Natural Catastrophes and Financial Development: An Empirical Analysis*

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Abstract

We estimate the causal effect of natural catastrophes on financial development. We focus on largest catastrophes in developing economies in 1960-2016, employ synthetic control method to compute the counterfactual and use the credit to GDP ratio as the measure of financial development. Our estimates show that the effects of natural catastrophes are sizable, statistically significant and long-lasting. We find that a decade after the catastrophe, credit/GDP ratio remains approximately 30% below its counterfactual. This result suggests that large-scale natural catastrophes severely undermine financial intermediation in developing economies.

Keywords: Natural catastrophes, financial development, synthetic control method

JEL Codes: G00, O11, Q54, Q56

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1 Introduction

Natural catastrophes cause tremendous damage to environment as well as to economy. The destructiveness and frequency of natural catastrophes has increased in last decades with rising and more volatile global temperatures (Emanuel, 2005; Webster et al., 2005). Melecky and Raddatz (2015) report that nearly 100 000 people die yearly because of natural catastrophes and the property damages constitute 0.23% of cumulative world output. Kahn (2005) and Loayza et al. (2012) show that the negative consequences of natural catastrophes are concentrated in developing countries with low quality of institutions. Klomp and Valckx (2014) find that the negative effects of catastrophes on economic growth increase over time. Although the research on the economic growth consequences of natural catastrophes is burgeoning (see surveys by Noy and Cavallo (2011) and Noy and duPont IV (2018) and meta-analysis by Klomp and Valckx (2014)), we know much less about the effects of these catastrophes on financial sector and especially financial development. The issues regarding the consequences of natural catastrophes become even more pertinent during the current COVID-19 global pandemic, which represents a global catastrophe of uncertain duration.

Theoretically, the effect of natural catastrophes on financial development is not clear-cut. Developed financial markets may serve as insurance mechanism enabling government and private sector to mitigate the consequences of catastrophe (Melecky and Raddatz, 2015). Therefore, credit to private and public sector may increase following the catastrophe. This effect can also be rationalized within Schumpeter creative destruction theory, as catastrophes may provide impetus for re-investment and resurgence of capital stock (Cavallo et al., 2013; Klomp, 2017).

On the other hand, natural catastrophes can undermine the functioning of financial sector especially in the environment of low initial financial development (with limited risk diversification possibilities), weak institutions and subdued economic activity. Financial intermediaries may reduce the availability of post-catastrophe financial resources due to heightened uncertainty regarding economic outlook and the negative growth consequences of natural catastrophes in countries with underdeveloped financial markets can be large and long-lasting (McDermott et al., 2014).[1] Governments may increase expenditures following the catastrophe or design pub-

[1]There are several related theories on the effect of natural resource windfalls. The windfalls increase the volume of deposits in financial institutions, if saved domestically. In countries with low institutional quality or macroeconomic imbalances, the increased volume of deposits can also be saved abroad contributing to greater foreign – but not domestic – financial development (Bhattacharyya and Hodler, 2014). If windfalls cause Dutch

lic loan scheme, foreign aid may be provided to relieve the consequences of catastrophes; these measures may crowd out the private credit (Melecky and Raddatz, 2015). Financial intermediaries may become more prudent in their lending behavior because catastrophes undermine their financial stability (Klomp, 2014). Underdeveloped financial systems characterized by credit constraints are less likely to provide finance to invest into new capital stock (McDermott et al., 2014). Overall, given the ambiguity of theoretical predictions, it is important to examine the effect of catastrophes on financial development empirically.

This is especially so because the evidence is scarce. The evidence on the effect of natural catastrophes on financial sector focuses on examining the consequences of catastrophes on bank behavior (Berg and Schrader, 2012; Hosono et al., 2016; Schuwer et al., 2019) or analyzing the effects of disasters on sovereign default (Klomp, 2017; Marto et al., 2018). Some related literature has focused on the effect of natural resource discoveries on financial development (Poelhekke and Beck, 2017).

Given that the specific timing of natural catastrophes is difficult to predict, one would be inclined to assume that the catastrophes represent an exogenous shock and that traditional regression analysis, such as ordinary least squares, provides the causal estimates of the effect of natural catastrophes on financial development. To the contrary, Kahn (2005) and Kellenberg and Mobarak (2008) show that even though the timing of catastrophes is largely unpredictable, the negative consequences of these catastrophes – especially if they are not severe – can be mitigated. The consequences can be mitigated especially in developed countries with a high quality of institutions (Kahn, 2005; Toya and Skidmore, 2007). Therefore, if country can undertake some measures in time $t-1$ that would influence the effect of catastrophes in t on financial development in $t+1$, then the estimated effect of catastrophes on financial development would be underestimated. Similarly, Nakamura and Steinsson (2018) advocate the focus on large shocks to facilitate identification in macroeconomic research.

For this reason, we focus on extreme large-scale natural catastrophes in developing countries and treat the resulting largest natural catastrophes during last several decades as exogenous shock to financial development. We identify these extreme large-scale natural catastrophes by taking the top 1% percentile of catastrophes ranked according to their severity. We use EM-DAT

disease and crowd out trade sector, the corporate demand for external finance may eventually fall and be replaced by greater demand by public sector (Poelhekke and Beck, 2017).

International Disaster Database and define severity using the ratio of death toll to country's total population. These are the events such as 2004 tsunami in Sri Lanka or 1970 earthquake in Peru (approximately 0.2%, resp. 0.5% of total population died in Sri Lanka and Peru during these catastrophes), i.e. devastating large-scale catastrophes with thousands of people killed, which effects are nearly impossible to be mitigated *ex ante*.

We employ synthetic control method to assess the effect of natural catastrophes on financial development.[2] This method represents a comparative event study approach and compares an actual post-catastrophe path of financial development with appropriate counterfactual of what the financial development would have been absent the catastrophe (Abadie et al., 2010). The counterfactual is built as the optimally weighted average of countries not exposed to catastrophe. These control countries are supposed to match as closely as possible the pre-catastrophe characteristics of the country experiencing the natural catastrophe.[3]

Synthetic control method generalizes difference-in-differences estimator and allows for time-varying individual-specific unobserved heterogeneity. The method has gained popularity recently and have been applied to study the economic growth consequences of trade liberalization (Billmeier and Nannicini, 2013), natural resource discoveries (Smith, 2015) or refugee waves (Peri and Yasenov, 2020). Using the synthetic control method, we can also analyze whether the consequences of natural catastrophes are long-lasting. The issue of long-term consequences of catastrophes deserves more attention according to the survey by Noy and Cavallo (2011). Importantly, using the synthetic control method, we also quantify the size of the effect of natural catastrophes on financial development to understand whether these effects are economically important.

We find that the effect of severe large-scale natural catastrophes on financial development is sizable, statistically significant and long-lasting. Even ten years after the catastrophe, financial development, as measured by the credit to GDP ratio, remains approximately 30% below its counterfactual. This result suggests that large-scale natural catastrophes severely undermine financial intermediation in developing economies.

[2] Athey and Imbens (2017) consider the synthetic control method: *"arguably the most important innovation in the policy evaluation literature in the last 15 years"*.

[3] We rely on cross-country dataset to provide an international perspective (similarly to Smith (2015), who examines the consequences of natural resource discoveries on economic growth using the synthetic control method). Employing regional data from single country could in principle improve the matching of pre-catastrophe trends between the treated region and control regions but regional data on financial development in developing countries are not available.

In general, our research contributes to two streams of literature. First, we contribute to the literature of the determinants of financial development by showing how nature-related shocks may have long-lasting effects and cause financial underdevelopment. Second, more broadly, our results contribute to emerging literature on the increased importance of natural-related shocks (such as climate-related shocks) for financial sector (especially for insurance sector but also for banking sector because insurance is more frequently offered by banks, too). This is so because we provide an empirical estimate of how strongly the financial development is affected by the natural-related shocks.

The rest of the paper is organized as follows: Section 2 discusses the related literature. Section 3 presents the synthetic control model, while Section 4 outlines our dataset. Section 5 contains our results. Section 6 concludes. Additional results are available in the Appendix.

2 Related Literature

While there is an extensive literature examining the determinants of financial development, the evidence on the effect of natural-related shocks on financial development is scarce. First, we provide a summary of the studies on the determinants of financial development. Second, we briefly discuss the literature examining the natural-related shocks – such as catastrophes or discoveries – on financial sector. Third, we briefly summarize the literature investigating the effect of natural catastrophes on economic growth.

The literature on the determinants of financial development typically emphasizes that the financial development is driven by institutional, economic and financial characteristics. More specifically, Rajan and Zingales (2003) show how the rule of law is conducive for financial development. Guiso et al. (2004) emphasize the role of social capital – generalized trust – as the determinant of financial development, while La Porta et al. (1998) find that the legal origin is the driving force of financial development. Other studies such as Baltagi et al. (2009) find that the trade and financial openness influence the level of financial development, while the results of Chinn and Ito (2006) suggest that financial regulations matter, too.

From our perspective, it is important to emphasize that the aforementioned studies do not specifically analyze the role of natural-related shocks for financial development. However, this lack of analysis is not surprising given the global focus of this studies (or the focus on the developed countries). Beck et al. (2003) represent an exception to these studies because they show how the existence of inhospitable environment shapes the (lack of) quality of institutions and financial underdevelopment. Therefore, Beck et al. (2003) consider the role of 'nature' for financial development at least indirectly.

Several articles analyze how households and firms react to natural catastrophes. Sawada and Shimizutani (2008) examine the effect of the Kobe Earthquake on the consumption. Using household survey data, Sawada and Shimizutani (2008) find that the consumption descreases only for credit-constrained households, while unconstrained households were able to smooth their consumption. McDermott et al. (2014) analyze the role of credit constraints for the effect of catastrophes on growth theoretically and then empirically using a panel of countries. McDermott et al. (2014) show how credit-constrained countries suffer greater economic losses following a catastrophe. Similarly, Noy (2009) finds that countries with higher levels of domestic credit adjust to a catastrophe with a smaller loss. Poontirakul et al. (2017) analyze the role of insurance for firm survival following a 2011 Christchurch Earthquake in the New Zealand but fail to find significant effect. De Mel et al. (2012) employ micro-level data from Sri Lanka and analyze the enterprise recovery following a December 2004 tsunami. De Mel et al. (2012) find that the recovery is rather slow even though enterprises with access to capital are able to recover faster.

