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# Pro-social behavior after a disaster: parochial or universal? Evidence from a natural experiment in Belgium

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JEL codes: D10, D64, L31.

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# Pro-social behavior after a disaster: parochial or universal?

# Evidence from a natural experiment in Belgium

(December 2016)


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## 1. Introduction

Beside their dire direct consequences, traumatic events such as wars or disasters have paradoxically been found to elicit positive societal responses . In their survey and meta-analysis of the literature on the impact of war exposure, Bauer et al. (2016) report overwhelming evidence that people exposed to war violence tend to behave more cooperatively and altruistically after the war. In other words, their behavior is more pro-social. The finding holds across countries, settings, and types of violence, in a range of situations, real as well as experimental. People exposed to war-related violence tend to increase their social participation by joining more local social and civic groups or taking on more leadership roles in their communities. Importantly, they also take more actions intended to benefit others, such as altruistic giving.

An emerging literature shows that natural disasters, such as earthquakes or tsunamis, have similar effects. In her book on the social consequences of disasters, Solnit (2009) collected historical evidence on earthquakes and hurricanes in the US and Mexico illustrating that people reacted with mutual aid and altruism. Rao et al. (2011) investigate pro-social behavior in the wake of the Wenchuan earthquake that hit China in 2008. They found that survey respondents from the devastated areas declared that they would give a larger amount in a dictator game and would devote more time to volunteer work compared to participants from non-devastated areas. They observed that, although the effect was stronger immediately after the earthquake, it persisted a year later. Caló-Blanco et al. (2016) exploit spatial and temporal variation in exposure to earthquakes in Chile, and find that exposure to earthquakes had a positive effect on several indicators of social cohesion, such as voting and donations to charities, and reduced the crime rate. Similarly, Cassar et al. (2012) use a series of experimental games that elicit trust, risk and time preferences in villages affected and not affected by the 2004 tsunami in Thailand. They find that the tsunami led to substantial increases in pro-social behavior and conclude that natural disasters fundamentally change people and the communities in which they live, often fostering a sense of community.

All that accumulated evidence points to a consistent effect of disasters on pro-social behavior towards the in-group, also referred to as parochial altruism. However, whether this positive effect of disasters extends to the out-group has not been investigated yet. However, if disasters affect the mindsets of the people that they hit, there is no reason to expect their effect to be restricted to parochial altruism only. Determining whether it does is important, because while parochial altruism may enhance intragroup cooperation and facilitate post-disaster

reconstruction, a lack of universal altruism may result in tensions with other groups. Testing whether a disaster can affect universal altruism, and in which direction, is the aim of the present paper.

To do so, we study the change in donations to an ongoing campaign to collect funds in Belgium for famine relief in Africa around the time that a disaster hit Belgium. Specifically, a catastrophic storm struck the Belgian outdoor pop festival of Pukkelpop, in the Flemish region of Limburg, on 18 August, 2011. Among the 60,000 festival participants, several hundreds were wounded, and five died. The disaster received intense media coverage. A few weeks before the storm, a consortium of humanitarian organizations had made an appeal to solicit donations from the Belgian public to combat famine in the Horn of Africa. Such donations are undeniably an act of altruism towards a group of people, (inhabitants of the Horn of Africa) who do not belong to the group hit by the disaster, (Belgians living in Flanders.) A single bank account was open for the duration of the campaign, from 22 July to 1 October, 2011. The disaster thus occurred in the midst of the fundraising campaign. The consortium granted us access to the bank balance sheet recording every donation received during the campaign. We can identify individual campaign contributions at the municipality-day level, and finely trace the impact of the disaster on donations not only over time, but also as a function of the proximity of municipalities to the disaster.

Our paper therefore complements previous studies that have investigated the impact of disasters on donations to determine whether donations for different disasters are substitutes or complements, in other words whether charities compete for donations. Using household data from the U.S. Panel Study of Income Dynamics, Brown et al. (2012) observe that donations for the victims of the 2004 Asian tsunami were positively correlated with subsequent donations to other charitable causes. Deryugina and Marx (2016) study the impact of tornado strikes in the United States on donations by US citizens as reported in tax declarations provided by the Internal Revenue Service. They observe that donations increase in a state in the two years after it was hit by a tornado that claimed more than five victims. Using a dataset on charitable contributions stemming from accounts run by the Charity Aid Foundation (CAF) in the United Kingdom, Scharf et al. (2016) study the impact of six fundraising appeals for disasters abroad on charitable contributions directly related to the appeals and donations to other charitable causes, domestic or foreign. They find that appeals increase charitable contributions to their own cause, and affect the timing of donations to other causes but not total donations to other causes over the whole disaster period.

Our study complements and improves those studies of the impact of disasters on donations in several ways. First and foremost, our dataset draws a clear distinction between the domestic location of the disaster, a Flemish music festival, and the foreign location of the cause of the fundraising campaign, famine relief in the Horn of Africa. We can therefore study the impact of a domestic disaster on donations to an overseas cause, and thereby observe the impact of the disaster on out-group altruism. We thus contrast with Brown et al. (2012) and Scharf et al. (2016), who study the impact of disasters abroad, and with Deryugina and Marx (2016), who study the impact of domestic disasters on total donations reported to the tax authority. Second, all those studies study the impact of disasters on donations to permanent causes. In other words, they look at how unforeseen disasters affect recurrent donations. By contrast, we test whether a disaster can affect donations to a different temporary emergency.

This is important, because determining whether donations to one charity reduce donations to others has important implications for the efficiency of the charity sector. In a recent contribution where they study the competition between charities in a spatial model where the number of charities is endogenous, Aldashev and Verdier (2009) show that the efficiency of the charity market depends on whether the size of the pool of donors is fixed or can increase with the number of charities. If the size of the market is fixed, then the number of charities may be larger than optimal from the point of view of beneficiaries, because of the common pool problem. Charities devote too many resources to attracting donors and not enough to the cause itself. Conversely, if the size of the market increases with communication efforts, then each charity generates a positive externality on others, because it raises the awareness of donors, thereby increasing the pool of donations. In that case, the number of charities can be too small.

