Do Disasters Affect the Tightness of Social Norms? *

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Abstract

Universally, social norms prescribe behavior and attitudes, but societies differ widely in how strictly individuals hold to the norms and sanction those who do not. This paper shows that large adverse events, henceforth "disasters", lead to tighter social norms. To establish this result, I combine data on the occurrences of conflicts, epidemics, and natural and economic disasters with the World Value Surveys, European Social Surveys, and Gallup World Polls. I use this data set to estimate the effect of disasters on the tightness of social norms in two ways: (i) investigating event-studies that compare individuals interviewed in the weeks before and after the same disaster; and (ii) examining variation in individuals' past exposure to disasters across countries and cohorts while controlling for country-, cohort-, and life-cycle-specific factors. The event-studies demonstrate that disasters tighten social norms by 9 percent of a standard deviation. The analysis of cross-country variation shows that the effect can persist for decades and is transmitted to the subsequent generation. The results are consistent with a conceptual framework in which disasters increase the returns to coordination within groups and suggest that past exposure to disasters partially explains within-group cohesion and intolerance for non-conformism.

Keywords: Social norms, intolerance, social cohesion, conflict, epidemics, natural disasters, economic shocks

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1 Introduction

Social norms, the widely shared opinions of how group members should behave, pervade human life. Be it in contexts such as cooperation, gender, consumption, education, or the labor market, growing evidence suggests that social norms can be important determinants of social outcomes and economic development. Understanding their determinants and how norms persist or change over time is therefore at the heart of an emerging literature.[1] Previous studies in this literature have almost exclusively focused on the behavioral standards that social norms imply. These papers typically compute the *means* of the distributions of normative opinions across countries or social groups and examine the factors that explain differences in these means. However, distributions of normative opinions not only vary in their means but also in their *variances*. A low variance reflects a tight distribution, indicating broad agreement on the behavioral standard and disapproval and sometimes even punishment if individual group members deviate from the norm.[2] While all social groups have both tight and looser norms, the seminal work by Gelfand et al. (2011) documented that the tightness of norms systematically differs across countries. In countries like India and South Korea, they found generally tight norms and low tolerance for deviation from accepted behavior. In countries like Israel and Brazil, they found looser norms and more tolerance for non-conformists.[3]

Why do some social groups have tighter norms than others? This paper studies whether exposure to large adverse events affects how tightly individuals hold to the norms and disapprove of those who do not. The answer to this question is not obvious ex-ante. On the one hand, disastrous events may increase the incentives for individuals to deviate from social norms. We have numerous historical examples of chaos, looting, and even the collapse of societies after lost wars, epidemics, or natural disasters.[4] On the other hand, disasters increase the potential benefits of social coordination and cooperation and, therefore, maintaining social norms. Recent studies in psychology have found that historical exposure to conflict, natural disasters, and diseases predicts tightness today and

[1] See Enke (2019); Ashraf et al. (2020); Atkin et al. (2020); Bursztyn et al. (2020b); Moscona et al. (2020) for recent evidence on how social norms affect important economic and social outcomes and, e.g., Voigtländer and Voth (2012); Alesina et al. (2013); Becker (2019); Schulz et al. (2019); Bursztyn et al. (2020a); Lowes and Montero (2020) for studies examining determinants of social norms.

[2] Appendix Figure A1 provides two concrete examples of how the behavioral standards (means) and tightness (variances) of social norms may differ.

[3] These differences seem to have important economic, social, and psychological consequences. For example, places with tight social norms engage less in disruptive innovation and have fewer political liberties. Examining the consequences of tightness for economic and social outcomes is an interesting line of future research in my view.

[4] See, e.g., Hays (2005); Diamond (2011)

argued that tight norms have evolved as an adaptive response to those disasters.[5] To determine which of the two effects dominates, I conduct a systematic empirical analysis, which yields a clear conclusion: For different types of disasters and populations, I consistently find that disasters lead to tighter social norms. The effect is persistent, transmitted to the subsequent generation, and more pronounced in low-income countries where governments tend to be weaker and coordination within groups relies more on social norms.

To establish this result, I combine occurrences of epidemics, conflict, and economic and natural disasters with the *World Values Surveys* (WVS) and the *European Social Survey* (ESS). Both data sets allow me to measure the tightness of social norms in two ways. The first measure captures tightness as revealed by people's normative opinions on dozens (ESS) or hundreds (WVS) of social norms. Specifically, I compute an individual-level measure of tightness that reflects how similar an individual's normative opinions are relative to a reference group that likely holds the same bundle of social norms. I define reference groups as all individuals who live in the same subnational region (e.g., U.S. states) and belong to the same ethno-linguistic group in the baseline analysis and assess the sensitivity to alternative, more fine-grained definitions in robustness checks. The second measure of tightness is a survey question that asks for people's perceptions of how important it is to "avoid doing anything people would say is wrong", in other words, how costly it is to deviate from the norms.

I employ two empirical strategies to estimate the effect of disasters on tightness: one for short-run effects within weeks and the other for longer-run effects over years. First, to study the short-term impact of disasters on tightness, I match data from nine waves of the ESS with information on conflict, terrorist attacks, outbreaks of epidemics, and natural disasters between 2002 and 2019. The identification strategy examines plausibly exogenous differences in the timing of the interviews relative to when these events occur. Specifically, I compare revealed and perceived tightness between individuals interviewed in the weeks immediately before a disaster occurred in their vicinity and individuals from the same subnational region but interviewed in the weeks immediately after that same disaster – two groups that are comparable along observables and comprise more than 10,400 respondents from 11 countries.[6] I find that individuals interviewed after a disaster hold more tightly to the norms than those interviewed just before the disaster. The effect is a sizeable 9 percent of a standard deviation, corresponding to 13% of the gap in

[5] Gelfand et al. (2011); Harrington and Gelfand (2014); Uz (2015); Gelfand (2018); Chua et al. (2019); Jackson et al. (2020); Gelfand et al. (2021)

[6] This analysis is limited to the ESS because it requires information on the dates of interviews, which the WVS does not record for most observations. Online Appendix C lists the disasters examined in the analysis.

average tightness between Denmark and Russia, where social norms are particularly tight and loose, respectively. The results also show that disaster-induced changes in the content of norms do not explain the effect. Controlling for the individual survey responses that constitute the revealed measure of tightness leaves the result virtually unchanged.

The second empirical strategy examines how lasting the effect of disasters on tightness is. Combining data on the occurrences of conflicts, epidemics, and economic and natural disasters with the *World Values Surveys* (WVS), I create a data set covering disasters and individuals in almost 80 countries. Using this data set, I estimate the effect of disasters on tightness by examining variation in people's past exposure disasters across countries and cohorts. Here, an observation is an individual in adulthood who may have been exposed to disasters during childhood or adolescence.[7] To construct measures of individuals' exposure to past disasters, I assign disaster occurrences based on individuals' birth year and country of current residence and compute average exposures to disasters during ages 1 to 19. The empirical specification examines a pseudo panel of individuals who live in different countries and are born in different cohorts. The equation reflects the logic of a difference-in-differences model and includes two sets of fixed effects: country-of-residence $\times$ survey-year fixed effects and birth-cohort fixed effects. The country-of-residence fixed effects are particularly important. They remove country-specific determinants of tightness such as institutions and deep history, including disasters that hit previous generations and whose effects may persist through intergenerational transmission. The analysis controls for these factors, focusing on the within-lifetime effects of disasters. The key identifying assumption is that the timing of disasters is exogenous to a given cohort within a specific country. Specifically, there must not be unobserved determinants of tightness that vary at the country-cohort level. The event-study findings presented above provide strong empirical support for the credibility of this assumption. Thus, any estimated difference should capture the causal effect of disaster experiences on the tightness of social norms.

The results show persistent positive and statistically significant relationships between past exposure to disasters and both measures of tightness today. Individuals perceive lower tolerance for non-conformism and their normative opinions are more similar relative to their reference group even many years after they were exposed to disasters early in their lives. The effect slowly fades over as individuals grow older but can persist for several decades and is transmitted to the subsequent generation.

I perform comprehensive robustness checks to assess the validity and sensitivity of this result. Importantly, the result holds up and is quantitatively similar in the subsample of native-born

[7]The empirical strategy is similar to Giuliano and Spilimbergo (2014).

respondents. In the baseline, I use the country where the survey was conducted to determine respondents' residences during their whole lifetime. This approach raises two concerns: (i) selective migration could drive the result if, for example, individuals who place little importance on norm adherence migrate from countries exposed to disasters to countries with fewer disasters; and (ii) the method may introduce measurement error among those individuals who recently immigrated to their current country of residence. Restricting the sample to native-born individuals addresses both concerns.

I then turn to mechanisms. I present a conceptual framework in which the tightness of norms depends on the relative returns to social coordination, which are affected by disasters. When a disaster hits, individuals respond by holding more tightly to the norms and punishing others who do not if the potential returns to social coordination outweigh the benefits of deviating from the norms. Importantly, this response is not specific to prosocial norms, where norm adherence is costly at the individual level but beneficial for the group as a whole, but applies to norms for organizing social interaction more generally, including non-cooperative norms such as those that derogate homosexuality, immigrants, or women. Individuals' extra willingness to punish is due to signaling benefits (showing norm commitment) or avoid being sanctioned for not sanctioning (Henrich, 2016). Three additional empirical results support this mechanism. First, I find that the effect of disasters on norm tightness is not specific to prosocial norms. To obtain this result, I categorize the norms in the WVS as either related to cooperation (e.g., "Is it justifiable to claim government benefits to which you are not entitled to?") or unrelated to cooperation (e.g., "Is it justifiable to get an abortion"). I find that disasters have no differential impact on the tightness of norms related to cooperation relative to the tightness of other norms. Second, I find that the effect of disasters on tightness is more pronounced in countries with low state capacity, proxied by the ratio of tax revenue to national GDP (fiscal capacity) or bureaucratic quality (coordinative capacity).[8] Fiscal capacity and coordination capacity refer to the capabilities of state agents to organize social interaction, among other things. These capabilities may act as substitutes to social norms (Bisin and Verdier, 2017), reducing the potential benefits of tighter norms in response to disasters. Third, I find evidence that the effect is symmetric: Past exposure to economic growth leads to looser social norms in low-income countries, where the potential returns to social coordination are likely to be sensitive to improved economic conditions because individuals live closer to the subsistence level.

In sum, the paper provides the first causally identified evidence that large adverse shocks lead to tighter social norms. A limitation lies in the fact that data on individuals' willingness to sanction

[8]See, e.g., Besley and Persson (2009); Berwick and Christia (2018)

norm violations is not available for large samples.

This research contributes to four streams of literature. A growing set of studies examine the determinants of cultural differences and change. This work suggests that today's differences are the outcome of an evolutionary process, in which the historical environment or historical shocks shaped their long-term evolution (Nunn and Wantchekon, 2011; Alesina et al., 2013; Lowes et al., 2017; Nunn and de la Sierra, 2017; Dell et al., 2018; Becker, 2019; Enke, 2019; Schulz et al., 2019; Giuliano and Nunn, 2020; Heldring, 2020; Lowes and Montero, 2020), and that cultural traits may change due to shorter-run factors that are felt within a person's lifetime (Giuliano and Spilimbergo, 2014; Campante and Yanagizawa-Drott, 2015; Bentzen, 2019; Bazzi et al., 2020). The findings in this paper demonstrate that shocks not only affect the behavioral standards that social norms but also their tightness.[9]

This insight improves our understanding of why adverse events can promote prosocial behavior within groups. A body of mostly experimental work has documented that natural disasters such as hurricanes or tornadoes can spur charitable giving (Fong and Luttmer, 2009; Deryugina and Marx, 2021) and that individuals exposed to conflict behave more cooperatively toward their group members in behavioral experiments (reviewed in Voors et al., 2012; Bauer et al., 2016). While related work in economics and the adjacent social sciences suggest that in-group bias and religiosity may be a proximate mechanism (Henrich et al., 2019; Shayo, 2020), this paper points to another proximate mechanism: large negative events, including natural disasters and wars, tighten social norms of cooperation and prosociality towards in-group members.

The results also relate to theoretical evidence that culture responds to the external environment and institutions and their interaction. Understanding these processes is important for understanding persistence and change (Tabellini, 2008; Acemoglu and Robinson, 2019; Nunn, 2020). The results suggest that the impact of external adverse shocks on culture depends on the institutional context, including the capacity of the state to organize collective action.

Last, the findings provide a rationale for why adverse conditions can lead to between-group divides and polarization in society (Autor et al., 2020; Atkin et al., 2020; Desmet and Wacziarg, 2020). Tighter norms in response to shocks entail greater homogeneity within groups and possibly a growing divide between groups if group members adhere to different social norms. While previous work treats the degree of norm adherence as a fixed parameter, this paper shows that it is affected by external shocks.

[9]This result is also consistent with recent evidence showing that individuals are more willing to tolerate limits on their civil rights and freedoms during the pandemic (Alsan et al., 2020).

The remainder of the paper is organized as follows. Section 2 describes the measurement of tightness of social norms. Section 3 describes the empirical strategies and report the main results. Section 4 develops a conceptual framework that illustrates why disasters may affect the tightness of social norms and provides additional empirical evidence consistent with this framework. Section 5 concludes.

2 Tightness of Social Norms and Its Measurement

Social norms are *widely shared opinions of how individual members of a social group should behave* (Elster, 1989; Fehr and Gächter, 2000; Ostrom, 2000; Krupka and Weber, 2013; Fehr and Schurtenberger, 2018b).[10] This definition contains two crucial elements. First, a social norm establishes a normative behavioral standard that applies to a particular group. This behavioral standard is not defined based on actual behavior but in terms of how group members should behave. It represents the behavior that group members expect from each other in a given situation. Empirically, this corresponds to the mean of the distribution of normative opinions.[11] Second, because the behavioral standard is based on widely shared opinions, which are also approved, non-conformism triggers disapproval and sometimes even punishment. The extent of disapproval is captured by the the variance of the distribution of opinions (Fehr and Schurtenberger, 2018a). A low variance reflects a tight distribution, indicating high consensus about the normative request and presumably greater disapproval for non-conformists. In other words, the variance in normative opinions indicates how important it is for group members to adhere to the behavioral standard. This element is known in psychology as the tightness of a norm.

Appendix Figure A1 provides a few concrete examples of behavioral standards and tightness using data from the *World Values Surveys* (WVS). The top panel shows the distribution of opinions among young men in Egypt, Norway, and the United States on whether mean should have more right to a job than women if jobs are scarce. I recode opinions to integers from 1 to 3, where higher numbers indicate more disagreement, and compute the means and variances for the three social groups. The example demonstrates that while the behavioral standards are different in Egypt and

[10] More specifically, this definition refers to what psychologists call injunctive social norms. Social norms have also been defined in terms of customs or actions that people regularly take – what is known in psychology as descriptive norms (Cialdini et al., 1990). Both kinds of norms affect behavior. However, I focus on injunctive norms because their power comes from people's willingness to punish others' deviation from them (Elster, 1989; Fehr and Gächter, 2000), that is, their tightness.

[11] Note that the definition also implies that there can only be one behavioral standard in a given social group. In other words, the distribution of opinions is single-peaked.

Norway, the tightness is similar. In contrast, the behavioral standards in Norway and United States are similar but the norm is tighter in Norway. The bottom panel illustrates how a social norm can look almost identical in terms of its behavioral standard but differ in its tightness. The plot shows the distributions of opinions among individuals in Sherpur (Bangladesh) and Uttar Predesh (India) on whether it is a problem if women have more income than men. Respondents state their opinion on a scale from 1 to 4, where higher values again indicate greater disagreement. The means in the two regions are almost identical, suggesting the same behavioral standard. But the variance in opinions is smaller in Sherpur than in Uttar Pradesh, indicating that the norm is tighter in Sherpur.

2.1 Measuring Tightness at the Individual Level

The psychology literature has relied on measures of tightness that are based on people's perceptions in relatively small samples (Gelfand et al., 2011; Chua et al., 2019), variances in responses to survey questions in the WVS (Uz, 2015), or a bundle of proxy measures (Harrington and Gelfand, 2014). For the purpose of this analysis, these measures are suboptimal because they are only available at the country or subnational region level and not over time, and therefore inhibit a causal identification strategy.

Thus, I develop new measures of tightness that allow for a causal analysis. The measures are based on questions in the latest versions of the WVS (waves 1 - 7) and the *European Social Survey* (ESS, waves 1 - 9) that are explicitly related to social norms. Both data sets consist of a series of nationally representative surveys covering roughly 100 and 40 countries from around the world and within Europe, respectively. They contain information on numerous social norms and record common demographic characteristics, such as respondents' sex, age, education, labor market status, and their subnational region of residence. The two data sets are unique in that they allow to measure tightness at the individual level in two distinct ways.

Revealed measure of tightness The goal of the first measure of tightness is to capture the extent to which individuals' normative opinions are similar relative to those of other people in their reference group that holds the same bundle of behavioral standards. Intuitively, this measure applies the concept of variance in opinions to individuals. The more similar someone's opinions are relative to his reference group, the more tightly that person holds to the behavioral standards, and the tighter are the social norms on average.

The measurement requires the identification of survey questions in the WVS and ESS that are social norms. Specifically, the definition of social norms entails that survey questions are

social norms if they ask respondents to state their normative opinions about a given topic. Closely following this definition, I confine attention to those questions that are either normative (e.g., should men have more right to a job than women if jobs are scarce) or descriptive but contain a normative component (e.g., do you want to have an immigrant as your neighbor). To ensure cross-country comparability, I drop questions that are asked in a few countries only or answered by few respondents (similar to the procedure in Desmet et al., 2017). This approach leaves me with 174 questions in the WVS and 62 questions in the ESS. Online Appendix B provides the list of questions for both the WVS and ESS.

The measurement also requires the definition of reference groups. One possibility is to think of all people within a country as the reference group. However, this approach will be problematic if there are distinct social groups in a country with their own social norms. In this case, the distribution of normative opinions may no longer be single-peaked, making it difficult to measure tightness based on the variance in opinions. To circumvent this problem, previous work has used subnational districts or ethno-linguistic groups as reference groups (Desmet et al., 2017; Atkin et al., 2020). Building on this approach, I define reference groups in my baseline analysis as all individuals who live in the same subnational region and belong to the same ethno-linguistic group. This definition results on average in 181 groups per country in the ESS and 61 groups per country in WVS. In robustness exercises, I use even finer definitions of references groups by adding interactions with cohorts, educational attainment, and income.

Among the survey questions, there are three types: those with a binary response (yes/no, agree/disagree), those with an ordered response (e.g., a scale from 1 to 10), and those with three or more categorical responses. Given these differences, two questions arise: How to compute a coherent variance-type measure at the individual level, in particular for the categorical questions; and how to convert dozens or hundreds of variance-type measures into a single tightness index?

I tackle these problems by computing the share of individuals' opinions that are identical to the most frequent opinion among members of the reference group. The following equation formally summarizes the summary statistic of tightness of social norms:

$$Revealed\ Tightness_i = \frac{1}{Q} \sum_q \mathbb{1}_{o_{iJ}^q = \bar{o}_J^q} \tag{1}$$

where i denotes individuals, q one of Q questions, o a response to q, J captures i's reference group, and $\bar{o}_J^q$ represents the social norm. First, for each question, I define the social norm in a given reference group as the modal opinion among members of that reference group (e.g., individuals in

a subnational district who speak the same language). Second, I set an indicator equal to one if an individual who belongs to that reference group states the same opinion. Third, I average over all questions to get a single index of how tightly an individual holds to the norms on average.

Cross-cultural psychologists argue that societies have markedly different answering styles and that there exist cross-cultural differences in preferences for yes-no answers versus a Likert-type scale rating (e.g., see the approach and discussion in Muthukrishna et al., 2020). To reduce the possibility that differences in answer distributions are due to differences in answering style rather than the actual opinions, I collapse responses into positive or negative values (e.g., very important and rather important were merged and not very important and not at all important are merged; if a midpoint existed, it is treated as a separate response).

Perceived measure of tightness The goal of the second measure of tightness is to capture how easy it is for individuals to deviate from the norms. Both surveys include a question that asks respondents to report their perceptions of exactly this. This survey question is contained in two waves of the WVS during the years 2006-2009 and 2010-2014 and nine waves of the ESS during the years 2002-2018. Respondents are given the description of a person: "It is important to this person to always behave properly; to avoid doing anything people would say is wrong." They then choose the answer that best describes how similar this person is to them: 1 ("not at all like me"), 2 ("not like me"), 3 ("a little like me"), 4 ("somewhat like me"), 5 ("like me"), and 6 ("very much like me").[12] The survey question is part of a larger set of questions designed by Schwartz (2012) to measure a system of human values. Schwartz (2012) argues that differences in response styles and closeness perceptions across individuals potentially introduce measurement error and recommends to subtract from the variable of interest the average answers respondents give to all other Schwartz human value questions. I follow this recommendation.