There is also a small literature examining the consequences of natural catastrophes on bank behavior. Klomp (2014) examines the effect of natural catastrophes on bank stability and finds that the probability of a banks' default increases following a catastrophe. The results by Klomp (2014) also suggest that financial development reduces the negative effects of catastrophe. Utilizing the 1995 Kobe Earthquake as natural experiment, Hosono et al. (2016) identify the bank loan supply and show how banks reduce lending to firms following a shock. Berg and Schrader (2012) analyze the credit demand and supply in response to volcanic eruptions in Ecuador. They show that while the firms' credit demand increases, bank restricts the access to credit. This imbalance is partially mitigated by the long-term firm-bank relationships. Similarly, Cortes and Strahan (2017) investigate capital relocation in response to increased bank lending to disasters using the county-level US data. Schuwer et al. (2019) investigate the consequences of Hurricane Katrina in 2005 for the US banks. They find a complex reaction of banks to this shock observing that better capitalized banks are better able to withstand shock and also help local economic development.

In addition, a related literature examines the effect of natural resource windfalls on financial development (as measured by the total lending and deposit taking), see Poelhekke and Beck (2017). Using structural vector autoregression framework, they find that natural resources abundance reduces financial development.

Keerthiratne and Tol (2017) analyze the effect of natural disasters on financial development. They employ the panel data for a global sample of countries over more than three decades. Using system generalized method of moments (GMM) estimator, their results suggest that financial development increases in response to disasters with the effect to be stronger in poorer countries and in countries with smaller agricultural sector. Our results contrasts with Keerthiratne and Tol (2017), as we find that catastrophes reduce financial development. However, there are a number of differences between Keerthiratne and Tol (2017) and our study, which can validate different findings. First, Keerthiratne and Tol (2017) examine an effect of wide arrays of disasters, both small and large, while we analyze only the largest catastrophes. Second, we use the econometric method from program evaluation literature and put a lot of emphasis on the identification strategy, while Keerthiratne and Tol (2017) use fixed effect estimator.[4] Fixed effects models feature time-invariant unobserved heterogeneity, which is unlikely to deliver identification when examining the consequences of natural catastrophes on economic (or financial) growth (Cavallo et al., 2013).[5] Third, we use a somewhat different measure of financial development (credit to GDP ratio), while Keerthiratne and Tol (2017) employ credit per capita (credit to population) as the measure of financial development. Credit to GDP is the most common measure of depth of financial institutions and is typically used as the basic measure of financial development (Cihak et al., 2013). Note that Cihak et al. (2013) develop a comprehensive cross-country database of financial development and do not mention credit per capita as the measure of financial development.

There is a growing literature examining the consequences of natural catastrophes on economic growth. Cavallo et al. (2013) investigate the effect of four large-scale natural catastrophes on economic growth. Using the synthetic control method, they obtain the causal estimates of catastrophes on growth and find that there is, on average, no effect of catastrophes on growth.

[4]The system GMM with lagged regressors as instruments is used in one of robustness checks.

[5]See the detailed explanation on page 1550 of Cavallo et al. (2013).

On the other hand, Felbermayr and Groschl (2014) and Loayza et al. (2012) find that the effect of natural catastrophes on economic growth depends on the severity of catastrophes. Small catastrophes have a weak or no effect on growth, while extreme catastrophes severely depress economic activity. However, Loayza et al. (2012) find some economic sectors, where growth consequences of catastrophes are positive. In addition, Melecky and Raddatz (2015) show that the insurance penetration and financial development cushion the negative effects of natural catastrophes on economic growth. Overall, the meta-analysis by Klomp and Valckx (2014) finds that the effect of catastrophes on economic growth is negative and that the effect becomes stronger over time.

Melecky and Raddatz (2015) investigate the government debt accumulation consequences of natural catastrophes, while Klomp and Valckx (2014) study the effect of natural catastrophes on the sovereign debt default. In both cases, the effects of catastrophes on public finances are negative. Klomp (2017) provides a detailed overview of studies examining the effects of natural catastrophes on other variables such as inflation, exchange rates or product availability.

3 Empirical Methodology

This section presents the synthetic control method, as developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). The method builds on difference in differences approach but allows for time-varying unobserved heterogeneity. It also shares elements from matching models. In general, the synthetic control method is intended for comparative case studies, i.e. to identify the effects of some intervention on the variable of interest.

Traditional approaches such as the cross-sectional regression or fixed effects model are likely to suffer from the lack of identification in our setting. For example, fixed effects model would need to rely on very strong assumptions such as that pre-catastrophe trends do not change after the catastrophe (Cavallo et al., 2013).

The synthetic control method considers two types of units (countries in our case), the first type being exposed to intervention (natural catastrophe in our case) and is labeled as treated unit. The second type not being exposed to intervention is labeled untreated units or control group. In addition, the method distinguishes between pre-intervention and post-intervention period (pre- or post-catastrophe period in our case).

The synthetic control method forms a weighted average of untreated units in the pre-catastrophe period in the way that it matches financial development of country exposed to catastrophe as closely as possible. The weighted average of untreated units is labeled as the synthetic control and serves for the computation of counterfactual in the post-catastrophe period in order to assess what would have financial development of treated country been if it did not experience a catastrophe. Therefore, the resulting synthetic control ('synthetic' financial development) is compared to actual financial development in the post-catastrophe period to evaluate the effect of natural catastrophe on financial development. The significance of the effect can be assessed using so-called placebo tests (Abadie et al., 2010) or computing the corresponding p-values (Cavallo et al., 2013).

The rest of this section introduces the synthetic control method formally. Suppose that we have data for $j = 1, \ldots, J+1$ units and $t = 1, \ldots, T$ time periods both for the variable of interest (outcome variable) and several other variables (predictors) which affect the outcome variable. Let us assume that the last unit $(J+1)$-th experiences the intervention in period $T_0 + 1$. As a result, there are T_0 pre-intervention periods, $1 \leq T_0 < T$. The remaining J units are not exposed to the intervention during the full sample period and represent a "donor pool", i.e. potential control units. The method assumes that predictors and outcome variables of remaining J units are not affected by the intervention for $t = 1, \ldots, T$. Next, the method also assumes that there are no anticipation effects, and predictors and outcome variable of the treated unit are not influenced by the intervention for $t = 1, \ldots, T_0$. If there are anticipation effects, T_0 should be shifted for earlier date.

We denote Y^I_{J+1t} as what would the value of the outcome variable in unit $J+1$ at time $t = 1, \ldots, T$, i.e. for the unit exposed to intervention. On the other hand, Y^N_{jt} denotes what would be the value of a unit $j = 1, \ldots, J+1$ at time $t = 1, \ldots, T$, i.e. the observed value for units not exposed intervention.

It follows that prior to intervention, i.e. for $t \leq T_0$, $Y^I_{jt} = Y^N_{jt}$ for the units $j = 1, \ldots, J+1$. The size of the intervention effect, α_{jt}, is defined as

$$Y^I_{jt} = Y^N_{jt} + \alpha_{jt} \tag{1}$$

To assess the intervention effect, which occurs for the unit $J+1$ in period T_0+1, we need to determine for $t > T_0 + 1$:

$$\alpha_{jt} = Y_{jt}^I - Y_{jt}^N = Y_{jt} - Y_{jt}^N \tag{2}$$

where Y_{jt}, $t = T_0+1, \ldots, T$, is the value of the outcome variable which is observable for the unit $J+1$ that experienced the intervention. Note that $(\alpha_{jT_0+1}, \ldots, \alpha_{jT})$ is the path of estimated effect after the intervention occurred. We need to estimate Y_{jt}^N, i.e. what would be the value of outcome variable in case the intervention did not happen. Suppose that Y_{jt}^N is determined by a factor model:

$$Y_{jt}^N = a_t + \mathbf{b_t Z_j} + \mathbf{c_t d_j} + e_{jt} \tag{3}$$

where a_t unknown common factor with constant factor loadings across units. Z_j denotes the $(m \times 1)$ vector of observed predictors with b_t being a $(1 \times m)$ vector of parameters to be estimated. c_t is a $(1 \times G)$ vector of unobserved common factors and d_j represents a $(G \times 1)$ vector of unknown factor loadings. e_{jt} denotes an error term.

In this respect, Abadie et al. (2010) propose:

$$\alpha_{J+1t} = Y_{J+1t} - \sum_{j=1}^{J} w_j^{\star} Y_{jt} \tag{4}$$

where $w_j^{\star}$, $j = 1, \ldots, J$, denotes a time-invariant weight for the unit j from the donor pool with $w_j^{\star} \geq 0$ for $j = 1, \ldots, J$ and $\sum_{j=1}^{J} w_j^{\star} = 1$, and Y_{jt}, $j = 1, \ldots, J$, is the observed outcome for unit j from the donor pool. The weights ,$w_j^{\star}$, should be set optimally so that the units from the donor pool match the pre-intervention behavior of treated unit, $J+1$, as closely as possible. The weights are estimated as follows.

Suppose that there are K predictors for the outcome variable. $\mathbf{X_1}$ $(K \times 1)$ represents a vector of their pre-intervention values for the treated unit, while $\mathbf{X_0}$ denotes $(K \times J)$ matrix of the values for the J potential control units. $\mathbf{V}$ is a $(K \times K)$ diagonal matrix with nonnegative components only. Each element of $\mathbf{V}$ on the main diagonal shows a relative importance for the particular predictor of outcome variable. Abadie and Gardeazabal (2003) show there are two steps how to compute the optimal weights $w_j^{\star}$, $j = 1, \ldots, J$. The first step requires $\mathbf{W} =$

$(w_1, \dots, w_J)'$ to sum up to one, to have non-negative elements and to minimize the function:

$$(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W}) \tag{5}$$

For the second step, it is important to realize that $\mathbf{W}^\star(\mathbf{V})$, i.e. $\mathbf{W}^\star$ is a function of $\mathbf{V}$, and depends on the relative importance of predictors.