Our dataset has several other desirable features that improve on other studies. First, by providing daily donation data, it allows us to finely track the timing of the effect of the disaster. Papers that use tax or survey data, such as Brown et al. (2012) or Deryugina and Marx (2016), can only assess the effect of disasters over a horizon of at least a year. Secondly, donations for famine relief were made through a standard bank account. This means that there was no fixed cost of giving, because domestic bank transfers are free and common in Belgium, making them a very standard way to make an invoice. Therefore, anyone could contribute. In particular, donors did not have to register, and there was no lower limit on donations. We can therefore study the impact of the disaster on all donors, as opposed to a set of regular registered donors making above-average donations. This distinguishes our data from the set

used by Scharf et al. (2016), because CAF accounts impose a minimum amount for donations. It also distinguishes our data from the IRS data used by Deryugina and Marx (2016), because the IRS does not report data for ZIP codes with fewer than a threshold number of donors, to preserve donors' anonymity.

We observe that a positive gap emerged between donations to the famine relief campaign between the municipalities that were closer to the disaster and those that were farther away in the days following the Pukkelpop festival disaster. The effect of the disaster was larger the closer the donors' municipalities to the municipality where the festival was organized. Other measures of proximity lead to similar results. This suggests that a foreign alternative charitable cause in fact benefitted from the local disaster. The rest of the paper is organized as follows. The next section describes the context of the fund raising campaign and of the disaster. Section 3 provides a theoretical discussion of the potential effects of the disaster on donations. Section 4 introduces our empirical strategy and our data, while Section 5 reports our results. Section 6 concludes.

## 2. The famine relief campaign and the local disaster

### *2.1. The Campaign to Stop Famine in the Horn of Africa*

Since the 1984 Ethiopian famine, a consortium of five large humanitarian organisations – Caritas, Handicap International, Doctors of the World, Oxfam and UNICEF – has joined forces to solicit donations from the Belgian public in the event of a large crisis. They do this though the common bank account with number 000-0000012-12, making the campaigns known to the public as "12-12". According to the president of the consortium, the single account and the accompanying campaign allow the public to express its solidarity at times when large amounts of money are needed in a short time span (Todts, 2011, p.2). Working as a consortium boosts the visibility and the efficiency of collecting donations far beyond the capacities of a single organisation. According to Todts (2011, p.6.) it reduces costs and avoids undue competition for the same goals.

Each campaign is a combination of radio, TV and social media events, documentaries, live testimonies, and news bulletins. An average citizen using radio, TV, newspapers, or social media is aware of the campaign. It cannot escape one's attention.

By launching temporary appeals related to specific causes, the consortium aims to increase visibility and leverage the public's emotion. As Todts (2011) argues, "When the public makes a donation in the event of a large crisis, he or she is doing that spontaneously, out of emotion. The citizen does not want to express this emotion by contributing one's more to the NGO that he or she regularly supports, but rather looks for a channel to express his own humanity in the face of so much suffering" (p.7).

Figure 1. The 12-12 Consortium logo


Recently, the consortium organized nationwide campaigns in response to the Asian tsunami (2004-2005), the earthquake in Haiti (2010), the flooding in Pakistan (2010), and the Campaign to Combat Famine in the Horn of Africa (2011, see Figure 1).

The consortium has been successful in raising money following several disasters. For instance, it collected 54 million euro after the Asian tsunami and 23 million euro after the earthquake in Haiti. It collected 8.8 million euro for the campaign to stop famine in the Horn of Africa that we study in this paper (Todts, 2011, p. 24-27).

### *2.2. The Pukkelpop Festival*

The Pukkelpop Festival is the second largest outdoor summer pop and rock festival in Belgium organized just outside the city of Hasselt. Hasselt is the capital of the province of Limburg, part of the region of Flanders in the east of Belgium, about 25 km from the Dutch border. The festival ground is a large meadow with a series of large trees providing shade for participants on sunny days. Around 60,000 people, most between 15 and 35 years old, attend it every year. It enjoys an international reputation among rock bands and rock audience for the high quality of its organization and its line-up. Every year, the performances and the atmosphere at the festival ground are widely covered by Belgium's national television, radio and newspapers. Performers are interviewed, the organizer is invited into talk shows, ticket sales are monitored, members of the audience are interviewed, and so on. The reason for this

nationwide attention is that Belgium is a small country, so music events of this kind attract an audience from every corner of the country. During the summer, the music festival scene in Flanders and Belgium is very lively and is a popular place for people to socialize during the summer. In its June 18, 2016 issue (p.24), the newspaper De Standaard (the leading paper in Flanders) described the experience of connectedness as the key ingredient of the summer festival scene.

### *2.3. The Development of a Super Cell and the Unfolding of the Disaster*

In the early afternoon of 18 August 2011, a few weather cells with a potential for storm moved from France into southwest Belgium. Strong but not yet catastrophic winds accompanied by heavy rain developed between the city of Mons and Brussels. The trajectory of the cells is depicted on Map 1. The storm continued its path over the northern part of the city of Leuven without much damage. It then went in the direction of the city of Hasselt, increasing its power. Upon arrival above the Pukkelpop Festival venue, around 6 pm that day, everything suddenly became dark, with lightning and very heavy rain. Meteorologists describe a super cell as producing "falling winds" that destroy everything in their path. In technical terms, the super cell became a bow echo with a booked echo (vortex), as can be derived from radar images (see Map 2 and Figure 1). Such a booked echo is well known in the meteorological literature and signals very strong winds because of the presence of a vortex at the mid-level (Hamid, 2011, p.3).[1]

Festival participants described wind so strong that it knocked down giant trees on the festival ground as if they were wooden matches. The first person to be killed was a young man who died when the construction of one of the huge tents collapsed and one of the poles fell on him. The British rock band Skunk Anansie was playing right at that moment. Band leader Skin later declared in an interview that she had never experienced anything like that. She was literally blown of the stage and had to run for her life.