3 Results

The empirical analysis comprises two empirical strategies: one to estimate the short-run effect of disasters on tightness of social norms within weeks and another strategy to estimate longer-run effects over decades.

[12] I recode the responses such that higher values indicate greater importance of norm adherence.

3.1 Short-run Effects

I begin by examining the short-run effect of disasters on revealed and perceived tightness of social norms. Similar to the identification strategy in Depetris-Chauvin et al. (2020), I estimate the impact of disasters on tightness within just weeks after a disaster. This analysis requires information on the exact dates of both disasters and interviews. I will focus on the ESS data set because such information is not consistently available in the WVS.

Disasters I am able to link the ESS interviews to common types of disasters in Europe. Specifically, I collect data on conflicts, outbreaks of epidemics, and natural disasters that occurred in Europe since 2000. The data on conflict comes from two sources. The first source is a geo-coded data set from the Uppsala Conflict Data Program (UCDP). Conflict events are two-sided battles or one-sided attacks that produce at least one fatality. I select events that caused at least five civilian fatalities. UCDP conflict data run from 1989 to 2018. I also use a second data source for conflicts which is provided by the Armed Conflict Location and Event Data Project (ACLED). This data set includes information on the date, location, and classification (battles, riots, explosions, violence against civilians) of any episode of political violence that involve either the government, rebel groups, militias, or civilians (Raleigh et al., 2010). The data set also includes information on the severity of the events, measured by the number of associated fatalities. Based on this information, I select events that involved violence against civilians and caused some fatalities (greater than zero).

The data on outbreaks of epidemics comes from the EM-DAT International Disasters Database that records worldwide epidemic occurrences and its effects. These data are compiled from UN agencies, non-governmental organizations, insurance companies, research institutes, press agencies, and other sources. The database includes epidemics (viral, bacterial, parasitic, fungal, and prion) meeting one or more of the following criteria: (i) 10 or more deaths; (ii) 100 or more individuals affected; (iii) declaration of a state of emergency; or (iv) calls for international assistance. Each epidemic is tagged with the country where it took place. When an epidemic affects several countries, the database contains separate entries for each country. EM-DAT provides information on the start and end date of the epidemic, the number of deaths and the number of individuals affected, where the number of individuals affected is how many require assistance with basic survival needs such as food, water, shelter, sanitation, and immediate medical treatment during the period of emergency.

Finally, EM-DAT also records occurrences of natural disasters. It provides the exact date of different types of natural disasters, from earthquakes to floods, storms, wildfires, and landslides, including the number of fatalities. To be included in the analysis, natural disasters must have

caused some fatalities (greater than zero) in the case of natural disasters that are highly local (such as earthquakes), and they must have caused at least 10 fatalities in the case of disasters that affect larger geographies (such as storms or floods). Of all the different types of natural disasters, I am able to link occurrences of earthquakes, storms, and floods.

Empirical strategy I estimate the impact of disasters on tightness of social norms using the following equation:

$$y_{irdyt} = \beta \; Post \; Disaster_{dt} + \alpha_{dr} + X_i\Gamma + \varepsilon_{irdyt} \tag{2}$$

where i, r, d, y and t denote individual, subnational region, disaster, year, and date of interview, respectively. The variable y_{irdyt} is either revealed or perceived; $Post \; Disaster_{dt}$ is the main regressor of interest, and takes value one if the respondent was interviewed in the days after a disasters hits her region, and zero otherwise. The variable X_i is a vector of baseline individual controls (i.e., fixed effects for education, gender, age, unemployment status, and marital status); α_{dr} are disaster-region fixed effects, ε_{irdyt} is the error term.

The coefficient β is the main coefficient of interest, capturing the effect of disasters. The presence of disaster-region fixed effects implies that I identify β by comparing respondents interviewed after a given disaster with all other respondents from the same subnational region before the same disaster. This absorbs all unobservable confounds that vary from one disaster to another, such as local political or economic events. I use robust standard errors, clustered on regions
$times$ survey wave in my benchmark specifications and report results for alternative clustering choices in the appendix.

I mainly focus on the sample of individuals interviewed in the 50 days before and after a disaster occurs and the sample of respondents exposed to only one disaster, which includes more than 10,400 individuals from 11 countries and 94 subnational regions. This narrow time window is appealing from an identification perspective because unobserved factors are more likely to be similar. I later show that the results remain virtually unchanged when replicating the analysis using larger time windows up to one year. Appendix Figure A5 shows the distribution of interviews in the proximity of the relevant disasters. Appendix Table A1 presents descriptive statistics. Specifically, approx. 37.7 percent of individuals were exposed to a disaster in the 50 days prior to the interview.

The identification strategy relies on the quasi-random nature of the date when a disaster hits relative to the timing of the survey interviews. Hence, the identifying assumption is that disasters did not interfere with the implementation of the survey. To assess the validity of the strategy, I conduct

Table 1: Event studies: balance in covariates

Covariate	Observations	Mean	Estimate	Std. error	p-value
Age	10,407	45.781	-0.733	0.737	0.322
Gender, male	10,407	0.482	0.003	0.012	0.785
Married	10,407	0.575	-0.014	0.013	0.269
Unemployed	10,407	0.055	0.007	0.005	0.138
Education	10,407	3.102	0.001	0.058	0.982
Immigrant	10,389	0.088	0.001	0.011	0.952
Ethnic minority	10,196	0.078	0.016	0.019	0.412

Notes: The table presents coefficients for 7 OLS regressions of individual-level characteristics on the post-disaster indicator and disaster-region fixed effects. Robust standard errors, clustered on region $\times$ survey wave, are reported. The F statistic of the null hypothesis that all independent variables of a regression of the indicator on the 6 covariates and the fixed effects are jointly zero is 0.704 ($p = 0.669$)

a balance test for several respondent characteristics that may potentially correlate with tightness. These characteristics include: gender, education, age, unemployment, immigration status, and belonging to an ethnic minority. I perform a balance test that compares individuals interviewed before and after a disaster. To ensure that I compare respondents from the same region interviewed around the same disaster, I regress each variable on the post-disaster indicator and disaster-region fixed effects. Table 1 shows that the characteristics are balanced between respondents interviewed before and after the same disaster, lending empirical support to the credibility of the identification strategy.

Results Table 2 reports the results from the specification presented in equation (2). I examine the impact of disaster events on revealed and perceived tightness in the baseline sample of all respondents within a window of 50 days of a disaster. Column 1 shows the results for the regression of revealed tightness on the dummy for being interviewed after a disaster hits, controlling for disaster-region fixed effects. I find a positive and statistically significant effect on revealed tightness. The coefficient becomes slightly larger in Column 2, where I additionally control for individual-level characteristics. Columns 3 reports the estimates of the impact of conflict, epidemics, natural disasters on revealed tightness, separately. All coefficients are positive and statistically significant.

I also find a positive and statistically significant effect on perceived tightness. Column 4 shows the result for the regression of perceived tightness on the dummy for being interviewed after

Table 2: Event-study estimates of the effect of disasters on tightness of social norms

	Tightness of social norms					
	Revealed			Perceived		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Disaster	0.095***	0.102***		0.080**	0.089***	
	(0.031)	(0.030)		(0.035)	(0.030)	
Post-Disaster × Conflict			0.137***			0.118*
			(0.044)			(0.063)
Post-Disaster × Epidemics			0.099**			0.084*
			(0.048)			(0.042)
Post-Disaster × Natural disasters			0.071*			0.071**
			(0.039)			(0.027)
Disaster × region FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual-level controls	No	Yes	Yes	No	Yes	Yes
N	10,407	10,407	10,407	10,407	10,407	10,407
R^2	0.061	0.079	0.079	0.048	0.120	0.120

Notes: OLS estimates, robust standard errors are clustered on subnational regions × survey wave and reported in parentheses. An observation is an individual. The sample includes respondents interviewed within 50 days before and after a disaster hits the region in which the individual was surveyed. Disasters included occurrences of conflict and terrorist attacks, outbreaks of epidemics, and lethal natural disasters (earthquakes, floods, and storms). The outcome is revealed tightness of social norms in columns 1 to 3 and perceived tightness of social norms in columns 4 to 6, as defined in the text. The main independent variable is *Post-Disaster*, which takes value one if the respondent was interviewed in the 50 days after a disaster, and zero otherwise. All regressions include disaster × region fixed effects; individual-level controls contain dummies equal to one if the respondent is male, married, or unemployed and fixed effects for age and highest educational attainment. The dependent variable is normalized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

a disaster hits the region, controlling for disaster-region fixed effects. As before, the coefficient becomes slightly larger in Column 4, where I additionally control for individual-level characteristics. Column 3 breaks shows the estimates for the three disaster types, separately. As before, all coefficients are positive and statistically significant.

The estimated effects are large: revealed and perceived tightness are approx. 9-10 percent of a standard deviation higher among respondents interviewed before a disaster relative to other respondents from the same subnational region interviewed just before. To put this effect size into context, I compare it to the gap in average tightness between Denmark and Russia, where social

norms are particularly tight and loose, respectively, according to the ESS data. The estimated effect corresponds to 13% of this gap.

A potential concern is that the effect of disasters on revealed tightness is driven by changes in the behavioral standards implied by the norms, that is, changes in the means rather than the variances of norms. Two pieces of evidence point against this possibility. First, the effect size for perceived tightness, whose measurement does not depend on the behavioral standard, is almost identical to the on for revealed tightness. Second, I perform a robustness check where I additionally control for the behavioral standards used to construct revealed tightness in the subsample of norms for which I have non-missing data for most individuals. Online Appendix Table A2 shows that the results are virtually unaffected. This suggests that the findings are not driven by changes in the contents of social norms.[13]

Next, I examine how the effect on tightness evolves in the days after a disaster hits individuals. Figure 1 plots the estimated coefficients and 95 percent confidence intervals for dummies for 15-day periods before and after disasters. The coefficients are estimated from a single regression in which I control for disaster $\times$ region fixed effects and individual-level characteristics. I normalize the coefficient for the 15 days just before the disaster to zero so that the other coefficients indicate how tightness changes over time relative to the eve of the disaster. The right (left) figure shows the coefficients for revealed (perceived) tightness. The figures confirm that social norms tighten within a short period after disasters and suggest that the effect persists. This result is further confirmed if I examine wider time windows around disasters. Online Appendix Tables A4 and A5 report the results for time windows of up to 200 days. The effect of disasters on tightness becomes smaller in magnitude as time passes but is remarkably stable even in larger time windows.[14]

[13]Online Appendix Table A3 reports that the result is robust to alternative definitions of reference groups that are more fine-grained than subnational region $\times$ ethno-linguistic group. Online Appendix Figure A6 plots the estimated coefficients and 95 percent confidence intervals for regressions of tightness of each of each social norm on the disaster dummy, controlling for disaster $\times$ region fixed effects and individual-level characteristics. Using the same specification, Online Appendix Figure A7 plots the estimated coefficients and 95 percent confidence intervals for the individual survey responses as outcome variables.

[14]In Online Appendix Table A6, I examine whether there is heterogeneity in the effect of disasters on tightness in individuals who belong to different age groups or have different educational attainment or household income. I find that the effect of disasters on tightness tends to be weaker in older and richer individuals. Online Appendix Table A7 shows that the effect tends to be stronger in individuals who belong to minority groups and groups that are being discriminated in a country.

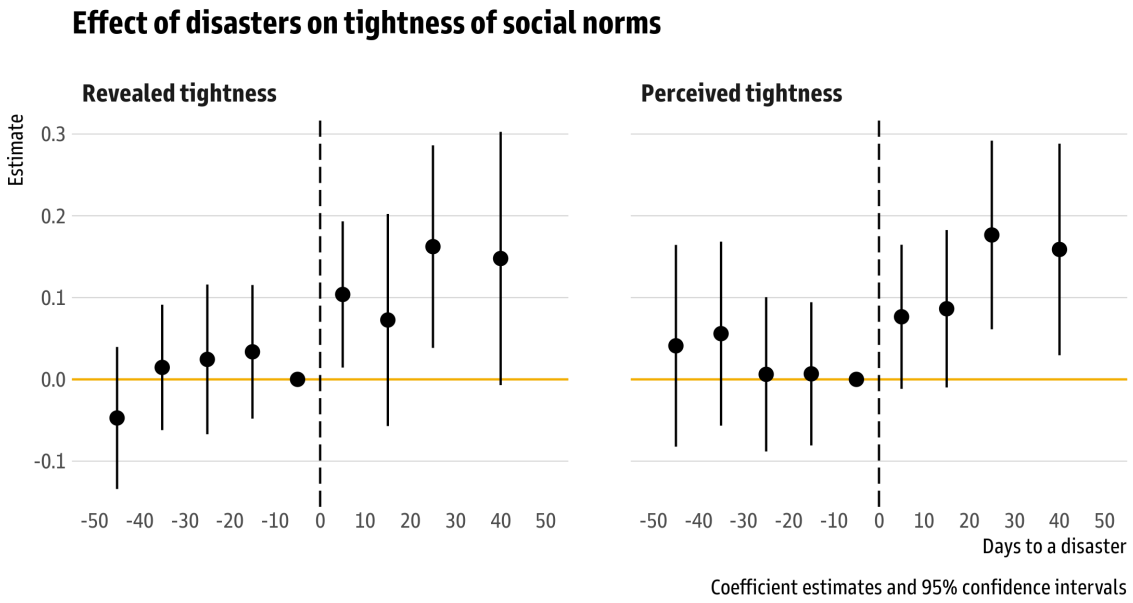

Figure 1: Tightness of social norms before and after a disaster

Notes: The figure plots the coefficients and the 95 percent confidence intervals for eight dummies indicating 10-day blocks and one dummy indicating a 20-day block from 50 days before to 50 days after a disaster hits. I switch to a 20-day block at the end of the period to keep to number of observations underlying each coefficient approximately constant. The coefficients for the period between 10 to 0 days before the disaster occurs is normalized to 0. Confidence intervals are based on robust standard errors, clustered on subnational region $\times$ survey-year. The coefficients are estimated from two regressions that control for disaster $\times$ region fixed effects and individual-level characteristics. The outcomes of the two regressions are revealed tightness (left) and perceived tightness (right) of social norms, as defined in the text. Disasters include events of conflict, outbreaks of disasters, and natural disaters (lethal earthquakes, storms, and floods).

3.2 Long-run Effects

I now turn to the analysis of how long-lasting the effect of disasters on tightness of social norms is. I use an empirical strategy that investigates whether past exposure to disasters affects how tightly individuals hold to their social norms and perceive tightness in countries across the world today. This strategy is common among papers that examine the persistent effects of events that occur during people's early lifetime (e.g., Giuliano and Spilimbergo, 2014). It requires historical information on when and where disasters occurred worldwide.

Past disasters around the world I collect data on the occurrences of the most widespread types of disasters around the world: conflict, epidemics, and natural and economic disasters. First, to

measure conflict, I use data from the Correlates of War Project (COW). The database records occurrences of conflict and their effects around the world (Sarkees and Wayman, 2010). The database includes interstate wars (conflicts that occur between or among recognized states) and intra-state wars (conflicts that predominantly occur within the recognized territory of a state). Each conflict is tagged with the country that was involved or affected. In the case of interstate conflicts, the database contains separate entries for each country. COW provides information on the start and end date of each conflict and the number of deaths. I focus on the conflicts that cause at least some deaths.

Second, EM-DAT, which I described in Section 3.1, provides data on epidemics for countries around the world. I set country-year level indicator equal to one if a country experienced an outbreak of an epidemic that caused some deaths in a given year.

Third, to proxy natural disasters, I collect data on powerful earthquakes, severe droughts, tropical storms, which are the three most devastating types of natural disasters in recent decades (e.g. Ritchie, 2014). Collecting data on several types of natural disasters has several advantages. Different types of natural disasters typically affect different parts of the world. For example, some countries (i.e., Italy and Turkey) frequently suffer from earthquakes but never see a hurricane, and vice versa. It also reduces the likelihood that unobserved characteristics specific to a particular type drive the results or that any particular result is a false positive.

The drought data come in the form of the self-calibrating Palmer Drought Severity Index (scPDSI) provided by the Climatic Research Unit. The CRU TS v.4.01 data set contains gridded measures for the scPDSI at monthly frequency for the whole world at a 0.5-degree resolution (approx. 55km). The data set is constructed from weather stations around the world and available from 1901 (van der Schrier et al., 2013). I count the number of months with severe droughts in a country in a given year (PDSI values of minus 3 or less). Using geo-spatial software to aggregate the gridded drought data to the country-year level and following standard procedures (e.g., Dell et al., 2012), I compute population-weighted average severe drought conditions for each country and year, where the weights are constructed from 2000 population data at 0.5-degrees resolution from the Global rural-urban Mapping project (Balk et al., 2006). The resulting data set contains yearly average frequencies of severe droughts for countries covered by the WVS.

The earthquake data come from the National Oceanic and Atmospheric Administration (NOAA). It provides a geo-coded data set that records information about each earthquake that was significant enough to create moderate damage or cause the loss of life. An earthquake is classified as significant in the database if it meets one of the following criteria: Moderate damage

(approximately $1 million or more), ten or more deaths, Magnitude 7.5 or greater, Modified Mercalli Intensity Xs or greater, or the earthquake generated a tsunami. To construct the earthquake exposure measures at the country-year level, I proceed as follows. Starting with the 2000 population gridded data at 0.5-degrees resolution from the Global rural-urban Mapping project, I assign earthquakes with Richter magnitude of 6 (strong) or greater to grids if they occurred within a radius of 100km to the grids' centroids.[15] For each grid and year, I count the frequencies of earthquakes. Then, I compute population-weighted average earthquake frequencies for each country and year. The resulting data set contains yearly average earthquake frequencies for countries included in the WVS.

The data on tropical storms come from Yang (2008) and Mahajan and Yang (2020), who construct a 'hurricane index' based on meteorological data on storm paths and intensities.[16] The data set provides estimates of the average storm exposure of residents in a given country and year since 1950. See the original papers for details on the construction of the index. The index is highly predictive of disaster damages and human losses experienced by countries in particular years.

Fourth, to measure economic disasters, I draw on data compiled by Barro and Ursúa (2008). The authors constructed time series on national consumption from historical and contemporary official sources for a sample of 42 countries and define periods of economic disasters when consumption drops by 10% or more from peak to trough.[17] I rely on this definition and set an indicator equal to one in years when a country goes through a consumption disaster.[18]

Measuring past exposure to disasters To construct measures of individuals' past exposure to disasters, I assign occurrences of these disasters based on individuals' birth year and country of current residence. I end up with a data set with information on disasters and tightness from

[15]These thresholds are common in the literature (e.g., Bentzen, 2019).

[16]The term 'hurricane' is typically used to describe severe tropical storms in the Atlantic and eastern Pacific. However, the same type of event is known as a 'typhoon' in the western Pacific and 'tropical cyclone' in the Indian Ocean and Oceania. A tropical storm is classed as a hurricane if sustains winds above 74 miles (119 kilometers) per hour.

[17]The dataset contains time series for both contractions in consumption in GDP. Both are correlated but GDP typically fluctuates more than consumption. For example, GDP contracted in the U.S. in the years after World War II by more than 10 percent, but consumption did not. I choose consumption because it is consumption what ultimately matters to individuals.

[18]For a few countries, consumption data are not available for earlier parts of the 20th century. In some cases, it is evident that the missing data are due to the Japanese occupation in World War II (i.e., Singapore 1942-1945, Malaysia 1941-1945, and the Philippines 1941-1945). In light of the documented hardship during the occupation, I code these as consumption disaster events. A similar logic applies to China, for which consumption data are not available before 1952. I code the period from 1937-1946 (Japanese occupation) and 1930-1934 (internal conflict) as consumption disasters. Finally, Uruguayan consumption data are missing for the period before 1960. I code the sharp drop in GDP during the Great Depression (1930-1934) as a disaster event.

individuals in over 70 countries. Using this data set, I compute individuals' average exposures to each disaster type during ages 1 to 19. To get a single index of exposure to past disasters, I normalize the different exposure measures to a z-score and add them up. Since information on economic disasters is available for a relatively small number of countries, I also compute an exposure measure that does not include economic disasters but covers more countries. Online Appendix Figures A10 and A11 plot the averages of these exposure measures by countries and cohorts.