Assume that $\mathbf{Y_1}$ is a $(T_0 \times 1)$ vector containing the values of the outcome variable in all T_0 pre-intervention periods for the treated unit. $\mathbf{Y_0}$ is a $(T_0 \times J)$ matrix of the corresponding values for the J potential 'donor pool' units. The algorithm searches all diagonal $(K \times K)$ matrices with nonnegative elements to set $\mathbf{V}$ such that it minimizes in the pre-intervention period the deviation of the outcome variable path of the synthetic control defined by $\mathbf{W}^\star(\mathbf{V})$ from the outcome variable path of the treated unit. As a result,

$$\mathbf{V}^\star = \underset{\mathbf{V} \in \mathcal{V}}{\arg\min}\, (\mathbf{Y_1} - \mathbf{Y_0}\mathbf{W}^\star(\mathbf{V}))'(\mathbf{Y_1} - \mathbf{Y_0}\mathbf{W}^\star(\mathbf{V})) \tag{6}$$

where $\mathcal{V}$ denotes the set of all diagonal $(K \times K)$ matrices with nonnegative elements. Once we obtain $\mathbf{V}^\star$, we use them to construct the optimal weights.[6] $\mathbf{W}^\star = \mathbf{W}^\star(\mathbf{V}^\star)$ The optimal weights, $\mathbf{W}^\star$, determine the optimal synthetic control, which is used to compute counterfactual of the outcome variable path after the intervention.

Similarly to Cavallo et al. (2013), we compute the average disaster effect across all the G largest disasters. This is beneficial because financial development at the yearly frequency can be affected by one-off shocks, which are largely eliminated by averaging across countries. Therefore, we compute the average of the lead-specific estimates of the disaster's impact, where $(\hat{\alpha}_{g,T_0+1}, \dots, \hat{\alpha}_{g,T})$ are estimates of the disaster's impact for treatment country g and leads $1, 2, \dots, T - T_0$ (see Eq. 4). Next, assume that for all the G disasters, we are able to compute lead-specific estimates of the disaster's impact for all post-disaster periods $T - T_0$. In practice, we take the minimum number of post-disaster periods across the G countries to make sure that all lead-specific estimates of individual disasters' impacts are available. Then, the average effect

[6]To assure the unique solution, note that the Eucledian norm of $\mathbf{V}^\star$ is normalized to one.

for the G largest disasters is obtained from individual effects as:

$$\overline{\alpha} = (\overline{\alpha}_{T_0+1}, \ldots, \overline{\alpha}_T) = \frac{1}{G}\sum_{g=1}^{G}(\hat{\alpha}_{g,T_0+1}, \ldots, \hat{\alpha}_{g,T}).$$

To assess whether the difference between the outcome variable path and counterfactual after the intervention is substantially different in a statistical sense, we employ two approaches. First, we compute the ratio of post- to pre-intervention mean squared prediction error (MSPE) for treatment country and all its available control countries (placebos) to assess the ability of a synthetic control i) to reproduce the pre-disaster evolution of a treatment's country credit to GDP ratio and ii) to gauge the effect of intervention on outcome variable. Second, we use the p-values developed by Cavallo et al. (2013) to assess whether intervention has had a statistically significant effect on the path of outcome variable. We describe these two procedures below.

Regarding the MSPE, we start by applying the synthetic control method to every potential control country for each treatment country despite that the control countries were not exposed to the intervention - this is often labeled as placebo effect.Therefore, for each potential control country, we obtain the actual path of credit to GDP and the counterfactual path determined by its synthetic control. The pool of available control countries includes all remaining controls and the actual treatment country. MSPE for post- and pre-intervention period is then computed as the average of squared differences between actual and counterfactual credit to GDP for that period, where intervention year is equal to the intervention year of the actual treatment country. Therefore, we proceed as if the natural catastrophe happened in the same year in one of the control countries.

Another inferential exercise is to compute the distribution of average catastrophe effects and determine the position of an actual effect among the placebo average effects as in Cavallo et al. (2013). We determine whether the effect estimated for a country in which the natural catastrophe occurred is large relative to the effects estimated for countries chosen at random (placebo countries that were not exposed to a catastrophe). As in determining the distribution of MSPE ratios above, we start by estimating all placebo effects and the effect of a catastrophe for an actual treatment country (i.e. country that was exposed to a natural catastrophe).

Given that we conduct inference about negative point estimates of the average effect[7] at

[7]We assume that on average, credit to GDP is negatively influenced by the catastrophe, and therefore the

every lead (in every year in the catastrophe's aftermath), we compute a lead-specific significance level (p-value) for the estimated catastrophe impact as:

$$p\text{-value}_l = \Pr(\hat{\alpha}^{PL}_{J+1,l} < \hat{\alpha}_{J+1,l}) = \frac{\sum_{j=1}^{J} I(\hat{\alpha}^{PL(j)}_{J+1,l} < \hat{\alpha}_{J+1,l})}{J}$$

where $\hat{\alpha}^{PL(j)}_{J+1,l}$ is the lead l-specific effect of a catastrophe when control country $j = 1, \ldots, J$ is assigned a placebo catastrophe in the same year as the original treatment country $J+1$. $\hat{\alpha}^{PL(j)}_{J+1,l}$ is computed in a similar way as $\hat{\alpha}_{J+1,l}$ following the procedure described in the beginning of this section (see the Eq. 4). Computing $\hat{\alpha}^{PL(j)}_{J+1,l}$ for every possible control $j = 1, \ldots, J$ of the treatment country $J+1$ enables us to characterize the distribution of placebo effects and assess the position of $\hat{\alpha}_{J+1,l}$ in that distribution.

Following Cavallo et al. (2013), we conduct inference about $\overline{\alpha}$, the average effect across G largest catastrophes, which is described in the following steps:

1. For each catastrophe of interest $g = 1, \ldots, G$, which is associated with one particular treatment country, we compute all placebo effects using the pool of available control countries. The number of available control countries for catastrophe g is $j_g = 1, \ldots, J_g$.

2. At each lead l, we compute every possible placebo average effect by taking one placebo estimate associated with each disaster g and then taking the average across G placebos. The number of possible placebo averages is:

$$N_{\overline{PL}} = \prod_{g=1}^{G} J_g$$

   Individual placebo averages are indexed by $np = 1, \ldots, N_{\overline{PL}}$.

3. We assess the position of the actual lead-specific average disaster effect $\overline{\alpha}_l$ in the distribution of $N_{\overline{PL}}$ placebo average effects. This involves $N_{\overline{PL}}$ comparisons.

4. We compute p-value at lead l for the average $\overline{\alpha}_l$ as:

$$p\text{-value}_l = \Pr(\overline{\alpha}^{PL}_l < \overline{\alpha}_l) = \Pr(\frac{1}{G}\sum_{g=1}^{G} \hat{\alpha}^{PL}_{g,l} < \overline{\alpha}_l) = \frac{\sum_{np=1}^{N_{\overline{PL}}} I(\overline{\alpha}^{PL(np)}_l < \overline{\alpha}_l)}{N_{\overline{PL}}}$$

lead-specific estimates of the average effect, which are defined as the average differences between the actual and contractual credit to GDP, are negative. This does not rule out the possibility that for some treatment countries, the estimated impact of a natural catastrophe can be positive.

4 Data

To select the natural catastrophes, we follow most of literature and use EM-DAT, the International Disaster Database maintained by the Centre for Research on the Epidemiology of Disasters (CRED) at the Université Catholique de Louvain in Brussels, Belgium.

The EM-DAT database contains information on the size and scope of natural (hydro-meteorological, geophysical and climatic) disasters since 1900 and includes data on monetary damage and total deaths. The natural disaster is included in the database if it meets at least one of the following criteria: (i) 10 or more people reported to be killed; (ii) 100 or more people reported affected; (iii) a declared state of emergency; (iv) a call for international assistance.

In line with Cavallo et al. (2013), we use the total deaths to population one year prior the catastrophe as the measure of severity of natural catastrophe. We find this measure preferable to monetary damages. The size of monetary damages is largely based on data from insurance companies and the monetary damages are likely to be underestimated in developing countries, as the insurance coverage increases with the level of economic development (Felbermayr and Groschl, 2014; Melecky and Raddatz, 2015). On the other hand, size of monetary damages may also be overestimated because governments may tend to exaggerate the damages in order to attract foreign aid (Noy, 2009).

The EM-DAT database contains many small disasters but: *"accounting for any macroeconomic impact necessitates focusing on a smaller subset of the disasters included in the EM-DAT database."* (Noy and Cavallo, 2011). We focus solely on extreme natural catastrophes to facilitate clean identification of the effects of catastrophes on financial development.[8] We consider extreme catastrophes, which effects are nearly impossible to be mitigated *ex ante* and are extremely devastating such as the 1970 Peru earthquake with a death toll of approximately 70 000 (according to EM-DAT database) or the 2004 tsunami that hit Sri Lanka and resulted in a death toll exceeding 35 000. In addition, synthetic control method does not require to know the actual severity of catastrophe. It is sufficient to know that the catastrophe has happened. This feature further addresses the issue of possible mis-measurement of the severity of catastrophes.

Conceptually, we define the extreme natural catastrophes as follows. We consider all disasters, as measured by the total number of people killed to total population (population as

[8]Therefore, our approach resembles the tail risk approach common in financial stability literature to examine consequences of the extreme negative shocks, see Corsi et al. (2018); Geraci et al. (2018).

of previous year), from EM-DAT database in the years 1960-2016[9], calculate the 99th percentile and use this percentile as the cut-off value. If the total number of people killed to total population exceeds the cut-off value (conditional on availability of data on financial development at least ten years prior and after the disaster[10]), we consider the disaster as extreme natural catastrophe.[11]

Note that we follow Cavallo et al. (2013) when defining the extreme natural catastrophes. They use percentile-based definition of catastrophes rather that often employed definition of one or two standard deviations above the mean. This is so because the distribution of death toll in natural disasters is extremely skewed and the mean serves as a poor proxy to judge the average intensity of disaster. Following Cavallo et al. (2013), the pool of control countries are those that did not experience large disaster in 1960-2016, or those that did not experience any catastrophe, and therefore do not appear in EM-DAT database.

As a result, we obtain ten extreme natural catastrophes and analyze its effect on financial development. The list of these ten treatment countries with the event year (i.e. when the catastrophe has happened), sample period, indication whether the country suffered from political revolution in the post-treatment period, type of catastrophe and the death toll is available in Table 1.[12] Note that we use data for at least ten years before and after the catastrophe to

[9]Note that we use the data up to 2016 because more recent data for our outcome variable – credit to GDP ratio – are not available in the World Bank databases yet.

