Economic Development in Extreme Environments

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Submitted in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy
in the Graduate School of Arts and Sciences

COLUMBIA UNIVERSITY

2014

Abstract

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The role of the environment in the development process is frequently framed as a one-way interaction, with humans decoupling from natural systems, and often damaging them in the process. When the environment is granted an influential role, it is often in establishing the initial conditions under which development will take place, for example, through natural resource endowments or climate factors. However, in some extreme environments, it may be responsible for not only the initial opportunities available in a society, but also for continuously shaping those opportunities through time. The field of Sustainable Development is fundamentally concerned with this two-way interaction between the environment and society, recognizing both as part of a coupled system. The chapters in this volume demonstrate some of the costs associated with development in an extreme environment using methods from climate science, ecology, remote sensing, and economics. By looking at places exposed to tropical cyclones, to persistent pollution resulting from fires that burn readily in a drought-prone location, to annual floods that frequently and randomly strike households in a country, we see that the environment critically shapes aspects of societies and their economic opportunities. By no means are all opportunities dictated by the environment. However, these chapters robustly illustrate that the environment imposes some critical boundaries on development in extreme environments and policies aiming to increase welfare must take account of the coupling of social and natural systems.

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Acknowledgments

First and foremost I would like to thank those who have led the Sustainable Development Ph.D. since I have joined: Doug Almond for help and astute advice along the way; Mona Khalidi for her endless support and patience; and most of all to John Mutter, who has provided me with wisdom and kind advice since before I joined the Ph.D.. For supporting my research, I thank Columbia University.

I am deeply grateful to my committee, my teachers, and my advisors. It is the nature of this unique Ph.D. program that I am able to count a great number among this list. I am fortunate to have met many faculty who have taken the time to offer me advice and feedback. I would particularly like to thank Max Auffhammer, Scott Barrett, Mark Cane, Pietro Ceccato, Ruth DeFries, Lisa Goddard, Raymond Guiteras, Macartan Humphries, Upmanu Lall, Malgosia Madajewicz, Ron Miller, Vijay Modi, Matt Neidell, Kristian Pop-Eleches, Ben Orlove, Jeffrey Sachs, Wolfram Schlenker, Chris Small, Pablo Suarez, Tara Troy, and Maria Uriarte.

The students in the Sustainable Development Ph.D. program have been a constant source of inspiration, encouragement, and learning over the past five years. They have been mentors and peers, and led by example. I would particularly like to thank Jesse Anttila-Hughes, Prabhat Barnwal, Xiaojia Bao, Denyse Dookie, Ram Fishman, Booyuel Kim, Kimberly Lai, Kyle Meng, Gordon McCord, James Rising, Tse-Ling Teh, Anna Tompsett and Jan von der Goltz. I must single out Solomon Hsiang, in particular, for the role he has played in helping me to find my feet with my research, and for forcing me to aim for only the highest standards.

To my friends beyond Sustainable Development, I must mention Somayya Ali, Matthew Fagan, Mary Heskel, Arezou Paksima, Christina Seybolt, Amy Spofford, Cynthia Thomson, and Kate Weinberger. I am grateful to Anja Tolonen, not least for her exceptional help and input while I have put this dissertation together. Meha Jain and Miriam Marlier deserve special thanks, as my research has benefited immensely from our long collaborations.

And finally, I would be nowhere without the support and love of my family.

Dedicated to my parents:

to the memory of my Father, Sultan, who gave me science;

and to my Mother, Barbara, who gave me art.

Chapter 1

Introduction

1.1 Overview

The extent to which the environment shapes society has long been a fundamental area of interest for humanity. In the past, ideas of strong environmental determinism would have had us believe that society was at the whims of a sometimes friendly and sometimes hostile environment. In the past few years, we have developed empirical techniques which allow us to see that the interaction of the environment and society is more subtle and nuanced than previously understood.

Prior to this, many attempts were made to estimate some form of the following equation

$$Y_i = f(env_i) \tag{1.1.1}$$

which attempts to identify the influence of some function of the environment upon a social outcome, Y_i. This resulted in a debate which broadly characterized the influence of environment upon development as one of two opposing views—one in which it affected initial conditions, but otherwise had no continuing effects, and one in which it drove much of the pattern of the wealth of nations.

This thesis in large part is dedicated to demonstrating a contrary viewpoint to this reductive debate. I claim that the environment plays a limited but deterministic role in defining the growth opportunities of societies, which is an important contributing factor to development. The chapters below provide evidence that environment matters for development, not just through establishing the initial conditions (Acemoglu et al., 2001), but through continually affecting the economic opportunities available to a society. This influence could be termed “weak environmental determinism”, as I make no claim that the environment leads to a complete inability for a society to develop. Rather, this work demonstrates that the environment, both through initial endowments and through continual exposures, needs to be considered in any development policy.

In these chapters, I use the techniques of climate science and remote sensing, combined with applied microeconomics to empirically identify impacts that constitute an ongoing burden or bound upon development in extreme environments. Moreover, I aim to look at long-term impacts where possible, to understand the extent to which the environment may be causally linked to distant and economically important outcomes for a society.

A general approach that unites this dissertation is the use of physically derived measures of environmental exposure. This addresses a fundamental problem with the literature to date on environmental impacts: that of endogeneity of right-hand side variables previously used. In each of the following projects, I utilize an index of disaster magnitude for three separate classes of natural hazard—floods, fires, and storms. The results from all three justify this approach and, furthermore, have a broader statement to make about the use of the natural sciences within social science. The startling result presented in chapter 2 of this prospectus is made possible because of the efforts of countless scientists working to document and catalog physical phenomena over time. The papers in the subsequent two chapters do not have the luxury of 60 years of historical physical data to look back upon. Yet they represent ongoing efforts at long-term data collection that are now in a state of comparative infancy. While the exact relationships I aim to identify here might change decades into the future when enough historical data are available, I think that the approach towards understanding them will only increase in importance over time.

1.2 Chapter summaries

A training in Sustainable Development encompasses many fields. All of the papers contained herein represent the values of an education in Sustainable Development, demonstrating a critical engagement and understanding of earth sciences, ecology, and geography, and combining these with the empirical techniques of empirical applied economics to estimate new relationships of both social and policy importance.

Chapter 2: The Causal Effect of Environmental Catastrophe on Long-Run Economic Growth: Evidence From 6,700 Cyclones. In Chapter 2, Solomon Hsiang and I investigate whether tropical cyclones have a long-term effect upon national economic growth. Beginning from the four hypotheses of the long-run dynamics following catastrophic shocks that are prevalent in the literature, we confirm the hypothesis of "no recovery" for tropical cyclone strikes. We do so by reconstructing a physical history from 1950-2008 of tropical cyclone intensity based on climate physics and examining the growth dynamics of countries in the years following a strike. Using panel data, we are able to exploit the random nature of the cyclone tracks, as well as restrict our inquiry to follow single countries through time. We show that national incomes decline relative to the pre-disaster trend, and remain below the trend for up to two decades. This result is remarkably generalizable, being exhibited in countries regardless of size and initial income, and in small island states. We find evidence that countries with less historical experience of tropical cyclones are less adapted, suffering larger losses in the event of a strike. Income loss arises from a small suppression of growth in the years following a tropical cyclone, and result in cumulatively large losses: a global 90^{th} percentile event effectively undoes 3.7 years of development on average. We then conduct "cyclone-free" counterfactual simulations, and observe the extent to which cross-sectional patterns of development can be explained by this environmental exposure. Finally, we link this impact to climate change projections and show the costs of business-as-usual climate change.

Chapter 3: Long-range Externalities and Human Capital: Evidence from atmospheric models and Indonesian megafires. In Chapter 3 Miriam Marlier and I examine the effects of environmental degradation in the tropics upon human capital development. We specifically look at impacts of exposure to wildfire pollution in Indonesia. Fires are lit for agricultural land-clearing, but the negative health externality is experienced by people all over the country. As in most developing countries, instrumental records of pollutants are largely non-existent, posing a challenge to public health due to a lack of information for effective policy-making. One approach has been to use satellite measures of pollution as a substitute for ground data. We overcome this challenge by using a novel dataset of air pollution modeled in a general circulation climate model. This affords us three advantages over previous studies that have used satellites: namely, we can isolate surface exposure, we can distinguish between pollutants, and we have a continuous historical record of exposure. We hypothesize that this environmental degradation will have persistent costs to society through effects on human capital formation. We find that children exposed to high levels of air pollution *in utero* show decreased physical development, similar to the effects of chronic malnutrition. This is true even in those districts far removed from the fire sources. The long-distances and long lag times before effects are observed imply that the true value of improvements to environmental quality in Indonesia is greater than the currently revealed value. In addition to this, pollution affects cognition contemporaneously, which has the potential to lead to labor market and educational effects. Further work is required to understand the effects on the national economy.

Chapter 4: The Impacts of Flooding on Human Capital Formation in Bangladesh In Chapter 4, Raymond Guiteras, Mushfiq Mobarak, and I look at the human capital effects of flooding in Bangladesh. We aim to understand medium- and long-term impacts associated with exposure to flooding in early life. As no comprehensive, spatially-explicit record of flooding history exists for Bangladesh, we create a measure of flood extent using remote sensing. This allows us to create a physically derived, exogenous measure of environmental exposure. Looking at child development, we see that a child exposed to floods *in utero* has an increased likelihood of stunting compared to children who were not exposed. We also find evidence of adaptation—households that are routinely exposed to larger or more frequent floods experience smaller impacts in the event of an abnormal flood than those who are exposed less often. Like the environmental exposure discussed in chapter 3, flooding is a common and often routine part of life in Bangladesh. However, given the large covariate nature of the shocks, it is likely that large sections of society may be affected *en masse* in the event of a flood, collectively lowering economic well-being, but not altering relative well-being within local societies. This chapter provides some indication of the extent to which the relatively extreme hydrological conditions in Bangladesh might be imposing some unrecognized limits on development opportunities.

Chapter 5: An Empirical Approach to Estimating the Climate Change Damage Function Currently we are experiencing a golden age in both empirical environmental and microeconomic research. Moreover, there is an exciting and burgeoning field merging at their intersection. A few attempts have been made to summarize this field (Dell et al., 2013; Greenstone and Jack, 2013) but few have been made to synthesize it. In Chapter 5, I discuss one of the first and most unique efforts to do so, bringing together work in climate science and economics to estimate the climate change "damage function" - the damages upon economic outcomes due to changes in global temperature. This chapter introduces and then extends the econometric analysis at the heart of the American Climate Prospectus, the first empirically-based and spatially-explicit economic risk assessment of climate change in the United States (Houser et al., 2014). It describes the process of bringing together the best estimates from recent applied microeconomics papers in climate and economics, in combination with highly-detailed climate projection output, to empirically estimate the full probability distributions of climate impacts in the United States. Beyond the contribution of bringing together this work, the current chapter extends the analysis to derive empirical damage functions for four economic sectors. This relationship is one of key importance in the climate assessment literature, and has to date been derived from researcher intuition rather than from data (Stern, 2013). This project, part of an ongoing larger effort, promises to fundamentally alter the way we understand the relationship between our economies and the climate.

Chapter 2

The Causal Effect of Environmental Catastrophe on Long-Run Economic Growth: Evidence From 6,700 Cyclones

Solomon M. Hsiang and Amir S. Jina [1]

[1]This chapter is available as NBER Working Paper No. 20352, August 2014.

Abstract

Does the environment have a causal effect on economic development? Using meteorological data, we reconstruct every country's exposure to the universe of tropical cyclones during 1950-2008. We exploit random within-country year-to-year variation in cyclone strikes to identify the causal effect of environmental disasters on long-run growth. We compare each country's growth rate to itself in the years immediately before and after exposure, accounting for the distribution of cyclones in preceding years. The data reject hypotheses that disasters stimulate growth or that short-run losses disappear following migrations or transfers of wealth. Instead, we find robust evidence that national incomes decline, relative to their pre-disaster trend, and do not recover within twenty years. Both rich and poor countries exhibit this response, with losses magnified in countries with less historical cyclone experience. Income losses arise from a small but persistent suppression of annual growth rates spread across the fifteen years following disaster, generating large and significant cumulative effects: a 90th percentile event reduces per capita incomes by 7.4% two decades later, effectively undoing 3.7 years of average development. The gradual nature of these losses render them inconspicuous to a casual observer, however simulations indicate that they have dramatic influence over the long-run development of countries that are endowed with regular or continuous exposure to disaster. Linking these results to projections of future cyclone activity, we estimate that under conservative discounting assumptions the present discounted cost of "business as usual" climate change is roughly $9.7 trillion larger than previously thought.

2.1 Introduction

The influence of environmental conditions on global patterns of economic development is the subject of continuing debate, primarily because identifying these causal effects is challenging. We examine how a specific type of environmental disaster, tropical cyclones, affect countries' growth in the long-run. We construct a novel data set of all countries' exposure to all cyclones on the planet using ground-, ship-, aerial-, and satellite-based meteorological observations combined with information on cyclone physics. We exploit natural random variation in the formation, path, and intensity of each storm as a source of exogenous within-country variation in disaster exposure, allowing us to identify cyclones' long-run impact on economic growth. Applying a difference-in-differences approach, we compare each country's growth rate to itself in the years immediately before and after exposure while accounting for the distribution of lagged effects imposed by cyclones strikes in preceding years. We obtain estimates that are both economically large and statistically precise: each additional meter per second[2] of annual nationally-averaged wind exposure lowers per capita economic output 0.37% twenty years later. When we explore the generalizability of this result, we find that it is "globally valid" in the sense that it holds around the world, appearing in each region independently and for countries of different income and geographic size.

The structure and impact of short-run macroeconomic disasters has been carefully studied (e.g. Barro (2006); Jones and Olken (2008); Gabaix (2012)) and recent empirical work has begun to identifying the long-run growth effects of specific shocks, such as currency crises, banking crises, political crises and civil wars (Cerra and Saxena (2008)), financial crises (Reinhart and Rogoff (2009)), tax increases (Romer and Romer (2010)), and changes in temperature (Dell, Jones and Olken (2012)). By assembling the first objective and comprehensive history of cyclone exposure, we build on these earlier results to provide the first global estimates of the effect of large-scale environmental disaster on long-run growth. The economic response to environmental disaster shares many features with the response to these previously studied shocks, in particular all of these shocks have negative long-run effects on income. In Table 2.1 we compare the magnitude and duration of these effects on income, including cyclone impacts from this study. The national income loss associated with a one standard deviation cyclone event is comparable in magnitude to loss associated with a tax increase equal to 1% of GDP, a currency crisis, or a political crises in which executive constraints are weakened. The

[2] 1 m/s = 3.6 km per hour $\approx$ 2.24 miles per hour.

Table 2.1: Effects of cyclones and other shocks to income per capita

Event Type	**Effect on Income**	**Observed After**	**In-Sample Probability**
Temperature increase (+1°C)*[1]	−1.0%	10 yrs	6.4%
Civil war[2]	−3.0%	10 yrs	6.3%
Tax increase (+1% GDP)**[3]	−3.1%	4 yrs	†16.8%
1 standard deviation cyclone	**−3.6%**	**20 yrs**	**14.4%**
Currency crisis[2]	−4.0%	10 yrs	34.7%
Weakening executive constraints [2]	−4.0%	10 yrs	3.7%
90th percentile cyclone	**−7.4%**	**20 yrs**	**5.8%**
Banking crisis[2]	−7.5%	10 yrs	15.7%
Financial crisis[4]	−9.0%	2 yrs	<0.1%
99th percentile cyclone	**−14.9%**	**20 yrs**	**0.6%**

*Poor countries only. **USA only. †Number of quarters with any tax change.

[1]Dell, Jones & Olken (AEJ: Macro, 2012), [2]Cerra & Saxena (AER, 2008), [3]Romer & Romer (AER, 2010), [4]Reinhart & Rogoff (AER, 2009)

income loss associated with a 90th-percentile cyclone event is comparable to losses from a banking crisis. The top percentile of cyclone events have losses that are larger and endure longer than any of these previously studied shocks. These results suggest that in addition to human-caused political and financial crises, large-scale natural environmental disasters play a important role in shaping patterns of global economic activity.

A key feature of the macroeconomic response to cyclones is that incomes do not recover in the long-run, defined here as the twenty years after a storm. This fact has profound implications. Unlike relatively rare financial crises, political crises, and civil wars, cyclones occur regularly and repeatedly, often striking the same population as prior events because the location of storms are determined by geophysical constraints. Because incomes do not recover after a cyclone, repeatedly exposing the same population to frequent storms results in an accumulation of income losses over time, effectively lowering that population's average growth rate relative to a cyclone-free counterfactual. Mathematically, the effect of increasing the average exposure to cyclones is very similar to increasing the rate of capital depreciation in standard growth models. Quantitatively, the in-sample probability of cyclones is most similar to that of banking crises (Table 2.1), which also slow growth when they occur repeatedly in the same country.

This result informs two important literatures. First, the role of geography in economic growth has been widely debated, with some authors suggesting that geographic condition may matter because they determine the "initial conditions" of an economy by affecting its institutions (Acemoglu, Johnson and Robinson (2002), Rodrik, Subramanian, and Trebbi (2004)) while other authors suggest that geographic conditions determine the "boundary conditions" of an economy throughout its development, perhaps by affecting the health of a population (Gallup, Sachs and Mellinger (1999); Kremer and Miguel (2004)) or the costs of trade (Frankel and Romer (1999)). Our results do not reject any of these theories, but they do provide empirical evidence that repeated exposure to cyclones is a specific boundary condition to development that, alongside institutional and capital factors, may be quantitatively important in certain contexts.

Second, the economic impact and optimal management of global climate change is heavily researched with strong theoretical foundations (Nordhaus (1996); Stern (2008); Weitzman (2009); Tol (2009); Heal (2009)) but less satisfying empirical grounding (Pindyck (2013)). Prior work has focused on temperature's effect on agriculture (e.g. Schlenker and Roberts (2009)), health (e.g. Deschênes, Greenstone, and Guryan (2009)), labor (e.g. Graff Zivin and Neidell (2014)), energy (e.g. Deschênes and Greenstone (2011)), social conflict (e.g. Hsiang, Burke, and Miguel (2013b)), and growth generally (e.g. Dell, Jones and Olken (2012)). Yet, the growth impact of tropical cyclones has not been considered in previous assessments of climate change. It is expected that the frequency and intensity of cyclones will change in response to climate change (Knutson et al. (2010b); Camargo and Hsiang (2014)), which our results indicate may have important economic consequences.

To identify the growth effect of tropical cyclones, we exploit random, within-country, year-to-year variation in the formation, path, and intensity of cyclones that is driven by stochastic ocean and atmospheric conditions. We apply the difference-in-differences approach developed by Deschênes and Greenstone (2007) whereby we identify the effect of storms using the residual variations in both cyclone exposure and growth that remain after country fixed effects, country-specific trends, and year fixed effects have absorbed average cross-sectional correlations and trends in both variables.

By including our physical measures of cyclone exposure in a flexible and robust model of growth, we are able to recover the within-country long-run effect of cyclones with precision. We find that GDP growth rates are depressed for the fifteen years that follow a cyclone strike, causing the trajectory of long-run income to diverge significantly from its pre-disaster trend. Within the twenty years following a cyclone there is no rebound in growth, so affected national incomes remain permanently lower than their disaster-free counterfactual. Our conclusion that no recovery occurs is robust, passing numerous specification and data checks. Furthermore, this result is strikingly general since we obtain similar estimates for marginal effects independently in each major cyclone region, in response to both large and small cyclone events, in countries of high and low income, and in countries of all different sizes. Our interpretation that these effects are causal is strengthened by a series of randomization procedures where we demonstrate that assigning the exact timing of specific cyclone events to correct countries is essential for obtaining our result—it is extremely unlikely that these findings could be a spurious artifact of global cross-sectional correlations or trends in growth. Furthermore, the long-run response of alternative macroeconomic measures corroborate this central finding. Interestingly, we find evidence that the effects of cyclones are largest in countries with less historical cyclone experience and smaller in more experienced countries. We interpret this finding as evidence that frequently exposed populations adapt to their local cyclone-climate by undertaking costly investments that partially insulate their economies from cyclones (Hsiang and Narita (2012)).

The effect of cyclones on growth is both large and persistent, causing it to exert substantial influence over global patterns of economic development. A one standard deviation in a year's cyclone exposure lowers GDP by 3.6 percentage points twenty years later, setting an average country back by almost two years of growth. For countries that are infrequently exposed to cyclones, this effect has only minor long-run implications as an average country's GDP is likely to grow by 50 percentage points during that period. However, tropical cyclone climates are a geographic feature of countries that are determined by oceanic and atmospheric patterns, so some countries are endowed with substantially higher levels of exposure than others. Because the effects of cyclone strikes do not fade with time, those countries that are repeatedly exposed to cyclones suffer from an income penalty that grows with each event. Thus, a cyclone-prone climate lowers a country's long-term growth rate substantially; however, because the onset of cyclone-induced losses is gradual, there is no obvious feature in its GDP series that a casual observer would be likely to notice.

To develop a sense of how important cyclones might be for determining global patterns of long-run growth, we simulate "counterfactual" GDP series where the effect of each country's cyclone history is artificially removed. While this approach generates only a coarse partial-equilibrium estimate for a cyclone-climate's total long-run effect, our simulations indicate that regular disaster exposure plays a major role in determining national income growth in regions where these storms are frequent since the cyclone-climate of many countries cost them several percentage points in their average annual growth rate. Within heavily exposed regions, we find that these simulated losses to cyclones explain roughly a quarter of the cross-country variation in long-run growth. For example, our results predict that the cyclone climates of China and the Philippines (neighbors separated by only 380 miles) generate a

6.2 percentage point difference in their average annual growth rates, when the observed difference in actual growth is 5.6 percentage points. Aggregating these simulation results globally, we estimate that the 4,174 cyclone-by-country events that occurred between 1950-2008 had the total effect of slowing the annual growth rate of World GDP by roughly 1.27% during the period 1970-2008. All of these simulation results should be interpreted with caution, as it is of course impossible to directly test if these estimated effects would manifest should all cyclones disappear from the planet—but they nonetheless force us to carefully consider the potential centrality of environmental disasters in determining both the distribution and quantity of global wealth.

We conclude by evaluating how these results alter our understanding of the social cost of anthropogenic climate change. We first develop a theoretical framework for computing the present discounted value of growth trajectories that are permanently altered by a changing cyclone climate. We then apply our estimates to this framework, combining them with future projections from the scientific literature, to compute the cost of future changes in the global tropical cyclone climate. We find that accounting for the long-run growth effects of a changing cyclone climate substantially alters the global cost of climate change under "business as usual." For example, we estimate that the present discounted value[3] (PDV) of losses rise by 6% of current GDP for the United States, 17% of GDP for Mexico, and 83% of GDP for the Philippines. Globally, accounting for this novel pathway raises the PDV of future losses by roughly \$9.7 trillion (13.8% of current World GDP). For comparison, we note that Nordhaus (2008) estimates that the total PDV of optimal global climate policy is \$5 trillion (in comparison to "no regulation", using a similar discount rate) which costs \$2 trillion to implement, for a net gain of \$3 trillion – with \$17 trillion in residual damages.

The remainder of this chapter is as follows. In Section 2 we provide background on tropical cyclones and the economic impact of natural disasters. In Section 3, we describe our construction of a global data file describing cyclone exposure for each $1^\circ \times 1^\circ$ pixel of the planet and how these data are collapsed to match macro-economic data. In Section 4 we explain and evaluate our econometric model. In Section 5 we present our main results for growth, numerous robustness checks, tests for spatial spillovers, results for non-growth outcomes, and evidence of adaptation. We then consider the implications of these result through simulations of cyclone-free growth (for comparison to recent history) and calculations for the expected cost of climate change incurred by altering the global cyclone distribution. In Section 6 we conclude with a discussion of policy implications.

[3]We use a 5% discount rate.

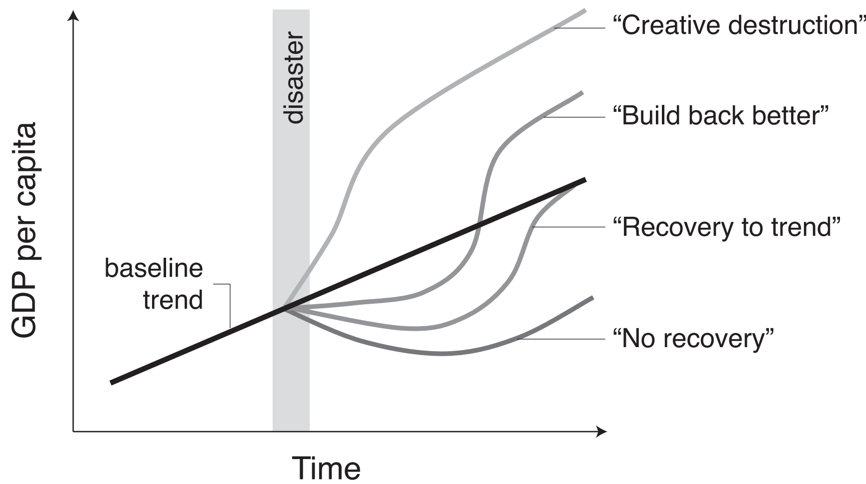

Figure 2.1: Four hypotheses, proposed in the literature, that describe the long-term evolution of GDPpc following a natural disaster.

2.2 Background

2.2.1 Economics of natural disasters

The notion that environmental disasters might have permanent long-run effects on income is not obvious, in part because it is frequently suggested that these events elicit economic responses fundamentally different from human-caused macroeconomic disasters (e.g. banking crises). In the absence of clear empirical evidence, prior literature has converged on four competing hypotheses that describe how economic output might respond to environmental catastrophes in the long-run, however no study has credibly falsified any of the four and the actual behavior of economies is widely disputed (Field et al. (2012)). Figure 2.1 schematically illustrates these four hypotheses:

1. The **"creative destruction" hypothesis** argues that disasters may temporarily stimulate economies to grow faster because demand for goods and services increase as populations replace lost capital, because inflowing international aid and attention following disaster may promote growth, or because environmental disruption stimulates innovation (Skidmore & Toya (2002)). This notion is partially motivated by the observation that construction industries often exhibit short-lived (1-2 year) increases in output after catastrophes (Belasen and Polachek (2008); Hsiang (2010); Deryugina (2011)), but it is unknown if this transient sector-specific response has enduring impact on the broader economy.

2. The **"build back better" hypothesis** argues that growth may suffer initially, since lives may be lost and productive capital destroyed, however the gradual replacement of lost assets with modern units has a positive net effect on long-run growth since the capital that is destroyed in a disaster may be older and outdated (Cuaresma, Hlouskova and Obersteiner (2008); Hallegatte and Dumas (2009)). This hypothesis might be true if firms do not upgrade their capital efficiently in the absence of disasters and if the productivity benefits of post-disaster capital upgrading exceed the productivity losses imposed by the disaster in the long run.

3. The **"recovery to trend" hypothesis** argues that growth should suffer for a finite period, but that it should eventually rebound to abnormally high levels, causing income levels to converge back to their pre-disaster trend. It is argued that this rebound should occur because the marginal product of capital will rise when capital and labor become relatively scarce after a disaster (due to destruction and mortality), causing individuals and wealth to migrate into devastated locations until output recovers to the regional trend (Yang (2008); Strobl (2011)). The underlying logic of this hypothesis has mixed empirical support: disasters do tend to trigger transfers of wealth into the affected region (Strömberg (2007); Yang (2008); Deryugina (2011)), however population inflows occur roughly as often as outflow or no migration (Smith et al. (2006); Vigdor (2008); Belasen and Polachek (2009); Hornbeck (2012); Strobl (2011); Boustan, Kahn and Rhode (2012); Bohra-Mishra, Oppenheimer, and Hsiang (2014)). The net effect of these wealth and population reallocations on long-run growth is unknown.

4. Finally, the **"no recovery" hypothesis** argues that disasters slow growth by either destroying productive capital directly or by destroying durable consumption goods (e.g. homes) that are replaced using funds that would otherwise be allocated to productive investments—but no rebound occurs because the various recovery mechanisms above fail to outweigh the direct negative effect of losing capital[4] (Field et al. (2012)). The latter effect may be particularly important if, in the wake of disaster, consumption falls so that the marginal utility of consumption rises enough that post-catastrophe consumption becomes preferable relative to investment (Anttila-Hughes and Hsiang (2011)). According to this hypothesis, post-disaster output may continue to grow in the long run, however it remains permanently lower than its pre-disaster trajectory.

[4]In addition to the impact of capital losses, it is also thought that disasters may generate enduring economic impacts by permanently altering the preferences of affected individuals (e.g. Cameron and Shah (2013)), by motivating populations to irreversibly disinvest in durable human or physical capital (e.g. Maccini and Yang (2009)) or by triggering political actions that have lasting economic consequences (e.g. Healy and Malhotra (2009)).

Recent reviews of the literature argue that the long-run effects of disasters remain a critical open question because recent attempts have not convincingly demonstrated whether any of the four hypotheses above can be rejected or hold generally (Cavallo and Noy (2011); Kellenberg and Mobarak (2011); Field et al. (2012)). This failure to eliminate hypotheses is theoretically unsatisfying, however we resolve this indeterminacy by using better data. The quality of prior estimates are affected by the endogenous nature of their independent variables: self-reported disaster counts and losses that are usually from the Emergency Events Database (EM-DAT). The quality and completeness of these self-reported measures are known to depend heavily on the economic and political conditions in a country (Kahn (2005), Strömberg (2007), Kellenberg and Mobarak (2008), Noy (2009), Hsiang and Narita (2012)), factors which also affect growth and thus might confound these results.

We overcome the challenges of omitted variables bias and endogenous disaster reporting by developing a novel data file describing year-to-year variation in each country's physical exposure to disaster. To do this, we focus on tropical cyclones, the class of natural disaster that includes hurricanes, typhoons, cyclones and tropical storms[5], and reconstruct every storm observed on the planet during 1950-2008. Unlike the self-reported statistics contained in EM-DAT, our objective measures of wind speed exposure and energy dissipation are fully exogenous, constructed using physical parameters and meteorological observations, so they are unlikely to be influenced by economic behavior or political actions within each country[6].

2.2.2 Tropical cyclones

Constructing a physical index of disaster exposure is essential to obtaining reliable inferences for their causal effect. However, because building a physical model to produce these indices is difficult, we focus on only a single type of disaster: tropical cyclones. We estimate that roughly 35% of the global population is seriously affected by tropical cyclones, making them one of the most broadly relevant

[5] Tropical cyclones are known as "tropical storms" or "hurricanes" in the Atlantic Ocean, "typhoons" in the Pacific Ocean, and "cyclones" in the Indian Ocean. Here, we refer to them as "tropical cyclones" or simply "cyclones."

[6] Our approach is identical to the desirable method outlined (but not implemented) by Noy (2009), who used EM-DAT data as an independent variable and assumed that it was not determined endogenously:

> "Without the exogeneity assumption, the only way to infer causality from our specifications would entail finding an appropriate instrument for the initial disaster impact (i.e., an index of disaster magnitude that is completely uncorrelated with any economic indicator). Regrettably, we did not find such an instrument....
>
> The exogeneity issue can potentially be fully overcome by producing an index of disaster intensity that depends only on the physical characteristics of the disaster (e.g., area affected, wave height, or storm circumference). The collection of such data from primary sources and the construction of a comprehensive index for the all the different disaster types are beyond the scope of this paper but may be worth pursing in future research." - p. 224

Table 2.2: Summary statistics for key variables in cyclone-exposed countries

Variable	Mean	Std. Dev.	Min.	Max.	N
Economic Characteristics					
Log GDPpc (Penn World Tables)	8.093	1.235	4.913	11.637	4914
Log GDPpc (World Development Indicators)	7.366	1.462	4.084	10.876	4248
Population (thousands)	32864	124191	7	1317066	6017
Small Island Developing State dummy	0.306	0.461	0	1	7905
Below median income (1970) dummy	0.643	0.479	0	1	5508
Physical Characteristics					
Tropical cyclones					
Wind speed (meters per second)	5.869	9.379	0	78.344	7905
Energy (standard deviations)	0.386	†1.271	0	19.41	7905
Log(land area)	9.606	3.984	-1.386	16.101	7905
Latitude (degrees north of Equator)	8.319	19.598	-41.577	59.388	7905

†The standard deviation of standardized *energy* is not equal to one because these summary statistics are computed for exposed countries only.

forms of disaster, in addition to being one of the most costly (Bevere, Rogers and Grollimund (2011)).

Tropical cyclones are large, violent and fast-moving storms that form over the oceans and cause physical damage and loss of life via intense winds, heavy rainfall, and ocean surges. We focus on tropical cyclones both because they are common and because variation in their timing, strength and location allow us to identify their effects using quasi-experimental techniques (Holland (1986), Freedman (1991), Angrist and Pischke (2008)). Tropical cyclones are considered "rapid onset" events[7], usually arriving, affecting and passing a given location within one or two days. They are unambiguously recognizable by meteorologists and are well defined in space, with an intense core roughly 100-200 kilometers across. Tropical cyclones' formation, over warm oceans, and trajectory, which may extend thousands of kilometers, are stochastic and difficult to predict more than a few days in advance. Thus, cyclone exposure at a specific location varies exogenously in its timing, intensity and duration. This randomness is essential to our analysis, since our ability to identify the causal effect of cyclones relies on the unpredictable year-to-year variation in the intensity of each country's cyclone exposure (Deschênes and Greenstone (2007)).

2.3 Data

Our central innovation is our construction of a novel data file describing the physical exposure of all countries to all known cyclones during 1950-2008, which we link to standard macroeconomic datasets.

[7] In contrast to "slow onset" hazards, such as drought.

Because macroeconomic data are available at the country-by-year level but we initially compute cyclone data at a $0.1° \times 0.1°$ global grid, a secondary contribution is developing a formal framework for aggregating spatially granular environmental exposure data to coarser country-by-year units that can be matched to macroeconomic data.

Summary statistics for both geophysical and economic data, aggregated to the country-by-year level, are presented in Table 2.2.

Tropical cyclone data

We expand on the approach of Hsiang (2010) and Hsiang and Narita (2012) to measure each location's history of cyclone exposure. We combine a database of ground, ship, aerial, and satellite-based observations with estimates for the distribution of winds within each cyclone at each moment in time to reconstruct what individuals on the ground would have experienced as each cyclone passed over them. We then use the micro-economic findings of Anttila-Hughes and Hsiang (2011) to provide insight into how we may collapse this spatially explicit data over countries of various sizes into scale-invariant measures that are appropriate for econometric analysis of economic growth, another scale-invariant measure.

Reconstructing a global history of tropical cyclone exposure

We generate measures of tropical cyclone incidence by reconstructing the wind field for every cyclone in the International Best Track Archive for Climate Stewardship (IBTrACS) database (Knapp et al. (2009)), the most complete global database of tropical cyclone observations[8]. IBTrACS merges tropical cyclone data collected from weather monitoring agencies and scientists around the world, who in turn have collected information on the intensity and position of tropical cyclones from ground, ship, aerial, and satellite based observations. For this analysis, we use IBTrACS records for 6,712 storms observed during 1950–2008. The completeness of this record is considered strongest since the late 1970's when satellite surveillance provided reliable monitoring of storms because changing patterns of human activity on the surface have raised concerns that earlier portions of the record are incomplete. For example, the opening of the Panama Canal in 1915 and World War II both substantially altered the spatial distribution of trans-Atlantic boat traffic, which in turn changed the likelihood that mid-ocean cyclones would be encountered and reported by ships (Vecchi and Knutson (2008)). However,

[8]These data are publicly available through the National Climate Data Center at http://www.ncdc.noaa.gov/ibtracs/index.php where they are described in detail.

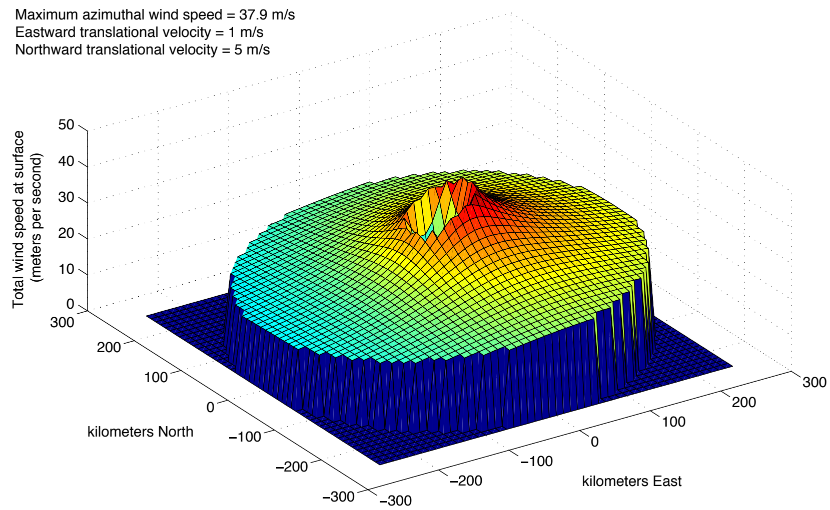

Figure 2.2: An example of the wind model used in the LICRICE model to reconstruct surface-level exposure to tropical cyclone winds. This particular example is a Category 1 storm traveling north-northeast.

we do not think these changes substantially bias the portions of the record that we utilize, since we are primarily concerned with economic activity over land and land-based observations of these storms are very likely more reliable prior to the satellite era.

IBTrACS provides only limited information regarding the state of each storm, which we transform into economically meaningful measures of exposure using an improved version of the Limited Information Cyclone Reconstruction and Integration for Climate and Economics (LICRICE) model first applied in Hsiang (2010) for the more limited Caribbean Basin context. IBTrACS reports the location of a cyclone's center, its minimum central surface air pressure, and its maximum sustained surface winds every six hours. Taken alone, this sequence of point-wise observations allows researchers to plot the trajectory of a storm's center and it's core intensity on a map, but it is difficult to infer the exposure of national economies to these events using only this single line. For example, the recorded trajectory of Hurricane Allen in 1980 completely missed the national boundaries of Haiti (i.e. Allen never made "landfall" in Haiti) but it would be a mistake to conclude that Haiti was not exposed to the storm: Hurricane Allen passed along the southern coast of Haiti, side-swiping Port-au-Prince, causing \$400 million (1980 USD) in damage, destroying 60% of the nation's coffee crop and leaving 835,000 people homeless (Longshore (2009)). Thus, to accurately capture the exposure of economies to cyclones, we reconstruct the winds that individuals and assets on the surface would have been exposed to rather

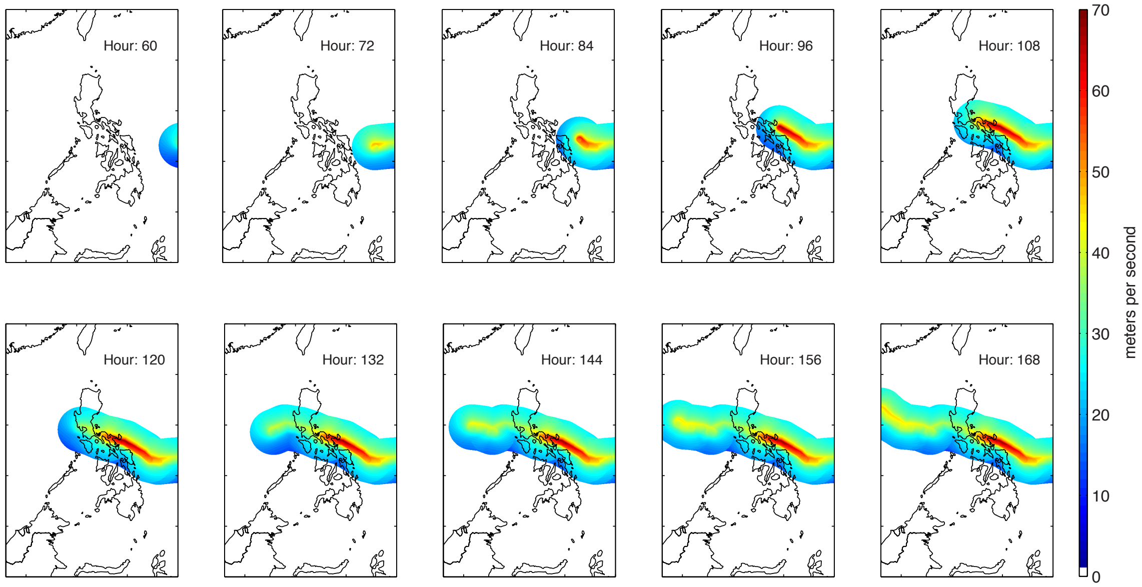

Figure 2.3: An example LICRICE reconstruction of location-specific tropical cyclone maximum wind speed exposure throughout the evolution of Super Typhoon Joan as it made landfall over the Philippines in October of 1970.

than simply tracking each storm's center. LICRICE does this by estimating the instantaneous wind field within the storm at each moment in time based on interpolations of the 6-hourly observations recorded in IBTrACS (see Figure 2.2 for an example). The structure of the wind field within each storm is based on (1) a statistical prediction for the size of the storm's inner core (known as the "eye") where the statistical model is fitted to detailed observations from aircraft reconnaissance missions that fly through a storm's center; (2) a structural model of the surface winds within a cyclone vortex that is scaled to the size estimate in (1) and the intensity measures from IBTrACS; and (3) the speed that the storm is translating over the surface. Using these reconstructed estimates for the wind field at each moment in time, LICRICE then integrates the exposure that pixels on the surface would have experienced during the life of the storm (see Figure 2.3 for an example).

We reconstruct wind exposure indices at each $0.1° \times 0.1°$ pixel between 48°N–48°S latitude for all 6,712 storms in the IBTrACS database during 1950–2008. This involves interpolating among the 191,822 points that represent storm-specific observations. Figure 2.4 displays this new data set of wind exposure for all points on Earth for every year in the sample.

To provide a useful point-wise summary statistic of this new data, we average pixel-level exposure across all 59 years of data for each pixel. This recovers the expected experience at each pixel, which we

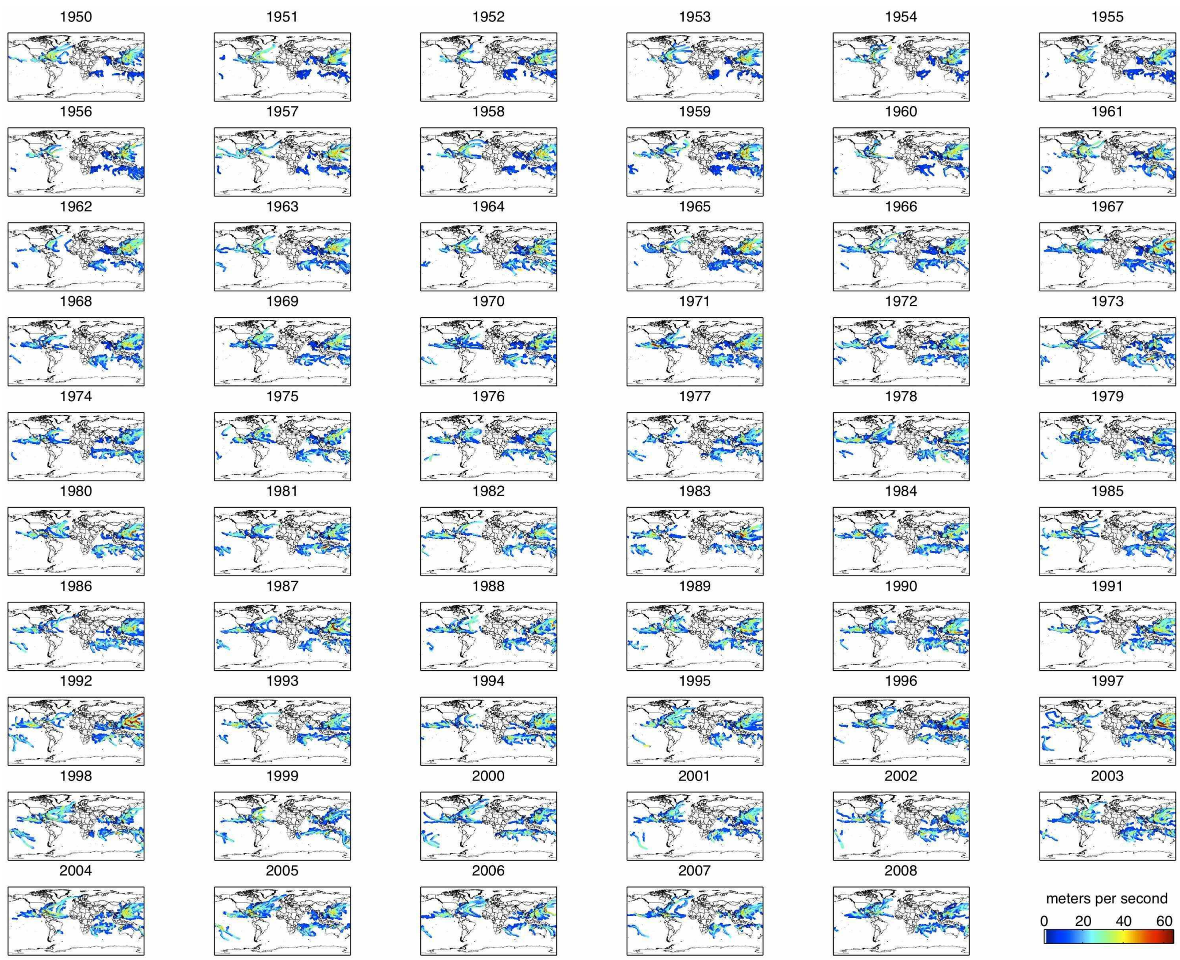

Figure 2.4: Global tropical cyclone exposure displayed as maximum wind speed for each pixel, for each year in the dataset.

term the "cyclone-climate" of that pixel and display in Figure 2.5. Cyclone exposure is not uniformly distributed around the planet, but instead it is concentrated in coastal countries in the tropics and middle latitudes. Countries very near the equator, such as Singapore, are not exposed to cyclones because the storms curve away from the equator as they conserve angular moment. Also, countries on the eastern coast of continents (eg. Madagascar) are generally more exposed than countries on western coasts (eg. Nigeria) because tropical cyclones are driven towards land by the westward blowing winds that dominate atmospheric circulations over regions where these storms form.

