to 709 in China, while the percentage of funded disasters varies between 3.1% (Nigeria) and 47.8% (Costa Rica).[34] There are further important differences in the geographic spread of disasters, with the average percentage of ADM1 regions hit by a disaster ranging from 4.5% in Turkey to 45.8% in Nepal.

[34] This does not involve any statement on the funding practice of the United States for disaster types not included in this analysis. Especially for African countries, complex emergencies and droughts are salient and receive generous support from the United States.

4. Empirical Strategy

We estimate a range of regression models to test the effect of power relations on the provision of disaster relief. We follow most previous work and estimate this relationship as a two-step process (Fink and Redaelli, 2011; Raschky and Schwindt, 2012). The first stage of the aid allocation model is the decision about whether or not to give aid; our dependent variable $Y_{e,c,t}$ is thus binary and takes the value of one if any aid is granted for a disaster *e* that took place in year *t* in recipient country c. The second stage measures the effect of favoritism on the amount of aid that was disbursed for a certain disaster, in (logged) 2015 US Dollars. As a starting point, we estimate empirical models of the following form:

$$Y_{e,c,t} = \beta_1 power_{e,c,t} + \beta_2 X_{e,c,t} + \gamma_c + \theta_e + \delta_t + \varepsilon_{e,c,t}. \quad (1)$$

Depending on the estimated model, $power_{e,c,t}$ is either a binary indicator that is equal to one if any ADM1 area hit by disaster *e* in year *t* in country *c* is the birth region of the leader governing the country at the time of the disaster, a continuous measure for the share of disaster victims belonging to the ethnic power groups, or the ethnic homeland as defined earlier.[35] $X_{e,c,t}$ is a vector of local area characteristics mentioned in the previous section (night light per capita, population density, ruggedness index). Note that while we constructed control variables for economic power and accessibility using time-invariant input data, they still vary between country-specific disasters to the extent that disasters hit different areas. Moreover, we include country fixed effects (γ_c) as we are interested in the effects of within-recipient-country factors. Additional specifications further include year-level fixed effects (δ_t) and disaster-type fixed effects (θ_e).[36]

To further test for omitted variables bias, we introduce a placebo model applicable to the binary exposure variables. The placebo models allow us to evaluate the presence of an important form of omitted variable bias. While our controls for local area characteristics capture the most obvious sources of confounding, powerful regions might differ from those that are not powerful in unobservable ways. Rather than comparing powerful regions to non-powerful regions, we compare them to regions that are not powerful at the time of the disaster but are

[35] This also includes the share or homeland of disaster victims belonging to a discriminated ethnic group at the time of the disaster.

[36] We also considered the inclusion of two variants of fixed effects at the ADM1 level. The first assigns a dummy variable to each area such that any disaster can potentially be assigned multiple fixed effects. In a more conservative version, we assign a separate fixed effect to each unique combination of ADM1 location, such that only disasters hitting exactly the same areas fall into the same group. Both tests are too conservative to have power.

powerful in the previous or subsequent period.[37] We therefore code disasters which did not hit any region that is powerful at the time of the disaster but hit at least one region which will be powerful in the next year (or, alternatively, used to be powerful in the previous year) with a binary indicator equal to one (and zero otherwise). The rationale for this test is that regions that just lost power or will gain power in the very near future exhibit the same underlying traits of currently powerful regions with the only difference that they do not currently control the central government. Where power leads and/or lags are statistically significant, the placebo test thus indicates that our identification strategy might suffer from omitted variables bias. Of course, such significant effects do not prove that regional and ethnic favoritism do not matter – regions that are represented in government at a certain time might as well already have power in the years immediately before entering power or after losing direct access to government. They would however make it impossible to distinguish such effects from omitted regional characteristics that are correlated with power and the probability to receive (large amounts of) humanitarian aid.

5. Results

Leader's birth region

We begin this section by examining whether disasters hitting the birth region of a country's leader exhibit a higher probability of receiving aid. Table 3 shows various specifications using our binary measure for birth region. Column 1 adjusts for country-level fixed effects exclusively. As can be seen, disasters hitting the leader's birth region have a 2.8 percentage points higher probability of receiving aid. To investigate the robustness of this result, we add local area characteristics of the disaster-affected regions (column 2), as well as disaster-type fixed effects and year fixed effects (column 3). Results are nearly identical.

Nevertheless, it may be possible that unobserved characteristics of powerful regions drive this relationship rather than the actual power status itself. We therefore perform the previously introduced placebo tests in columns 4 through 7. Column 4 adds a binary indicator that is equal to one if a disaster hits a set of currently not powerful regions that includes at least one region which used to be powerful in the year prior to the disaster. In a similar spirit, column 5 adds a binary indicator for disasters affecting a set of currently not powerful regions that includes at least one region which became powerful in the year after the disaster. To increase the statistical power of this exercise, we further pool the past-year and next-year placebos of

[37] Following the same logic, we can conduct placebo tests for homelands of discriminated rather than powerful ethnic groups.

columns 4 and 5 in the form of a joint placebo equal to one if a disaster hit regions which are currently not powerful but there is at least one region which used to be powerful in the previous year or became powerful in the subsequent year (column 6). Lastly, we extend the time frame to three years before and after a disaster to further increase the number of available placebo events (column 7).[38]

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

Outcome: any funding	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Birth region hit	0.028**	0.025*	0.032**	0.031**	0.028**	0.031**	0.026*
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Nightlight per capita		0.013	0.018				
		(0.040)	(0.032)				
Population density		0.000	-0.000				
		(0.000)	(0.000)				
Ruggedness index		-0.008**	-0.007*				
		(0.003)	(0.004)				
Placebo region hit (past)				0.070*			
				(0.041)			
Placebo region hit (next)					0.044		
					(0.045)		
Placebo region hit (both)						0.056*	0.040**
						(0.031)	(0.020)
Fixed effects							
Country FE	X	X	X	X	X	X	X
Disaster-type FE			X				
Year FE			X				
Observations	5,152	5,134	5,134	5,137	5,137	5,122	5,077
R^2	0.060	0.060	0.139	0.061	0.059	0.060	0.061

Coefficients are provided with heteroscedasticity-robust standard errors in parentheses below. Significance levels are: *** 1%, ** 5%, * 10%. Columns 4 to 6 conduct placebo tests using one year before and/or after a disaster, while column 7 extends the focus to +/- three years.