[10]Since our dataset runs from 1960 and we need data for at least ten years before the catastrophe, this eliminates the countries with extreme disasters in the 1960s such as Bangladesh, Cuba, Haiti or Iran. Using a sufficient number of years before and after the natural catastrophe is critical for the credibility of synthetic control method (Abadie et al., 2010). We opt for ten years period before and after the catastrophe as in Cavallo et al. (2013).

[11]Moreover, several natural catastrophes might occur for one treatment country during the sample period because total deaths to population indicator might exceed the percentile cutoff more than once. In this case, we take the first severe catastrophe observed and redefine the post-treatment/post-catastrophe period to terminate in a year prior to the second severe catastrophe. Note that this procedure – also applied by Cavallo et al. (2013) – combined with the need to have data at least ten years prior to catastrophe eliminates Bangladesh, Haiti and Iran from our dataset. Note that there can be multiple observations for each country-year pair in the EM-DAT database depending on how many natural catastrophes occurred for a particular country in that year, each catastrophe will have its own entry. Therefore, for all catastrophes in the database, we sum up the total deaths for observations from the same country and year, thus collapsing the database to have only one total deaths observation per country and year. However, some of the observations in the database contain missing values for variable total deaths, meaning that the sum of total deaths for a particular country-year might not necessarily equal the true value if total deaths are not available for at least one catastrophe in that country-year. The issue of missing values will be analyzed specifically in detail in the results section.

[12]Note that these are mostly small countries from the global perspective. This is a favorable feature because the synthetic control method requires that the stable unit treatment value assumption is met, i.e. that the catastrophes in these small countries do not affect post-catastrophe observations in control countries. Even though we cannot rule out some indirect effects, such as that catastrophe affects the trade integration between the treated country and control countries, together with Cavallo et al. (2013) we believe that these indirect effects are small. Note that we exclude three extreme catastrophes from our sample: Afghanistan in 1998, Indonesia in 2004 and Venezuela in 1999. We exclude Afghanistan because of missing data on the outcome variable. Following Abadie et al. (2010) and Cavallo et al. (2013), we exclude Indonesia and Venezuela because of poor pre-treatment

match pre-catastrophe characteristics and to assess the consequences of catastrophes on financial development. As a result, we consider the catastrophes between 1970 and 2006.

Regarding our data on financial development, we use the credit to GDP ratio. This is most commonly employed indicator (measuring the depth of financial institutions) with very good data availability (Valickova et al., 2015). Given that we primarily investigate the catastrophes occurring in the 1970s-1980s, financial development measures other than the credit to GDP ratio are not available for a vast majority of countries.

We include a number of predictors, Z_j, and motivate the selection of these predictors by the relevant previous research (Cavallo et al., 2013; Poelhekke and Beck, 2017). Therefore, to match the pre-disaster path of the credit to GDP ratio of treatment country, we use the following predictors: credit to GDP ratio averaged over the first half of the pre-treatment period; real GPD per capita at purchasing power parities (PPP) from the World Bank World Development Indicators (WDI); inflation (annual growth rate of the GDP deflator) from WDI; trade openness (real exports plus real imports as a percentage of real GDP) from WDI; capital stock computed by the perpetual inventory method using data from the Penn World Tables; land area in km^2; population from WDI; secondary education attainment from Lutz et al. (2007); absolute value of latitude from Cavallo et al. (2013); Polity 2, an autocracy-democracy index ranging between -10 (total autocracy) and 10 (total democracy) from the Polity IV dataset described in Marshall and Jaggers (2002); total natural resources rents as a percentage of GDP from WDI; and net official development assistance (ODA) received as a percentage of GNI from WDI. Therefore, the predictors contain several macroeconomic, institutional, political, geographical and financial variables to account for the financial development. Clearly, there is a certain uncertainty regarding which variables to consider as predictors. For this reason, we follow previous literature, as stated above, and assess whether we can match closely pre-catastrophe financial development using these predictors (which is typically the case, as the next section shows). In addition, we also restrict the set of predictors and examine whether the synthetic control method results change in one of our robustness checks.

Some treatment countries do not have data available for all the above predictors. As a result, the predictor pool for them is smaller, see the list of all treatment countries and individual

fit, i.e. we did not find control countries that would sufficiently match the pre-catastrophe characteristics of these two countries.

Table 1: **Treatment countries: Basic Data Characteristics**

Treatment country	Event year	Sample period	Political revolution	Type of catastrophe	Number of people killed
Dominican Rep.	1979	1960–2016		storm, flood	1 432
Ecuador	1987	1960–2016		earthquake (2)	5 002
Guatemala	1976	1960–2016	x	earthquake	23 000
Honduras	1974	1960–1997		storm	8 000
Nicaragua	1972	1960–1997	x	earthquake	10 000
Pakistan	2005	1960–2016		earthquake, storm, flood (5)	74 032
Peru	1970	1960–2015		earthquake (2), flood	66 826
Papua New Guinea	1998	1973–2016		earthquake	2 182
Sri Lanka	2004	1960–2016		earthquake, flood	35 405
Turkey	1999	1960–2016		earthquake (5)	17 982

Notes: Treatment countries are those that experienced severe natural catastrophe. Event year is defined as the year of the first disaster. Sample period is determined by the availability of the data on the outcome variable - Domestic credit to private sector (% of GDP). Political revolution denotes the countries in which a revolution occurred within the first ten years after the disaster (based on Polity IV database). The data for Honduras and Nicaragua are up to 1997 because these countries experienced another natural catastrophe in 1998. The numbers in parentheses after the type of catastrophe suggest how many (if not one) catastrophes of such type occurred in the event year. x denotes the occurence of political revolution in the post-catastrophe period.

predictors in Table A2 in the Appendix.

5 Results

We provide our estimates regarding the effect of natural catastrophes on financial development in this section. We present the average causal effect across countries that we examine, the country-specific results are available in the Appendix.

We present our baseline results in Figure 1. Our results show that the actual financial development stagnates after the catastrophe but its synthetic counterpart gradually increases. As a result, we find that the financial development effects of natural catastrophes are sizable and long-lasting. The results suggest that a decade after the catastrophe, credit/GDP ratio remains approximately 30% below its counterfactual.

We present the evolution of financial development ten years before and after the natural catastrophe. The results show that we are able to match closely the pre-catastrophe evolution of financial development vis-a-vis its synthetic counterpart. This suggests that predictors and control countries have been reasonably chosen. Importantly, the control countries are broadly on the same level of economic development as the treated countries.[13] The ten years period

[13]See Table A1 on the synthetic control weights. Despite we do not restrict the donor pool to developing

(averaged across countries) is also chosen to reduce the focus on year-to-year fluctuations in financial development due to one-off shocks.

The average effect is based on seven developing countries (Dominican Republic, Ecuador, Honduras, Nicaragua, Pakistan, Peru, Sri Lanca), while Guatemala, Nicaragua and Turkey are excluded. Following Cavallo et al. (2013), we exclude Guatemala and Nicaragua because they suffered political revolutions[14] in the post-treatment period (i.e. within ten years after the catastrophe) and Turkey because there is a disagreement regarding whether it is a developing country.[15] However, we consider these countries in alternative specifications. The list of donor pool countries as well as the synthetic control weights are available in the Appendix.

Therefore, our results show that extreme large-scale natural catastrophes undermine financial intermediation.[16] As a consequence, our results accord more with the theories emphasizing that catastrophes (1) undermine the stability of financial intermediaries making them more prudent to extend credit (Klomp, 2014), (2) are accompanied with increased government expenditures and foreign aid crowding out private credit (Melecky and Raddatz, 2015), (3) reduce economic growth, damage collateral, heighten uncertainty and subsequently the demand for financial services and (4) do not spur investment into new capital stock in countries with underdeveloped financial systems (McDermott et al., 2014).

The synthetic control method is largely silent on the transmission mechanism - i.e. on the specific channels through which natural catastrophes undermined financial development. The further complication is the limited data availability for developing countries in the 1970s, when many of the severe natural catastrophes that we study occurred. For example, the World Bank's Global Financial Development Database often provides the detailed data on various aspects of financial development, financial stability or the access to finance only from the 2000s (only the variables such as credit to GDP is available for the period before 2000). Therefore,

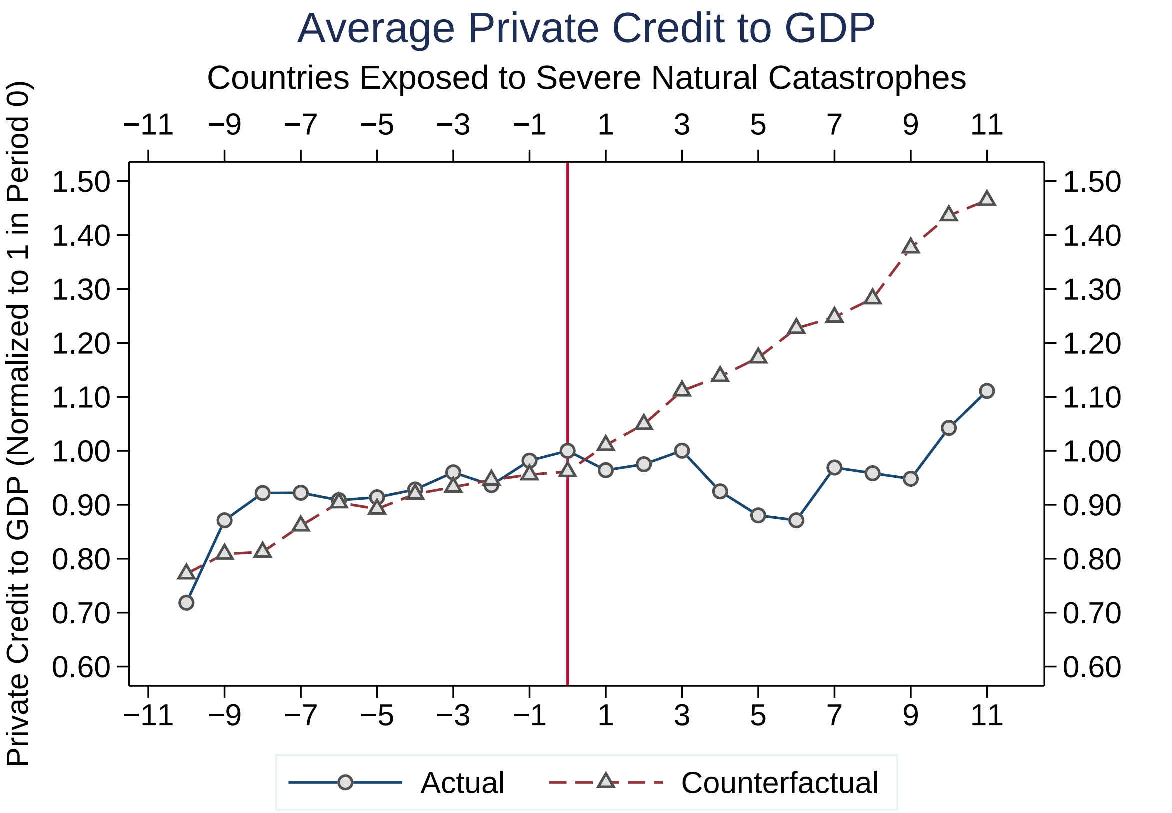
countries only (and therefore include both developed and developing countries), the synthetic control method itself chooses developing countries as relevant controls. This further confirms the usefulness of our econometric approach.