Empirical strategy The regression equation examines a pseudo panel of individuals who live in different countries and are born in different cohorts. The equation follows the logic of a difference-in-differences model, and includes country-of-current-residence $\times$ survey-year fixed effects and birth-cohort fixed effects. Specifically, I estimate the following baseline specification:

$$y_{ibct} = \beta \; Early \; Lifetime \; Disasters_{bc} + \alpha_{ct} + \delta_b + X_i'\Gamma + \varepsilon_{ibct} \tag{3}$$

where y_{ibct} is revealed or perceived tightness of social norms reported by individual i born in cohort b, resident in country c, and surveyed in year t; $Early \; Lifetime \; Disasters_{bc}$ refers to the average exposure to disasters during ages 1 to 19, which vary at the birth-cohort $\times$ country-level. Disaster exposures are assigned to individuals based on the country they reside in when the survey is conducted. α_{ct} and δ_b denote the full sets of country-of-current-residence $\times$ survey-year and birth-cohort fixed effects. The equation also controls for X_i, which in the baseline specification denotes a vector of pre-determined demographic covariates, including a gender indicator equal to one if i is male, age, and age squared. I allow that idiosyncratic differences, ε_{ibct}, are correlated across individuals within a given birth-cohort $\times$ country.

The coefficient β is the main coefficient of interest, capturing the effect of average exposure of the different types of disasters that occurred during individuals' early lifetime. By conditioning on country-of-current-residence $\times$ survey-year fixed effects, the empirical specification absorbs country-specific determinants of tightness such as institutions, deep history, or current economic conditions and allow these determinants to change over the years of the survey data collection. By conditioning on birth-cohort fixed effects, the specification also absorbs all variations across cohorts that might induce different degrees of tightness. Finally, by including controls for age and age squared, the specification removes life cycle effects such as age-related increases in tightness.

In addition to the baseline specification, I estimate specifications that: (i) use alternative clustering choices (country level, cohort level, and country-cohort two-way clustering); and (ii)

include additional individual-level controls (an indicator equal to one if the respondent (i) is married, (ii) is unemployed, (iii) fixed effects for her highest educational achievement, (iv) and fixed effects for her income scale). Note that these individual-level controls may be 'bad controls' because disasters may, for example, affect individuals' employment status and educational choices. Still, including these controls in separate regressions allows me to examine the robustness of the results to these potentially important co-determinants of norm adherence.

The identifying variation comes from (i) within-country differences across cohorts and (ii) cross-country differences within the same cohort. For example, the first comparison is between individuals born in 1958 and living in the U.S. with other U.S. residents born in 1975. The second comparison is between U.S. individuals born in 1958 with individuals born in the same year but living in Japan. The effect is identified if $E(\varepsilon_{icbt}|\alpha_{ct}, \delta_b, X_i) = 0$ is satisfied. The identification assumption is violated if unobserved factors that vary at the country-cohort level are systematically correlated with both the occurrences of disasters and the tightness of social norms today. The timing of disasters in the form of outbreaks of epidemics or violent conflict and potent natural and economic disasters are arguably exogenous to a given cohort within a specific country. The results of the previous event-studies also strongly support this identifying assumption. Therefore, any estimated difference should capture the causal effect of past exposure to disasters on the tightness of social norms.

Results I now report the results from the specification presented in equation (3). Figure 2 summarizes the main findings for perceived and revealed tightness and the five distinct types of disasters that people have been exposed during childhood and adolescence. The graph plots the coefficients and the 95% confidence intervals of the disaster exposure variable interacted with age group dummies to track the evolution of the effect of disasters on tightness as individuals grow older. Start with epidemics. In the decade after an epidemic occurred, that is, in people's 20s, the coefficients on both perceived and revealed tightness is positive and statistically significant. This effect fades over time, as people grow older and memory may fade, but it remains positive and statistically significant up to people in their 50s for perceived tightness. The coefficients on exposure to war in the bottom panel look very similar. The standardized coefficients are slightly smaller in size but the effect is more persistent over time. The results suggest that exposure to war has long-lasting effects on tightness that that persist for decades through people's lives. They also show that economic disasters and natural disasters have positive and statistically significant effects on both perceived and revealed tightness that persist for some years.

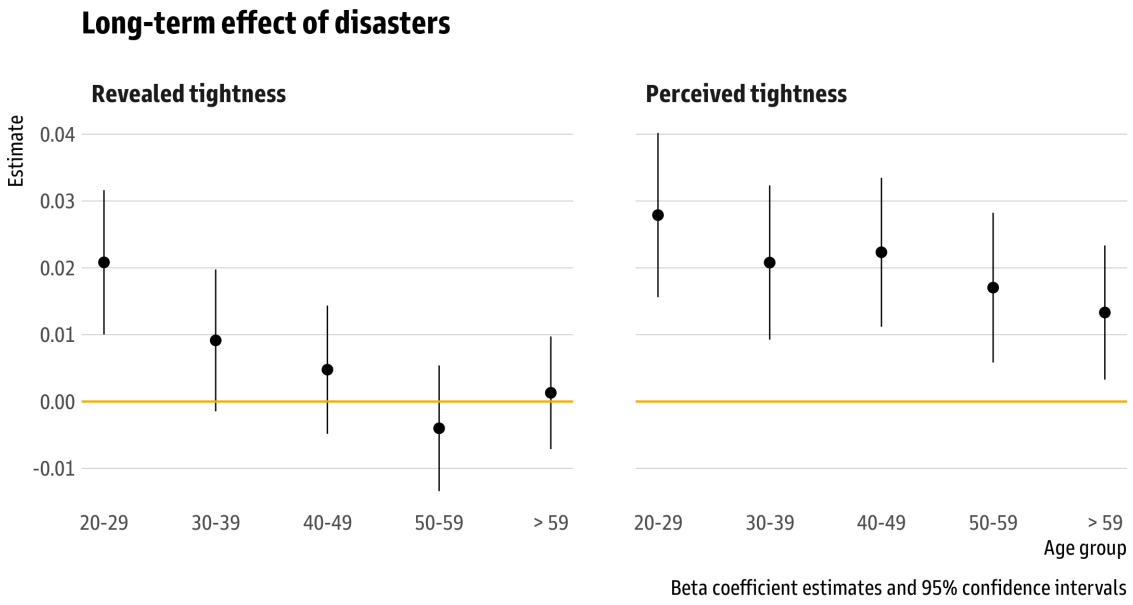

Figure 2: Long-run effects of exposure to disasters during early lifetime on tightness of social norms today

Notes: The figure plots the coefficients and the 95 percent confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from two regressions which include fixed effects for country-of-residence $\times$ survey-year and birth cohort and controls for gender, age and age squared. The outcomes are revealed tightness (left) and perceived tightness (right). Disasters include epidemics, wars, economic disasters, earthquakes, and droughts. All variables are normalized to z-scores.

Overall, the figure presents a fairly consistent picture. In all cases, the effects of disasters on tightness are positive and statistically significant for the age group closest to the experience window, that is, in their 20s, and tend to become weaker as individuals get older. These findings suggest that disasters have long-lasting effects on the tightness of social norms, which slowly disappear over time.

Note that all specifications include country-of-current-residence $\times$ survey-year fixed effects. This implies that the effect is not merely driven by differential exposure to disasters across countries, but by differential experiences across cohorts within countries and how these differences evolve between countries.

Sensitivity and robustness checks I now turn to a discussion of the robustness and sensitivity of the estimates. First, I verify that the results are not sensitive to including other, more endogenous,

individual-level characteristics such as education or income. The baseline analysis only controls for age and gender in terms of individual characteristics because these are clearly exogenous factors that might co-determine tightness. Controlling for potentially endogenous covariates may be problematic because these variables may be a function of past exposure to disasters themselves. Still, as a sensitivity check, Online Appendix Table A8 replicates the analysis, also controlling for marital status, education, an indicator equal to one if the respondent is unemployed, and income levels. The results are almost unchanged if these additional covariates are accounted for.

A second potential concern that I consider is selective migration. To construct the estimates of past exposure to disasters, I assign occurrences of disasters based on the country individuals reside in at the time of the survey data collection. The procedure raises the concern that selective migration may affect the result. For example, a positive association between disasters and tightness would emerge if individuals who tightly hold to the norms migrate from countries hit by disasters to countries that were not affected. It is also possible that migration introduces measurement error to the disaster variable because disasters are mistakenly assigned to individuals. To address these concerns, I restrict the sample to native-born individuals. Compared to the main analysis, I lose up to approx. 50 percent of the sample size because the WVS does not consistently provide information on respondents' immigration status. Nevertheless, the results are unaffected and are quantitatively similar. Online Appendix Table A9 reports these results.

Third, I assess the sensitivity to different clustering choices. The baseline specification clusters standard errors on country $\times$ birth-cohort, which is the level at which treatment occurs. As a sensitivity check, I replicate the analysis using alternative clustering choices. Online Appendix Figure A21 and A22 report the results when standard errors are clustered at the (i) birth-cohort level, (ii) country level, (iii) and two-way at the country and cohort level. The results are very stable.

3.3 Intergenerational Transmission

To evaluate the intergenerational transmission of the disaster impact on tightness, I examine which values parents feel are important to teach their children. I measure this using a question, taken from the WVS, that asks parents which values they believe are important to instill in their children. "Here is a list of qualities that children can be encouraged to learn at home. Which, if any, do you consider especially important?" Respondents are then given a list of the following eleven traits: (i) Good manners; (ii) Obedience; (iii) Feeling of responsibility; (iv) Tolerance/respect for others; (v) Unselfishness/generosity; (vi) Imagination; (vii) Independence; $(viii)$ Determi-

nation/perseverance; (ix) Hard work; (x) Thrift; (xi) Religious faith. Respondents were able to respond "yes" to any of the traits they felt were important to instill in their children.

I begin by first examining the extent to which past exposure to disasters affects whether individuals believe it is important, on average, to teach children these values, which I measure by the share of the 11 traits the respondent answered yes to. Estimates of equation 3 with this measure as the outcome variable are shown in the left-hand plot of Figure 3. I find that, on average, no statistically significant effect of exposure to disasters on the importance of teaching these values to children.

I next turn to the traits that are most clearly related to tightness of social norms: good manners, obedience, and feeling of responsibility. Using responses for these traits, we examine the fraction of the three traits that a respondent believes are important to instill in children. Estimates of equation 3 with this measure as the dependent variable are shown in the right-hand plot of Figure 3. I find a positive and statistically significant effect of exposure to disasters on the importance of instilling these values in their children. The effect size is similar to the one documented before for tightness.

The results suggest that exposure to disasters results in greater parental investments to instill values related to tight social norms in children. This indicates that the impact of disasters on tightness is passed down to the subsequent generation.

3.4 From Tightness of Norms to Homogeneity in Behavior

In this section, I examine the behavioral consequences of disaster-induced tightness. Having shown that exposure to disasters reduces variance in normative opinions, I now ask whether this effect translates into greater behavioral conformity.

I start by investigating if exposure to disasters decrease variance in prosocial behavior. There is much evidence that social norms are an important determinant of prosocial behavior, including volunteering, helping strangers, and charitable giving (Benabou and Tirole, 2006; Ariely et al., 2009; DellaVigna et al., 2012; Kimbrough and Vostroknutov, 2016). I use survey data on self-reported prosocial behavior from the Gallup World Polls (GWP). GWP are a series of nationally representative surveys fielded worldwide, collecting responses from approximately 1,000 individuals in each country and survey wave. The surveys include the following question: Have you done any of the following in the past month?: (i) donated money to a charity; (ii) volunteered your time to an organization; (iii) helped a stranger or someone you did not know who needed help. Respondents were able to respond "yes" to any of the actions they had performed in the month preceding the survey interview.

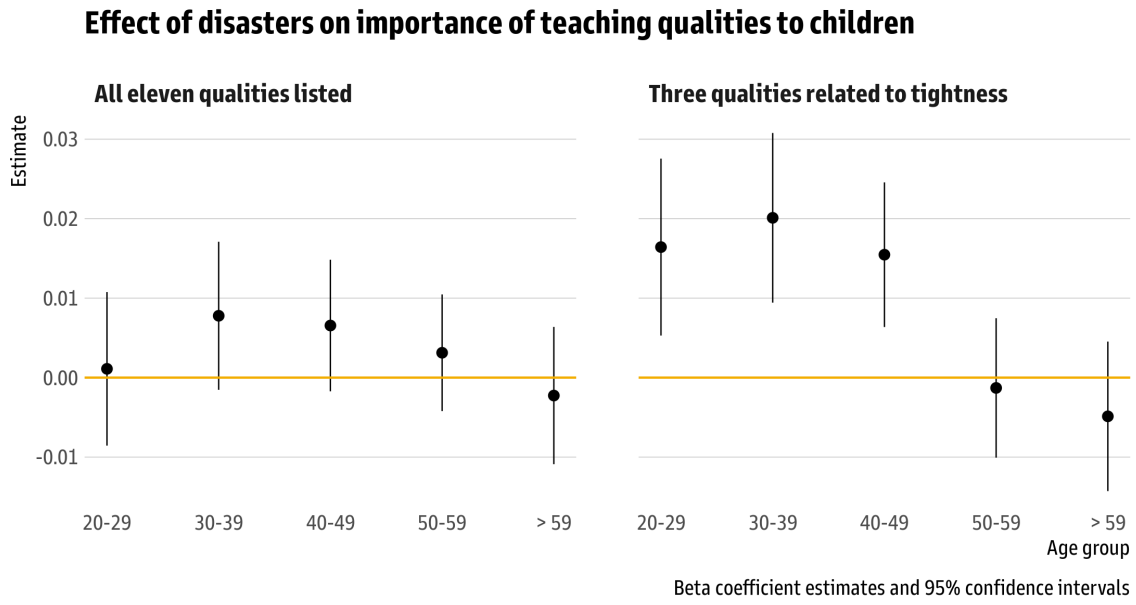

Figure 3: Testing for intergenerational transmission of the effect of exposure to disasters on tightness of social norms

Notes: The figure plots the coefficients and the 95 percent confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from two regressions which include fixed effects for country-of-residence $\times$ survey-year and birth cohort and controls for gender, age, and age squared. The outcome is the fraction of qualities that the respondent reports being important to teach children at home. In the graph on the left, the dependent variable is the average across eleven quantities. In the graph on the right, the dependent variable is the average across three qualities that are related to tightness: obedience; good manners; and responsibility. Disasters include epidemics, wars, economic disasters, earthquakes, and droughts. All variables are normalized to z-scores.

I examine the extent to which past exposure to disasters affects how similar individuals behave relative to their reference group in adulthood. To construct the dependent variable, I follow as much as possible the procedure used to compute revealed tightness above. For each of the three questions, I define the social norm in a given reference group as the modal action among members of that reference group and set an indicator equal to one if the individual who belongs to that reference group performed the action. I then compute the share of indicators equalling one to get a single index of norm adherence. The only difference to the procedure above is how I define reference groups. Before, I defined reference groups as individuals living in the same subnational district and belonging to the same ethno-linguistic group. It is impossible to adopt this definition here because the GWP does not record ethnic groups or language. Instead, I define reference groups as

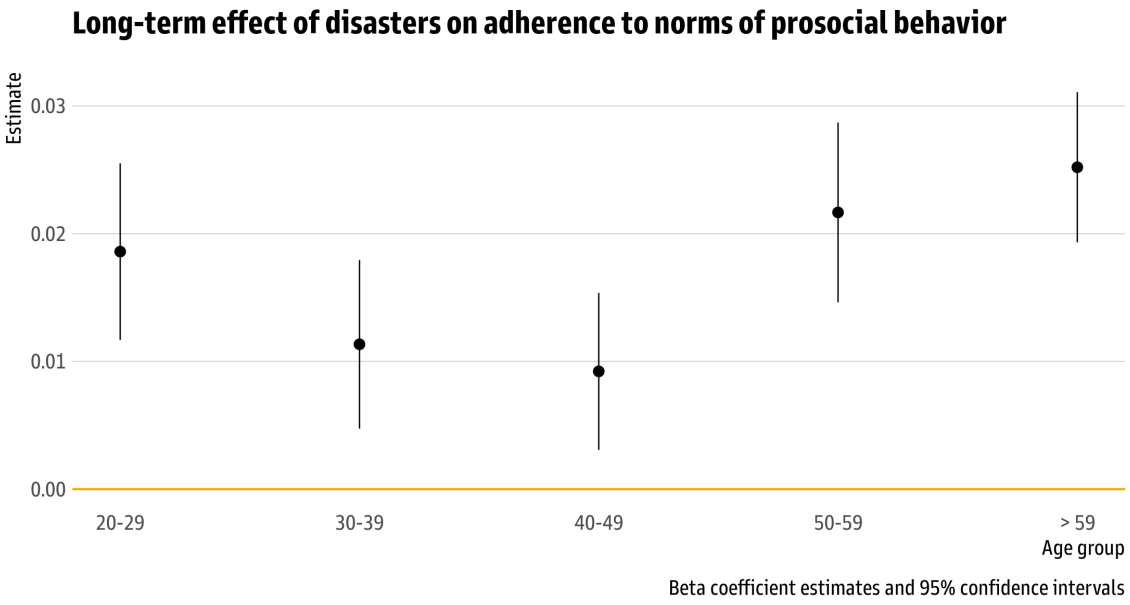

Figure 4: Testing for behavioral consequences of the effect of past exposure to disasters

Notes: The figure plots the coefficients and the 95 percent confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from a regression which includes fixed effects for country-of-residence $\times$ survey-year, birth cohort, gender, and age groups. The outcome is the the share of respondents' actions that match the local norms of prosocial behavior. Disasters include epidemics, wars, economic disasters, earthquakes, and droughts. All variables are normalized to z-scores.

individuals from the same subnational district, which results in 18 groups per country on average.

I estimate equation 3 with this measure as the outcome variable. The estimates are shown in Figure 4. I find a positive and statistically significant effect of past exposure to disasters on adherence to prosocial norms. Strikingly, the effect size is almost identical the effect size for tightness of social norms of cooperation for individuals younger than 50 and even larger for older individuals. The results are also very similar if I additionally control for the content of the social norms, that is, include fixed effects for the modal behavior among individuals of the same reference group (see Online Appendix Table A10).

This result suggests that past exposure to disasters results in greater adherence to local prosocial norms. This indicates that the impact of disasters on tightness translates into lower variance in behavior.

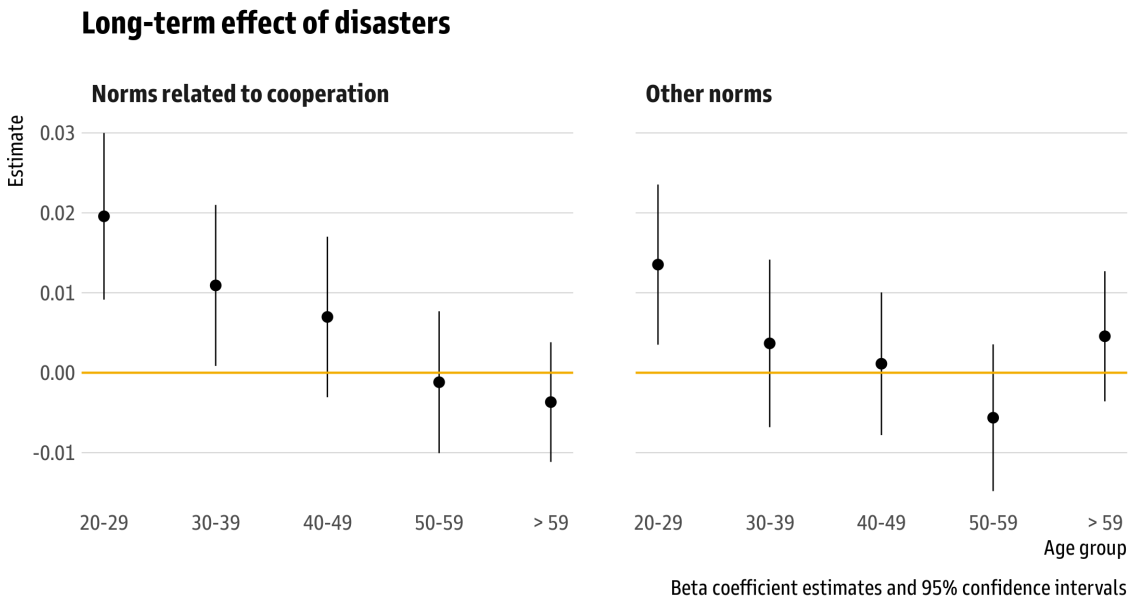

Figure 5: Long-run effects of exposure to disasters on tightness of norms of cooperation versus tightness of other norms

Notes: The figure plots the coefficients and the 95 percent confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from two regressions which include fixed effects for country-of-residence $\times$ survey-year, birth cohort, gender, and age. The outcomes are tightness of norms related to cooperation (left) and tightness of other norms (right). Disasters include epidemics, wars, economic disasters, earthquakes, and droughts. All variables are normalized to z-scores.