If we abstract 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, differences between our variable of interest and the placebo dummies can be understood as the net effect of disasters hitting the birth region of the current leader. As can be seen in table 3, while not always statistically significant, we find coefficients *larger* than our main estimate for both placebo indicators, suggesting that our initial estimates were upward- rather than downward-biased. When using the pooled version of the placebo test, as well as when extending the time period to three years around a disaster, the additional placebo tests confirm the potential upward bias

[38] There is a clear variance-bias-tradeoff here: While selecting periods which are further from the disaster date increases the power of statistical tests, it also raises the probability that other events than only power changes have occurred in between.

in our estimates. In summary, we find that birth regions are slightly more likely to receive humanitarian aid than other regions, but cannot rule out that this result reflects omitted variables bias rather than the causal effect of favoritism.

Table 4: Birth region effect on aid amounts in US dollars

Outcome: log funding	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Birth region hit	0.576***	0.601***	0.325*	0.591***	0.516***	0.538***	0.686***
	(0.176)	(0.176)	(0.176)	(0.177)	(0.180)	(0.181)	(0.184)
Nightlight per capita		-3.103	-1.789				
		(4.388)	(4.566)				
Population density		-0.005*	-0.002				
		(0.003)	(0.003)				
Ruggedness index		-0.083	-0.154**				
		(0.064)	(0.069)				
Placebo region hit (past)				0.255			
				(0.357)			
Placebo region hit (next)					-0.961**		
					(0.383)		
Placebo region hit (both)						-0.118	0.358
						(0.298)	(0.245)
Fixed effects							
Country FE	X	X	X	X	X	X	X
Disaster-type FE			X				
Year FE			X				
Placebo tests							
Bias-corrected p-values				0.0583	<0.0001	0.0003	0. 0758
Birth region vs. placebo (p-value)				0.3815	0.0002	0.0427	0. 2438
Observations	620	620	620	616	615	611	604
R^2	0.169	0.174	0.340	0.170	0.176	0.172	0.173

Coefficients are provided with heteroscedasticity-robust standard errors in parentheses below. Significance levels are: *** 1%, ** 5%, * 10%. Columns 4 to 6 conduct placebo tests using one year before and/or after a disaster, while column 7 extends the focus to +/- three years.

To investigate the influence of regional favoritism on the amount of humanitarian aid, we re-estimate all regression models from table 3, focusing on the log of disaster relief in 2015 US dollars as outcome variable (table 4). The number of observations is hence limited to disasters receiving any funding in the first place.[39] Our results show that disasters hitting

[39] An alternative would be to address potential selection effects using a Heckman model. We however have no reasonably excludable variable that would allow the identification of such model beyond exploiting the non-linearity of the selection equation and therefore do not explore this possibility.

leaders' birth regions receive substantially higher amounts of aid than disasters hitting other regions, with funding increasing approximately by between 38% and 82% (columns 1 to 3).[40]

We further repeat the placebo tests of table 3 and now find coefficients which are smaller than our main estimates or even negative. Importantly, in contrast to the birth region effect, none of the positive placebo coefficients is statistically significantly different from zero. Moreover, the birth region effect remains statistically significant when we deduct the omitted variable bias captured by the placebo dummies from the coefficient on birth region (row "bias-corrected p-values"), showing that even the statistically insignificant placebo coefficients that are positive are not sufficiently large to explain our findings. As a more conservative test, we directly test our birth region effect against the placebo estimate (row "birth region vs. placebo (p-value)").[41] While statistical power in the allocation regression is limited by a smaller sample size compared to the selection regression, we still reject two out of four tests for equality. Taken together, the placebo analysis suggests that our results are not driven by characteristics inherent to potentially powerful regions. We thus interpret our results as causal.

Ethnic favoritism

A further potential dimension of favoritism exists along ethnic lines rather than birth regions. Again, we first investigate the role of favoritism in the decision to grant aid (table 5). We include both the share of disaster victims belonging to the ethnicity currently in power and the share of disaster victims belonging to a discriminated ethnicity.

Column 1 shows the naïve regression model adjusted for country fixed effects. Column 2 further includes the full set of local area characteristics as well as disaster-type and year fixed effects. The results show that a one percentage point increase in the share of discriminated disaster victims is associated with a 0.14 to 0.16 percentage points increase in the probability to receive aid, with statistical significance at the one percent level. The share of powerful victims also registers a positive coefficient; the coefficient is significant at the five percent level once we include control variables as well as year and disaster-type fixed effects (in column 2). Our results thus imply that recipient country leaders can channel aid to their ethnic and regional

[40] 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.

[41] This test differs from the previous test in that we now also take into account the standard error of the placebo estimate. It can therefore be understood as a worst-case scenario which does not only take into account that the birth region effect may be smaller than the point estimate but also that the placebo effect could be larger than its point estimate. Moreover, placebo coefficients imprecisely estimated increase the conservative nature of this test to the extent that it increases the likelihood of Type II error due to inflated standard errors of placebo coefficients.

homelands, but that the donor does ensure that disasters hitting discriminated regions also receive aid, even though in this case one might assume that the national government is less likely to ask for assistance.

This finding holds when we replace the continuous measures by ethnic homelands as defined in the methods section above. Using these alternative measures (columns 3 and 4), we again find that both disasters hitting homelands of powerful groups and those affecting homelands of discriminated groups tend to receive aid more often, with effects ranging from 4.7 to 8.3 percentage points. When we focus on homelands of powerful groups who govern without a coalition, results are barely altered (column 5).