The unfolding of the disaster could be followed live on radio and television. The media was already at the venue before the storm given the nationwide coverage of the festival, so the entire Belgian public was informed immediately about what was going on. It is important to know that public radio in Belgium is organized by regionalized broadcasting companies (VRT for the Flemish part and RTBF for the Francophone part). No other

[1] The technical explanation of the development of the supercell into a bow echo with bookend echo (vortex) is based on a technical note from the Royal Meteorological Institute in Brussels. (Hamid, 2011).

broadcasting companies have nationwide radio coverage. For news items, most Belgians listening to the radio are tuned to these public channels. As the evening progressed, Belgians turned their TV sets on and learned that five people had died at the Festival, that all other acts at the festival were cancelled, and that thousands of participants were leaving the venue. Because of the overburdening of the local mobile phone antennas (60,000 people calling at the same time), many parents could not get hold of their children. Some jumped into their cars to find them and bring them back home. This caused massive traffic jams several hours into the disaster. Belgian TV also reported that hundreds of local residents opened up their houses to give shelter to participants.

The morning after the disaster all Flemish newspapers put large pictures of the disaster on their front pages, with stories of sorrow and heroism. The papers published stories about parents and children not finding one another, editorials questioning the safety of the tents, interviews with weather specialists, and so on. The daily newspaper “Het Belang van Limburg”, which is the most widely read newspaper in the province of Limburg, devoted almost its entire edition to the calamity. This amount of reporting continued during the following days.

Given the regionalized and linguistic political organization of Belgium (Dutch-speaking Flanders in the north, French-speaking Wallonia in the south, and Brussels as a bilingual capital in the center), we focus on Flanders because the disaster took place there. The reason is that the media is organized at the region-linguistic level, meaning that the Flemish audience rarely tunes into the francophone media and vice-versa. As the calamity happened in Flanders, that region was primarily affected.

Map 1: The Development of the super cell above the Pukkelpop Festival Location near the City of Hasselt

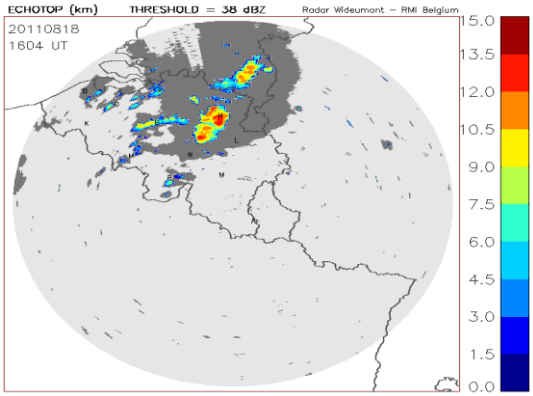



Figure 4: the power of the wind in the super cell

Figure 5: The day after, destruction at the Pukkelpop Festival campsite

All pictures taken by Hamid (2011) and published in his technical note.

## 3. Conceptual framework

Anecdotal evidence suggests that disasters may compete for donations. For instance, in the aftermath of the 2004 Asian tsunami, the NGO Doctors Without Borders stopped accepting tsunami relief donations and requested donors to contribute to its general Emergency Relief Fund instead, because it had received enough funds to finance its operations related to the disaster. The belief that charities compete is also widely held among practitioners, as Aldashev and Verdier (2010) report.

An intuitive rationale is that if donors devote a fixed budget to donations, then donations to a new cause will necessarily decrease donations to existing causes. Scharf et al. (2016) moreover remark that if people can achieve a given level of warm glow through support to various good causes, then getting warm glow from one cause would reduce the need to get warm glow from another. They refer to that phenomenon as a form of “moral licensing”.

In the case of the Pukkelpop disaster, those arguments would imply that, because people living around the disaster contributed to helping festival participants, and were later offered the possibility to contribute to a fund to support the victims of the disaster, they had a smaller budget to devote to the famine relief campaign. They might have viewed it as less important, because they had already catered to another good cause.

Using individual panel data, Reinstein (2011) finds that the within-individual correlation of donations to different types of charity tends to be negative, suggesting that

charities are substitutes and compete for donations. Ek (2015) uses a real-donation laboratory experiment where participants can contribute to different charities supporting different causes. He finds that contributions to one charity come at the expense of other contributions.

In contrast to those results, three classes of mechanisms may have prompted the Pukkelpop disaster to increase donations to famine relief: fear and guilt, empathy and identification, and social norms.

Firstly, a disaster typically elicits fear and guilt. The terror management theory elaborated by Greenberg et al. (1986) posits that the fear of mortality prompts people to follow cultural norms. Any event that makes death more salient, such as a disaster, would therefore increase their propensity to follow cultural norms. If cultural norms value generosity, then people should give more, as Jonas et al. (2013) observe. More generally, Tedeschi and Calhoun (2004) developed the psychological concept of post-traumatic growth to capture the profound changes in beliefs, attitudes and behavior observed in individuals after a traumatizing event. Their work describes how affected people become more civic-minded after a disaster. Increasing one's solidarity with other people in the wake of a disaster fits that pattern.

Scharf et al. (2016) provide a formal model of the effect of disasters on donations that emphasizes the role of guilt. Their starting point is that a potential donor has to devote his or her budget not only to alternative charities but also to consumption. A disaster will admittedly raise the marginal utility to donate to a charity related to the disaster by raising the salience of that charity, but may at the same time reduce the marginal utility of consumption, by increasing guilt. That effect would prompt the donor to reduce consumption, thereby increasing his or her total donations. If the second effect dominates, a disaster may result in increased donations to charities that are unrelated to the disaster.

Secondly, a disaster affecting one group of people may increase their empathy with the victims of other disasters. As Lévy-Garboua et al. (2006, p. 601) recall, empathy, defined as "an other-oriented emotional response congruent with the perceived welfare of another person", has been repeatedly found to increase help to individuals in need. Stürmer et al. (2006) provide evidence suggesting that their results extend to groups, not just individuals. Moreover, similarity of experience between the helper and the beneficiary facilitates empathy-motivated help, as Batson et al. (1996) observed. Andreoni and Rao (2011) manipulated empathy in a dictator game by randomly allocating the roles of dictator and receiver only after the two subjects had been asked to fill a form where they indicated what

they would do if they were the dictator and the other the receiver. Comparing the outcome of the treatment with the outcome of standard dictator games, Andreoni and Rao (2011) observed that dictators who had been put in the receiver's shoes transferred significantly more to the receiver than standard dictators. A disaster may therefore increase empathy between the group of people who experienced or witnessed the disaster, and other people suffering from another disaster, prompting the former to increase donations to the latter. In our case, Belgian donors witnessing the disarray of their fellow citizens may have felt more sympathetic to Africans suffering from famine, because both groups had been hit by a disaster.