[14]Peru had political revolution more than ten years after the catastrophe. Political revolutions may blur the estimated effect because they may lead to various economic disruptions.

[15]While World Bank does not list Turkey among low income countries, United Nation includes Turkey among developing countries (United Nations, 2018). Strictly speaking based on economic grounds, it is difficult to categorize Turkey as the developing country and for this reason, we include Turkey in the sample only in the robustness checks.

[16]It is important to mention that we base our results on Cavallo et al. (2013). Using a similar dataset to ours, they find that the effect of natural catastrophes on economic growth is, on average, zero. Note that credit-GDP represents our measure of financial development. Therefore, the underlying assumption is that the denominator in the ratio of credit-GDP does not change a lot.

Figure 1: **The Effect of Severe Catastrophes on Financial Development**



Notes: Actual and counterfactual path of domestic credit to private sector (% of GDP) 10 years before and after the catastrophe. Excluding countries with political revolutions in post-treatment period and Turkey. Average taken across countries with extreme natural catastrophe that did not experience any political revolution within the first ten years after the catastrophe. Turkey is excluded in the baseline estimation because of disagreement whether it is a developing country.

we focus on the Papua New Guinea, Pakistan, Sri Lanka and Turkey because their natural catastrophes happened in the late 1990s or in the 2000s. Examining the World Bank's Global Financial Development Database (its last available version from July 2018), we find that the banks' z-score (i.e. the variable assessing the stability of bank industry) typically deteriorated following the catastrophe. The z-score of Papua New Guinea banks decreased from 4.81 in the catastrophe year to 3.34 and 1.87 in the following two years, respectively, and recovered only three years after the catastrophe. The banks' z-score in Turkey exhibited a similar and even more pronounced trend. The z-score returned to pre-catastrophe values only five years after the catastrophe. Similarly, the banks' z-score dropped significantly in the catastrophe year in Sri Lanka. However, we do not observe a deterioration of banks' z-score in Pakistan. Therefore, the results suggest some support to the notion that the bank instability propagated the shocks from natural catastrophes into financial development. Unfortunately, the data on the access to finance (i.e. to shed light on the bank prudence) are not available in these countries to investigate the propagation mechanism further.

In addition, our results broadly support the theoretical prediction by McDermott et al. (2014), who show that countries with underdeveloped financial markets suffer the consequences of natural catastrophes even in the long-term. This corresponds to our finding that the negative effects of large-scale natural catastrophes on financial development in developing countries are long-lasting. In broad terms, our results are somewhat at odds with studies examining the negative effects of bombing (as another negative supply side shock) during the wars on economic activity (Davis and Weinstein, 2002; Miguel and Roland, 2011); these studies suggest that economic recovery is relatively fast.

Interestingly, Poelhekke and Beck (2017) make a similar observation but for natural resource windfalls, i.e. that lending and deposits fall following the windfalls. While windfalls are often interpreted as the positive shock, catastrophes are typically thought of as the negative shock. Poelhekke and Beck (2017) rationalize their findings of "natural resource curse in finance" with a increased role of government in resource-rich economies with public sector crowding out private sector. It is obvious that our results can be compared only if we assume that positive and negative shocks can be treated symmetrically. However, windfalls do not necessarily need to represent positive shock, as they may give rise to Dutch disease or undermine the quality of institutions (for example, because of increased corruption) (van der Ploeg, 2011). The additional difference between our results and Poelhekke and Beck (2017) is likely to be driven that we focus on severe catastrophes in a few countries, while the Poelhekke and Beck (2017) sample is global and includes windfalls of various intensity.

The distribution of post/pre-intervention MSPE ratios is plotted in Figure A2 for treatment countries that did not experience any political revolution within the first ten years after the catastrophe (and in Figure A3 for treatment countries in which the political revolution occurred). This allows us to compare the gap between actual and counterfactual credit to GDP path obtained for a treatment country with the gaps obtained for its placebos. Generally, for a treatment country whose pre-intervention characteristics are replicated accurately with its synthetic control, the pre-intervention MSPE should be low, pushing the ratio to be high.

However, if there is only a negligible effect of the intervention or if there is no effect at all, post-intervention MSPE will be low, making the overall ratio lower as well. Therefore, treatment countries with post/pre-intervention MSPE ratios clearly standing out in the distribution suggest that a catastrophe had a significant impact on the credit to GDP and that we were able to match a pre-catastrophe trajectory of the treatment's country credit to GDP.

In addition, following Cavallo et al. (2013), we also estimate the p-values to assess whether the average effect of catastrophes on financial development is statistically significant. The results are available in Figure A4 in the Appendix and clearly show that the probability that the effect of catastrophes on financial development is statistically significant by chance is close to zero. Overall, our results suggest that the effect of extreme natural catastrophes on financial development is negative, statistically significant and economically sizable.

When interpreting the results, it should be noted that the advantage of synthetic control method lies in providing the causal estimate and counterfactual in order to identify the size of the estimated effect. On the other hand, synthetic control method says less on actual transmission mechanism (Abadie et al., 2010).

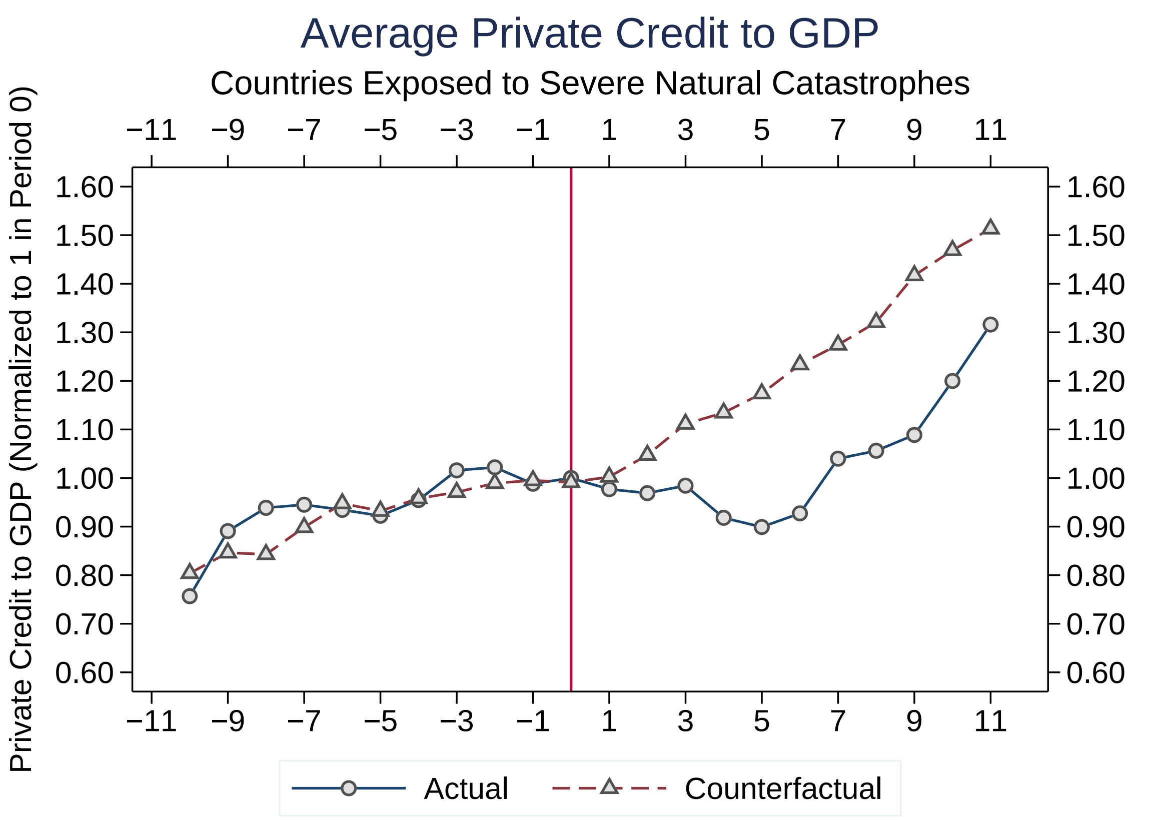
5.1 Robustness Analysis

We provide a number of robustness checks to assess whether our baseline results regarding the effect of natural catastrophes on financial development hold. However, it should be mentioned that our baseline specification is preferable based on economic reasoning (for example, we find preferable to exclude the countries with political revolutions from the baseline results, as we argue above).

First, we exclude the countries, which suffered the political revolution in less than 10 years after the natural catastrophe but we retain Turkey in the list. Therefore, we exclude Guatemala and Nicaragua and present the average effect of natural catastrophes on financial development based on the remaining eight countries. We present the results in Figure 2. The results broadly correspond to our baseline findings. The severe catastrophes still have a negative effect on financial development. The actual financial development remains 15% below its counterfactual even ten years after the catastrophe.