4 Mechanisms

4.1 Conceptual Framework

4.2 Additional Evidence Consistent with the Framework

Tightness of norms related to cooperation versus other norms

Heterogeneity between countries with high and low state capacity I now break down the effects of past exposure to disasters on tightness of social norms by low-income and high-income countries. Individuals in low-income countries live closer to subsistence levels and governments typically lack the resources for disaster relief. Disasters are therefore more likely to threaten the survival of entire communities.

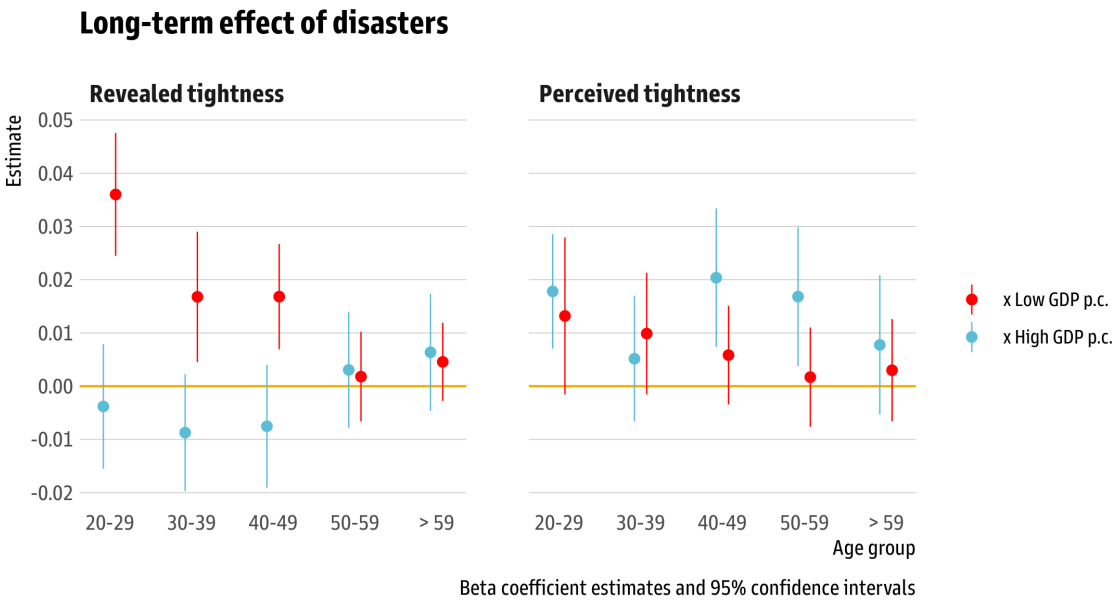

Figure 6: Heterogeneity in the long-run effects of exposure to disasters on tightness of social norms

Notes: The figure plots the coefficients and the 95 percent confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from four regressions which include fixed effects for country-of-residence $\times$ survey-year and birth cohort and controls for gender, age and age squared. The outcomes are revealed tightness (left) and perceived tightness (right). Coefficients colored in red (blue) are from regressions using the subsample of countries whose average GDP per capita 2000-2015 is below (above) the sample median. Disasters include epidemics, wars, earthquakes, and droughts, but exclude economic disasters. All variables are normalized to z-scores.

In Figure 6, I present an overview of the estimated coefficients using the baseline specification. The figure plots the coefficients and the 95 percent confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from four regressions which include fixed effects for country-of-residence $\times$ survey-year and birth cohort and controls for gender, age and age squared. The outcomes are revealed tightness (left) and perceived tightness (right). Coefficients colored in red (blue) are from regressions using the subsample of countries whose average GDP per capita 2000-2015 is below (above) the sample median. Disasters include epidemics, wars, earthquakes, and droughts, but exclude economic disasters. I find stronger effects on revealed tightness in low-income countries but no difference in perceived tightness.

Symmetry of the Effect

5 Conclusion

Why do some people hold more tightly to social norms and are less tolerant for those who do not than others? This paper has examined whether exposure to large adverse events affects the tightness of norms. Combining data on the occurrences of conflicts, epidemics, and natural and economic disasters with rich survey data, this paper has demonstrated that individuals who were exposed to disasters cling more tightly to their social norms because of greater returns to social coordination. To the degree that local norms prescribe cooperative behavior, disasters promote prosociality. However, disasters can also increase adherence to other norms, including non-cooperative ones. The effect of disasters on the tightness of norms are most pronounced in countries with low state capacity, where social norms tend to be more important to maintain social coordination.

The results shed light on three related issues. First, they provide a rationale for why some societies are more culturally diverse than others. A large literature demonstrates that the cultural differences we see today across societies are the result of an evolutionary process. The findings in this paper suggest that this evolutionary logic also applies to differences in within-country *variability* in these cultural traits across societies. When social norms are tight, it restricts the scope of acceptable behaviors, beliefs, and attitudes, and thereby fosters cultural homogeneity.

Second, the paper offers a novel explanation for why short-run adverse shocks such as conflict sometimes lead to greater cooperation within groups. By tightening local norms, such shocks promote prosocial behavior if local norms are prosocial.

Third, the paper demonstrates that individuals who experience threats to their living standards cling more tightly to their communitys norms, values, and beliefs, and become less tolerant of others who behave or think differently, even within relatively short periods. This is especially relevant for our understanding of the growing polarization in many Western society.

Future research might be able to study how disasters affect individuals' willingness to sanction those who deviate from the norms.

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A Additional Figures and Tables

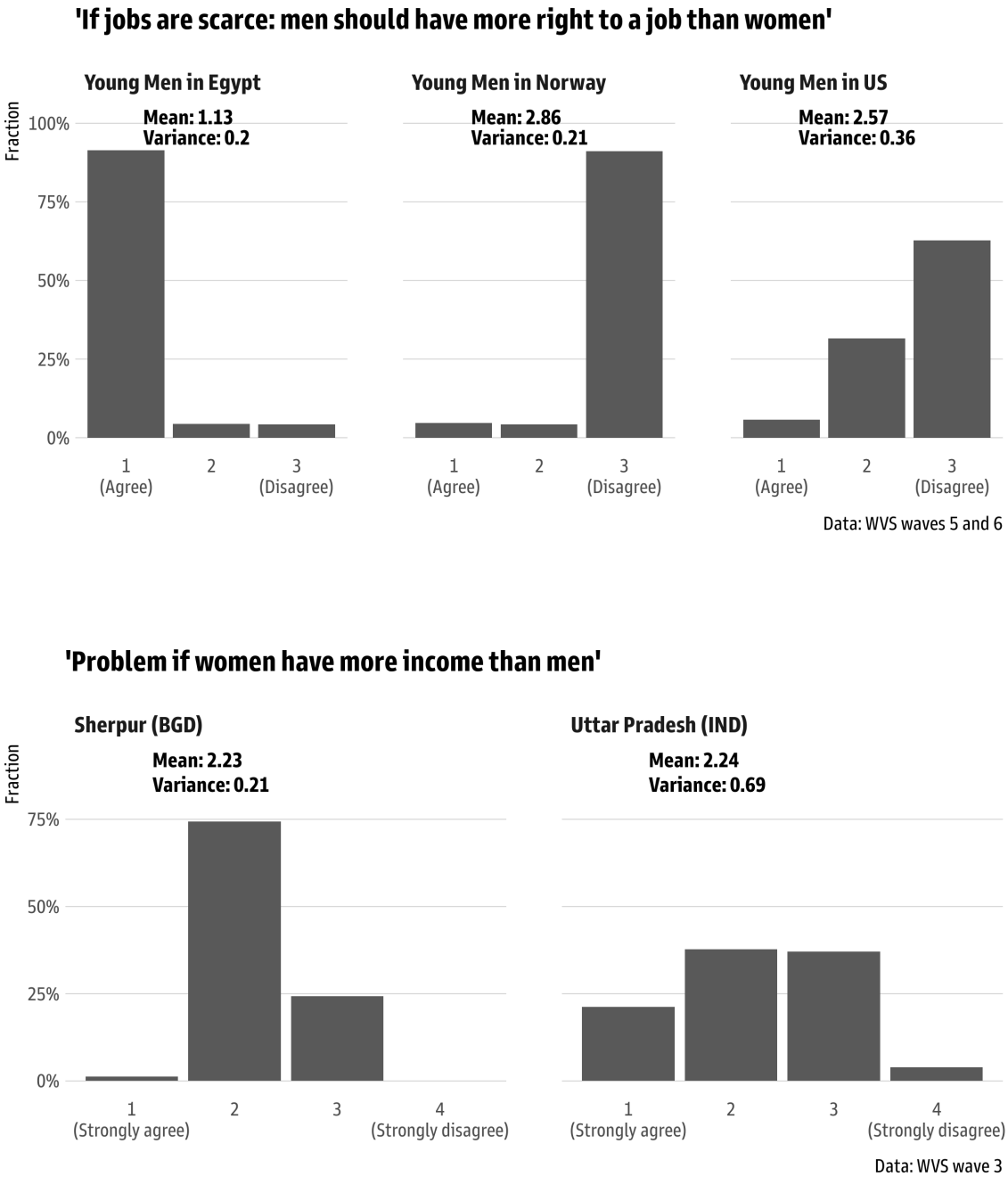

Figure A1: Examples of content and tightness of two gender norms taken from the WVS

Figure A2: Stability of social norms in the ESS. The figure shows the correlation coefficients between social norms in t and $t-1$.

Figure A3: Country-level averages of perceived tightness of social norms over years in the ESS

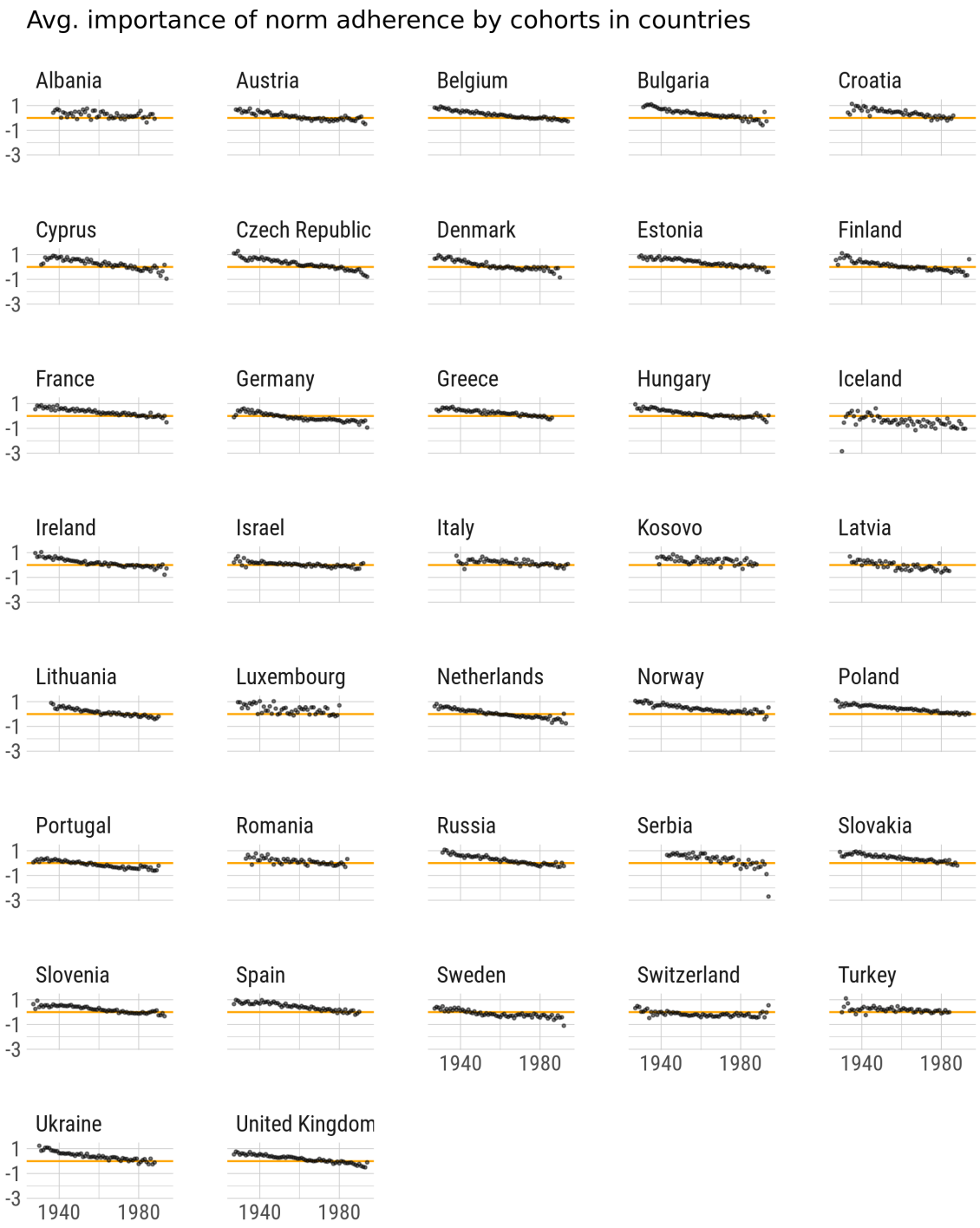

Figure A4: Cohort-level averages of perceived tightness of social norms by countries in the ESS

A.1 Analysis of Short-run Effects

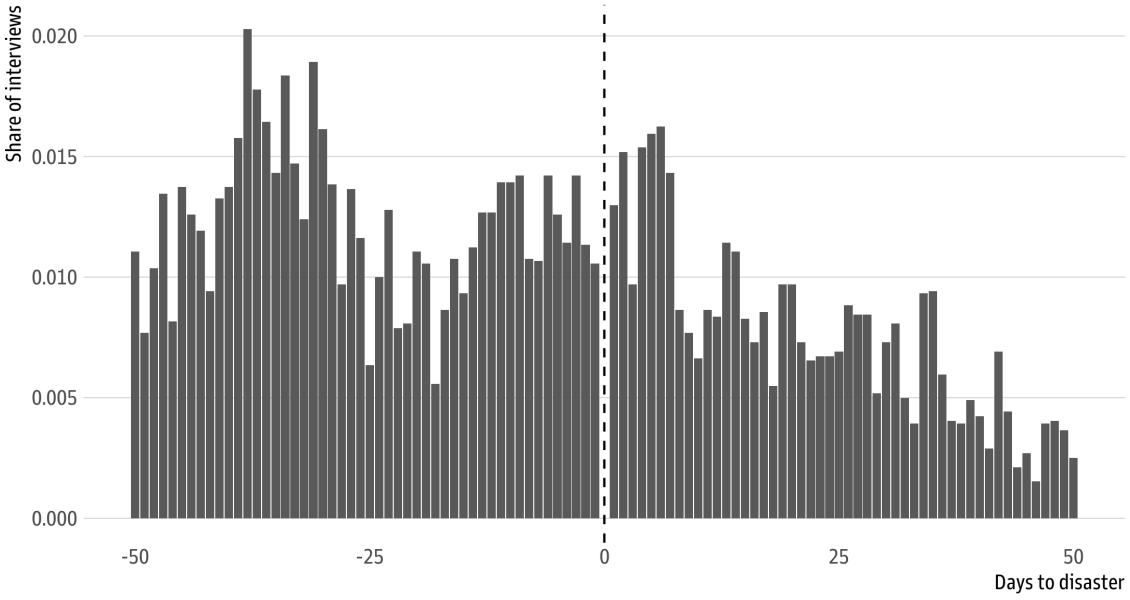

Figure A5: Density of interviews in the proximity of disasters in the event study analysis