Thirdly, a disaster may increase the visibility of pro-social behaviors and the salience of pro-social norms of behavior. The social comparison theory, put forward by Festinger (1954), posits that individuals compare themselves to others especially when objective standards do not exist. One consequence is that individuals should be prompted to act more pro-socially and donate more if they see that others act pro-socially and donate more. This argument has been borne out by field experiments. Frey and Meier (2004) report that students who were asked to donate to a social fund reacted to information about past donations. In the field experiment, a random sample of students were informed that 64 percent of students had donated in the past, while another sample of students were informed that only 46 percent of students had donated. Frey and Meier (2004) observed that students who had been told the larger share of past students had donated were more likely to donate. Shang and Croson (2009) performed a related experiment in an on-air fund raising campaign for a radio station. When calling the station, potential donors would be informed of the donation of one other donor. Shang and Croson (2009) observed that donors would donate more if the announced amount was larger than the median of donations. Both experiments show that the behavior of peers affects donations.

Social pressure may complement the role of peers. The role of social pressure was observed by DellaVigna et al. (2012) in a field experiment. They took advantage of a door-to-door fundraising campaign where a subset of target households were informed in advance of the exact time when they would be solicited, allowing them to avoid meeting a fund-raiser. They found that households who had been warned were less likely to open their door than those who had not been warned, suggesting that they would avoid the pressure cost of saying no to a fund-raiser. A disaster prompting solidarity may exacerbate the social pressure to donate.

In the case of the Belgian disaster, potential donors were confronted with a large display of generosity, as the neighbors of the festival mobilized to help the victims of the

festival. One may contend that they may have tried to conform to a norm of generosity by donating to the famine relief campaign.

In line with the contention, Brown et al. (2012), Deryugina and Marx (2016), and Scharf et al. (2016) document that donations to specific disasters were positively correlated with donations to other charities. However, those results are only indicative, as they do not pertain to the impact of a domestic disaster on a faraway cause, such as the one that we study. Lange and Stocking (2012) report the outcome of a field experiment suggesting that donations in cash may complement direct help. They study the donors' response to being invited by e-mail by one charity to volunteer for the other. They find that the total donations to the two charities from donors who were invited to volunteer were larger than those from donors who had not been invited. Accordingly, time and money donations could be complementary. In the case of the Pukkelpop festival, people around the festival initially contributed direct help. If donations in time and money are complementary, people around the festival may therefore have also increased their donations in cash to the combat famine in the Horn of Africa campaign.

The mechanisms discussed above, positive or negative, may be amplified by the media coverage of disasters. Olsen et al. (2003) argue that the sevenfold difference between the emergency aid received by India following the 1999 cyclone and Mozambique following the 2000 floods can be explained by the fourfold difference in media coverage received by the two disasters in the US and Western Europe. Brown and Minty (2008) use data on individual internet donations in the US following the December 2004 Asian Ocean tsunami. They find that an additional minute of nightly news coverage on network television increased online donations by up to 20.8% on the same day. Because television networks were on the premises to cover the festival, the Pukkelpop disaster was immediately covered on Belgian televisions. It then was the lead story of newscasts and newspapers for several days.

## 4. Data and Empirical Identification Strategy

### *4.1. Data*

We had access to the detailed statement of account of Consortium 12-12 during the combat famine in the Horn of Africa campaign. The database was handed to us in an anonymized

version by Consortium 12-12. The data contains information on the day each donation was received, the zip code of the donor, and the amount of the donation (in euro). As the Combat Famine in the Horn of Africa Campaign started on July 22, 2011 and lasted till September 30, 2011, the disaster at the Pukkelpop Festival occurred almost in the middle of the campaign, on August 18, 2011.

We combine these data with geographic information on the municipalities of the donors, including the distance of each municipality to the Pukkelpop site as well as the fact that the site is located in the Flemish region and the province of Limburg. The data contain a total of 84,614 donations, of which 779 (0.91 %) exceeded 1,000 euro. We exclude these large donations from our analysis, because they are most likely donated by institutional donors such as companies or banks.

For Flanders, there are 534 different zip codes in the donation data, representing all municipalities in Flanders. There are 308 municipalities in Flanders , many of which used to be subdivided in smaller entities that were merged into one larger municipality in a reform of 1977. Some of these smaller entities kept their own zip code, which is why we have more zip codes than municipalities in the data.

The average number of non-zero donations per zip code is 123, and the average amount of non-zero donations is 65 euro per zip code. In the province of Limburg, the average non-zero amount was 61 euro in the three weeks before the disaster and 67 euro in the three weeks after, whereas in the rest of Flanders it was 65 euro before and 66 euro after.

### *4.2. Method*

Our identification strategy rests on the idea that Flemish municipalities received a treatment on 18 August 2011, and that the intensity of the treatment was related to their proximity to the disaster. All our estimations are therefore variants of the following equation:

$$donation_{it} = \gamma_i + \lambda_t + \beta.after_t*disaster_i + u_{it} \quad (1)$$

where: - $donation_{it}$ is the average donation from municipality $i$ on day $t$;

- $after_t$ is a dummy variable taking the value one after 18 August 2011;
- $disaster_i$ is a variable measuring the proximity of municipality $i$ to the disaster;
- $\gamma_i$ is a municipality fixed effect;
- $\lambda_t$ is a day fixed effect;

- $\beta$ is a coefficient;
- $u_{it}$ is the error term.

