

CATASTROPHIC NATURAL DISASTERS AND ECONOMIC GROWTH

Eduardo Cavallo, Sebastian Galiani, Ilan Noy, and Juan Pantano*

Abstract—We examine the average causal impact of catastrophic natural disasters on economic growth by combining information from comparative case studies. For each country affected by a large disaster, we compute the counterfactual by constructing synthetic controls. We find that only extremely large disasters have a negative effect on output in both the short and the long runs. However, we also show that this results from two events where radical political revolutions followed the disasters. Once we control for these political changes, even extremely large disasters do not display any significant effect on economic growth.

I. Introduction

LARGE, sudden natural disasters such as earthquakes, tsunamis, hurricanes, and floods generate destruction on impact. Recent events such as the Indian Ocean tsunami in 2004, Hurricane Katrina in 2005, and the Haitian and Chilean earthquakes in 2010 received worldwide media coverage, and there is an increasing sense of awareness among the general public about the destructive nature of disasters. Much research in both the social and natural sciences has been devoted to increasing our ability to predict disasters, while the economic research on natural disasters and their consequences is fairly limited.¹ In this paper, we contribute to closing this gap by carefully examining the causal effect of the occurrence of large natural disasters on gross domestic output in both the short and long runs.

Growth theory does not have a clear-cut answer on the question of whether natural disasters should affect economic growth. Traditional neoclassical growth models predict that the destruction of capital (physical or human) does not affect the rate of technological progress and, hence might only enhance short-term growth prospects as it drives countries away from their balanced-growth steady states. In contrast, endogenous growth models provide less clear-cut predictions with respect to output dynamics. For example, models based on Schumpeter's creative destruction process may even

asccribe higher growth as a result of negative shocks, as these shocks can be catalysts for reinvestment and upgrading of capital goods (see, for example, Caballero & Hammour, 1994). In contrast, the AK-type endogenous growth models in which technology exhibits constant returns to capital predict no change in the growth rate following a negative capital shock, while endogenous growth models that exploit increasing returns to scale in production generally predict that a destruction of part of the physical or human capital stock results in a lower growth path and, consequently, a permanent deviation from the previous growth trajectory.

Thus, the question of whether natural disasters affect economic growth is ultimately an empirical one, precisely the one we address in this study.² Few papers have attempted to answer this question, and although the evidence is pointing toward the conclusion that large natural disasters negatively affect economic growth in the short term, it is still inconclusive.³ Furthermore, the bulk of the empirical evidence available focuses on the short-run effects.⁴

We contribute to this literature by bringing a new methodological approach to answer the question of sign and size of the short- and long-run effects of large natural disasters on growth. In particular, following Abadie, Diamond, and Hainmueller (2010), we pursue a comparative event study approach, taking advantage of the fact that the timing of a large, sudden natural disaster is an exogenous event. The idea is to construct an appropriate counterfactual—what would have happened to the path of GDP of the affected country in the absence of the natural disaster?—and assess the disaster's impact by comparing the counterfactual to the actual path observed. Importantly, the counterfactuals are not constructed by extrapolating preevent trends from the treated

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*Cavallo: Inter-American Development Bank; Galiani: University of Maryland; Noy: University of Hawaii and Victoria University of Wellington; Pantano: Washington University in St. Louis.

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¹ In particular, very little is known about whether output losses in the aftermath of natural disasters are recovered. This is an important question for the development literature. Barro (2006, 2009) has shown that the infrequent occurrence of economic disasters has much larger welfare costs than continuous economic fluctuations of lesser amplitude. However, at the empirical level, we still do not know much about the aggregate effects of natural disasters.

² The macroeconomic literature generally distinguishes between short-run effects (usually up to five years) and longer-run effects (anything beyond that horizon). The first recent attempt to empirically describe short-run macroeconomic dynamics following natural disasters is Albala-Bertrand (1993). In a related literature, Kahn (2004) and Kellenberg and Mobarak (2008) study the relationship between economic development and vulnerability to natural disasters. Yang (2008) studies the impact of hurricanes on international financial flows.

³ Skidmore and Toya (2002) find that in a cross-sectional, long-run perspective, output growth is positively correlated with natural disaster frequency. This is an interesting but somewhat different question relative to the one we pursue here. They look at how disaster risk correlates with growth in the long run, not at what the causal impact of a given disaster is. It is particularly difficult to tackle their question in a more causal framework as it is difficult to find exogenous changes in a country's inherent risk to suffer natural disasters. Noy and Nualsri (2007) find an adverse effect on output growth using a five-year average panel (as in Barro & Sala-i-Martin, 2003), see also Raddatz, 2009, and Loayza et al., 2009.

⁴ For example, using a panel vector autoregression (VAR) framework on a sample of low-income countries, Raddatz (2007) finds that natural disasters have an adverse short-run impact on output dynamics. Noy (2009) finds a similar result exploiting cross-country variability by means of the Hausman-Taylor random effects estimator. See Cavallo and Noy (2011) for a detailed survey of this literature.

countries but rather, following Abadie and Gardeazabal (2003), by building a synthetic control group—that is, using as a control group other untreated countries that, optimally weighted, estimate the missing counterfactual of interest. Given the macronature of the question we investigate, we believe this methodology provides the best feasible identification strategy of the parameter of interest. To the best of our knowledge, ours is the first paper that applies this quasi-experimental design to a topic within the economic growth literature.

In the cross-country comparative case studies we describe here, we compare countries affected by natural disasters to a group of unaffected countries. The analysis is feasible only when some countries are exposed and others are not. Thus, we focus our analysis only on large events rather than on recurrent events that are prevalent everywhere. Moreover, the methodology requires that we can trace the evolution of the outcome variable for several years after the event. For that reason, we limit the sample to disasters that occur before the year 2000. In addition, we adapt the synthetic control methods developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010) to combine information from several large disasters.

From the outset, we stress that we are not testing or distinguishing among alternative growth theories of the relationship between natural disasters and economic growth. Instead, we attempt to rigorously establish the direction and magnitude of the average causal effect of large natural disasters on economic growth, an important piece of evidence not yet conclusively established in the literature.

- Our results show that only very large disasters, whereby “large” is defined in relation to the distribution of direct damages caused by the natural event, display an impact on GDP growth in the affected countries in both the short and the long runs. The effects are both statistically significant and economically meaningful. For example, ten years after the disaster, the average GDP per capita of the affected countries is (on average) 10% lower than it was at the time of the disaster, whereas it would be about 18% higher in the counterfactual scenario in which the disaster did not occur.
- However, these large effects are all driven by events that were followed by radical political revolution: the Islamic Iranian Revolution in 1979 and the Sandinista Nicaraguan Revolution in 1979. Those not followed by radical political changes do not show significant subsequent effects on economic growth.
- For milder events, we do not find evidence of any significant impact on GDP growth in either the short or the long run.

Thus, we find that only very large natural disasters, followed by radical political revolution, show long-lasting negative economic effects on economic growth. Even very large natural disasters, when not followed by disruptive political reforms that alter the economic and political system, including the system or property rights, do not display significant effects on economic growth.

The structure of the paper is as follows. Section II presents the empirical methodology, and section III describes the

data. Results are discussed in section IV, and the conclusion follows.

II. Empirical Methodology

Identification of the causal effect of natural disasters on economic growth is difficult. Estimates of the effect of natural disasters on GDP exploiting cross-sectional variability are likely to be severely biased upward (in absolute value) due to the fact that, *ceteris paribus*, empirically the magnitude of natural disasters is larger among poor countries. Though stratifying the analysis by income level might help to attenuate this omitted variable bias, it can hardly be argued that it would solve the problem.