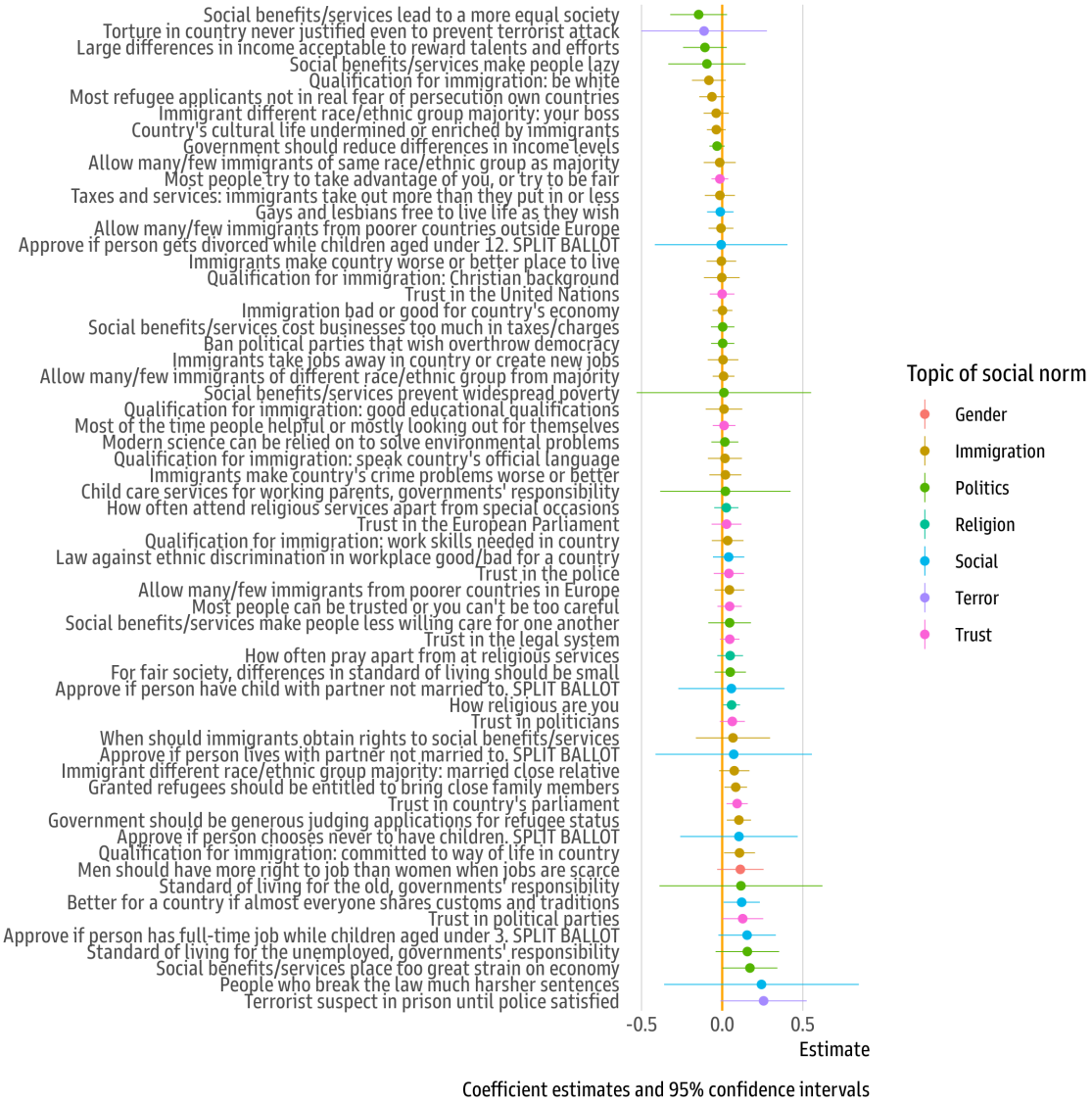

Figure A6: The effects of disasters on tightness of individual norms

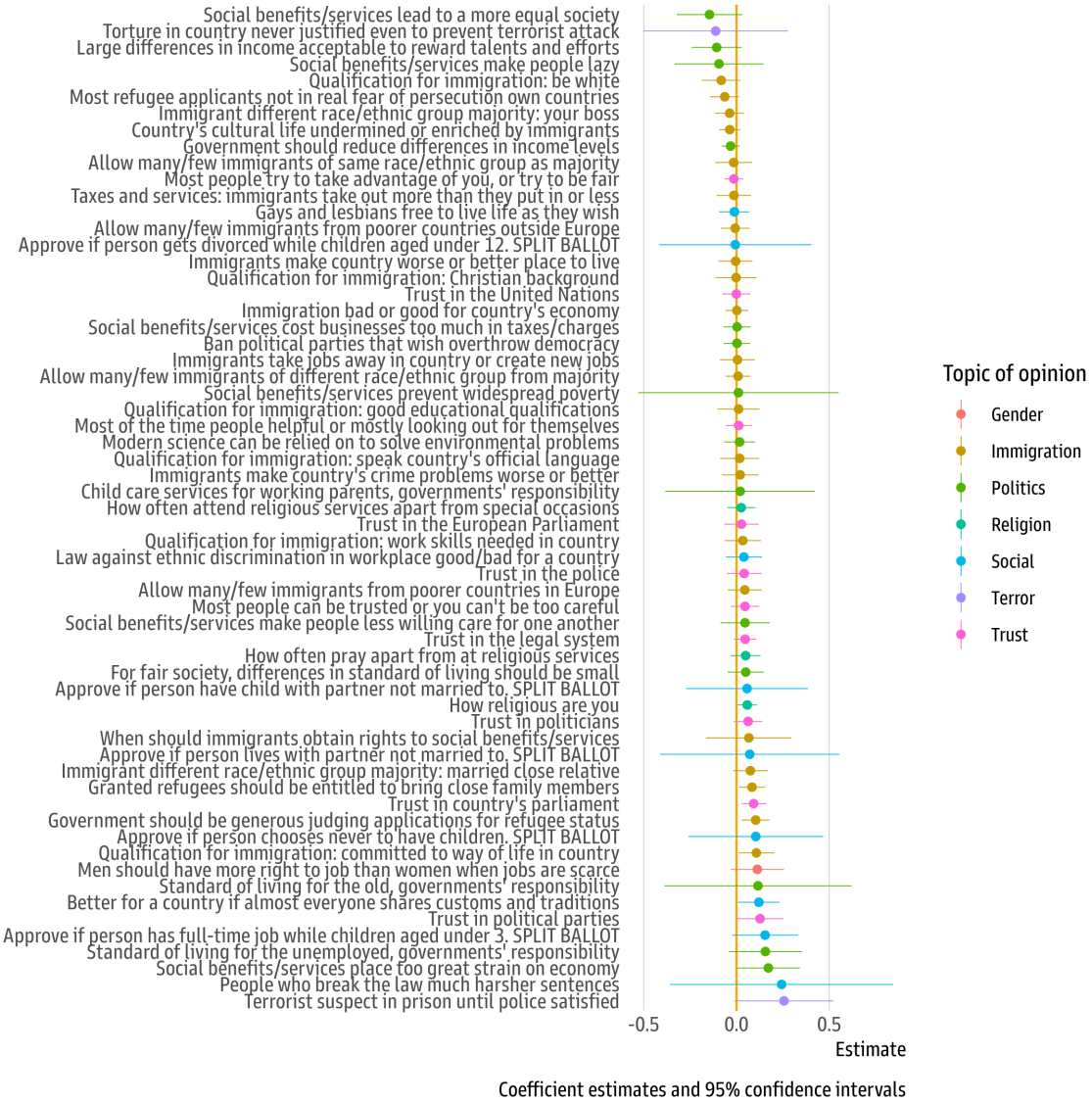

Figure A7: The effect of disasters on opinions

Table A1: Summary statistics event study analysis

Statistic	N	Mean	St. Dev.	Min	Max
Dependent variables					
Revealed tightness of social norms	10,407	0.000	1.000	−4.910	3.609
Perceived tightness of social norms	10,407	0.000	1.000	−3.803	3.233
Independent variable					
Post-Disaster	10,407	0.377	0.485	0	1
Individual covariates					
Age	10,407	45.781	17.364	18	99
Gender, male	10,407	0.482	0.500	0	1
Married	10,407	0.575	0.494	0	1
Education	10,407	3.102	2.127	1	55
Unemployed	10,407	0.055	0.227	0	1

Notes: Revealed tightness of social norms is computed based on the same data set, as described in the text. *Perceived tightness of social norms* is taken from waves 1 to 9 of the European Social Survey (ESS) conducted in years 2002-2018. *Post-Disaster* is an indicator equal to one in the days following a lethal conflict event, outbreak of an epidemic, or natural disaster (lethal earthquake, flood, or storm). The ESS also contains individual-level covariates, including age, gender, marital status, highest educational attainment, and unemployment status.

Table A2: Robustness: controlling for the content of social norms

	Revealed tightness (std.)		
	(1)	(2)	(3)
Post-Disaster	0.061**	0.065***	0.064***
	(0.028)	(0.013)	(0.012)
Disaster × region FE	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes
Norm content FE	No	Yes	Yes
N	10,407	8,009	8,009
R^2	0.075	0.093	0.086

Notes: OLS estimates, robust standard errors are clustered on subnational regions × survey wave and reported in parentheses. An observation is an individual. The sample includes respondents interviewed within 50 days before and after a disaster hits the region in which the individual was surveyed. The outcome is revealed tightness of social norms, based on the balanced subset of norms for which most respondents replied, as defined in the text. The main independent variable is *Post-Disaster*, which takes value one if the respondent was interviewed in the 50 days after a disaster, and zero otherwise. All regressions include disaster × region fixed effects; individual-level controls contain dummies equal to one if the respondent is male, married, or unemployed and fixed effects for age and highest educational attainment; norm content fixed effects contain dummies for the modal opinions used to compute revealed tightness. The modal values are computed for each disaster treatment status separately. In column 3, revealed tightness is computed based on social norms that are computed for each treatment status separately. The dependent variable is normalized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A3: Robustness: alternative definitions of reference groups

	Revealed tightness (std.)				
	(1)	(2)	(3)	(4)	(5)
Post-Disaster	0.102***	0.041	0.062	0.120***	0.128***
	(0.030)	(0.036)	(0.037)	(0.031)	(0.029)
Disaster × region FE	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	Yes
N	10,407	10,615	10,615	10,347	10,244
R^2	0.079	0.099	0.101	0.071	0.073

Notes: OLS estimates, robust standard errors are clustered on subnational regions × survey wave and reported in parentheses. An observation is an individual. The sample includes respondents interviewed within 50 days before and after a disaster hits the region in which the individual was surveyed. The outcome is revealed tightness of social, based on different definitions of respondents' reference groups. In column 1, reference groups are defined based on subnational regions × ethnolinguistic group, reproducing the baseline results; in column 2, reference groups are defined based on countries; in column 3, they are defined based on subnational regions; in column 4; they are defined based on region × ethnolinguistic group × old / young; and in column 5; they are defined based on region × ethnolinguistic group × old / young × high / low-educated. The correlation coefficient between baseline definition and the alternative definitions is 0.83 or higher. The main independent variable is *Post-Disaster*, which takes value one if the respondent was interviewed in the 50 days after a disaster, and zero otherwise. All regressions include disaster × region fixed effects; individual-level controls contain dummies equal to one if the respondent is male, married, or unemployed and fixed effects for age and highest educational attainment. The dependent variable is normalized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A4: Persistence of event-study estimate of the effect of disasters on tightness of social norms

	Perceived tightness (std.)			
	+/- 50 days	+/- 100 days	+/- 150 days	+/- 200 days
	(1)	(2)	(3)	(4)
Post-Disaster	0.089***	0.055	0.063	0.056
	(0.030)	(0.038)	(0.044)	(0.045)
Disaster × region FE	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
N	10,407	13,905	20,040	20,922
R^2	0.120	0.133	0.131	0.133

Notes: OLS estimates, robust standard errors are clustered on subnational region × survey wave and reported in parentheses. An observation is an individual. The sample includes respondents interviewed within *x* days before and after a disaster. The outcome in all columns is perceived tightness of social norms. The main independent variable is *Post-Disaster*, which takes value one if the respondent was interviewed in the *x* days after a disaster, and zero otherwise. All regressions include disaster × region fixed effects; individual-level controls contain dummies equal to one if the respondent is male, married, or unemployed and fixed effects for age and highest educational attainment. The dependent variable is normalized to *z*-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A5: Persistence of event-study estimate of the effect of disasters on tightness of social norms

	Revealed tightness (std.)			
	+/- 50 days	+/- 100 days	+/- 150 days	+/- 200 days
	(1)	(2)	(3)	(4)
Post-Disaster	0.102***	0.124***	0.099**	0.070
	(0.030)	(0.045)	(0.049)	(0.044)
Disaster × region FE	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes
N	10,407	13,905	20,040	20,922
R^2	0.079	0.078	0.069	0.066

Notes: OLS estimates, robust standard errors are clustered on subnational region × survey wave and reported in parentheses. An observation is an individual. The sample includes respondents interviewed within *x* days before and after a disaster. The outcome in all columns is revealed tightness of social norms. The main independent variable is *Post-Disaster*, which takes value one if the respondent was interviewed in the *x* days after a disaster, and zero otherwise.All regressions include disaster × region fixed effects; individual-level controls contain dummies equal to one if the respondent is male, married, or unemployed and fixed effects for age and highest educational attainment. The dependent variable is normalized to *z*-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A6: Heterogeneity in event-study estimates

	Tightness of social norms					
	Revealed			Perceived		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Disaster	0.077*	0.169**	0.246***	0.146***	0.080*	0.087
	(0.040)	(0.074)	(0.065)	(0.053)	(0.045)	(0.062)
Post-Disaster × Age $40-60$	0.032			−0.039		
	(0.044)			(0.052)		
Post-Disaster × Age >60	0.041			−0.172**		
	(0.067)			(0.070)		
Post-Disaster × Education 2nd tercile		−0.144*			−0.011	
		(0.079)			(0.060)	
Post-Disaster × Education 3rd tercile		−0.074			0.013	
		(0.089)			(0.059)	
Post-Disaster × Income 2nd tercile			−0.179***			−0.009
			(0.065)			(0.064)
Post-Disaster × Income 3rd tercile			−0.245***			−0.026
			(0.078)			(0.075)
Disaster × region FE	Yes	Yes	Yes	Yes	Yes	Yes
N	10,407	10,397	7,072	10,407	10,397	7,072
R^2	0.063	0.063	0.072	0.099	0.060	0.066

Notes: OLS estimates, robust standard errors are clustered on subnational region × survey wave and reported in parentheses. An observation is an individual. The sample includes respondents interviewed within 50 days before and after a disaster hits the region where the individual is surveyed. The outcome is revealed tightness of social norms in columns 1 to 3 and perceived tightness of social norms in columns 4 to 6, as defined in the text. The main independent variable is *Post-Disaster*, which takes value one if the respondent was interviewed in the 50 days after a disaster, and zero otherwise. All regressions include disaster × region fixed effects. The specifications in columns 1 and 4 dummies for age groups; in columns 2 and 5 they contain dummies for education terciles; and in columns 3 and 6 they contain dummies for income terciles. The dependent variable is normalized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A7: Heterogeneity in event-study estimates: Minority groups

	Perceived tightness (std.)				
	(1)	(2)	(3)	(4)	(5)
Post-Disaster	0.104***	0.108***	0.099***	0.106***	0.084***
	(0.035)	(0.033)	(0.032)	(0.034)	(0.030)
Minority indicator		0.113*	0.055		
		(0.057)	(0.082)		
Post-Disaster × Minority indicator			0.106		
			(0.087)		
Discriminated group indicator				−0.086	−0.169**
				(0.057)	(0.076)
Post-Disaster × Discriminated group indicator					0.184**
					(0.077)
Disaster × region FE	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	Yes
N	10,407	10,196	10,196	10,296	10,296
R^2	0.120	0.120	0.121	0.120	0.121

Notes: OLS estimates, robust standard errors are clustered on subnational regions × survey wave and reported in parentheses. An observation is an individual. The sample includes respondents interviewed within 50 days before and after a disaster hits the region in which the individual was surveyed. The outcome is perceived tightness of social norms. All regressions include disaster × region fixed effects and individual-level controls, as in the main analysis. The dependent variable is normalized to *z*-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

A.2 Analysis of Long-run Effects

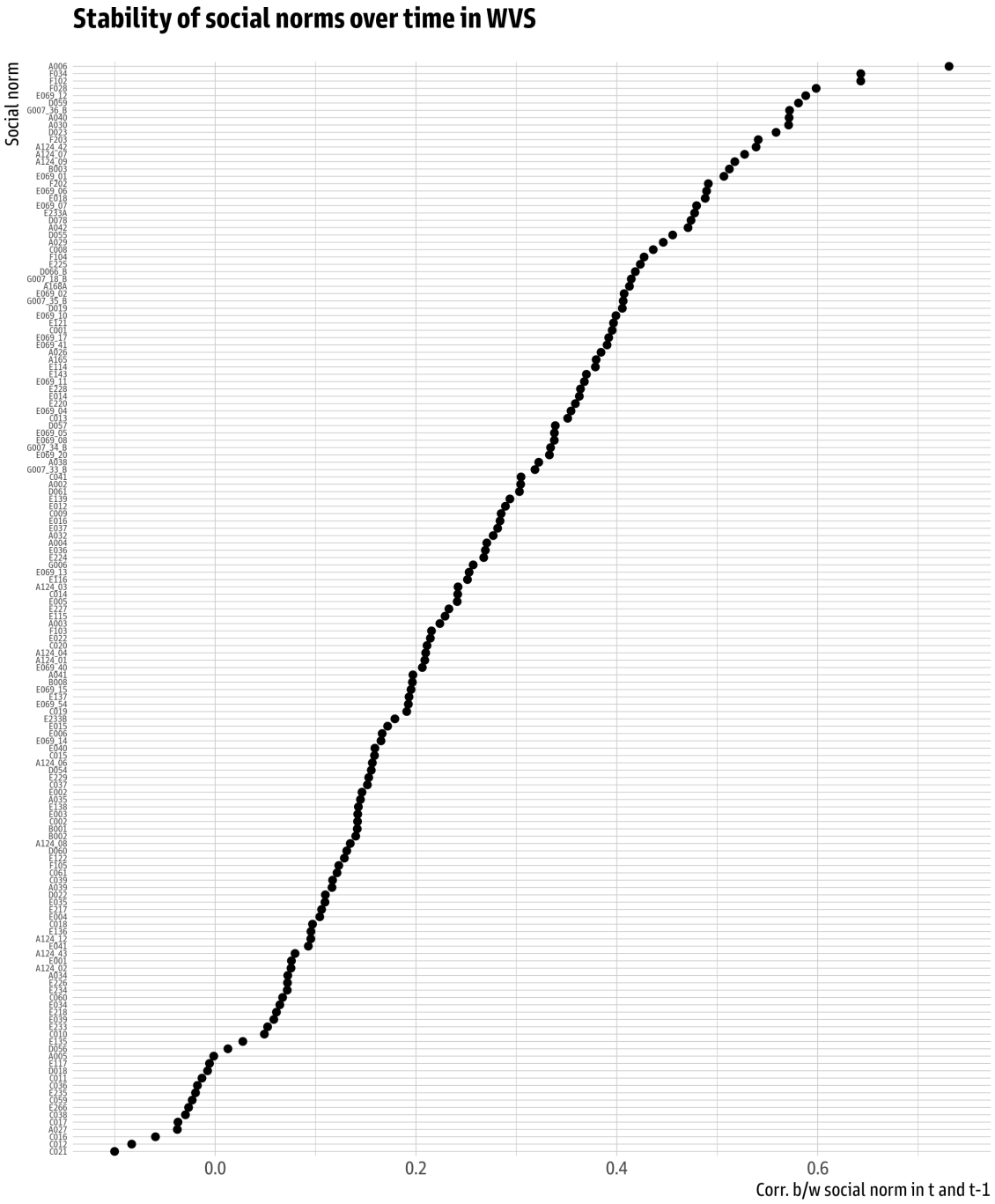

Figure A8: Stability of social norms at the region level in the WVS. The figure shows the correlation coefficients between social norms in t and $t-1$ at the region level.

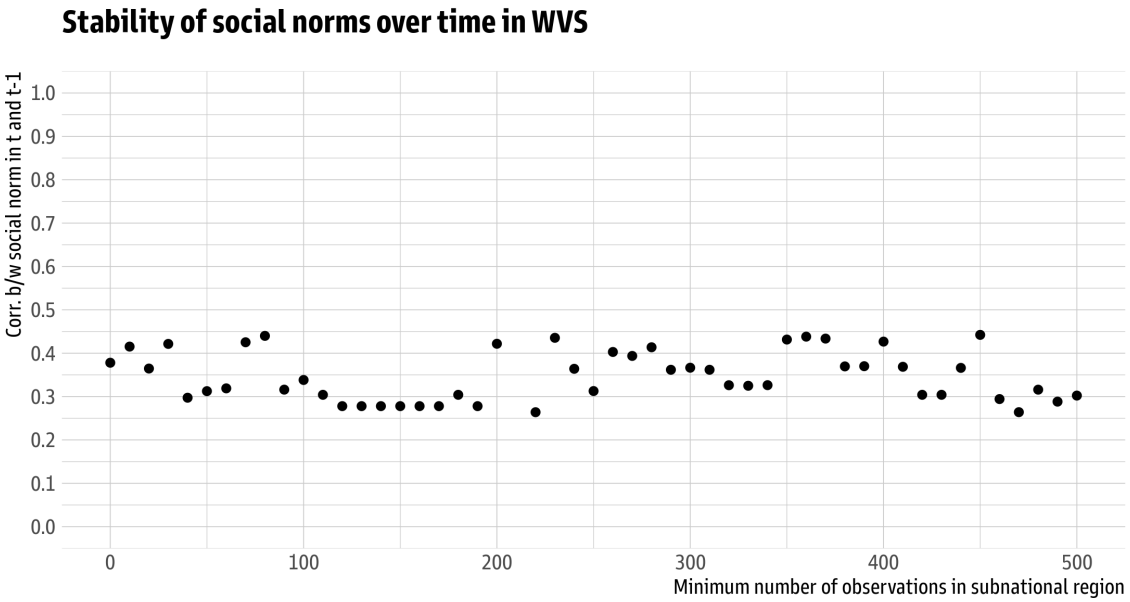

Figure A9: Stability of social norms at the region level in the WVS. The figure shows the correlation coefficients between social norms in t and $t-1$ at the region level. The x-axis denotes the cutoff in terms of minimum number of respondents in a region used to compute the correlation coefficient.

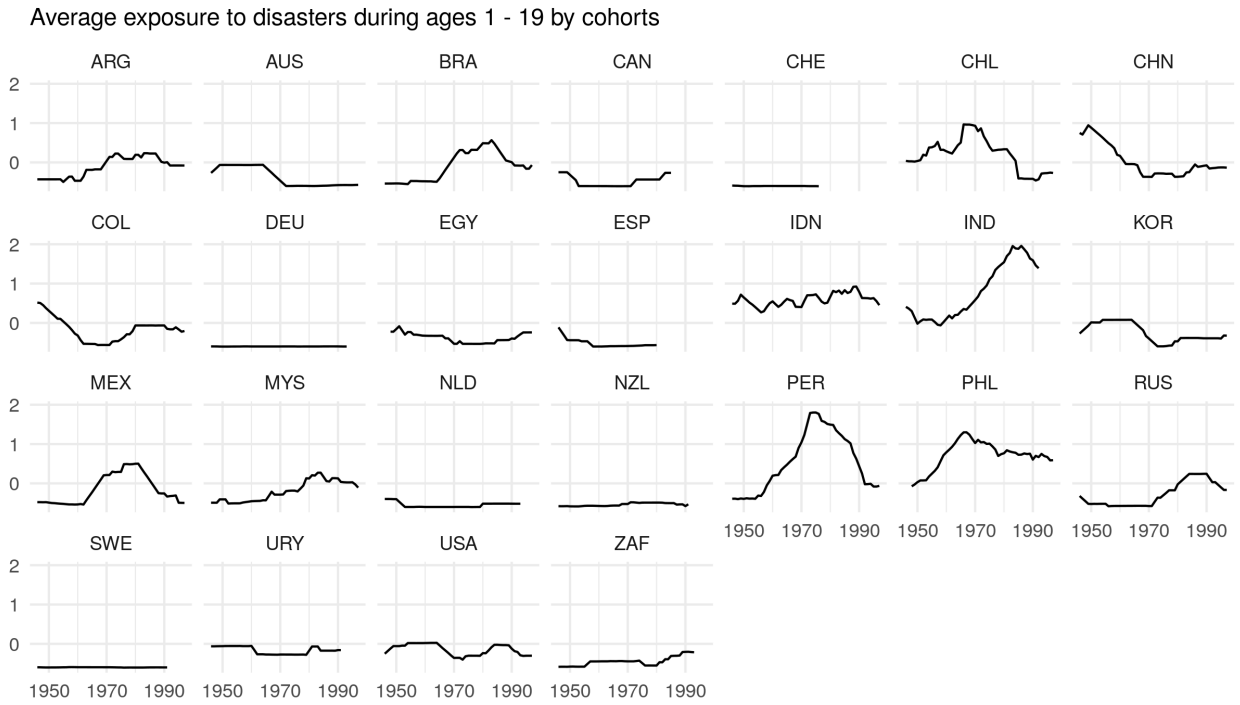

Figure A10: Cohort-level average exposure to disasters during ages 1 - 19. This variable is computed by computing the average of exposures to epidemics, war, economic disasters, and natural disasters (average of exposure to earthquakes and droughts), where all variables are standardized to z-scores.