We include municipality fixed effects to control for the unobserved characteristics of municipalities that may result in different donation values, for instance income. As we observe donations over a couple of months, the municipality fixed effect will capture most of the difference across municipalities, except those related to the disaster, as other characteristics of municipalities, such as income or demographics, are unlikely to evolve significantly over such a short time span. The day fixed effect allows controlling for the evolution of donations over time that is common to all municipalities, like the time that has elapsed since the beginning of the appeal.

The critical coefficient measuring the impact of the disaster on donations is $\beta$, which the coefficient of the interaction term. If it turns out significant, then it means that donations in municipalities hit by the disaster followed a different path from donations in other municipalities after the disaster. If it is significantly negative, it means that the local disaster resulted in lower donations for famine relief in Africa in the municipalities hit by the disaster. This would imply that the two causes are substitutes in the eyes of donors. Conversely, if the coefficient is significantly positive, it means that the local disaster prompted donors in affected municipalities to give more for famine relief. The two charities may therefore be complements.

## 5. Findings

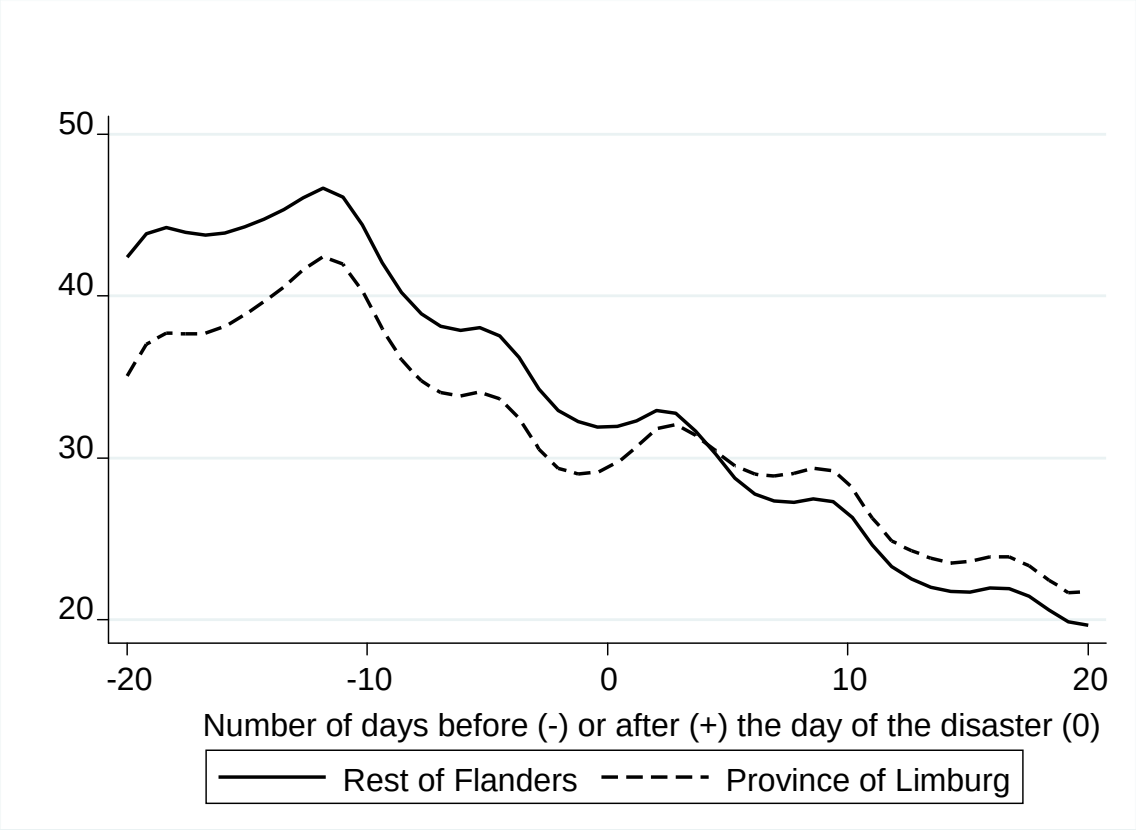
In this section we first describe the evolution of donations around the disaster. We then report our baseline econometric results.

### *5.1. A first look at the data*

We illustrate our empirical identification strategy first by examining the non-parametric relationship between the amount of a donation and the date on which the donation took place. In Figure 6 we estimate a kernel-weighted local polynomial regression of the amount on the date of transfer using an Epanechnikov kernel. Our unit of analysis here as well as in the remainder of the analysis is the municipality-day level. The average non-zero donation was 32 euro per municipality per day. This includes days were donations were zero for some municipalities. Figure 6 separately plots the day-by-day evolution of average

donations in municipalities located in Limburg and in the rest of Flanders for the three weeks before the disaster and the three weeks thereafter. The dotted line plots average donations in Limburg while the solid line those in the rest of Flanders.

Figure 6: Mean Amount per Donation before and after the Disaster (in euro), Province of Limburg versus the rest of Flanders




Both curves display a downward trend, with amounts being higher in the first weeks, which corresponds to the timing of the start of the campaign. This suggests some erosion of interest a few weeks after the beginning of the appeal for donations in July. One may also remark that average donations were initially lower in Limburg than in the rest of Flanders. This observation is in line with Limburg being the poorer province of Flanders. For our purpose, however, the trend is not the key issue.

What really matters is the evolution of the difference in donations between Limburg and the rest of Flanders. Two points must be made here. First, before the disaster, the trend in Limburg follows that of the rest of Flanders. In other words, the trends were parallel in the

treated and non-treated groups of municipalities before the disaster. The data therefore meet the common trend assumption necessary for a differences-in-differences estimation.[2]

Second, we observe a difference emerging right on the day of the disaster. Average donations in Limburg start increasing at a faster pace than donations in the rest of Flanders. After a couple of days, donations in Limburg exceed those in the rest of Flanders. One can therefore observe a differential evolution of donations between the region hit by the disaster and the rest of Flanders after the disaster, only emerging after the disaster. The disaster accordingly had a positive impact on donations in Limburg.