In principle, it is possible to develop numerous measures of wind exposure. Here we utilize two wind indices, based on climate physics, that summarize cumulative cyclone wind exposure in different ways. Each index has its own strengths and weaknesses.

The first measure is a *power dissipation density index* (hereafter "energy"), first developed in

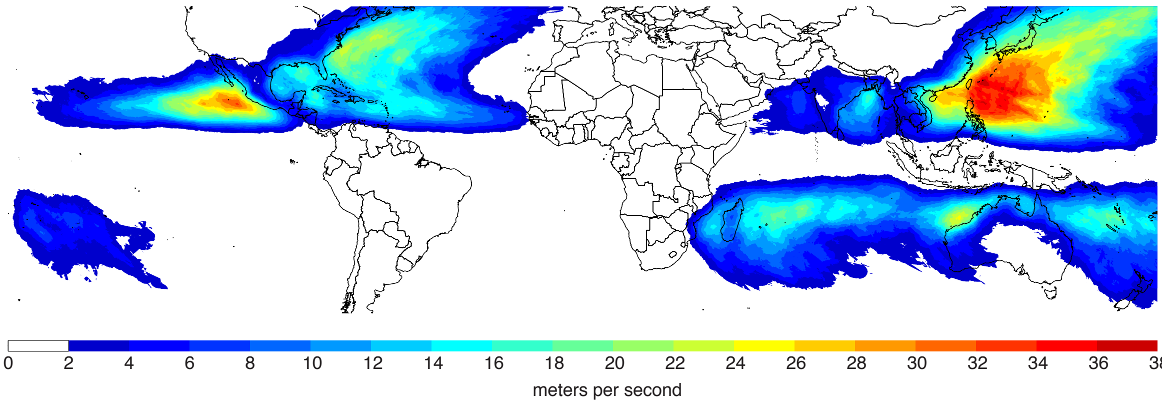

Figure 2.5: Global tropical cyclone exposure climatology derived from LICRICE. Colors denote the average (across years) maximum wind speed for all tropical cyclone events during 1950-2008. See Figure 2.4 for year-by-year data.

Hsiang (2010), which describes the total quantity of energy that a storm dissipates at the surface as it passes over a location[9]. Storms with more intense winds dissipate more energy, as do storms that move more slowly over a location. The power dissipation density index is an intuitive measure for aggregating exposure across storm events or across pixels within a country because energy is a conserved physical quantity, making it a sensible value to sum across events. However, the units are the relatively unintuitive *meters-cubed per seconds-squared* (m^3/s^2), so we standardize its units for expositional and notational convenience.

The second index of cyclone exposure is the *maximum wind speed* (hereafter "wind speed") experienced over the course of all storms in a given year, which was first introduced in Hsiang and Narita (2012). Measuring incidence with maximal wind exposure is appealing because most rigid materials used to construct durable capital fail catastrophically at a critical level of stress, so only the maximum wind speed is essential for predicting whether capital will be heavily degraded[10]. Wind speed has the additional benefit that it is measured in the physically intuitive units of meters per second (m/s), so we leave wind speed unstandardized. Notably, unlike energy, a pixel's measure of wind speed is unchanged if a second weak storm strikes that pixel after a stronger event has already passed.

Wind speed and energy are correlated with one another, but we focus our attention on results that use wind speed as an independent variable because its units are intuitive, it produces more conservative estimates in this study, and it produced more robust estimates in Hsiang and Narita (2012), probably

[9]This measure is related to "accumulated cyclone energy" (ACE) and the "power dissipation index" (PDI) which are commonly used in the field of meteorology (Emanuel (2005)).
[10]This idea was first discussed in the economics literature by Nordhaus (2010).

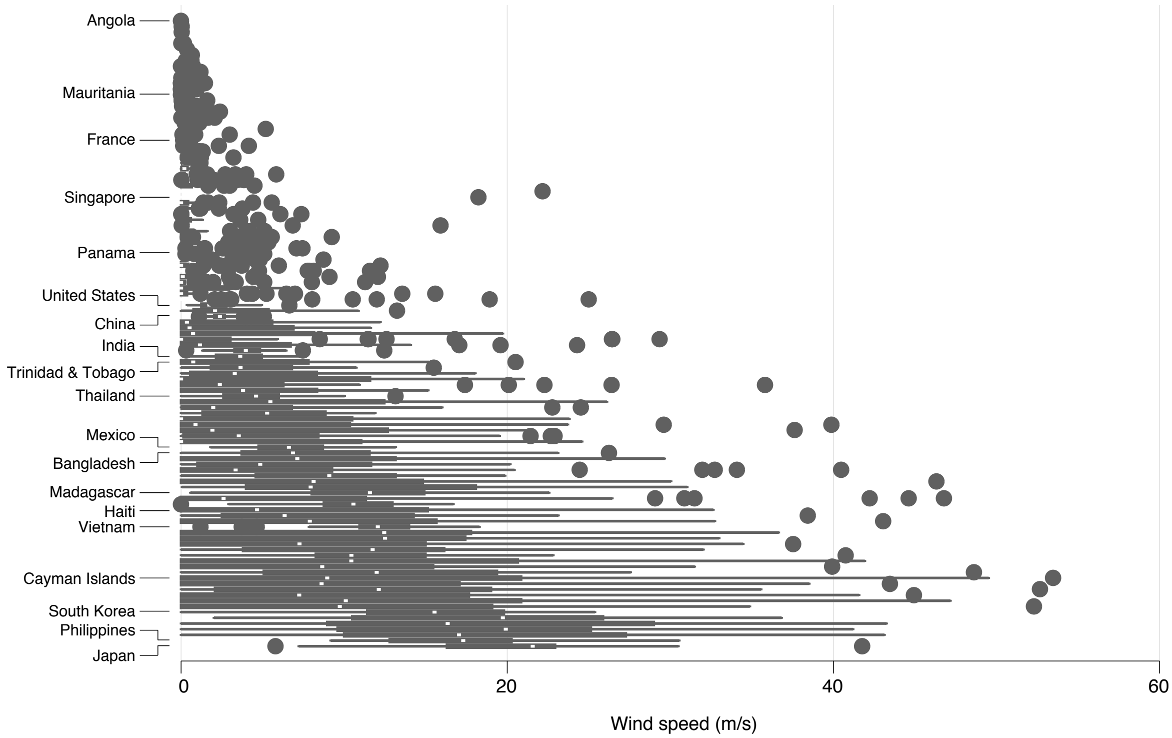

Figure 2.6: Boxplot of within-country distributions of country-by-year wind speeds during 1950-2008 for exposed countries. Boxes are interquartile ranges, white stripe is the median, circles are outliers. Countries are ordered according to their mean exposure across years. Countries with no positive exposure observations are not shown.

because its distribution is less skewed than energy. For related reasons but in different contexts, Hsiang and Narita (2012) and Anttila-Hughes and Hsiang (2011) also focus on wind speed, a fact that proves useful when we compare our results to those of these other studies. Nonethelsss, we also present results using energy as an independent variable to check the robustness of our findings.

We do not explicitly model other dimensions of tropical cyclones that are known to be economically meaningful, such as excess rainfall, storm surges, and landslides. We do not characterize countries' exposure to these other processes because they are more heavily influenced by idiosyncratic geographic features, making them computationally difficult to model, however the impact of these measures will be captured by our estimates to the extent that they are correlated with these wind field indices. For physical reasons, all three factors will be correlated with overall wind exposure. Thus our wind indices can be considered proxy measures for all dimensions of cyclone exposure.

Matching cyclone data to economic units of observation

The data file of reconstructed storm exposure can be resolved with high spatial and temporal resolution, since each $0.1° \times 0.1°$ pixel of the Earth's surface takes different values every hour. Yet the unit of observation for macroeconomic data that we match with cyclone exposure is the country-by-year. Linking these two data sets requires that we collapse the cyclone exposure data in an economically sensible way. Economic growth is a scale-free measure that does not depend on the size of an economy. Ideally, we may construct an appropriate measure of cyclone exposure at the country-year level that is similarly scale-free and does not depend on the physical or economic size of a country, so that we recover a scale-invariant relationship between economic growth and cyclone exposure. Such a relationship would describe the average pixel-level relationship between pixel-level growth and pixel-level exposure[11].

Prior micro-econometric work by Anttila-Hughes and Hsiang (2011) indicates that the probability of destruction of assets, loss of total income, and increase in infant mortality change approximately linearly with local wind exposure. Because of this, we can collapse pixel-level wind exposure to the country-by-year unit using a spatially-weighted average over all pixels in a country[12]. For pixels indexed by p each of area a_p exposed to wind speeds (or energy) S_p, contained in country i which has n pixels in total, this is simply

$$\bar{S}_i = \frac{\sum_{p \in i} S_p a_p}{\sum_{p \in i} a_p} \tag{2.3.1}$$

This measure can be thought of intuitively in one of two ways: it is the expected exposure of a unit of land that is selected at random from a country or it is the exposure all units of land would have if wind exposure could be "spread out" evenly across all locations in a country. Because many pixels in a country may experience low wind exposure in a year and these values are averaged along with high exposure pixels, spatially averaged country-by-year wind speed measures will tend to be substantially lower than the maximum wind speeds reported at the center of intense storms. Figure 2.6 displays the distribution of country-by-year average wind speed exposure across years for all countries that ever have a non-zero value in the sample (hereafter "exposed countries"). Notably, there is substantial year-to-year variation in exposure within most countries and there is substantial overlap in exposure

[11]Using scale-free variables to link geophysical measurements of cyclones to economic measurements has been successfully replicated at the national level in regional (Hsiang (2010)) and global data sets (Hsiang and Narita (2012)) and at the level of both provinces and larger administrative regions using Filipino household data (Anttila-Hughes and Hsiang (2011)). As one might expect when using scale-free variables, in all of these cases the estimated effect-sizes were approximately invariant in the geographic size of the observational units.

[12]For the United States, Alaska is omitted from the average.

levels across countries. Japan and the Philippines experience the highest average exposure while India and Trindad & Tobago have median levels of average exposure (among exposed countries).

Constructing the scale-free measure $\bar{S}_i$ requires that the weighted sum of all pixel-level exposures is divided by the area of a country. This normalization is analogous to normalizing GDP by population to recover per capita GDP or normalizing new income by previous income to recover income growth in percentage terms. As with all normalizations, a larger denominator will result in a smaller measure of $\bar{S}_i$ if the numerator is held fixed. Thus a physically identical cyclone event that affects exactly one pixel will result in a larger value for $\bar{S}$ in a small country relative to a large country. This is the desired effect of using a scale-free measure, since *ceteris paribus* the single pixel affected by the storm will be more economically important in percentage terms in the smaller country because it is a larger fraction of the entire country. This approach follows the spirit of Nordhaus (2006) and aims to recover the average effect of cyclone exposure on an average pixel—it is agnostic about how land in a pixel is used[13]. One may think of this approach as trying to capture cyclone activity as one dimension of a pixel's endowment. We are essentially asking whether cyclone activity affects growth similar to how one might ask whether good soils or freezing temperatures affects growth in a pixel.

Two important questions invariably arise when cyclone exposure is collapsed using Equation 2.3.1. First, does area-weighting somehow bias response functions in favor of small countries, since their denominator is small? Our approach scales exposure to the pixel level, but it is possible that pixels within a small country will have a fundamentally different response from pixels within a large country, so one might be concerned that our results over-represent the unique response of small country pixels. This issue, however, is a question about heterogenous responses to cyclones and not a question of scaling, so it is best addressed by stratifying samples according to country size—an exercise we conduct in our results section (we find that countries exhibit remarkably similar responses at the pixel level across all sizes, except for the very smallest and largest countries). Second, will our estimates be biased because some cyclones strike heavily populated or economically critical locations while other cyclones strike empty regions? This is not a concern, so long as there is not correlation between the overall intensity of a storm (as measured by the average across pixels) and the likelihood that

[13] It may be possible to reduce our measurement error by using population-weights, following Dell, Jones and Olken (2012) and Hsiang, Meng and Cane (2011), or capital-weights, following Nordhaus (2010), when aggregating our exposure measure. However, we fear that if populations strategically locate themselves or capital in response to cyclone risk, this may bias our estimated coefficients in some unknown way since some populations may be more or less likely to relocate based on other factors that are unrelated to cyclones but might also affect growth. Thus, we use area-weights because populations cannot manipulate this parameter, giving us confidence that our independent variable is fully exogenous. This conservative approach may mean that our estimation is inefficient, in the sense that it does not take advantage of all available data, but this should only make our inferences more conservative.

the most intense regions within that storm strike the most economically active (or vulnerable) pixels within a country. The condition for unbiased estimation restricts the spatial correlation of exposure and economic activity *within* a storm to be unrelated to the intensity *across* storms.[14]. So long as

[14] Suppose pixels have heterogenous pre-storm capital K_p (capital could be physical, human, social, political, etc.) which has a long run production $f(K_p)$. Damage to this capital from a storm suffered at p is $D(S_p, K_p)$, a function of storm intensity S_p experienced at pixel p. Anttila-Hughes and Hsiang (2011) find $D(S_p, K_p) = \alpha K_p S_p$, where α is a constant describing the marginal fraction of capital that is destroyed by each additional unit of S_p. Thus, $\alpha S_p \in [0, 1]$ for observed values of S_p. We assume a similar linear form holds generally.

Long-run output lost to a storm is the difference between output with baseline capital when no storm occurs (our simple counterfactual here, but a trend could be accounted for) and output with storm-damaged capital, both summed over all pixels in country i:

$$lost\_income_i = \sum_{p \in i} f(K_p) - \sum_{p \in i} f(K_p - \underbrace{\alpha K_p S_p}_{D(S_p, K_p)}).$$

If changes to the total capital stock from a single storm are modest relative to the curvature of $f(.)$, by Taylor's theorem we can linearize $f(K_p - \alpha K_p S_p) \approx f(K_p) - f'(K_p)\alpha K_p S_p$ at each pixel. Letting $g(K_p) = f'(K_p)\alpha K_p$, we write

$$\begin{aligned} lost\_income_i &\approx \sum_{p \in i} f(K_p) - \sum_{p \in i} \left(f(K_p) - f'(K_p)\alpha K_p S_p \right) \\ &= \sum_{p \in i} g(K_p) S_p \end{aligned}$$

Thus losses are roughly the inner product of storm intensity in each pixel and the marginal effect of storm intensity on production in each pixel, where the latter depends on both the capital density at p and the shape of the production function. Because we do not have observations of $g(K_p)$ for each pixel, we must find some way to estimate aggregate lost growth as a function of wind exposure. As in Equation 2.3.1 we denote area averages with a bar such that $\bar{x}_i = \sum_{p \in i}(x_p a_p) / \sum_{p \in i} a_p \approx \sum_p x_p / n_i$. The approximation holds if pixel areas do not vary substantially within a country, which is a reasonable approximation for almost all countries since pixel area is proportional to cosine of latitude and few countries exposed to tropical cyclones span large ranges of latitudes at high latitudes (where the derivative of cosine is large). Because there are many pixels in each country, we rewrite the sum of pixel impacts, i.e. the total lost income, in terms the average over pixels:

$$\begin{aligned} lost\_income_i &\approx n_i \, \overline{(g(K_p)S_p)}_i \\ &= n_i \, \overline{g(K_p)}_i \, \bar{S}_i + n_i \mathrm{Cov}_p(g(K_p), S_p) \end{aligned}$$

where the second term is the covariance across pixels between $g(K_p)$ and storm intensity for a specific cyclone event. Because the size of these terms scale with the size of a country n_i, we normalize by the initial size of the economy $n_i \, \overline{f(K_p)}_i$ so lost income is in terms of lost growth, a scale-invariant economic measure

$$lost\_growth_i = \frac{lost\_income_i}{initial\_income_i} \approx \underbrace{\left(\frac{\overline{g(K_p)}_i}{\overline{f(K_p)}_i} \right)}_{\hat{\beta}} \bar{S}_i + \underbrace{\frac{\mathrm{Cov}_p(g(K_p), S_p)}{\overline{f(K_p)}_i}}_{\varepsilon} \tag{ℵ}$$

where the coefficient of interest, labeled $\hat{\beta}$, does not scale with the size of the country n_i. The form of Equation ℵ is useful because it links a national summary statistic describing area-averaged cyclone exposure $\bar{S}_p$ to a national summary statistic describing economic growth. The factor denoted $\hat{\beta}$ is the coefficient that we will attempt to measure empirically—it is the average marginal effect of cyclone exposure on long-run output in percentage terms. The form of Equation ℵ is what motivates us to use the spatial average of cyclone exposure across pixels to aggregate pixel-level cyclone exposure to the country-year level to match the units of observation in macro-economic data.

The term denoted ε is a residual that is likely mean zero—it is the covariance across pixels of cyclone exposure in a single storm and the marginal effect of cyclone exposure across pixels, normalized by total output of i. Importantly, it is a country-by-storm specific residual. The intuition behind this term is that sometimes a cyclone will cause unexpectedly large damages because the most intense part of the storm will pass directly over a location that has either a high capital density or a large sensitivity to cyclones (e.g. Hurricane Katrina), this will cause covariance between S_p and $g(K_p)$ to be positive and the lost growth from this event to be abnormally large relative to what we expect based on the average intensity of exposure $\bar{S}_p$. In other cases, the most intense part of a storm may pass over an uninhabited region, in which case this covariance will be negative and the lost growth will be abnormally low relative to expectation. On average across years, we assume ε is approximately zero because cyclone exposure within each storm is unlikely to be systematically correlated with economic activity on the ground.

Importantly, holding other factors constant, we will obtain an unbiased estimate of $\hat{\beta}$ if we estimate the expected value of Equation ℵ using observed values of $\bar{S}_i$ so long as ε is not correlated with $\bar{S}_i$. Thus ordinary least squares will be relatively more intense storms do not differentially strike centers of economic activity within a country, it is unnecessary to account for the spatial distribution of economic activity in our measure of storm exposure in order to obtain an unbiased estimate for the effect of storms on growth.

Economic data

We obtain gross domestic product (GDP) data for 1970-2008 from the Penn World Tables[15] (PWT) (Summers and Heston (1991)) as well as the World Development Indicators (WDI) file (World Bank (2008)). GDP is inflation adjusted and measured in *per capita* units. For robustness, we separately examine and compare results using both PWT and WDI which, in combination with our two cyclone measures, provides us with four pairs of independent and dependent variables that we evaluate separately. In robustness checks, we also utilize other macroeconomic measures from the WDI file, such as international aid.

Additional climate data

Because recent evidence suggests that temperature and precipitation both influence economic growth (Miguel, Satyanath and Sergenti (2004); Barrios, Bertinelli and Strobl (2010); Hsiang (2010); Dell, Jones and Olken (2012)) and these variables may be correlated with patterns of tropical cyclone exposure over time (Auffhammer, Hsiang, Schlenker and Sobel (2013)), we construct spatially averaged measures of annual mean temperature and precipitation using data files from the Center for Climatic Research at the University of Delaware (Legates and Willmot (1990a), Legates and Willmot (1990b)). However, because the University of Delaware (UDEL) data relies on spatial interpolation of weather station observations, it does not provide coverage for many island countries around the world. To overcome this issue, we also utilize "reanalysis" output from the Climate Data Assimilation System produced by the National Center for Environmental Prediction (NCEP) and the National Center for Atmospheric research (Kalnay et al. (1996)). Reanalysis techniques use a physical model (similar to a weather model) to assimilate data sources, allowing all missing data points to be estimated based on observed data and known physical relationships (see Auffhammer, Hsiang, Schlenker and Sobel (2013)

unbiased if

$$\mathrm{Cov}_t\left(\frac{\mathrm{Cov}_p(g(K_p), S_p)}{\overline{f(K_p)}}, \bar{S}_i\right) = 0$$

where the outer covariance is across years (i.e. different storms). The intuition behind this condition is that Equation ℵ is unbiased if there is no correlation between the average intensity of a storm ($\bar{S}_i$) and the likelihood that the most intense regions within that storm strike the most economically active (or vulnerable) pixels within a country ($\mathrm{Cov}_p(g(K_p), S_p)$).

[15]We use version 7.0 of the PWT (from 2011), however our results also hold if we use version 6.2 and 6.3.

for a complete discussion), enabling us to retain our entire sample of interest while also accounting for historical temperature variations.

2.4 Empirical approach

To estimate the causal effect of cyclones on long run growth we adopt a differences-in-differences approach, modeling first differences of the logarithm of GDP (economic growth) as an impulse-response function that is linear in contemporaneous and historical area-averaged tropical cyclone exposure $\bar{S}$ out to a maximum lag length k. Our approach follows the general framework for identifying the effect of random weather events laid out in Deschênes and Greenstone (2007). We account for unobservable differences in average growth rates between countries using a country fixed effect γ, which might arise, for example, because of countries' different geographies (Gallup, Sachs and Mellinger (1999)), cultures (Sala-i-Martin (1997)) or institutions (Acemoglu, Johnson and Robinson (2002)). We flexibly account for common nonlinear trends and year-specific common shocks using a year fixed effect δ, and we account for country-specific trends in growth rates θ, which may account for country-specific changes in economic policies as well as long-run conditional convergence (Barro and Sala-i-Martin (2003)). Because growth is the first derivative of income levels, including both country fixed effects and country-specific trends in a growth regression allows the trajectory of income levels in each country to exhibit an independent intercept, an independent slope and an independent curvature. In extensions of our main model, we also control for various time-varying controls X, such as trade openness (Sachs, Warner, Aslund and Fischer (1995)) or rainfall (Miguel, Satyanath and Sergenti (2004)). Indexing countries by i and years by t, this approach leads us to the flexible and parsimonious model:

$$\ln(GDP_{i,t}) - \ln(GDP_{i,t-1}) = \sum_{L=0}^{k} \left[\beta_L \times \bar{S}_{i,t-L}\right] + \gamma_i + \delta_t + \theta_i \times t + \eta \times X_{i,t} + \epsilon_{i,t} \tag{2.4.1}$$

where the parameters of interest are the coefficients β. We estimate Equation 2.4.1 using ordinary least squares (OLS) and follow the approach in Hsiang (2010) by assuming that the disturbance ϵ may be heteroscedastic and serially correlated within a country for up to 10 years (Newey and West (1987)) and spatially correlated across contemporaneous countries up to a distance of 1000 km[16] (Conley (1999)). The timing, location and intensity of cyclone exposure is unpredictable and stochastic across

[16] 1000 km was chosen because it is roughly twice the diameter of a storm and it also roughly describes the approximate average distance inland that storms may travel after landfall.

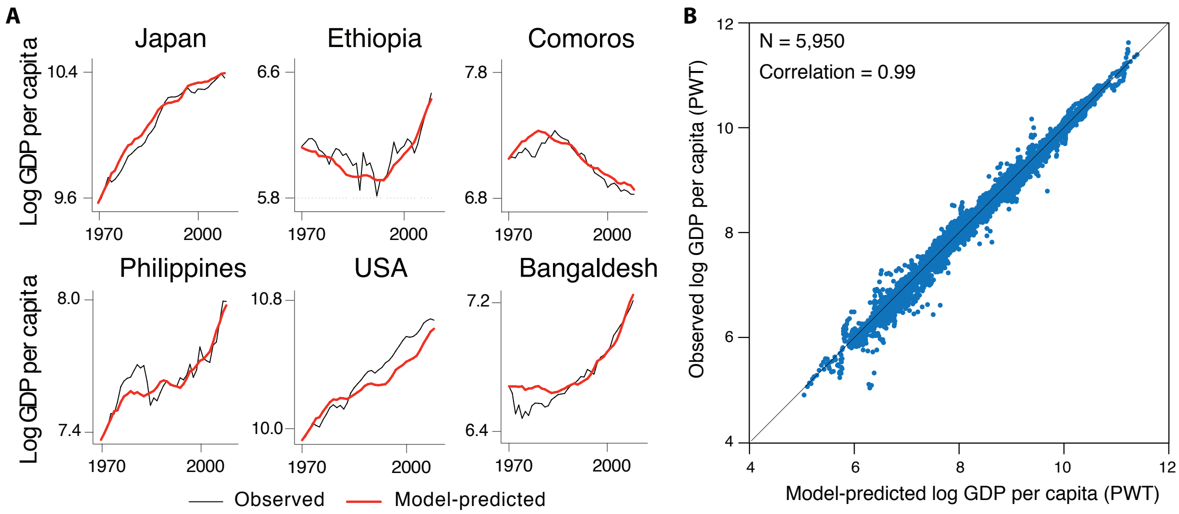

Figure 2.7: (A) Model predictions compared to observed income trajectories for six example countries. Model predictions are estimated using Equation 2.4.1 and then integrated from observed initial incomes in 1970. (B) Model predictions vs observed income for the full sample.

years, conditional on each country's average climate and trends in climate, whose effects are absorbed by country fixed effects, year effects and county specific trends. This allows us to assume that $\bar{S}$ is exogenous and uncorrelated with other unobserved factors ϵ that influence growth, permitting the causal effect of cyclones β to be identified. We note that it is unlikely that social, political or economic events within a country systematically influence our measurement of cyclone exposure because the LICRICE reconstruction of $\bar{S}$ primarily relies on satellite or other scientific observations.

The reduced form of Equation 2.4.1 does well at capturing a variety of behaviors for the slow moving changes in income that have been observed since 1970, as demonstrated in Figure 2.7A where predicted values from Equation 2.4.1 are integrated to estimate log income. Idiosyncratic and temporary disturbances in growth are not captured well with this model, however these high-frequency variations are not the focus of this analysis since we are interested in long-run growth; and the overall performance of the model is strong despite this shortcoming. Figure 2.7B plots predicted income against observed income and we note that the overall correlation is 0.99[17].

We estimate Equation 2.4.1 in first differences of $\ln(GDP)$ because year-to-year GDP growth is approximately trend-stationary. However, for a tropical cyclone that occurs in year t, we are interested in long-run GDP growth out to the period $t+j$, which is the sum of year-to-year growth effects for the years t to $t+j$ inclusive. Thus, after we estimate Equation 2.4.1, we construct the cumulative effect

[17]This value refers to the correlation between the full set of model predicted values and observations, not the R^2 value of the model.

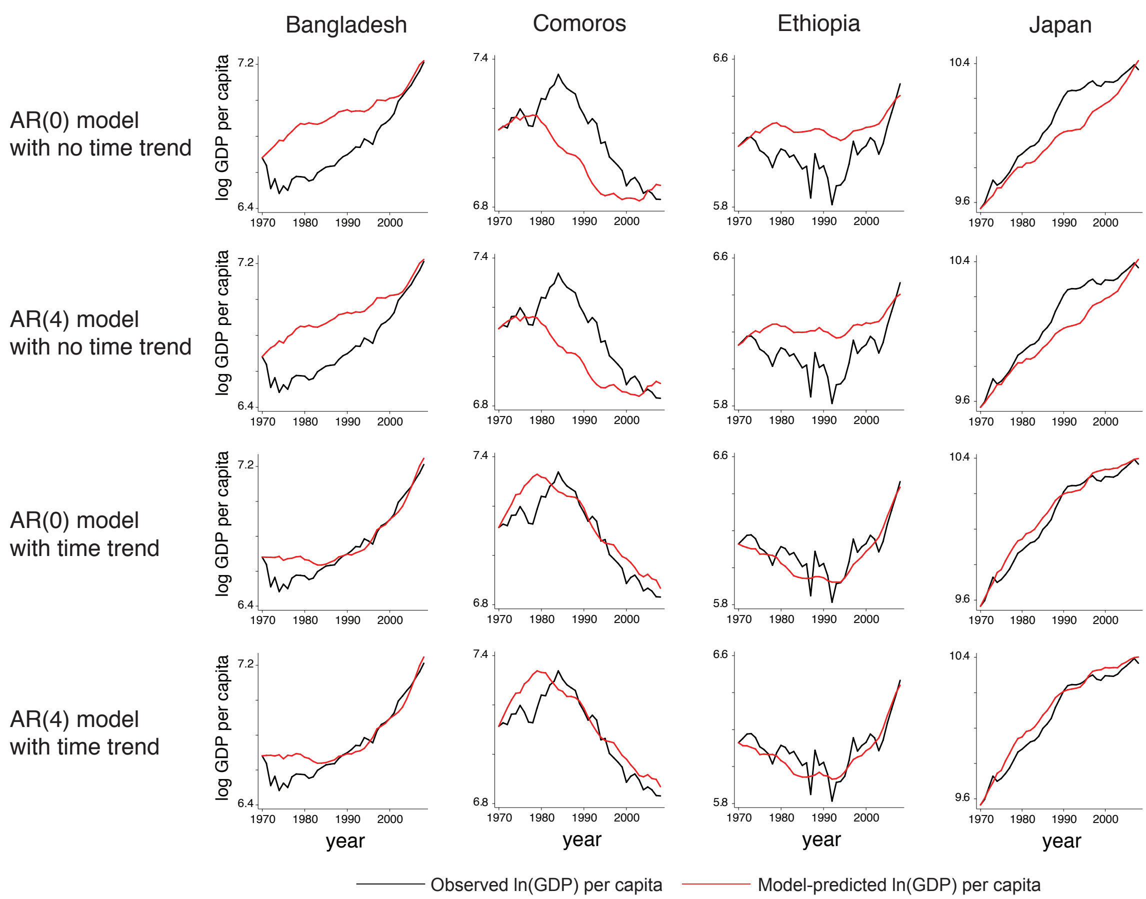

Figure 2.8: Model predictions compared to observed income trajectories for example countries (as in Figure 2.7). Model specifications differ by row, varying whether zero or four auto-regressive terms are included as regressors and whether country specific trends in growth θ_i are included.

of a cyclone j years after exposure via the summation

$$\Omega_j = \sum_{L=0}^{j} \beta_L. \tag{2.4.2}$$

For brevity and clarity, we only present the long-run growth effects Ω_j and omit estimates of β_L, however it is straightforward to difference our estimates for Ω to recover the OLS coefficients β.

Previous studies have estimated variations on Equation 2.4.1 with fewer lags and focusing only on the years during and just following disaster exposure, often measured as a binary variable. However, previous studies could not or did not try to identify whether the long-run growth effect Ω was measurable or economically important. Thus, in addition to our novel data, another innovation in our analysis is to examine a model that spans two full decades ($k = 20$), the longest lag length for which our estimates seem reliable (our panel is only 39 years long) and for which we do not have to drop any observations (our cyclone data reconstruction begins in 1950). In our results section we experiment with alternative lag lengths and observe no appreciable change in our results.

Our main specification (Equation 2.4.1) is a distributed lag model, where the lags of interest describe current and historical cyclone exposure. This simple approach has been successfully employed by other studies of growth where the regressors of interest are temporary events that are plausibly exogenous (Miguel, Satyanath and Sergenti (2004); Romer and Romer (2010); Barrios, Bertinelli and Strobl (2010); Dell, Jones and Olken (2012)) since it is unbiased (Greene, (2003)). Yet growth in the short run tends to be auto-regressive, leading many researchers to estimate auto-regressive distributed lag models in these settings (Cerra and Saxena (2008); Romer and Romer (2010); Hsiang (2010)). We employ this latter approach in a robustness check to our main result where we follow Cerra and Saxena (2008) and Romer and Romer (2010) by introducing up to four years of lagged growth as regressors in Equation 2.4.1.

One feature of our specification that is not always present in regressions of this form is the country-specific linear trend in growth θ_i[18]. This term describes how each country's growth rate may drift over time relative to global trends in growth. Because growth is the first derivative of income, allowing a trend in growth rates is equivalent to allowing countries' income trajectories to have a non-zero second derivative, i.e. each 40-year income trajectory may be curved differently. Inspection of Figure 2.7A suggests that this component of the model is likely important, since different countries within the sample have income trajectories that are convex and concave, as well as some with almost zero-curvature[19]. Inclusion of four years of auto-regressive terms in the model does not correct for this issue, as we demonstrate in Figure 2.8 where we show comparisons of model predictions with and without θ_i and auto-regressive terms for four important example countries. Auto-regressive terms help the model capture high-frequency but small amplitude business cycles while inclusion of θ_i is often important for accurately modeling long-rung income growth. Nonetheless, for completeness we also estimate a version of Equation 2.4.1 that omits θ_i as a robustness check.

[18]See Hsiang, Burke and Miguel (2013b) for a discussion.

[19]Failing to include country-specific values for θ_i in our model is equivalent to assuming that the income trajectories of all countries are curved equally, a hypothesis that we easily reject with a joint F-test for the restriction $\theta_i = 0 \ \forall \ i$ (Hsiang and Meng (2014)).

2.5 Results

We first establish that tropical cyclones have a large and robust negative effect on long-run GDP per capita. We then demonstrate that other macroeconomic variables exhibit similar behavior and we provide evidence that populations adapt to their geographically determined cyclone-climate. We next use simple simulations to understand the extent to which these effects might influence global patterns of economic development and compare our results, quantitatively, to related findings in the literature. Finally, we conclude by computing how these results influence estimates for the social cost of climate change.

2.5.1 Main result: the long-run effect of disaster on GDP growth

The first panel of Figure 2.9 presents our main result: the long-run effect of tropical cyclones on GDP relative to a country's pre-disaster baseline trend[20]. The plot depicts $\Omega_{t\in[-5,20]}$ after Equation 2.4.1 is estimated. Following a cyclone event, GDP declines steadily for roughly fifteen years relative to a counterfactual trajectory that would have been observed had the event never occurred. Fifteen years after a strike, GDP is 0.38 percentage points lower for every additional 1 m/s of wind speed exposure and exhibits no sign of recovery after twenty years.

The magnitude of the observed effect is large. Within the set of countries (58%) that are ever hit by cyclones, a one standard deviation increase in wind speed is equal to 9.4 m/s of wind exposure, generating a loss of $9.4 \times 0.38 = 3.57$ percentage points two decades later. A "one-in-ten" country-year event[21] reduces long-run GDP by 7.4% and a "one-in-one-hundred" country-year event depresses it 14.9%. The largest event in our sample (78.3 m/s) is estimated to have reduced long-run GDP by 29.8%. To succinctly summarize the size of our main result and the frequency of these storms, Figure 2.11 displays the distribution of country-by-year cyclone observations and the long-run GDP loss associated with 5, 10, 20 and 40 m/s events.

The structure of this result allows us to decisively reject the hypotheses that per capita national incomes benefit from tropical cyclone incidence ("creative destruction") ($p < 1 \times 10^{-4}$) or recover to their pre-disaster trajectory ("build back better" or "recovery to trend") within twenty years ($p < 0.001$). It is instructive to consider what happens to growth rates, as it aids in understanding the

[20] As discussed above, the "baseline trend" is depicted as a straight line, however we allow the baseline trend in our models to have intercepts, slopes and curvatures that vary by country as well as common year-specific shocks.

[21] The 90th percentile in wind speed is 19.5 m/s and the 99th percentile 39.2 m/s.

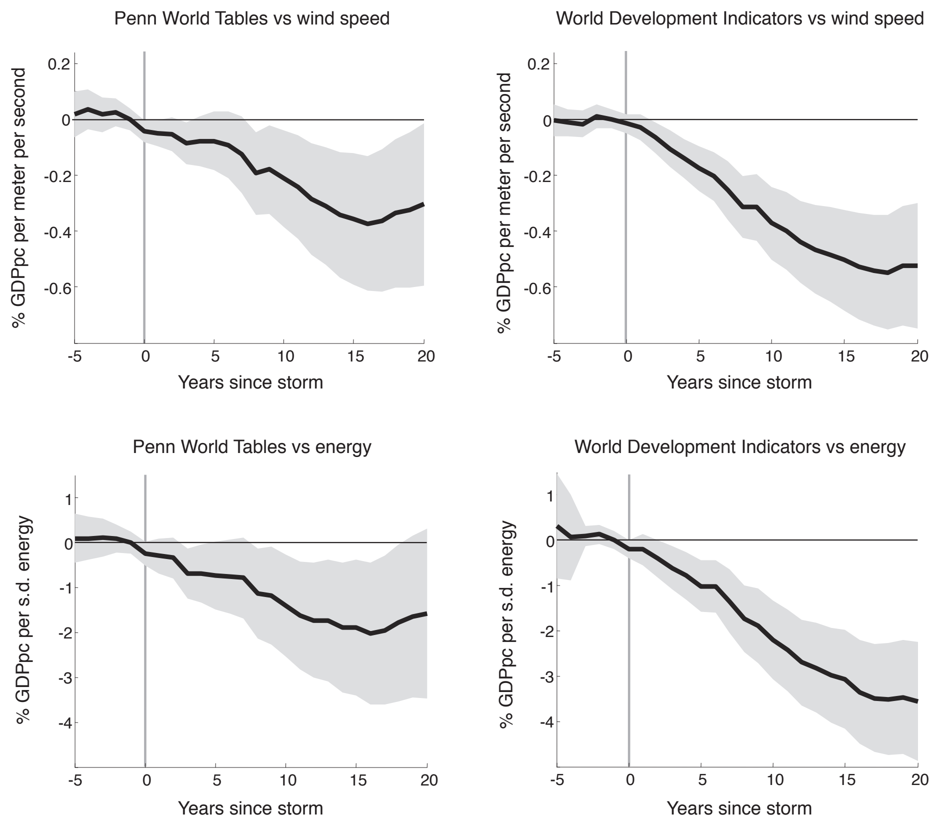

Figure 2.9: The marginal cumulative effect of tropical cyclone exposure on long-run GDPpc growth. A zero-effect would indicate that a country follows its baseline trajectory after it was exposed to a cyclone. Each panel uses a different pairing of dependent variable data source and a different measure of cyclone exposure. 95% confidence intervals (robust to spatial and serial correlation) are shaded. Appendix Table 2.9 reports exact estimates.

structure of this result. Figure 2.10 presents the results as individual year lags (along with a 5-year moving average). We see that, following a cyclone disaster, the *instantaneous growth rate of GDP* declines, and stays suppressed for the following 15 years. In the year that a cyclone strikes, a large decline in growth rates is observed. The following years do not appear to show a systematic decrease, but between 5 and 15 years after a strike, we see a persistent suppression of growth rates. After year 17, growth rates appear to stabilize near the pre-disaster levels and may show some sign of exceeding them. However income levels remain permanently lower than the pre-disaster trend line due to a decade of suppressed growth. The “no recovery” hypothesis (Figure 2.1) describes the true behavior of GDP following a cyclone disaster.

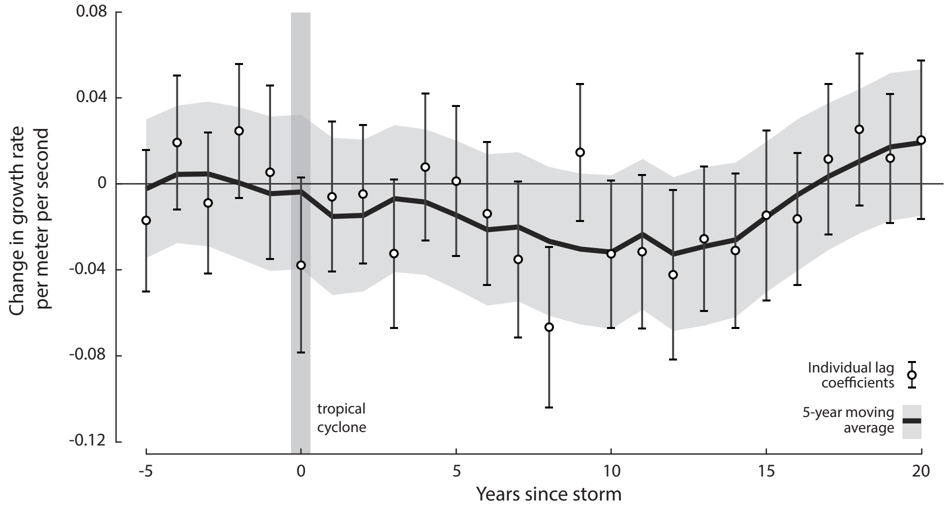

Figure 2.10: The marginal effect of tropical cyclone exposure on annual GDPpc growth rates. Individual lag effects are shown with 95% confidence intervals, along with a 5-year moving average of effect and confidence interval values. Lag effects here correspond to the first difference of values in figure 2.9. Though noisy, estimates indicate that growth rates are suppressed in years following a tropical cyclone strike.

Robustness of the main result

We check the robustness of this result by using alternative data sets, alternative specifications, randomization tests, subsampling of our data, and spatial lag models.

Data selection We replicate our main finding using the WDI, our alternative measure of GDP, and energy, our alternative measure of cyclone exposure. The remaining panels of Figure 2.9 presents these alternative estimates. Under all four pair-wise combinations of the data, we obtain essentially the same result, although estimates using WDI as the dependent variable tend to have smaller standard errors. We present exact parameter estimates for several lags in Appendix Table 2.9[22] using all four pairs of data, noting that if the effect sizes are standardized, wind speed produces estimates that are 33% larger than those using energy, although they are not statistically different from one another and both are statistically different from zero. We also note that the estimated effect one year after exposure is 30–50% smaller if the WDI data file is used instead of the PWT data file, however the point estimates converge in the following year.

[22] Table 2.9 presents values from models without forward lags.

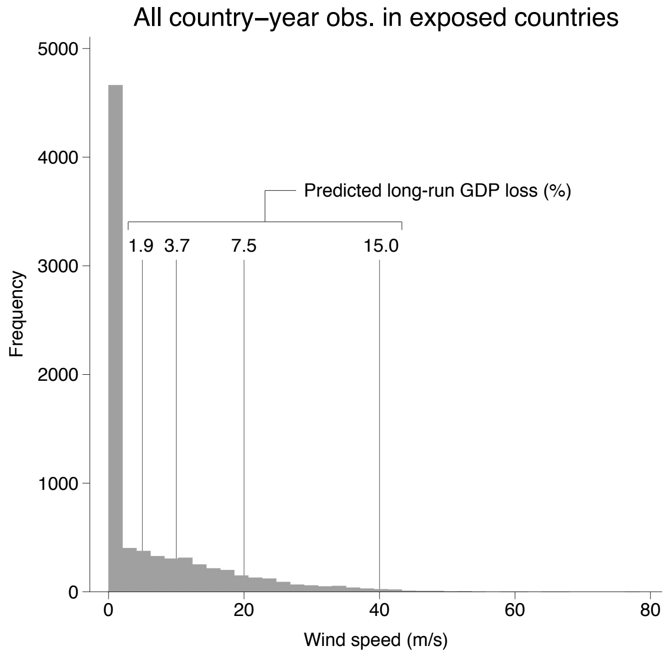

Figure 2.11: Pooled distribution of country-year tropical cyclone exposure. The expected long-run GDPpc loss associated with 5, 10, 20 and 40 m/s storm events are indicated.

Nonparametric time controls In order to produce reliable inferences, it is essential that we account for basic cross-sectional patterns and trends using country and year fixed effects. However, we continue to obtain our main result if we omit country-specific trends θ_i or if we introduce region-by-year fixed effects, as shown in columns 1 and 3 of Table 2.3. Allowing countries to exhibit independent trends in growth causes the long-run growth effects to be slightly larger than if country-level trends are omitted, however we easily reject the hypothesis that country-specific trends in growth are common across countries. Further, we find additional evidence that a model omitting country-specific trends is misspecified when we conduct a test of forward lags (leads) and find that forward lags are statistically significant (they should not be). Thus, for the remainder of the paper we rely on the model with both common year effects and country-specific trends (column 2 of Table 2.3) since it is the most parsimonious model that passes this forward lag test. Notably, all estimates of Ω are significantly different from zero when the statistically irrelevant forward lags are dropped, explaining why the tabulated standard error estimates appear different from those presented in Figure 2.9.