A natural solution is to rely on longitudinal data to control for time-invariant unobservable variables. Nevertheless, exploiting the within-country variability requires that the group of countries that are not shocked by natural disasters (i.e., the control group) allow us to estimate what would have been the growth rates of the affected countries (i.e., the treatment group) in the absence of the shocks. Unfortunately, this assumption is difficult to be satisfied in general. If, for example, the countries in the control group, on average, were to grow at a faster rate than those affected by natural disasters even in the absence of these shocks, panel data estimates will also tend to be biased upward (in absolute value). One can attempt to control for the differential trends across countries by controlling for country-specific trends in the econometric model. This entails extrapolating to the postshock period the preshock trends, which is a strong assumption, especially over long-periods of time.

To overcome the problems of identification outlined above, we need to find a group of countries that have had the same secular trends in the dependent variable analyzed (GDP or GDP growth rates) and likely would have had the same secular behavior in the absence of the shocks studied. We can then use this group to estimate the counterfactual and conduct a causal analysis. We do this by adopting a novel methodological approach: comparative case studies. This approach is more general than the fixed-effects model commonly applied in the empirical literature. The fixed-effects model allows the presence of unobserved confounders but restricts the effect of those confounders to be constant in time. Instead, the approach we adopt here allows the effects of confounding unobserved characteristics to vary with time. Below we describe this approach in detail.

A. Estimating the Impact of Large Disasters with Comparative Case Studies

Case studies focus on particular occurrences of the interventions of interest. In a case study, one is usually interested in finding the effects of an event or policy intervention on some outcome of interest. In a cross-country comparative case study, we compare countries affected by the event of interest (in our case, a large natural disaster) to a group of

unaffected countries. We first focus on establishing some notation to evaluate the effect of a large disaster for a single country. We later aggregate the country-specific effects into an average effect.

We observe $J + 1$ countries. Without loss of generality, let the first country be the one exposed to a large natural disaster, so that we have J remaining countries that serve as potential controls. Below we will be more explicit about how we combine the information from different disasters. In comparative case studies, it is assumed that the treated unit is uninterrupted exposed to treatment after some initial intervention period. In our case, we consider the occurrence of the catastrophic event as the initiation of the intervention period (which includes the disaster's aftermath).⁵

Following Abadie et al. (2010), let Y_{it}^N be the GDP per capita that would be observed for country i at time t in the absence of the disaster, for countries $i = 1, \dots, J + 1$, and time periods $t = 1, \dots, T$. Let T_0 be the number of periods before the disaster, with $1 \leq T_0 < T$. Let Y_{it}^I be the outcome that would be observed for country i at time t if country i is exposed to the disaster and its aftermath from period $T_0 + 1$ to T . Of course, to the extent that the occurrence of a large disaster is unpredictable, it has no effect on the outcome before the intervention, so for $t \in \{1, \dots, T_0\}$ and all $i \in \{1, \dots, N\}$, we have that $Y_{it}^I = Y_{it}^N$.⁶

Let $\alpha_{it} = Y_{it}^I - Y_{it}^N$ be the effect of the disaster for country i at time t if country i is exposed to the intervention in periods $T_0 + 1, T_0 + 2, \dots, T$ (where $1 \leq T_0 < T$). Note that we allow this effect to potentially vary over time. Again, the intervention, in our context, is the disaster and its aftermath. Therefore;

$$Y_{it}^I = Y_{it}^N + \alpha_{it}. \quad (1)$$

Let D_{it} be an indicator that takes the value 1 if country i is exposed to the intervention at time t and value 0 otherwise. The observed output per capita for country i at time t is

$$Y_{it} = Y_{it}^N + \alpha_{it}D_{it}. \quad (2)$$

Because only the first country (say, country "1") is exposed to the intervention and only after period T_0 (with $1 \leq T_0 < T$), we have that

$$D_{it} = \begin{cases} 1 & \text{if } i = 1 \text{ and } t > T_0 \\ 0 & \text{otherwise} \end{cases}.$$

⁵ While a natural disaster is usually followed by substantial aid, Becerra, Cavallo, and Noy (2012) note that postdisaster aid is actually a small fraction of the extent of damages, even for very large disasters with a lot of media attention. Yang (2008), in contrast, finds substantial aid flows following hurricanes. In any event, our results identify only the aggregate impact of the natural disasters (including, if any, whatever effective ameliorating action was pursued).

⁶ The assumed unpredictability of natural disasters is not inconsistent with the fact that some countries are more prone than others to suffer natural disasters. In a sense, this risk is already discounted and may influence the steady-state growth rate of the country. But conditional on this underlying propensity, the specific timing of occurrence is unpredictable.

Our parameters of interest are $(\alpha_{1,T_0+1}, \dots, \alpha_{1,T})$, the lead-specific causal effect of the catastrophic event on the outcome of interest. For $t > T_0$,

$$\alpha_{1t} = Y_{1t}^I - Y_{1t}^N = Y_{1t} - Y_{1t}^N. \quad (3)$$

Note that Y_{1t}^I is observed. Therefore, to estimate α_{1t} , we need only to come up with an estimate for Y_{1t}^N .

Now consider a $(J \times 1)$ vector of weights $W = (w_2, \dots, w_{J+1})'$ such that $w_j \geq 0$ for $j = 2, \dots, J + 1$ and $w_2 + w_3 + \dots + w_{J+1} = 1$. Each particular value of the vector W represents a potential synthetic control, that is, a particular weighted average of control countries. Also let Z_i be an $(r \times 1)$ vector of observed predictors for GDP per capita (not affected by the natural disaster)⁷.

Suppose that there exists a set of weights $(w_2^*, \dots, w_{J+1}^*)$ satisfying $\sum_{j=2}^{J+1} w_j^* = 1$ such that

$$\sum_{j=2}^{J+1} w_j^* Y_{j1} = Y_{1,1} \quad (4)$$

⋮

$$\sum_{j=2}^{J+1} w_j^* Y_{j,T_0} = Y_{1,T_0} \quad (5)$$

$$\sum_{j=2}^{J+1} w_j^* Z_j = Z_1. \quad (6)$$

Abadie et al. (2010) suggest using

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$$

for $t \in \{T_0 + 1, \dots, T\}$ as an estimator of α_{1t} .

The system of equations in (4), (5), and (6) can hold exactly only if $(Y_{1,1}, \dots, Y_{1,T_0}; Z_1')$ belongs to the convex hull of

$$\{(Y_{2,1}, \dots, Y_{2,T_0}; Z_2'), \dots, (Y_{J+1,1}, \dots, Y_{J+1,T_0}; Z_{J+1}')\}.$$

In practice, it is often the case that no set of weights exists such that these equations hold exactly in the data. Then, the synthetic control observations will be selected so that they hold approximately.

B. Computational Details

The outcome variable of interest, GDP per capita, is observed for T periods for the country affected by the catastrophic event Y_{1t} , ($t = 1, \dots, T$) and the unaffected countries Y_{jt} , ($j = 2, \dots, J + 1$; $t = 1, \dots, T$). Let $T_1 = T - T_0$ be the number of available postdisaster periods. Let Y_1 be the $(T_1 \times 1)$ vector of postdisaster outcomes for the exposed country and Y_0 be the $(T_1 \times J)$ matrix of postdisaster

⁷ We discuss the covariates used in the implementation in section III.

outcomes for the potential control countries. Let the $(T_0 \times 1)$ vector $K = (k_1, \dots, k_{T_0})$ define a linear combination of predisaster outcomes: $\bar{Y}_i^K = \sum_{s=1}^{T_0} k_s Y_{is}$. Consider M of such linear combinations defined by the vectors K_1, \dots, K_M . Let $X_1 = (Z'_1; \bar{Y}_1^{K_1}, \dots, \bar{Y}_1^{K_M})'$ be a $(k \times 1)$ vector of predisaster output linear combinations and output predictors not affected by the disaster for the exposed country, with $k = r+M$. Similarly, let X_0 be a $(k \times J)$ matrix that contains the same variables for the unaffected countries. That is, the j^{th} column of X_0 is $(Z'_j; \bar{Y}_j^{K_1}, \dots, \bar{Y}_j^{K_M})'$.