### *5.2. Benchmark results*

In our baseline specification, we consider that only the municipalities located in the same province as the festival were hit by the disaster. We therefore define $disaster_i$ as a dummy variable taking the value one if municipality *i* belongs to the province of Limburg. It therefore distinguishes Limburg from the rest of Flanders. This makes sense as the province-level, situated in the administrative hierarchy between the region (Flanders) and the municipality, is regarded as a geographic/administrative entity that is an integral part of the self-identification and self-description of Belgians. The results of that specification are reported in Table 1 below.[3]

*** Insert Table 1 around here ***

The first column of the table reports the results of the estimation assuming the event window starts one week before the disaster and ends one week after the disaster. The second column considers an event-window starting two weeks before and ending two weeks after the disaster. The third column considers respectively three weeks before and after the disaster. In all three specifications, the coefficient of the interaction term between the Limburg and After

[2] Table A1 in the appendix substantiates that finding. In that table, we regress donations in municipalities on municipality fixed effects, a time-trend, and an interaction between the trend and a Limburg dummy, for several time windows before the disaster. While the trend is statistically significant, the interaction term between the time trend and the Limburg dummy is insignificant in all four regressions. In other words, regardless of the width of the study window, the trend in Limburg can never be statistically distinguished from the trend in the rest of Flanders, at standard levels of confidence.

[3] For the differences-in-differences identification to work, trends must be parallel in Limburg and the rest of Flanders prior to the disaster. Table A1 in the appendix shows that this was the case by regressing donations on a trend and an interaction of the trend with the Limburg dummy, during the pre-disaster part of the window. While the trend is statistically significant, the interaction never is, implying that Limburg's trend did not differ from that of the rest of Flanders before the disaster.

dummies is significant at the five percent level or beyond. The coefficient is always positive, implying that municipalities in the province where the disaster occurred started giving more money for famine relief in Africa after the disaster. In addition, the magnitude of the coefficient is stable across specifications, ranging from 5.27 to 6.24. In other words, municipalities from Limburg started donating five to six more euros per day to famine relief than other municipalities in Flanders.

One way to gauge the quantitative significance of the effect is to recall that the Stop Hunger in the Horn of Africa Campaign, in the three-week period after the disaster, received 1,446 contributions between 1 and 1,000 euros from people residing in the province of Limburg, with an average of 67 euro per contribution. The estimated effect of the Pukkelpop disaster is therefore roughly equivalent to ten percent of the total amount contributed by the residents of Limburg in these three weeks. Moreover, if we multiply the number of donors by the estimated average effect of the Pukkelpop disaster on the amount of individual donations, we can derive that the campaign received a total 'surplus' in the order of 7,600 to 9,000 euros in the three weeks following the disaster.[4]

## 6. Robustness checks and extensions

Our baseline specification opposes the province where the disaster happened, Limburg, to other Flemish provinces. Here we assume that the impact of the disaster stopped at the border of Limburg with the rest of Flanders. However, municipalities close to the disaster but located outside of Limburg could also have been affected by the disaster. We therefore estimate another specification, where we measure the intensity of the treatment by geographic distance to the city of Hasselt, the capital of Limburg, which is located very close to the disaster. To let the effect be non-linear, we define four concentric circles. The first includes all municipalities located within a 20-kilometer radius from Hasselt. The second circle includes all the municipalities located between 20 and 50 kilometers from Hasselt. The third circle features the municipalities that are located between 50 and 100 kilometers from Hasselt. Municipalities located further than 100 kilometers belong to the fourth circle. We created a dummy variable for each circle and interacted it with the After dummy. Table 2 below reports the results of the estimations using those interaction terms.

[4] One should notice that those estimates are conservative, because they rest on the implicit assumption that donors from other provinces were not affected by the disaster. If donors from other provinces were also affected then the effect of the disaster on total donations may be larger.

*** Insert Table 2 around here ***

Like in Table 1, we broaden the study window from one week to three weeks before and after the disaster. The first column reports the results obtained with the narrowest study window. The coefficients of the interaction terms are all negative, but none is significant at standard levels of significance, suggesting that the window is likely too short to let us identify reactions that are specific to concentric circles.

When the study period is extended to two weeks before and after the disaster, like in Column 2.2, the coefficients remain negative, and all become significant at the ten-percent level or beyond. This shows that, after the disaster, all concentric circles gave less than the first circle, which was the closest to the disaster. By symmetry, it means that municipalities located within twenty kilometers of the disaster started giving more than the others after the disaster. We obtain the same result when the study window is extended to six weeks, as reported in Column 2.3.

We performed a series of t-tests to compare the three coefficients. The tests cannot reject the hypothesis that the coefficients of "Circle 2 * After" and "Circle 3 * After" are equal. They however do reject the hypothesis that the coefficients of "Circle 3 * After" and "Circle 4 * After" are equal. The difference is significant at the ten-percent level, and nearly at the five-percent level. As the coefficient of "Circle 4 * After" is lower than the other two, it suggests that the effect of the disaster on donations was larger closer to the disaster.

The previous table allows the effect of distance to be non-linear, but pools together all the days following the disaster. Yet it stands to reason that the effect takes time to materialize, and probably fades away as time goes by. Moreover, the disaster occurred on a Thursday and was then followed by a weekend. Finally, an appeal to donations to a fund supporting the victims of the Pukkelpop festival was launched exactly a week after the disaster on Thursday, August 25. To take into account the possibility that the effect varied over time, we estimated the differences in differences model with time-varying coefficients. Specifically, we defined three dummy variables capturing three periods: the weekend immediately following the disaster, the week following the disaster from Monday to Thursday, when the Pukkelpop donations appeal was announced, and a period starting after the Pukkelpop donations appeal.[5]

[5] We include Thursday, 25 August in the period between the weekend and the appeal, because the appeal was announced around mid-day, and the earliest press article commenting on it was posted online after 2pm on that day. Therefore, the appeal really became effective on the following day. However, we also estimated a

We defined three dummy variables over those periods, and interacted them with the Limburg dummy. Table 3 reports the outcome of the regressions controlling for those interactions.