Table 5: Ethnic favoritism effect on the probability to receive humanitarian aid

Outcome: any funding	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share powerful	0.026	0.047**					
	(0.021)	(0.022)					
Share discriminated	0.161***	0.136***					
	(0.045)	(0.043)					
Homeland (powerful)			0.047***	0.053***		0.047***	0.055***
			(0.011)	(0.011)		(0.011)	(0.012)
Homeland (discriminated)			0.083***	0.066***	0.080***	0.090***	0.081***
			(0.020)	(0.020)	(0.020)	(0.021)	(0.022)
Homeland (powerful, no coalition)					0.050***		
					(0.014)		
Nightlight per capita		0.141		0.150			
		(0.361)		(0.359)			
Population density		-0.000		-0.000			
		(0.000)		(0.000)			
Ruggedness index		-0.005		-0.004			
		(0.004)		(0.004)			
Placebo homeland (powerful)						0.023	0.041
						(0.078)	(0.056)
Placebo homeland (discriminated)						0.056	0.017
						(0.064)	(0.033)
Fixed effects							
Country FE	X	X	X	X	X	X	X
Disaster-type FE		X		X			
Year FE		X		X			
Placebo tests							
Bias-corrected p-values (power)						0.0322	0.2573
Bias-corrected p-values (discriminated)						0.1047	0.0035
Power. homeland vs. placebo (p-value)						0.7541	0.8083
Discr. homeland vs. placebo (p-value)						0.6064	0.0998
Observations	4,176	4,158	4,176	4,158	4,176	4,016	3,701
R^2	0.060	0.138	0.064	0.141	0.063	0.066	0.070

Coefficients are provided with heteroscedasticity-robust standard errors in parentheses below. Significance levels are: *** 1%, ** 5%, * 10%. Column 6 conducts a placebo test using one year before and after a disaster, while column 7 extends the focus to +/- three years.

The advantage of the homeland variables is that we can conduct placebo tests similar to those performed in tables 3 and 4. We however limit our attention to the pooled placebo tests (i.e., not distinguishing between regions which were powerful in the previous or the subsequent year) as changes in the ethnic power status are much less frequent compared to changes in birth regions.[42] Column 6 of table 5 shows the test using one year before and after the disaster for this definition of placebos, while column 7 extends the range to +/- three years. While the placebo coefficients are positive, they are strictly smaller than our main estimates and remain statistically insignificant. P-values for bias corrected coefficients suggest that we again have sufficient statistical power to reject the hypothesis of no *net effect* in two out of four cases. The more conservative test incorporating the uncertainty in the placebo coefficients does not yield significant results in most cases. Taken together, while the statistical power of the placebo tests is limited, we find some evidence for upward bias for the coefficients of the powerful and discriminated ethnicities, although placebo coefficients are larger and more precisely measured for the latter.

Table 6: Ethnic favoritism effects on aid amounts in US dollars

Outcome: log funding	(1)	(2)	(3)	(4)	(5)
Share powerful	0.389	0.030			
	(0.284)	(0.315)			
Share discriminated	1.151**	0.271			
	(0.523)	(0.526)			
Homeland (powerful)			0.311*	0.160	
			(0.172)	(0.171)	
Homeland (discriminated)			0.664*	0.561	0.552
			(0.339)	(0.343)	(0.339)
Homeland (powerful, no coalition)					-0.028
					(0.199)
Nightlight per capita		-2.579		-2.731	
		(8.224)		(8.058)	
Population density		-0.003		-0.002	
		(0.003)		(0.003)	
Ruggedness index		-0.206***		-0.188***	
		(0.074)		(0.072)	
Fixed effects					
Country FE	X	X	X	X	X
Disaster-type FE		X		X	
Year FE		X		X	
Observations	502	502	502	502	502
R^2	0.162	0.341	0.167	0.346	0.161

Coefficients are provided with heteroscedasticity-robust standard errors in parentheses below. Significance levels are: *** 1%, ** 5%, * 10%.

Finally, we turn to the analysis of ethnic favoritism in the allocation of aid. Table 6 replicates the regressions of table 5. However, given the limited number of placebo events in

[42] In the total set of 4,176 disasters included in the ethnicity regressions, the placebo variable for powerful homelands equals one in 20 cases and the placebo variable for discriminated homelands equals one in 23 cases. The numbers change to 54 and 68 if we extend the scope to three years before and after a disaster.

this setting, we do not perform placebo tests in the sample of disasters receiving aid. As can be seen, there is no significant effect of the share of ethnicities that hold power on the amount of aid (columns 1 and 2). While we again find a positive and statistically significant coefficient for discriminated ethnicities in the basic model only adjusted for country fixed effects, this result is not robust to the inclusion of local area characteristics, disaster-type and year fixed effects (column 2).[43] Results are similar when we use homeland dummies instead of the continuous measure. While we find significant and positive coefficients in the less conservative regression of column 3, coefficients are no longer significant in column 4, where we control for local area characteristics, disaster-type and year fixed effects. There is also no significant effect when we focus on the most powerful ethnic groups only (in column 5).

Overall, our results for powerful ethnicities are less robust compared to those for birth regions discussed above. While we found the strongest effects of being the leaders' birth region on the *amount* of humanitarian aid, ethnic favoritism is stronger at the selection stage. However, contrary to the allocation stage of the birth region analysis, our preferred placebo test cannot separate the effects of holding office from unobserved regional characteristics in two out of four cases.

6. Tests for robustness and extensions

Alternative aid data

As mentioned earlier – while being the chief agency for granting humanitarian aid – OFDA is not the only US government agency that can get involved in disaster relief. In table 7, we therefore assess how representative our results are for US humanitarian aid overall, by replacing OFDA aid by total humanitarian aid provided by the US government, in years where OFDA reports such data (1964 to 2004).[44] In order to separate effects of changes in sample size from those caused by changes in the outcome variable, we first re-estimate basic and extended models corresponding to columns 1 and 3 of table 4, using only the 1964-2004 period (columns 1 and 3 in table 7). Despite the reduction in sample size, we find birth region effects that are remarkably similar to our analysis using the full range of years.

We then compare these models to identical models using total US government aid rather than OFDA aid only (columns 2 and 4). While the alternative outcome variable is likely to

[43] With a continuous exposure, the percentage effect on funding in US Dollars can be approximated by calculating $\beta_1 * 100$. As the ethnicities measures are however scaled such that they range from zero to one, we need to divide by 100 again (since we want to express marginal increases in terms of percentage points), and can hence interpret the coefficients directly as effect measures.

[44] This analysis is only done for the second-stage models, as we do not generally know whether other US agencies provided aid if OFDA did not, given that this information is based on OFDA's annual reports.

suffer from a higher degree of measurement error, given that OFDA reports on funding from other US agencies rely on information that these agencies provided to OFDA, birth region coefficients remain very similar in size and are statistically significant at the ten percent level at least, raising confidence in the representativeness of OFDA aid. We repeat this exercise for the corresponding ethnicity regressions of table 6. Given the lack of robust results in the regressions above, it is unsurprising that coefficients are not precisely estimated in most specifications of columns 5 to 8 (with a further reduction in sample size and the additional noise in the alternative aid data).