The vector W^* is chosen to minimize some distance, $\|X_1 - X_0 W\|$, between X_1 and $X_0 W$, subject to $w_2 \geq 0, \dots, w_{J+1} \geq 0$ and $\sum_{j=2}^{J+1} w_j^* = 1$. In particular, we will consider

$$\|X_1 - X_0 W\|_V = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)},$$

where V is a $(k \times k)$ symmetric and positive semidefinite matrix.

Although this inferential procedure is valid for any choice of V , the choice of V influences the mean square error of the estimator (that is, the expectation of $(Y_1 - Y_0 W^*)'(Y_1 - Y_0 W^*)$). The optimal choice of V assigns weights to a linear combination of the variables in X_0 and X_1 to minimize the mean square error of the synthetic control estimator. The choice of V can also be datadriven. One possibility is to choose V such that the resulting synthetic control country approximates the trajectory of the outcome variable of the affected country as well as outcome predictors in the predisaster periods. Indeed, we will choose V such that the mean squared prediction error of the outcome variable is minimized for the preintervention periods. One obvious choice for the set of linear combinations of predisaster outcomes $(\bar{Y}_{i1}^{K_1}, \dots, \bar{Y}_{i1}^{K_M})$ would be

$$\begin{aligned} \bar{Y}_{i1}^{K_1} &= Y_{i1} \\ &\vdots \\ \bar{Y}_{i1}^{K_{T_0}} &= Y_{iT_0}. \end{aligned}$$

This would in essence include the entire predisaster output per capita path as input to build the synthetic control. Alternatively, we can use the first half of the predisaster trend outcomes to match the affected country with the control countries.⁸ That is, $(\bar{Y}_{i1}^{K_1}, \dots, \bar{Y}_{i1}^{K_M})$ would be

$$\begin{aligned} \bar{Y}_{i1}^{K_1} &= \bar{Y}_{i1}^{K_1} = Y_{i,1} \\ &\vdots \\ \bar{Y}_{i1}^{K_M} &= \bar{Y}_{i1}^{K_{\frac{T_0-1}{2}}} = Y_{i,\frac{T_0-1}{2}}. \end{aligned}$$

Indeed, by exploiting only the first half of the predisaster trend to form the synthetic match, we are reserving the second

⁸This period varies across countries, depending on when the disaster occurs relative to the earliest year in our sample.

half for out-of-sample validation. We are then more confident in the ability of the synthetic control to replicate the counterfactual trajectory. In the actual implementation below, we match on the average value of the covariates for the predisaster period and the first half of the predisaster trajectory for GDP per capita.

In this paper, we extend the idea in Abadie et al. (2010) generalizing the placebo approach to produce quantitative inference in comparative case studies. We now discuss how to combine the placebo effects to account for the fact that we will be interested in doing inference about the average (normalized) effect estimated across the country-specific comparative case studies of each disaster.⁹

Recall our lead specific estimates of the disaster on the country of interest (say, country 1) are denoted by $(\hat{\alpha}_{1,T_0+1}, \dots, \hat{\alpha}_{1,T})$ for leads 1, 2, ..., $T - T_0$. Now consider taking the average disaster effect across G disasters of interest, say, the G largest disasters. Assume for simplicity that for all these G disasters, we are able to compute the $T - T_0$ lead specific estimates of disaster impact. Then the estimated average effect for the G largest disasters is given by

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

C. Statistical Significance of Estimated Effects

The standard errors commonly reported in regression-based comparative case studies measure uncertainty about aggregate data. This mode of inference would logically produce 0 standard errors if aggregate data were used for estimation. However, perfect knowledge of the value of aggregate data does not reduce to 0 our uncertainty about the parameter of interest: the effect of a large disaster on output per capita. Not all uncertainty about the value of the estimated parameters comes from lack of knowledge of aggregate data. In comparative case studies such as ours, an additional source of uncertainty derives from our ignorance about the ability of the control group to reproduce the counterfactual. There is some uncertainty about how the affected country would have evolved in the absence of the disaster. Large-sample inferential techniques are not well suited for comparative case studies when the number of units in the comparison group or the number of periods in the sample is relatively small. Following Abadie and Gardeazabal (2003) and Abadie et al. (2010), we use exact inference techniques, similar to permutation tests, to conduct inference in comparative case

⁹We match each country with its synthetic counterpart using the path of GDP per capita. Therefore, the estimated country-specific effect of the disaster is measured as the difference in the actual and counterfactual evolution of GDP per capita. The size of the effect will depend on the level of GDP per capita. The same decline in GDP per capita is more important in a poorer country. Given these scale effects, we need to normalize the estimates before pooling the country-specific results to come up with the average effect of a disaster. We normalize by setting the GDP per capita of the affected country (for each of the disasters we consider) to be equal to 1 in the disaster year.

studies. These methods allow valid inference regardless of the number of available control countries and the number of available predisaster periods. However, the accuracy of inference increases with the number of control countries or the number of available time periods.

As in classical permutation tests, we apply the synthetic control method to every potential control in our sample. This allows us to assess whether the effect estimated by the synthetic control for the country affected by the disaster is large relative to the effect estimated for a country chosen at random (which was not exposed to a large disaster). This inferential exercise is exact in the sense that regardless of the number of available comparison countries and time periods, it is always possible to calculate the exact distribution of the estimated effect of the placebo disasters. More generally, this inferential exercise examines whether the estimated effect of an actual natural disaster is large relative to the distribution of the effects estimated for the countries not exposed to such disasters. More formally, assume that we are doing inference about negative point estimates at every lead (every year in the disaster's aftermath). We can then compute a lead-specific significance level (*p*-value) for the estimated disaster impact as

$$\begin{aligned} p\text{-value}_l &= \Pr(\widehat{\alpha}_{1,l}^{PL} < \widehat{\alpha}_{1,l}) = \frac{\sum_{j=2}^{J+1} I(\widehat{\alpha}_{1,l}^{PL(j)} < \widehat{\alpha}_{1,l})}{\# \text{ of control countries}} \\ &= \frac{\sum_{j=2}^{J+1} I(\widehat{\alpha}_{1,l}^{PL(j)} < \widehat{\alpha}_{1,l})}{J}, \end{aligned}$$

where $\widehat{\alpha}_{1,l}^{PL(j)}$ is the lead l -specific effect of a disaster when control country j is assigned a placebo disaster at the same time as country 1. $\widehat{\alpha}_{1,l}^{PL(j)}$ is computed following the same procedure outlined for $\widehat{\alpha}_{1,l}$. By computing $\widehat{\alpha}_{1,l}^{PL(j)}$ for every country j in the control pool for country 1, we can characterize the distribution of placebo effects and assess how the estimate $\widehat{\alpha}_{1,l}$ ranks in that distribution.

To conduct valid inference for $\bar{\alpha}$ we need to account for the fact that the average smooths out some noise. We then construct a distribution of average placebo effects according to the following steps:

1. For each disaster g of interest, we compute all the placebo effects using the available controls $j_g = 2, \dots, J_g + 1$ corresponding to disaster g .
2. At each lead, we compute every possible placebo average effect by picking a single placebo estimate corresponding to each disaster g and then taking the average across the G placebos. There are many possible placebo averages:

$$\begin{aligned} N_{PL} &= \text{Number of possible placebo averages} \\ &= \prod_{g=1}^G J_g. \end{aligned}$$

We index all these possible placebo averages by $np = 1, \dots, N_{PL}$. This number grows very quickly in G and the typical J_g .