Second, we present the average effect of natural catastrophes on financial development for all ten countries, i.e. we neither exclude the countries with subsequent political revolutions

Figure 2: **The Effect of Severe Catastrophes on Financial Development: Excluding Countries with Political Revolutions**

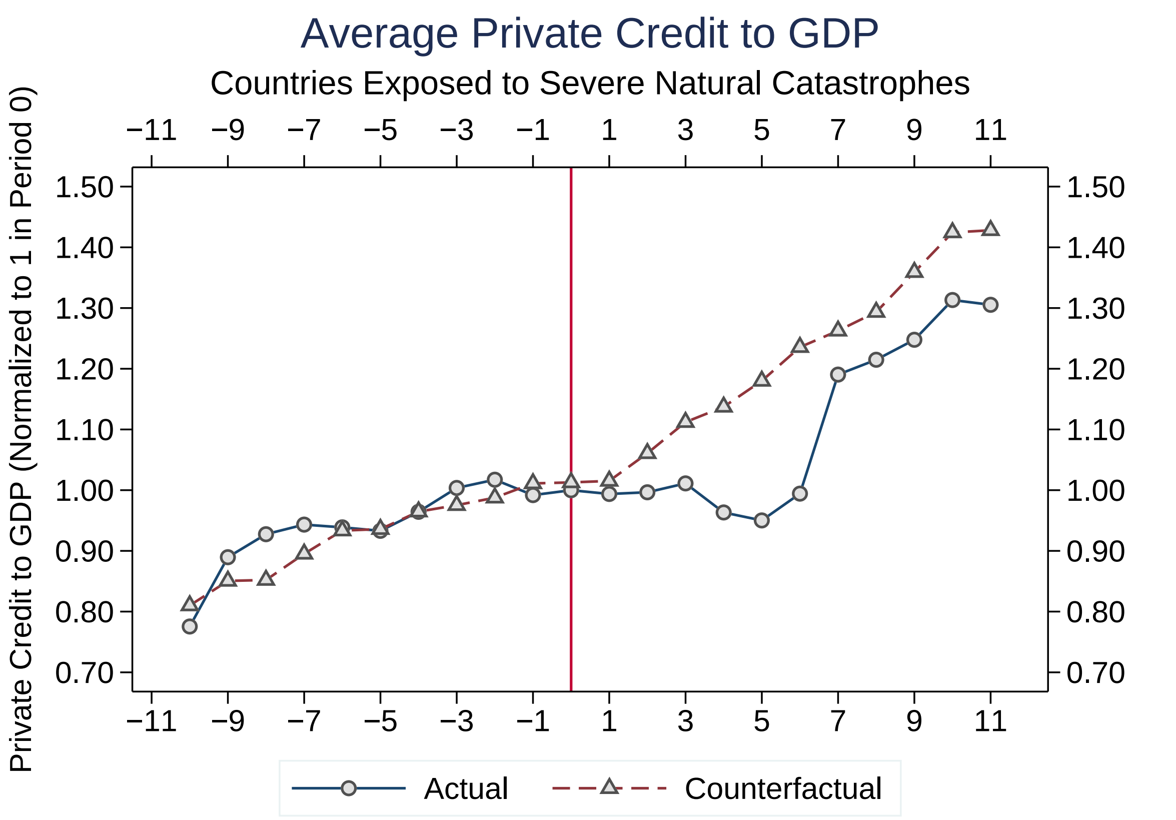


Notes: Actual and counterfactual path of domestic credit to private sector (% of GDP) 10 years before and after the catastrophe. Average taken across countries with extreme natural catastrophe that did not experience any political revolution within the first ten years after the catastrophe.

nor Turkey. The results are available in Figure 3. Again, we confirm our baseline finding in observing that catastrophes undermine financial development. We estimate the average effect to be broadly similar to the baseline findings five years after the catastrophe but the effect becomes subsequently weaker to some 10%.

Third, we present the average effect of severe catastrophes on financial development excluding the countries, which has some missing data on the death toll. Suppose, for example, that the country experiences three large earthquakes in the first, second and third week of July. The death toll for the third week is not available but the death toll in the first and second week is large enough for the country to qualify among 99% percentile in terms of death toll. As a result, we exclude the following countries: Ecuador (catastrophe in 1987) and Turkey (catastrophe in 1999). The underlying reasoning to exclude these countries is that if some data

Figure 3: **The Effect of Severe Catastrophes on Financial Development: All Countries**

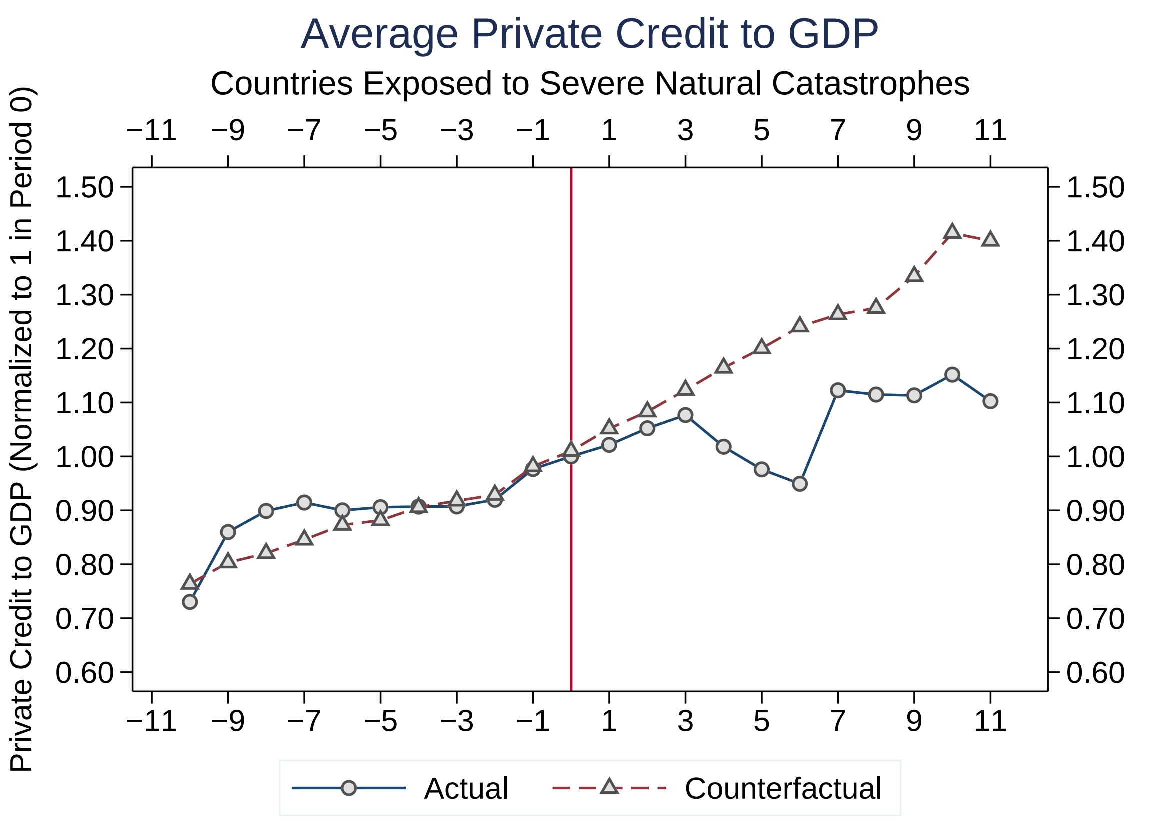


Notes: Actual and counterfactual path of domestic credit to private sector (% of GDP) 10 years before and after the catastrophe. Average taken across all countries with extreme natural catastrophe.

are missing, maybe the remaining available data are of lower quality. Our results suggest that natural catastrophes undermine financial intermediation and the actual financial development is nearly 30% below its counterfactual a decade after the catastrophe, which constitutes a nearly identical estimate to the baseline findings. We present these results in Figure 4.

Fourth, we examine the effect of severe catastrophes on financial development and restrict the set of predictors to be identical for all countries. Previously, we employed different predictors for different countries, which is dictated by the data availability issues, to a certain extent. Table A2 provides a list of predictors and in this robustness check, we use only those predictors, which are available for all countries. We present the results in Figure 5. We find that the effects of catastrophes on financial development are sizable with the actual credit to GDP ratio to be 60% below the counterfactual a decade after the catastrophe.

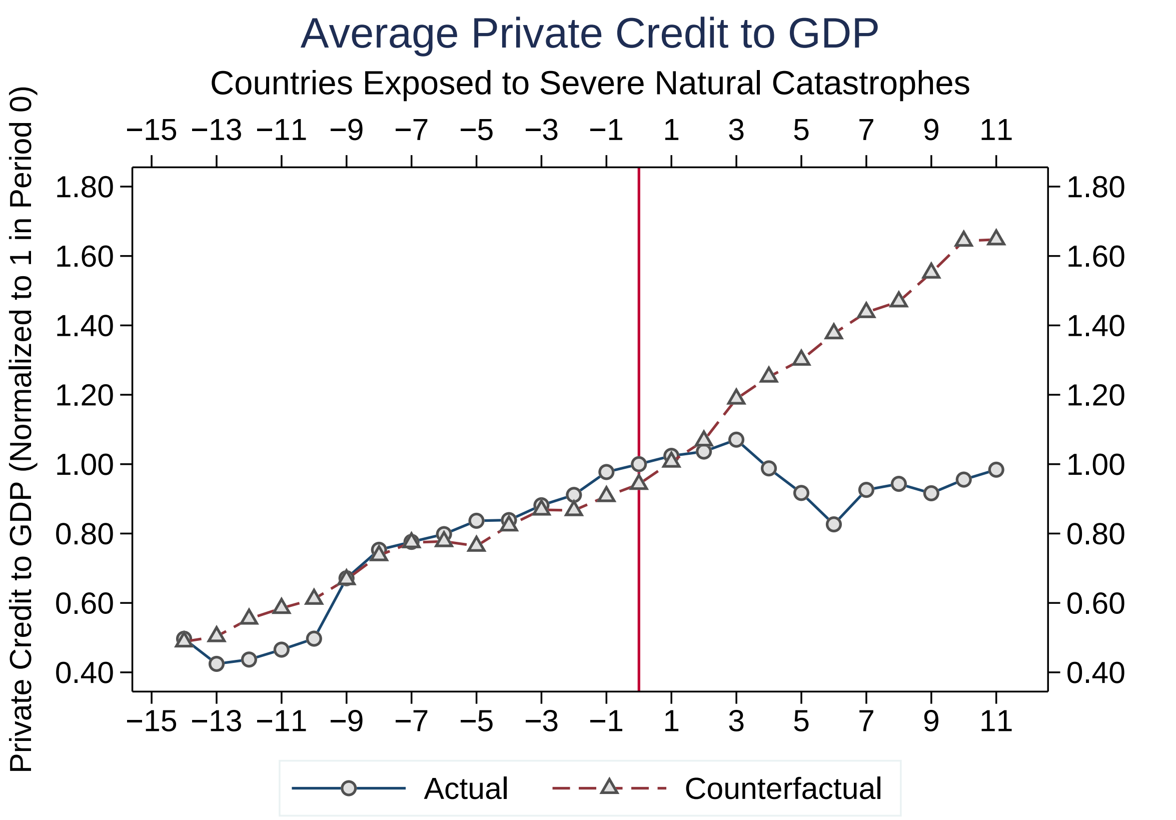
Figure 4: **The Effect of Severe Catastrophes on Financial Development: Countries with Missing Data Excluded**



Notes: Actual and counterfactual path of domestic credit to private sector (% of GDP) 10 years before and after the catastrophe. Average taken across countries with extreme natural catastrophe excluding those with missing observations on death toll for at least one of the catastrophes in the treatment year.