Table 7: OFDA vs. total US humanitarian aid amounts

	(1) *OFDA aid*	(2) *Total aid*	(3) *OFDA aid*	(4) *Total aid*	(5) *OFDA aid*	(6) *Total aid*	(7) *OFDA aid*	(8) *Total aid*
Birth region hit	0.475**	0.442*	0.422*	0.517**				
	(0.215)	(0.247)	(0.226)	(0.251)				
Share powerful					0.087	-0.077	0.194	0.055
					(0.318)	(0.423)	(0.404)	(0.463)
Share discr.					1.344**	0.626	0.740	-0.599
					(0.564)	(0.735)	(0.631)	(0.789)
Nightlight p.c.			-3.160	-4.364			-3.662	-6.719
			(4.847)	(5.356)			(8.977)	(10.099)
Pop. density			-0.002	-0.003			-0.002	-0.003
			(0.003)	(0.004)			(0.003)	(0.004)
Ruggedness			-0.188**	-0.251***			-0.200**	-0.311***
			(0.077)	(0.093)			(0.085)	(0.101)
Fixed effects								
Country FE	X	X	X	X	X	X	X	X
Disaster-type FE			X	X			X	X
Year FE			X	X			X	X
Observations	429	429	429	429	360	360	360	360
R^2	0.180	0.227	0.308	0.366	0.190	0.211	0.317	0.370

Coefficients are provided with heteroscedasticity-robust standard errors in parentheses below. Significance levels are: *** 1%, ** 5%, * 10%.

Regional differences

It is interesting to investigate whether the core results of our analysis are consistent across geographic regions or are rather driven by single world regions (table 8). In particular, much of the previous literature on ethnic favoritism has centered on Africa, such that it would be plausible if effects were driven by this sub-sample.[45] We therefore introduce a binary indicator for the Sub-Saharan African (SSA) countries in our sample which we interact with our exposure variables from the adjusted models (with country fixed effects, local area characteristics, disaster-type fixed effects and year fixed effects included) of tables 3 to 6. We find that favoritism in SSA does not differ significantly from the remaining countries in our sample, neither for leaders' birth regions (columns 1 and 2) nor the ethnicity-based measures (columns

[45] An important exception is De Luca et al. (2018) who have shown that ethnic favoritism is as important in other parts of the world.

5 and 6). A further possibility is that due to the geographic proximity and the historical involvement of the US government in local politics, favoritism is driven by Latin America and the Caribbean (LAC) region. Similar to before, we construct a binary indicator for LAC countries and interact it with our main exposure variables. Again, in spite of one highly significant negative interaction with the share of powerful victims in the first stage model (column 7), there are overall no systematic differences between LAC and non-LAC countries.

Table 8: Regional differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any aid	*Log funding*	*Any aid*	*Log funding*	*Any aid*	*Log funding*	*Any aid*	*Log funding*
Birth region hit	0.029**	0.397**	0.025*	0.427*				
	(0.015)	(0.188)	(0.015)	(0.232)				
Birth region hit * SSA	0.021	-0.571						
	(0.041)	(0.485)						
Birth region hit * LAC			0.023	-0.260				
			(0.032)	(0.357)				
Share powerful					0.043*	0.141	0.085***	0.169
					(0.024)	(0.348)	(0.023)	(0.384)
Share discriminated					0.122**	0.089	0.180***	0.747
					(0.057)	(0.617)	(0.045)	(0.610)
Share powerful * SSA					0.031	-0.799		
					(0.058)	(1.133)		
Share discriminated * SSA					0.047	0.248		
					(0.089)	(1.333)		
Share powerful * LAC							-0.170***	-0.259
							(0.059)	(0.667)
Share discriminated * LAC							-0.292	-2.692*
							(0.206)	(1.493)
Controls								
Country FE	X	X	X	X	X	X	X	X
Local area characteristics	X	X	X	X	X	X	X	X
Disaster-type FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Observations	5,134	620	5,134	620	4,158	502	4,158	502
R^2	0.139	0.341	0.139	0.340	0.138	0.343	0.140	0.345

Coefficients are provided with heteroscedasticity-robust standard errors in parentheses below. Significance levels are: *** 1%, ** 5%, * 10%.

US economic and political interests

We further investigate to what extent our results are moderated by US economic and geopolitical interests. 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 US imports from and exports to disaster-affected countries as shares of US GDP (columns 1, 2, 7 and 8 in table 9). To mitigate concerns about endogeneity and to reflect the fact that trade flows are volatile, we calculate the average trade flows of the past three years for each disaster-year. Political alignment is captured by a continuous measure for the share of votes in the United Nations General Assembly in which the disaster-affected countries were in agreement with the

United States (“UNGA” in table 9, columns 3, 4, 9 and 10).[46] Finally, we exploit the fact that our sample covers both disasters happening during the Cold War and after to test whether the fall of the Iron Curtain has affected favoritism in humanitarian aid (columns 5, 6, 11 and 12 in table 9).

Table 9: US economic and political interests

	(1) *Any aid*	(2) *Log funding*	(3) *Any aid*	(4) *Log funding*	(5) *Any aid*	(6) *Log funding*	(7) *Any aid*	(8) *Log funding*	(9) *Any aid*	(10) *Log funding*	(11) *Any aid*	(12) *Log funding*
Birth region hit	0.038**	0.310	0.114***	1.005**	0.035**	0.485**						
	(0.015)	(0.206)	(0.041)	(0.460)	(0.015)	(0.193)						
Share powerful							0.050**	0.149	0.133***	-0.069	0.047**	-0.085
							(0.024)	(0.386)	(0.047)	(0.675)	(0.022)	(0.385)
Share discriminated							0.122**	0.837	0.222**	0.480	0.140***	0.264
							(0.047)	(0.654)	(0.109)	(1.194)	(0.046)	(0.661)
Birth region * Trade	-0.016*	0.147										
	(0.009)	(1.045)										
Share powerful * Trade							-0.021	-0.360				
							(0.020)	(1.028)				
Share discr. * Trade							0.002	-1.023				
							(0.042)	(0.957)				
Trade	0.025***	-0.121					0.038*	0.241				
	(0.008)	(0.229)					(0.020)	(0.890)				
Birth region * UNGA			-0.276**	-2.344								
			(0.132)	(1.437)								
Share powerful * UNGA									-0.295*	0.220		
									(0.155)	(1.802)		
Share discr. * UNGA									-0.331	-0.845		
									(0.333)	(2.983)		
UNGA			0.018	-0.550					0.274	-2.961		
			(0.136)	(1.495)					(0.183)	(2.024)		
Birth reg. * Cold War					-0.015	-0.745*						
					(0.039)	(0.433)						
Share pow. * Cold War											0.001	0.305
											(0.041)	(0.604)
Share discr. * Cold War											-0.019	0.016
											(0.103)	(0.813)
Controls												
Country FE	X	X	X	X	X	X	X	X	X	X	X	X
Local area characteristics	X	X	X	X	X	X	X	X	X	X	X	X
Disaster-type FE	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Observations	4,941	612	5,094	613	5,134	620	4,148	498	4,122	494	4,158	502
R^2	0.136	0.344	0.141	0.344	0.139	0.344	0.137	0.352	0.139	0.346	0.138	0.341