3. We rank the actual lead-specific average disaster effect $\bar{\alpha}_l$ in the distribution of N_{PL} average placebo effects (This involves N_{PL} comparisons.)
4. We compute the lead l specific *p*-value for the average as

$$\begin{aligned} p\text{-value}_l &= \Pr\left(\frac{1}{G} \sum_{g=1}^G \widehat{\alpha}_{g,l}^{PL} < \bar{\alpha}_l\right) \\ &= \Pr(\bar{\alpha}_l^{PL} < \bar{\alpha}_l) \\ &= \frac{\sum_{np=1}^{N_{PL}} I(\bar{\alpha}_l^{PL(np)} < \bar{\alpha}_l)}{\#\text{ of possible placebo averages}} \\ &= \frac{\sum_{np=1}^{N_{PL}} I(\bar{\alpha}_l^{PL(np)} < \bar{\alpha}_l)}{N_{PL}}. \end{aligned}$$

III. Data Description

A. Data Sources

We exploit a comprehensive data set of 196 countries covering the period 1970 to 2008. The data on real GDP per capita at purchasing power parities (PPP) come from the World Bank World Development Indicators (WDI). Following a voluminous empirical growth literature (see, among others, Barro & Sala-i-Martin, 2003, and Mankiw, Romer, & Weil, 1992) and attempting to maximize the preevent fit of the models, the GDP predictors (the vector Z_i in equation [4]) are (a) trade openness (real exports plus real imports over real GDP), from WDI; (b) capital stock computed through the perpetual inventory method using data from the Penn World Tables (PWT);¹⁰ (c) land area (in km²); (d) population; (e) secondary education attainment, from Lutz et al. (2007); (f) latitude (in absolute value); and (g) polity 2, an aggregate indicator of democracy from the Polity IV database described in Marshall and Jaggers (2002).

The data on natural disasters and their human and economic impacts are from the EM-DAT database collected by the Centre for Research on the Epidemiology of Disasters (CRED) at the Catholic University of Louvain. The EM-DAT database has worldwide coverage, and contains data on the occurrence and effects of natural disasters from 1900 to the present.¹¹ CRED defines a disaster as a natural event that overwhelms local capacity, necessitating a request for external assistance. For a disaster to be entered into the EM-DAT database at least one of the following criteria must be fulfilled:

¹⁰ We construct series for capital stock using data from the PWT. Total investment in PPP terms is obtained by multiplying the PPP adjusted investment ratios to GDP (ki) by real GDP per capita ($rgdpl$) and population (pop). Then, following the methodology presented in Easterly and Levine (2001), the perpetual inventory method is used to construct the series for capital stock.

¹¹ The data are publicly available at: <http://www.cred.be/>.

TABLE 1.—DISTRIBUTION OF DISASTER TYPE, 1970–2008

	Disaster	Observations	(%)
Disaster level	Earthquake	816	12.5
	Storm	2,617	40.1
	Flood	3,097	47.4
	Total	6,530	100.0
Country-year level	Earthquake	179	6.9
	Storm	747	28.8
	Flood	996	38.4
	Combined	675	26.0
	Total	2,597	100.0

Authors' calculation based on EM-DAT.

(a) 10 or more people are reported to have been killed; (b) 100 people have been reported affected; (c) a state of emergency is declared; or (d) international assistance is called for. These disasters can be hydrometeorological disasters (floods, wave surges, storms, droughts, landslides, and avalanches), geophysical disasters (earthquakes, tsunamis, and volcanic eruptions), and biological disasters covering epidemics and insect infestations (though these are much less frequent).

The EM-DAT database includes two good measures of the magnitude of the disaster: the number of people killed and the amount of direct damage (measured in U.S. dollars).¹² Since we presume that the impact of a specific natural disaster on the economy depends on the magnitude of the disaster relative to the size of the economy, we standardize the two disaster measures. We divide the number of people killed by the population size in the year prior to the disaster and divide the direct cost measure of the disaster by the previous year's GDP.

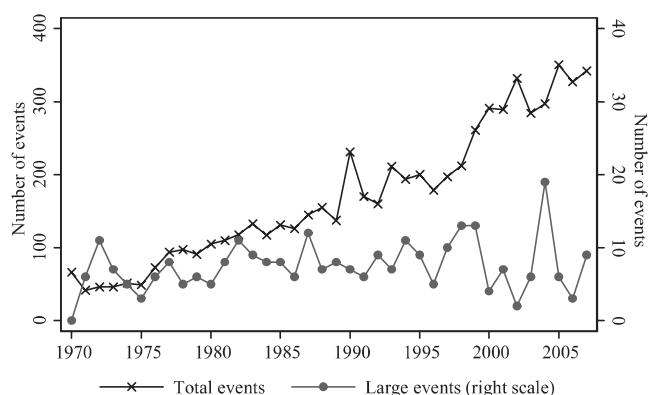
In our econometric analysis in the next section, we rely on the variable "number of people killed," divided by total population, to define the magnitude of the natural disasters. Moreover, we focus primarily on the three types of disasters that are more common and for which the data are more reliable: earthquakes (including tsunamis), floods, and windstorms.

There are 6,530 events recorded in the database between 1970 and 2008, of which 47.4% are floods, 40.1%, are storms, and 12.5% are earthquakes (table 1). Often more than one event is recorded on a given country-year. In those cases, we add up the corresponding disaster magnitudes and define a combined disaster for that country-year. From a first look at the data, disasters are fairly common. Out of a total of ($39 \times 196 = 7,644$) year-country observations, 34% (2,597 observations) meet the requirements to be designated as a natural disaster. These events are distributed between storms (29%), floods (38%), and "combined" (26%). Earthquakes are much less frequent (7% of the country-year observations).

Moreover, as can be seen in figure 1, there is a positive trend in the prevalence of total events over the sample period. However, this trend is somewhat deceptive as it appears

¹² The amount of damage reported in the database consists only of direct damages (for example, damage to infrastructure, crops, housing) and does not include indirect or secondary damages.

FIGURE 1.—INCREASING PREVALENCE OF NATURAL DISASTERS 1970–2007



Large events are those whose intensity is above the mean of the normalized killed distribution. Authors' calculations based on EM-DAT.

TABLE 2.—DISTRIBUTION OF DISASTER TYPE, LARGE EVENTS, 1970–2008

	Disaster	Observations	(%)
Disaster level	Earthquake	75	26.0
	Storm	131	45.3
	Flood	83	28.7
	Total	289	100.0
Country-year level	Earthquake	24	18.6
	Storm	63	48.8
	Flood	21	16.3
	Combined	21	16.3
	Total	129	100.0

Large events are those whose intensity is above the mean of the respective normalized distribution of number of people killed. Authors' calculation based on EM-DAT.

to be driven by improved recording of mild events rather than by an increase in the frequency of occurrence of total events.¹³ Furthermore, truly large—and, conceivably more catastrophic—events are rare. Both of these facts are shown in figure 1 and table 2, where we restrict the sample to large events and where "large" is defined in relation to the world mean of direct damage caused by natural disasters.¹⁴ As it is evident from figure 1, there is no time trend for the subset of large events. Moreover, the frequency of occurrence of large disasters is significantly smaller than that of all events (right versus left scales in figure 1). This suggests a high incidence of small disasters in the sample or, more precisely, that the threshold for what constitutes a disaster (and hence gets recorded in the data set) is quite lenient.

It is important to notice that many of the events that are recorded in the data set do not correspond to the catastrophic notion of natural disaster that one has in mind when thinking about the potential effect of natural disasters on the macroeconomy. Therefore we will be focusing on disasters whose magnitudes are particularly large according to some precise thresholds that we define below.