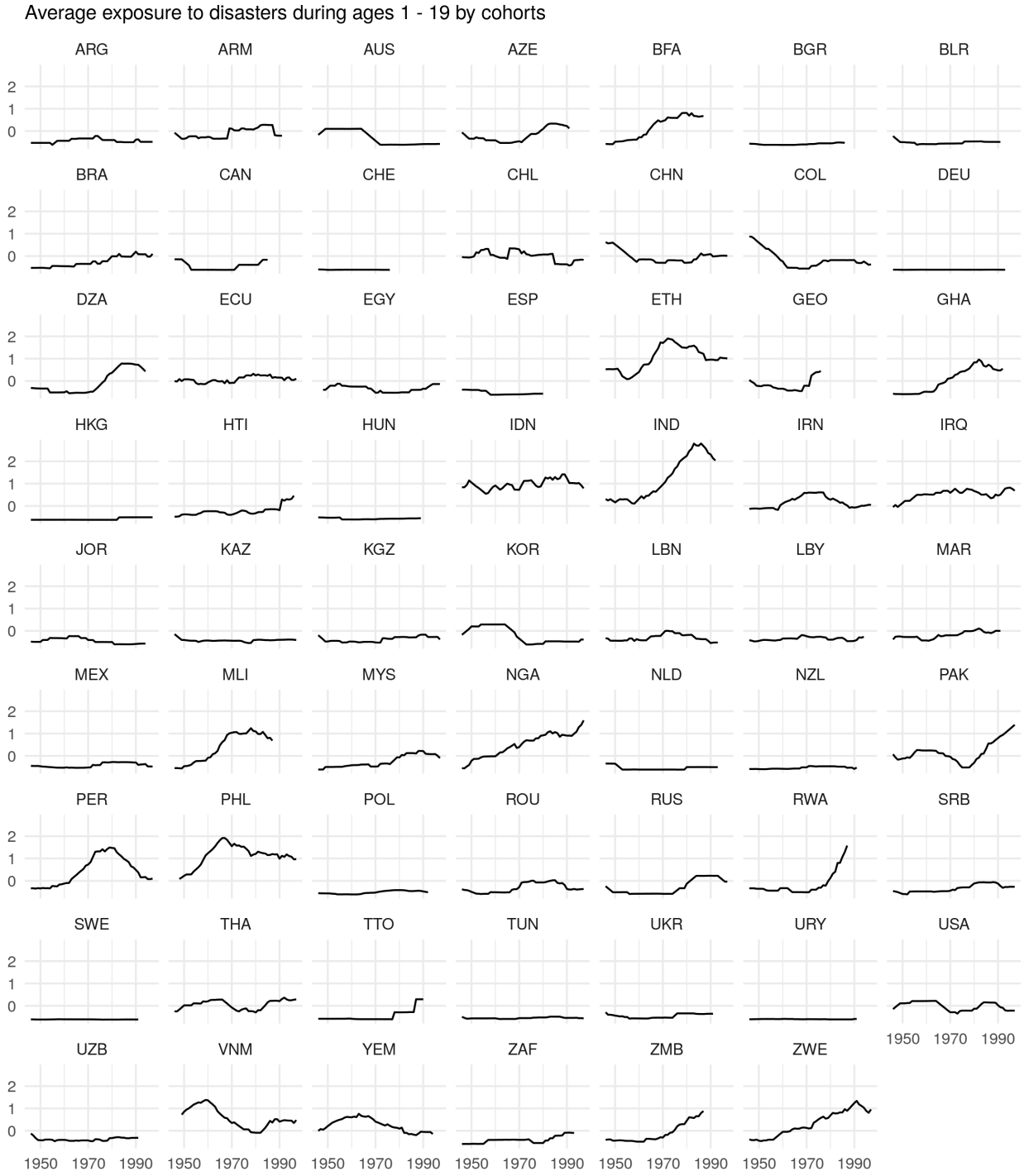

Figure A11: Cohort-level average exposure to disasters during ages 1 - 19. This variable is computed by computing the average of exposures to epidemics, war, and natural disasters (average of exposure to earthquakes and droughts), where all variables are standardized to z-scores.

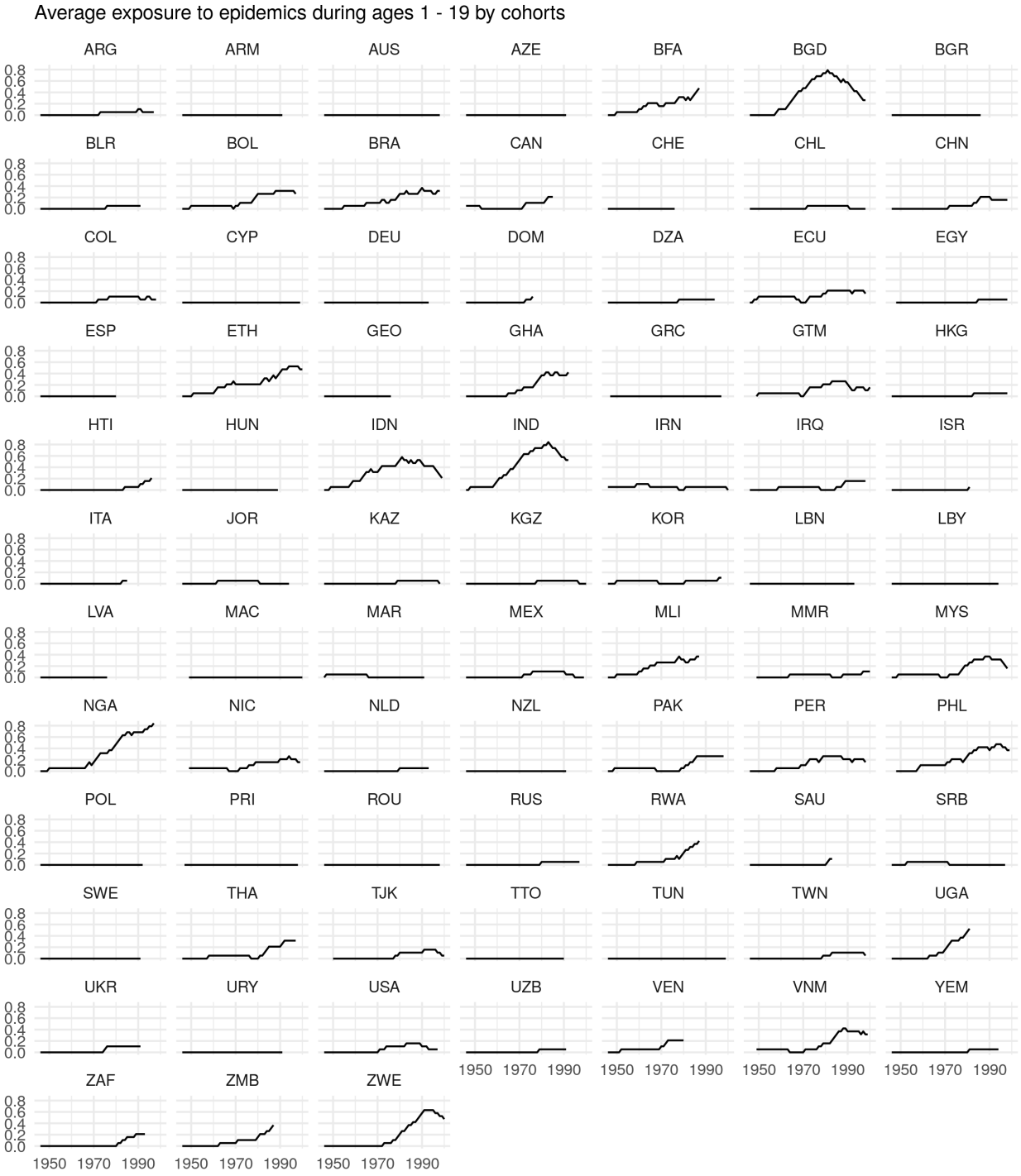

Figure A12: Cohort-level average exposure to epidemics during ages 1 - 19.

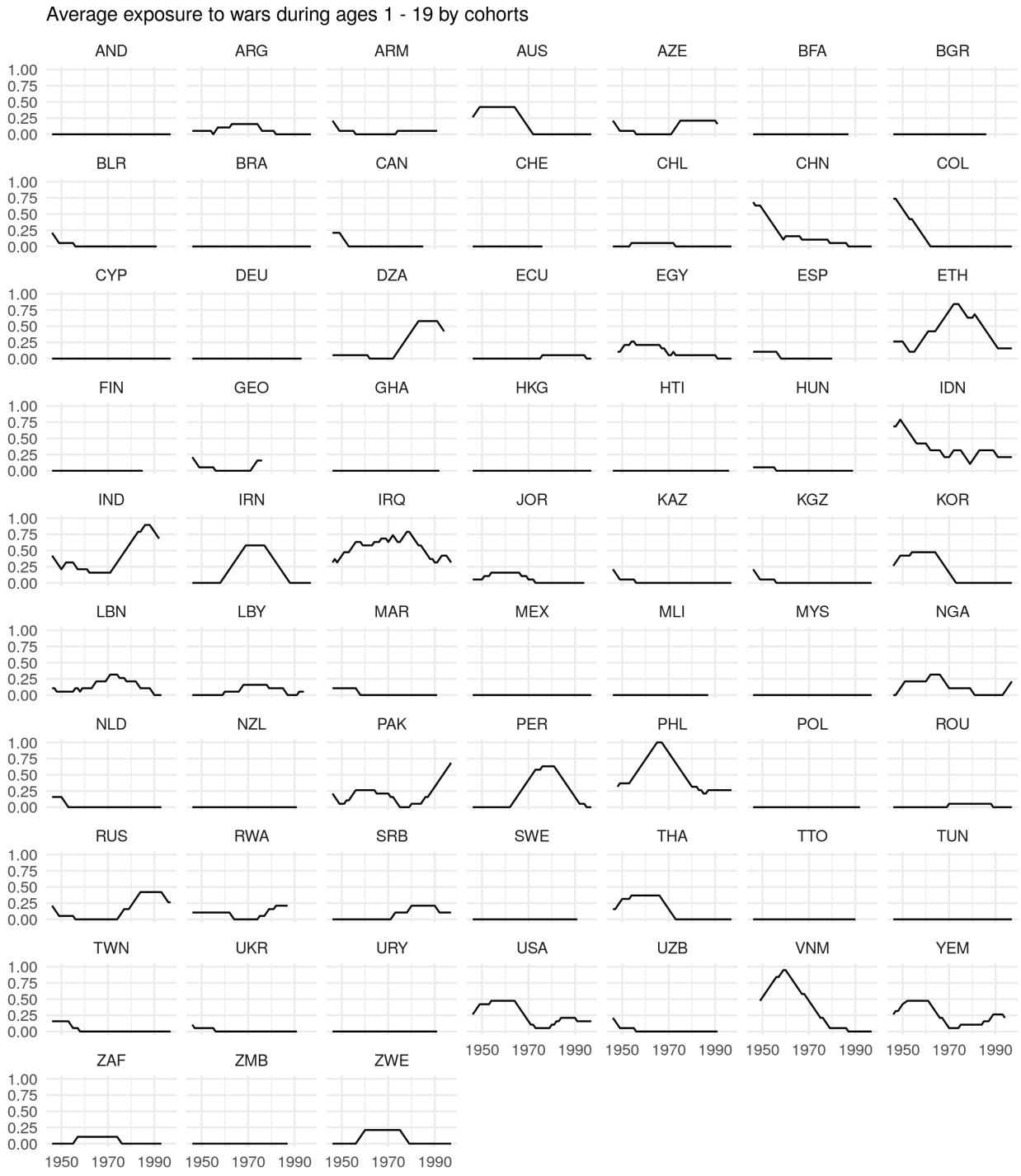

Figure A13: Cohort-level average exposure to wars during ages 1 - 19.

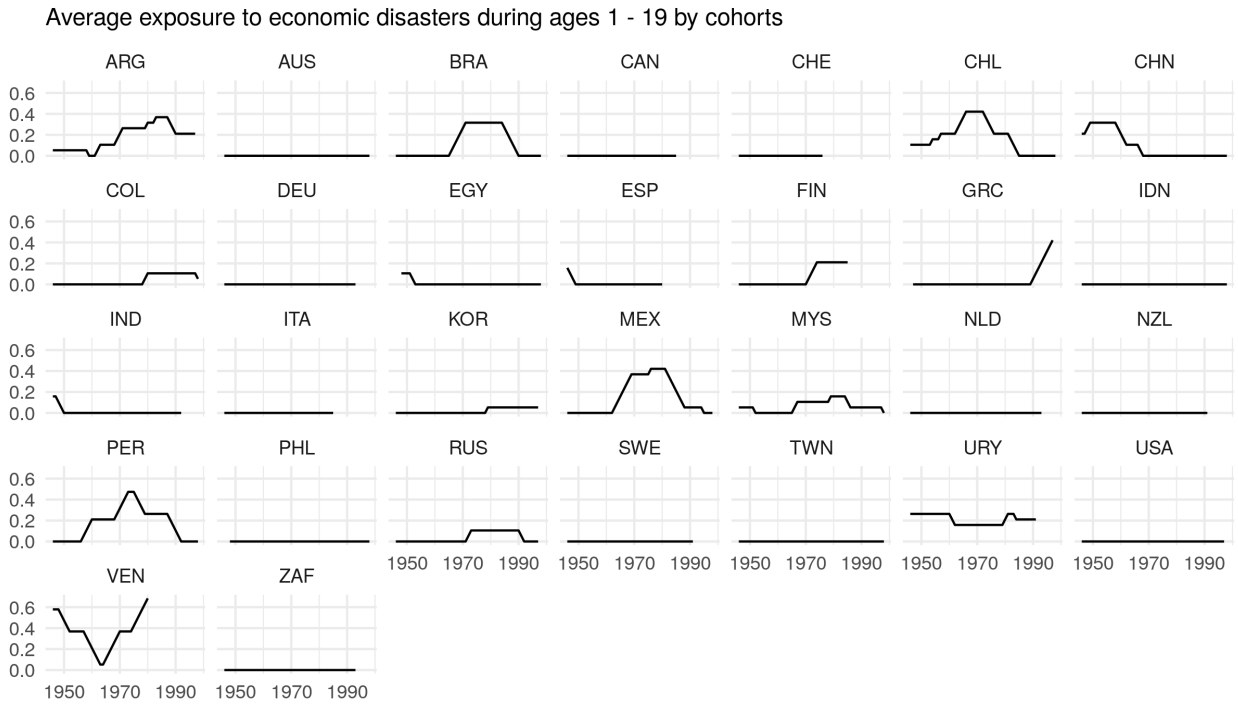

Figure A14: Cohort-level average exposure to economic disasters during ages 1 - 19.

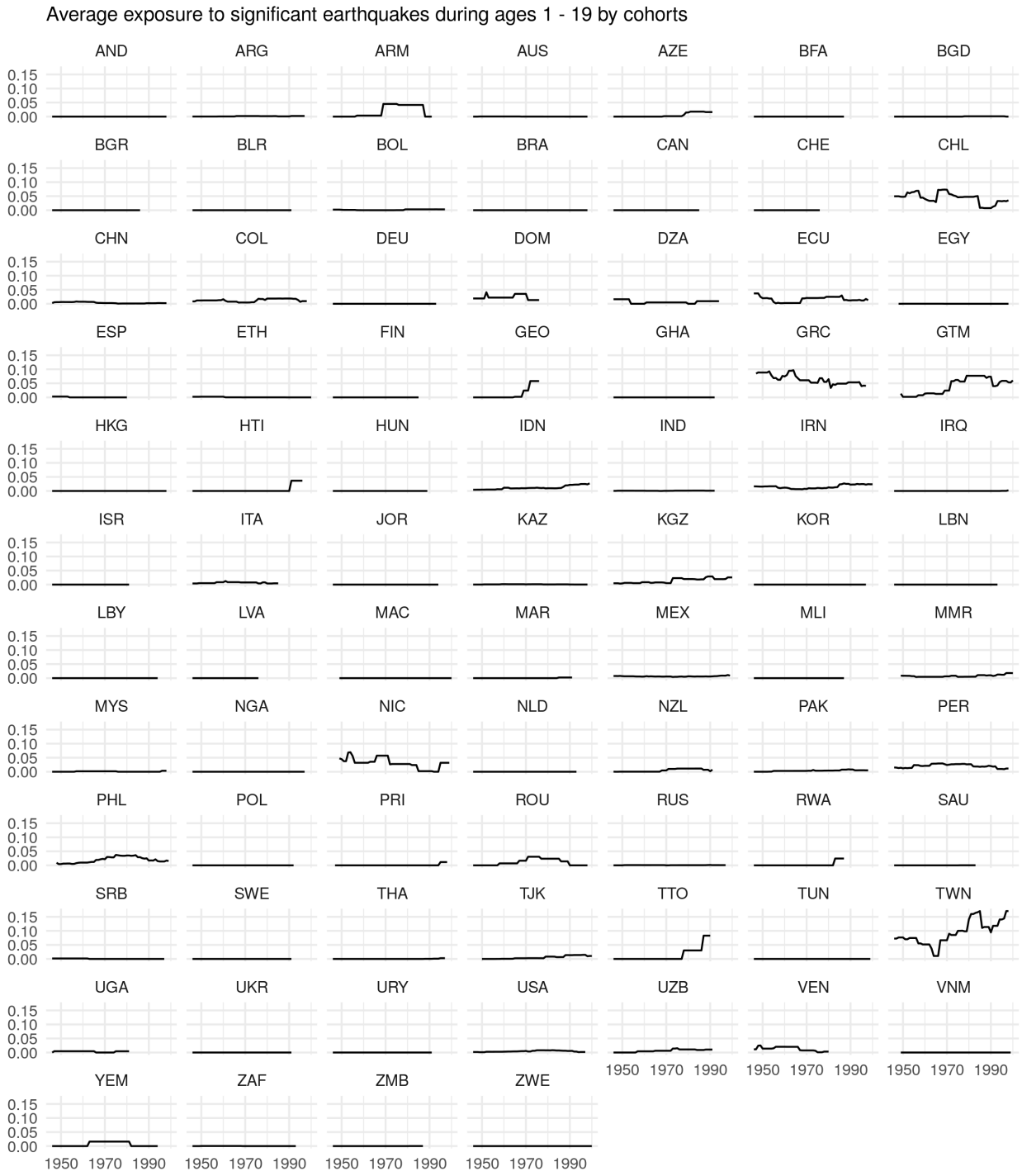

Figure A15: Cohort-level average exposure to significant earthquakes during ages 1 - 19.