*** Insert Table 3 around here ***

The first column of Table 3 reports the outcome of a regression where the study window is only two weeks long. As the window does not go beyond the first week, it does not allow estimating the coefficient of the "Limburg * Between the weekend and the appeal" interaction term. In the regression, the coefficient of the interaction of the disaster with the dummy variable capturing the first weekend is statistically insignificant, suggesting that donors did not really react to the disaster in the first two days following the event. The coefficient of the second interaction term, however, is positive and significant at the five-percent level, suggesting that donors living in the province of the disaster indeed reacted by giving more than other donors in the first week following the disaster. The effect estimated here can be interpreted as a pure effect of the disaster, as no appeal to donations for the victims of the festival had been made.

Columns 3.2 and 3.3 report the same specification, but can include interaction terms for the period starting after the Pukkelpop appeal. Their results are consistent with those of Column 3.1. They show that donations in Limburg municipalities were not different from those of other municipalities during the weekend following the disaster. However, the coefficient of the interaction of "Limburg * Between the weekend and the appeal" is positive and significant beyond the five-percent level in both regressions, like in Column 3.1. In addition, they show that the effect lasted after the Pukkelpop appeal, as the coefficient of the "Limburg * After the appeal" variable is positive and statistically significant in both regressions, at the five-percent level in Column 2 and at the one-percent level in Column 3. In all regressions, t-tests reject the hypothesis that the coefficient of the first interaction term is equal to the other two. Accordingly, the disaster only started affecting donations after the weekend following the festival.

The coefficient of the "Limburg * Between the weekend and the appeal" dummy is larger than the coefficient of the "Limburg * After the appeal" dummy. However, t-tests cannot reject the hypothesis that the two coefficients are equal. This is important, because it shows that the launch of the appeal for donations to the fund supporting the victims of the

specification where 25 August was part of the after-appeal period. The difference between the coefficients of the dummies capturing the before- and after-appeal periods was smaller, but the results were qualitatively unchanged.

festival, at the end of the first week, did not significantly affect donations for famine relief in Africa.

One may also remark that the magnitudes of the coefficients of the interaction terms estimated in Regressions 3.2 and 3.3 are very close. This means that our results are robust to the length of the study window.

The timing of the disaster is critical to our identification strategy. One may therefore worry that the effect that we capture is an artifact or due to luck. To address that concern, we perform a series of placebo tests on the date of the disaster. Specifically, we run the same regressions as before, but move the date used to define the After dummy around the true day of the disaster. We move it forward and backward by ten and fifteen days around August 18. The results of the series of placebo tests is reported in Table 4.

*** Insert Table 4 around here ***

When doing so, we in general find that the coefficient of the interaction term fails to be significant at standard levels of statistical significance. This is true for any placebo date for the two, four, and six weeks windows. Accordingly, the effect that we capture truly reflects a change around the day of the disaster and is not driven by chance.

One might wonder whether the effect of the disaster on average donations that we observe is driven by an increase in a subset of donations. To see if this the case, we replace average donations by median donations in our baseline estimations. The result of those regressions is reported in Table 5.

*** Insert Table 5 around here ***

The results reported in Table 5 are strikingly in line with those of Table 1. Specifically, we observe that the coefficient of the interaction variable is positive and significant at the five-percent level or beyond, regardless of the length of the study window. Moreover, the magnitude of the estimated effect is the same as in the baseline regression, specifically between five and six euros per day. As the results obtained for the mean and the median donations are the same, we can conclude that the effect is not limited to a particular subset of donations, but likely affected all of them regardless of their size.

As an extension to our main results, and as a way to interpret our results, we also applied our baseline estimation to the number of donations per municipality instead of

average donations. The idea is to test whether the impact of the disaster ran through the intensive or extensive margin. The results of that alternative estimation are reported in Table 6.

*** Insert Table 6 around here ***

The results show no effect of the disaster on the number of donations. More precisely, the coefficient of the interaction between Limburg and the After dummy is never significant at standard levels of statistical significance, regardless of the length of the study window. This suggests that the impact of the disaster essentially operated through the size of donations rather than through the number of donors.

## 7. Concluding comments

This paper has provided evidence that a disaster affecting one country can prompt its residents to behave more pro-socially towards beneficiaries located in a foreign country faraway. Specifically, the residents of the Belgian province of Limburg increased their donations to a campaign for famine relief in the horn of Africa after a disaster that hit a local outdoor music festival. The estimated order of magnitude of the effect evolves around ten percent of donations. Placebo tests lend credence to the assumption that the observed variation in donations was caused by the disaster. Conversely, we find no effect of the disaster on the number of donations, which suggests that the impact operated at the intensive margin.

The effect was larger in municipalities located closer to the disaster. It materialized in the days that followed the festival and lasted at least three weeks. Furthermore, it lasted even after the festival organizers launched their own appeal for donations, suggesting that charities do not necessarily compete for funds.

Although we can quantify the effect of the disaster on donations and find that it operated at the intensive margin, we can only make hypotheses about the exact behavioral mechanisms that were at work. Also, whereas we observe that the effect lasted at least three weeks, we cannot determine when it stopped. Finally, we focus on one type of pro-social behavior and one type of traumatic event, and cannot claim that our results can be generalized to other behavior and events. Future research should target those unanswered questions.

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## Tables

Table 1: Baseline results. Limburg vs. rest of Flanders

| | (1.1) | (1.2) | (1.3) |
|---|---|---|---|
| Length of the window | Two weeks | Four weeks | Six weeks |
| Limburg * After | 5.270 | 5.270 | 6.236 |
| | (2.109)** | (2.797)*** | (3.637)*** |
| Constant | 35.34 | 27.12 | 50.73 |
| | (16.63)*** | (10.93)*** | (23.79)*** |
| Municipality fixed effect | Yes | Yes | yes |
| Day fixed effect | Yes | Yes | yes |
| Observations | 8,010 | 15,486 | 21,894 |
| R-squared | 0.186 | 0.172 | 0.154 |
| Adjusted R-squared | 0.157 | 0.150 | 0.173 |
| Number of municipalities | 534 | 534 | 534 |

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Concentric circles around the disaster.