¹³ See Cavallo and Noy (2011) for a discussion of this issue.

¹⁴ Here, a "large" disaster occurs when its incidence, measured in terms of people killed as a share of population, is greater than the world pooled mean for the entire sample period.

B. Defining Large Disasters

Our treatment effects methodology requires us to have a binary treatment indicator for the occurrence of a disaster. As a first approximation, we could define a large disaster as one in which the magnitude is more than, for example, 2 standard deviations above the country-specific mean. Note, however, that we are interested in large disasters where “large” is defined from a worldwide perspective. While a given disaster might be large relative to the history of disasters within the country, it may be small in a more global context. Then it is better to define a large disaster using the pooled worldwide mean. In this case, a disaster would be large when its magnitude exceeds 2 standard deviations above the world mean.¹⁵ In figure 2 we present the distribution of disaster magnitudes.

Since the distribution is so skewed, the mean (plus 1 or 2 standard deviations) is a poor indicator of location, so we use a percentile-based definition of “large disaster.” Thus, we consider the 99th, 90th, and 75th percentiles of the world distribution of the number of people killed (as a share of population) as cutoff values to define a large disaster.¹⁶ We will use this variable for reasons that we explain below. The 99th cutoff is equivalent to a natural disaster that kills more than 233 people per 1 million inhabitants. Although the number is large, many recent large events exceed this rate. For example, the 2004 Indian Ocean tsunami killed 772 people per 1 million inhabitants in Indonesia, and almost 2000 per 1 million inhabitants in Sri Lanka. Moreover, by the latest accounts, the 2010 earthquake in Haiti killed over 20,000 people per 1 million inhabitants (see Cavallo, Powell, & Becerra, 2010). The 90th cutoff is equivalent to a natural disaster that kills approximately 17 people per 1 million inhabitants. For example, this is within the estimated mortality range of the 2010 earthquake in Chile. Finally, the 75th cutoff corresponds to a natural disaster that would kill approximately 7 people per 1 million inhabitants. This is approximately the mortality rate of Hurricane Katrina that struck the United States in 2005.

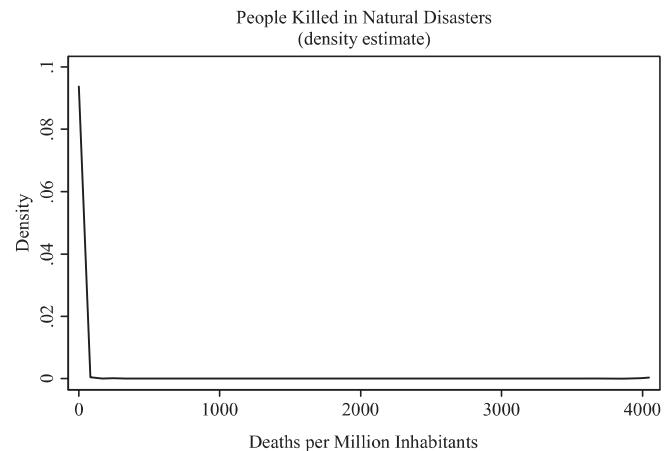
Moreover, the methodology we use requires that we can trace the evolution of the outcome variable for several years after the event. For that reason, we limit the sample to disasters that occur before the year 2000. Taking this into consideration, we end up with subsamples of 8 natural disasters that are large based on the 99th percentile, 164 natural disasters based on the 90th percentile, and 444 natural disasters based on the 75th percentile cutoffs, respectively.

However, we do not have full data on the GDP per capita predictors for all these events, and we were not able to construct valid counterfactuals for all the natural disasters in our

¹⁵ Heger, Julca, and Paddison (2008) report that disasters affect very small countries (especially small island states) disproportionately, but our concern is the generalizability of any of our results. We therefore follow the practice in the cross-country growth literature that excludes very small countries (with population less than 1 million) from cross-country estimations.

¹⁶ For each definition of “large disaster”, the pool of countries available to form the synthetic control is redefined accordingly. The pool of control countries includes those that did not experience any large disaster, including countries that experience disasters of lesser magnitude.

FIGURE 2.—DISTRIBUTION OF DISASTERS MAGNITUDES



Authors' calculations based on EM-DAT and WDI databases.

sample (there are natural disasters for which we could not match the preevent GDP trajectory to that of a synthetic control group).¹⁷ Thus, the effective number of events in every subsample ends up being smaller. In particular, we end up with 4 events that are large based on the 99th percentile, 18 events based on the 90th percentile, and 22 events based on the 75th percentile cutoffs respectively. (See table A1 in the online appendix for the list of events in each category.) Finally, note that for some countries, we have several large disasters over the sample period. In those cases, we use data only before and after (up to the subsequent disaster) the first large disaster observed during the sample period.¹⁸

Obviously, the disaster magnitude as reported in the data set is a combination of the physical intensity of the underlying event with the economic conditions of the affected countries. Nevertheless, in our view, that is the best estimate of the magnitude of the shock to the economy, and hence the potential causing variable in our study.

Still, it is interesting to examine the correlation of the two main magnitude variables used in the literature with pure physical measures of disaster intensity such as the Richter scale for earthquakes and wind speed for storms. Unfortunately, the disaster intensity data are less readily available so we can perform the analysis for only a limited set of events.¹⁹ Table 3 shows the correlations between these physical measures of disasters and the damage measures for disaster magnitude.

¹⁷ Identification relies heavily on matching the pretreatment secular behavior of the outcome variable of interest. Thus, discarding from the analysis the unmatched events is similar to confining the analysis to the common support when using matching estimators.

¹⁸ Then, when defining large disasters according to the different percentile cutoffs, what qualifies as a first disaster for a highest percentile cutoff does not necessarily coincide with what qualifies as first disaster for a lower percentile cutoff.

¹⁹ Information taken from the database of the National Oceanic and Atmospheric Administration (NOAA). <http://www.noaa.gov/>. Yang (2008) and Strobl (2012) use meteorological records to investigate the economic impacts of hurricanes in specific regions, but these cannot be generalized to other regions or other types of disasters with comprehensive coverage.

TABLE 3.—PHYSICAL AND DAMAGE MEASURES OF DISASTER MAGNITUDE:
DISASTER LEVEL DATA

	Damage over GDP (log)	Killed over Population (log)
Richter scale (log)	4.556*** [1.166]	7.951*** [0.944]
Wind Speed (log)	2.464*** [0.452]	0.965*** [0.241]
Land Area (log)	-1.030*** [0.105]	-0.393*** [0.0862]
Island state dummy	-2.387*** [0.530]	-0.347 [0.456]
Latitude (absolute value)	0.0143 [0.0115]	-0.0503*** [0.0143]
Constant	2.324 [2.425]	-8.839*** [2.765]
<i>R</i> ²	0.320	0.333
<i>N</i>	232	255
	428	375

Robust standard errors in brackets: * $p < 0.10$, ** $p < 0.05$, *** and $p < 0.01$. Authors' calculations based on EM-DAT and WDI datasets.

Overall, population killed by the disaster correlates better with the exogenous natural measures of disasters in the sense of having a higher goodness of fit for both measures. More important, it is a variable more accurately measured, especially in poor countries. Also, the number of people killed is more comparable across countries than value-based measures. Thus, we will stick to it to measure our disaster magnitude variable when selecting a pool of large disasters in the econometric analysis below.