Randomization tests To check whether our model is mis-specified, a fact that might generate spurious or biased findings, we randomize our sample to generate false data that we then use to re-

Table 2.3: Long-run growth vs. wind speed with alterations to time controls

	(1)	(2)	(3)
Dependent variable	Growth (%) from PWT		
Independent variable	Wind speed		
Marginal cumulative effects of 1 additional m/s exposure			
5 years	-0.0944**	-0.0895**	-0.0938**
	(0.0392)	(0.0427)	(0.0456)
10 years	-0.211***	-0.223***	-0.215***
	(0.0605)	(0.0711)	(0.0731)
15 years	-0.306***	-0.378***	-0.376***
	(0.0734)	(0.0938)	(0.0986)
20 years	-0.247***	-0.374***	-0.383***
	(0.0854)	(0.113)	(0.122)
Country FE	Y	Y	Y
Year FE	Y	Y	
Region × year FE			Y
Country-specific linear trend†		Y	Y
Observations	6415	6415	6415
Adjusted R^2	0.122	0.144	0.157

Standard errors in parentheses are robust to spatial (1000km) and serial (10-year) correlation. Lagged cumulative effects of wind speed every 5 years are displayed, but effects of all years are estimated. †A country-specific linear trend with country fixed effects in the growth regression translates into a country-specific quadratic trend in cumulative growth (i.e. income).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

estimate the model in Equation 2.4.1. As an ancillary benefit, these placebo tests also allow us to check whether the asymptotic confidence intervals we use for inference are properly sized. Holding observations of GDP fixed, we randomize observations of cyclone exposure (either wind speed or energy) without replacement 10,000 times, each time re-estimating Equations 2.4.1-2.4.2. We conduct this randomization in three different ways[23]:

1. Entire sample – Randomly re-assign each cyclone observation.

2. Between countries – Randomly re-assign each country's complete history of cyclone exposure to another country while preserving the ordering of years. This preserves the time structure within the data, thereby testing whether global or regional trends might generate spurious correlations.

[23] A Stata function to implement these three randomization procedures in a generalized panel context is available at http://blogs.cuit.columbia.edu/asj2122/code/randomization-code.

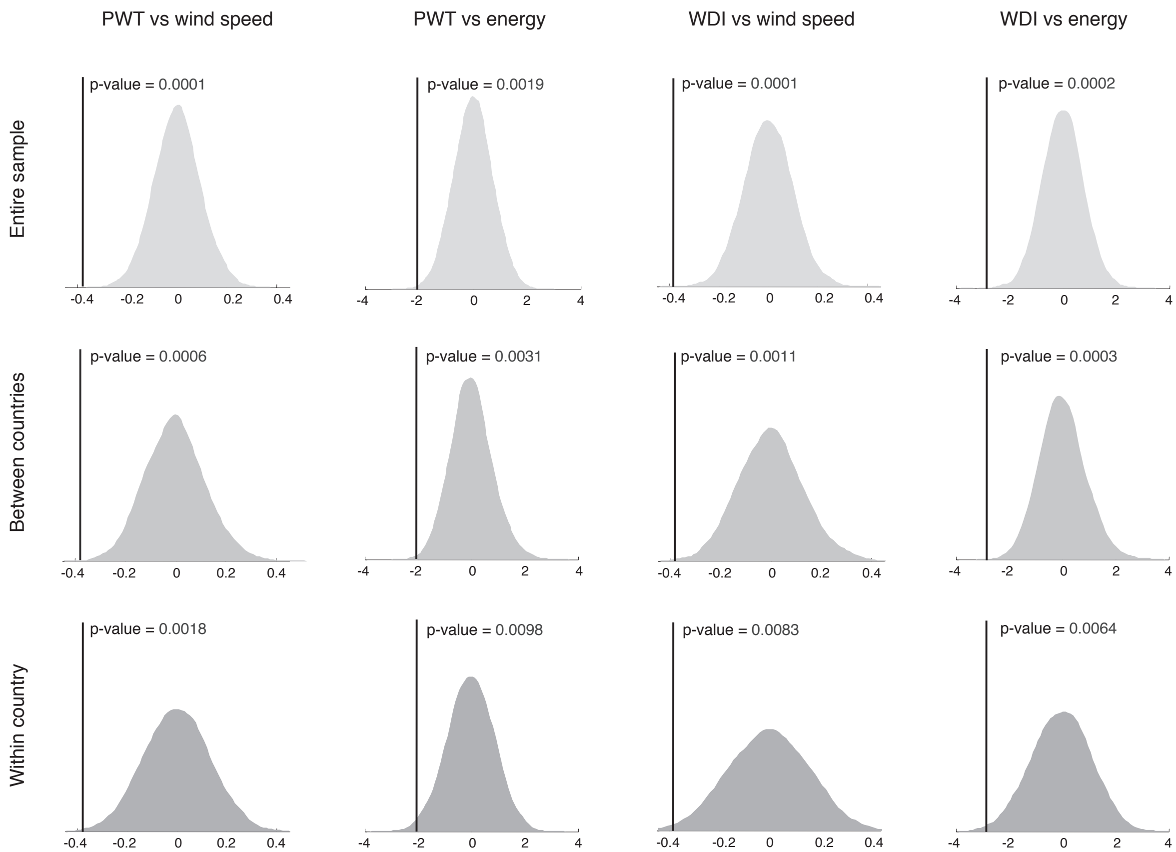

Figure 2.12: Distribution of point estimates for 15-year lag determined by re-estimating Equations 2.4.1-2.4.2 on randomized placebo datasets. Each distribution corresponds to the different dependent-independent variable pairing (columns) for one of three different randomization schemes (rows). Each distribution is constructed by repeating the randomization and estimation procedure 10,000 times. Coefficients from the estimate using real data are shown as vertical lines with exact p-values. In all 12 cases, exact p-values <0.01.

3. Within country – Randomly re-order each country's time-series of cyclone exposure while keeping it assigned to the original country. This alters only the time structure of the data, thereby testing whether time invariant cross-sectional patterns across countries might generate spurious correlations.

Figure 2.12 displays the the distribution of point estimates for the fifteenth year ($\Omega_{t=15}$) under each of these randomization schemes using each of the four pair-wise combinations of data – the figure depicts the result of 120,000 randomizations in total. Under all three procedures and all four sets of data, the distribution of point estimates are properly centered at zero, indicating that the model in Equation 2.4.1 is unlikely to produce biased results. Furthermore, the point estimate we obtained when we used the true data is plotted as a vertical line, accompanied by an exact p-value that we compute by using

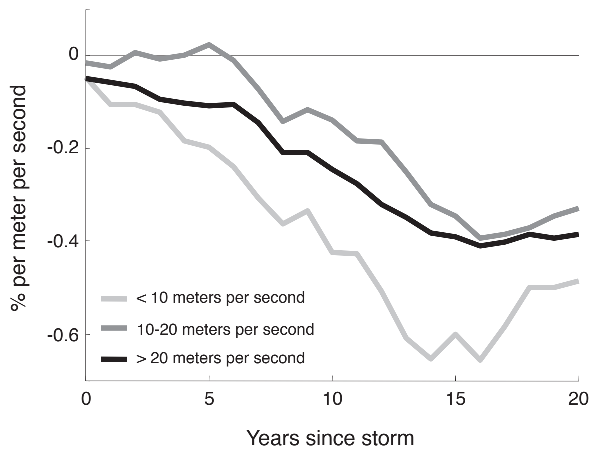

Figure 2.13: Long-run marginal cumulative effects of cyclone exposure for small (<10 m/s), medium (10-20 m/s), and large (>20 m/s) exposure levels.

the outcomes from these randomizations. In each of the twelve cases, these p-values remain below 0.01 – suggesting that our result is extraordinarily unlikely to occur by chance.

Testing for non-linearity Work by Nordhaus (2010) and Mendelsohn, Emanuel, Chonobayashi and Bakkensen (2012) indicated that direct damages from cyclones in the United States are a highly nonlinear power function of maximum wind speed at landfall, reporting "super elasticities" of 9 and 5, respectively. Hsiang and Narita (2012) use output from LICRICE at the country-by-year level to examine whether this super-elastic relationship holds generally, but instead obtain an elasticity of unity. This suggests that once the over-land trajectory of storms is accounted for, the relation is at most an exponential function. Hsiang (2014) reconciles this difference by demonstrating that the previously reported super elasticities were an artifact of assuming a power-function relationship when wind speeds are so high that their logarithm is essentially a linear function, and that parameter estimates similar to Hsiang and Narita (2012) are obtained with Nordhaus' original data if a power function is not assumed *ex ante*. Yet, when Antilla-Hughes and Hsiang (2011) use LICRICE to examine capital and income losses at the household level, they find that both are linear in wind speed. As it remains unexplained why aggregate damage estimates should be nonlinear when local loss is linear[24], it is important that we examine whether our linear model of long-run growth is justified, especially since the assumption of local linear loss was used to inform the area-averaging used to collapse pixel-level cyclone data. To test the linearity of the long-run growth effect, we separately estimate the marginal effect of cyclone

[24] Perhaps it is because estimates of direct damages contain systematic biases, since they require on-the-ground tabulation of losses which are subject to observational errors.

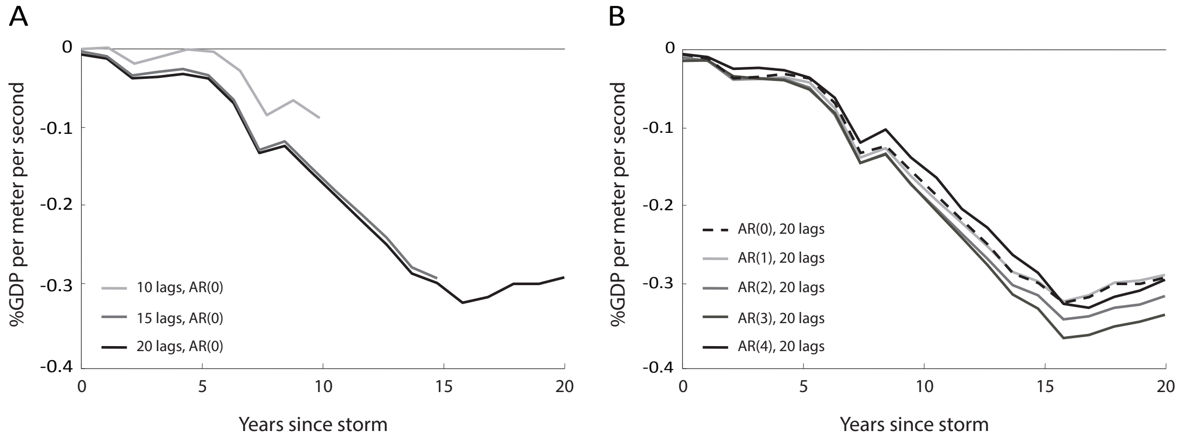

Figure 2.14: A) Long-run marginal cumulative effects estimated with 10 and 15 lags, compared to the main effect estimated with 20 lags. B) Long-run marginal cumulative effects for AR(1)-AR(4) models.

exposure within three different exposure levels of wind speed: 0–10, 10–20 and > 20 m/s. Figure 2.13 displays the long-run marginal effect of cyclone exposure for all three types of events. These estimates are somewhat noisier than earlier estimates, since the number of storm events is a subsample of the original sample and the variance in the independent variable is smaller, however the point estimates similar and we see no significant or systematic changes in these marginal effects as storm intensity grows. The long-run growth effect of cyclone exposure appears to be approximately linear in cyclone intensity.

Lag length We examine whether the maximum lag length k we select alters our result by estimating the model using 10 and 15 lags instead of 20. The results are shown in Figure 2.14A. Estimates using only 10 lags do not diverge from zero for the first five years and then are negative but smaller in magnitude. Estimates using 15 lags are essentially identical. In general, there is greater risk of including too few lags in a distributed lag model rather than too many, since unnecessary distant lags will simply appear as noise and will not bias a model but too few lags may generate bias if omitted lags important and are correlated with included lags (Greene, 2003). We thus consider the longer lag models more reliable, but note that negative—albeit smaller—effects are observable using only ten lags.

Auto-regressive controls We examine whether accounting for auto-regressive behavior in growth affects our results by including 1-4 auto-regressive controls in the model, following Cerra and Saxena (2008) and Romer and Romer (2010). Results for AR(1)-AR(4) models are shown in Figure 2.14B,

Table 2.4: Controlling for climatic variables

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Growth (%) from PWT				
Sample restrictions			Missing small islands†		
	Pooled exposed and unexposed countries				Exposed only‡
Marginal cumulative effect of 1 additional m/s wind speed					
5 years	-0.0895**	-0.0882**	-0.0166	0.00482	-0.0152
	(0.0427)	(0.0436)	(0.0511)	(0.0519)	(0.0526)
10 years	-0.223***	-0.220***	-0.163*	-0.127	-0.182**
	(0.0711)	(0.0730)	(0.0918)	(0.0895)	(0.0898)
15 years	-0.378***	-0.370***	-0.265**	-0.207*	-0.302**
	(0.0938)	(0.0964)	(0.125)	(0.123)	(0.122)
20 years	-0.374***	-0.363***	-0.236	-0.181	-0.299**
	(0.113)	(0.117)	(0.147)	(0.145)	(0.142)
Temp. (NCEP data)		Y			
Temp. (UDEL data)			Y	Y	Y
Precip. (UDEL data)				Y	Y
Observations	6415	6376	5737	5737	3232
Adjusted R^2	0.144	0.142	0.137	0.136	0.157

All models contain country fixed effects, year fixed effects, and country-specific linear trends. Temperature and precipitation are spatially averaged over each country-year in the sample and are each allowed to influence growth linearly. NCEP reanalysis temperature data is fully global in coverage. UDEL temperature and precipitation data come from a gridded reconstruction based on interpolated station data. Standard errors in parentheses are robust to spatial (1000km) and serial (10-year) correlation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ †Because UDEL data is interpolated, data for many small islands that are strongly affected by cyclones are missing, causing them to be dropped from the sample. ‡Dropping countries that are never exposed to tropical cyclones in the sample.

plotting only the direct effects captured in β and not the indirect effects through the auto-regressive process. Auto-regressive models recover results that are indistinguishable from our benchmark model, which is AR(0).

Climatological controls We examine whether time-varying climatic conditions affect our results. We are particularly concerned about climatological confounders (Auffhammer, Hsiang, Schlenker and Sobel (2013)), since the intensity and distribution of tropical cyclones are influenced by global climatic patterns that also may affect economic outcomes[25]. In column 2 of Table 2.4 we account for country-

[25]For example, the El Niño-Southern Oscillation inhibits storm formation in some regions while promoting it in others (Tartaglione, Carissa, Smith and O'Brian (2003); Camargo and Sobel (2005); Hoyos, Agudelo, Webster and Curry (2006)) while it also influences economic outcomes around the world by altering global rainfall and temperature patterns (Brunner (2002); Hsiang, Meng and Cane (2011)).

level exposure to changes in temperature, a variable that affects annual growth rates (Dell, Jones and Olken (2012)), using NCEP reanalysis data that allows us to retain all countries in the original sample. Our point estimates are unchanged relative to our benchmark model in column 1. In column 3 we control for temperature using UDEL data, a different data source, and we find that our point estimates are roughly 30% smaller and less significant, although this change is not itself statistically significant. The change in point estimates and standard errors is primarily due to our dropping 600+ country-year observations for small islands that are missing from the UDEL data[26], but we opt to use the UDEL data because it allows us to also account for precipitation[27]. Accounting for both temperature and precipitation in column 4 appears to to reduce the magnitude and significance of cyclones further, however this change in estimates is mostly driven by countries that are never exposed to tropical cyclones. When we remove countries that are never exposed to cyclones (e.g. Bolivia) but continue to account for temperature and precipitation using the UDEL data, shown in column 5, we obtain estimates that are similar in both magnitude and significance to our baseline result using the full sample.

Endogenous controls We examine whether the inclusion of some time-varying control variables that a population determines endogenously, but are traditionally included in growth regressions[28], affect our result. Including these endogenous controls is likely to be a case of "bad control" (Angrist and Pischke (2008)), since unobservables may influence these controls as well as how populations respond to cyclones[29]. Thus, we only present these results as a robustness check and do not think that they should be interpreted causally. In Table 2.5 we account for lagged income[30], population growth and trade openness, both for the full sample and the restricted sample of exposed countries (column 6). We find that our estimates are similar to our baseline result using the full sample.

Subsamples of the data In Figure 2.15 we check whether specific subgroups of countries are driving our result and find that our estimates are globally generalizable. As discussed above, we measure cyclone exposure using scale-free intensive variables, a fact that should make the physical size

[26]The UDEL data is interpolated from land-based weather stations, so many islands that do not have their own weather stations are dropped from the reconstruction.

[27]Rainfall data from NCEP is less reliable because it is driven by a model simulation.

[28]For examples, see Sachs, Warner, Aslund and Fischer (1995), Barro (1998), Sala-i-Martin (1997) and Barrios, Bertinelli and Strobl (2010).

[29]See Hsiang, Burke and Miguel (2013b) for a discussion of this issue.

[30]Careful readers may notice that the coefficient on lagged income is larger in magnitude than traditional estimates for convergence rates (Barro and Sala-i-Martin (2003)). This is because we flexibly allow for countries to have trending growth rates, which is not the standard approach in traditional models. In Appendix Table 2.10, we demonstrate that if we remove these country-specific trends from the model then we obtain more familiar estimates for convergence rates.

Table 2.5: Controlling for endogenous economic factors

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Growth (%) from PWT					
Sample restrictions	Pooled exposed and unexposed countries					Exposed only‡
Marginal cumulative effect of 1 additional m/s exposure						
5 years	-0.0895**	-0.0766*	-0.0896**	-0.0826**	-0.0689	-0.0577
	(0.0427)	(0.0430)	(0.0427)	(0.0417)	(0.0420)	(0.0412)
10 years	-0.223***	-0.225***	-0.223***	-0.202***	-0.202***	-0.182***
	(0.0711)	(0.0689)	(0.0711)	(0.0702)	(0.0681)	(0.0681)
15 years	-0.378***	-0.439***	-0.377***	-0.346***	-0.402***	-0.376***
	(0.0938)	(0.0930)	(0.0939)	(0.0930)	(0.0920)	(0.0916)
20 years	-0.374***	-0.512***	-0.373***	-0.322***	-0.453***	-0.411***
	(0.113)	(0.113)	(0.113)	(0.112)	(0.112)	(0.111)
$ln(GDPpc)_{t-1}$†		-14.52***			-14.64***	-13.99***
		(1.541)			(1.543)	(1.506)
$Pop.Growth_{t-1}$			-8.508		-9.101	0.803
			(11.30)		(10.74)	(13.97)
$Openness_{t-1}$				0.0321***	0.0357***	0.0307***
				(0.0110)	(0.0101)	(0.00982)
Observations	6415	6415	6415	6415	6415	3834
Adjusted R^2	0.144	0.206	0.144	0.148	0.211	0.226

All models contain country fixed effects, year fixed effects, and country-specific linear trends. "Exposed" countries are those countries that are ever exposed to tropical cyclones in the sample. Standard errors in parentheses are robust to spatial (1000km) and serial (10-year) correlation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. †The coefficient on lagged income is larger in magnitude than standard estimates because standard models do no account for country-specific linear trends in growth rates – to verify that standard estimates are obtained when these trends are dropped, see Appendix Table 2.10. ‡Dropping countries that are never exposed to tropical cyclones in the sample.

of countries irrelevant. We explicitly check this assumption in the first panel where we divide countries into terciles based on their surface area (km^2) and observe that their long-run growth responses are all similar to the average response (the largest countries exhibit a positive point estimate for intermediate lag lengths, although it is difficult to interpret because it is not statistically significant). In the second panel we stratifying the sample according to whether they are above and below the median income in 1970, however we find that the two groups respond almost identically. In the third panel we isolate Small Island Developing States (SIDS) and find that their response is similar to that of other countries. Finally, we examine Asia, North America and Oceania separately and find, consistent with Hsiang and Narita (2012), that the response to cyclones in these three geographic regions are similar.

We further explore the effect of country size in the long run. We reestimate our model but include

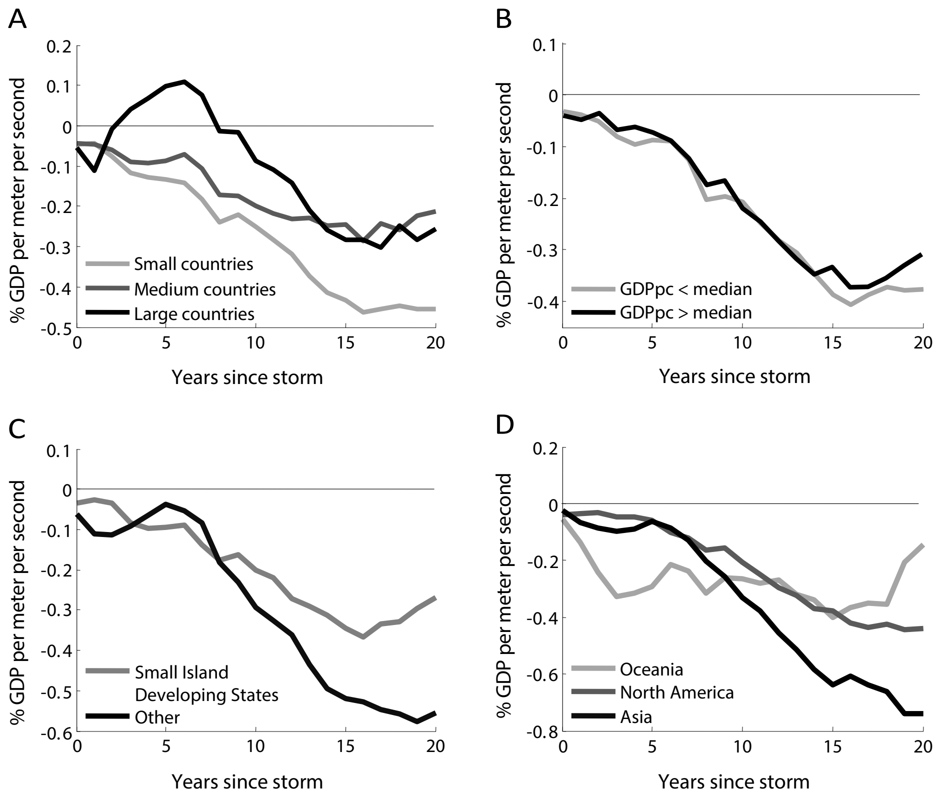

Figure 2.15: The estimated effect of cyclones on GDPpc for subsamples stratified by A) country size, B) income in 1970, C) Small Island Developing State (SIDS) status, and D) region.

flexible interactions for log of country size with cyclone exposure for every year. We plot the 15-year cumulative effect of 1 m/s wind speed exposure for five equally sized bins of log country size in Figure 2.16 as well as this cumulative effect as quadratic, cubic, and quintic polynomials of log country size. Overlaying the pooled estimate, we see that the long run effect for most county sizes falls within the confidence interval of the pooled estimate and is always negative. There is some indication that cumulative effects shrink in magnitude slightly as countries become larger, but this pattern is not significant and reverses for the highest country size bin. There are more extreme negative effects for the very smallest countries and the very largest countries in the quintic polynomial model, but the most extreme effects do not show up in the binned model as they are driven by outlier cases. The marginal effects estimated at any location in the size distribution are not significantly different from the pooled estimate. This is a weak test because the sample is thin (these point-wise effects are also not statistically different form zero), but the structure of the point estimates indicate that the pooled sample estimate is a good approximation for the behavior of the sample in general and that

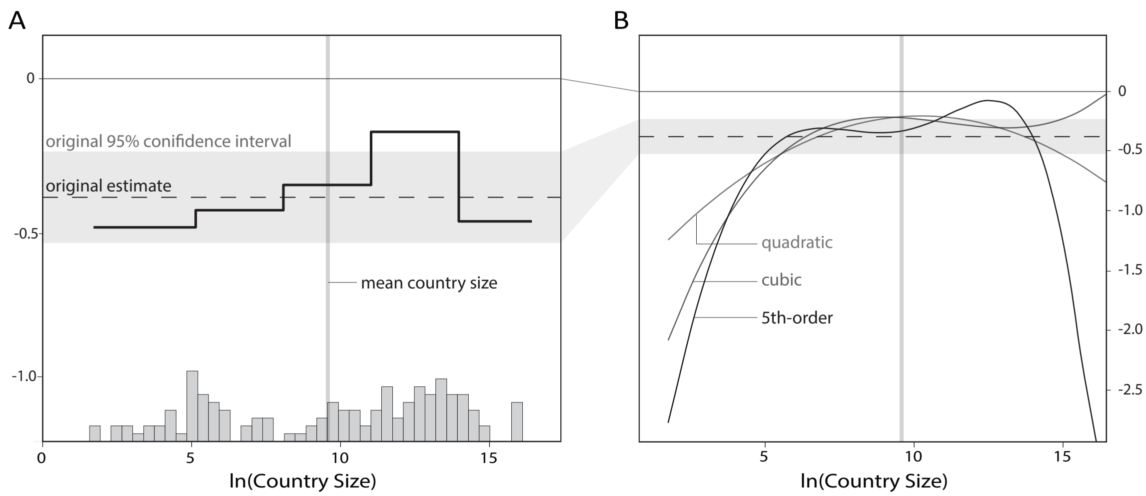

Figure 2.16: Non-linear interactions with country size. A) Effect of the 15-year lag term on growth estimated as an interaction with log country size in 5 discrete bins. Original estimate and 95% confidence interval are shaded. B) 15-year lag term on growth with country size interacted as quadratic, cubic, and 5th-order polynomials. Original estimate and 95% confidence interval are shaded.

our scale-free collapse of cyclone exposure is a reasonable approach for comparing countries of different sizes[31].

Spatial lag models We examine whether cross-country spillovers within a region drive our result. It might not be the case that cyclones are bad for growth but instead being nearby a cyclone is *good* for long-run growth, e.g. additional moderate rainfall might be beneficial (Barrios, Bertinelli and Strobl (2010)) or tourists may change their behavior if a cyclone strikes a potential destination[32] (Hsiang (2010)). We account for these spatial spillovers using a spatial lag model (Cressie and Wikle (2011)) where i's growth is affected by i's cyclone exposure plus all temporal lags in the average exposure of neighbors j whose centroids fall within concentric annuli (around i's centroid) with 400km widths[33] out to a maximum distance of 2000km. Figure 2.17A displays an example of these annuli around Haiti. This spatio-temporal distributed lag model is extremely flexible and has 120 lags estimated simultaneously: 20 temporal lags for each of 5 annuli plus each country's own set of 20 lags (the zero radius annulus). Coefficients describing how cyclone exposure 15 years prior at various distances affect a country's growth are plotted in Figure 2.17B, where the effect at "0 km" is the effect on i of i's own cyclone exposure. We find that there is essentially no effect of cyclones at distances greater than 400

[31]We thank Benjamin Jones for this suggestion.
[32]We thank Wolfram Schlenker for this suggestion.
[33]This distance was chosen based on the average size of a single storm, which is on the order of 200km.

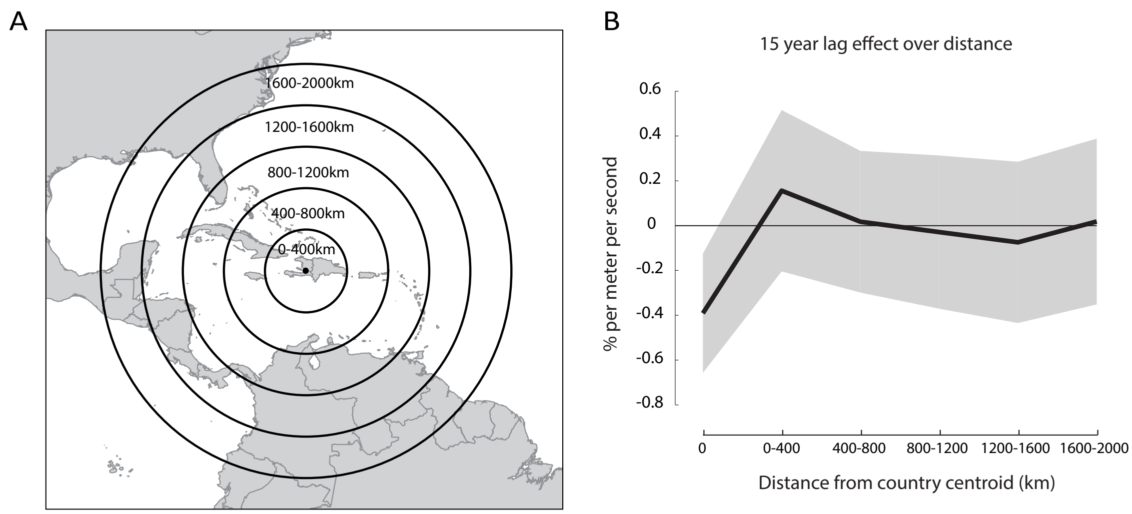

Figure 2.17: Spatial lag model illustrating the potential spillover effects of tropical cyclones. A) Example of the annuli used to construct spatial lags used in the model. B) Estimates of the 15-year lag term for the growth effect of tropical cyclone exposure for all spatial lags, i.e. the effect on i's growth when j at a distance d_{ij} is exposed to cyclones. d_{ij} is binned using annuli as in (A) and plotted on the horizontal axis. 95% confidence intervals are shaded.

km and there is suggestive but insignificant evidence of a modest positive spillover among immediate neighbors. Importantly, the estimated effect of a country's own exposure remains essentially unchanged from the baseline model.

Other long-run macroeconomic impacts of disaster

Having demonstrated that the long-run GDP response to cyclones is economically large, robust and generalizable, we check whether other macroeconomic variables exhibit similar behavior. To do this, we estimate analogs to Equations 2.4.1-2.4.2 replacing GDP with other microeconomic variables. We plot the long-run effects Ω in the panels of Figure 2.18. Overall, the behavior of these alternative macroeconomic measures broadly corroborate our main finding that cyclones adversely affect long-run income growth.

Sources of income In Figure 2.18A, we decompose GDP into income from agriculture, industry and services to examine how each responds to tropical cyclone exposure. All three types of income decline gradually, exhibiting long-run effects that are not statistically different from the long-run effect of total GDP (dashed line). Long-run declines in agriculture and services are similar to total GDP,

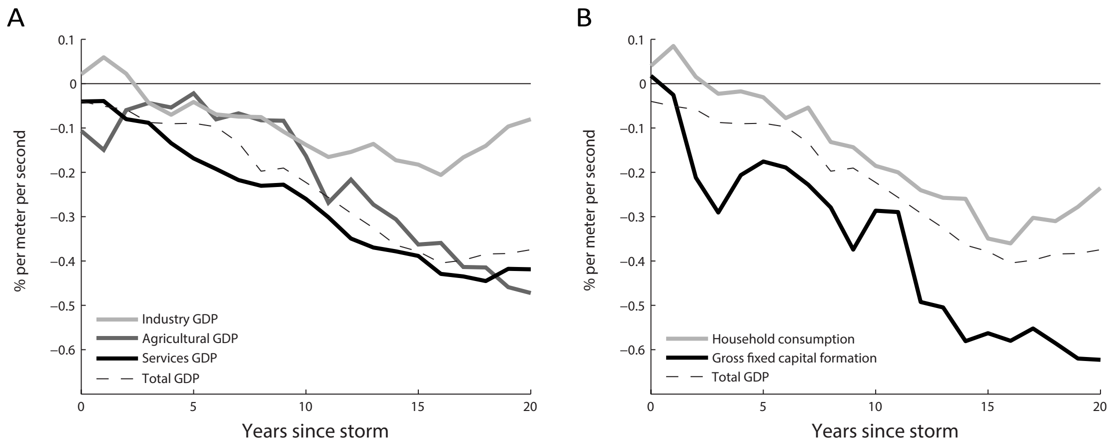

Figure 2.18: A) The estimated effect of cyclones on different components of GDP and B) on consumption and capital formation. Dashed line is the effect on total GDP for comparison.

however the long-run losses in industry are roughly half the size (in percentage points) of total GDP losses. Industry might suffers less because it requires a high spatial-density of capital, and thus firms face a stronger incentive to invest in capital protection (Hsiang and Narita (2012)), but we lack the data to test this hypothesis.

Consumption and Investment If populations insure themselves, they may smooth their consumption to insulate themselves from transient income losses[34] (Udry (1994); Townsend (1995); Kunreuther and Michel-Kerjan (2009)). However, long-run income losses to cyclones are persistent and exhibit no recovery, so insurance and savings mechanisms aimed at long-run income will be unsustainable, giving populations no choice but to lower their consumption to match their long-run income. In Figure 2.18B, we observe this pattern: the magnitude and dynamics of the consumption response closely matches that of the income response. Figure 2.18 also shows that long-run gross capital formation (investment) declines in the wake of cyclones, again matching the response of income.

Trade Cyclone incidence and the resulting long-run loss of GDP does not generate an obvious prediction for trade patterns – for example, a disaster might increase imports of capital used in rebuilding efforts, but the observed decline in GDP might also reduce the demand for costly imported goods. The latter probably explains the true response better, since as we show in Figure 2.19, long-run imports

[34]It is worth noting that recent evidence from the Philippines indicates that households struggle to smooth consumption across the transient component of cyclones-induced income losses (Anttila-Hughes and Hsiang (2011)). Anttila-Hughes and Hsiang hypothesize that this is due to the spatially-coherent nature of these income shocks, which makes them more difficult to insure than spatially-uncorrelated, idiosyncratic events (Townsend (1995)).

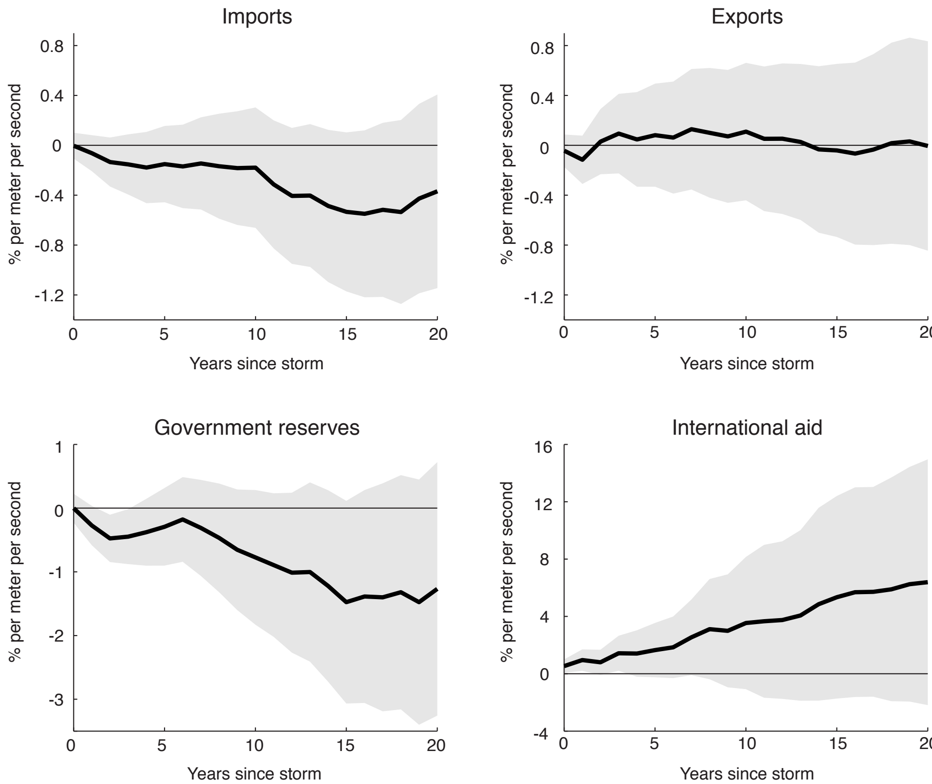

Figure 2.19: The estimated effect of cyclones on long-run growth of imports, exports, government reserves and international aid. 95% confidence intervals are shaded.

fall at roughly the same rate as income. In contrast, we find no long-run effect on exports. Such an asymmetric trade response is consistent with demand-driven models of trade, since cyclone events should have no effect on distant economies that consume exported goods[35].

Government reserves Government reserves decline in the long-run (Figure 2.19); however, the magnitude of this decline is much larger than the long-run income loss: for each addition 1 m/s in cyclone exposure, government reserves decline by more than 1% in the long run. The long run effect on reserves is only marginally significant, but a differentially rapid depletion of government reserves is unsurprising since governments provide disaster relief and absorb many uninsured losses, generally without expanding their revenue (Kunreuther and Michel-Kerjan (2009), Deryugina (2011)).

[35] It is worth noting that an economically small, temporary and statistically insignificant decline in exports does occur just following a cyclone strike. Perhaps this occurs because export infrastructure is damaged or because domestic production of exports temporarily declines, similar to the temperature-related findings of Jones and Olken (2010).

International Aid A substantial literature has examined the size, structure and political economy of domestic and international relief aid immediately following disasters (Garrett and Sobel (2003), Achen and Bartels (2004), Eisensee and Stromberg (2007), Stromberg (2007), Yang (2008), Healy and Malhotra (2009), Deryugina (2011)). However, to the best of our knowledge, no study has examined whether disasters generate long-run impacts on international non-relief aid payments. If international donors redistribute wealth based on international differences in income, then a gradual reduction of income should have the secondary impact of gradually increasing international transfers to the affected country. In Figure 2.19, we display that this intuition is consistent with the data, further supporting our main results: in the decades following a cyclone, international non-relief transfers gradually but permanently rise relative to their counterfactual trajectory[36]. Understanding this phenomenon will be pursued in future work.

2.5.2 Evidence of adaptation to disaster-prone environments

As illustrated in Figure 2.5, the risk of tropical cyclone exposure varies dramatically. Theory predicts that in countries where the cyclone climate is intense, populations will find it beneficial to invest in protective measures (Hsiang and Narita (2012)). Thus, to further support our main result that cyclones reduce long-run growth, we examine whether our estimated long-run GDP response exhibits patterns of adaptation that are consistent with economic theory.

Optimal Adaptation in Theory Assume that countries can exert costly adaptive effort e to reduce their long-run losses in the event that a cyclone strikes[37]. If the cost function for e is convex, then populations will exert adaptive effort until the marginal cost of additional effort equals its expected marginal benefit. The benefit of this adaptive effort is determined by a country's cyclone climate, because effort only provides benefits when a cyclone actually strikes, so countries that experience more intense or more frequent cyclones should have greater returns to adaptation. Thus, we expect that countries endowed with more intense cyclone climates will invest more in costly adaptation, reducing their marginal long-run losses when a cyclone strikes. Denoting a country's optimal level of adaptive

[36] Deryugina (2011) described an analogous phenomena for non-disaster domestic transfers to counties within in the United States, where unemployment payments increase for ten years after a cyclone strikes. Here we document the appearance of a similar phenomena that lasts two decades in the "international social safety net."

[37] For example, governments could build seawalls or invest in early-warning systems.

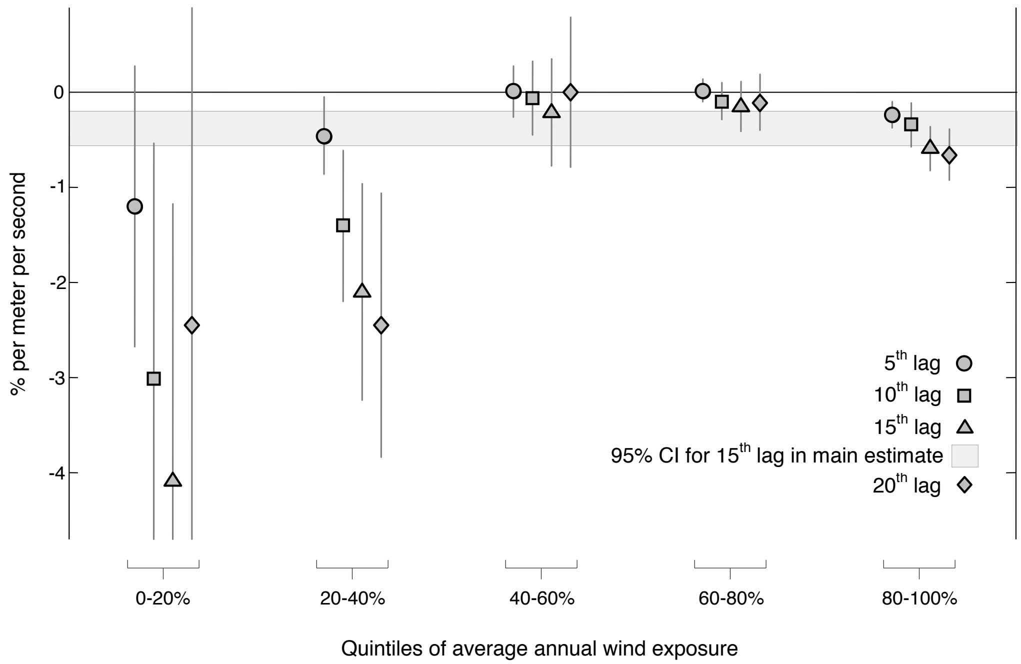

Figure 2.20: The cumulative GDPpc response to TC exposure, stratified by countries' cyclone climate (defined as average exposure over all years). For each quintile, four lagged effects are shown (5-, 10-, 15-, and 20-year effects). Vertical lines represent the 95% confidence intervals. The grey horizontal bar shows the confidence interval of the 15th-lag in our main (pooled) estimate.

effort e^* and income Y, the above logic predicts

$$\frac{\partial e^*}{\partial \bar{\bar{S}}} > 0 \tag{2.5.1}$$

where $\bar{\bar{S}}$ is expected storm exposure, a summary statistic for a location's typhoon-climate[38]. Unfortunately, we cannot directly observe whether this is true because we do not observe e^*. However, increasing effort reduces marginal long-run losses ($-\partial Y/\partial S$) to a fixed level of actual cyclone exposure $\bar{S}$

$$\frac{\partial}{\partial e} \cdot \left(-\frac{\partial Y(e)}{\partial \bar{S}}\right) < 0. \tag{2.5.2}$$

This enables us to infer that adaptation is occurring if we see that marginal losses decline as climates intensify. Assuming populations optimize and noting that $\Omega = \partial Y/\partial \bar{S}$, we can multiply Equations

[38]Hsiang and Narita (2012) demonstrated that average exposure was an approximately sufficient statistic for the incentive to adapt in the context of direct aggregate damages.

2.5.1 and 2.5.2 to obtain

$$\frac{\partial}{\partial \bar{\bar{S}}} \cdot \frac{\partial Y(e^*)}{\partial \bar{S}} = \frac{\partial \Omega(e^*)}{\partial \bar{\bar{S}}} > 0 \tag{2.5.3}$$

a result that we can investigate empirically. For a more complete treatment of optimal adaptation to tropical cyclone climates, as well as additional empirical evidence, we refer readers to Hsiang and Narita (2012).

Cross-Sectional Evidence of Adaptation We test Equation 2.5.3 by examining whether cyclone-induced losses vary with the climatological endowment of different countries. To do this, we stratify our sample of countries into quintiles according to their average level of cyclone exposure $\bar{S}$. We then estimate Equation 2.4.1-2.4.2 for each quintile separately and display the marginal long-run growth effect of disaster ($\partial\Omega(e^*)/\partial\bar{S}$) in Figure 2.20. Consistent with Equation 2.5.3, the marginal effect of cyclone exposure becomes more positive (declining in magnitude) as the average risk of exposure increases. The effect of disaster on the quintile with lowest risk (0–20%) is the most negative, while the effect on the second quintile (20–40%) is less negative and the effect on the three quintiles with highest risk (40–100%) is closest to zero. These three quintiles with high cyclone risk all exhibit responses that are statistically indistinguishable from the average effect presented throughout this study (grey stripe) and the magnitude of the "naive" response among poorly adapted populations (in the first quintile) is roughly eight times larger. Taken together, these findings support the hypothesis that populations adapt to cyclones, bolstering our main thesis that cyclones adversely affect growth since there would be no incentive for populations to adapt if cyclones were benign.