Coefficients are provided with heteroscedasticity-robust standard errors in parentheses below. Significance levels are: *** 1%, ** 5%, * 10%.

As can be seen from table 9, trade with the United States does not significantly affect the degree of favoritism in humanitarian aid. The exception is column 1, which shows that the probability that birth regions receive aid decreases with larger trade with the United States: for every one percentage point increase in trade over US GDP with a disaster-affected country, the probability that disasters hitting leaders’ birth regions receive aid decreases by approximately 1.6 percentage points. US trade is likely spread across recipient regions with and without power, so that an allocation that is free of the recipients’ political motives best matches US interests.

The results also show that the probability to receive aid (but not the amounts of aid) decreases with UN General Assembly voting, at the five percent level of significance: for every

[46] The UN voting data are taken from Voeten et al. (2009) and contain several measures for proximity based on voting patterns. We use the fraction of votes for which the United States was in agreement with the respective recipient country (abstentions are counted as half-agreement). To mitigate endogeneity, we always use the fraction of votes that countries were in agreement with the United States the year before a disaster.

one percentage point increase in agreement in UN General Assembly voting, the probability that disasters in leaders' birth regions attract aid shrinks by approximately 0.3 percentage points.[47] A large literature investigates the extent of vote buying, emphasizing that aid can be used to buy support from relatively distant countries or rather to support allies (e.g., Vreeland and Dreher, 2014). According to our results, humanitarian aid is given to powerful regions more frequently in countries that are more distant to the United States, and therefore support the "vote buying" hypothesis. Taken together, our results suggest that geopolitical and economic ties to some extent affect the degree of favoritism in the allocation of humanitarian assistance.

Finally, table 9 shows that Cold War-period favoritism does hardly differ from those in the period thereafter (columns 5, 11 and 12). The only exception is the birth region-effect on the amount of aid given (column 6), which was significantly smaller during the Cold War-period compared to the time thereafter. If at all, favoritism did thus become more relevant in the more recent period rather than being pertinent to the Cold War.

7. Conclusion

Over the 1964-2015 period that is covered in our sample, natural disasters have killed 373 people per year, on average, in the countries included in the analysis. According to the United Nations' principles guiding the giving of humanitarian aid "[h]umanitarian assistance must be provided in accordance with the principles of humanity, neutrality and impartiality."[48] The United States' official policy is to provide humanitarian assistance "based on need alone" (US Government, 2002, cited in Kevlihan et al., 2014: 839).[49] At the same time, OFDA (2010) and USAID (2005) explain that a response to humanitarian disaster must be in the interest of the government of the United States.

The previous literature is mixed regarding the importance of the US government's political interests in granting disaster aid (Kevlihan et al., 2014). However, in order to ensure that aid is granted "based on need alone" it is essential to also take account of power relation within *recipient* countries that might influence aid decisions. In the first sub-national analysis of such interests, we have identified the importance of ethnic and regional favoritism for the

[47] Note that the interpretation of marginal effects depends on the scale of the interaction variable. Trade is expressed as percentage of GDP and reaches a maximum of 3.75. Voting agreement is instead expressed as fraction and ranges between zero and one. An increase in the latter variable by one unit hence reflects a change in voting agreement by 100 percentage points. The raw coefficient therefore needs to be divided by 100 to represent the consequences of an increase by only one percentage point. Moreover, as the outcome is binary in the selection equation, we multiply the coefficients by 100 in order to obtain the effect on the outcome in terms of percentage points, leading to the provided interpretations.

[48] UNGA A/RES/46/182, 78th plenary meeting on 19 December 1991 http://www.un.org/documents/ga/res/46/a46r182.htm (last accessed January 10, 2018).

[49] Also see OFDA (1991: 7), according to which "assistance is to suffering people, not governments."

allocation of disaster aid. According to our results, subnational regions where the leader of a country is born receive more humanitarian aid when hit by disasters. We corrected for the bias that might arise from powerful regions being different from other regions that are unrelated to current political connections, and find this result to be robust. While we also find small positive birth region effect on the probability to receive aid, we cannot rule out that this result is driven by unobservable characteristics of powerful regions. What is more, regions populated by powerful ethnic groups have a higher probability to receive aid. At the same time, aid amounts among disasters with positive aid flows are not affected by ethnic favoritism. Similarly, regions that are populated by ethnic groups which are discriminated against by the national government receive aid more often than those inhabited by politically irrelevant or powerless groups. However, while we were able to disentangle the effects of differences between regions with and without power more broadly from the causal effect of being involved in the executive at a certain point in time for the analysis focusing on birth regions, we are unable to fully do so for ethnic power. We believe that this part of our analysis provides interesting results and acknowledge that they have to be taken with a grain of salt.

We further investigated the heterogeneity of our main effects and found mixed results. First, our results show that the probability that disasters hitting a leader's birth region receive aid decreases with the amount of trade with the United States and UN General Assembly voting in line with the United States. It thus seems that governments that are commercially and politically more distant from the United States are best able to make use of the aid for their own domestic goals. Second, we found that, if at all, regional favoritism became stronger rather than weaker with the end of the Cold War. The mechanisms that we uncovered in this paper thus remain highly relevant.

Our results have important policy implications. To the extent that the recipient government's political considerations rather than need alone determine whether and how much aid is given in response to disasters, humanitarian aid influences existing regional and ethnic relationships and may increase regional inequality, as leaders' birth regions and ethnically powerful regions tend to be among the richer regions (Dreher et al., 2016). Whether and to what extent humanitarian aid indeed exacerbates inequality between sub-national regions is an important question that we leave for future research.