IV. Results

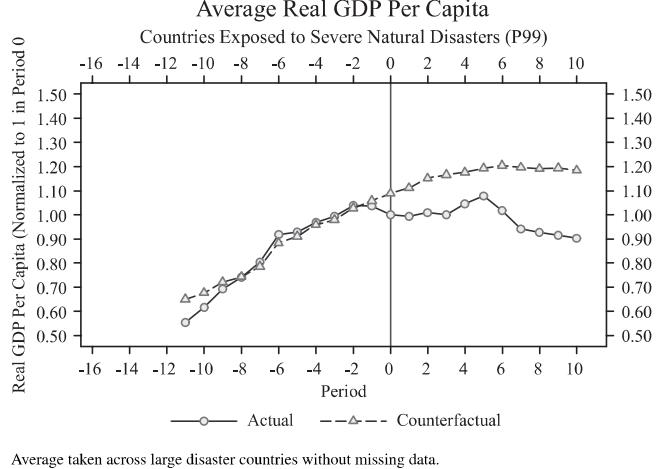
In this section we present our estimates of the average causal impact of large disasters on real GDP per capita for countries that experienced such large disasters between 1970 and 2000 and that have the available data required for a comparative case study. Recall that for those countries that experienced several large disasters, only the first is used, and their postdisaster data are used only up to the year preceding the second large disaster.

A. Overall Effects

As in the program evaluation literature, our estimator does not disentangle between direct and indirect causal effects of the natural disasters on the outcome of interest. It just estimates the overall average causal effect. Though this is always an important distinction, in our case, it is not clear-cut how to draw the line between those effects. Indeed, it might well be argued that all of the total effect of natural disasters on economic growth is indirect. With this caveat in mind, we now present our estimates of the overall average causal effects of natural disasters on economic growth.

Figures 3, 5, and 7 present the average causal impact of a large disaster on real GDP per capita for the three different definitions of “large disaster” adopted: P99, P90, and P75. P“X” for $X = 75, 90$, and 99 denotes the group of countries without missing data that were exposed to disasters in which

FIGURE 3.—LARGE DISASTERS ABOVE 99 PERCENTILE



Average taken across large disaster countries without missing data.

the magnitude of the disaster was above the X^{th} percentile in the world distribution of disaster magnitudes.

As can be seen, large disasters seem to have a lasting impact on GDP per capita when we define a large disaster to be one above the 99th percentile of the magnitude distribution. The effects are sizable. For example, ten years after the disaster, the GDP per capita of the affected countries is (on average) 10% lower than it was at the time of the disaster, whereas it would be about 18% higher in the counterfactual scenario in which the disaster did not occur. Moreover, note that by extrapolating the predisaster trend into postdisaster years to construct the counterfactual, we would be overestimating the effect of the disaster. Table A2 in the online appendix presents the weights associated with the constructed synthetic control country for each of the four countries experiencing the top 1% disasters. Table 5 also reports a low root mean squared prediction error (RMSPE) relative to the average GDP per capita over the period. Figure 4 presents exact inference for the results in the P99 group. When computing placebo averages, we refine our inference approach and include only the averages computed with placebos for which we obtained as good a pretreatment fit as the country that they serve as control for. Thus, this evidence suggests that a natural disaster would cause, on average, a statistically significant decline in GDP per capita all the ten years in its aftermath. The probability of observing such declines by pure chance is close to 0 in every period.

In figure 5, where we define a large disaster using the 90th percentile, we do not find any effect of disasters on output. Actual and counterfactual GDP per capita follow each other closely, not only before but also after the occurrence of the disaster. Whatever slight difference we find between them is not statistically significant at conventional levels (see figure 6).

Again, considering our most lenient definition of large disaster using the 75th percentile (P75) in figure 7, we do not find any effect of disasters on output. As can be seen in figure 8,

FIGURE 4.—ADJUSTED SIGNIFICANCE LEVELS FOR P99

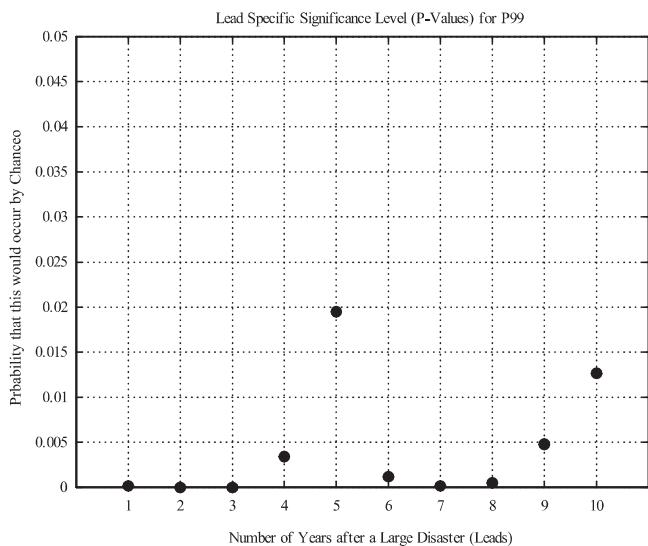
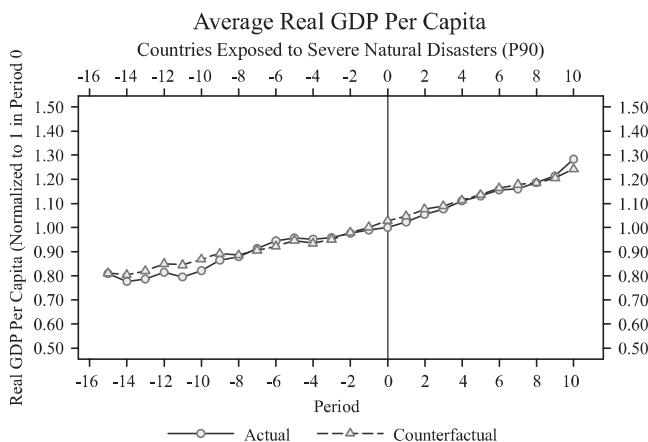


FIGURE 5.—LARGE DISASTERS ABOVE 90 PERCENTILE



Average taken across large disaster countries without missing data.

none of the differences between the actual and counterfactual GDP per capita is statistically significant.²⁰

Taken at face value, these results suggest that only large natural disasters affect, on average, the subsequent performance of the economy. For example, our results can be used to estimate the likely long-term impact of the catastrophic earthquake that struck Haiti on January 12, 2010. By the metric of the number of fatalities as a share of population, the Haiti earthquake is the most catastrophic one in the modern era, killing as many as five times more people per 1 million inhabitants than the worst event in our comprehensive sample (the 1972 earthquake in Nicaragua). If Haiti were to

²⁰ Qualitatively similar results are obtained if we replicate all the analyses in this section using the disaster magnitude measure based on monetary value of damages. For top 1% disasters, the effect is statistically significant only seven years after the disaster. For top 25% disasters, results are in general not significantly different from 0, except at leads 7 and 8. Results available on request.