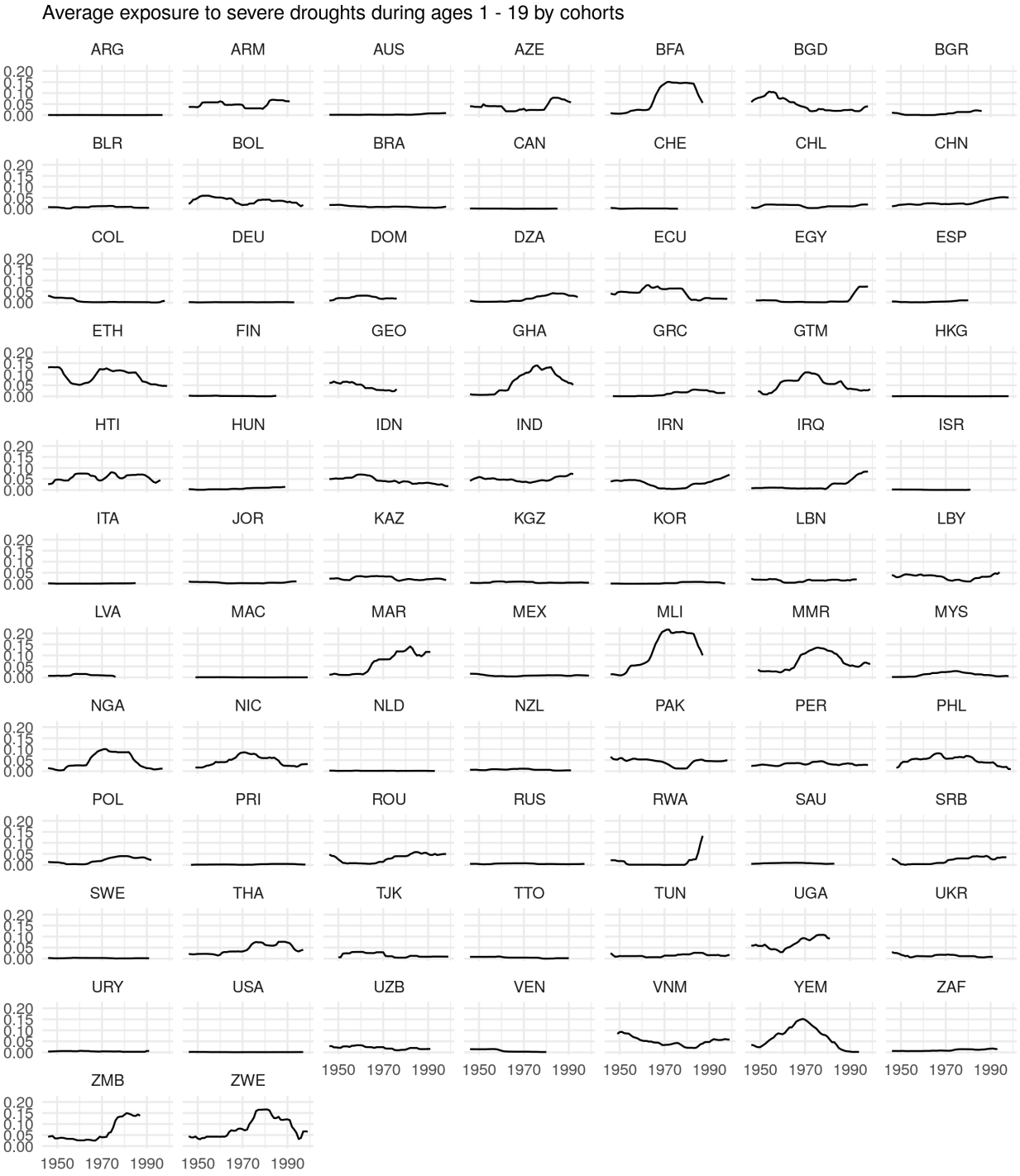

Figure A16: Cohort-level average exposure to significant earthquakes during ages 1 - 19.

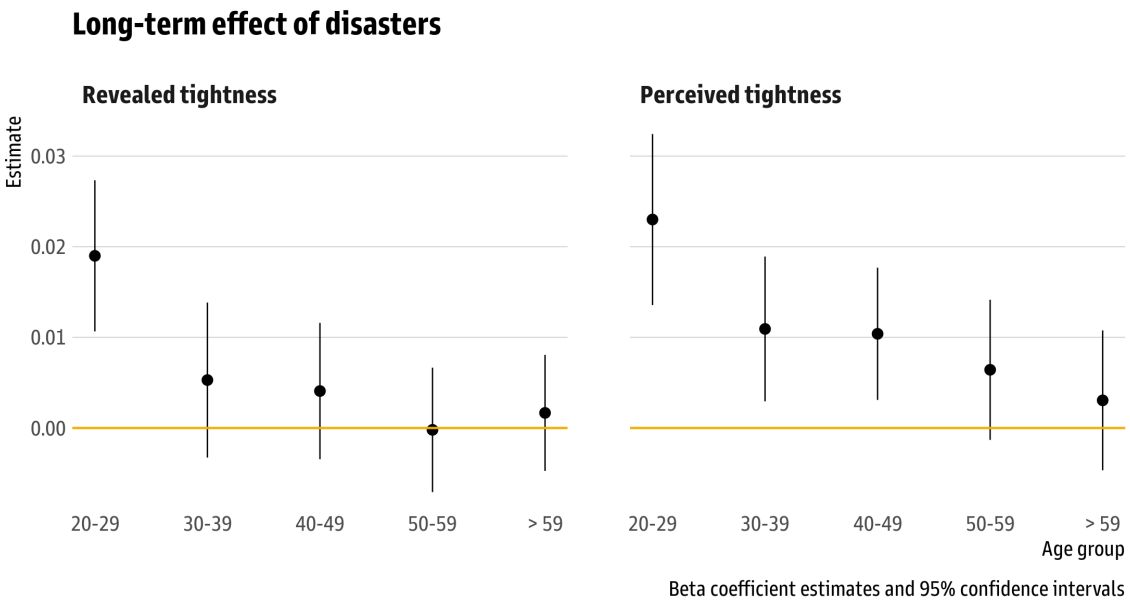

Figure A17: Robustness: Long-run effects of exposure to disasters on tightness of social norms, excluding economic disasters

Notes: The figure plots the coefficients and the 95% confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from two regressions which include fixed effects for country-of-residence $\times$ survey-year, birth cohort, gender, and age. The outcomes are revealed tightness (left) and perceived tightness (right). Disasters include epidemics, wars, earthquakes, and droughts, but exclude economic disasters. All variables are normalized to z-scores.

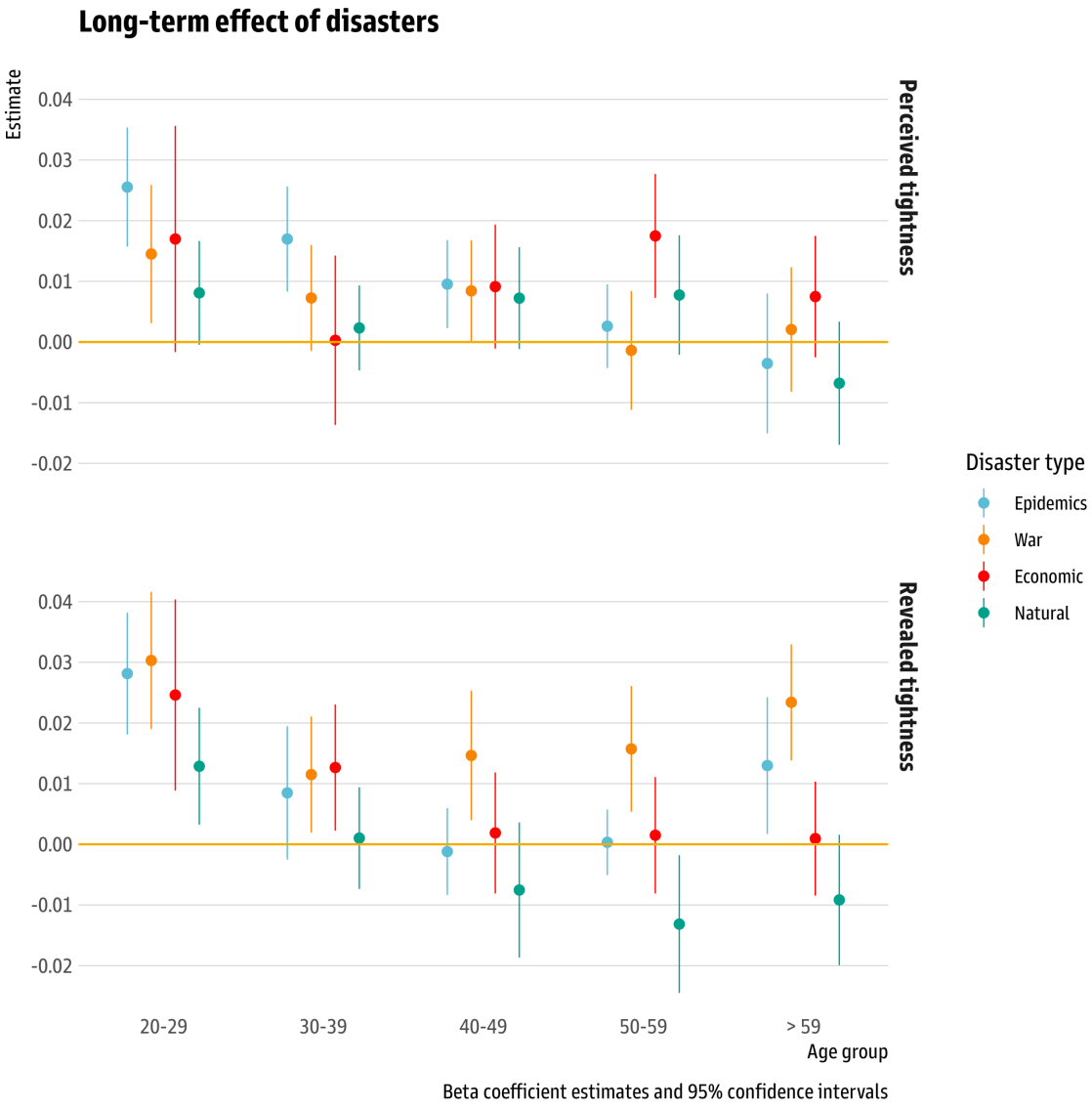

Figure A18: Long-run effects of exposure to disasters on tightness of social norms

Notes: OLS estimates, robust standard errors are clustered at the country-cohort level and reported in parentheses. The outcomes are perceived and revealed tightness. The main independent variables are exposure to epidemics, wars, economic disasters, and natural disasters during ages 4 to 20. These variables are interacted with age group dummies to track the evolution of the effect as individuals grow older. All regressions include fixed effects for country-of-current-residence $\times$ survey-year and birth cohort; the regressions also contain individual-level controls for gender, age, and age squared. All variables are normalized to z-scores.

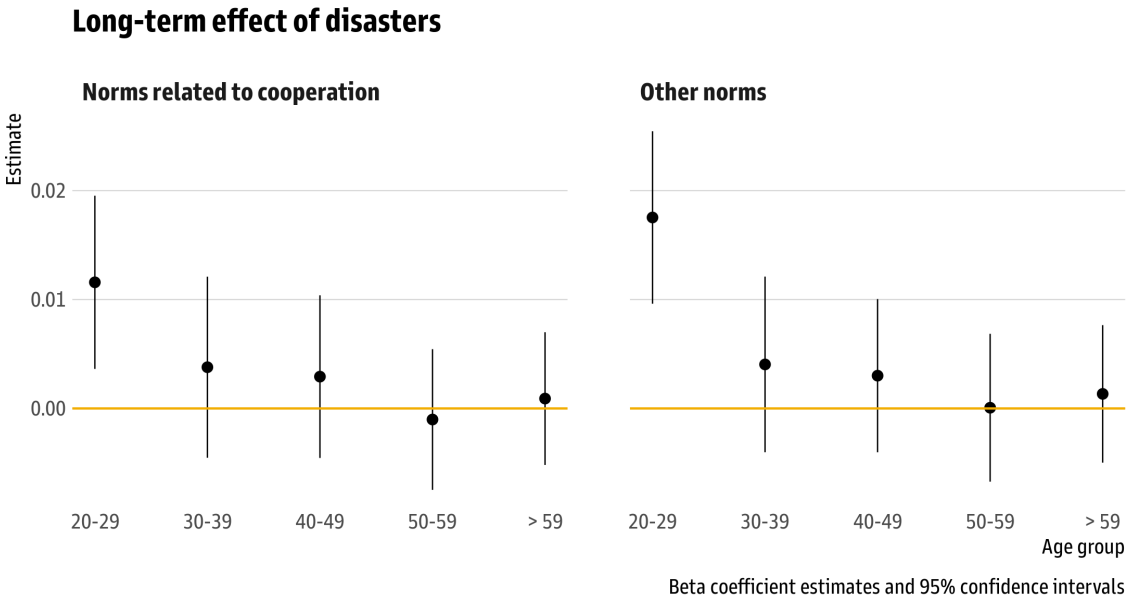

Figure A19: Robustness: cooperation versus other norms, excluding economic disasters

Notes: The figure plots the coefficients and the 95% confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from two regressions which include fixed effects for country-of-residence $\times$ survey-year, birth cohort, gender, and age. The outcomes are tightness of norms related to cooperation (left) and tightness of other norms (right). Disasters include epidemics, wars, earthquakes, and droughts, but exclude economic disasters. All variables are normalized to z-scores.

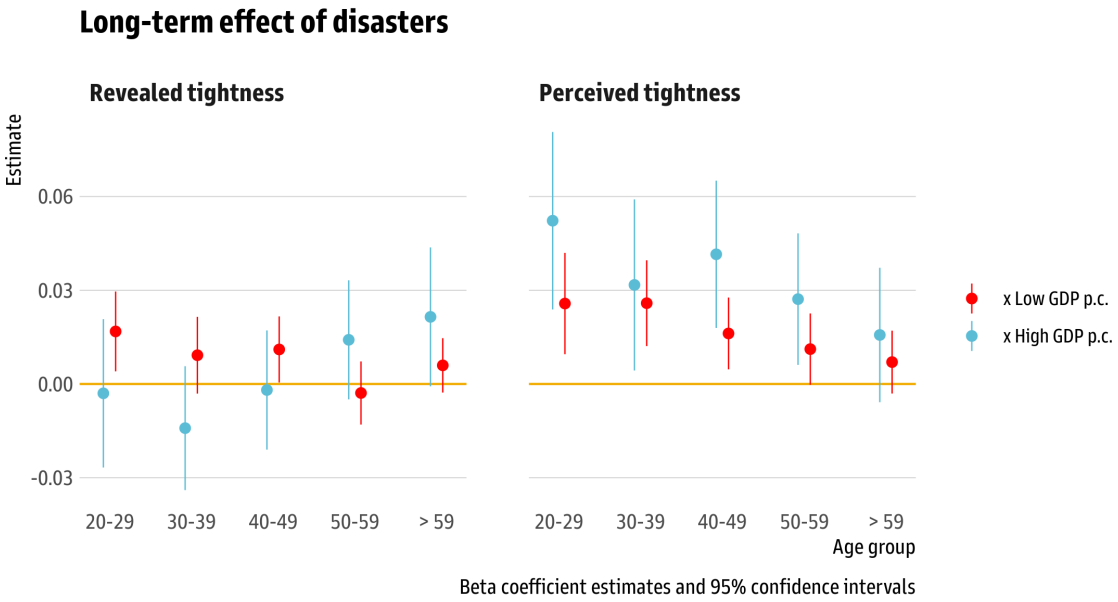

Figure A20: Heterogeneity in the long-run effects of exposure to disasters on tightness of social norms

Notes: The figure plots the coefficients and the 95% confidence intervals for five dummies indicating individuals' age groups from 20 to 70 interacted with exposure to disasters between ages 1 to 19. Confidence intervals are based on robust standard errors, clustered at the country-cohort level. The coefficients are estimated from four regressions which include fixed effects for country-of-residence $\times$ survey-year, birth cohort, gender, and age. The outcomes are revealed tightness (left) and perceived tightness (right). Coefficients colored in red (blue) are from regressions using the subsample of countries whose average GDP per capita 2000-2015 is below (above) the sample median. Disasters include epidemics, wars, economic disasters, earthquakes, and droughts. All variables are normalized to z-scores.

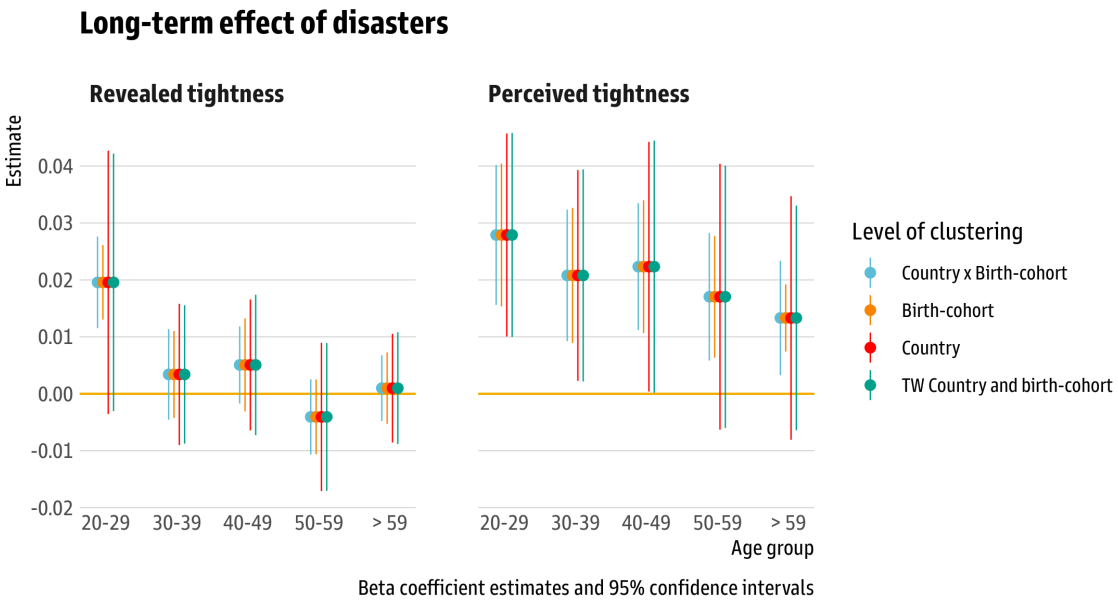

Figure A21: Robustness: clustering at alternative levels (baseline specification, exposure to disasters, including economic disasters)

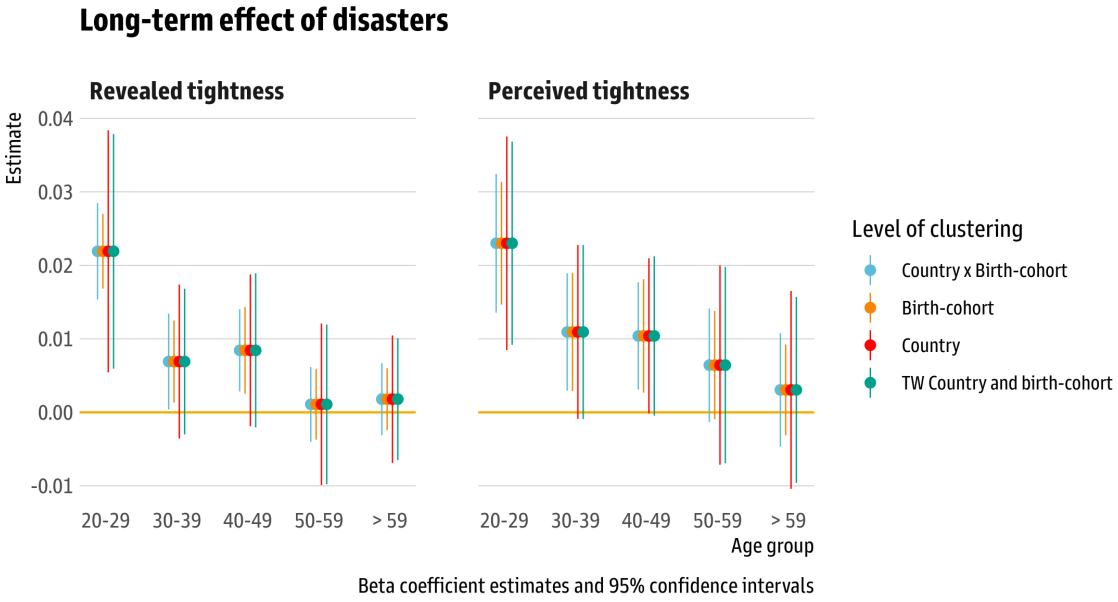

Figure A22: Robustness: clustering at alternative levels (baseline specification, exposure to disasters, excluding economic disasters)

Table A8: Robustness: including additional individual-level controls

	Tightness of social norms			
	Revealed		Perceived	
	(1)	(2)	(3)	(4)
Disasters (incl. economic disasters) × Age 20 - 29	0.018***		0.025***	
	(0.005)		(0.007)	
Disasters (incl. economic disasters) × Age 30 - 39	0.003		0.017**	
	(0.005)		(0.006)	
Disasters (incl. economic disasters) × Age 40 - 49	0.003		0.019***	
	(0.005)		(0.006)	
Disasters (incl. economic disasters) × Age 50 - 59	−0.003		0.012**	
	(0.004)		(0.006)	
Disasters (incl. economic disasters) × Age > 59	0.004		0.010*	
	(0.004)		(0.006)	
Disasters (excl. economic disasters) × Age 20 - 29		0.022***		0.022***
		(0.004)		(0.005)
Disasters (excl. economic disasters) × Age 30 - 39		0.003		0.009**
		(0.004)		(0.004)
Disasters (excl. economic disasters) × Age 40 - 49		0.005		0.009**
		(0.003)		(0.004)
Disasters (excl. economic disasters) × Age 50 - 59		−0.0002		0.001
		(0.003)		(0.004)
Disasters (excl. economic disasters) × Age > 59		0.006*		0.001
		(0.003)		(0.004)
No. of countries	31	74	30	73
Country × survey-year FE	Yes	Yes	Yes	Yes
Birth cohort FE	Yes	Yes	Yes	Yes
Baseline individual-level controls	Yes	Yes	Yes	Yes
Additional individual-level controls	Yes	Yes	Yes	Yes
N	106,176	183,756	57,266	109,155
R^2	0.155	0.166	0.064	0.067