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Appendix

Table A1: Summary statistics by country

Country	N	Any aid (%)	Funding (in 1000 US-$)[a]	Casualties	Birth region hit (%)	Percentage regions hit[b]	Percentage powerful[c]	Percentage powerful (no coal.)[c]	Percentage discr.[c]	Night light per 1,000 people[d]	Population density[e]	Ruggedness[f]
Afghanistan	143	5.6	494.6 (718.6) [32.3,2071.4]	143.5 (502.2) [0.0,4700.0]	7.9	8.1 (10.3) [2.9,79.4]	80.1 (30.4) [0.0,100.0]	6.0 (18.0) [0.0,98.5]	11.6 (28.2) [0.0,98.4]	1.4 (1.8) [0.0,8.3]	5.1 (18.3) [0.1,134.7]	3.5 (1.8) [0.1,8.2]
Argentina	86	11.6	151.4 (191.6) [8.9,578.8]	11.6 (19.8) [0.0,100.0]	27.9	12.1 (13.5) [4.2,100.0]				71.0 (45.0) [5.9,267.2]	10.1 (27.5) [0.0,149.5]	0.6 (0.9) [0.1,5.9]
Bangladesh	215	4.7	2340.9 (2781.5) [35.9,7447.3]	833.2 (9519.0) [0.0,138866.0]	30.2	30.7 (22.2) [14.3,100.0]	88.6 (5.0) [42.4,96.4]	88.6 (5.0) [42.4,96.4]	10.9 (4.8) [3.5,53.5]	2.3 (0.7) [0.3,4.6]	15.0 (10.3) [1.6,82.4]	0.1 (0.1) [0.0,1.2]
Bolivia	64	32.8	231.8 (255.6) [9.9,916.0]	25.9 (37.8) [0.0,250.0]	39.1	38.2 (33.8) [11.1,100.0]	62.3 (28.8) [14.0,99.7]	25.5 (27.0) [0.0,83.7]	0.0 (0.0) [0.0,0.0]	20.7 (9.4) [6.1,59.0]	2.6 (2.5) [0.0,15.4]	1.7 (1.0) [0.1,3.4]
Brazil	164	12.8	131.0 (166.4) [5.4,715.6]	59.1 (159.7) [0.0,1500.0]	16.5	6.3 (5.5) [3.7,40.7]	59.6 (33.6) [0.0,100.0]	34.5 (31.6) [0.0,90.3]	0.0 (0.0) [0.0,0.0]	33.7 (13.6) [7.7,71.6]	22.1 (23.6) [0.0,84.0]	0.5 (0.4) [0.0,2.9]
Chile	68	13.2	1737.4 (3028.4) [14.8,9115.4]	31.2 (88.8) [0.0,562.0]	27.9	20.5 (20.6) [6.7,100.0]	90.8 (5.9) [71.6,99.0]	90.8 (5.9) [71.6,99.0]	1.7 (4.4) [0.0,19.7]	41.3 (20.3) [14.4,142.5]	4.0 (6.0) [0.0,20.2]	2.9 (1.0) [1.0,7.1]
China	709	4.2	310.2 (489.0) [33.6,2078.2]	614.3 (9702.7) [0.0,242000.0]	9.0	7.0 (8.4) [3.2,100.0]	81.7 (24.5) [1.7,99.9]	81.7 (24.5) [1.7,99.9]	3.4 (14.5) [0.0,97.6]	12.3 (8.4) [2.2,70.5]	5.4 (5.9) [0.0,58.9]	2.0 (1.1) [0.0,7.7]
Colombia	133	13.5	827.6 (1482.5) [9.7,5274.4]	225.3 (1890.2) [0.0,21800.0]	13.5	13.5 (21.8) [3.0,87.9]	79.2 (21.5) [0.8,99.4]	79.2 (21.5) [0.8,99.4]	1.8 (6.4) [0.0,40.3]	23.7 (11.8) [0.2,74.7]	12.0 (17.1) [0.0,77.0]	1.9 (0.9) [0.0,5.5]
Costa Rica	46	47.8	151.2 (149.8) [6.4,525.0]	6.6 (10.1) [0.0,47.0]	21.7	35.7 (23.5) [14.3,85.7]	79.4 (8.5) [37.3,88.0]	79.4 (8.5) [37.3,88.0]	1.5 (3.2) [0.0,14.8]	51.3 (16.2) [25.4,115.6]	6.6 (9.7) [0.1,39.2]	2.4 (0.7) [1.0,4.3]
Dominican Rep.	47	14.9	1318.3 (2518.1) [55.4,6858.0]	35.7 (114.1) [0.0,688.0]	34.0	34.0 (37.4) [3.1,100.0]				20.7 (5.5) [11.7,46.0]	10.6 (9.6) [0.6,44.9]	1.4 (0.7) [0.2,3.6]
Ethiopia	68	10.3	154.2 (147.4) [32.8,400.5]	40.4 (73.0) [0.0,311.0]	24.1	16.5 (14.9) [8.3,75.0]	39.8 (38.7) [0.0,99.8]	1.9 (7.7) [0.0,48.8]	35.9 (43.3) [0.0,99.5]	0.7 (0.9) [0.0,3.9]	14.2 (49.6) [0.2,226.9]	1.6 (1.2) [0.1,6.2]
Guatemala	68	14.7	651.7 (1551.6) [14.6,4961.9]	379.5 (2790.4) [0.0,23000.0]	36.9	22.6 (26.7) [4.5,100.0]	61.2 (25.0) [4.6,94.0]	61.2 (25.0) [4.6,94.0]	9.7 (22.5) [0.0,91.4]	13.4 (6.5) [0.9,41.3]	10.9 (17.3) [0.2,56.3]	2.2 (1.0) [0.2,5.0]
Haiti	79	24.1	19803.4 (72705.6) [17.6,319070.8]	2994.9 (25036.2) [0.0,222570.0]	45.9	29.1 (28.0) [10.0,100.0]				1.4 (0.8) [0.0,2.9]	10.5 (11.7) [1.2,37.1]	2.6 (0.7) [1.1,4.4]
Honduras	45	28.9	217.2 (227.0) [37.4,836.3]	89.2 (418.1) [0.0,2800.0]	25.6	23.6 (27.1) [5.6,100.0]				20.4 (7.1) [2.2,35.2]	3.0 (2.5) [0.1,7.7]	2.2 (0.8) [0.4,4.8]