FIGURE 6.—ADJUSTED SIGNIFICANCE LEVELS FOR P90

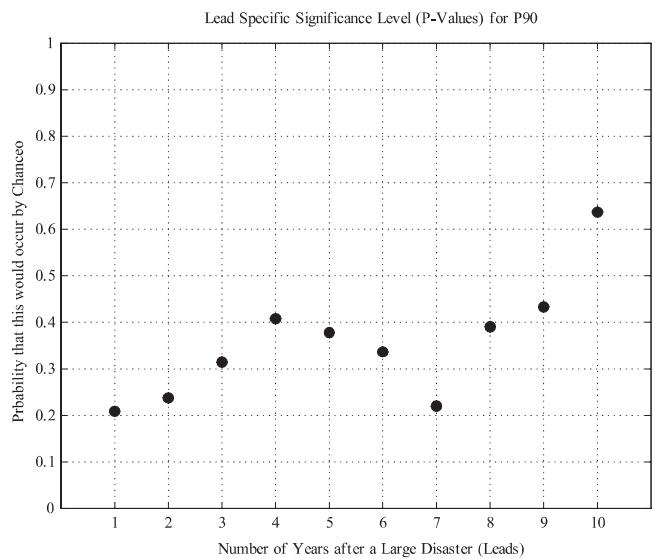
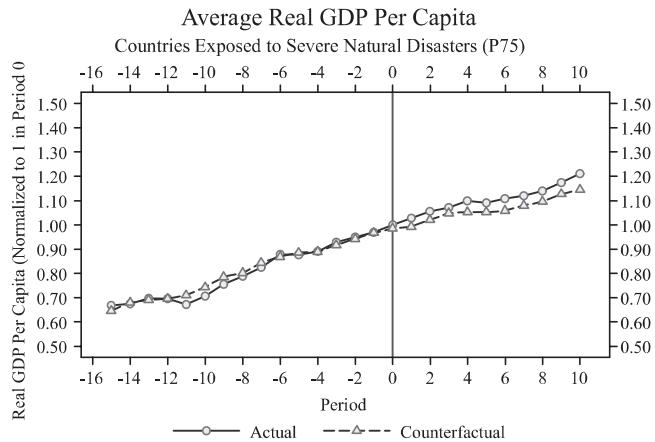


FIGURE 7.—LARGE DISASTERS ABOVE 75 PERCENTILE



Average taken across large disaster countries without missing data.

FIGURE 8.—ADJUSTED SIGNIFICANCE LEVELS FOR P75

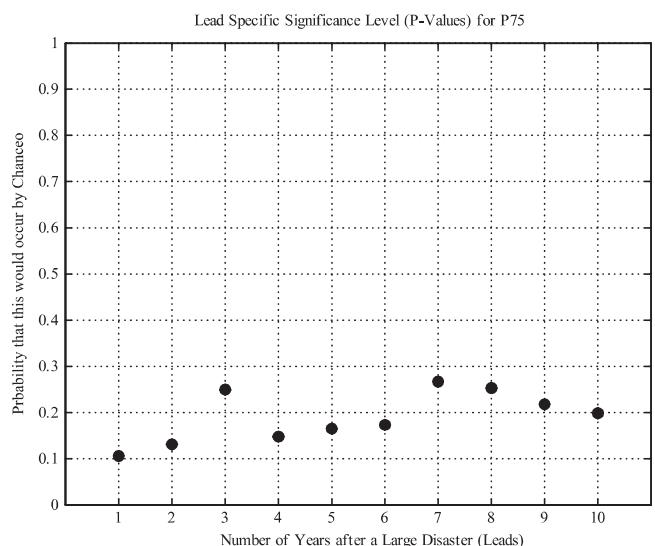
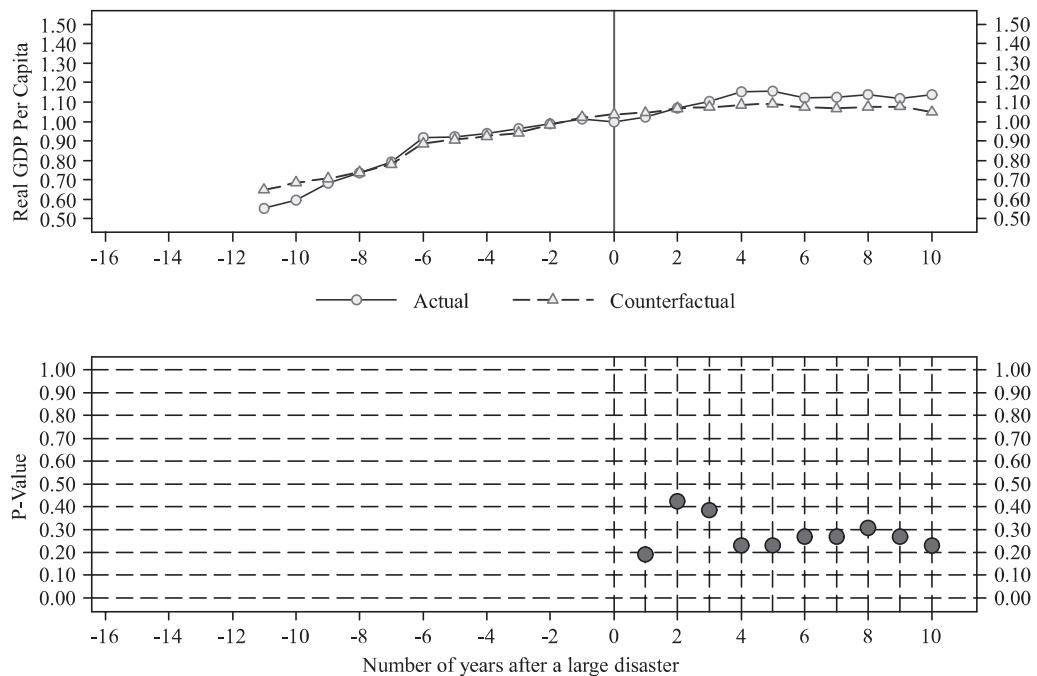


FIGURE 9.—LARGE DISASTERS NOT FOLLOWED BY POLITICAL REVOLUTIONS



experience the average long-term impact of a P99 disaster, we estimate that by 2020, it would have an income per capita of \$1,060, while it could have had a per capita income of about \$1,410 had the earthquake not occurred (all figures in PPP 2008 international dollars). Instead, the devastating earthquake that struck Chile on February 27, 2010, one of the strongest earthquakes ever recorded, is also an informative case to consider. According to information from the Chilean government (as of March 20, 2010), the earthquake killed 342 people out of a population of approximately 17 million (this is within the mortality range of our P90 subsample). By our estimates, such an event is not likely to generate long-term adverse impact on per capita GDP.

Another question that is worth investigating is whether the geographic location of the disaster within the country (center versus periphery, rural versus urban, coastal versus inland) matters for the postdisaster outcomes. One would expect that these geographical details could affect the capacity of the government to implement its reconstruction plans efficiently. For example, the Port-au-Prince earthquake of 2010 also destroyed much institutional capacity and adversely affected the ability of the Haitian government to implement postdisaster plans. Unfortunately, we are unable to answer this question with the data we use in this paper.

B. Effects Controlling for Radical Political Revolutions

Two of the four disasters in the “treated” group of very large disasters (i.e., those defined by the 99th cutoff) were followed by political revolutions. These were the cases of 1979 Islamic Iranian Revolution, which occurred right after the