It is important to note three important features of this adaptive response. Firstly, unadapted countries, though having larger marginal income losses due to tropical cyclone exposure, experience far fewer storms. This implies that total losses over the sample period are likely smaller in these countries than in frequently exposed countries. Secondly, even though heavily exposed populations appear to adapt extensively compared to "naive" populations, these heavily exposed populations continue to suffer losses that are both economically large and statistically indistinguishable from the average response presented in Figure 2.9. This implies that the average effect presented in Figure 2.9 describes the effect of cyclones on highly adapted and regularly exposed populations[39], so it is a good approxi-

[39] The average effect presented throughout the study is dominated by the response of high risk countries because those

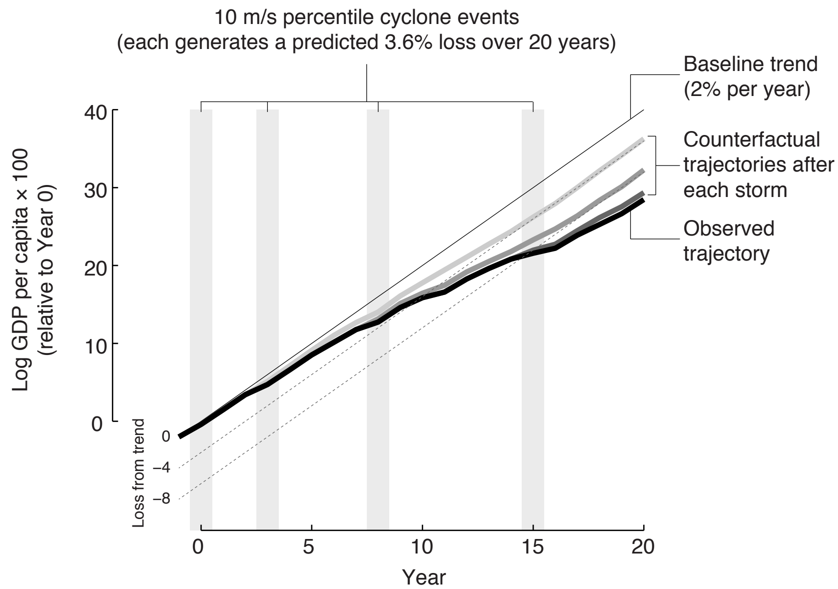

Figure 2.21: An example GDPpc trajectory in the presence of repeated cyclone exposure. We set exposure to 10 m/s in years 0, 3, 8, and 15. The penalty to income increases with each event, leading to a substantial divergence from the pre-disaster trend in income growth. The counterfactual trajectories that the country would have followed, had exposure stopped after each of the first three storms, are grey. The observed trajectory (black) appears as an almost-smooth line with an average growth rate that is depressed relative to the pre-disaster baseline.

mation for the average economic impact of most cyclones observed on the planet. Finally, no countries undertake "complete adaptation" by driving their marginal damages to zero despite the fact that populations currently exposed to cyclones have been similarly exposed for centuries. If one assumes that populations are well-informed, this would indicate that the net benefit of additional adaptation effort is zero, or very low, given each country's current equilibrium level of adaptive effort $e^{\star}$, suggesting the cost function in e is likely convex (Hsiang and Narita (2012)).

2.5.3 The effect of cyclone-prone climates on long-run economic development

Up to this point, we have identified the long-run growth effect of cyclones using a within-country estimate that relies only on each country's year-to-year variation in cyclone exposure. The country fixed effects of our model absorb all cross-sectional differences in cyclone climates and growth, preventing these average differences from influencing our estimates. However, as discussed in the last section and illustrated in Figures 2.5 and 2.6, there are strong cross-sectional differences countries' average cyclone

countries experience more cyclone effects during the period of observation.

exposure: some countries are regularly struck by strong cyclones while other countries are rarely hit. How do the long-run growth effects that we estimate above interact with these cross-sectional patterns in countries' geographic endowments to influence their long-run economic development?

If a country is repeatedly hit by cyclones, that country will continuously accumulate growth penalties that can substantially alter that country's income trajectory. Figure 2.21 illustrates this process by demonstrating how the effect of sequential storms add up for a single country. Each storm has a long-run effect that permanently alters a country's growth trajectory, and any storms that follow further lower that country's long-run growth. Had the "build back better" or "recovery to trend" hypotheses been true, then the effect of sequential storms would be smaller or would vanish, since later storms would either replace or offset the effects of earlier storms. However, because national incomes exhibit "no recovery," the effect of sequential storms simply add to one another, creating an income penalty (relative to the trend) that grows monotonically with time.

Two aspects of this process make it particularly insidious for detection by analysts, possibly explaining why this effect has not be characterized in earlier studies. First, the long-run growth response of an individual storm onsets very gradually (recall Figure 2.9), so detecting the cumulative effect of cyclones by visually examining GDP time-series should be nearly impossible. Consider Figure 2.21: four large storms ($>$ 1 s.d.) strike over the course of two decades, however the observed trajectory of GDP appears smooth and almost perfectly linear. There is no "trend-break" of otherwise abrupt movement in GDP that would attract the attention of an analyst, a problem that would only be compounded if realistic noise were added to this figure. Second, the cross-sectional variation in average cyclone exposure is correlated with many other confounding static variables, such as latitude or distance to coasts, so it is unlikely that any cross-sectional regression of average growth rates could alone reliably identify the long-run growth effect of a country's stationary cyclone climate. Together, these two facts make it very difficult to detect the effect of cyclone climates on long-run development using analytical methods other than the deconvolution that we employed here. Nonetheless, given the strength of the results above, we have no choice but to conclude that for those countries which are regularly or perpetually exposed to cyclone disasters, a new permanent reduction in long-run national income is quietly suffered each time a storm strikes, accumulating on top of similar historical penalties and causing these countries to grow slower than they otherwise would.

Simulating alternative development trajectories The tropical cyclone climate of each country is stationary, preventing us from directly identifying the effect of a *cyclone climate* on average growth

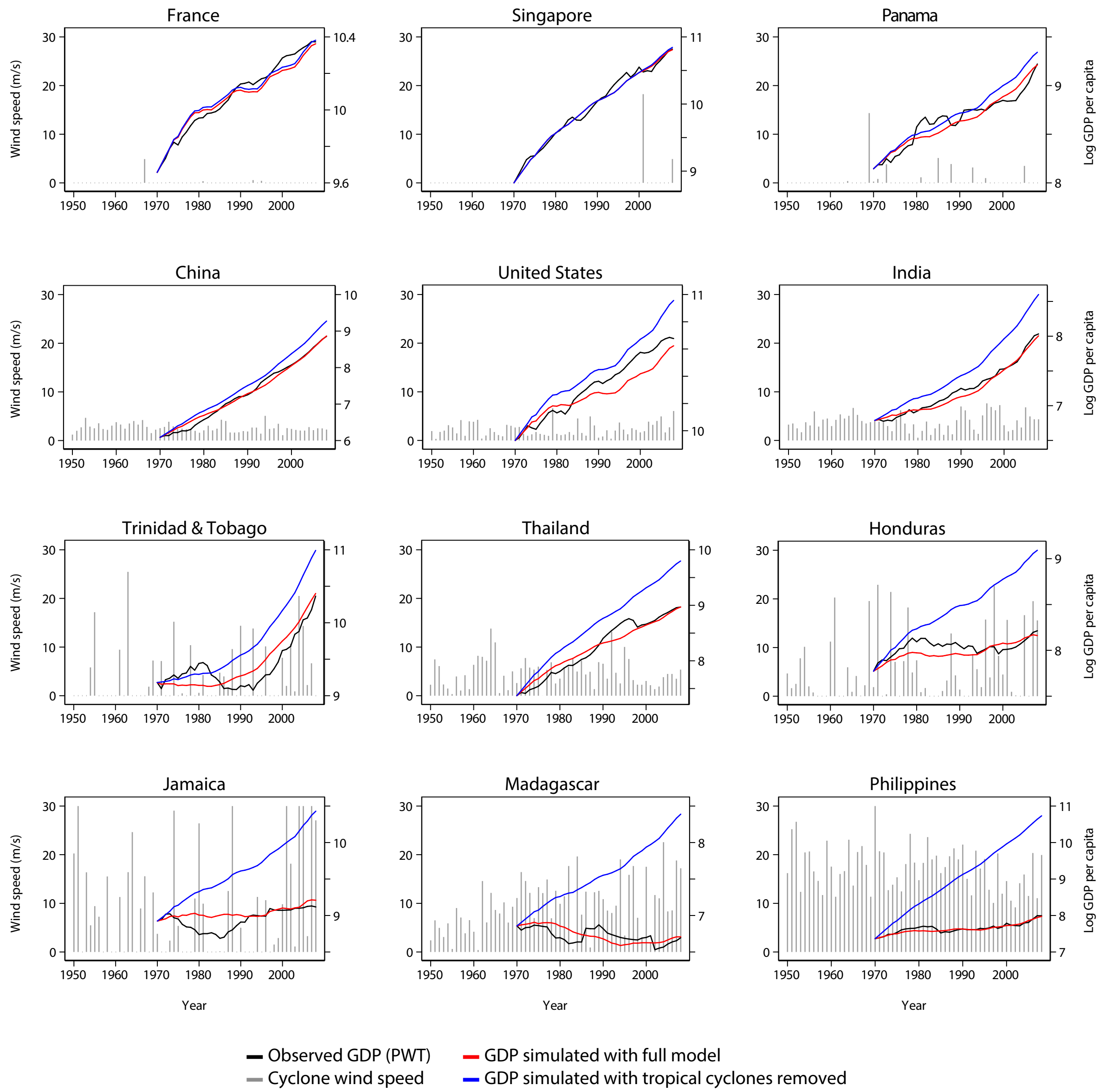

Figure 2.22: Simulations of log GDPpc growth with (red) and without (blue) tropical cyclone exposure. Observed log GDPpc is black and cyclone exposure in each year are vertical grey lines. Countries are presented in ascending order based on their average tropical cyclone exposure, from low (France, top left) to high (Philippines, bottom right). The difference between the slopes of the red and blue simulations gives an estimate of the partial-equilibrium growth effect of observed tropical cyclone exposure in comparison to a counterfactual "no storm" world (which is never observed). India and Trinidad & Tobago represent the median tropical cyclone climates within the sample of countries that are ever exposed. Plots for all countries are shown in Appendix Figures 2.26-2.27

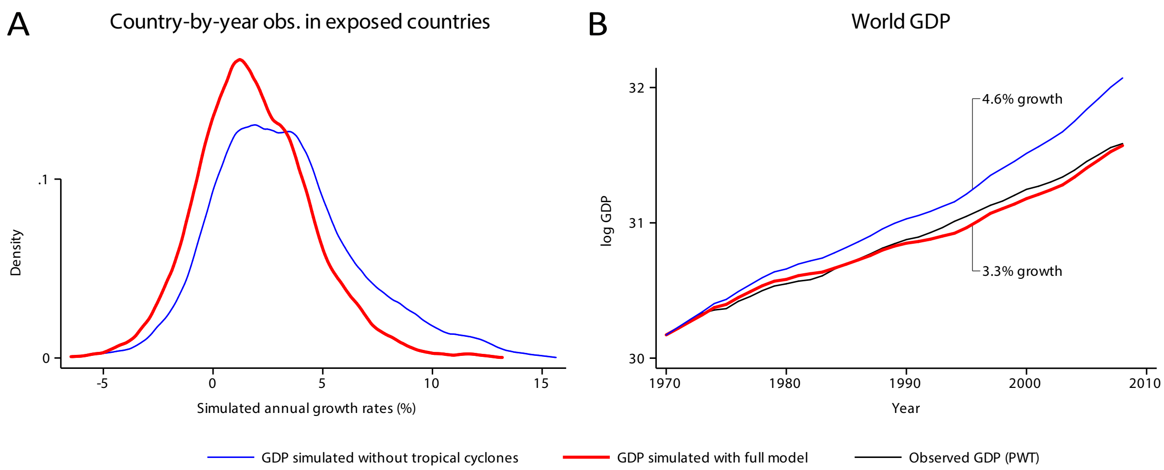

Figure 2.23: A) The global distribution of predicted annual growth rates for exposed countries using the complete growth model (red) and the model where historical cyclone exposure is removed (blue) during 1970-2008. B) the observed trajectory of World GDP (black, 3.5% growth) and simulated trajectories with and without tropical cyclones included in the model. Japan, Taiwan and Hong-Kong are not included in these plots (see text).

rates in the presence of other omitted geographic variables – however we can use our inter-temporally-identified estimate for the effect of an *individual cyclone* to estimate the cumulative influence of each countries' climate (repeated exposure to individual cyclones) on its average rate of growth. To estimate the partial effect of each cyclone climate on long-run development, we use our parameter estimates to compute how each country's income trajectory (starting in 1970) would have looked had its cyclone exposure been fixed at zero since 1950. This approach is simplistic, since it assumes that nothing else in a country changes when all cyclones are removed, but it is a useful benchmark since it provides us with a sense of scale for the overall effect of each countries' cyclone climate. To remove the effect of all cyclones from historical growth, we preserve the coefficients from our baseline model (Equation 2.4.1) but eliminate the tropical cyclone terms. Letting $\mathbf{S_{i,t}}$ be the vector of cyclone exposure for years t to $t-k$, we rewrite Equation 2.4.1

$$\Delta \ln(GDP)_{i,t} = f(\bar{\mathbf{S}}_{\mathbf{i,t}}) + g_{i,t} + \epsilon_{i,t}, \tag{2.5.4}$$

to make clear that our model is additively separable in the cyclone-related terms contained in $f(.)$ and "everything else" contained in $g_{i,t}$: country fixed effects, year fixed effects and country-specific trends. Using our parameter estimates, we predict "actual" historical growth for each country by using all the terms of Equation 2.5.4. We then construct a "cyclone-free" growth history for each country by setting $\bar{S}_{it} = 0$ for all observations[40] while keeping g unchanged and predicting annual growth again. Using observed incomes in 1970 as initial conditions, we can integrate both "actual" and "cyclone-free" income trajectories using these two alternative growth histories. For each country, the difference between these two trajectories represents our estimate for the partial effect of that country's tropical cyclone climate.

Cyclone climates and global economic development

Figure 2.22 displays the simulated "actual" income trajectory using the full model (red) and the "cyclone-free" model (blue), overlaid with the observed trajectory of income (black) for twelve example countries that face a variety of cyclone climates[41] (cyclone events are grey). In countries endowed with very weak cyclone climates (eg. France and Singapore), removing storms has almost no effect on the model's prediction for long-run income: both the full and truncated model essentially predict identical trajectories that both mirror the true trajectory. However, as cyclone climates become progressively more intense, the long-term trajectories for income begin to diverge. The "cyclone-free" model invariably predicts higher incomes because cyclones negatively influence growth, but the magnitude of this divergence depends strongly on the cyclone climate. For countries endowed with median cyclone climates, India and Trinidad & Tobago, models with and without cyclone effects forecast income levels that differ by roughly fifty log-points (65%) after an integration period of thirty-nine years (1970-2008). In countries endowed with stronger cyclone climates, such as as Thailand or Honduras, removing cyclones from the model increases final incomes by about one-hundred log-points (172%) during the same integration period. In some countries that exhibit almost no actual growth, such as Jamaica, or negative growth, such as Madagascar, the removal of cyclones from our model generates forecasts for moderate rates of positive growth. Finally, in countries endowed with extremely intense cyclone climates, such as the Philippines, our simulations suggests that growth is slowed dramatically: the cyclone-free model exceeds the full model by 300 log-points (2,000%) after the thirty-nine year integration period. This effect of removing Filipino cyclones is one of the most extreme cases, equivalent to raising the average annual growth rate in the Philippines by roughly 7.3 percentage points, and would cause growth in the Philippines to match that of its near neighbor China. For all countries in the simulation, we list the estimated effect of their cyclone climate on their average growth rate in

[40] Effectively dropping $f(.)$ from the model.

[41] Equation 2.5.4 predicts the long term evolution of income well (when all terms are retained), regardless of the cyclone climate in each country. Results are presented for all countries with non-missing 1970 GDP in Appendix Figures 2.26-2.27.

Appendix Table 2.11.

We summarize the total effect of cyclones on global economic growth in Figure 2.23. Panel A plots the distribution of annual country-by-year growth rates with and without cyclones included in the model. When cyclones are removed, the distribution of growth rates shifts upwards, with the mean increasing from 2.01% per year to 3.80% per year. Importantly, we remove Japan, Taiwan and Hong-Kong from this exercise since their growth rates become so high ($> 13\%$) that it seems unlikely that they could plausibly sustain such high growth rates, since factors unrelated to cyclones are likely to limit output growth in other ways. Collecting results across the remaining 107 countries for which GDP data in 1970 exists (63 of which are ever exposed to cyclones), we compute the trajectory of World GDP during 1970-2008, using both the full and cyclone-free simulations. The results are displayed in Figure 2.23B. Using the full model, World GDP grows at 3.28% annually, near the 3.55% growth rate that was actually observed. When cyclones are removed from the simulation, World GDP grows 4.56% per year. Differencing the trajectories of the two simulations suggests that World GDP has been growing 1.27% slower (95% confidence interval = [1.08, 1.47]) than it would in a "counterfactual" world with no cyclones.

Cyclone climate as an explanatory variable in cross-country comparisons of growth

There are large differences in the tropical cyclone climates that countries are endowed with, and our simulations suggest that cyclones can have a large impact on average growth rates in countries that are repeatedly exposed to them. Thus, we can ask how much of the cross-country variation in average growth is explained by cross-country variation in cyclone climates. To explore this question, we compute the average annual growth rate in simulations with and without cyclones – the difference between these two numbers is growth that is "missing," which we attribute to each country's cyclone climate[42]. In Figure 2.24 we examine three regions where cyclones are prevalent, adding each country's "missing" growth to its historically observed growth, allowing us to visualize how the distribution of growth rates might change if cyclones had not affected these countries (also see Appendix Figure 2.28). If cyclones explained all of the cross-country differences in growth rates within each region, then we would expect all countries within a region to have the same growth rate once we accounted for the cyclone growth penalty. We do not observe this, indicating that cyclones are only one of many factors that probably influence growth – however, we do observe that the distribution of growth rates within

[42] The annual average growth rate in the full simulation is equal to the observed annual average growth rate. This is because the sample average is equal to the average prediction in OLS.

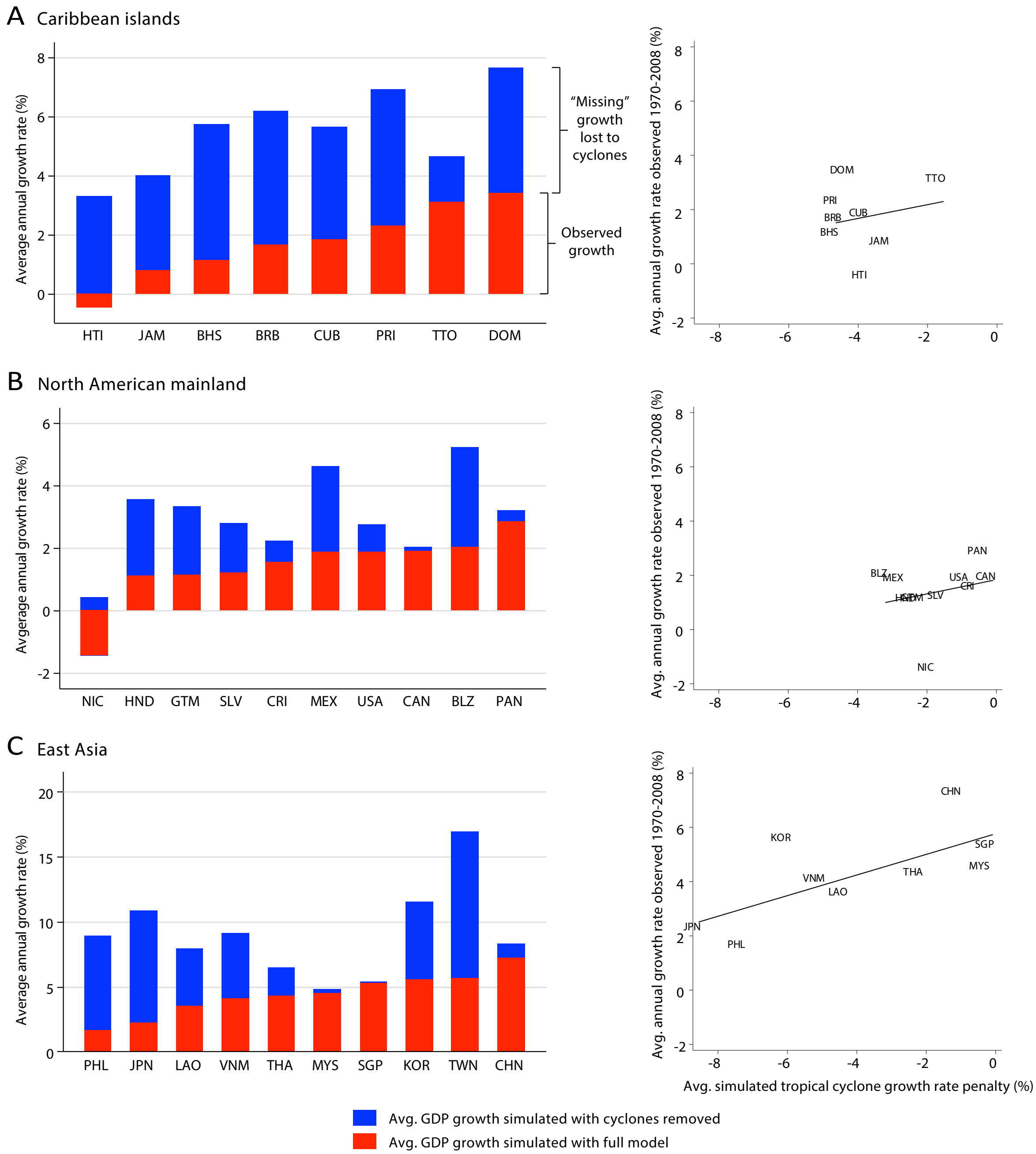

Figure 2.24: Left column: Average annual growth rates as observed historically (red bars, equal to simulated growth with full model) and average growth in simulations where cyclones are removed (blue bars). The difference in the height of the bars is the "missing" average annual growth loss to cyclones. See also Appendix Figure 2.28. Results for all countries in the simulation are tabulated in Appendix Table 2.11. Right column: Within-region cross-sectional regressions of average annual growth rates as historically observed against the growth penalty (i.e. "missing growth") attributable to each country's tropical cyclone climate. Table 2.6 reports coefficients and pooled estimates. Taiwan is omitted because it is an extreme outlier.

Table 2.6: Cyclone climate as a predictor of average growth

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Average annual growth rate observed 1970-2008 (%)					
Independent variable	Simulated growth penalty from cyclone climate (%)					
Cross sectional coefficient	0.382***	0.358***	0.259	0.254	0.263	0.380***
	[0.127]	[0.133]	[0.274]	[0.827]	[0.411]	[0.143]
Observations	34	27	18	8	10	9
Whithin-region R^2	0.275	0.265	0.053	0.044	0.061	0.479
	Regions in sample					
East Asia	Y	Y				Y
N. America mainland	Y	Y	Y		Y	
Caribbean islands	Y	Y	Y	Y		
S. Asia	Y					
Oceania	Y					

Regressor is the average difference between annual growth predicted with the full model and the model where tropical cyclone exposure is set to zero. Models with more than one region in the sample include region fixed effects. Observed growth rates are from the PWT. Also see Figure 2.24. Bootstrapped standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

each region flattens out somewhat once the cyclone growth penalty is accounted for. For examples, we point out that in the absence of cyclones our estimates suggest that average growth rates in Jamaica and Trinidad & Tobago would be substantially nearer to one another (4.0:4.7 rather than 0.8:3.1), similar to Guatemala and Panama (3.3:3.2 rather than 1.1:2.9) and the Philippines and China (8.9:8.4 rather than 1.7:7.3).

In the right column of Figure 2.24, we formalize this comparison by plotting the cross-sectional regression of each country's observed average growth against our estimate for each country's cyclone-induced growth penalty. Within each region, countries with a larger (more negative) growth penalty tend to grow more slowly. In Table 2.6 we present coefficients for this regression, including region fixed effects to account for the large differences in average growth rates between regions. In this simple cross-sectional model, average growth tends to fall 0.38% per year for each 1% increase in the simulated cyclone-induced growth penalty (column 1). In this limited sample of cyclone-exposed countries, the within-region R-squared is 0.28, indicating that the cyclone climates of countries predicts a substantial amount of the observed cross-country variation in their average growth rates. It is likely that this estimate suffers from some attenuation bias – since we measure cyclone exposure imperfectly – and probably omitted variables bias as well – since there are important covariates that are correlated with cyclone climate which we do not attempt to account for here. Nonetheless, we think it is notable that the positive correlation between our calculated growth penalty and actual growth reduction appears independently within different regions with a relatively stable magnitude[43] (columns 2-6).

These simulations help us understand the extent to which repeated disaster exposure might influence economic development in countries endowed with different cyclone climates, however they should be interpreted cautiously. Even though we account for adaptation to average cyclone exposure, in terms of expected losses, we cannot account for the numerous and interacting general equilibrium adjustments that might accompany a large change in the global distribution of cyclones. For example, if all cyclones were removed from the Earth, patterns of global trade would surely adjust – an effect that we do not capture here. In addition, there may be unobservable factors that limit growth in certain countries, so it may be impossible for some countries to achieve the growth rates that our cyclone-free models suggest. However, it is also worth noting that in some cases, these estimates may underestimate the effect of cyclones since there may be secondary impacts – such as a civil war that might not have occurred without disaster-induced economic contraction (Hsiang, Burke and Miguel (2013b)) – that further reduce long-run growth. We feel that all these caveats are substantive enough that the exact values retrieved from the "cyclone-free" simulations should not be interpreted too literally. However, we think that the general distribution and magnitude of these quantities indicate that tropical cyclones, and perhaps disasters more generally, are a feature of the planet that exert a strong influence over global patterns of economic development.

Comparisons with previous studies

While several studies have examined different impacts of disasters, few can be directly compared to our results. However, two prior studies combine cyclone metrics from LICRICE with alternative data sets to examine short-run economic losses, so we use these earlier studies as benchmarks to consider whether the size of our estimates are reasonable. Hsiang and Narita (2012) examine how the direct, self-reported, short-run economic damages in the Emergency Events Database (EM-DAT) respond to country-level maximum wind speed in a global sample of countries. A linearization of their result indicates that direct, short-run damages increase by roughly 0.33% of GDP per m/s on average, which

[43] Because the sample sizes are small and the variation in the independent variable is limited for samples that do no include East Asia, the standard errors for these estimates tend to be large.

is similar in magnitude to our estimate of 0.37% of GDP per m/s in long-run losses (Appendix Figure 2.29, top panel). Anttila-Hughes and Hsiang (2011) examine household data from the Philippines to identify short-run income losses using province-by-year variation in wind speed. They estimate that an average household suffers short-run income losses of 6.6% in the average year, which is similar in magnitude to the average 7.5% annual loss of long-run income that we estimate the Philippines suffers as a country (Appendix Figure 2.29, lower panel). Without a theory relating short and long-run income losses after disaster, we refrain from speculating whether the short-run losses identified in these two earlier studies represent the exact same income losses that we observe in this study. However, the *magnitude* of the short and long-run losses are similar, suggesting that the long-run estimates we present here may be reasonably sized.

2.5.4 Projecting the cyclone-related cost of anthropogenic climate change

There is concern that anthropogenic climate change may cause the frequency and distribution of tropical cyclones to change, thereby raising (or lowering) cyclone-induced costs borne by coastal populations (Emanuel (2005), Stern (2006), Nordhaus (2010), Mendelsohn et al. (2012), Hsiang & Narita (2012)). Forecasting the response of cyclones to future climatic conditions has proven difficult and it remains a field of active research – however, there is some consensus on general patterns (Knutson et al. (2010b)). Globally, there are likely to be fewer total storms that achieve tropical cyclone status but the storms that do occur are likely to stronger on average. There is likely to be a reduction in the absolute number of relatively weaker storms, little change in the absolute number of strong storms, and an increase in the absolute number of very strong storms[44]. However, these statistics are global statistics and stronger patterns are expected at the basin level in some cases. For example, there is relatively broad consensus across models that total energy dissipation over the West Pacific will increase and that it will decrease over the Southern Hemisphere. Here we use basin-level forecasts to estimate the Present Discounted Value (PDV) of anthropogenic changes to the global tropical cyclone climate under a "business as usual" scenario (A1B).

Valuing an altered income trajectory caused by an altered cyclone climate

Above we showed that a marginal increase in cyclone exposure by one unit at time t led to a reduction of income by Ω_j at time $t+j$. We did not have enough data to observe income losses more than twenty

[44] We refer interested readers to Knutson, Landsea and Emanuel (2010a) and Knutson et al. (2010b) for reviews of this active literature.

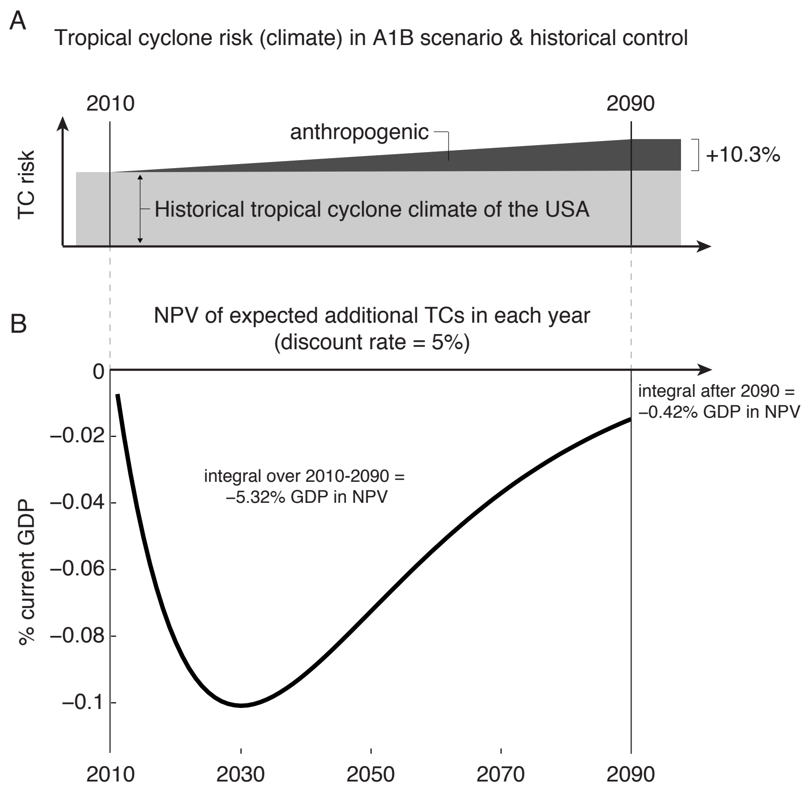

Figure 2.25: Example calculation of NPV of changes to the tropical cyclone climate of the United States under "Business as usual" (as tabulated in Table 2.7). (A) The tropical cyclone climate of the United States linearly intensifies to the projected intensity between 2010 and 2090 (the midpoint of the 2080-2100 averaging period of Emanuel et al). After 2090, the climate of the United States remains unchanged at it's new intensity. (B) The NPV (discount rate = 5%) of income losses that are expected to be incurred by the intensified climate (as it will be experienced in each year) is computed for each year and integrated to $t = \infty$. Most of the expected loss in NPV will be caused by cyclone events before 2040.

years after a cyclone event[45], so here we assume that income loss is permanent, thus $\Omega_s = \Omega_{20}$ for all $s \geq 21$. Assume a discount rate of ρ.

Letting expected cyclone exposure in the absence of anthropogenic forcing be S_0 in every period, the expected exposure of a population under climate change is

$$S(t) = S_0 + \Delta(t) \tag{2.5.5}$$

where $\Delta(t)$ is the total effect of all historical climate changes on S at the moment t. Before T_1, human activity has no effect so $\Delta(t) = 0$ for $t < T_1$. At some point T_2 in the future, the climate stabilizes so

[45]Estimates with more lags (not shown) suggest that $\Omega_{30} \approx \Omega_{20}$.

we set $\Delta(t) = \bar{\Delta}$ for $t > T_2$. Between T_1 and T_2, the cyclone climate exhibits transient behavior.

Our goal is to compare a cyclone-dependent income stream $Y(S_0)$ that is unaffected by climate change with a similar income stream that is affected by climate change $Y(S(t))$. A simple way to summarize the difference between these two trajectories, in a manner useful to policy, is to compute the PDV of their difference

$$PDV[Y(S_0) - Y(S(t))] = PDV[\partial Y/\partial S \times \Delta(t)] \tag{2.5.6}$$

which is true because the marginal impact of cyclone exposure is approximately invariant in the intensity of exposure (losses are linear), so we only need to consider the anthropogenic changes $\Delta(t)$.

To compute this value, we first evaluate the PDV of a single cyclone event with a magnitude of one at time $t = 0$, which we denote κ:

$$\kappa = \left[\sum_{j=0}^{20} \Omega_j e^{-\rho j}\right] + \frac{\Omega_{20}}{\rho} e^{-21\rho}. \tag{2.5.7}$$

The first term is the PDV of losses that occur in the year of the cyclone and the twenty years that follow. The second term is the PDV of the permanent income reduction Ω_{20} that is observed every period $t \geq 21$. κ is the marginal change in PDV(Y) that occurs because of a cyclone at time t if the future losses caused by that cyclone were discounted back to the moment t. Thus, the total losses from a permanent change in climate is this marginal effect κ times the change in the climate, at each period where the climate has changed, discounted back to $t = 0$

$$PDV[\partial Y/\partial S \times \Delta(t)] = \int_{T_1}^{T_2} \kappa \Delta(t) e^{-\rho t} dt + \frac{\kappa \bar{\Delta}}{\rho} e^{-T_2 \rho}. \tag{2.5.8}$$

The first term is the cumulative loss that is incurred by all changes in cyclone risk that occur before the climate stabilizes. The second term is the discounted value of the permanent climate shift $\bar{\Delta}$ that is the new steady state after $t = T_2$.

Generic climate scenarios Table 2.7 describes the PDV of changes to the current tropical cyclone climate under several different discount rates – here we focus on the 5% discount rate for succinctness[46].

[46] Much literature has discussed what discount rates should be used for climate change projections. A 5% discount rate is at the higher end of the spectrum of values that are advocated, so we focus on it in an effort to be both conservative but reasonable. For a review and perspectives on discount rates, see Gollier (2012).

Table 2.7: PDV of changes to the global tropical cyclone climate under "business as usual" (A1B)

	PDV as percentage of current GDP					
Discount rate:	1.0%	3.0%	5.0%	7.0%	10.0%	
Generic climate scenarios						
(standard errors in parentheses)						
A single +1 m/s tropical	-34.49	-9.86	-5.12	-3.20	-1.86	
cyclone event today	(-10.39)	(-2.97)	(-1.55)	(-0.98)	(-0.58)	
An abrupt +1 m/s climate	-1549.93	-29.81	-1.88	-0.17	-0.01	
intensification in 2090	(-466.84)	(-8.97)	(-0.57)	(-0.05)	(0.00)	
A linear climate intensification	-2382.10	-124.95	-25.19	-8.13	-2.32	
to +1 m/s in 2090†	(-717.49)	(-37.61)	(-7.63)	(-2.49)	(-0.73)	
North Atlantic: linear increase up to +10.3% in 2090						Current climate (m/s)
Bahamas	-3048	-160	-32.2	-10.4	-3.0	12.4
Belize	-2029	-106	-21.5	-6.9	-2.0	8.3
Costa Rica	-304	-16	-3.2	-1.0	-0.3	1.2
Cuba	-2673	-140	-28.3	-9.1	-2.6	10.9
Dominican Rep.	-2738	-144	-29.0	-9.3	-2.7	11.2
Guatemala	-1304	-68	-13.8	-4.5	-1.3	5.3
Honduras	-1455	-76	-15.4	-5.0	-1.4	5.9
Haiti	-2625	-138	-27.8	-9.0	-2.6	10.7
Jamaica	-2420	-127	-25.6	-8.3	-2.4	9.9
Mexico	-1629	-85	-17.2	-5.6	-1.6	6.6
Nicaragua	-1081	-57	-11.4	-3.7	-1.1	4.4
Trinidad & Tobago	-950	-50	-10.0	-3.2	-0.9	3.9
United States	-560	-29	-5.9	-1.9	-0.5	2.3
West Pacific: linear increase up to +19.1% in 2090						
China	-1194	-63	-12.6	-4.1	-1.2	2.6
Japan	-9600	-504	-101.5	-32.8	-9.4	21.1
Korea	-6937	-364	-73.4	-23.7	-6.8	15.2
Laos	-4492	-236	-47.5	-15.3	-4.4	9.9
Malaysia	-235	-12	-2.5	-0.8	-0.2	0.5
Philippines	-7878	-413	-83.3	-26.9	-7.7	17.3
Thailand	-2176	-114	-23.0	-7.4	-2.1	4.8
Vietnam	-5291	-278	-56.0	-18.1	-5.2	11.6
Oceania: linear decrease down to -13.8% in 2090						
Australia	1238	65	13.1	4.2	1.2	3.8
Indonesia	71	4	0.7	0.2	0.1	0.2
New Zealand	904	47	9.6	3.1	0.9	2.7
Papua New Guinea	194	10	2.0	0.7	0.2	0.6
North Indian: linear decrease down to -5.8% in 2090						
Bangladesh	1054	55	11.1	3.6	1.0	7.6
India	533	28	5.6	1.8	0.5	3.9
Sri Lanka	499	26	5.3	1.7	0.5	3.6

Estimates for climatic intensification are the relative changes in basin-wide power dissipation between simulations of the twentieth-century and the period 2080-2100 under the A1B emissions scenario averaged across seven climate models (Emanuel et al. (2008)). Also see discussion in (Knutson et al. (2010b)). Projections assume that country-level exposure increases in proportion to basin-level activity and basin-level activity strengthens or weakens linearly between 2010 and 2090 (the midpoint of the 2080-2100 averaging period). Models agree strongly on the sign of West Pacific (7/7) and Oceania (6/7) projections. Models disagree more regarding North Atlantic (4/7) and North Indian (4/7) projections (see Appendix Figure 2.30). Estimates using the 5% discount rate are converted to 2010 US$ in Table 2.8. †See Figure 2.25 for a graphical explanation of this calculation.

In the top panel, we tabulate the PDV of three types of generic scenarios that are not specific to a country. The first scenario is a single 1 m/s cyclone event today, which has PDV equal to κ, i.e. -5.1% of GDP (Eq. 2.5.7). This value is sizable because income losses from a single event are permanent relative to the counterfactual income trajectory. The second scenario is an abrupt intensification of the climate that occurs at 2090 and which persists indefinitely – a plot of $\Delta(t)$ would be a step function where the discontinuity is at $t = 2090$. The PDV of this scenario is -1.9% of GDP, equal in value to the second term in Eq. 2.5.8. The PDV of this permanent shift in the climate is lower than the one-time event that occurs immediately only because it occurs in the distant future and is thus discounted, although the total quantity of additional cyclone risk endured in the second scenario is much larger than in the first scenario. The third generic scenario is a linear intensification of the climate that begins in 2010 and ends in 2090, where the climate stabilizes in 2090 at 1 m/s above its initial risk level in 2010 (see Figure 2.25 for a graphical example). The PDV of this third scenario is the sum of both terms in Eq. 2.5.8, and its value exceeds in magnitude both the first and second scenarios. With a high discount rate (10%) the cost of this third scenario is only slightly higher in PDV than a single event today (2.3:1.9) because the costs from the gradually intensifying climate are heavily discounted, whereas for a low discount rate (1%) the PDV of this scenario is very large compared to a single event (2382:34) because the quantity of total additional risk is much larger and it is not heavily discounted. At a 5% discount rate the PDV of this scenario is still quantitatively large, amounting to -25.2% of GDP. Panel B of Figure 2.25 illustrates the timing of losses under this scenario with a 5% discount rate using actual climate values for the United States – most of the loss in PDV arises from the intensification of the cyclone climate that occurs during 2020-2050 because the distant future is still heavily discounted.

Application to a "Business as usual" scenario To apply quantities to Equation 2.5.8, we combine our empirical estimates of Ω_j with basin-specific estimates of $\Delta(t)$ from Emanuel et al (2008) (shown in Appendix Figure 2.30 for reference). Emanuel et al do not model transient cyclone climates because it is computationally expensive – instead they model the cyclone climates during 2080-2100 under the A1B scenario as if it were a steady-state climate[47]. Thus, we set $T_1 = 2010$ and $T_2 = 2090$, because it is the midpoint of the averaging period in Emanuel et al. Emanuel et al report $\bar{\Delta}$ for each basin in aggregate, averaged over seven climate models. For simplicity, we follow Emanuel (2011) and

[47]Emanuel et al also model the cyclone climate during the twentieth century as if it were a steady state and report the difference, which is analogous to $\bar{\Delta}$. This procedure is useful because it removes any constant bias exhibited by individual models.

assume that $\Delta(t)$ increases linearly from zero in 2010 to $\bar{\Delta}$ in 2090, analogous to the third generic scenario described above, and that the climate of each country intensifies in proportion to the basin-level aggregate. Figure 2.25 depicts how estimates from Emanuel et al (2008) are converted to a cyclone climate trajectory for the United States, which is then valued at each moment in time.

The lower panels of Table 2.7 present the PDV of the A1B scenario for several major countries in each basin, as a percentage of each country's current GDP. Because the timing of these climatological changes are assumed to be identical across basins, the difference between countries arises from differences in the sign and magnitude of climatic change across basins as well as the differing baseline climatologies of each country[48] (far right column). Anthropogenic climate change is expected to cause moderate intensification of North Atlantic cyclone activity[49] (+10.3%), which has a sizable negative NPV for many countries (we again focus on the 5% discount rate). Caribbean islands lose the largest fraction of income, with losses that generally exceed 20% of current GDP in NPV, while mainland North America loses somewhat less, with losses in the vicinity of 5-15% of current GDP. The United States, is expected to lose the NPV-equivalent of 5.9% of current GDP. The West Pacific faces the most extreme intensification of cyclone activity[50] (+19.1%) causing large losses for many countries in East Asia. The NPV of expected losses exceed 40% of GDP for many countries, such as Vietnam and South Korea, and rises above 80% of current GDP in the cases of the Philippines and Japan. China is expected to lose the equivalent of 12.6% of its current GDP. Oceania and the North Indian Ocean are anticipated to have *reduced* tropical cyclone exposure under anthropogenic climate change (-5.8% and -13.8% respectively), causing their expected income streams to rise in the future. Because these climatological changes are small to moderate in magnitude and the initial cyclone climatology of these countries is somewhat weaker, these gains from climate change tend to be smaller (in percentage terms) than the losses described above. Australia and Bangladesh benefit the most in percentage terms, with gains valued at 13.1% and 11.1% of current GDP (resp.). India is expected to gain the NPV-equivalent of 5.6% of its current GDP.

[48] These estimates are a linear rescaling of the third generic scenario (Eq. 2.5.8) where $\bar{\Delta}$ is set to the fractional intensification of basin-level activity times each country's baseline climatology.

[49] "Cyclone activity" here is total power dissipation.

[50] The intensification of the West Pacific is highly consistent across models, so it should be considered the most certain of these scenarios. See Appendix Figure 2.30.

2.6 Discussion

A growing literature has examined the short-run economic impact of natural disasters and environmental insults more generally, however it has been widely debated whether extreme events have any permanent long-run impact on economic outcomes. Here, we have constructed a novel global dataset of exogenous natural disasters and are the first to demonstrate that permanent losses to national income are large, frequent and generalizable to populations around the globe, regardless of their income level, geography or the scale of the disaster. Permanent changes in consumption, investment, trade and international aid all reflect the observed changes in national income, corroborating this result. Furthermore, our result is supported by global patterns of income losses, which match theoretical predictions for the structure of climate-based adaptations, and two prior studies that produce similarly sized estimates using different data. Collectively, these findings lead us to reject the "creative destruction," the "build back better," and the "recovery to trend" hypotheses for post-disaster impacts – leading us to embrace the "no recovery" hypothesis as the best description of the data.

The estimated impact of cyclones on long-run growth emerges gradually, rendering it virtually undetectable to a casual observer, but it persists for more than a decade, generating strikingly large cumulative losses that have dramatic implications for economic development. Within the 58% of countries that are affected by cyclones, a one standard deviation event reduces long-run GDP by 3.6 percentage points, and a "one-in-ten" country-year event reduces long-run GDP by 7.4% twenty years later. For countries that are frequently or persistently exposed to cyclones, these permanent losses accumulate, causing annual average growth rates to be 1-7.5 percentage points lower than simulations of "cyclone-free" counterfactuals. Across the global sample of affected countries, simulations suggest that the 2.0% average annual growth rate that we observe in the real world is depressed relative to the 3.8% growth that we would observe in a counterfactual world that had no tropical cyclones[51]. Taken together, these results suggest that the global tropical cyclone climate is likely to play an important role in determining the global distribution of countries' growth rates as well as the global rate of economic growth. Application of these estimates to a projection of climate change indicates that through its influence on cyclone activity, anthropogenic warming will have a substantial impact on the income trajectory of countries, with a PDV cost for individual countries that ranges from +13.1% (a benefit) to −101.5% (a loss) of current GDP.

[51] See text above for many caveats of this result.

Implications for disaster risk management policies In general, natural disaster policies have two prongs: pre-disaster risk reduction and post-disaster income-smoothing. The latter is often the focus of actual policy, however the former has received substantial recent attention as researchers demonstrate that it is sometimes highly cost-effective (Healy and Malhotra (2009), Deryugina (2011), UNISDR (2011)). The discussion of these two policy-instruments often assumes that they are substitutes for one another, in terms of raising social welfare, and that the efficient allocation of public funds should be based on their cost-efficacy. However, our results suggest that while both instruments may have positive net-present value, they are not substitutes in the long-run. Post-disaster income smoothing is achieved through borrowing, transfers and insurance mechanisms. These measures may be effective at reducing welfare losses in the short run, but they may generate no net income. Thus, if incomes decline in the long-run, then the primary welfare gains from smoothing will arise from simply *delaying* consumption losses. We observe that long-run income loses unfold gradually over the course of fifteen years, suggesting that some income smoothing measures are probably slowing the decline in national income. However, despite access to these instruments, we do not observe that populations "catch up" with their pre-disaster trajectory, suggesting that these instruments may have limited long-run impact. In contrast, pre-disaster investments that reduce risk, such as infrastructure hardening and early-warning systems, are likely to influence long-run outcomes after disaster. Many risk reduction measures are similar or identical to adaptive investments, and the results in Figure 2.20 suggest that adaptive behaviors are probably effective at lowering the marginal long-run effect of cyclones. Policy-makers should optimally allocate public resources between post-disaster income smoothing and pre-disaster risk reduction. If future welfare is discounted heavily, then long-run income is not important and the optimal allocation shifts towards income smoothing. As policy-makers care more about the future, then risk reduction becomes more important since its impact on future income is enduring.