India	521	7.7	1089.2 (2853.3) [19.2,14793.6]	302.0 (1301.8) [0.0,20005.0]	6.0	6.5 (7.0) [2.9,52.9]	92.1 (18.9) [1.5,100.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	8.6 (5.0) [0.6,32.0]	15.9 (42.9) [0.0,503.0]	1.1 (1.7) [0.0,8.3]
Indonesia	400	12.5	468.4 (1373.7) [5.4,7427.8]	487.6 (8288.9) [0.0,165708.0]	8.5	4.9 (5.9) [2.6,57.9]	38.1 (38.7) [0.1,99.9]	19.0 (30.7) [0.0,99.6]	0.6 (1.3) [0.0,9.7]	6.6 (5.8) [0.0,84.5]	15.9 (30.6) [0.1,168.5]	1.3 (0.7) [0.1,3.3]
Iran	167	4.2	2184.4 (3873.6) [35.3,10733.5]	714.6 (4247.1) [0.0,40000.0]	8.4	6.7 (7.2) [3.2,45.2]				111.4 (239.6) [9.8,3071.1]	1.2 (4.0) [0.0,48.9]	3.1 (1.8) [0.1,9.6]
Kenya	78	5.1	1856.6 (2573.3) [37.3,5576.4]	41.0 (116.2) [0.0,1000.0]	19.2	29.3 (23.0) [12.5,87.5]	59.2 (36.4) [0.3,99.9]	0.0 (0.0) [0.0,0.0]	23.4 (36.7) [0.0,99.7]	1.7 (1.5) [0.1,7.2]	7.4 (11.7) [0.1,57.8]	0.8 (0.8) [0.1,3.6]
Madagascar	51	37.3	566.8 (696.2) [29.8,2820.4]	79.2 (155.5) [0.0,860.0]	42.0	44.8 (27.8) [16.7,100.0]				0.8 (0.5) [0.0,2.3]	0.6 (0.3) [0.2,1.6]	1.2 (0.2) [0.8,1.8]
Malaysia	53	9.4	308.5 (533.6) [35.9,1260.7]	14.4 (38.7) [0.0,270.0]	17.0	14.5 (10.6) [6.7,53.3]	85.0 (22.1) [4.8,98.3]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	30.5 (11.6) [3.2,53.8]	9.7 (11.2) [0.2,43.1]	0.7 (0.4) [0.1,2.0]
Mexico	191	9.9	986.4 (2437.4) [31.1,10631.6]	81.2 (690.0) [0.0,9500.0]	4.7	7.7 (9.2) [3.1,100.0]	70.0 (19.9) [1.9,94.5]	70.0 (19.9) [1.9,94.5]	1.7 (7.6) [0.0,62.0]	43.3 (15.4) [6.9,117.2]	13.8 (25.0) [0.0,124.2]	1.8 (1.0) [0.0,5.1]
Mozambique	71	22.5	1826.3 (3310.3) [34.4,13854.5]	58.9 (127.5) [0.0,800.0]	36.2	31.8 (24.6) [9.1,100.0]	17.3 (26.6) [0.0,99.0]	0.0 (0.0) [0.0,0.0]	0.0 (0.0) [0.0,0.0]	2.8 (2.2) [0.0,7.4]	7.7 (10.1) [0.1,46.3]	0.5 (0.2) [0.0,1.1]
Myanmar	46	23.9	3902.3 (11418.6) [9.6,38294.0]	3067.6 (20392.6) [0.0,138366.0]	19.6	15.7 (16.0) [6.7,80.0]				2.7 (2.5) [0.1,15.0]	1.7 (1.9) [0.1,8.6]	1.5 (0.9) [0.1,4.5]
Nepal	79	15.2	2964.6 (9452.5) [14.0,32965.7]	233.9 (1004.3) [0.0,8831.0]	39.0	45.8 (28.7) [20.0,100.0]	47.4 (21.1) [17.9,100.0]	9.1 (17.9) [0.0,62.2]	0.0 (0.0) [0.0,0.0]	1.9 (0.8) [0.0,3.3]	2.5 (1.0) [0.2,4.3]	5.0 (1.4) [1.8,7.7]
Nicaragua	49	34.7	333.2 (702.8) [40.0,2978.4]	220.4 (1426.6) [0.0,10000.0]	18.4	20.0 (22.4) [5.9,100.0]	87.7 (16.1) [26.7,100.0]	87.7 (16.1) [26.7,100.0]	0.0 (0.0) [0.0,0.0]	11.9 (7.0) [0.5,22.6]	4.6 (4.7) [0.0,16.7]	1.0 (0.3) [0.1,2.2]
Nigeria	97	3.1	58.3 (55.7) [19.8,122.2]	205.1 (787.6) [0.0,7289.0]	6.2	7.6 (8.8) [2.7,48.6]	63.5 (34.9) [0.0,99.2]	5.3 (18.4) [0.0,94.0]	0.7 (4.3) [0.0,40.8]	4.3 (5.8) [0.2,38.5]	32.1 (55.5) [0.8,367.1]	0.3 (0.2) [0.0,1.1]
Pakistan	155	13.5	16250.0 (52549.8) [32.8,231781.0]	656.8 (5934.7) [0.0,73338.0]	18.2	20.2 (15.1) [12.5,87.5]	54.6 (35.3) [0.0,98.6]	0.0 (0.0) [0.0,0.0]	6.0 (14.1) [0.0,99.7]	11.3 (5.8) [0.0,27.5]	33.2 (65.0) [0.1,283.8]	2.3 (2.7) [0.0,9.5]
Panama	45	28.9	109.6 (123.0) [0.2,429.7]	7.2 (11.5) [0.0,48.0]	33.3	19.1 (17.6) [7.7,100.0]	71.2 (20.2) [24.0,97.2]	71.2 (20.2) [24.0,97.2]	0.0 (0.0) [0.0,0.0]	23.7 (7.6) [5.5,34.0]	3.8 (5.1) [0.0,19.4]	1.4 (0.6) [0.6,3.2]
Papua New Guinea	54	16.7	165.1 (114.4) [38.1,333.8]	60.7 (297.1) [0.0,2182.0]	4.2	7.5 (8.0) [4.3,34.8]				0.5 (0.6) [0.0,2.1]	10.5 (22.3) [0.4,110.7]	1.9 (0.8) [0.5,3.4]