| | (2.1) | (2.2) | (2.3) |
|---|---|---|---|
| Length of the window | Two weeks | Four weeks | Six weeks |
| Circle 2 * After | -2.986 | -5.464 | -4.796 |
| | (-0.819) | (-1.903)* | (-2.011)** |
| Circle 3 * After | -2.869 | -4.448 | -5.645 |
| | (-0.882) | (-1.685)* | (-2.609)*** |
| Circle 4 * After | -2.897 | -5.374 | -3.676 |
| | (-0.901) | (-2.036)** | (-1.695)* |
| Constant | 38.34 | 46.95 | 46.95 |
| | (11.39)*** | (22.41)*** | (22.24)*** |
| Municipality fixed effect | Yes | yes | yes |
| Day fixed effect | Yes | yes | yes |
| Observations | 7,725 | 14,935 | 21,115 |
| R-squared | 0.192 | 0.178 | 0.179 |
| Adjusted R-squared | 0.166 | 0.159 | 0.160 |
| Number of municipalities | 515 | 515 | 515 |

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Non-linear effect over time.

| Length of the window | (3.1) Two weeks | (3.2) Four weeks | (3.3) Six weeks |
|---|---|---|---|
| Limburg * 1st week-end after | 1.505 | 1.639 | 2.321 |
| | (0.660) | (0.763) | (1.094) |
| Limburg * Between the weekend and the appeal | 6.471 | 8.224 | 8.905 |
| | (1.667)* | (2.309)** | (2.454)** |
| Limburg * After the appeal | | 4.945 | 6.317 |
| | | (1.980)** | (3.056)*** |
| Constant | 33.52 | 27.25 | 31.59 |
| | (16.62)*** | (12.22)*** | (12.88)*** |
| Municipality fixed effect | Yes | yes | yes |
| Day fixed effect | Yes | yes | yes |
| Observations | 6,942 | 14,418 | 21,894 |
| R-squared | 0.207 | 0.178 | 0.173 |
| Adjusted R-squared | 0.175 | 0.155 | 0.154 |
| Number of municipalities | 534 | 534 | 534 |

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Placebo tests.

| | (4.1) -15 days | (4.2) -10 days | (4.3) +10 days | (4.4) +15 days |
|---|---|---|---|---|
| **2 weeks window** | | | | |
| Limburg * After | 2.582 | 1.446 | -0.203 | 0.702 |
| | (1.158) | (0.759) | (-0.0745) | (0.187) |
| Observations | 8,010 | 8,010 | 8,010 | 8,010 |
| R-squared | 0.146 | 0.230 | 0.124 | 0.079 |
| Adjusted R-squared | 0.121 | 0.196 | 0.107 | 0.0682 |
| **4 weeks window** | -0.186 | 2.398 | 2.358 | -0.495 |
| Limburg * After | (-0.111) | (1.322) | (1.099) | (-0.202) |
| Observations | 15,486 | 15,486 | 15,486 | 15,486 |
| R-squared | 0.265 | 0.188 | 0.146 | 0.092 |
| Adjusted R-squared | 0.237 | 0.163 | 0.132 | 0.0822 |
| **6 weeks window** | | | | |
| Limburg * After | -0.494 | 2.368 | 2.479 | 1.673 |
| | (-0.285) | (1.577) | (1.405) | (0.751) |
| Observations | 21,894 | 21,894 | 21,894 | 21,894 |
| R-squared | 0.301 | 0.241 | 0.153 | 0.106 |
| Adjusted R-squared | 0.276 | 0.217 | 0.138 | 0.116 |
| Municipality fixed effect | yes | Yes | yes | yes |
| Day fixed effect | yes | Yes | yes | yes |
| Number of municipalities | 534 | 534 | 534 | 534 |

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Dependent variables: median donation. Limburg vs. rest of Flanders

| | (5.1) | (5.2) | (5.3) |
|---|---|---|---|
| Length of the window | Two weeks | Four weeks | Six weeks |
| | | | |
| Limburg * After | 5.638 | 5.058 | 6.052 |
| | (2.443)** | (2.739)*** | (3.711)*** |
| Constant | 32.83 | 26.02 | 44.86 |
| | (16.01)*** | (10.59)*** | (22.41)*** |
| Municipality fixed effect | yes | yes | yes |
| Day fixed effect | yes | yes | yes |
| Observations | 8,010 | 15,486 | 21,894 |
| R-squared | 0.152 | 0.118 | 0.134 |
| Adjusted R-squared | 0.131 | 0.132 | 0.121 |
| Number of municipalities | 534 | 534 | 534 |

Robust t-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Dependent variables: number of donations. Limburg vs. rest of Flanders

| | (6.1) | (6.2) | (6.3) |
|---|---|---|---|
| Length of the window | Two weeks | Four weeks | Six weeks |
| | | | |
| Limburg * After | 0.0624 | 0.425 | 0.404 |
| | (0.203) | (0.826) | (0.791) |
| Constant | 1.278 | 0.615 | 2.436 |
| | (14.20)*** | (4.239)*** | (29.55)*** |
| Municipality fixed effect | yes | yes | yes |
| Day fixed effect | yes | yes | yes |
| Observations | 8,010 | 15,486 | 21,894 |
| R-squared | 0.369 | 0.274 | 0.375 |
| Adjusted R-squared | 0.241 | 0.383 | 0.273 |
| Number of municipalities | 534 | 534 | 534 |

Robust t-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A1: Testing for a common trend in donations before the disaster

| | (A1.1)<br>One week | (A1.2)<br>Two weeks | (A1.3)<br>Three weeks |
|---|---|---|---|
| Trend | 0.571 | -1.056 | -0.445 |
| | (1.675)* | (-9.979)*** | (-6.182)*** |
| Limburg * trend | 0.173 | 0.0529 | 0.251 |
| | (0.256) | (0.204) | (1.577) |
| Constant | 6.090 | 85.14 | 57.04 |
| | (0.424) | (20.18)*** | (21.61)*** |
| Observations | 3,738 | 7,476 | 10,680 |
| R-squared | 0.001 | 0.012 | 0.003 |
| R-squared | 0.000344 | 0.00849 | 3.38e-09 |
| Number of post2 | 534 | 534 | 534 |
| F test | 2.218 | 58.75 | 20.04 |

Robust t-statistics in parentheses