Overall, our set of robustness checks show that extreme catastrophes undermine financial development in all the cases regardless whether we include countries with subsequent political revolutions, consider not only developing countries, restrict the set of predictors or exclude countries with some of the data missing. The estimated average effect varies somewhat but this is not so surprising because we subject our baseline findings to a number of stringent tests.

Figure 5: **The Effect of Severe Catastrophes on Financial Development: Identical Set of Predictors**



Notes: Actual and counterfactual path of domestic credit to private sector (% of GDP) 10 years before and after the catastrophe. Average taken across large disaster countries using the identical set of predictors.

6 Conclusions

Traditionally, the financial development literature has focused on the determinants such as the rule of law, trade openness or financial openness (Rajan and Zingales, 2003; Baltagi et al., 2009; Chinn and Ito, 2006). In this paper, we show that natural catastrophes substantially affect financial development if the catastrophes are severe. The analysis of the consequences of natural-related shocks gains importance, as the natural disasters became more frequent and destructive during last decades (Emanuel, 2005; Webster et al., 2005).

The evidence on how natural-related shocks affect financial development is scarce and only handful studies examined this issue. We contribute to the literature by estimating the causal effect of catastrophes on financial development and by computing the counterfactual, i.e. what financial development would be absent the natural catastrophe, which, to our knowledge, has not been undertaken before.

Comparing the actual post-catastrophe financial development with the counterfactual allows us to assess not only the statistical significance but also economic importance of the effect. We use the synthetic control method to compute the counterfactual (Abadie et al., 2010). In addition to estimating the causal effect and counterfactual, the method also has other favorable features such as that it controls for unobserved time-varying heterogeneity. Unlike most other papers examining the economic consequences of natural catastrophes, we also investigate the long-term effect of catastrophes. We focus on ten largest natural catastrophes during last six decades in developing economies because this helps us to identify the causal effect. The specific timing of catastrophes is not predictable and importantly, the effects of these extremely devastating natural catastrophes are nearly impossible to be mitigated *ex ante*.

Our estimates suggest that the effects of natural catastrophes are sizable, statistically significant and long-lasting. We find that a decade after the catastrophe, our measure of financial development – credit/GDP ratio – remains approximately 30% below its counterfactual. This finding is novel and to our knowledge, not available in the literature before. We subject the baseline findings to a several robustness checks such as excluding the countries, which experienced subsequent political revolution or using the identical set of predictors across countries. Overall, our results show that the natural catastrophes severely undermine financial intermediation.

In terms of future research, we believe it would be worthwhile to examine the long-term effects of large-scale natural catastrophes on financial inclusion or access to finance in developing economies. The cross-country data on financial inclusion or access to finance are already available but its time coverage is typically limited preventing a researcher to conduct multi-country analysis with the current datasets.

It would also be interesting to examine whether (and to what extent) the type of natural catastrophes, the duration of its occurrence, and its global scale matters. The natural catastrophes we examine in this paper are largely one-off extreme negative supply-side shock at the national level. However, a different type of catastrophes such as the current COVID-19 global pandemic are potentially long-lasting and global. Global and long-lasting catastrophes may have even more devastating consequences, especially in developing countries characterized by a low level of financial development.[17] Therefore, these catastrophes may further increase a wedge in the level of financial development between developed and developing countries.

[17]This may occur for a variety of reasons. For example, the uncertainty regarding the intensity and duration of this catastrophe is greater than the natural catastrophes we examine in this paper. Its global nature may reduce the willingness to provide foreign aid. The potentially long duration of COVID-19 pandemic increases the chances of concurrent occurrence with some other natural catastrophes such as hurricanes or earthquakes creating a double shock.

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Appendix (can be published online)

Synthetic control weights

Table A1: **Treatment Countries and the Synthetic Control Weights**

Dominican Republic	
Bolivia	0.405
Republic of Korea	0.132
Malaysia	0.284
Thailand	0.178
Ecuador	
Colombia	0.425
Uganda	0.575
Guatemala	
Costa Rica	0.174
Gabon	0.039
Morocco	0.022
Mexico	0.066
Nigeria	0.656
Philippines	0.009
Thailand	0.036
Honduras	
Cameroon	0.082
Costa Rica	0.021
Republic of Korea	0.226
Malaysia	0.176
Thailand	0.496
Sri Lanka	
Ghana	0.541
Republic of Korea	0.116
Philippines	0.331
Thailand	0.012
Nicaragua	
Austria	0.037
Costa Rica	0.203
Paraguay	0.649
Syrian Arab Republic	0.111
Pakistan	
Cameroon	0.148
Colombia	0.451
India	0.099
Paraguay	0.303
Peru	
Argentina	0.180
Costa Rica	0.172
Ghana	0.432
Philippines	0.174
Uruguay	0.042
Papua New Guinea	
Costa Rica	0.032
Israel	0.005
Malaysia	0.102
Niger	0.792
Sierra Leone	0.070
Turkey	
Argentina	0.010
Colombia	0.391
India	0.103
Mexico	0.101
Uganda	0.395

Notes: Treatment countries (in bold) and the weights assigned to countries making up each treatment country's synthetic control. Each treatment country denotes one particular synthetic control method analysis.

Table A2: **Treatment Countries and Predictors**

	Dominican Republic	Ecuador	Guatemala	Honduras	Sri Lanka	Nicaragua	Pakistan	Peru	Papua New Guinea	Turkey
GDP per capita	x	x	x	x	x	x	x	x	x	x
Inflation	x	x	x	x	x	x	x	x	x	x
Trade openness	x	x	x	x	x	x	x	x	x	x
Capital stock	x	x		x	x	x	x	x	x	x
Land area (km^2)	x	x	x	x	x	x	x	x	x	x
Population	x	x	x	x	x	x	x	x	x	x
Education attainment	x	x	x	x	x	x	x	x		x
Latitude (absolute value)	x	x	x	x	x	x	x	x	x	x
Democracy score	x	x	x	x	x	x	x	x	x	x
Resources rents (% of GDP)	x	x	x	x	x		x		x	x
Net ODA received (% of GNI)	x	x	x	x	x		x	x	x	x

Notes: Treatment countries and their predictors used in the synthetic control analysis. All countries include outcome variable averaged over the first half of pre-treatment period as a predictor. Other predictors are averaged over the whole pre-treatment period.