Implications for economic development policies Tropical cyclone exposure effectively displaces a country's GDP trajectory in time – following cyclone exposure, a country's income does not recover to its pre-disaster trajectory but instead settles on a new trajectory that is parallel but below the original trajectory. Thus, a simple way to summarize our result is to compute how much "un-development" occurs as a result of cyclone exposure. Within the sample of cyclone-affected countries, a one standard deviation event is equal to 9.4 m/s of wind exposure which generates a long-run loss of 3.57% of GDP. Because average annual growth in this sample is 2.00% per year, each one standard deviation event effectively undoes 1.8 year's worth of economic development[52]. Using this metric, each 1 m/s marginal increase in annual wind exposure undoes 2.3 month's worth of average development. For countries endowed with cyclone climates where they are repeatedly exposed to cyclone events, there is no choice but to adapt to these adverse conditions. Here (Figure 2.20) and elsewhere (Hsiang & Narita (2012), Anttila-Hughes & Hsiang (2011)) there is evidence that adaptation to cyclones is feasible, but the fact that no countries exhibit zero marginal losses indicates that the cost of additional adaptation remains binding for most populations (Hsiang & Narita (2012)). If policy-makers are able to encourage technological innovations or otherwise lower the cost of adaptive investments, this should increase populations' voluntary adoption and investment in adaptive technologies, which in turn should lower their long-run economic losses to disaster and raise their growth rate. In addition, it is possible that some populations may have underinvested in adaption because they undervalue its benefit – perhaps because it is difficult for populations to observe a return on investment for protective technologies. This study suggests that instead of conceptualizing adaptive investments as simply "protective," they can in fact be conceptualized as "revenue generating investments" since they effectively raise a population's expected future income stream.

Implications for climate change policies Optimal climate change policy balances the cost of reducing greenhouse gas emissions with the benefits of limiting global climatic changes. In practice, computing the total benefit of climate change policy requires that we identify the various pathways through which climate changes affect society and then enumerate the costs or benefits of these various impacts. It has been recognized for some time that anthropogenic climate change might alter tropical cyclone frequency or intensity (Emanuel (1999)) and recently there has been some effort to quantify the social cost of these projected changes (Nordhaus (2010), Mendelsohn et al (2012), Houser et al. (2014)), however these recent efforts have focused on the immediate destruction of assets in storms and have not accounted for their impact on long-run economic growth. The present study provides evidence that this later mechanism is economically important in scenarios of future warming, with a social cost that is larger in magnitude than the projected cost of additional asset destruction. Accounting for the effect of tropical cyclones on long-run growth will raise our estimate for the global social cost of climate change substantially. For a sense of scale, our estimates suggest that under the "Business as usual" scenario (with a 5% discount rate[53]) the PDV of lost long-run growth is $855 billion for

[52] An event at the 90th percentile reverses 3.7 year's worth of develoment, and an event at the 99th percentile undoes 7.5 years worth of development.

[53] At a 3% discount rate, these values rise by a factor of 4.9.

Table 2.8: PDV of the change in countries' income trajectories resulting from the "business as usual" climate change scenario (A1B)

Country	**PDV using 5% discount rate***		
	Estimate (†billion US$)	95% confidence interval bounds	
Japan	-4,461.1	-1,813.6	-7,108.5
China	-1,364.5	-554.7	-2,174.3
South Korea	-1,026.4	-417.3	-1,635.6
Taiwan	-991.9	-403.2	-1,580.5
United States	-855.0	-347.6	-1,362.4
Hong Kong	-354.0	-143.9	-564.1
Philippines	-299.3	-121.7	-476.9
Mexico	-260.3	-105.8	-414.7
Vietnam	-160.1	-65.1	-255.1
Thailand	-140.6	-57.2	-224.0
Cuba	-40.0	-16.3	-63.7
Puerto Rico	-34.5	-14.0	-55.0
Dominican Republic	-33.0	-13.4	-52.6
Spain	-13.2	-5.4	-21.1
Guatemala	-13.2	-5.4	-21.1
Canada	-10.9	-4.4	-17.4
Indonesia	-10.9	-4.4	-17.3
Malaysia	-9.8	-4.0	-15.6
Cambodia	-9.3	-3.8	-14.8
Laos	-9.2	-3.7	-14.7
Jamaica	-7.1	-2.9	-11.2
France	-6.3	-2.6	-10.1
Portugal	-5.5	-2.2	-8.8
Singapore	-5.3	-2.1	-8.4
Honduras	-4.7	-1.9	-7.5
Haiti	-4.0	-1.6	-6.4
El Salvador	-3.6	-1.5	-5.8
Trinidad & Tobago	-3.2	-1.3	-5.0
Bahamas	-3.1	-1.3	-4.9
All others	-15.5	-6.3	-24.7
Pakistan	3.1	1.3	4.9
Sri Lanka	5.1	2.1	8.2
New Zealand	13.0	5.3	20.7
Bangladesh	26.1	10.6	41.6
Australia	140.0	56.9	223.1
India	264.2	107.4	420.9
Total losses	**-10,159**	**-4,130**	**-16,188**
Total gains	**455**	**185**	**725**
Net PDV (global)	**-9,704**	**-3,945**	**-15,463**

*Value of income stream under A1B less control scenario. †Values are PPP adjusted and based on 2010 income.

the United States[54] (5.9% of current GDP), $299 billion for the Philippines (83.3% of current GDP), $1 trillion for South Korea (73% of current GDP), $1.4 trillion for China (12.6% of current GDP), and $4.5 trillion for Japan (101.5% of current GDP)[55] – values for other countries are tabulated in Table 2.8. Aggregating these estimates across all countries alters the PDV of "full mitigation" relative to "business as usual" by $9.7 trillion ($5.2 trillion without Japan). For comparison, we note that Nordhaus (2008) calculates that the total PDV of optimal global climate policy is $5 trillion (in comparison to no regulation, using a similar discount rate) which costs $2 trillion to implement, for a net gain of $3 trillion[56]. Thus, accounting for the long-run growth impact of cyclones will raise the marginal benefit of green house gas mitigation, thereby increasing the incentive for populations to undertake somewhat stronger mitigation measures. Importantly, however, because these losses are relatively focused in the coastal countries of North America and East Asia, these results are likely to influence the optimal policies of these particular countries more strongly than they influence optimal global policy.

[54] For consistency with our analysis, here we use PPP adjusted GDP for 2010 listed in the PWT.

[55] There are a small number of countries that benefit, however these gains are modest compared to losses globally (in total dollars). For example, India and Australia are by far the biggest "winners," with income trajectories that rises in PDV by $264 and $140 billion (resp.). Bangladesh receives the third largest benefit, a mere $26 billion in PDV.

[56] Nordhaus notes that under optimal management, using his model, there are $17 trillion in residual damages that remain even after optimal regulation.

Appendix

2.A Supplementary Tables

Table 2.9: Results for all dependent-independent variable pairs

	(1)	(2)	(3)	(4)
Dependent variable	Growth (%)			
Independent variable	Wind speed (m/s)		Energy (sd)	
Growth data source	PWT	WDI	PWT	WDI
Marginal cumulative effect of 1 additional unit of exposure				
1 years	-0.0509**	-0.0241	-0.334**	-0.191
	(0.0208)	(0.0218)	(0.166)	(0.164)
2 years	-0.0584**	-0.0512**	-0.358*	-0.405**
	(0.0259)	(0.0249)	(0.186)	(0.190)
3 years	-0.0876***	-0.0798***	-0.654***	-0.629***
	(0.0303)	(0.0275)	(0.214)	(0.205)
4 years	-0.0903***	-0.105***	-0.688***	-0.844***
	(0.0349)	(0.0292)	(0.242)	(0.221)
5 years	-0.0895**	-0.129***	-0.722**	-1.052***
	(0.0427)	(0.0322)	(0.289)	(0.235)
6 years	-0.0974**	-0.147***	-0.716**	-1.095***
	(0.0473)	(0.0372)	(0.318)	(0.272)
7 years	-0.133**	-0.188***	-0.796**	-1.419***
	(0.0535)	(0.0444)	(0.355)	(0.330)
8 years	-0.197***	-0.232***	-1.118***	-1.746***
	(0.0591)	(0.0488)	(0.392)	(0.358)
9 years	-0.190***	-0.225***	-1.204***	-1.859***
	(0.0647)	(0.0524)	(0.435)	(0.399)
10 years	-0.223***	-0.272***	-1.484***	-2.160***
	(0.0711)	(0.0582)	(0.461)	(0.424)
11 years	-0.257***	-0.297***	-1.695***	-2.316***
	(0.0747)	(0.0623)	(0.486)	(0.457)
12 years	-0.292***	-0.327***	-1.813***	-2.538***
	(0.0797)	(0.0660)	(0.509)	(0.486)
13 years	-0.325***	-0.349***	-1.876***	-2.633***
	(0.0837)	(0.0702)	(0.534)	(0.498)
14 years	-0.364***	-0.368***	-2.033***	-2.761***
	(0.0893)	(0.0772)	(0.571)	(0.542)
15 years	-0.378***	-0.383***	-2.069***	-2.851***
	(0.0938)	(0.0820)	(0.594)	(0.568)
16 years	-0.405***	-0.403***	-2.221***	-3.061***
	(0.0975)	(0.0879)	(0.617)	(0.599)
17 years	-0.398***	-0.415***	-2.153***	-3.146***
	(0.102)	(0.0910)	(0.643)	(0.619)
18 years	-0.384***	-0.419***	-2.038***	-3.142***
	(0.104)	(0.0955)	(0.672)	(0.647)
19 years	-0.383***	-0.387***	-1.909***	-3.037***
	(0.109)	(0.100)	(0.692)	(0.669)
20 years	-0.374***	-0.379***	-1.825**	-3.090***
	(0.113)	(0.105)	(0.733)	(0.700)
Observations	6415	6952	6415	6952
Adjusted R^2	0.144	0.191	0.144	0.191

All models contain country fixed effects, year fixed effects, and country-specific linear trends. Standard errors in parentheses are robust to spatial (1000km) and serial (10-year) correlation. Each column displays coefficents from our model with a different data pairing. Column (1) replicates column (2) of Table 2.3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: Convergence behavior with no linear time-trend

	(1)	(2)	(3)	(4)
Dependent variable	Growth (%) from PWT			
Country sample	All	Exposed	All	Exposed
Marginal cumulative effect of 1 additional m/s exposure				
5 years	-0.0944**	-0.0822**	-0.0662*	-0.0570
	(0.0392)	(0.0390)	(0.0396)	(0.0392)
10 years	-0.211***	-0.190***	-0.181***	-0.163***
	(0.0605)	(0.0616)	(0.0608)	(0.0613)
15 years	-0.306***	-0.282***	-0.316***	-0.294***
	(0.0734)	(0.0744)	(0.0740)	(0.0746)
20 years	-0.247***	-0.212**	-0.302***	-0.268***
	(0.0854)	(0.0870)	(0.0856)	(0.0869)
$ln(GDPpc)_{t-1}$			-4.015***	-4.022***
			(0.555)	(0.588)
Observations	6415	3834	6415	3834
Adjusted R^2	0.122	0.150	0.139	0.171

All models contain country fixed effects, year fixed effects, and country-specific linear trends. "Exposed" countries are those countries that are ever exposed to tropical cyclones in the sample. Standard errors in parentheses are robust to spatial (1000km) and serial (10-year) correlation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.11: Observed and simulated country-specific growth rates with and without cyclones

Country	Prediction with full model* (%)	Prediction with cyclones removed (%)	Cyclone climate growth penalty (%)	Country	Prediction with full model* (%)	Prediction with cyclones removed (%)	Cyclone climate growth penalty (%)
ARG	1.11	1.11	0.00	JOR	0.78	0.78	0.00
AUS	2.12	3.77	-1.65	JPN	2.29	10.84	-8.55
AUT	2.47	2.47	0.00	KEN	0.16	0.16	0.00
BDI	0.65	0.65	0.00	KOR	5.59	11.55	-5.96
BEL	2.25	2.25	0.00	LKA	3.39	5.05	-1.66
BEN	0.50	0.50	0.00	LSO	2.30	2.30	0.00
BFA	1.17	1.17	0.00	LUX	3.54	3.54	0.00
BGD	1.48	4.83	-3.35	MAR	2.29	2.36	-0.07
BOL	0.68	0.68	0.00	MDG	-0.34	4.06	-4.41
BRA	2.02	2.03	0.00	MEX	1.87	4.62	-2.75
BRB	1.67	6.19	-4.52	MLI	2.03	2.03	0.00
BWA	6.36	6.42	-0.06	MOZ	1.35	2.48	-1.13
CAF	-1.26	-1.26	0.00	MRT	0.82	0.84	-0.02
CAN	1.92	2.02	-0.10	MUS	3.90	11.52	-7.62
CHE	1.31	1.31	0.00	MWI	0.19	0.36	-0.17
CHL	2.60	2.60	0.00	MYS	4.54	4.79	-0.25
CHN	7.30	8.38	-1.08	NAM	0.87	0.90	-0.03
CIV	-0.17	-0.17	0.00	NER	-0.54	-0.54	0.00
CMR	0.96	0.96	0.00	NGA	1.28	1.28	0.00
COG	1.49	1.49	0.00	NIC	-1.43	0.43	-1.87
COL	2.44	2.48	-0.04	NLD	2.00	2.00	0.00
COM	-0.54	1.78	-2.32	NOR	2.81	2.81	0.00
CPV	2.74	4.96	-2.23	NPL	1.34	1.53	-0.19
CRI	1.56	2.23	-0.66	NZL	1.31	2.62	-1.31
CYP	3.09	3.09	0.00	PAK	2.10	2.27	-0.17
DNK	1.86	1.86	0.00	PAN	2.86	3.20	-0.34
DOM	3.42	7.64	-4.22	PER	1.00	1.00	0.00
DZA	1.27	1.27	0.00	PHL	1.65	8.93	-7.28
ECU	1.86	1.86	0.00	PNG	1.90	2.17	-0.27
EGY	3.37	3.37	0.00	PRI	2.29	6.93	-4.64
ESP	2.34	2.51	-0.17	PRT	2.82	3.22	-0.40
ETH	0.78	0.79	-0.01	PRY	1.70	1.70	0.00
FIN	2.57	2.57	0.00	RWA	0.65	0.65	0.00
FJI	1.70	6.24	-4.54	SEN	0.53	0.93	-0.40
FRA	1.94	1.99	-0.05	SGP	5.33	5.42	-0.09
GAB	0.60	0.60	0.00	SLE	-0.14	-0.13	-0.01
GBR	2.12	2.12	0.00	SLV	1.23	2.79	-1.57
GHA	1.40	1.40	0.00	SWE	1.83	1.83	0.00
GIN	-0.25	-0.19	-0.06	SYC	4.44	5.79	-1.35
GMB	1.21	1.72	-0.51	SYR	1.53	1.53	0.00
GNB	1.95	2.36	-0.42	TCD	0.80	0.80	0.00
GNQ	8.90	8.90	0.00	TGO	-1.46	-1.46	0.00
GRC	2.34	2.34	0.00	THA	4.31	6.48	-2.17
GTM	1.14	3.33	-2.19	TTO	3.12	4.66	-1.55
HKG	4.49	14.74	-10.25	TUN	2.80	2.80	0.00
HND	1.13	3.54	-2.41	TUR	2.26	2.26	0.00
HTI	-0.46	3.33	-3.79	TWN	5.69	16.88	-11.19
IDN	4.06	4.17	-0.10	TZA	1.54	1.57	-0.02
IND	3.18	4.75	-1.57	UGA	0.77	0.77	0.00
IRL	3.33	3.33	0.00	URY	2.13	2.13	0.00
IRN	0.80	0.80	0.00	USA	1.89	2.76	-0.88
ISL	2.98	2.98	0.00	VEN	0.42	0.59	-0.18
ISR	2.02	2.02	0.00	ZAF	1.12	1.14	-0.02
ITA	1.96	1.96	0.00	ZMB	-0.57	-0.57	0.00
JAM	0.81	4.00	-3.19	ZWE	-0.82	-0.59	-0.23

*By construction, observed growth rates are the same as predictions with the full model.

2.B Supplementary Figures

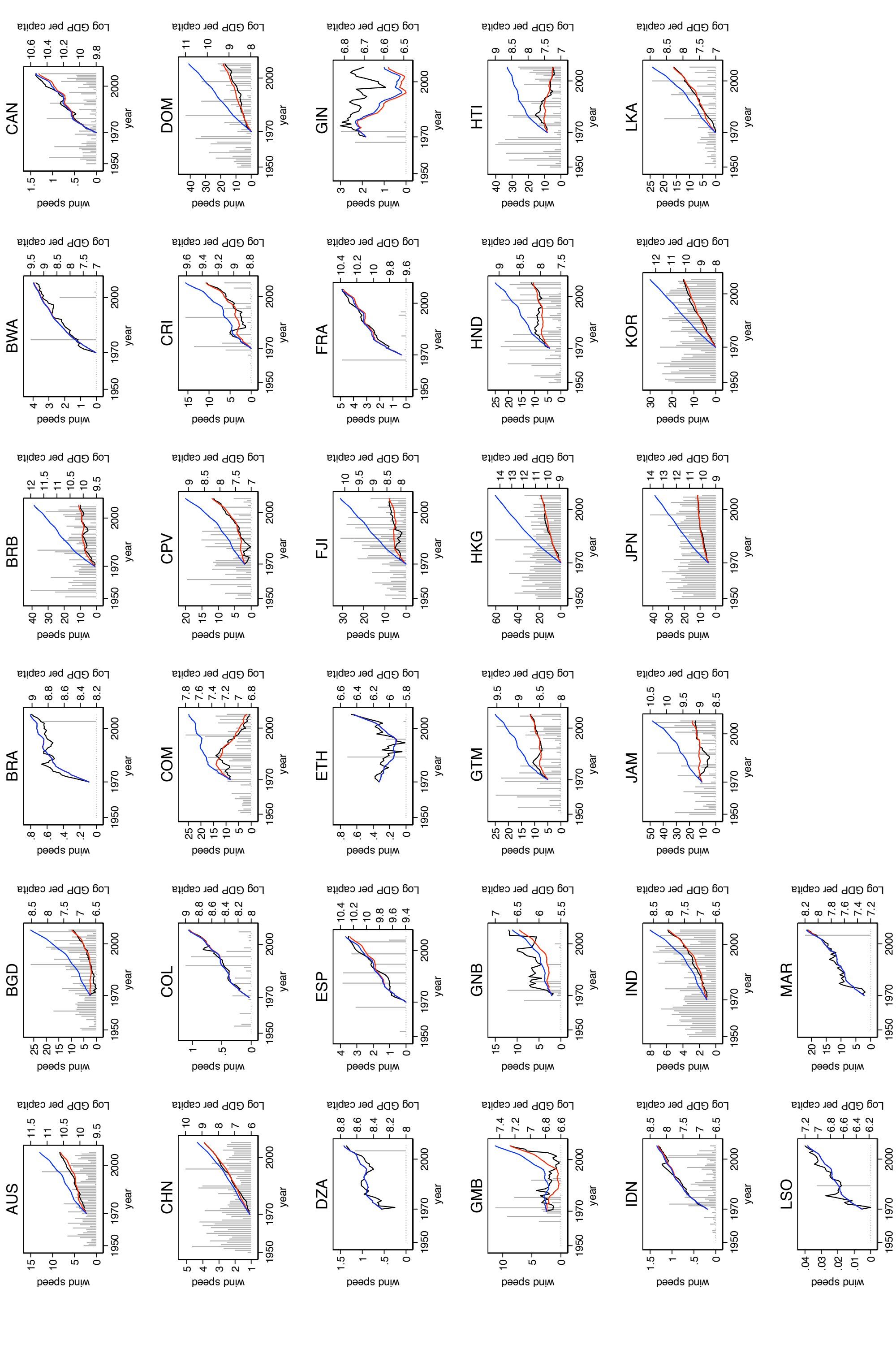

Figure 2.26: Simulations of log GDPpc with (red) and without (blue) tropical cyclones for exposed countries (right axis). Vertical grey bars display each country's wind exposure in each year (left axis).

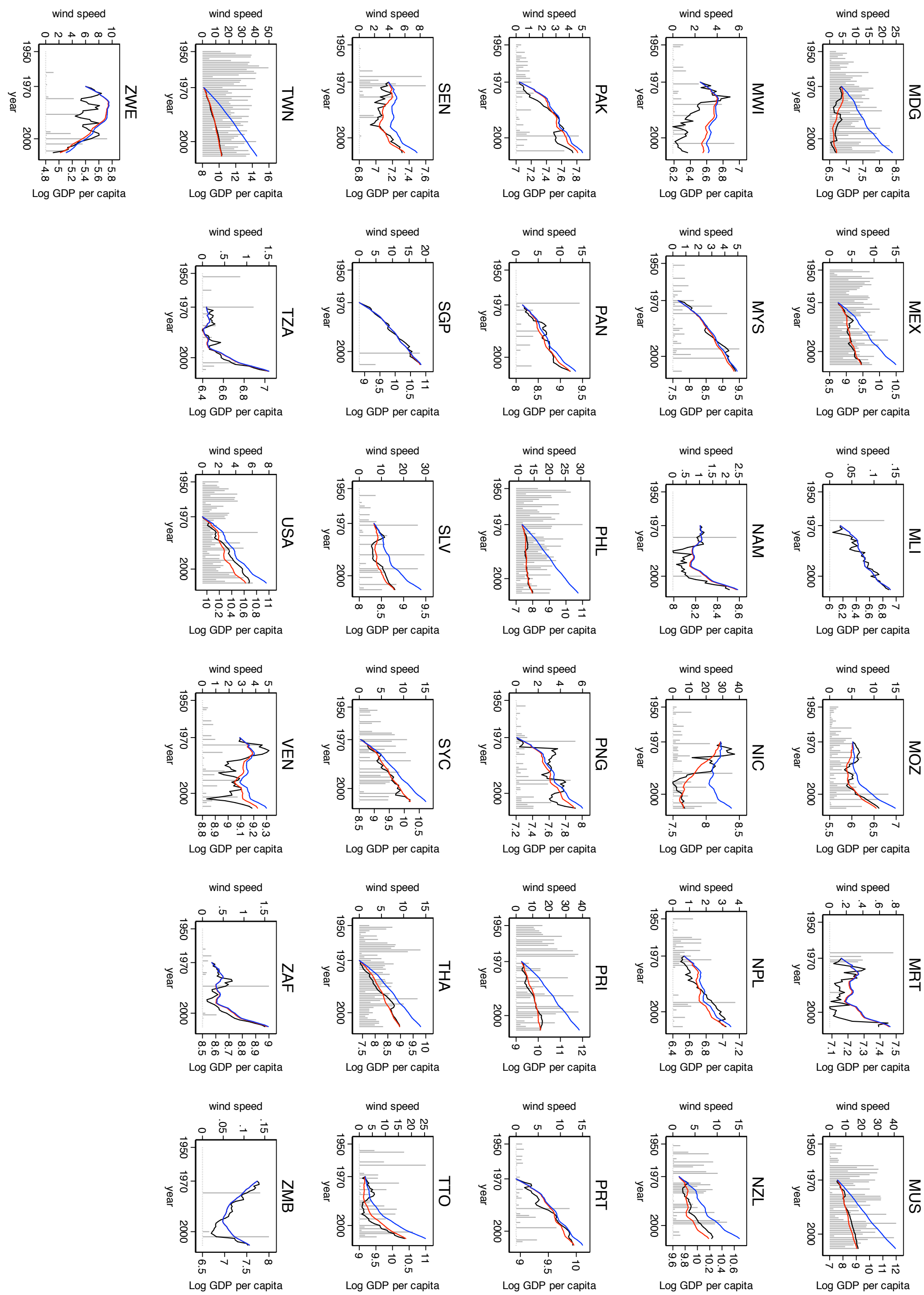

Figure 2.27: Figure 2.26 continued.

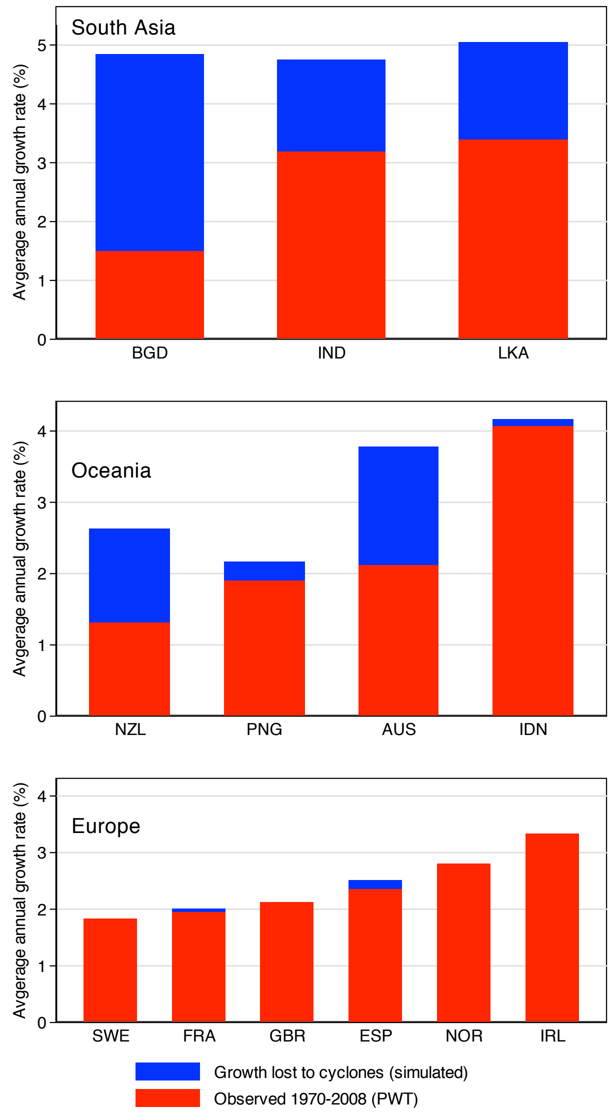

Figure 2.28: Average annual growth rates as observed historically (dark bars, equal to simulated growth with full model) and average growth in simulations where cyclones are removed (light bars). The difference in the height of the bars is the “missing” average annual growth loss to cyclones.

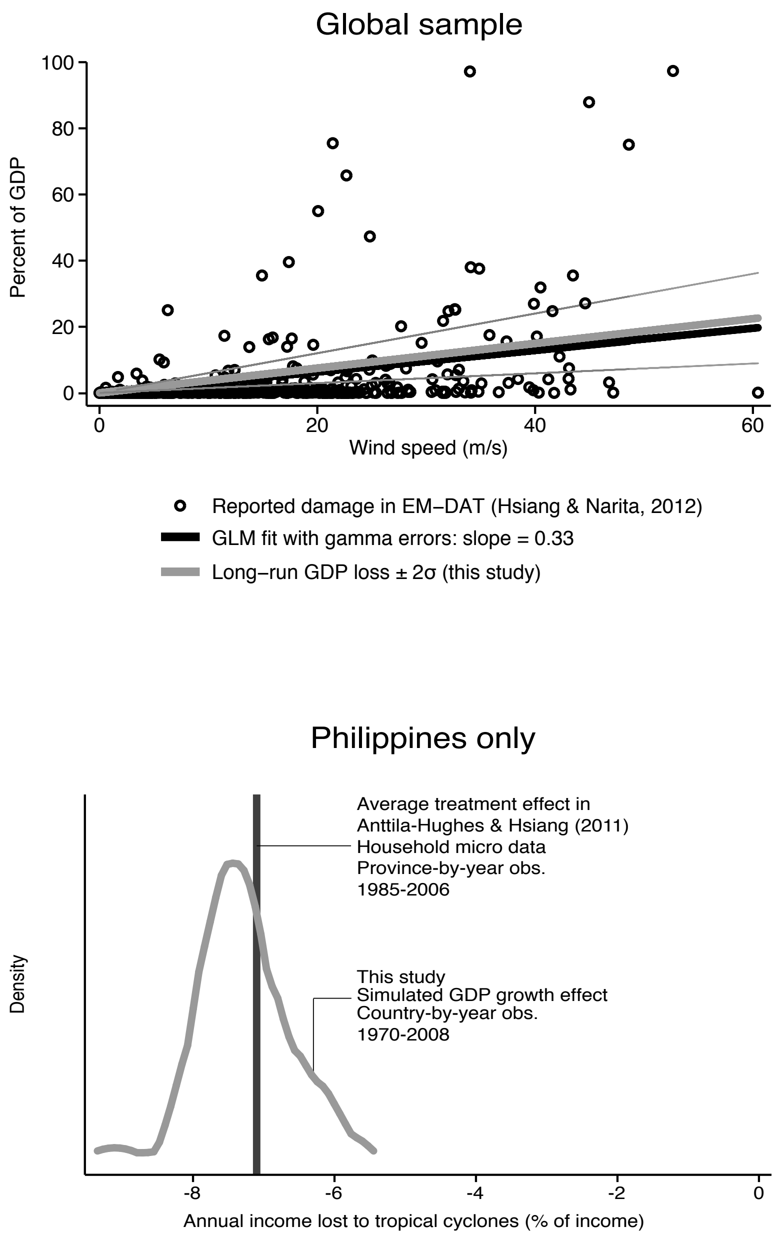

Figure 2.29: Quantitative comparison of our results to related estimates in the literature. Top: Hsiang and Narita (2012) estimate the relationship between self-reported capital damages and the maximum wind speed measure used in this study. Bottom: Anttila-Hughes and Hsiang (2011) estimate the average income lost to Filipino households due to tropical cyclone exposure in the prior year.

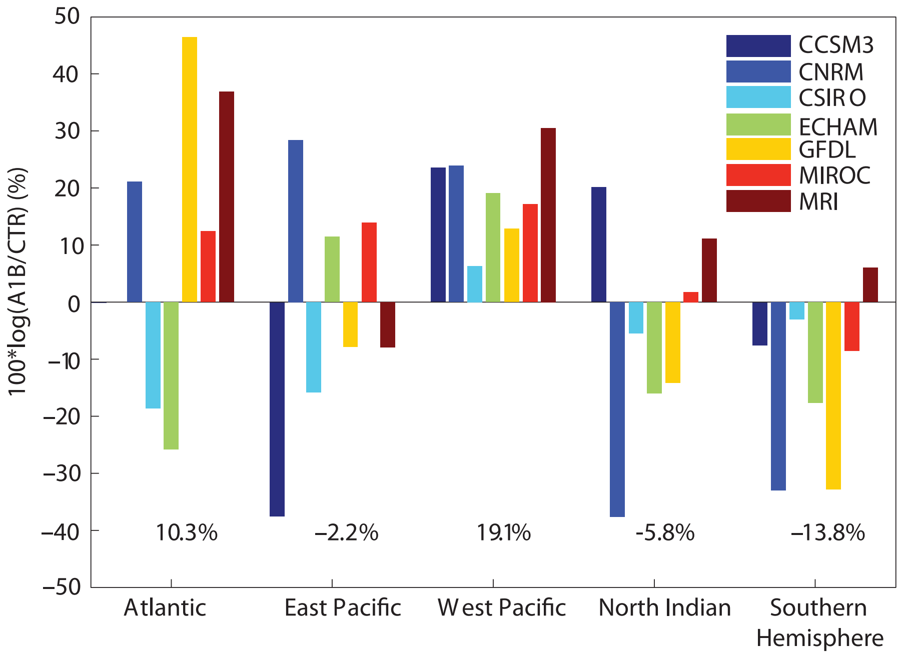

Figure 2.30: Projections for climatic intensification in basin-wide power dissipation between simulations of the twentieth-century and the period 2080-2100 under the A1B emissions scenario using seven climate models, from Emanuel et al. (2008). Percentages for each basin are the multi-model mean. Models agree strongly on the sign of West Pacific (7/7) and Oceania (6/7) projections. Models disagree more regarding North Atlantic (4/7) and North Indian (4/7) projections. Also see discussion in (Knutson et al. (2010b)). Figure from Knutson et al. (2010b).

Chapter 3

Long-range Externalities and Human Capital: Evidence from atmospheric models and Indonesian megafires

Amir S. Jina and Miriam E. Marlier

Abstract

Poor environmental quality has potentially large health and productivity costs for society, and yet the willingness to pay for improvements to environmental quality appears to be low. We examine the effects of air pollution from wildfires in Indonesia, caused by land-clearing for agriculture, upon two aspects of human capital development. We posit the idea that the poor environment is not caused by a low valuation of environment quality but by a failure to acknowledge the full production cost of agricultural practices as the effects are carried by a third party and often unrecognized. Overcoming problems of lack of appropriate data sources, we recreate pollution exposure using a biogeochemical model of fires together with a general circulation climate model. We look at exposure to fine particulate matter and ozone in the months leading up to and immediately after birth to see their effects upon physical development. Children exposed to a one standard deviation shock in particulate matter pollution in the second trimester in utero experience a .05 standard deviation decrease in long-term measures of physical development. We then look at contemporary cognitive affects, and find that, even at levels below that of global air quality standards, particulate matter pollution decreases cognitive test scores. This effect can be as large as that for indoor air pollution. These two effects, largely unobserved, will lead to productivity decreases and losses for society, constituting an extra burden to development in this extreme environment.

3.1 Introduction

Environmental quality in developing countries is often low, resulting from wide-scale human-caused degradation of land and use of natural resources. The costs to health and productivity of this degradation are potentially large, and yet willingness-to-pay for environmental improvements seems to be low (Greenstone and Jack, 2013). One reason for this paradox could be that market failures and externalities lead to a large gulf between people's true valuation versus their revealed valuation of environmental quality. We wish to understand the extent to which such an externality can lead to a distortion in the demand for improvements in environmental quality.

In this chapter we focus on air pollution from wildfires in Indonesia. These large fires are lit for agricultural land-clearing allowing for extra income from increasing the available land for plantations. However, due to the particular ecology and climate in this region (van der Werf et al., 2008), the pollution from such fires can travel thousands of kilometers, and the fires themselves can smolder for long periods of time.

Air pollution, and in our specific case, fine particulate matter ($PM_{2.5}$) and ozone (O_3) can have large effects on public health, by exacerbating cardiovascular and respiratory diseases. A large literature in public health looks at these effects in adults, typically in developed countries (for example, Bell et al., 2007). Among developing countries, Indonesia has received comparatively more attention (Frankenberg et al., 2005; Marlier et al., 2012). Infant mortality has also been a focus of this literature, with many studies in public health attempting to understand the physiological response to certain pollutants (Kannan et al., 2006; Shah and Balkhair, 2011). Effects on infant mortality have been demonstrated in the United States (Chay, 2003) and in Indonesia (Jayachandran, 2009). Studies in Indonesia have previously used satellite measurements to overcome a lack of data.

Studies in economics have increasingly added to this literature by expanding the set of outcomes to include effects upon, for example, education and labor productivity (Chang et al., 2014; Lavy et al., 2012; Zivin and Neidell, 2012). The literature in economics has also added to the understanding of confounding in exposure measures (i.e., households sorting into high and low pollution areas), and the behavioral aspects of exposure by considering avoidance behaviors and their role in mitigating the effects of pollution (Moretti and Neidell, 2011). There has increasingly been a shift towards looking at impacts in the long-term, as summarized in Currie et al. (2013) and Graff Zivin et al. (2013).

Research into environmental impacts in developing countries can be a challenge, with little data on outcomes or exposure. Frequently, satellites are used to fill these data gaps. However, for air pollution, satellite remote sensing is an imperfect measure of pollution at the surface, and can lead to bias in the results. We overcome this by using a physical model of pollution, allowing us to isolate surface concentrations. We demonstrate the potential for long-term health and human development consequences of fine particulate matter ($PM_{2.5}$) exposure. Our analysis indicates that children exposed to high levels of $PM_{2.5}$ *in utero* are more likely to suffer from impaired physical development. These impacts have been demonstrated elsewhere to lead to fewer educational and labor market opportunities, constituting a long-term burden upon society above and beyond the immediate costs of mortality. We also examine whether contemporaneous exposure to $PM_{2.5}$ can lead to impairment of cognitive functioning, as suggested by Calderón-Garcidueñas et al. (2008, 2011) and examined for schooling outcomes by Lavy, Ebenstein and Roth (2012), though little other evidence exists. These impacts can be seen as a persistent burden to development in a country with this particular environment.

The rest of the chapter proceeds as follows: in section 2 there is a background discussion of air quality standards, the physical effects of $PM_{2.5}$ and O_3, and motivation for the climate modeling approach taken in this study. Section 3 discusses the data used in this analysis, focusing on the construction of the modeled pollution data. Section 4 details the econometric methods aplied. Section 5 presents results on physical development and cognition. The final section discusses some of the implications of the results.

3.2 Background

Low air quality can negatively impact human health in a number of ways, through cardiovascular and respiratory mortality in the short- (Bell et al., 2004, 2007) and long-term (Jerrett et al., 2009). Infant mortality has been robustly established in a number of cases (for example, Chay, 2003; Jayachandran, 2009) and other adverse birth outcomes (e.g., low birthweight) have been associated (Kannan et al., 2006), yet there is some uncertainty about the exact mechanism through which infants and fetuses may be affected.

The majority of the studies of ambient air pollution focus on the developed world, and yet the projected health effects in developing countries are large (Marlier et al., 2012). Unfortunately, there are a number of limitations to conducting research on air pollution in developing countries due to the inherent difficulty of monitoring both environmental quality and health outcomes. Three challenges present themselves:

- **Incomplete data on health and other outcomes of interest:** Health data on mortality is often inaccurate or underreported in many developing countries. When trying to identify a spatially-explicit exposure to environmental quality, differential reporting risks underestimating effects in rural areas. Moreover, data on morbidity often do not exist, and many non-mortality outcomes may go unreported. There is an additional concern that other outcomes of potentially economic interest (like long-term effects upon human capital) are also unreported.
- **Lack of air pollution monitoring data:** Outside of large urban centers, and even within them, many countries do not have air quality monitoring infrastructure. For example, in the area of the current study, Marlier et al. (2012) states that there are only 19 stations across the region, with only one O_3 and two $PM_{2.5}$ monitoring stations within Indonesia itself. Many of the other stations only report visibility measurements, and so are unable to distinguish between different types of pollution. This last point is important because different pollutant species (e.g., $PM_{2.5}$ versus O_3) have different health impacts.
- **Difficulty retrieving surface concentrations from remote sensing:** Satellites have previously been used (Frankenberg et al., 2005; Jayachandran, 2009) to identify effects of pollution on infant mortality in Indonesia, and air quality monitoring using satellite instruments like the Moderate Resolution Infrared Spectrometer (MODIS)[1] has become a common practice. However, a satellite measurement of air quality detects the integral of air quality over the entire atmospheric column. This can pose problems when conducting research on surface level exposure, as different fires can lead to pollution at different levels of the atmosphere. Andreae and Merlet (2001) explain that "smoldering emissions [...] tend to be emitted during less vigorous phases of a fire and therefore remain closer to the ground, while [...] emissions from the flaming phase [...] rise to higher altitudes". Therefore, the concentration at the surface and the column-integrated values obtained by satellites may not be perfectly correlated. This may introduce substantial bias into these earlier results.

To account for these three issues, we choose to focus on outcomes measured in a randomly sampled general purpose survey. This ensures that we overcome problems associated with reporting bias mentioned in point 1 above. For exposure, we derive historical time series of each pollutant by combining data from remote sensing of fires, a biogeochemical model to identify emissions, and a general circulation climate model to model the transport of pollution and isolate surface level exposures. Details of

[1] MODIS data are used in 4 to detect flooding exposure. They have clear advantages when detecting surface properties, but are considered unsuited to the current task.

each data source are discussed below. As the fires do not directly emit $PM_{2.5}$ and O_3, this model allows us to capture their secondary production as a result of fire emissions. GFED contributes black carbon, which combines with other aerosols (the finer particles of which make up $PM_{2.5}$). Ozone is formed in a reaction between sunlight and nitrogen oxides (NOx) or volatile organic compounds (VOCs). We are consequently able to isolate the surface level exposure for each pollutant separately.

Table 3.1: Summary statistics for pollution measures

Variable	Mean	(Std. Dev.)	Min.	Max.	N
$\mathbf{O_3}$ (parts per billion)	36	(13)	6	195	10352
$\mathbf{PM_{2.5}}$ (μgm^{-3})	12	(13)	1	405	10352

Summary statistics are at individual level exposures for all births from 1997-2007 in the IFLS sample. Value above is the value on the day of birth.

Health effects of $\mathbf{PM_{2.5}}$ and $\mathbf{O_3}$ $PM_{2.5}$ and O_3 can affect health and well-being through various physiological channels. $PM_{2.5}$ consists of particulates smaller than 2.5μm in diameter. Particulate matter consists of a mixture of solid and liquid particles suspended in the air, and has sources that are both natural (for example, sea spray and sand) and anthropogenic (for example, black carbon from incomplete combustion of fuels). Many other species of $PM_{2.5}$ are created in atmospheric reactions between other forms of pollution. Particulates enter the lungs and damage the respiratory system, and fine particulates can pass into the blood and affect many other parts of the body, in particular by limiting the oxygen binding capacity of blood cells. O_3 is not directly created by human activities, but is formed in a secondary reaction of nitrous oxides or volatile organic compounds in the presence of sunlight. O_3 exposure can immediately aggravate the respiratory system, as it reacts with the mucus linings of the nose, throat and lungs. These effects can be quick to manifest, so exposure can often be limited by going indoors, where O_3 quickly breaks down, unlike $PM_{2.5}$ which will persist.

Shah and Balkhair (2011) list the potential mechanisms for negative outcomes resulting from in utero exposure. It is noted that O_3 may have negative effects upon birthweight, but the mechanism is not fully understood. O_3 exposure may affect neurodevelopment, but the consequences of that have not been fully explored. $PM_{2.5}$ can affect a fetus through being absorbed into the bloodstream of the mother, and hence affecting blood flowing through the placenta. If $PM_{2.5}$ deposition in organs provokes an inflammatory response, it can lead to pre-term labor, though like with O_3, long-term effects of in utero exposure are not well understood. The fetal origins hypothesis of early life exposure to shocks has been a thriving area of research in economics (Almond and Currie, 2011). However, the events studied are frequently large uncommon shocks, whereas we focus here both on large shocks and on a more routinely experienced level of exposure, indicating that the effects we see are a routine part of life in this environment.

The World Health Organization sets global standards for air quality for all major pollutants (WHO, 2005)[2]. The air quality standard for $PM_{2.5}$ is 25 μgm^{-3} , while the standard for surface O_3 is 50ppb[3]. Pollution levels in our sample are significantly higher than these standards, as can be seen in table 3.1, and are frequently beyond levels which are used to calculate the *endangerment* of doses of pollution.

3.3 Data

3.3.1 Socioeconomic data

Data on socioeconomic outcomes are obtained from the Indonesian Family Life Survey (IFLS; Strauss et al., 2009). Outcomes related to health and socioeconomic status of individuals and households are extracted along with geographic identifiers, birth-timing, and survey dates in order to match spatio-temporally with air pollution data. The IFLS is a panel survey with four rounds that covers over 60,000 households, is nationally representative, and contains observations from 13 out of 27 Indonesian provinces. A map showing surveyed districts is displayed in figure 3.2A.

Physical development of children under five is measured using standardized height-for-age (HAZ), weight-for-age (WAZ), and weight-for-length (WHZ) measures (de Onis, 2006). These anthropometric measures are calculated for each child based on contemporary measurements of height, weight, and age, and standardized according to an international cohort sample from the World Health Organization[4]. Consequently, the sample for analyses involving physical development is restricted to children under five.

As a proxy for cognition, we use Raven's *Standard Progressive Matrices* (Raven's tests) results as well as performance on simple arithmetic tests. An example of a Raven's test question is given in figure

[2]WHO health standards for each pollutant can be found at:
http://www.who.int/phe/health_topics/outdoorair/outdoorair_aqg/en/

[3]WHO air quality standards report the O_3 standard as $100\mu gm^{-3}$ while we present results here in the customary ppb. The mass is converted to ppb using

$$\mu gm_{-3} = \frac{ppb \times 12.187 \times M}{Temperature}$$

where M is the molecular weight of O_3 and the denominator is the temperature in Kelvin. We assume the average temperature from table 3.2.