Table A1 continued: Summary statistics by country

Country	N	Any aid (%)	Funding (in 1000 US-$)[a]	Casualties	Birth region hit (%)	Percentage regions hit[b]	Percentage powerful[c]	Percentage powerful (no coal.)[c]	Percentage discr.[c]	Night light per 1,000 people[d]	Population density[e]	Ruggedness[f]
Peru	122	20.5	1164.8 (3127.3) [19.2,15209.5]	628.6 (6043.0) [0.0,66794.0]	21.3	16.5 (22.6) [4.0,100.0]	72.1 (24.5) [9.8,99.9]	72.1 (24.5) [9.8,99.9]	0.0 (0.0) [0.0,0.0]	16.2 (7.6) [3.1,37.5]	4.5 (8.3) [0.0,31.9]	2.1 (0.9) [0.0,4.6]
Philippines	437	13.3	1411.3 (4494.0) [5.8,33129.2]	119.4 (577.8) [0.0,7354.0]	21.3	11.7 (13.8) [1.3,97.5]	68.2 (25.1) [0.3,99.5]	68.2 (25.1) [0.3,99.5]	3.8 (11.0) [0.0,91.1]	4.6 (2.3) [0.0,16.0]	34.9 (59.5) [0.5,267.7]	1.3 (0.7) [0.1,5.1]
South Africa	79	7.6	70.0 (72.8) [32.8,217.7]	25.3 (64.8) [0.0,506.0]	26.6	20.1 (17.3) [11.1,88.9]	80.4 (37.8) [1.4,100.0]	1.5 (3.6) [0.0,16.7]	12.4 (30.7) [0.0,94.4]	36.8 (14.1) [10.2,85.4]	6.3 (6.0) [0.0,27.8]	2.4 (0.9) [0.3,4.6]
Sri Lanka	68	17.6	185.1 (260.9) [39.9,897.0]	33.4 (68.0) [0.0,346.0]	39.7	30.2 (19.6) [11.1,88.9]				13.2 (3.0) [3.1,19.9]	9.9 (9.1) [0.6,36.0]	0.7 (0.6) [0.0,2.7]
Tajikistan	53	28.3	555.1 (1234.2) [34.4,4813.7]	10.3 (25.9) [0.0,168.0]	43.4	31.3 (19.0) [20.0,80.0]	87.7 (5.7) [77.5,94.9]	87.7 (5.7) [77.5,94.9]	0.0 (0.0) [0.0,0.0]	17.5 (11.8) [0.0,68.5]	7.6 (12.7) [0.0,40.3]	3.4 (2.3) [0.8,9.3]
Tanzania	63	7.9	60.7 (20.7) [41.2,95.8]	64.1 (263.8) [0.0,2025.0]	9.5	8.6 (10.1) [3.3,50.0]				1.8 (1.9) [0.0,6.8]	7.7 (16.5) [0.1,118.6]	0.6 (0.4) [0.0,1.6]
Thailand	94	14.9	224.8 (394.4) [26.8,1203.6]	42.8 (121.7) [0.0,813.0]	13.0	7.6 (9.4) [1.3,53.2]	93.9 (12.3) [28.1,100.0]	93.9 (12.3) [28.1,100.0]	2.9 (9.1) [0.0,54.9]	24.9 (10.7) [3.9,81.0]	3.4 (7.9) [0.2,37.3]	1.1 (0.6) [0.0,2.8]
Turkey	117	12.0	3838.6 (8553.6) [16.5,32615.9]	285.4 (1649.0) [0.0,17127.0]	13.7	4.5 (12.8) [1.2,98.8]	75.9 (21.8) [20.6,95.6]	75.9 (21.8) [20.6,95.6]	17.7 (20.4) [0.0,58.5]	49.3 (53.1) [10.1,530.0]	5.2 (11.9) [0.0,98.1]	2.7 (1.3) [0.5,6.0]
Vietnam	164	15.9	274.9 (325.5) [34.4,1126.7]	92.5 (308.2) [0.0,3682.0]	11.0	9.9 (12.1) [1.6,95.2]	82.6 (23.1) [4.4,100.0]	82.6 (23.1) [4.4,100.0]	0.0 (0.6) [0.0,6.9]	6.4 (2.5) [0.5,14.3]	5.2 (8.9) [0.4,92.9]	2.0 (1.2) [0.0,6.1]
All countries	5,189	12.1	1981.4 (16374.3) [0.2,319070.8]	373.1 (6175.8) [0.0,242000.0]	16.7	13.8 (18.4) [1.2,100.0]	71.9 (31.5) [0.0,100.0]	47.5 (41.2) [0.0,100.0]	4.1 (14.6) [0.0,99.7]	17.7 (49.9) [0.0,3071.1]	12.2 (31.3) [0.0,503.0]	1.7 (1.5) [0.0,9.6]

Table displays means, (standard deviations) and [min,max] for the main variables used in the analysis. For binary indicators only means are reported.

[a] Reported funding is in constant 2015 US-$. Source: OFDA annual reports for the fiscal years 1990 to 2015; Drury et al. (2005) for the years 1964-1989.

[b] Percentage of regions hit is the number of ADM1 regions that are hit by a disaster relative to the total number of ADM1 regions in a country times 100. Source: EM-DAT international disaster database, Guha-Sapir (2018).

[c] Percentage powerful, percentage powerful (no coal.) and percentage discr. refer to the estimated percentage of victims in each disaster belonging to a powerful (with/without coalition) or discriminated ethnicity, respectively. Source: Ethnicity data from IPUMS and DHS. Ethnic categories are from *EPR* (Vogt et al., 2015).

[d] 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, population-weighted averages were created (as further explained in the article) before calculating the means, SD and value ranges depicted in this table. Source: National Oceanic and Atmospheric Administration (NOAA), https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html.

[e] Population density is scaled in terms of 100 people per km^2. Source: WorldPop Project, http://www.worldpop.org.uk.

[f] Ruggedness is scaled in terms of 100s of meters of elevation differences, based on the Terrain Ruggedness Index that indicates average differences in elevation per 30 arc seconds grid cell. Source: Nunn and Puga (2012).