Country-specific results

Figure A1: **Treatment countries without political revolutions**

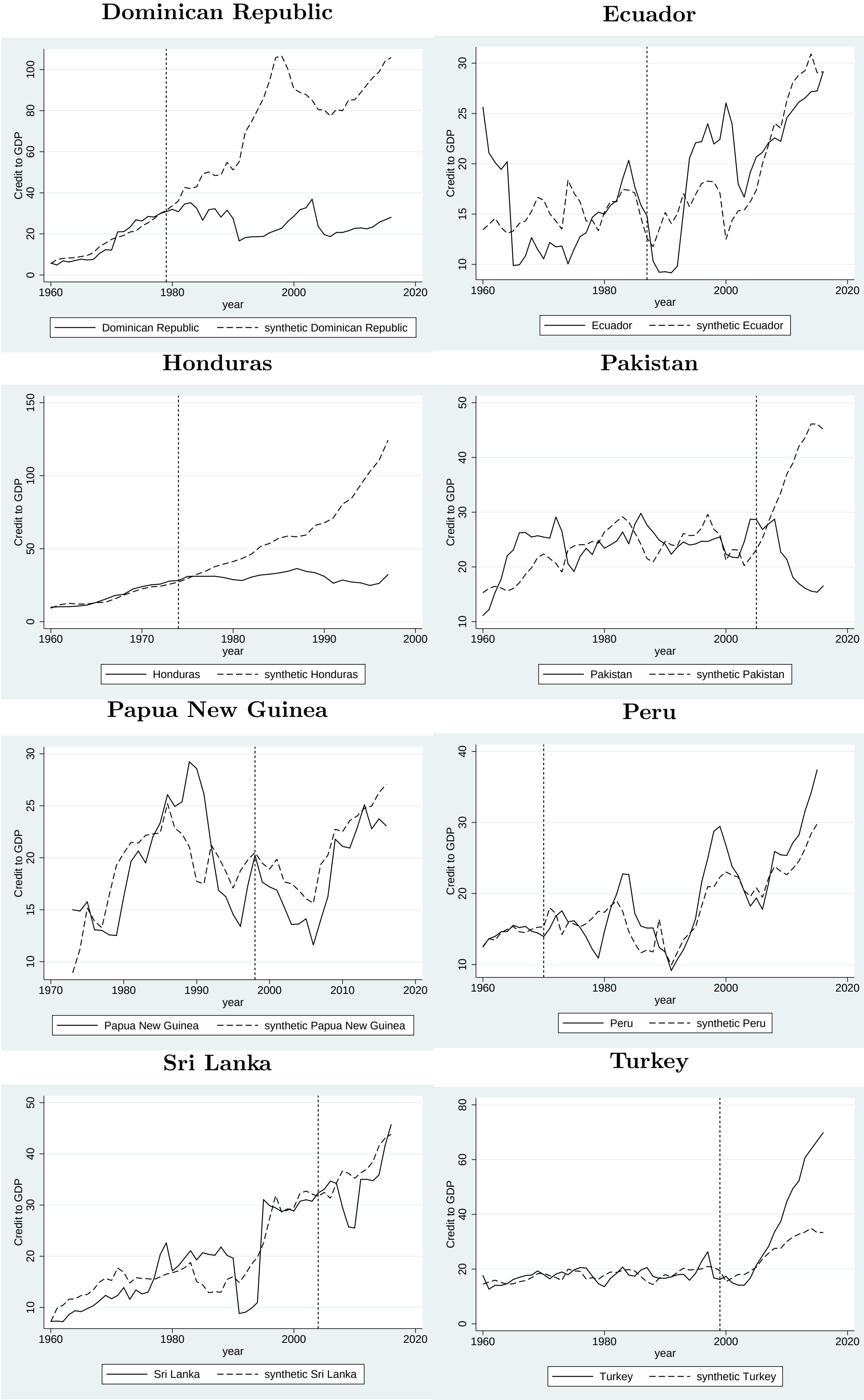


Figure A2: **Distribution of post- to pre-intervention MSPE**

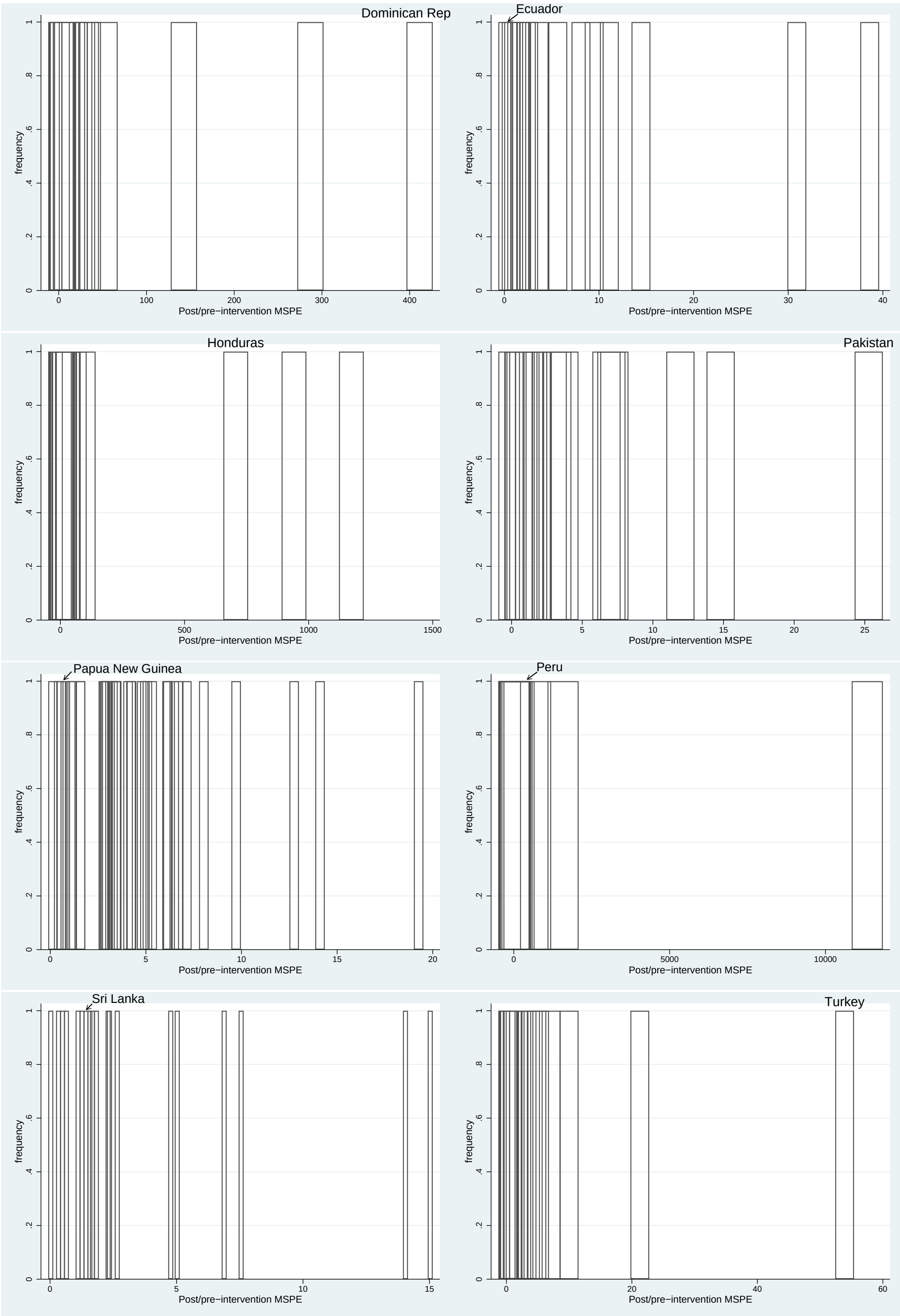
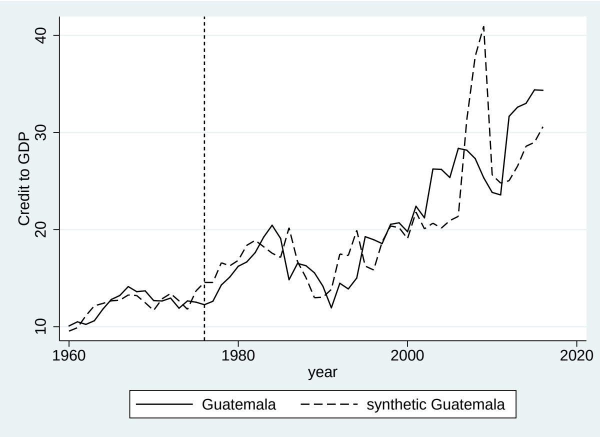
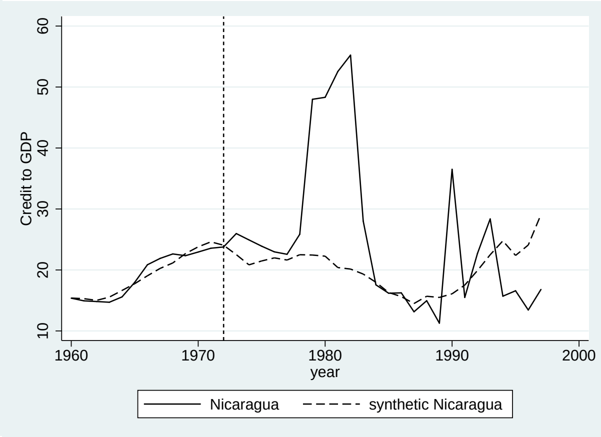


Figure A3: **Treatment countries with political revolutions**

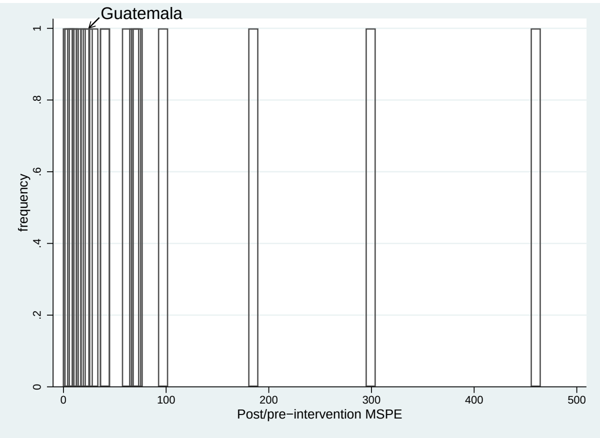
Guatemala

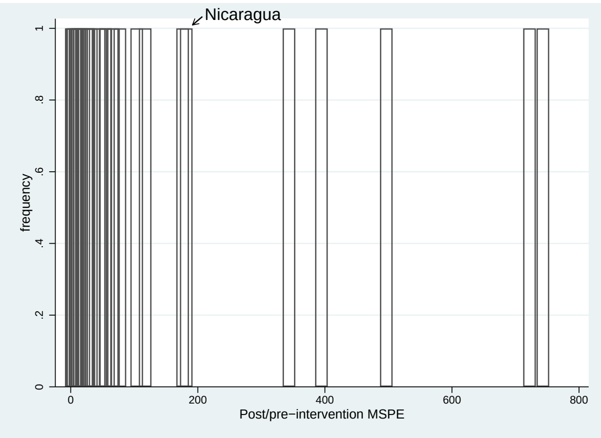


Nicaragua



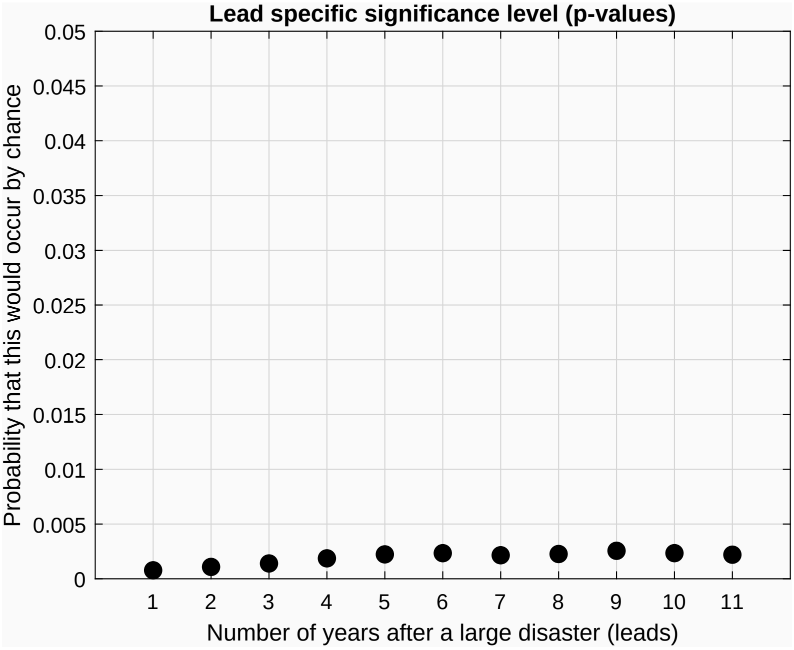
Distribution of post- to pre-intervention MSPE





Inference

Figure A4: **Estimated Average Catastrophe Effect: P-Values**



Notes: Lead-specific p-values for the estimated average catastrophe impact. Average catastrophe impact was computed using the data on large catastrophe countries that did not experience any political revolution within the first ten years after the natural catastrophe.