[4]Anthropometric measures are calculated using STATA packages provided by WHO found at: http://www.who.int/childgrowth/software/en/

3.7. The sample for cognitive outcomes comprises all household members between the ages of 7 in the most recent round (2007) and those aged less than 24 during the previous round (2000). We weighted each of the test questions by the inverse mean of the number of people answering correctly, allowing us to weight the final tests by difficulty. Scores are calculated as percentages and standardized test scores. Sample details are given in table 3.2. In addition to these two, other acute health outcomes (especially respiratory conditions) are obtained.

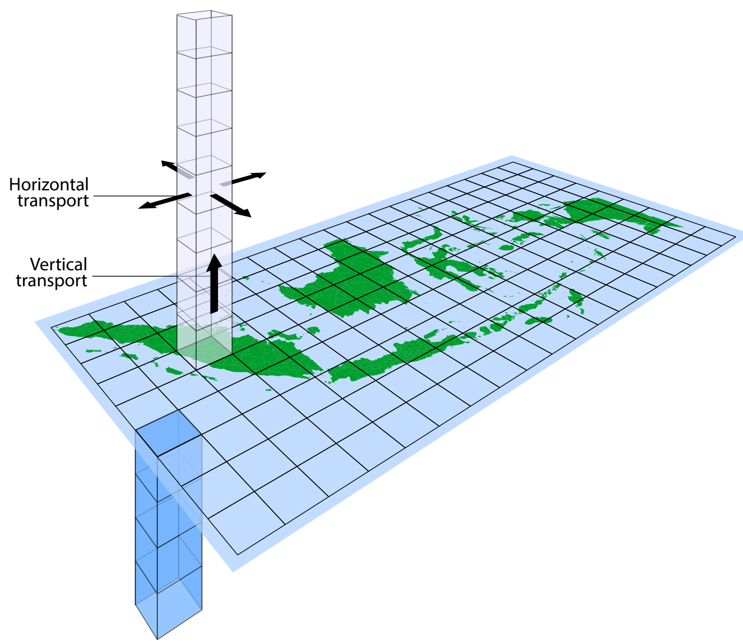

Figure 3.1: Heuristic figure of climate model over Indonesia.

3.3.2 Air Pollution Data

Air pollution data is obtained from a novel combination of two models:

1. **Global Fire Emissions Database version 3 (GFED):** A global fires emissions database that is created using satellite detection of active fires in combination with a biogeochemical model that tracks those fires and estimates the combustion characteristics, and hence emissions. These data are at 0.5°×0.5° resolution.

Table 3.2: Summary statistics

Variable	Mean	(Std. Dev.)	Min.	Max.	N
Panel A: In Utero Analysis					
Environmental variables					
$PM_{2.5}$ (trimester av., μgm^{-3})	12.9	(11)	0	221	4851
O_3 (trimester av., ppb)	35.8	(10.9)	0	116.7	4851
Temperature (birth month av., °C)	25.4	(1.5)	20	29.1	4833
Rainfall (birth month av., mm)	152.9	(101.5)	0	484.1	4833
Individual characteristics					
Sex	0.5	(0.5)	0	1	4851
Height-for-Age Z	-1.3	(2.7)	-22	51.6	4851
Weight-for-Length Z	0.3	(5.9)	-12.5	91.2	4792
Weight-for-Age Z	-0.7	(3.9)	-9.6	74.7	4829
Age	2	(1.5)	0	7	4851
Household characteristics					
Urban	0.5	(0.5)	0	1	4851
Uses wood/charcoal stove	0.3	(0.5)	0	1	4849
Owns house	0.7	(0.5)	0	1	4849
Smoker in family	0.7	(0.5)	0	1	4845
Panel B: Cognition Analysis					
Environmental variables					
$PM_{2.5}$ (test day, μgm^{-3})	10.3	(5.8)	0.6	39.6	11083
O_3 (test day, ppb)	32.5	(10.3)	6.4	81.6	11083
Termperature (test day, °C)	26.6	(1.4)	23.3	30.4	11198
Individual characteristics					
Age	13.5	(4.5)	7	27	11201
Sex	0.5	(0.5)	0	1	11201
Ever attended school	1	(0.1)	0	1	4312
Raven's test score (%)	59.6	(23.5)	0	100	11201
Test duration	11.6	(19.1)	0	1090	11200
Household characteristics					
Urban	0.5	(0.5)	0	1	11201
Uses wood/charcoal stove	0.4	(0.5)	0	1	11163
Owns house	0.8	(0.4)	0	1	11163
Smoker in family	0.7	(0.5)	0	1	11166

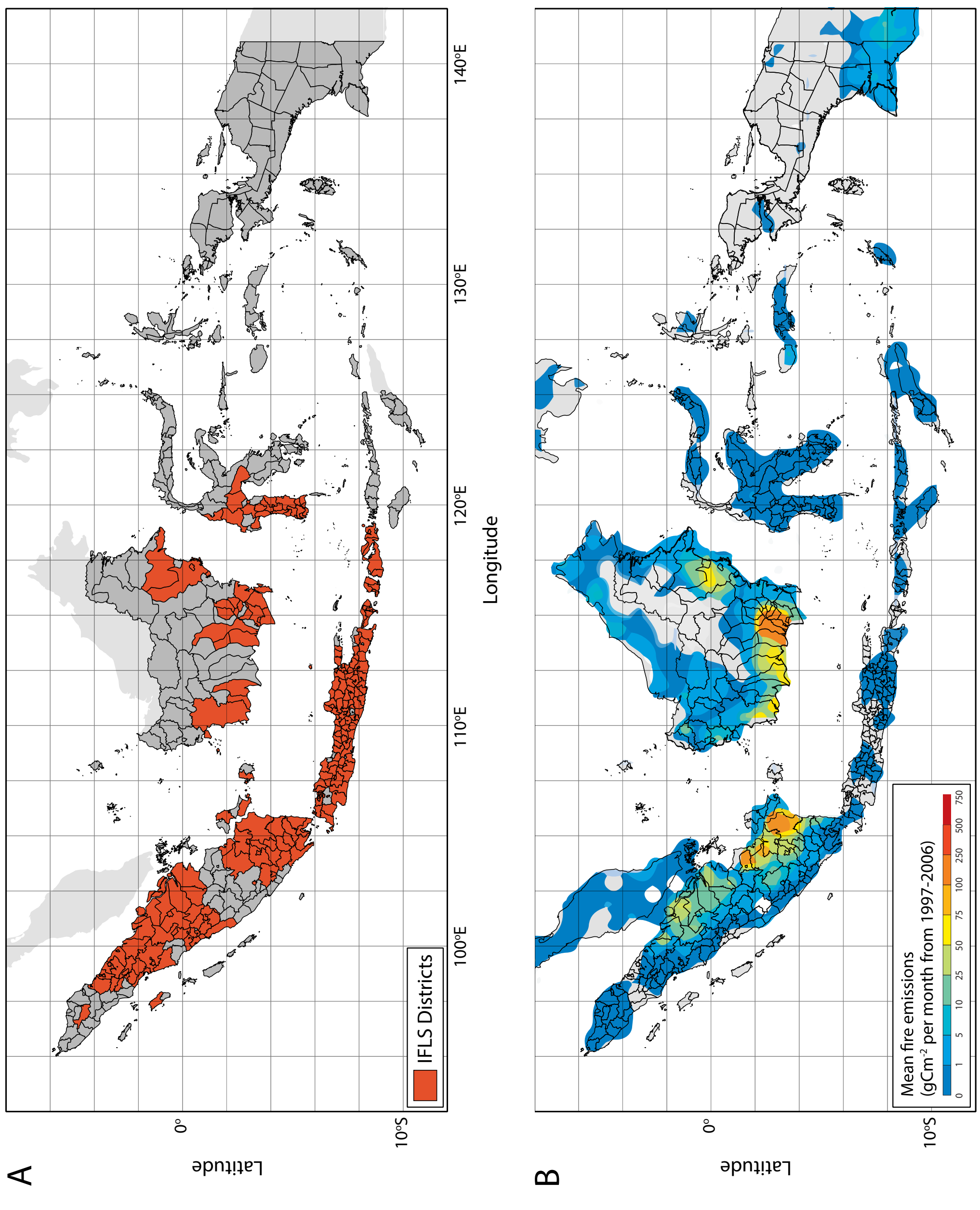

Figure 3.2: **A:** Map of districts surveyed in the Indonesian Family Life Survey. **B:** Map of mean fire emissions (in gCm^{-2} per month) averaged over 1997-2008 taken from the Global Fire Emissions Database.

2. **NASA Goddard Institute of Space Studies (GISS) General Circulation Model (GCM):** The NASA GISS-E2-PUCCINI (*hereafter* GISS) model is a global climate model. For our purposes, it is run with 40 vertical layers and at a spatial resolution of $2° \times 2.5°$. Figure 3.1 illustrates the resolution of the model over Indonesia. Both vertical and horizontal transport of energy and mass within the atmosphere are simulated. The presence of 40 atmospheric layers allows us to extract surface layer pollution. The model is run over the period from 1997 to 2008, with observed meteorological fields.

Emissions of both $PM_{2.5}$ and O_3 were calculated as in Marlier et al. (2012), by combining these two models. Fire emission data from GFED is used to drive emissions in the GISS model. The surface of Indonesia in the GISS model is loaded with time-varying surface level emissions from GFED. Atmospheric dynamics and historical meteorological observations cause the various emitted particles to move both horizontally and vertically in the atmosphere. The GISS model features components to capture atmospheric chemistry and aerosols, to allow for the creation of O_3 and $PM_{2.5}$ through secondary reactions as discussed in the previous section. The fire emissions also contribute black and organic carbon, which are components of $PM_{2.5}$ in addition to sea salt, dust, and sulfate. Data from the model are displayed in figure 3.4.

We follow Marlier et al. (2012) and model 24-hour surface $PM_{2.5}$ concentrations and 12pm-3pm O_3 surface concentrations[5] with the GISS model.

Model validation

Data from the model are obtained through a single run, and so it is difficult to understand the level of uncertainty or potential measurement error involved in the model. As such, a number of model validation procedures are performed to assess the accuracy of the model. The lack of gauged observations in Indonesia has been previously noted. However, we can compare the existent gauge measurements with the data from the model to verify its accuracy. Figures 3.3A and 3.3B, reproduced from Marlier et al. (2012), shows GISS $PM_{2.5}$ plotted along with the "extinction coefficient", a measure of the scattering of light due to particles in the atmosphere, averaged over all stations in Indonesia. While not a perfect measure of particulate matter in the atmosphere, given the lack of instrumental records of particulates, it provides the most direct comparison. This should be highly positively

[5] Typical O_3 values used in air quality standards are calculated in 8-hour intervals. We find that the three hour concentrations are an equivalent measure of peak ozone.

correlated with $PM_{2.5}$ in the atmosphere, which we can see from the figure[6]. Modeled O_3 values show a much higher correlation with observational data when compared to daily O_3 gauges. Figure 3.3C shows the monthly time-series for a single O_3 monitor in Bukit, Indonesia[7].

3.4 Methods

We use geographic locations given in the IFLS survey to match observations to daily $PM_{2.5}$ values. They are matched in two ways for different analyses: 1) by birth date for early life exposure, and 2) by survey date for contemporaneous exposure. We then model the response of various outcome variables as a function of our $PM_{2.5}$ measure, P, in the k periods before and after the critical exposure period. We model in utero impacts using

$$Y_{i,T} = \sum_{L=-3}^{2} [\beta_L \times P_{i,t+L}] + \sum_{L=-3}^{2} [\gamma_L \times Z_{i,t+L}] + \theta_i + \delta_t + \epsilon_{i,T} \tag{3.4.1}$$

where $Y_{i,T}$ is an anthropometric outcome for individual i measured at the time of the survey, T. P is the average of $PM_{2.5}$ over three month periods, match to birth date (t), and we include lags for all three trimesters in utero, and the two quarters after birth (referred to for convenience as the 4^{th} and 5^{th} trimesters). Z consists of climate (temperature and precipitation) and O_3 controls. θ_i accounts for unobservable differences between districts, δ_t accounts for common shocks at the time of birth, and $\epsilon_{i,t}$ is a normal error term. Coefficients of interested are the β_L terms, which describe the effects of lagged pollution measures derived from the model. For identification, we assume that individuals do not time their conception based upon air pollution exposure.

For models of cognition, we wish to understand how random variations of pollution on the day of exposure will affect cognition. This is in line with a number of other findings (Chang et al., 2014; Lavy et al., 2012; Zivin and Neidell, 2012) looking at productivity and educational outcomes. We estimate the equation

$$score_{i,t} = \sum_{L=-3}^{0} [\beta_L \times P_{i,t+L}] + \sum_{L=-3}^{0} [\gamma_L \times Z_{i,t+L}] + \theta_i + \delta_t + \kappa_i \times X_{i,t} + \epsilon_{i,T} \tag{3.4.2}$$

where $score$ is the test score on day t, $X_{i,t}$ is a vector of individual and household controls, and all other terms are as before. Note that exposure is measure on the day of the test, and lags are

[6] The lack of a true "zero" value in the extinction coefficient is due to the lower bound on scattering of light due to presence of a gaseous atmosphere

[7] For more information on model validation, interested readers are referred to Marlier et al. (2012)

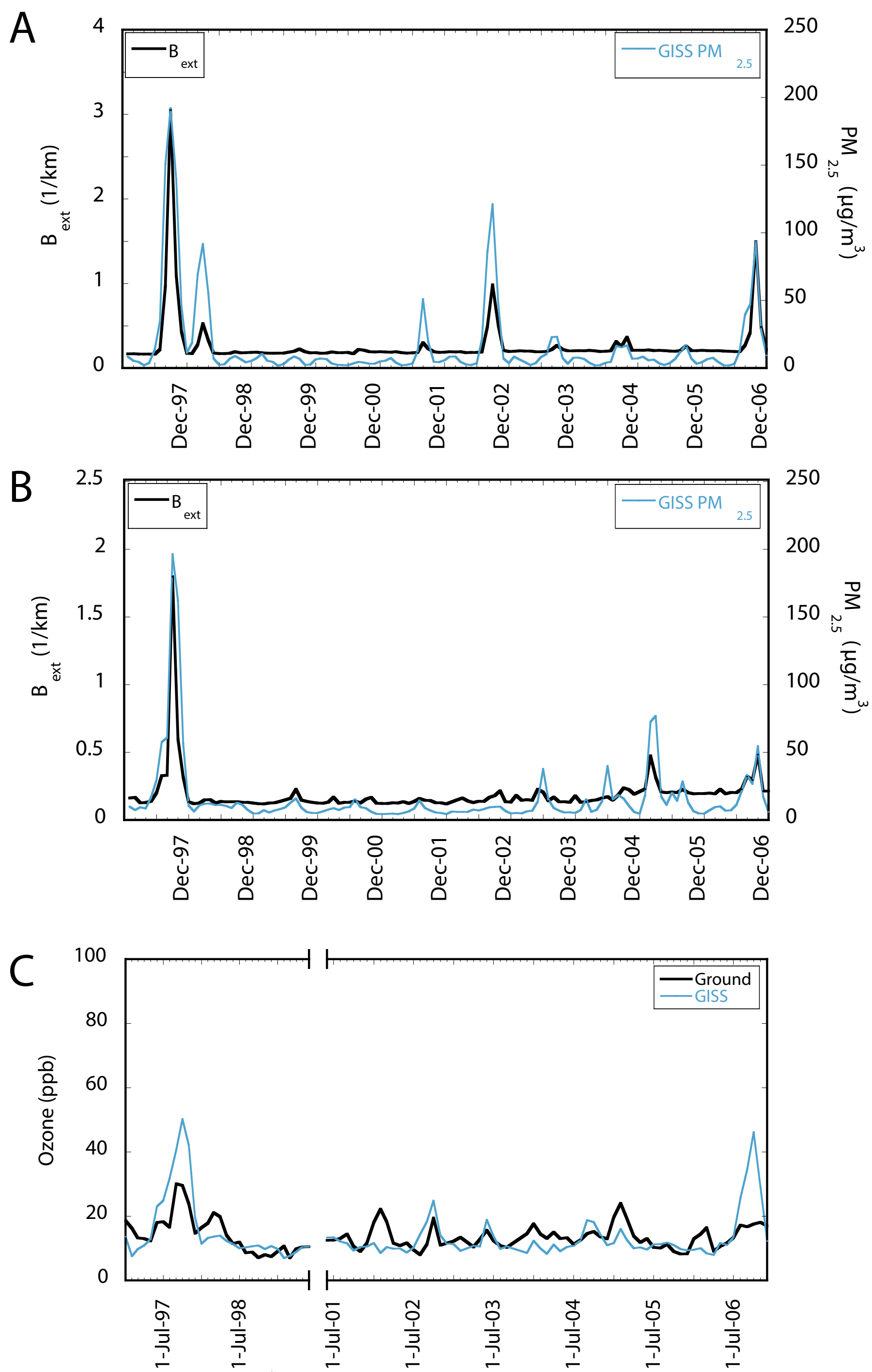

Figure 3.3: Validation of modeled $PM_{2.5}$ for (A) non-Borneo and (B) Borneo fires. (C) Validation of modeled O_3 comparing a single monitor within Indonesia. Reproduced from Marlier et al. (2012).

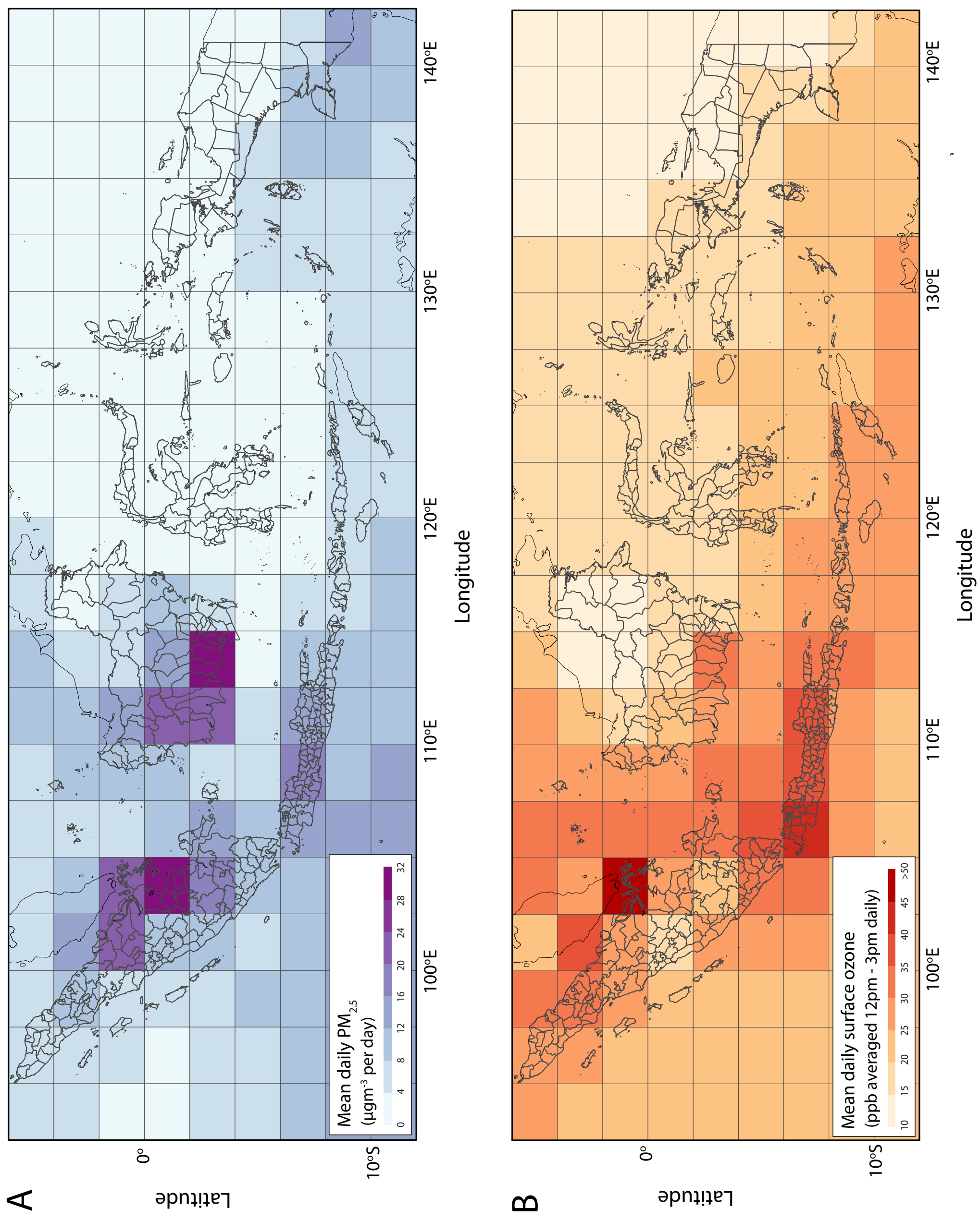

Figure 3.4: **A:** Annual average of daily $PM_{2.5}$ from 1997-2008, derived frm the NASA GISS GCM. **B:** Annual average of daily O_3 from 1997-2008, derived frm the NASA GISS GCM.

included to correct for serial correlation in the pollution measures. The identifying assumption is that surveys were randomly rolled out across the country, and were not timed based on days of high or low air quality. Though fire activity is restricted to certain seasons and areas of Indonesia, because the resulting pollution is affected by broad weather patterns (included in the model data) and atmospheric circulation, we assume that P is exogenous. All standard errors are clustered at the climate-model pixel level, which will account for spatial correlation within pollution exposure.

3.5 Results

3.5.1 *In utero* impacts

Table 3.3 shows result of early life exposure to $PM_{2.5}$ upon physical development—in this case “height-for-age” Z-scores (HAZ). The preferred model is that in column (1), with district, birth-month and birth-year fixed effects. Our independent variable is the average $PM_{2.5}$ within the stated period. For convenience throughout, we refer to the first and second three month periods after birth as the 4^{th} and 5^{th} trimesters.

A one μgm^{-3} increase in the 90-day average $PM_{2.5}$ experienced during the second trimester of pregnancy leads to a 0.05 standard deviation decrease in height-for-age at the time of measurement. This is enough to move nearly 2% of the non-stunted children in our sample into a condition of stunting. This effect remains after excluding districts that have the largest active fires in the country, and so the exposed individuals are being affected at a distance.

Changes to height resulting from impaired physical development are more likely to persist, and will not respond as quickly to an improved environment or nutrition as weight will. For this reason, we predict that weight-for-age and weight-for-height ratios will not show strong evidence of an *in utero* effect. Table 3.6 lends some support to this idea, while also indicating some potentially positive effects of fires. This may be due to the relationship between widespread burning and agricultural productivity in the following year, though this requires further study.

Robustness

Climate and O_3 controls A number of studies demonstrate the effects of other climate variables upon birth outcomes. Deschênes et al. (2009) show that high temperatures at the time of birth leads to increased likelihood of low birthweight. Though it is unclear how birthweight is related to later

Table 3.3: Main result: HAZ

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	**Height-for-Age Z-score**						
Sample restrictions		All		No high fires	No fires	Boys	Girls
Trimester average $PM_{2.5}$							
1^{st} trimester	0.00245*	0.00171	0.00262*	0.000192	0.00317	0.00551***	0.000495
	(0.00139)	(0.00159)	(0.00143)	(0.00691)	(0.00937)	(0.00165)	(0.00280)
2^{nd} trimester	-0.00505***	-0.00401***	-0.00530***	-0.00880**	-0.00777	-0.00619***	-0.00344
	(0.00145)	(0.00114)	(0.00153)	(0.00350)	(0.00467)	(0.00187)	(0.00247)
3^{rd} trimester	-0.00206	-0.00186	-0.00185	-0.00972**	-0.00382	0.000456	-0.00404
	(0.00262)	(0.00295)	(0.00255)	(0.00404)	(0.00569)	(0.00722)	(0.00248)
4^{th} trimester	0.000789	0.00175	0.00117	-0.00491	-0.00253	0.000522	0.00242
	(0.00223)	(0.00227)	(0.00305)	(0.00467)	(0.00387)	(0.00356)	(0.00347)
5^{th} trimester	-0.00257	-0.00200	-0.00201	-0.000870	0.00670	0.00228	-0.00438
	(0.00320)	(0.00271)	(0.00329)	(0.00769)	(0.00488)	(0.00479)	(0.00305)
Birth-month FE	Y		Y	Y	Y	Y	Y
Birth-year FE	Y		Y	Y	Y	Y	Y
Birth-month×year FE		Y					
Controls			Y			Y	Y
Observations	4711	4711	4679	4219	4153	2420	2259
Adjusted R^2	0.081	0.083	0.093	0.078	0.078	0.084	0.113

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

physical development (Currie and Rossin-Slater, 2012), we control for temperature in the same periods as our $PM_{2.5}$ exposure. Precipitation at birth has been demonstrated by Maccini and Yang (2009) to have long term effects upon human capital outcomes, though this effect is largely economic, rather than physiological. Finally, though the mechanism through which O_3 can affect birth outcomes is ambiguous, we also consider it important to control for O_3 in our regressions. Table 3.4 includes each of these factors in turn, followed by a model with all simultaneously included. Column (1) is the main model. We see that the coefficient on the second trimester of $PM_{2.5}$ remains negative and significant through all models, though it decreases in magnitude. It should be noted that none of the estimates is significantly different between models.

Thresholds and air quality standards Air quality standards are presented as absolute thresholds, as discussed in previous sections. We use these thresholds to see if there may be a non-linear effect of days at or above these thresholds. In table 3.5, we see that for thresholds of 25, 50, and $100\mu gm^{-3}$, the effects remain negative for second trimester exposure. Higher levels display higher responses, implying some non-linearity in the response function at high levels. However, the effects sizes from the lower threshold are equivalent in magnitude to the previous results.

Migration Migration is a concern if it results in incorrect assignment of exposure at birth. To examine this, we run regressions restricting the sample to only those households that are classified as having not moved since the previous round of the survey. We do not know the exact timing of moving, so migration is treated in two ways: either the family moved to the current location immediately after the previous round, in which case our estimates are accurate, or they moved immediately before the current survey round, in which case the restriction to only non-moving households is correct. The truth is, of course, in between these two cases, so the results could be seen as a bound on the coefficients. Results are shown in table 3.8. We see that restriction to non-moving households does increase the effect size, but it is not significantly different from the effect on all households. This leads us to prefer the model with all households as the more conservative estimate.

It could also be possible that, in accordance with discussions on avoidance, households will take behavioral responses to mitigate some of the effects of pollution. Using a linear probability model, we predict the likelihood of a household being recorded as having moved to a new district. We see that the coefficient on $PM_{2.5}$ in the first three months after birth[8] is both significant and an order of magnitude

[8] Table omitted.

Table 3.4: Climate controls

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	**Height-for-Age Z-score**					
Trimester average $PM_{2.5}$						
1^{st} trimester	0.00245*	0.00257	0.00251*	0.000430	0.00118	0.00113
	(0.00139)	(0.00153)	(0.00139)	(0.00160)	(0.00164)	(0.00178)
2^{nd} trimester	-0.00505***	-0.00511***	-0.00467***	-0.00540***	-0.00394*	-0.00389*
	(0.00145)	(0.00158)	(0.00142)	(0.00171)	(0.00204)	(0.00211)
3^{rd} trimester	-0.00206	-0.00173	-0.00321	-0.00485*	-0.00499*	-0.00530**
	(0.00262)	(0.00256)	(0.00266)	(0.00271)	(0.00257)	(0.00252)
4^{th} trimester	0.000789	0.00133	0.000335	0.00364*	0.00270	0.00455
	(0.00223)	(0.00231)	(0.00279)	(0.00181)	(0.00191)	(0.00284)
5^{th} trimester	-0.00257	-0.00306	-0.00226	-0.00411	-0.00456	-0.00405
	(0.00320)	(0.00317)	(0.00284)	(0.00319)	(0.00330)	(0.00317)
Trimester average O_3						
1^{st} trimester				0.00374	0.00326	0.00359
				(0.00283)	(0.00315)	(0.00308)
2^{nd} trimester				0.000307	-0.00186	-0.00207
				(0.00183)	(0.00228)	(0.00242)
3^{rd} trimester				0.00488**	0.00440	0.00514
				(0.00225)	(0.00289)	(0.00305)
4^{th} trimester				-0.00467*	-0.00294	-0.00409
				(0.00263)	(0.00262)	(0.00256)
5^{th} trimester				0.00285	0.00306	0.00324
				(0.00242)	(0.00363)	(0.00345)
Temperature		Y			Y	Y
Precipitation			Y		Y	Y
O_3				Y	Y	Y
Other controls						Y
Observations	4711	4711	4711	4711	4711	4679
Adjusted R^2	0.081	0.082	0.083	0.081	0.084	0.095

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: Thresholds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	25 μgm^{-3} threshold days			50 μgm^{-3} threshold days			100 μgm^{-3} threshold days		
Trimester average PM$_{2.5}$									
1st trimester	0.000821	0.000610	-0.000176	0.00600**	0.00488	0.00509	0.00696**	0.00419	0.00360
	(0.00338)	(0.00420)	(0.00332)	(0.00270)	(0.00298)	(0.00342)	(0.00277)	(0.00306)	(0.00474)
2nd trimester	-0.00428**	-0.00296*	-0.00179	-0.00889***	-0.00620**	-0.00686*	-0.0145***	-0.0117***	-0.0124***
	(0.00161)	(0.00145)	(0.00301)	(0.00232)	(0.00233)	(0.00335)	(0.00366)	(0.00319)	(0.00417)
3rd trimester	-0.00268	-0.00208	-0.00589	-0.000931	-0.000816	-0.00569	-0.00572	-0.00543	-0.0113
	(0.00404)	(0.00494)	(0.00366)	(0.00643)	(0.00703)	(0.00690)	(0.00750)	(0.00752)	(0.00769)
4th trimester	0.000705	0.00119	0.00308	0.000213	0.00233	0.00204	-0.00400	-0.000481	-0.00202
	(0.00261)	(0.00292)	(0.00325)	(0.00397)	(0.00415)	(0.00464)	(0.00560)	(0.00582)	(0.00563)
5th trimester	-0.00250	-0.00138	-0.00362	-0.00150	-0.000768	-0.00484	-0.00241	-0.00230	-0.00609
	(0.00382)	(0.00347)	(0.00375)	(0.00568)	(0.00506)	(0.00582)	(0.00658)	(0.00552)	(0.00657)
Birth-month FE	Y		Y	Y		Y	Y		Y
Birth-year FE	Y		Y	Y		Y	Y		Y
Birth-month×year FE		Y			Y			Y	
P, T, O$_3$ Controls			Y			Y			Y
Observations	4711	4711	4711	4711	4711	4711	4711	4711	4711
Adjusted R^2	0.081	0.083	0.083	0.081	0.083	0.084	0.081	0.083	0.084

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

greater than that of other exposure periods. A one standard deviation shock in $PM_{2.5}$ would make a household approximately 1.5% more likely to migrate. This would suggest that households with newborn children may take defensive measures out of concern for their child's health. Unfortunately, as indicated by the above results, much of the damage may have occurred in the previous six months of exposure.

Randomization We consider $PM_{2.5}$ exposure to be a function of some set of geographic and ecological characteristics, X_{it}, that are location-specific, as well as a random shock term c_i.

$$smoke_{it} = X_{it} + c_i \tag{3.5.1}$$

If we are concerned that some spurious relationship is driving these results, we could expect there to be a correlation between c_i above and the error term in our models. To examine whether some spurious relationship, correlated with smoke exposure, may be driving our results, we perform a randomization test that is designed to break the association. We randomize within district for the strictest possible test, as any spurious cross-sectional patterns driving the results will remain after a reordering of temporal characteristics within that district.

We re-estimate two models for the randomization procedure. First, we run a model with average $PM_{2.5}$ and O_3 and no controls (in fig. 3.5A), and second we run a model with full controls for temperature, precipitation, and ozone, as well as "endogenous" regressors – asset wealth, cookstove, and smoking (in fig. 3.5B). Each model is randomized within district 10,000 times. Based on the discussion above, if there was a strong cross-sectional correlation driving the results between districts (i.e., if some omitted geographical or social factor that differed between locations led to spurious results) or if the treatment was correlated with the anthropometric outcomes, we would expect to see a distribution of coefficients that was centered on a value other than zero. In both cases, displaying only the coefficient on the second trimester, we see that the distribution is approximately normal and centered on zero, implying that there is no such spurious association driving our results. Calculating exact p-values from these results, both original coefficients (vertical line in fig. 3.5) have $p_{exact} < 0.01$.

Other anthropometric measures In columns (1) to (3) of table 3.6 , we compare the response of HAZ with other anthropometric measures. HAZ is a measure of chronic health or malnutrition problems, whereas the other two measures displayed—weight-for-height (WHZ) and weight-for-age

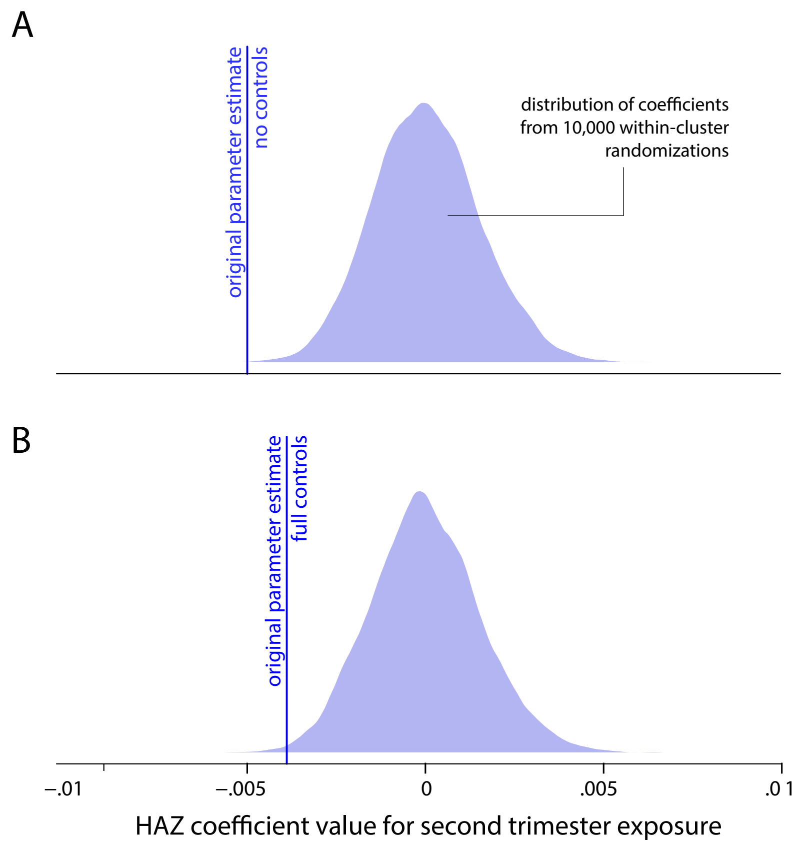

Figure 3.5: **A:** Distribution of coefficients from 10,000 iteration within-cluster randomization of model with no controls. Original estimate plotted as vertical line. **B:** Same as A, except model has precipitation, temperature, and ozone, as well as endogenous controls.

(WAZ)—are measures of *acute* health impacts and are not necessarily correlated with HAZ (Victora, 1992). A child would have a low WHZ or WAZ if in the recent past they were subjected to a nutritional shock, for example. We hypothesize that these measures should not respond to exposure at birth. Indeed, we see no effect of in utero exposure to air pollution on acute nutritional measures. This lends further robustness to the result, as the measures of WHZ and WAZ are often highly correlated with poverty, implying that our effect is not merely caused by cross-sectional differences in incomes across Indonesia.

Selection and scarring Another issue of concern is that air pollution increases infant mortality, as seen in Jayachandran (2009). As Shah and Balkhair (2011) suggests, negative birth outcomes due to pollution exposure will underestimate the true effect, since weaker fetuses will not survive until birth – an effect known in the literature as "harvesting". One approach towards controlling for this selection effect would be to estimate the direct infant mortality and miscarriage rates due to pollution exposure. We could then use this rate to increase our sample with false observations corresponding to the missing children, assuming that each of the children who did not survive would have had the worst observed outcome in the dataset. Unfortunately, our data makes this exercise impossible. In columns 4 to 7 of table 3.6, we present suggestive evidence of "harvesting". Columns (4) and (5) show the effect in utero exposure on reporting a cough in the 4 weeks before the survey was taken. We see that those exposed in their second and third trimester are *less likely* to have reported a respiratory problem. This would suggest that the surviving cohort may have been selected as those with stronger respiratory systems. For robustness, we see no similar effect with diarrhea, which should not be affected. The effect on coughing is, however, small, and leads us to have confidence in the, albeit underestimated, effect in our main results.

3.5.2 Cognitive impacts

Table 3.7 shows the results for the effect of contemporaneous exposure to high-levels of $PM_{2.5}$ on cognitive ability. The outcome variable is a standardized, difficulty-weighted test score. Independent variables are $PM_{2.5}$, O_3 , and temperature on the day of the test. Three lags are included, but not shown. Results from column (3), with both day-of-week and week-of-year fixed effects, are preferred. The effect of a one standard deviation increase in $PM_{2.5}$ roughly corresponds, on average, to a decrease of 2-4% on a test score, equivalent to answering one question out of twenty incorrectly. It should be

Table 3.6: Other anthropometric measures and health outcomes

	(1)	(2)	(3)		(4)	(5)	(6)	(7)
Dependent variable	HAZ	WHZ	WAZ		All cough		Diarrhea	
Trimester average $PM_{2.5}$								
1^{st} trimester	0.00245*	0.000701	0.00264*		-0.000234	-0.000218	-0.0000721	0.0000317
	(0.00139)	(0.00188)	(0.00131)		(0.000170)	(0.000255)	(0.000186)	(0.000223)
2^{nd} trimester	-0.00505***	-0.000271	-0.00189		0.000360	-0.0000427	0.0000826	-0.0000356
	(0.00145)	(0.00164)	(0.00203)		(0.000274)	(0.000342)	(0.000187)	(0.000259)
3^{rd} trimester	-0.00206	-0.00165	-0.00127		-0.000569***	-0.000476*	0.0000213	0.0000354
	(0.00262)	(0.00205)	(0.00152)		(0.000183)	(0.000271)	(0.000184)	(0.000196)
4^{th} trimester	0.000789	0.00309	0.00268		0.00162**	0.000556	0.000270	0.000180
	(0.00223)	(0.00229)	(0.00213)		(0.000769)	(0.000664)	(0.000238)	(0.000230)
5^{th} trimester	-0.00257	-0.00246	-0.00254		-0.00153***	-0.000484	-0.000345	-0.000230
	(0.00320)	(0.00249)	(0.00239)		(0.000278)	(0.000437)	(0.000301)	(0.000304)
P, T, O_3 controls						Y		Y
Observations	4711	4602	4773	10145	10145	10145	10145	
Adjusted R^2	0.081	0.052	0.096	0.076	0.077	0.061	0.061	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

noted that, although these effects are modest, the levels of exposure that we observe for these test days is far below the mean in the sample period as a whole. Our identifying variation here is the random roll-out of surveys across the country, but this occurred during a low fire year, and predominantly in the less polluted time of year. The 90^{th} percentile event in this sample of test takers for both $PM_{2.5}$ and O_3 is actually *below* that of both the WHO standards[9]. This would imply that significant effects could result from even low levels of exposure, and that exposure at the highest levels in the sample may have large effects on cognition.

Robustness

Lag length selection Results in table 3.7 include lags of three days of pollution. We control for lags for two reasons. First, we worry that pollution exposure on the previous days could influence health, cognition or behavior on the day of the test. The effect on health and cognition would be primarily physiological, but there is also a possibility of behavioral changes in line with the findings on avoidance behavior and air pollution (Moretti and Neidell, 2011). It is possible that bad pollution on previous days would lead to test-takers shifting activity indoors, thus becoming less exposed and improving their outcomes on the test. Second, each of the pollution measures shows a high degree of autocorrelation. Without the inclusion of any lags, the main independent variable would be correlated with the residual, leading to bias that would make inference impossible. To choose the number of lags included in the model specification, we ran the main model with zero controls with zero up to thirty daily lags. Figure 3.6 plots the coefficient on $PM_{2.5}$ on the day of the test, with various numbers of lags. There is a clear jump between zero and one lags, and then the coefficient appears to stabilize. Note that none of the coefficients are significantly different from each other. We choose the model with three lags as it is a conservative estimate of the effect size of contemporaneous $PM_{2.5}$ exposure.

3.6 Discussion

These results allow us to draw two separate conclusions:

- the first is that, in lieu of having access to accurate monitoring and health data, modeled and appropriately validated data on air quality, combined in a novel way with an all-purpose dataset, can be used to understand the impacts of air quality on health.

[9] Our sample of test-takers experiences a 90^{th} percentile event as $18.2\mu gm^{-3}$ of $PM_{2.5}$ compared to a $25\mu gm^{-3}$ WHO standard, and 46.6ppb of O_3 compared to a 50ppb WHO standard.

Table 3.7: Cognitive effects

	(1)	(2)	(3)
Dependent variable	Difficulty-weighted test score (Z)		
Environmental exposure			
$PM_{2.5}$	-0.00582**	-0.00401*	-0.00419*
	(0.00259)	(0.00219)	(0.00215)
O_3	0.00269	0.00316	0.00323
	(0.00236)	(0.00245)	(0.00247)
Temperature	0.0404	0.0400	0.0428
Household characteristics			
Urban	0.0949**	0.0910*	0.0923*
	(0.0434)	(0.0529)	(0.0529)
Asset wealth	0.135***	0.131***	0.131***
	(0.0150)	(0.0144)	(0.0143)
Smoker	-0.0701**	-0.0774***	-0.0767***
	(0.0271)	(0.0273)	(0.0274)
Charcoal/wood stove	-0.0516*	-0.0550**	-0.0563**
	(0.0267)	(0.0238)	(0.0240)
Age	0.326***	0.322***	0.322***
	(0.0177)	(0.0172)	(0.0172)
Age^2	-0.00972***	-0.00958***	-0.00959***
	(0.000610)	(0.000589)	(0.000589)
Day-of-week FE	Y		Y
Week-of-year FE		Y	Y
Observations	10707	10707	10707
Adjusted R^2	0.214	0.216	0.216

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

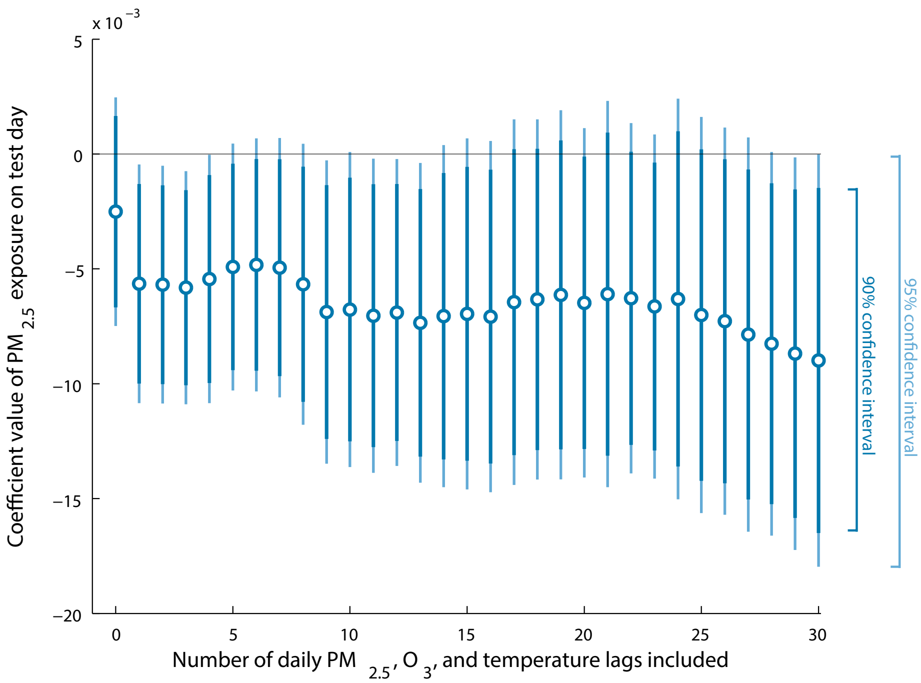

Figure 3.6: Coefficient of $PM_{2.5}$ on test day for 31 models each run with an increasing number of pollution lags. Note that each point is the same coefficient from a *separate regression* with an increasing number of lags, not the lag coefficients themselves.

- the second, more importantly, lets us begin to understand some of the medium- to long-term impacts of exposure to $PM_{2.5}$ at different critical periods through the life-cycle. That the result found for weight-for-age, a measure of nutrition, is weaker or absent, this implies that the effect is physiological, not economic.

As noted, the 90^{th} percentile event in our sample of test takers for both $PM_{2.5}$ and O_3 is actually *below* both of the WHO standards. A 90^{th} percentile event is $18.2\mu gm^{-3}$ of $PM_{2.5}$ compared to a $25\mu gm^{-3}$ WHO standard, and 46.6ppb of O_3 compared to a 50ppb WHO standard. This would imply that significant effects could result from even low levels of exposure, and that exposure at the highest levels in the sample may have large effects on cognition. It would also suggest that, like Chang et al. (2014), the standards may have been set too high, giving important guidance for policy makers, and suggestive evidence about not only the educational impacts, but the productivity impacts as well.