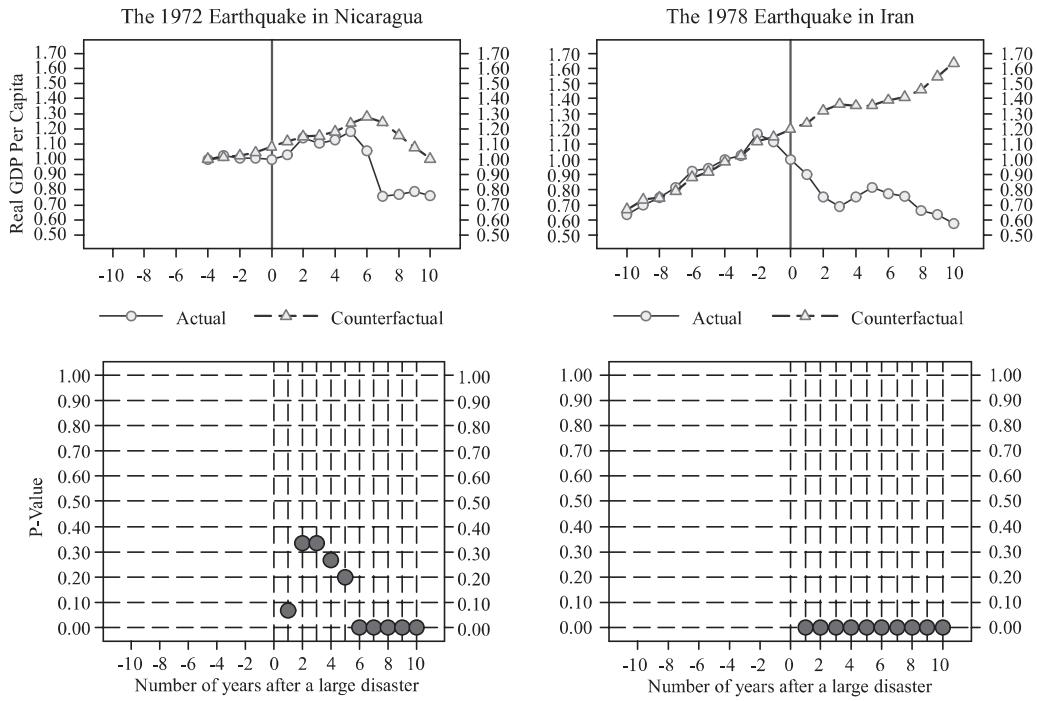
1978 earthquake, and the Sandinista revolution in Nicaragua that deposed the Somoza regime, also in 1979, a few years after the earthquake that devastated Managua. Though it is possibly that these natural disasters somehow affected the likelihood of those radical political revolutions, we cannot substantiate such a causal claim.²¹ Irrespective of that, in the structural spirit of analyzing the effect of the natural disasters on economic growth controlling for the effect of these political revolutions, it is of interest to separate the analysis between the cases where the natural disaster was followed by radical political revolution, as it was in the cases of Iran and Nicaragua, which certainly affected the working of the economy, and those that were not followed by political revolution, such as the cases of Honduras (1974) and the Dominican Republic (1979) (see Table A1 in the online appendix).²²

Figures 9 and 10 present this analysis. In figure 9 we observe that when we restrict the analysis to the subset of large disasters (in the 99th percentile) that were not followed by

²¹ Nevertheless, in the case of Nicaragua, it has been argued that the 1972 earthquake that devastated Managua played a role in the fall of Somoza. Instead of helping to rebuild Managua, Somoza siphoned off relief money to help pay for National Guard luxury homes, while the homeless poor had to make with hastily constructed wooden shacks. This greatly contributed to eroding the remaining support of Somoza’s regime among many businessmen and the middle class (see, among others, Merrill, 1993). In the case of Iran, the earthquake served the organization of the revolution, in particular, by having coordinated the organization of Khomeini’s Revolutionary Guard that played a key role in advancing the revolution activities (see Keddie, 2006).

²² Of course, if the disasters did not cause the political change, the overall average effect previously estimated would be biased upward (in absolute value) due to these subsequent negative shocks correlated with the treatment indicator used in the analysis.

FIGURE 10.—LARGE DISASTERS FOLLOWED BY POLITICAL REVOLUTIONS



by radical political revolutions, we find no effects of the disaster on GDP per capita in either the short or the long run.

In figure 10 we observe large long-lasting effects of a catastrophic disaster when followed by radical political revolutions. As can be seen in the figure, the earthquakes in Nicaragua and Iran produced large and statistically significant effects on output per capita. Note, however, that Nicaragua, after a short-lived (one-year) small but statistically significant decline, was fully recovering from the disaster (in terms of GDP per capita).²³ However, it dropped again, in a much more pronounced way, with the revolution, six to seven years after the disaster. This result confirms once again the salient importance of the political organization of societies in determining their economic performance (see, among others, Acemoglu, Johnson, & Robinson, 2005).

Thus, we find that only very large natural disasters followed by radical political revolution show long-lasting negative economic effects on economic growth. Even very large natural disasters, when not followed by disruptive political reforms that alter the economic system, including the system of property rights, do not display significant effects on economic growth.²⁴

²³ Thus, it is difficult to argue that these two cases display long-term significant effects because they happened to be earthquakes, as opposed to other types of natural disasters.

²⁴ This failure to find a negative long-run impact is similar to research that concluded that major wartime bombing campaigns in Japan and Vietnam had no discernible long-run effects. See Davis and Weinstein (2002) and Miguel and Roland (2011). Excluding Iran and Nicaragua from the analysis for the 90th and 75th cutoff points does not change the analysis significantly.

C. Caveats

As in all the program evaluation literature, the Abadie et al. (2010) synthetic control methodology assumes that the causing factor does not affect the control observations directly or indirectly (stable unit treatment value assumption). While direct effects can be trivially ruled out in all the cases studied, it may be that the disaster had an indirect effect on other countries, especially if the affected country is large and if the control group is composed mostly of close trading partners. Though this is difficult to be ruled out entirely in all the cases studied, it is unlikely to constitute a first-order amelioration effect. For this to happen, it has to be the case that the output possibility frontier of a country would be affected substantially by the demand from another country, something economists do not believe plausible, especially in the midterm. In any event, for most of the cases we study, the control groups are generally composed of several countries, most of which are unlikely to be the main trade partner of the country affected.

Two additional drawbacks of the synthetic control methodology, which could be extended to all nonexperimental reduced-form causal analysis, need to be acknowledged. First, as it is always the case in the absence of experimental variation, quasi-experimental identification is not possible without identification assumptions. It might well be that other big events that occur simultaneously or after the disaster took place end up driving the results. Indeed, in the two cases where we found a significant overall effect on GDP per capita, we also identified other subsequent large shocks (the Islamic revolution in Iran and the regime change in

Nicaragua) as a potentially important contributing factor of the reported effect. Indeed, based on all our evidence, we argue that it is plausible that natural disasters, even large ones, had no long-term effect at all on economic growth. Second, the synthetic control methodology estimates only the reduced-form aggregate impact of the catastrophe on the outcome of interest, without identifying the channels of transmission, which could include the presence of effective reconstruction policies, possibly resulting from international aid. This should be borne in mind when interpreting our results.

V. Conclusion

We examined the impact of natural disasters on GDP per capita by combining information from comparative case studies obtained with a synthetic control methodology recently developed in Abadie et al. (2010). The procedure involves identifying the causal effects by comparing the actual evolution of postdisaster per capita incomes with a counterfactual series constructed by using synthetic controls.

Our estimates provide new evidence on the short- and long-run per capita income effects of large natural disasters. Contrary to previous work, we find that natural disasters, even when we focus on only the effects of the largest natural disasters, do not have any significant effect on subsequent economic growth. Indeed, the only two cases where we found that truly large natural disasters were followed by an important decline in GDP per capita were cases where the natural disaster was followed, though in one case not immediately, by radical political revolution, which severely affected the institutional organization of society. Thus, given this evidence, we conclude that unless a natural disaster triggers a radical political revolution; it is unlikely to affect economic growth. Of course, this conclusion does not neglect the direct cost of natural disasters such as the lives lost and the costs of reconstruction that are often quite large.

Given the absence of long-run impacts on per capita incomes, it is likely that the disaster's effect is mostly felt through reduced consumption that finances reconstruction or long-run higher levels of indebtedness, though examining these conjectures is outside the scope of our study.

Blanchard and Katz (1992) have shown that the long-run adjustment to regional shocks in the United States usually occurs through migration rather than through changes in incomes or employment. Our failure to find any long-term impacts on per capita incomes is clearly consistent with their findings, though international migration data for the cases we examine are unavailable.

Finally, our results are informative about the average long-term costs of natural disasters and can also be useful to other literatures, such as those attempting to quantify the likely costs of any future climate change and evaluating various climate-change mitigation policies.

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