Notes: OLS estimates, robust standard errors are clustered at the country-of-residence × birth-cohort level and reported in parentheses. An observation is an individual. All variables and data sources are described in the text. All regressions include fixed effects for country-of-residence × survey-year and birth-cohort; baseline individual-level controls contain a dummy equal to one if the respondent is male and controls for age and age squared. Additional individual-level controls contain dummies equal to one if the respondent is married and unemployed and fixed effects for educational attainment and income scale. All variables are normalized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A9: Robustness: restrict sample to native born individuals

	Tightness of social norms			
	Revealed		Perceived	
	(1)	(2)	(3)	(4)
Disasters (incl. economic disasters) × Age 20 - 29	0.017***		0.007	
	(0.006)		(0.010)	
Disasters (incl. economic disasters) × Age 30 - 39	0.002		−0.001	
	(0.006)		(0.010)	
Disasters (incl. economic disasters) × Age 40 - 49	−0.003		0.005	
	(0.005)		(0.008)	
Disasters (incl. economic disasters) × Age 50 - 59	−0.015***		0.006	
	(0.005)		(0.008)	
Disasters (incl. economic disasters) × Age > 59	−0.002		−0.008	
	(0.004)		(0.010)	
Disasters (excl. economic disasters) × Age 20 - 29		0.018***		0.015**
		(0.005)		(0.007)
Disasters (excl. economic disasters) × Age 30 - 39		0.004		0.008
		(0.005)		(0.006)
Disasters (excl. economic disasters) × Age 40 - 49		0.005		0.005
		(0.004)		(0.005)
Disasters (excl. economic disasters) × Age 50 - 59		−0.004		0.001
		(0.004)		(0.005)
Disasters (excl. economic disasters) × Age > 59		0.002		−0.007
		(0.003)		(0.006)
No. of countries	28	66	21	51
Country × survey-year FE	Yes	Yes	Yes	Yes
Birth cohort FE	Yes	Yes	Yes	Yes
Baseline individual-level controls	Yes	Yes	Yes	Yes
N	82,505	149,110	28,568	61,786
R^2	0.148	0.150	0.043	0.055

Notes: OLS estimates, robust standard errors are clustered at the country-of-residence × birth-cohort level and reported in parentheses. An observation is an individual. The sample is restricted to native born individuals. All variables and data sources are described in the text. All regressions include fixed effects for country-of-residence × survey-year and birth-cohort; baseline individual-level controls contain a dummy equal to one if the respondent is male and controls for age and age squared. All variables are normalized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table A10: Long-run effect of disasters on adherence to norms of prosocial behavior

	Adherence to prosocial norms					
	(1)	(2)	(3)	(4)	(5)	(6)
Disasters (incl. economic disasters) × Age 20 - 29	0.018***	0.019***				
	(0.004)	(0.004)				
Disasters (incl. economic disasters) × Age 30 - 39	0.011***	0.012***				
	(0.003)	(0.003)				
Disasters (incl. economic disasters) × Age 40 - 49	0.009***	0.009***				
	(0.003)	(0.003)				
Disasters (incl. economic disasters) × Age 50 - 59	0.022***	0.022***				
	(0.004)	(0.004)				
Disasters (incl. economic disasters) × Age > 59	0.025***	0.026***				
	(0.003)	(0.003)				
Disasters (excl. economic disasters) × Age 20 - 29			0.011***	0.011***	0.005**	0.005**
			(0.004)	(0.004)	(0.002)	(0.002)
Disasters (excl. economic disasters) × Age 30 - 39			0.006	0.006	0.001	0.002
			(0.004)	(0.004)	(0.002)	(0.002)
Disasters (excl. economic disasters) × Age 40 - 49			0.006	0.006	0.003	0.004*
			(0.004)	(0.004)	(0.002)	(0.002)
Disasters (excl. economic disasters) × Age 50 - 59			0.012***	0.012***	0.008***	0.008***
			(0.004)	(0.004)	(0.002)	(0.002)
Disasters (excl. economic disasters) × Age > 59			0.020***	0.020***	0.011***	0.012***
			(0.004)	(0.004)	(0.003)	(0.003)
No. of countries	31	31	31	31	72	72
Country × survey-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline individual-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Norm content FE	No	Yes	No	Yes	No	Yes
N	285,897	270,168	285,897	270,168	580,267	535,915
R^2	0.059	0.061	0.058	0.060	0.049	0.053

Notes: OLS estimates, robust standard errors are clustered at the country-of-residence × birth-cohort level and reported in parentheses. An observation is an individual. All variables and data sources are described in the text. All regressions include fixed effects for country-of-residence × survey-year and birth-cohort; baseline individual-level controls contain a dummy equal to one if the respondent is male and dummies for age groups (20-29, 30-39, 40-49, 50-59, above 59); norm content fixed effects contain dummies for the modal opinions used to compute the adherence to norms of prosocial behavior measure. All variables are normalized to z-scores. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

B List of Social Norms

B.1 European Social Survey

Variable	Label
ppltrst	Most people can be trusted or you can't be too careful
pplhlp	Most of the time people helpful or mostly looking out for themselves
trstprl	Trust in country's parliament
trstlgl	Trust in the legal system
trstplc	Trust in the police
trstplt	Trust in politicians
trstprt	Trust in political parties
trstep	Trust in the European Parliament
trstun	Trust in the United Nations
gincdif	Government should reduce differences in income levels
freehms	Gays and lesbians free to live life as they wish
prtyban	Ban political parties that wish overthrow democracy
scnsenv	Modern science can be relied on to solve environmental problems
imsmetn	Allow many/few immigrants of same race/ethnic group as majority
imdfetn	Allow many/few immigrants of different race/ethnic group from majority
impcntr	Allow many/few immigrants from poorer countries outside Europe
imbgeco	Immigration bad or good for country's economy
imueclt	Country's cultural life undermined or enriched by immigrants
imwbcnt	Immigrants make country worse or better place to live
trrprsn	Terrorist suspect in prison until police satisfied
trrtort	Torture in country never justified even to prevent terrorist attack
rlgdgr	How religious are you
rlgatnd	How often attend religious services apart from special occasions
pray	How often pray apart from at religious services
eimpcnt	Allow many/few immigrants from poorer countries in Europe
gvrfgap	Government should be generous judging applications for refugee status
imbleco	Taxes and services: immigrants take out more than they put in or less
imdetbs	Immigrant different race/ethnic group majority: your boss

Variable	Label
imdetmr	Immigrant different race/ethnic group majority: married close relative
imtcjob	Immigrants take jobs away in country or create new jobs
imwbcrm	Immigrants make country's crime problems worse or better
lwdscwp	Law against ethnic discrimination in workplace good/bad for a country
pplstrd	Better for a country if almost everyone shares customs and traditions
qfimchr	Qualification for immigration: Christian background
qfimcmt	Qualification for immigration: committed to way of life in country
qfimedu	Qualification for immigration: good educational qualifications
qfimlng	Qualification for immigration: speak country's official language
qfimwht	Qualification for immigration: be white
qfimwsk	Qualification for immigration: work skills needed in country
rfgbfml	Granted refugees should be entitled to bring close family members
rfgfrpc	Most refugee applicants not in real fear of persecution own countries
acldnmr	Approve if person have child with partner not married to. SPLIT BALLOT
advcyc	Approve if person gets divorced while children aged under 12. SPLIT BALLOT
aftjbyc	Approve if person has full-time job while children aged under 3. SPLIT BALLOT
alvgptn	Approve if person lives with partner not married to. SPLIT BALLOT
anvcld	Approve if person chooses never to have children. SPLIT BALLOT
dfincac	Large differences in income acceptable to reward talents and efforts
sbstrec	Social benefits/services place too great strain on economy
sbprvpv	Social benefits/services prevent widespread poverty
sbeqsoc	Social benefits/services lead to a more equal society
sbbsntx	Social benefits/services cost businesses too much in taxes/charges
sblazy	Social benefits/services make people lazy
sblwcoa	Social benefits/services make people less willing care for one another
imsclbn	When should immigrants obtain rights to social benefits/services
mnrgtjb	Men should have more right to job than women when jobs are scarce
hrshsnt	People who break the law much harsher sentences
hrshsnta	People who break the law much harsher sentences

B.2 World Values Surveys

Variable	Label
A001	Important in life: Family
A002	Important in life: Friends
A003	Important in life: Leisure time
A004	Important in life: Politics
A005	Important in life: Work
A006	Important in life: Religion
A007	Important in life: Service to others
A025	Respect and love for parents
A026	Parents responsibilities to their children
A027	Important child qualities: Good manners
A029	Important child qualities: independence
A030	Important child qualities: Hard work
A032	Important child qualities: feeling of responsibility
A034	Important child qualities: imagination
A035	Important child qualities: tolerance and respect for other people
A038	Important child qualities: thrift saving money and things
A039	Important child qualities: determination perseverance
A040	Important child qualities: religious faith
A041	Important child qualities: unselfishness
A042	Important child qualities: obedience
A124_01	Neighbours: People with a criminal record
A124_02	Neighbours: People of a different race
A124_03	Neighbours: Heavy drinkers
A124_04	Neighbours: Emotionally unstable people
A124_06	Neighbours: Immigrants/foreign workers
A124_07	Neighbours: People who have AIDS
A124_08	Neighbours: Drug addicts
A124_09	Neighbours: Homosexuals
A124_12	Neighbours: People of a different religion
A124_42	Neighbours: Unmarried couples living together

Variable	Label
A124_43	Neighbours: People who speak a different language
A165	Most people can be trusted
A168A	Do you think most people try to take advantage of you (10 point scale)
B001	Would give part of my income for the environment
B002	Increase in taxes if used to prevent environmental pollution
B003	Government should reduce environmental pollution
B008	Protecting environment vs. Economic growth
B016	Tradition vs. high economic growth
C001	Jobs scarce: Men should have more right to a job than women
C002	Jobs scarce: Employers should give priority to (nation) people than immigrants
C008	Work compared with Leisure
C010	Second choice if looking for a job
C011	Important in a job: good pay
C012	Important in a job: not too much pressure
C013	Important in a job: good job security
C014	Important in a job: a respected job
C015	Important in a job: good hours
C016	Important in a job: an opportunity to use initiative
C017	Important in a job: generous holidays
C018	Important in a job: that you can achieve something
C019	Important in a job: a responsible job
C020	Important in a job: a job that is interesting
C021	Important in a job: a job that meets one't abilities
C036	To develop talents you need to have a job
C037	Humiliating to receive money without having to work for it
C038	People who don't work turn lazy
C039	Work is a duty towards society
C041	Work should come first even if it means less spare time
C059	Fairness: One secretary is paid more
C060	How business and industry should be managed
C061	Following instructions at work

Variable	Label
D001_B	How much you trust: Your family (B)
D017	Ideal number of children
D018	Child needs a home with father and mother
D019	A woman has to have children to be fulfilled
D022	Marriage is an out-dated institution
D023	Woman as a single parent
D054	One of main goals in life has been to make my parents proud
D055	Make effort to live up to what my friends expect
D056	Relationship working mother
D057	Being a housewife just as fulfilling
D058	Husband and wife should both contribute to income
D059	Men make better political leaders than women do
D060	University is more important for a boy than for a girl
D061	Pre-school child suffers with working mother
D066_B	Problem if women have more income than husband (B)
D078	Men make better business executives than women do
E001	Aims of country: first choice
E002	Aims of country: second choice
E003	Aims of respondent: first choice
E004	Aims of respondent: second choice
E005	Most important: first choice
E006	Most important: second choice
E012	Willingness to fight for country
E014	Future changes: Less emphasis on money and material possessions
E015	Future changes: Less importance placed on work
E016	Future changes: More emphasis on technology
E018	Future changes: Greater respect for authority
E022	Opinion about scientific advances
E034	Basic kinds of attitudes concerning society
E035	Income equality
E036	Private vs state ownership of business

Variable	Label
E037	Government responsibility
E039	Competition good or harmful
E040	Hard work brings success
E041	Wealth accumulation
E069_01	Confidence: Churches
E069_02	Confidence: Armed Forces
E069_04	Confidence: The Press
E069_05	Confidence: Labour Unions
E069_06	Confidence: The Police
E069_07	Confidence: Parliament
E069_08	Confidence: The Civil Services
E069_10	Confidence: Television
E069_11	Confidence: The Government
E069_12	Confidence: The Political Parties
E069_13	Confidence: Major Companies
E069_14	Confidence: The Environmental Protection Movement
E069_15	Confidence: The Women's Movement
E069_17	Confidence: Justice System/Courts
E069_20	Confidence: The United Nations
E069_40	Confidence: Charitable or humanitarian organizations
E069_41	Confidence: Banks
E069_54	Confidence: Universities
E114	Political system: Having a strong leader
E115	Political system: Having experts make decisions
E116	Political system: Having the army rule
E117	Political system: Having a democratic political system
E121	Democracies are indecisive and have too much squabbling
E122	Democracies aren't good at maintaining order
E123	Democracy may have problems but is better
E135	Who should decide: international peacekeeping
E136	Who should decide: protection of the environment

Variable	Label
E137	Who should decide: aid to developing countries
E138	Who should decide: refugees
E139	Who should decide: human rights
E143	Immigrant policy
E220	We depend too much on science and not enough on faith
E224	Democracy: Governments tax the rich and subsidize the poor.
E225	Democracy: Religious authorities interpret the laws.
E226	Democracy: People choose their leaders in free elections.
E227	Democracy: People receive state aid for unemployment.
E228	Democracy: The army takes over when government is incompetent.
E229	Democracy: Civil rights protect peoples liberty against oppression.
E233	Democracy: Women have the same rights as men.
E233A	Democracy: The state makes people's incomes equal
E233B	Democracy: People obey their rulers
E235	Importance of democracy
E266	Some people think that having honest elections makes a lot of difference in thei
F028	How often do you attend religious services
F034	Religious person
F102	Politicians who dont̂t believe in God are unfit for public office
F103	Religious leaders should not influence how people vote
F104	Better if more people with strong religious beliefs in public office
F105	Religious leaders should not influence government
F114A	Justifiable: Claiming government benefits to which you are not entitled
F114B	Justifiable: Stealing property
F114C	Justifiable: Parents beating children
F114D	Justifiable: Violence against other people
F115	Justifiable: Avoiding a fare on public transport
F116	Justifiable: Cheating on taxes
F117	Justifiable: Someone accepting a bribe
F118	Justifiable: Homosexuality
F119	Justifiable: Prostitution

Variable	Label
F120	Justifiable: Abortion
F121	Justifiable: Divorce
F122	Justifiable: Euthanasia
F123	Justifiable: Suicide
F135A	Justifiable: Sex before marriage
F199	Justifiable: For a man to beat his wife
F203	The only acceptable religion is my religion
G006	How proud of nationality
G007_18_B	Trust: Your neighborhood (B)
G007_33_B	Trust: People you know personally (B)
G007_34_B	Trust: People you meet for the first time (B)
G007_35_B	Trust: People of another religion (B)
G007_36_B	Trust: People of another nationality (B)
I001	One of the bad effects of science is that it breaks down peoples ideas of rig
I002	It is not important for me to know about science in my daily life

C List of Disasters in Analysis of Short-run Effects

Type	Date	Country	Region (ESS)	Deaths	Details
Conflict	2002-03-27	ISR	Central	29	Fifteen Dead, 126 wounded in Netanya bombing.; Hamas claims Israel suicide bombing.; Death toll from Israeli hotel suicide bombing rises to 29
Conflict	2002-09-19	ISR	Tel Aviv	5	At least five dead in bombing on Israeli bus.; Hamas claims responsibility for Tel Aviv bus bomb.
Conflict	2002-11-21	ISR	Jerusalem	11	Suicide bomber kills 11 commuters, schoolkids in Jerusalem bus blast; Hamas claims lethal Jerusalem bus bombing;
Conflict	2002-11-28	ISR	Northern	6	Gunmen fire open fire in north Israel, two dead.; UPDATE 2-Gunmen kill four, wound 36 in northern Israel.
Conflict	2003-01-05	ISR	Tel Aviv	22	NA
Conflict	2003-03-05	ISR	Haifa	16	NA
Conflict	2016-01-01	ISR	IL	2	Two people were killed and at least seven wounded Friday when an Israeli Arab gunman opened fire on a pub and nearby cafe in central Tel Aviv.
Conflict	2016-12-19	DEU	DE3	12	Police: Truck attack that killed 12 in Berlin 'intentional'
Conflict	2017-01-03	ISR	IL	1	Two separate shootings occurred in Haifa on Tuesday, in which one person was killed and another injured.
Conflict	2018-12-11	FRA	FRF1	5	French police official says 4 killed in Strasbourg shooting; Islamic State says Strasbourg shooter was one of its soldiers, gives no evidence
Earthquake	2008-10-11	RUS	North Caucasus	13	NA
Earthquake	2011-05-11	ESP	ES61 ES62	10	NA
Earthquake	2017-08-23	ITA	ITF3	2	NA
Epidemic	2003-02-27	IRL	Border West Midland Mid-East Mid-West Dublin South-East South-West	NA	NA
Epidemic	2003-03-01	GBR	Scotland North West Yorkshire and The Humber North East West Midlands East Midlands London South East South West Wales East of England	NA	NA

Epidemic	2003-03-09	CHE	Genferseeregion Zentrales Mittelland Nordschweiz Zentralschweiz Tessin Ostschweiz	NA	NA
Epidemic	2003-03-09	DEU	Schleswig-Holstein Hamburg Niedersachsen Bremen Nordrhein-Westfalen Hessen Rheinland-Pfalz Baden-Württemberg Bayern Saarland Berlin Brandenburg Mecklenburg-Vorpommern Sachsen Sachsen-Anhalt Thüringen	NA	NA
Epidemic	2003-03-26	ESP	Castilla-la Mancha Comunidad Valenciana Illes Balears Cataluña Galicia Andalucía País Vasco Castilla y León Comunidad de Madrid Comunidad Foral de Navarra Principado de Asturias Canarias Cantabria Aragón	NA	NA
Epidemic	2006-01-01	TUR	Istanbul Western Marmara Aegean Eastern Marmara Western Anatolia Mediterranean Central Anatolia Western Black Sea Eastern Black Sea North Eastern Anatolia East South east	NA	NA
Flood	2006-07-01	TUR	Western Marmara	12	NA
Flood	2009-06-22	CZE	Moravskoslezsko Jihozapad	13	NA
Flood	2013-11-18	ITA	ITG2	18	NA
Flood	2018-10-14	FRA	FRJ1	14	NA
Storm	2002-10-26	DEU	Schleswig-Holstein Niedersachsen Nordrhein-Westfalen Hessen	11	Jeannet
Storm	2003-06-07	DEU	Schleswig-Holstein Hamburg Niedersachsen Bremen Nordrhein-Westfalen Hessen Rheinland-Pfalz Baden-Württemberg Bayern Berlin Brandenburg Mecklenburg-Vorpommern Sachsen Sachsen-Anhalt Thüringen	10	NA
Storm	2007-01-18	DEU	Schleswig-Holstein Hamburg Niedersachsen Bremen Nordrhein-Westfalen Hessen Rheinland-Pfalz Baden-Württemberg Bayern Berlin Brandenburg Mecklenburg-Vorpommern Sachsen Sachsen-Anhalt Thüringen	11	Kyrill
Storm	2007-01-18	GBR	Scotland North West Yorkshire and The Humber North East West Midlands East Midlands London South East South West Wales East of England	13	Kyrill
Storm	2009-01-23	ESP	Cataluña Galicia	14	Klaus

Storm	2009-01-23	FRA	Sud Ouest Méditerranée	11	Klaus
Storm	2018-10-29	ITA	ITC ITF ITG 12	Storm	Adrian
Storm	2020-01-19	ESP	ES61 ES62 ES52 ES41 ES12 ES53 ES51 ES24	17	Gloria