One other point to note is that of comparable effects sizes between $PM_{2.5}$ and the coefficients on being a smoking household or having a woodstove. In the regression, we have controlled for household wealth, and so the effect of a woodstove, beyond just being associated with lack of wealth, is -0.052. A 90^{th} percentile day in the sample, even with very low exposure as we have here, has an effect size of -0.076, which is larger than the effect of a woodstove. Indoor air pollution from cookstoves has received massive policy attention to date, but these results would suggest that ambient air pollution may be having effects of similar magnitudes.

These results are important because they point to an often ignored societal impact of environmental exposure. It is known from other studies that shocks in early life can persist throughout the life-cycle, in effect programming an individual for lower health, education, and labor market prospects. We show that this potentially large negative cost can be attributed the specific environment of Indonesia, and must be considered when cost-benefit analyses are performed. It must also be recognized that the geography of Indonesia imposes a specific cost upon economic activity there, of which we have looked at only a small part. Development policy in this part of the world must take this geography-specific component into consideration. This is particularly important given the insidious nature of these impacts. Their long-term and unobservable nature ensures that the negative consequences will likely not be taken into consideration by those affected, and the exact cause of the impacts may go unattributed. This lack of information will likely lead to a market failure that could partly explain the low willingness to pay for environmental improvements observed in Indonesia and other developing countries.

Appendix

3.A Supplementary Tables and Figures

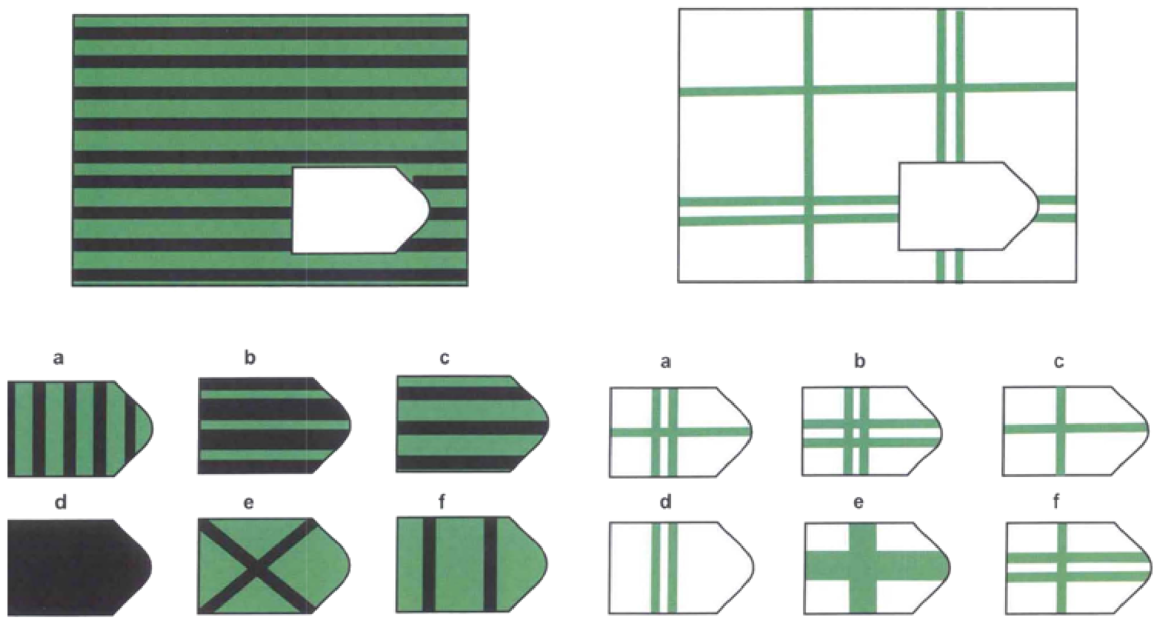

Figure 3.7: An example of two questions from Raven's Standard Progressive Matrices used in the IFLS.

Table 3.8: Migration

	(1)	(2)	(3)	(4)
Model	No controls		Full controls	
Sample restrictions	All	Non-movers	All	Non-movers
Trimester average $PM_{2.5}$				
trimester 1 average	0.00245*	0.00448***	0.00262*	0.00459***
	(0.00139)	(0.00130)	(0.00143)	(0.00139)
trimester 2 average	-0.00505***	-0.00532***	-0.00530***	-0.00544***
	(0.00145)	(0.00157)	(0.00153)	(0.00167)
trimester 3 average	-0.00206	-0.00235	-0.00185	-0.00199
	(0.00262)	(0.00163)	(0.00255)	(0.00150)
trimester 4 average	0.000789	0.00236	0.00117	0.00299
	(0.00223)	(0.00267)	(0.00305)	(0.00395)
trimester 5 average	-0.00257	0.000359	-0.00201	0.000666
	(0.00320)	(0.00250)	(0.00329)	(0.00266)
Observations	4711	4061	4679	4039
Adjusted R^2	0.081	0.091	0.093	0.102

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 4

The Impacts of Flooding on Human Capital Formation in Bangladesh

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[1]This research was partly funded by a grant from the International Growth Centre (IGC).

Abstract

Flooding is one of the costliest and deadliest natural disasters on the planet, affecting more people each year than any other disaster. South Asia is a particularly flood-prone region, with the nation of Bangladesh—situated on a delta—being especially vulnerable. Yet, the full impacts of this natural hazard are not well understood. We aim to understand what medium- and long-term impacts are associated with exposure to flooding in early life. By creating a measure of flood extent using satellite observations for all of Bangladesh in the period 2000-2013 and matching this to outcomes on physical development of infants, we analyze one hidden cost of natural disaster impacts in this low-lying developing country. We find that exposure to abnormal floods at the time of birth or *in utero* leads to an increase in stunting by approximately 2% and an overall decline in standard measures of height-for-age among children under 5 years of age. Flooding exposure appears to affect boys more than girls. We also find evidence of adaptation—households that are routinely exposed to larger or more frequent floods experience smaller impacts in the event of an abnormal flood than those who are exposed less often. By examining impacts on first-born versus other children, we see that investments may partially compensate for this effect. These results on physical development are important because lower height-for-age ratios and stunting are associated with high infant mortality and illness and more broadly with negative educational and labor market outcomes in later life. This could represent a significant extra cost associated with environmental impacts in Bangladesh and other flood-prone developing countries, one that will be exacerbated as climate change increases the frequency and severity of flooding over South Asia.

4.1 Introduction

Floods are one of the most devastating of all natural disasters experienced by society. For example, in 2011 there were over 6,000 people killed, 140 million affected, and $70 billion in estimated damages caused by floods globally (EM-DAT, 2012). South Asia is particularly susceptible to flooding (Mirza, 2010), and Bangladesh, a deltaic country situated at the outflow of the Ganges-Brahmaputra-Meghna (GBM) basin, is among the most affected countries (Ali, 1999). Most of Bangladesh lies at or slightly above sea-level and is at the outlet of a river system which drains an area ten times as large as its own surface area (see figure 4.1). Due to the projected changes from climate change, Bangladesh will likely be further impacted (Mirza, 2002). In addition to these projected changes, rainfall and flooding in Bangladesh are subject to large interannual variability, influenced by ENSO (Chowdhury, 2003) and remote hydro-meteorological conditions in the larger GBM basin (Chowdhury and Ward, 2004; Shaman et al., 2005) which currently often lead to devastating disasters.

Large floods draw international attention and support which to a great degree can decrease the immediate impacts of the disaster. Del Ninno et al. (2001) details the losses in crops, arable land, household assets, and lives as a result of the 1998 floods which inundated 80% of the country. Del Ninno states that the worst immediate impacts were averted due to policy interventions on a large scale that saw food and other necessities distributed to affected areas. However, the long-term effects of an environmental shock of this size have rarely been studied. Hsiang and Jina (2014) find evidence that, for tropical cyclones, national incomes decline and do not recover after nearly two decades, which suggests that response to immediate impacts of a disaster does not preclude potentially large longer-term negative impacts. In addition to large events, Bangladesh experiences many smaller floods every year which receive little or no attention from the government or international community, but can lead to substantial losses for affected households. From self-reported damage assessments Gray and Mueller (2012) report that, on average, floods can cause damage to rural households that are as much as 20% of annual income, but few received assistance. This is a common trait among smaller disasters in developing countries (Besley and Burgess, 2002). These “small disasters”, with more common levels of exposure, are suspected to be responsible for large costs once the full impacts are quantified (UNISDR, 2013) and could be responsible for longer-term impacts, which may affect the development possibilities in Bangladesh. This leads to two key questions:

1. Are there prolonged negative outcomes resulting from flooding or similar disasters and environ-

mental conditions? This dynamic has been observed for rainfall (Maccini and Yang, 2009)

2. What indirect impacts (e.g., market access interruption or health effects) may increase the total costs of floods beyond the direct damages? Previous research suggests that the indirect effects may be substantial in comparison to directly measured damages (Anttila-Hughes and Hsiang, 2012).

An important contribution of this chapter is to examine the impacts from average levels of exposure rather than purely looking at low-probability, high-impact events. In this way, we can think of the detected impacts as the "environmental burden" that needs to be accommodated into the development process. We specifically examine the medium- and long-term impacts on human capital associated with exposure to flooding in early life (Almond and Currie, 2011). Early life shocks have been exploited in many different studies, and yet few of them have been able to demonstrate the extent to which subsequent investments may mitigate the early impacts. By looking at higher order births, we find suggestive evidence that household investments can undo the worst impacts of early life exposure, though they do not entirely erase them.

We exploit the stochastic temporal variation in exposure to flooding to identify the effect upon physical development of infants. As no comprehensive, national-scale database of flooding exists for Bangladesh, we create an exogenous, physically-derived measure of flood extents using remote sensing. This ensures that our measure is not subject to reporting bias. Following the development of this remote sensing product, effect sizes are estimated by matching to data on infant health.

We find that exposure to abnormal floods at the time of birth or while *in utero* leads to an increase in stunting on the order of 2% and an overall decline in standard measures of height-for-age among children under 5 years of age. Flooding exposure appears to affect boys more than girls, and larger floods have larger effects upon physical development. We also find evidence of adaptation across locations, and see that households that are routinely exposed to larger or more frequent floods experience smaller impacts in the event of an abnormal flood than those who are exposed less often.

These results on physical development are important because lower height-for-age ratios and stunting are associated with high infant mortality and illness (Black et al., 2008) and more broadly with negative educational and labor market outcomes in later life (Black et al., 2007; Glewwe and Miguel, 2007). This could represent a significant extra cost associated with this extreme environment than is traditionally accounted for in Bangladesh and other flood-prone developing countries. These measures are also important indicators of child health in themselves and are often used as a metric of the success of health interventions (for example, Gertler (2004)).

The rest of the chapter proceeds as follows: in section 4.2 there is a background discussion of flooding and flood impacts, as well as a summary of some of the relevant findings dealing with early life exposure to environmental shocks. Section 4.3 discusses the data used in this analysis. Section 4.4 details the remote sensing and econometric methods used herein. Section 4.5 presents results from this early analysis. The final section draws some conclusions for development and climate policy.

4.2 Background

4.2.1 Hydrology of Bangladesh

Bangladesh is a deltaic country lying at the confluence of three major world rivers - the Ganges, the Brahmaputra, and the Meghna. This river basin is known as the Ganges-Brahmaputra-Meghna (GBM) basin. Bangladesh has a monsoonal climate, with the South Asian Monsoon providing much of the direct rainfall to Bangladesh and the GBM basin between the months of May and September. The entire area of the GBM basin is 10 times larger than the land area of Bangladesh (see fig. 4.1). With roughly 46% of the country lying below 10 metres elevation[2], it is extremely flood-prone, with major floods having historically inundated up to 70% of total land area (Del Ninno et al., 2001). There are a number of distinct types of floods, each of which is caused by a different set of geophysical phenomena, and each of which may lead to different outcomes. Mirza et al. (2003) identify the four main types of floods affecting Bangladesh, which can be defined as follows:

1. **Flash Floods** have the shortest lead-time of prediction and often lead to the highest loss of life. They typically occur in mountainous areas or areas bordered by mountains. Land degradation and deforestation all contribute to the formation of flash floods. They often occur with little or no notice, and the high velocities involved lead to much damage and loss of life.

2. **Riverine Floods** are caused by rising water levels in a river due to precipitation that falls over the entire river basin. This hazard is slow onset and typically lasts from weeks to months. These large floods contribute most to the economic costs of flooding globally, either directly through damages to property or indirectly through interruptions to supply chains and averted economic activity.

[2] Bangladesh low elevation coastal zones estimated by Center for International Earth Science Information Network (CIESIN).

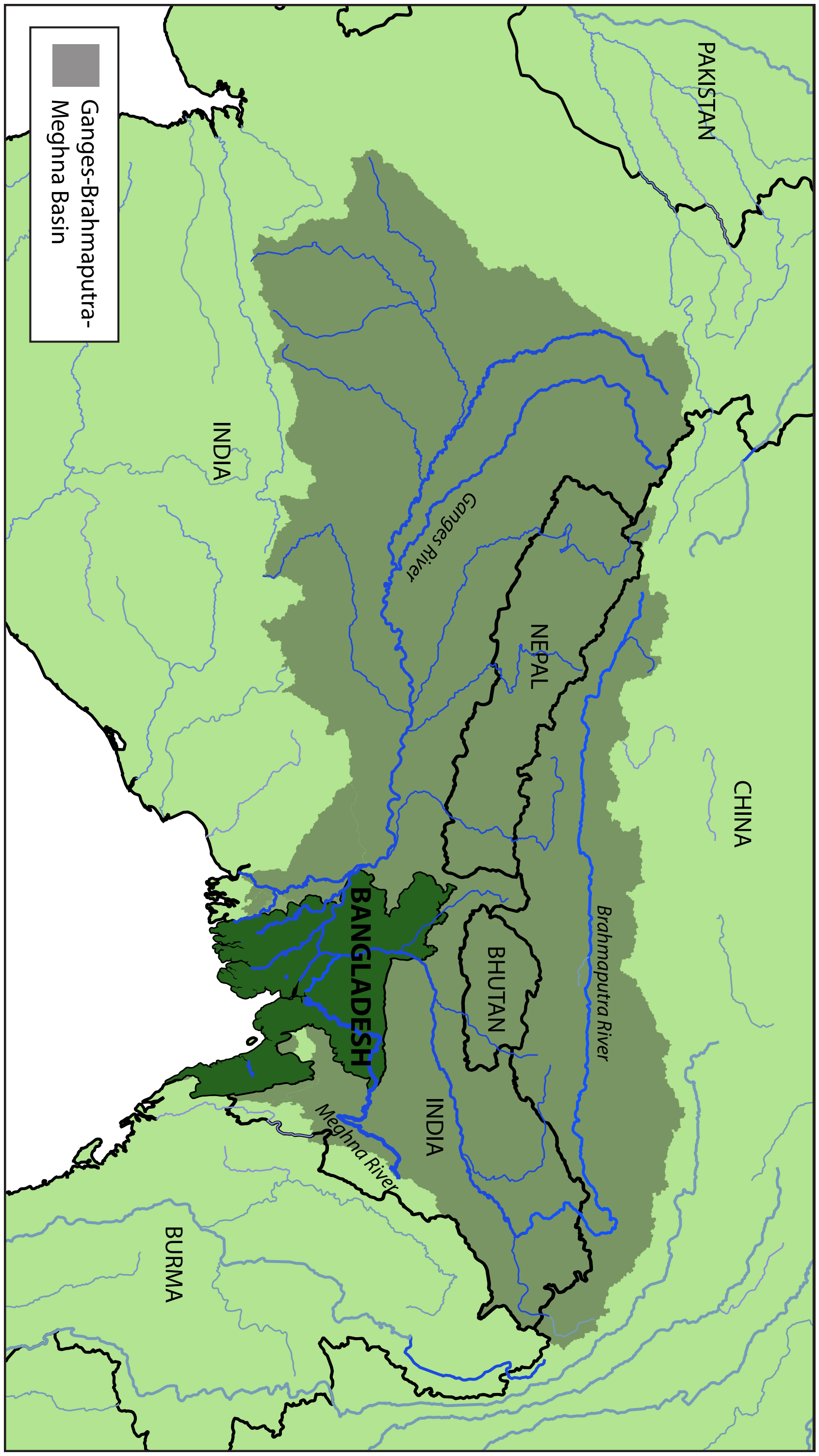


Figure 4.1: Map of the Indo-gangetic plain showing the location and size of Bangladesh relative to the Ganges-Brahmaputra-Meghna basin. Bangladesh drains water from an area about 10 times greater than itself.

3. **Monsoonal/Rain Floods** are floods caused by direct precipitation overhead. While this type of flood can cause much damage, it is typically more predictable than flash flooding.

4. **Storm Surge** is caused by tropical cyclones or depressions and affect coastal areas only. A storm surge occurs when a combination of low pressure and high winds lead to higher levels of sea water arriving at a coast. These floods can have major consequences due to the suddenness of onset and the associated cyclone hazard, but high waters typically do not remain for more than a few days.

The time resolution of the data that we use in the present study is periods of 16-days, with each year containing twenty three of these periods. This suggests that the measure of flood extent that we derive in this analysis is possibly too coarse to capture all but the largest of flash floods, so typically we will be discussing the impacts of riverine flooding. As noted, this is longer in duration than other types of flooding, and the timing of onset can lead to widespread damages to crops as well as loss of income if crops cannot be replanted.

It is important to note that flooding is a natural part of the ecology in many places. Flood plains are typically areas with high agricultural productivity due to the high nutrient loads and replenishment of soil moisture resulting from inundation. Thus, we must draw the distinction between "normal" and "abnormal" floods. Paul (1984) details coping mechanisms for floods in Bangladesh, including liquidation of assets and informal risk-sharing mechanisms, that confer a level of adaptive capacity to households to deal with flooding. Paul (1984) defines an abnormal flood as one that is larger in extent than the climatological average flood or uncharacteristically early or late in a season. In the case of a shock that affects a large part of the risk-sharing network, or one severe enough to damage productive assets, the floods are seen to exceed the capacity of a household and negative effects will be felt. In these cases, a sizable proportion of household income could be lost: self-reported damage assessments indicate that, on average, floods can cause damage to rural households of around 20% of annual income (Gray and Mueller, 2012) .

4.2.2 Natural disaster shocks

Floods, like all natural disasters, can have both direct and indirect effects. Additionally, effects can be short- or long-term. Research on natural disasters has typically been characterized by three different characteristics, with few if any looking comprehensively within all three:

1. **Direct versus indirect impacts.**

2. **Long-term versus short-term impacts.**

3. **Self-reported versus physical measures of exposure.**

Much of the disaster literature focuses on the short-term, direct effects, though recent work has shown that indirect effects may eclipse the direct ones in scope and cost. For example, Deryugina (2011), looking at the United States, finds that payments for unemployment insurance in counties affected by hurricanes increases and remains elevated for a decade after a hurricane, leading to costs greater than the hurricane damages. In a developing country setting, Anttila-Hughes and Hsiang (2012) find that the loss of life indirectly associated with typhoons is an order of magnitude higher than immediate death tolls. Many other studies dealing with natural disasters, however, have suffered from potentially large bias due to endogeneity of their measure of natural hazards - typically self-reported measures. There are few examples of researchers using exogenous, physically-derived measures of natural hazards in order to identify socioeconomic effects. Notable among them are Hsiang (2010), which uses a reconstruction of tropical cyclones to examine climate impacts in the Caribbean, Strobl (2012), which follows the method of Hsiang (2010) to examine the direct hurricane impacts in the Caribbean, and chapter 2 of this dissertation.

Though natural disasters command a great deal of public and academic attention, the long-term consequences of exposure to natural hazards has been relatively understudied. The results of chapter 2 of this dissertation are a notable exception, finding that there are multi-decadal negative effects upon economic growth due to tropical cyclones. Though dealing with a different hazard, it is hypothesized that other disasters can have similar consequences on long-term economic outcomes. The current analysis will try to understand if an environmental shock in early life due to flooding can lead to the type of longer term economic declines seen in Hsiang and Jina (2014).

There are a number of ways that effects of floods might persist in a population. For example, the shock to household income due to a flood can threaten food security through loss of earnings. Banerjee (2007) finds that, in flood affected districts of Bangladesh, while wages do seem to rebound in the long-term, in the aftermath of a flood there is a notable decline. Deryugina (2011) finds that short-run output in the construction industry may rise after disaster because demand rises, and this may lead to migration into disaster affected regions which provides some measure of compensation for lost revenue streams. Similarly, Strömberg (2007), Yang (2008) and Deryugina (2011) observe that natural disasters tend to cause transfers of wealth into the affected region. This is because the marginal product of capital will rise when capital and labor become relatively scarce after disaster, causing individuals and wealth to migrate into devastated locations (Miguel and Roland, 2010; World Bank, 2010). Many of these results, however, were derived in developed countries (notably the United States) or countries affected by one-time shocks like wars, and it is not immediately clear that the inflows of wealth will occur in poorer agricultural lands that are persistently affected by an environmental hazard, like flooding.

Additionally, the health consequences, like immediate loss of life (Jonkman, 2005) as well as disease (due to contaminated water, for example) and injury (Ahern et al., 2005; Du et al., 2010), can place an extra burden on household incomes. In the case of large floods, government and international attention can ameliorate the worst effects of this shock to incomes. Del Ninno et al. (2003) find that, after the 1998 floods in Bangladesh, a government food distribution program reduced the worst effects of the disaster and vulnerable groups were able to maintain an adequate calorie intake, thus avoiding many negative health effects. The authors note that this is not always the case for smaller disasters.

It is unclear, then, if these results and successful interventions, like those after the 1998 floods noted in the previous paragraph[3], can be generalized to the case of all flooding in Bangladesh, where floods often damage many of the poorest regions and migration or transfers into those regions may be low or non-existent. Many of the long-term impacts on health, education, and labor could be as a result of disinvestment in capital after a loss of income. It has been argued that the loss of physical capital encourages households to invest relatively more heavily in human capital since it is more durable, which would improve prospects for long-run growth (Skidmore and Toya, 2002), however recent evidence suggests that disinvestment in durable physical or human capital following a disaster may be an irreversible consequence that will lead to persistent effects throughout the economy (Anttila-Hughes and Hsiang, 2012; Banerjee and Watts, 2010; Duflo, 2000; Jacoby and Skoufias, 1997; Maccini and Yang, 2009; Udry, 1994).

A potential source of uncertainty in this paper is the extent to which population movements might compensate for the worst effects of natural disasters. In the analysis presented here, we exclude observations in which a household is recorded as having moved. Large population movements in Bangladesh due to natural hazards would result in the worst affected households being absent from the sample, which will bias our results downward. Gray and Mueller (2012) discuss the extent to

[3]A brief summary of historical floods in Bangladesh can be found in Mirza (2002).

which climate-related hazards lead to internal migration in Bangladesh and find that flooding itself has a small impact on mobility, though larger for women and the poor. Penning-Rowsell et al. (2013) find that there is little permanent movement away from hazard prone areas despite the dangers. Both of these findings provide some support for not considering migration as a major source of bias in flood-related impacts.

4.2.3 Methods for detecting floods

Flood detection using remote sensing has a long history (Sanyal and Lu, 2004; Tralli et al., 2005). Multiple instruments have been used to try to capture the extents of floods[4]. Flood mapping has typically been done for the purposes of disaster relief or for infrastructure planning. Creating a credible flood risk map is essential to insurance, zoning, and construction in many places around the world. For the purpose of disaster relief, flood mapping needs to be extremely accurate, so that areas of need can be identified. This often requires a combination of remote sensing, GIS, and hydrological modeling that can be data- and computationally-intensive. Hazard mapping tends to not require perfect delineation of floods, but just express a level of risk of flooding in a given area. For the current purpose, both approaches are inappropriate. We follow Sakamoto et al. (2009) and develop a measure of flood extent using solely remote sensing. This allows for the creation of a dataset which can be matched to specific periods, exploiting the stochastic timing and extent of floods to identify their effects.

4.2.4 Climate change and flooding in South Asia

The main driver of floods in South Asia is the South Asian Monsoon. This is an extremely complex regional realization of a global climatological feature that affects most of the tropics. It varies on intra-seasonal, inter-annual, and decadal timescales (Wang, 2006). In addition, there is a long-term change to the climate in the region due to anthropogenic climate change (IPCC, 2007), though it remains unclear what the exact direction of the precipitation changes will be. The existing vulnerability and the future changes have become a major policy concern in the region (Field et al., 2012; World Bank, 2010). Mirza (2010) notes that most of the changes to flood extent and depth will occur in Bangladesh between 0 and 2^oC warming, causing increasing damage to crops and affecting public health, particularly of poorer women and children. With most climate scientists agreeing that 2^oC warming is inevitable, the consequences for Bangladesh will be severe. Flooding in Bangladesh is governed in a large part

[4] among them MODIS, AVHRR, and Landsat

by events outside its boundaries in the larger GBM basin (Chowdhury and Ward, 2004; Shaman et al., 2005) and is also subject to influence by ENSO (Chowdhury, 2003). Recent projections have suggested that a warming world would contain more positive ENSO-like events, and this change will further exacerbate flooding in the region.

4.2.5 Early-life exposure to environmental shocks

This paper also engages with the flourishing literature in economics on early life impacts of shock and their long-term consequences, often known as the Barker Hypothesis. Almond and Currie (2011) provide a good summary of the literature in this field. The current paper is distinct for several reasons, but perhaps most notable is the focus on frequent, "mild" environmental exposure rather than a low-frequency, catastrophic shock. Research on early-life exposures has often focused on large, distinct, and unique shocks; for example, a rapid decline in air pollution due to a recession and its effect upon infant mortality (Chay, 2003), exposure to radioactive fallout from Chernobyl (Almond et al., 2007), or a catastrophic famine (Chen and Zhou, 2007). There is a concern that some of the effects observed in these papers with unique or distinctive shocks may not be possible to generalize to more common environmental exposures. As flooding is a relatively common and natural part of rural life in Bangladesh, we seek to find the negative effects from exposure to this mundane environmental phenomenon, as it will be more likely to be generalizable across the region and into the future.

4.3 Data

Flood Data Flood data are calculated from satellite observations of surface reflectance taken from the Moderate-Resolution Imaging Spectroradiometer (MODIS) instruments operated by NASA. MODIS is an array of two satellites that scan the Earth's surface every two days, recording reflectance values over 36 bands in the visible and infra-red spectra at various spatial resolutions. As clouds are opaque to visible and infra-red light, cloud cover will restrict the use of images for detecting surface properties. Due to this, data are processed into cloud-free composites of 8 or 16 days, of which we use the latter. Composite data are available for the period between 2000-2013 at 250m×250m resolution. This results in a total of 3,159×2,482 pixels for each of 253 time periods for four separate reflectance bands (resulting in approximately 8×10^9 observations). In addition, a preprocessed index for vegetation is also obtained (Huete et al., 1997). Details of the construction of our flood index is given in section 4.4.

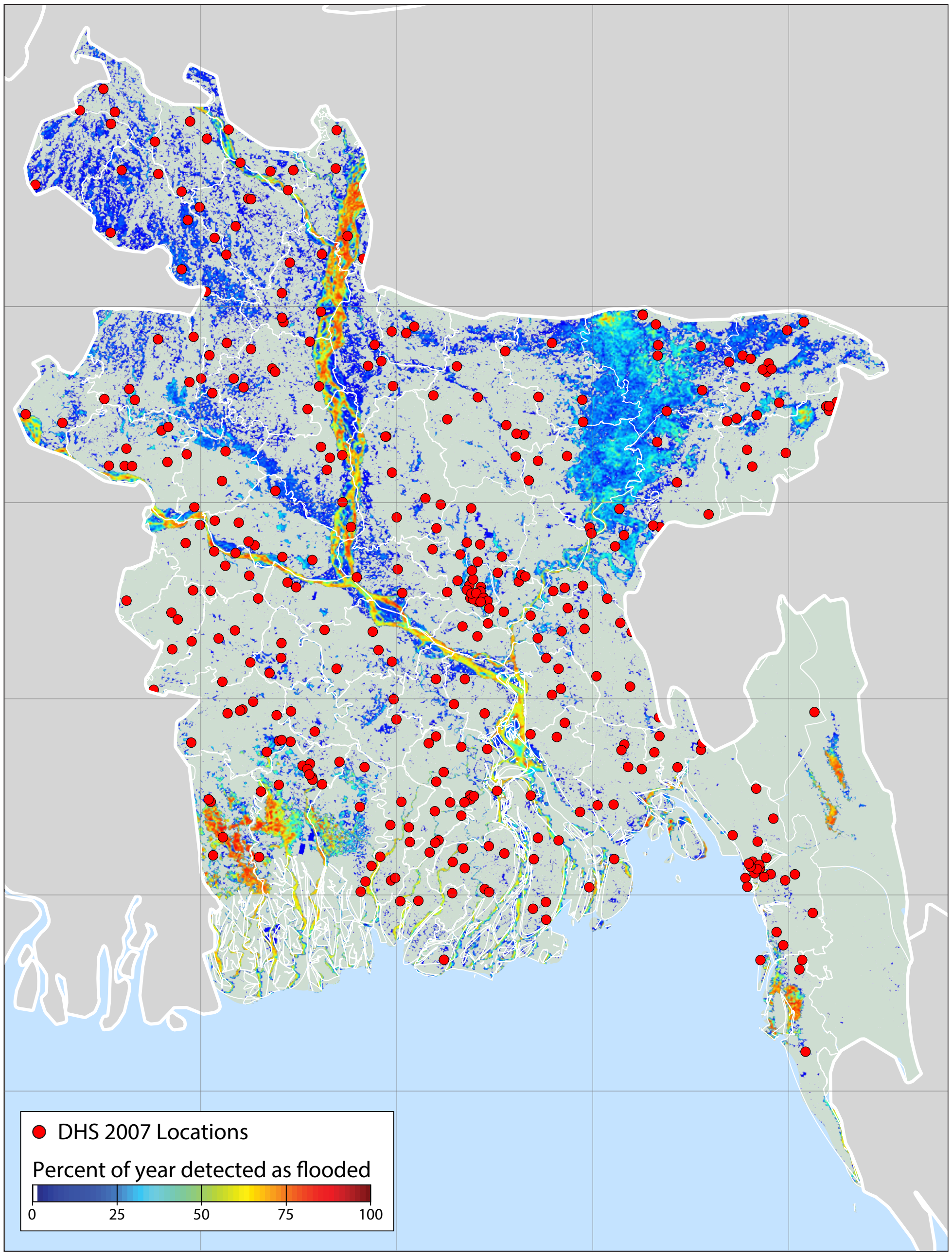


Figure 4.2: Map of Bangladesh showing the flooded regions in 2007 with the locations of DHS 2007 survey clusters overlaid. Pixels in which there were floods detected for at least one of the twenty-three observations periods of the satellite through the year are shown as colored pixels. Orange or red pixels will usually signify permanent water (i.e., rivers or lakes).

Other climate data Recent evidence suggests that precipitation influences health and economic outcomes in early life (Maccini and Yang (2009)). Since Bangladesh drains an area many times greater than its own size (as in figure 4.1), it can be expected that flooding will be driven by rainfall in the wider basin, and that it is possible that precipitation may be correlated with flooded extents. For this reason, we include precipitation as a control in some models. Daily precipitation is obtained from the Tropical Rainfall Measuring Mission (TRMM) for 1998-2013. This is a $0.25^o \times 0.25^o$ gridded rainfall product measured by a network of satellites. As this resolution translates to roughly 25×25 km resolution at the latitude of Bangladesh, it is considerably larger than the resolution on our flood measure.

Due to potential error introduced by boundary effects of using single pixels, we take spatially-weighted averages of precipitation over each district of the Bangladesh and match these precipitation measures to clusters based upon a cluster's presence within that district. Bangladesh has 64 districts with an average surface area of 2305 km^2. For robustness, we use individual TRMM pixels matched to DHS clusters and an average of the overlying pixel and the 8 adjacent pixels. The daily TRMM data are then temporally aggregated to match the 16-day periods at which the MODIS data are available and standardized for each district. Following Maccini and Yang (2009), we also look at rainfall totals throughout the agricultural season prior to birth.

Table 4.1: Summary statistics

Variable	**Mean**	**(Std. Dev.)**	**Min.**	**Max.**	**N**
Birth trimester flood proportion	0.04	(0.06)	0	0.49	15286
Birth trimester rainfall (Z)	0.02	(0.38)	-0.91	1.39	14401
Gender	0.51	(0.5)	0	1	18586
Age (days)	891.27	(524.24)	1	1823	18586
Stunting	0.44	(0.5)	0	1	18541
Urban	0.32	(0.47)	0	1	18586
HH education (years completed by HH head)	4.89	(4.05)	0	17	18567

Socioeconomic Data For the current analysis, the 2004, 2007, and 2011 rounds of the Bangladesh Demographic and Health Surveys (DHS, 2005, 2009, 2013) are used. The DHS is a nationally representative sample survey which contains information on health and socioeconomic indicators. Standardized World Health Organization (WHO) measures of physical development of children under the age of 5 years old are calculated from weight, height, and age variables (de Onis, 2006). The resulting anthropometric measures of *height -for-age*, *weight-for-age*, and *weight-for-height* are unitless Z-scores that are derived from comparison to internationally measured cohorts of children[5]. The DHS also contains limited information on household socioeconomic characteristics that we use as controls in the analysis. In particular these are highest education level of household and an asset index of wealth following Filmer and Pritchett (2001). The locations of the DHS survey clusters from 2007 are shown along with flooded extents from the same year in figure 4.2.

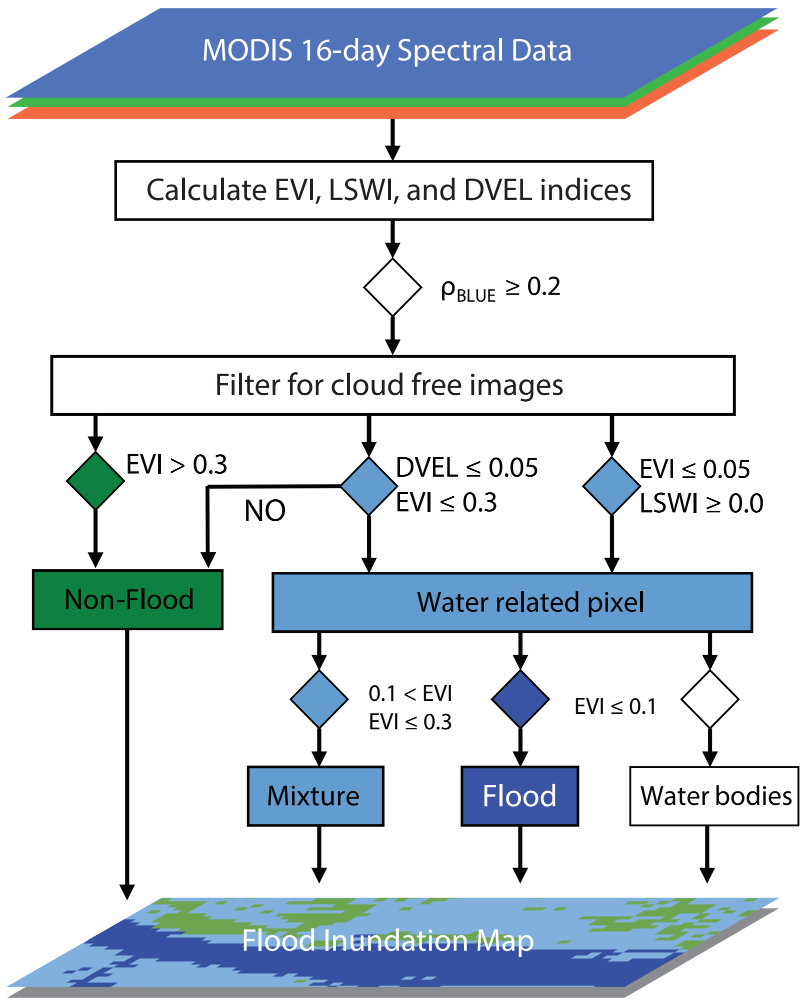


Figure 4.3: Classification algorithm for detecting floods using MODIS data, adapted from Sakamoto et al. (2009).

[5]The anthropometric measures are calculated using the WHO STATA code available here: http://www.who.int/childgrowth/software/en/

4.4 Methods

4.4.1 Constructing an exogenous measure of flood extent

Remote sensing of flood extents has received much attention from the remote sensing and natural hazards communities, though many of the methods have been expensive and data-intensive, often requiring extensive hydrological modeling. However, the intention of many of these efforts is to perfectly delineate flood-zones in near-realtime for disaster relief efforts or to categorize areas of higher long-term flood risk for disaster or infrastructure planning. For the latter purposes, the specific timing of events are not important. The former methodology creates a snapshot of great spatial detail, while the latter looks over long time horizons to produce the average flood-risk of an area. Exact timing of floods is not a main priority of either method.

For the current statistical purpose we require a measure of flooding that is comprehensive both throughout time and across a large spatial area. This allows us to assign historical flood conditions to individuals based on location at certain critical periods (i.e., while they are infants). Additionally, in contrast to the two approaches discussed above, we wish to look historically at the precise timing of floods for a large number of individuals, so we can accept some error in our detected flood zones. We follow the method of Xiao et al. (2006), taking further developments of that method as detailed in Sakamoto et al. (2009). This method was developed to identify paddy agriculture in the Mekong Delta, Vietnam. Paddy agriculture is conducted by inundating fields, which leaves a spectral signature of standing surface water mixed with vegetation. This also identifies flooded zones and performs well when compared to more data intensive methods of flood delineation in Bangladesh (Islam et al. 2010).

The intuition behind the method is to construct two measures, one of which is sensitive to surface water and the other to surface vegetation (or greenness). If the value of the index for water surpasses that for greenness then we can say that there is overlying surface water. In practice we relax this simple assumption and use a buffer threshold value (given by the difference between surface water and greenness) to ensure accuracy.

We begin by calculating the Land Surface Water Index (LSWI) for all MODIS data over Bangladesh

$$LSWI = \frac{\rho_{NIR} - \rho_{MIR}}{\rho_{NIR} + \rho_{MIR}} \tag{4.4.1}$$

where ρ refers to the intensity of light at a particular wavelength indicated by the subscript. Here, NIR is near infra-red, MIR is middle infra-red[6]. Equation 4.4.1 gives an approximate measure of "blueness" at the surface and is analogous to the Normalized Difference Vegetation Index[7]. However, it cannot accurately determine surface water when used in isolation, and so we exploit the difference between LSWI and a vegetation index, in this case the Enhanced Vegetation Index (EVI):

$$EVI = \frac{\rho_{NIR} - \rho_{RED}}{\rho_{NIR} + 6 \times \rho_{RED} - 7.5 \times \rho_{BLUE} + 1} \tag{4.4.2}$$

where, as above, ρ refers to the intensity of light at a particular wavelength, with $BLUE$ and RED being light in the blue and red regions of the spectrum, respectively.

The difference between LSWI and EVI is referred to as DVEL. An algorithm is then applied that assigns one of three values to each pixel based upon the values of each of these three indices:

- **Non-flood**: Pixels which show no evidence of standing surface water
- **Mixed**: Pixels which show a mixture of standing water and vegetation
- **Flood**: Pixels which are unambiguously flooded over their whole extent

The temporal dimension of the data is then exploited to assign "permanent water" status to pixels which are flooded for more than 180 days in a year. The exact threshold values were modified and validated with extensive ground-truthing in Bangladesh during the monsoon season in 2012, though some concerns are noted below. This classification procedure is visualized in figure 4.10. Figure 4.5 displays the results of this classification for 2004, a notably bad flood year.

Flood extents are then calculated for each survey cluster in the DHS by calculating the percent of land that is classified as flooded in a circle of radius 5km around each cluster. This distance is chosen for several reasons. Firstly, to preserve anonymity the DHS geolocations are randomly jittered by 5 km, so we wish to capture the actual location within our measure. Secondly, the average extent of a flood that can be considered damaging would have to be greater than the pixel size available, so 10 kilometers is chosen as an appropriate magnitude. Figure 4.4 shows the flooded percentage of an example survey location. The high level of "mixed" pixels in the winter months in both figures 4.5

[6] The wavelengths observed in each of the spectral bands is as follows: $BLUE$: 459 - 479 nm; RED: 621 - 670 nm; NIR: 841 - 875 nm; MIR: 2105 - 2155 nm.

[7] $NDVI = \frac{\rho_{NIR} - \rho_{RED}}{\rho_{NIR} + \rho_{RED}}$

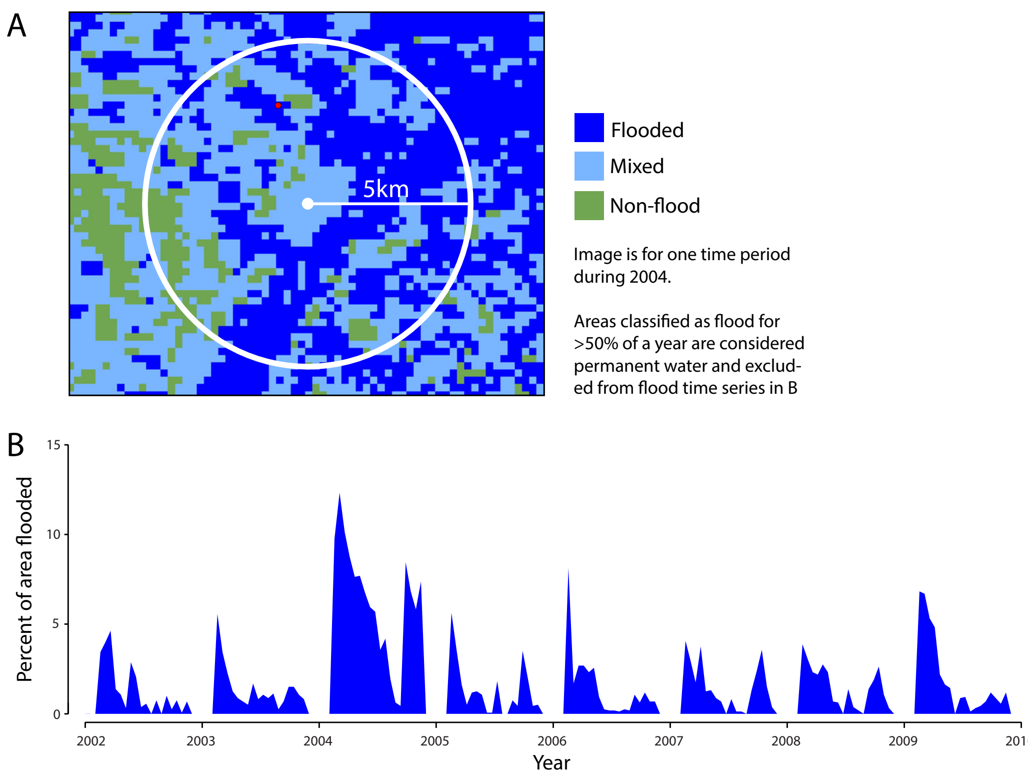


Figure 4.4: Flood histories for each DHS location over the three rounds (2004, 2007, 2011) are obtained by calculating the percentage of each land water class within a 10km diameter bounding area. Example for a single time period for a single DHS location. Single period landcover classification is amended in the time series to contain permanent water bodies as those that are classed as flooded for greater than 50% of a year.

and 4.4 is likely due to winter rice cultivation. We then standardize the flood magnitudes for every 10 kilometer area surrounding the survey clusters. This is achieved by taking the mean of each satellite observation period (of 16-days) and averaging over all years, after which Z-scores of flooding extent are calculated[8]. Standardized flood extents are matched to each observation in the DHS by cluster number and birth-date for the 9 month periods before and after birth, and aggregated to trimesters.

Some concerns with flood extent measure It should be noted that the flood measure as presented is subject to a number of limitations that will be addressed as the research proceeds. Chief among these concerns is the effect of cloud cover on the ability to detect floods. Inspecting figure 4.5, we see that for periods in the middle of the year, during the monsoon season, there are large areas of missing values due to clouds. It is highly likely that these areas are subject to significant flooding in the monsoon season, and so the absence of these pixels will lead to a downward bias on our estimates. We address this by employing a number of interpolation methods, including simple nearest neighbor completion of missing pixels, and more complex methods, for example spline smoothing interpolation as employed by Jain et al. (2013). This will allow for the calculation of the flood extent during periods with missing values due to cloud cover by interpolating between observed surface data points. Results presented below use exposure interpolated with nearest neighbor matches.

4.4.2 Econometric analysis of the impact of flooding

We model physical development of children under the age of five as a linear function of flood exposure at birth, including *in utero* and *post partum* exposure in some model extensions. We estimate the following model:

$$Y_{i,T} = \sum_{L=-k}^{k} [\beta_L \times FLOOD_{i,t-L}] + \gamma \times X_i + \delta_i + \mu_i + \nu_i + \epsilon_{i,t} \tag{4.4.3}$$

where $Y_{i,T}$ is the outcome of interest at time T, when the observation is taken; X_i is a vector of controls; and δ, μ, and ν are birth-order[9], birth-month, and location fixed effects, respectively. The coefficient of interest is β_L for time $t = 0$ and periods of observation both before and after birth. Equation 4.4.3 is estimated using ordinary least squares (OLS). Throughout, standard errors are clustered by DHS

[8]This is done to account for adaptation to flood hydrology. A community more adapted to larger floods will only see the abnormally large floods as threatening livelihoods, and similarly for regions that suffer less extensive floods.

[9]Birth-order is of noted importance for human capital investments (e.g. Black et al., 2005). This is particularly relevant for South Asia, which is home to the largest concentration of stunted children in the world, thought to largely be related to birth-order effects (Jayachandran and Pande, 2013).

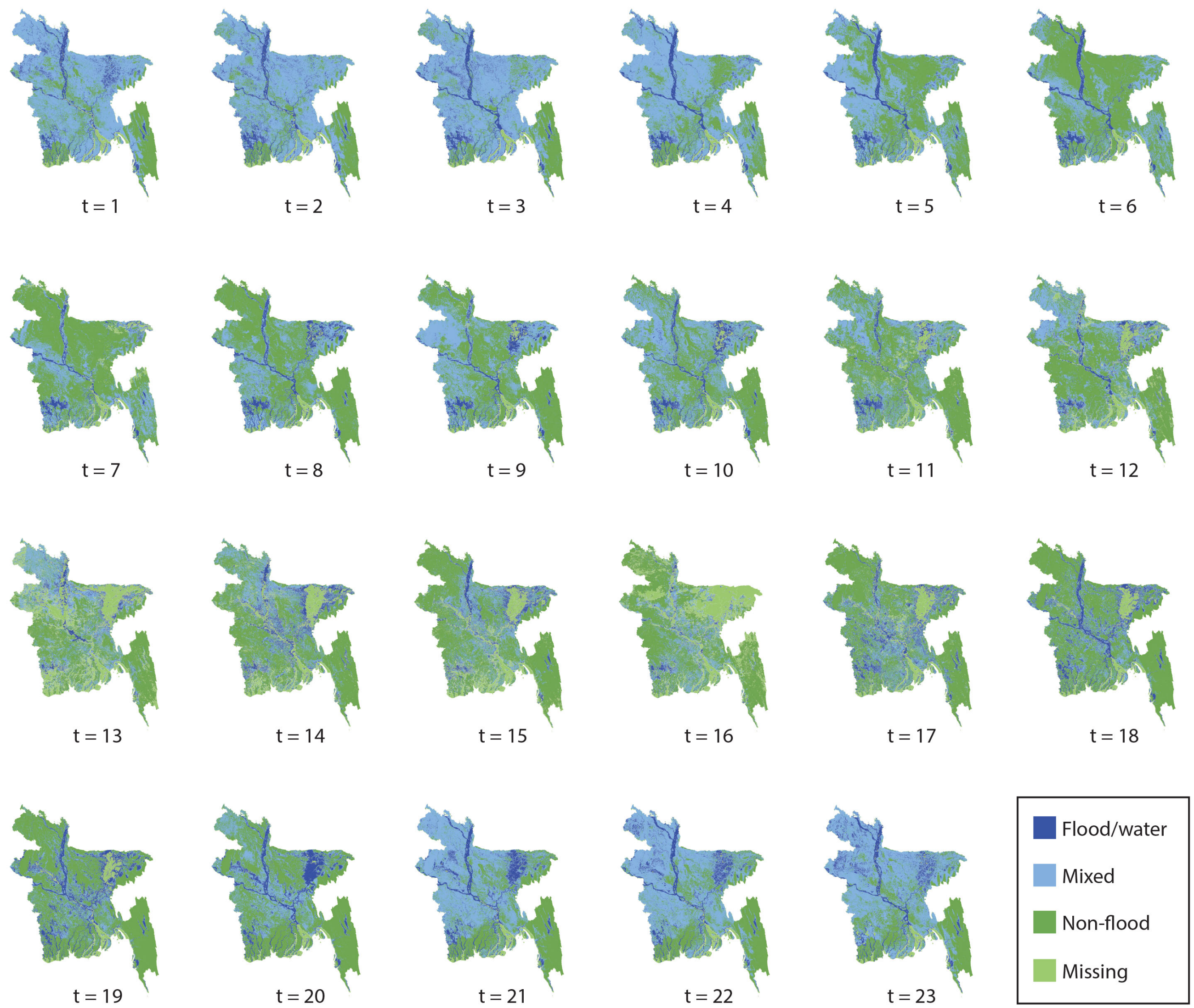


Figure 4.5: Progression of flooding through 2004. Each time period corresponds to a 16-day interval during the year, with t=1 beginning on January 1st and ending on January 16th and so on. As indicated in the text, missing pixels due to monsoonal clouds will downwardly bias results for the flooding season. The high proportion of mixed pixels during the winter months are likely due to winter (boro) rice cultivation.

survey cluster. Because we are exploiting the exact timing of floods for identification, and because this timing and extent is largely stochastic and cannot be predicted long in advance, we assume our measure of flooding, $FLOOD$, is exogenous.

4.5 Results

In this section, the main result is presented. Robustness checks are then performed to understand the timing of flood impacts on physical development, the effect of precipitation on outcomes, comparisons between the various anthropometric measures, and the differences between the two survey periods. We also look for evidence of adaptation to flooding, and examine whether there is non-linearity in the response to flood extent.

Main result

The main results are shown in tables 4.2 and 4.4. In table 4.2the dependent variable is stunting. Stunting is classified as having a height-for-age Z-score greater than two standard deviations below the mean. In table 4.4 results for height-for-age are presented. Table 4.2 present our main findings. The model with birth month and birth year fixed effects (column 3) is our preferred specification. We see that exposure to flooding in the second and third trimesters in utero increases the likelihood of stunting. A one standard deviation shock in the third trimester would increase the likelihood of stunting by about 2%. Columns (4) and (5) divide the sample by gender and we see that the effect sizes reported for boys is large and significant while the effect size for girls is not significantly different from zero. This is consistent with findings in the literature that male fetuses are weaker (for example, Almond and Currie, 2011), and supports the idea that the effect here is at least in part a physiological effect due to some harm in utero rather than an economic one resulting from conscious choices in the household.

Robustness

Precipitation controls The inclusion of precipitation controls is presented in table 4.3. Precipitation lag lengths match those of flood exposure and are included as averages over trimesters. We see that the inclusion of precipitation controls has little effect upon the estimated effects sizes. This is consistent with the idea discussed in the introduction, that flooding in Bangladesh is often less about

Table 4.2: Main results: Stunting

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Stunted				
Sample restrictions	All			Boys	Girls
Proportion of area flooded during trimester					
1^{st} trimester	0.458***	0.237	0.209	0.486**	-0.120
	(0.149)	(0.146)	(0.146)	(0.215)	(0.243)
2^{nd} trimester	0.502***	0.259*	0.267*	0.275	0.219
	(0.159)	(0.150)	(0.147)	(0.220)	(0.230)
3^{rd} trimester	0.604***	0.312**	0.302**	0.466**	0.145
	(0.148)	(0.146)	(0.145)	(0.206)	(0.244)
4^{th} trimester	0.505***	0.210	0.182	0.144	0.267
	(0.139)	(0.136)	(0.136)	(0.216)	(0.213)
Birth month FE	Y		Y	Y	Y
Birth year FE		Y	Y	Y	Y
Education & wealth controls			Y		
Observations	15112	15112	15093	7663	7449
Adjusted R^2	0.062	0.104	0.125	0.095	0.114

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

immediate rainfall overhead as it is about rainfall across the entire GBM basin. The coefficients on the rainfall variables is interesting, showing similarity to Maccini and Yang (2009). Above average rainfall in the months of birth lead to positive outcomes, as it potentially signifies a good agricultural season.

Agricultural season restriction We restrict the sample to only those children who are in utero during the flood season, beginning with the completion of a full trimester of flood season. If flood season begins in May, one full trimester of flood season exposure will begin in August. While we still see an effect of in utero exposure, it has shifted towards the second trimester and the third trimester coefficient has decreased. This suggests that it is flood earlier in the year (at abnormal times) that may have more of an impact, since an April flood for example, will only appear in this stratification in the second or first trimester variables.

Other anthropometric measures We choose height-for-age as a measure of physical development that can be affected by early life exposure, and in particular, long-term malnutrition. Other anthropometric measures may respond differently to exposure to flooding at birth. The relationship between stunting (measured with height-for-age) and wasting (measured with weight-for-age/height) has been a subject of long discussion in the nutritional literature (e.g. Bhutta et al., 2008; Victora, 1992. Victora (1992) notes that stunting can be considered as a longer term response to malnutrition whereas wasting is considered a short-term or "acute" response, while also noting that stunting may be caused by other limiting factors like micronutrient deficiency. In many parts of the world there is a surprising lack of correlation between indicators of stunting and indicators of wasting in children. As weight is more more variable and dependent upon recent nutritional status, we would expect that weight-for-age and weight-for-height Z-scores would not respond to early life exposure to environmental shocks. Columns (1)-(3) of table 4.4 tests this hypothesis and finds that neither weight-for-height (2) nor weight-for-age respond to flooding exposure in the third trimester significantly.

Variation by survey period Within the birth years in our sample, 2004 is noted as the worst flood year. This could lead to a difference in outcomes between surveys as each round has a different number of children born in high flood years. Figure 4.6 shows a kernel density estimation for the standardized distribution of flooding at birth for all children in the 2004, 2007 and 2011 rounds, and confirms the higher flood exposure of children measured in the earlier samples (with more mass at the higher end of the distribution). To see if this measurement period affects our results, we run the model for each

Table 4.3: Precipitation and agricultural season

	(1)	(2)	(3)
Dependent variable	Stunting		
Sample restrictions	All		Ag. season
Proportion of area flooded during trimester			
1st trimester	0.237	0.253	0.369*
	(0.146)	(0.160)	(0.214)
2nd trimester	0.259*	0.298*	0.441*
	(0.150)	(0.164)	(0.252)
3rd trimester	0.312**	0.306*	0.161
	(0.146)	(0.161)	(0.216)
4th trimester	0.210	0.208	0.169
	(0.136)	(0.142)	(0.180)
Rainfall average during trimester			
1st trimester		0.0291**	
		(0.0138)	
2nd trimester		-0.00373	
		(0.0154)	
3rd trimester		-0.0397***	
		(0.0138)	
4th trimester		0.0171	
		(0.0136)	
Observations	15112	13474	8695
Adjusted R^2	0.104	0.100	0.104

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.4: Other anthropometric measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	HAZ	WHZ	WAZ	Stunting			
Sample restrictions	All			2004	2007	2011	Birth order>1
Proportion of area flooded during trimester							
1st trimester	-0.586	-0.785**	-0.816**	0.828**	0.284	-0.101	0.275
	(0.398)	(0.363)	(0.326)	(0.365)	(0.226)	(0.222)	(0.175)
2nd trimester	-0.639	-0.335	-0.449	-0.0704	0.454**	0.0339	0.439**
	(0.411)	(0.366)	(0.326)	(0.415)	(0.229)	(0.220)	(0.181)
3rd trimester	-0.656*	-0.316	-0.341	0.184	0.339	0.155	0.379**
	(0.387)	(0.355)	(0.332)	(0.404)	(0.213)	(0.227)	(0.180)
4th trimester	-0.404	-0.265	-0.243	0.0257	0.251	0.0399	0.432**
	(0.397)	(0.329)	(0.323)	(0.396)	(0.174)	(0.228)	(0.168)
Observations	15112	15598	15246	2891	5054	7167	10024
Adjusted R^2	0.137	0.068	0.133	0.155	0.122	0.090	0.114

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

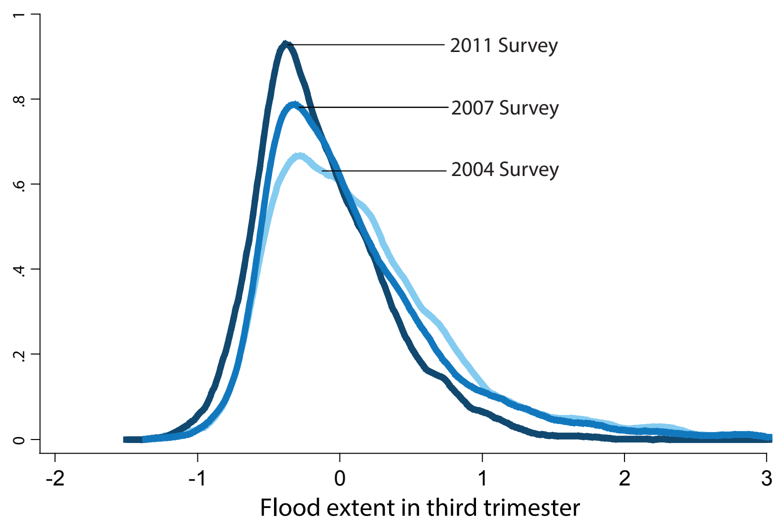


Figure 4.6: Kernel density estimations of trimester of standardized birth flood exposure for children from each survey round. 2004 and 2007 survey years show more children exposed to above normal flooding.

survey year separately. Results are displayed in columns (4) - (7) of table 4.4. While we do see some differences between years in many coefficients, the third trimester coefficient remains quite stable, with 2007 seeming to have a larger marginal effect on stunting.

Birth-order effects If there is an economic component to the impacts that we see upon human capital (i.e., households disinvest in infants in times when household incomes contract), we might also expect to see a different effect for children with older siblings. This is because, when viewed purely as an investment, an earlier child is more valuable (Black et al., 2005). For this reason, we would expect to see an increase in the effect of floods on younger siblings. Figure 4.7 plots the values of the birth order fixed effects - essentially the birth order group specific intercepts. Compared to the child born first, we see that higher order births have worse anthropometric measures compared to the first born child. This implies a comparative lack of investments in higher order children. To understand the role birth order plays, we restrict the sample to only higher order births (from the second child upwards) in column (7) of 4.4. We see that the marginal effect of flood exposure is magnified for all periods, with significant increases of stunting due to exposure in the second and third trimesters. The coefficient on the third trimester is about 25% larger than in the case with the full sample.

This could result from two, potentially simultaneous, explanations. First, in the event of a shock, a family may differentially spend on their infant children. A first-born child may suffer less of a loss

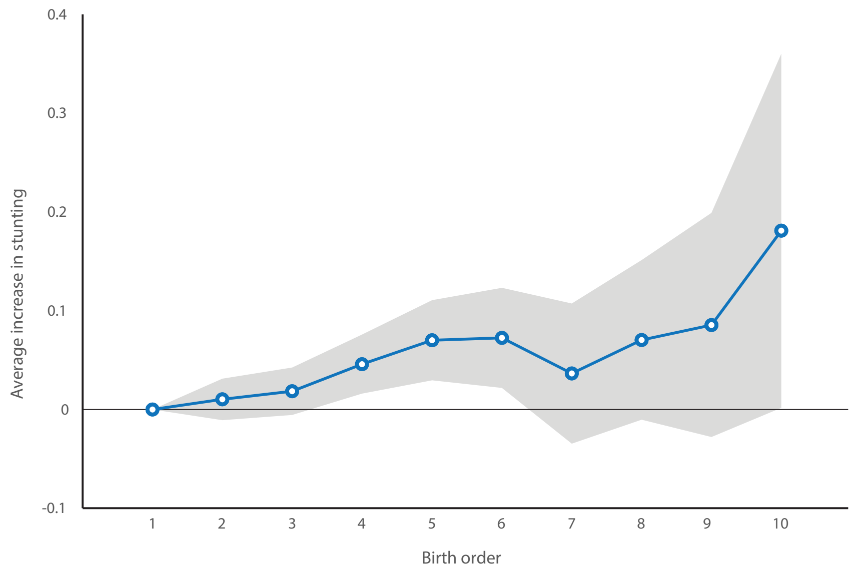


Figure 4.7: Values of the intercept for each of the birth order fixed effects (those above 10 are omitted).

of well-being in the case of a contraction of household income, or a mother pregnant with a first-born child may be better provided for. A second explanation is that, after the effects of early life exposure become apparent, a family may invest differently in a first-born versus a later born child. This would imply that, while the initial impact of the shock might be equal for all children, compensatory behavior *ex post* may partially decrease the impact. If there is a difference in this compensatory behavior across a number of children, this would account for these results. This finding agrees with previous findings on birth order and physical development in South Asia (Jayachandran and Pande, 2013), but the addition of the compensatory behavior with regards to environmental shocks changes the interpretation to one of both development and environmental factors. Results for higher birth order for the other WHO anthropometrics are shown in appendix table 4.5.

Spatial lags A concern for the analysis is that negative effects of flooding could be due to selection, as wealthier or more healthy households move small distances away from more flood prone areas. this would cause the more marginal households to remain in flood prone areas, and negative effects would result, while areas at greater distances from floods would experience positive effects. We test this explicitly with a spatial lag model. We estimate a model like our main specification, but specifically including flood exposure from annuli that incorporate the area around each DHS cluster in 5km increments. Fig. 4.8 present both a graphical depiction of the distances used in the analysis and the results from the spatial lag model itself. We see that for areas within the 5km buffer around the community, effects on stunting are larger than at greater distances, though none of the coefficients is significant. This may indicate that spatial displacement is not a concern to identification, but it cannot be rigorously excluded as a possibility.

Evidence of adaptation Households that are more frequently exposed to larger floods may display greater adaptation to flooding. This can be achieved either through investments in protective measures, or disinvestments in susceptible capital. To examine whether this is the case, the sample is divided into quintiles of average exposure to flooding over the whole period (2000-2013). The model is run with interaction terms between flood quintiles and trimesters of flood exposure. The results are displayed in figure 4.9. For every trimester, households that are less frequently exposed experience a larger marginal response to flooding. This declines as exposure increases for each of the in utero periods. For households in the middle quintile, effects look to be close to zero, but importantly they remain statistically indistinguishable from our main effect (shown in gray). Impacts increase slightly as we look at more exposed regions. This is consistent with costly adaptation.

4.6 Discussion

This paper presents results of the effects of flooding on physical development outcomes in Bangladesh. The main innovation is the creation and use of an exogenous measure of flood exposure derived from remote sensing and its combination with pre-collected survey data. This allows us to identify an effect of early life exposure to flooding. We see negative effects on physical development of infants, with the effect being larger in boys than girls, and also find evidence of adaptation to this natural hazard.

However, the exact mechanism of these impacts is unclear. As previously noted, flooding is a complex hazard which has both positive and negative effects. For example, a flood can cause some damage to crops in a given year, but lead to improvements in soil moisture and fertility in subsequent seasons, leaving the net effect unclear. This is why the timing of both floods and of births that we exploit to identify our effects are so critical—a flood early in the season may destroy crops, while later in the season it may only reduce the yield slightly. Given that we see significant impacts while a child is *in utero*, it can be surmised that the effect is a physiological one channeled through the mother. This may be due to nutrition, maternal stress, maternal effort levels, or some other factor. However,

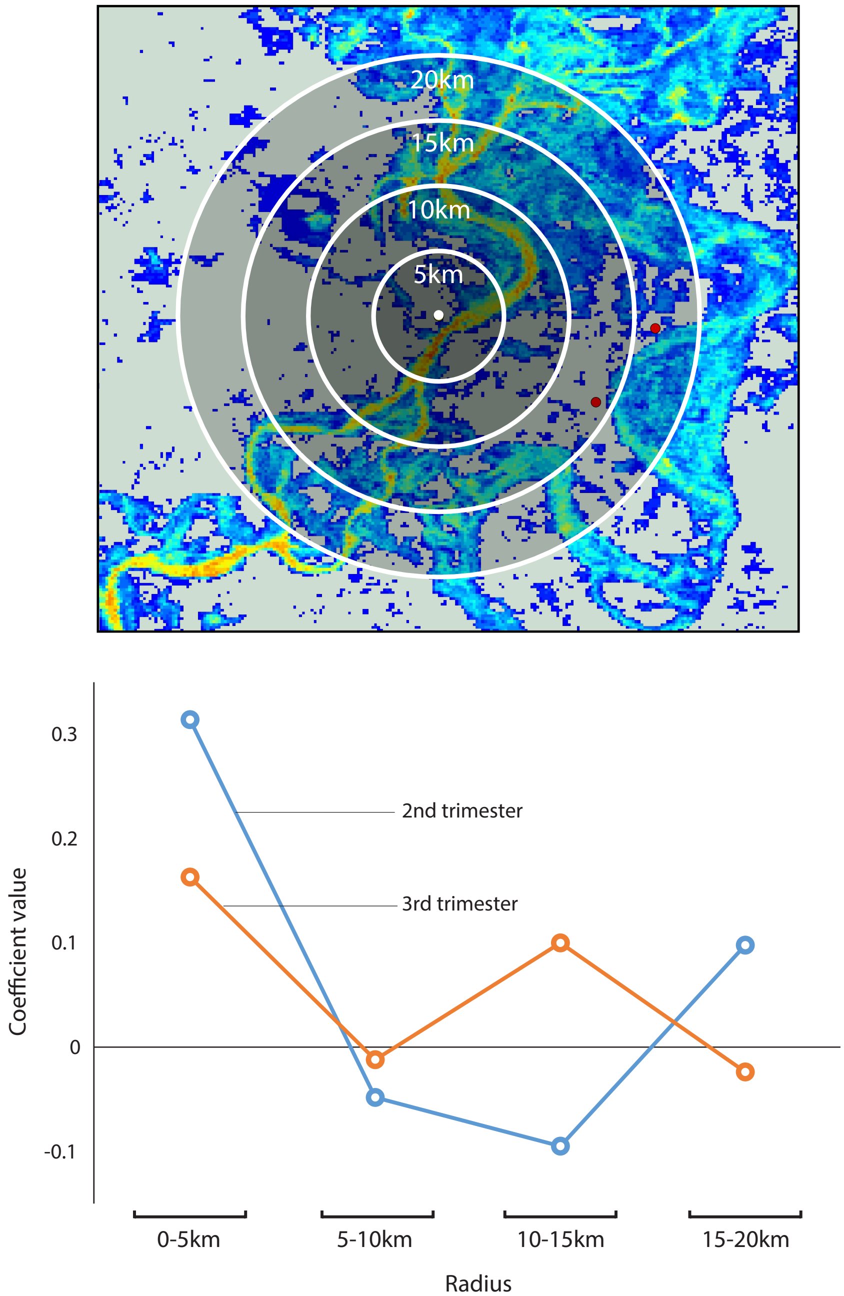


Figure 4.8: Upper panel shows radii used for flood extent calculation, centered on a riverside community. Inner radius of 5 kilometers is used in the main specification. The annuli from 5-10, 10-15, and 15-20 kilometers, are used in the spatial lag model. Lower panel shows results for second and third trimester exposure on stunting in each buffer.

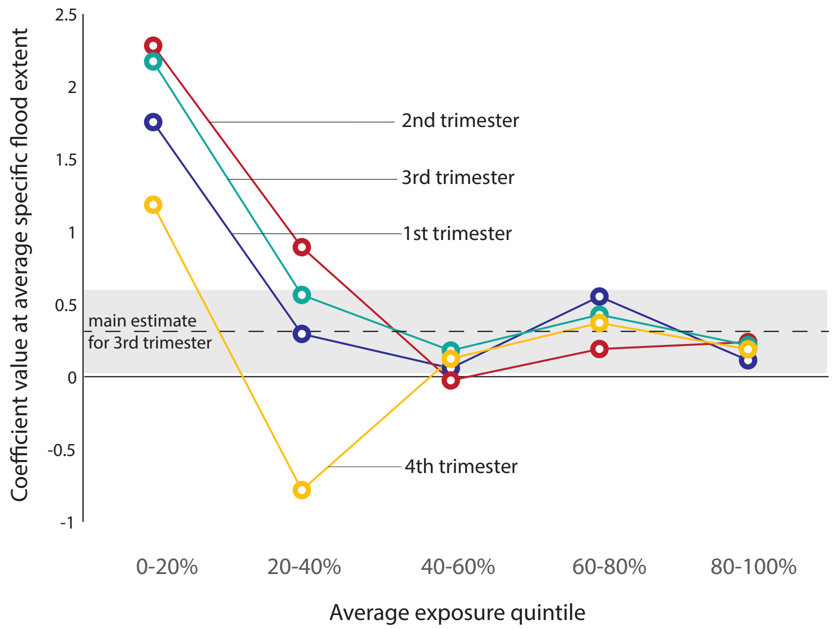


Figure 4.9: Coefficients of trimester exposure to flooding interacted with average flood extent within location, measured over 2000-2012. Each line represents the impact of flooding exposure upon stunting in a single trimester for five different levels of average flood extent. The marginal effect on floods is larger in locations that are less exposed, implying that households adapt to frequent exposure.

the results on birth order would suggest that some behavioral response is at play, though the exact details are unclear from the data.

It is also unclear the extent to which infant mortality may be biasing these results. If there is significant "harvesting" of fetuses or young children due to exposure to flooding, then we might be missing some of the worst effects in our analysis. This would cause downward bias of our effect sizes as only the stronger children would survive. Despite this, we still see an effect of "scarring" in our results, and so this may be a lower bound on the effects of flooding exposure.

We find these results on physical development to be interesting because lower height-for-age ratios and stunting are associated with negative educational and labor market incomes in later life. This could represent a significant extra cost associated with environmental impacts in Bangladesh and other flood-prone developing countries. We explicitly aim to understand the effects of more mild environmental exposure rather than large shocks, as the latter might lead to a lack of generality of the results. This paper, and further work related to these results, aim to quantify the burden of a common exposure rather than an uncommon one.

Appendix

4.A Supplementary Tables and Figures

Table 4.5: Other anthropometric measures with higher birth order

	(1) haz	(2) whz	(3) waz
Trimester 1	-0.391	-0.509	-0.555
	(0.480)	(0.438)	(0.417)
Trimester 2	-0.910*	0.299	-0.245
	(0.496)	(0.423)	(0.388)
Trimester 3	-0.722	0.00569	-0.0757
	(0.493)	(0.430)	(0.414)
Trimester 4	-0.897*	-0.0298	-0.368
	(0.461)	(0.391)	(0.385)
Observations	10024	10436	10113
Adjusted R^2	0.147	0.071	0.133

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

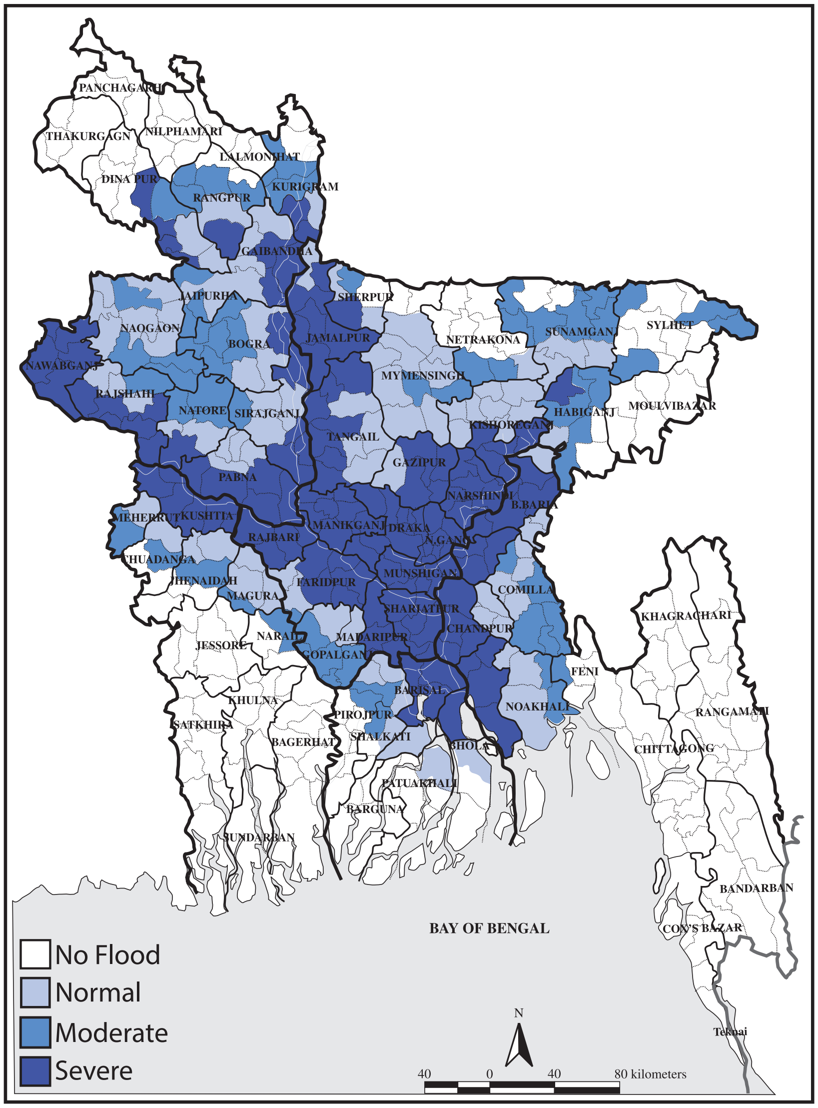


Figure 4.10: An illustration of flood susceptibility in Bangladesh, drawn from flood extent of the 1998 floods. Adapted from Del Ninno et al. (2001).

Chapter 5

An Empirical Approach to Estimating the Climate Change Damage Function

Amir S. Jina, James Rising, Solomon M. Hsiang, Robert Kopp, and DJ Rasmussen[1]

[1]This chapter describes the ecnometric analysis that formed a foundation of the American Climate Prospectus (Houser et al., 2014) (ACP). Physical climate modeling and projections discussed in section 5.2 result from work by Robert Kopp and DJ Rasmussen. Text in a number of sections draws heavily from the technical appendix for the ACP. It was funded under a joint initiative of Michael Bloomberg, Henry Paulson, and Thomas Steyer.

Abstract

The impact of climate change in the 21st century is one of the most important social and policy challenges in human history. Yet to take effective action to mitigate the worst impacts of climate change, we need to quantify those impacts as accurately as possible. Projecting these impacts upon society, as well as calculating the social cost of carbon (SCC) remains an unmet challenge. In this chapter, we construct the first empirically-based and spatially-explicit damage functions of climate change for the United States for four economic sectors - crop yields, crime, labor productivity, and mortality.

We first identify a group of rigorous studies that use climate variability to identify responses to temperature and precipitation, while controlling for unobserved differences between locations. To incorporate multiple studies from a single sector, we employ a meta-analytical approach that draws on Bayesian methods commonly used in medical research. Looking broadly across sectors, we generate a series of aggregate response functions for each sector using this meta-analytical method. We combine response functions in each sector with downscaled physical climate projections to estimate climate impacts out to the end of the century, incorporating uncertainty from statistical estimates, weather, climate models, and different emissions scenarios. Impacts in each sector can then be calculated as a function of temperature, which identifies the sectoral damage functions. A combination of these damage functions is essential for modeling the impacts of climate change as inputs to Integrated Assessment Models.

5.1 Introduction

Economic assessment of climate change requires detailed knowledge of the "damage function" - the function that translates global temperature increases into economic damages. This function is at the core of every economic assessment of climate change. Yet, it has become increasingly clear that the damage function in the current generation of assessments is fundamentally flawed, often based upon unfounded assumptions and unjustified choices by the researchers involved, with many of the leaders of this field openly discussing the crisis (Pindyck, 2013; Stern, 2013). It is, quite simply, too important a number to get wrong.

Recent years have seen an explosion of empirical research leading to an unprecedented advance in our knowledge of the interaction of society and the environment Dell et al. (2013). As the amount of research continues to grow, it becomes more important that studies be used to collectively answer societally important questions. The approach of this chapter towards climate impact assessment is an effort to holistically integrate this research. Our vision was to create a system to not only incorporate recent discoveries, but also to be updated with new research and new findings as they become available. We present below a flexible, open-source, and adaptive system to combine our best estimates of environmental impacts, allowing us to constantly learn the broad societal effects from an evolving body of research. The current approach is not limited to climate, however, as it can be extended to project many different types of impacts (e.g., from policy changes). The tools we have designed can become a central hub enabling researchers to collaborate on a larger body of socially important research.

We identify and employ a meta-analytical approach (described in section 5.3.1) that draws on Bayesian methods commonly used in medical research and previously implemented in Hsiang et al. (2013c). Using these techniques, we design an open-source tool which can update aggregated dose-response functions in real time as new research becomes available. With the method of meta-analysis in place, we then identify a group of rigorous studies across a number of climate-impacted sectors. Combining the dose-response functions from individual studies (detailed in section 5.3.2), we generate a series of aggregate response functions for each sector. Finally, to understand the impact of climate upon each sector, we take the product of our response functions and the downscaled physical climate projections described in 5.2, giving us partial equilibrium impacts out to the end of the current century at a United States county-level.(described in section 5.4). Using these impacts data aggregated nationally, we can then derive sectoral damage functions - climate impacts as a function of temperature increases (above the 1980-2010 mean). These are the first empirically-derived damage functions of climate change in the United States, and will provide the foundation of a global derivation which is part of an ongoing project.

5.2 Climate data

This section provides only a brief summary of the climate modeling section of the ACP[2].

One of the primary challenges of a detailed risk assessment of climate change is to have climate projections which reflect the scale of human activity. For this reason, we required climate data at the county level in the United States, which would allow us to derive impacts in a spatially explicit way, and look at distributional effects of climate change in high-resolution. Data from each of the CMIP5 models was downscaled to sub-county pixel level and aggregated up using weighted averages to county scale.

Typically, each of these climate models is run to provide a "best guess" of the future climate. The ideal experiment, however, would be to run these resource intensive models many times using variations of initial conditions, in order to obtain a probability distribution of future temperatures. In lieu of this, many projections take an average of the ensemble of climate models and assume this is the "median" climate change scenario, though this assumption is never tested. Our approach is to use a low resolution global climate model to populate a probability distribution of future temperatures. CMIP5 models are then scaled and duplicated to fit with this global temperature distribution, and each of these models are downscaled. This provides a much better approximation of the probability distribution of future temperatures than does a simple ensemble average. These are the climate data used for the remainder of this analysis.

5.2.1 Representative concentration pathways

Throughout this chapter we will refer to the representative concentration pathways (RCPs). These are the climate change scenarios used in the fifth assessment report of the IPCC. Temperature projections for the RCPs are shown in fig. 5.1. RCP 2.6 is the low emissions scenario in which concentrations of CO_2 stabilize by mid century due to concerted mitigation effort. RCP 8.5 is a "business as usual"

[2]A full treatment of these data and the methods to derive them can be found Technical Appendix I of Houser et al. (2014). These are the result of work by Robert Kopp and DJ Rasmussen.

scenario in which growth continues on a carbon intensive path and CO_2 keeps accumulating in the atmosphere. RCP 4.5 falls between these two, and is considered the median scenario.

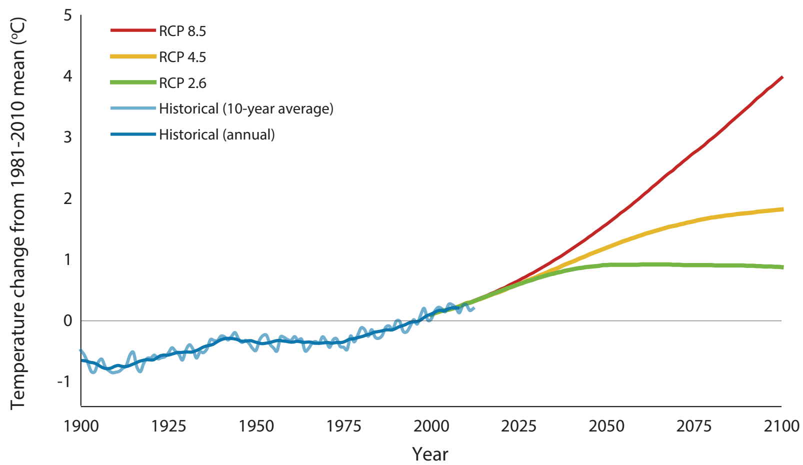


Figure 5.1: Global temperature projections using the CMIP5 models, showing historical temperature and temperature from three representative concentration pathways (RCPs). Figure adapted from Houser et al. (2014)

5.3 Methods

5.3.1 Meta-Analysis Approach

Before we discuss the studies used to apply our climate impacts to the US economy, we identify an approach to aggregate the impact functions from numerous studies of a single sector, previously implemented in Hsiang et al. (2013c). The empirical impact functions that are drawn from studies listed in appendix 5.A are treated as conditional distributions, conditioned on weather variables such as mean temperature and precipitation. This representation facilitates the meta-analysis while also capturing the range of uncertainty in the empirical estimates. Each distribution is evaluated at a given quantile only when it is applied to data, as described in section 5.4.

The impact estimates that combine results from more than one study apply a Bayesian hierarchical model structure, as described by Gelman et al. (2013). This approach simultaneously estimates a distribution of possible effect sizes underlying the individual studies, as well as a degree of "partial-pooling". If the individual study estimates are consistent with a single underlying effect, their estimates are pooled to accurately estimate the effect. However, if the study estimates are inconsistent with each other, the underlying effect is estimated to be only loosely informed by each study, which is then considered to have its own idiosyncratic effect.

Consider a collection of climate impact functions, $f_i(\beta_i|T)$, for $i \in \{1, \ldots, N\}$ indexing independently published results. Here, $f_i(\beta_i|T)$ is a probability distribution for β_i conditioned on a weather variable T. We wish to combine these estimates into a single conditional distribution, $g(\hat{\beta}|T)$, where $\hat{\beta}$ is called the "hyper-parameter". We treat each value of T independently, so we will write these functions as $f_i(\beta_i)$ and $g(\hat{\beta})$. Under hierarchical merging, the conditional parameter distributions are required to be normally distributed, and we assume normal errors in all applicable response functions. The governing equations are,

$$\theta_i \sim \mathcal{N}(\hat{\beta}, \tau^2)$$
$$\beta_i \sim \mathcal{N}(\theta_i, \sigma_i^2)$$

where β_i is a measured parameter, corresponding to a true (unobserved) parameter θ_i which characterizes the response for study i. σ_i^2 is the standard error of β_i. We are interested in $\hat{\beta}$, the underlying hyper-parameter, and τ^2, the variance between models. We apply non-informative priors to $\hat{\beta}$ and τ. That is, $p(\hat{\beta}) \propto 1$ and $p(\tau) \propto 1$. The values of β_i and σ_1^2 are provided by the published studies, and the rest of the parameters are simultaneously estimated. We approximate the posterior by producing draws and constructing a histogram for each conditional distribution. Figure 5.2 shows an example of how pooled and Bayesian hierarchical results differ for a combination of two simple impact functions.

To support the management of empirical results, the meta-analysis combination process, and the application of these results to data, we used the Distributed Meta-Analysis System (DMAS)[3] described in **?**. DMAS was created as part of the ACP project. DMAS is a database of results that can be easily recombined into many different meta-analyses is designed to be expanded in a decentralized manner, by "crowd-sourcing" from scientists working independently to detail their empirical findings[4]. The DMAS library results are conditional distributions, representing one or more parameter estimates typically in a dose-response curve. The following representations are used for impact functions identified in this chapter:

Discrete-Discrete Probability Models: The discrete-discrete probability model represents either

[3] Available online at http://dmas.berkeley.edu/

[4] A similar process of decentralized collection of results has begun for drug discovery (Lessl et al., 2011).

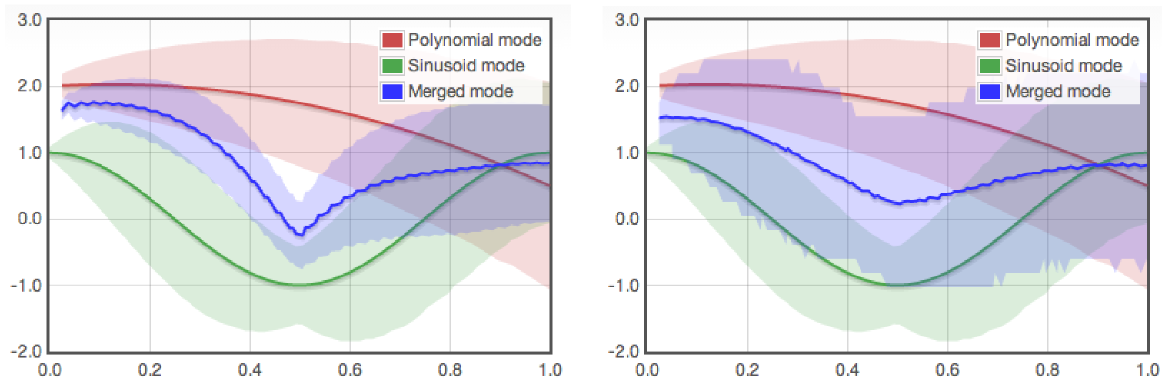


Figure 5.2: Pooled (**left**) and Bayesian hierarchical (**right**) estimates for a constructed polynomial and sinusoidal response function. The pooled distribution is calculated as $p(\hat{\beta}|x) = p(\cap_{i=1}^{N} \hat{\beta} = \beta_i|x) \propto \prod_{i=1}^{N} p(\beta_i|x)$, where $p(\beta_i|x)$ is the conditional distribution for either the polynomial or sinusoidal response function at a given value of x. The 95% confidence interval of the pooled result does not overlap with the individual estimates when they are far from each other. The confidence intervals on $\hat{\beta}$ are wider, reflecting the uncertainty in resolving the two estimates.

a sampled approximation to a continuous probability density function, $f(y|x)$, at discrete values of $y \in \{y_i\}$ and $x \in \{x_i\}$, or a probability mass function of the same form. This is most appropriate when the collection of response outcomes is limited or categorical. Both the dependent and independent variables may be either categorical or sampled at a collection of numerical levels. For continuous functions, the sampling of the dependent variable, $\{y_i\}$, and the independent variable, $\{x_i\}$, can be uneven. This model can be treated as a matrix $P = (p_{ij} = f(y_j|x_i))$. Discrete-discrete probability models are the ultimate form for any Bayesian hierarchical meta-analysis result, after draws from the posterior distribution are organized into histograms.

Spline Models: The spline model represents a continuous conditional probability function, using a spline to denote the log of its values.

$$f(y|x) = \begin{cases} e^{a_0 + b_0 y + c_0 y^2} & \text{for } y_0 \leq y < y_1 \\ e^{a_1 + b_1 y + c_1 y^2} & \text{for } y_1 \leq y < y_2 \\ \ldots & \ldots \end{cases}$$

Distinct splines are described at distinct values of the conditioning variable $x \in \{x_i\}$, which may be categorical or numerical and may be sampled unevenly. The lowest value of y_0 for each x may be $-\infty$, and the highest value of y_1 may be ∞. Spline models are used for most impact functions, since they provide arbitrary resultion on the shape of the conditional distribution curve.

Bin Models: A bin model represents a model defined across continuous spans, where the distribution is constant over each span. It is a combination of information describing the width of each bin and an underlying categorical model of one of the other model types describing each bin's probability distribution. Bin models are used for degree-day impacts as in Schlenker and Roberts (2009), with an underlying spline curve representation for each bin.

Impact functions were chosen after a comprehensive literature review according to selection criteria specified in the following section. Model values were obtained from the papers, from the authors, or in some cases analyses were rerun to ensure comparability with other studies.

5.3.2 Micro-founding impact functions

We develop empirical, micro-founded impact functions for a number of sectors seen to be economically important. These include agriculture, crime, health, and labor. Within each sector, we draw on statistical studies that robustly account for a number of potential confounding factors when trying to identify the impacts of climate. For the current analysis, we make no claim to having performed a exhaustive quantitative meta-analysis from the reviewed papers. Numerous high-quality and insightful studies are omitted from sectors, though many studies were used to confirm the validity of the selected papers. However, we have designed our approach to be *inclusive in the long-run* by building an open-source system for meta-analysis and collaboration. Incorporating each study took considerable effort, often requiring new data, efforts on the part of the original authors and ourselves to rerun analyses, and extensive discussions to ensure an accurate interpretation of results[5]. Our final selection required studies to meet the following criteria:

1. **Nationally representative**. We required that studies be conducted at national level or be drawn from a representative random sample of the entire US. This was of particular relevance to health sector studies. For example, many that we considered performed detailed time-series analysis of single or multiple cities (e.g., Curriero et al., 2002; Anderson and Bell, 2009). While these were high-quality studies, inclusion would have required either a weighting scheme based on city populations or an assumption of national generalizability.

[5] In this process, we are indebted to each of the authors listed in section 5.A

2. **Analyze recent time-periods in US history**. As we are concerned with potential effects of adaptation, we preferred studies that identified effects as close to the present as possible.

3. **Robust to unobserved factors that differ across spatial units** (jurisdictions, counties, or states). We placed an emphasis on studies that were able to control for unobservable differences between spatial units of analysis with the inclusion of fixed effects. This required the use of longitudinal or panel data, as cross-sectional comparisons between could suffer from omitted variable bias.

4. **Identify responses to high-frequency climatic variables** (days or weeks). The importance of using high-frequency data to estimate climate impacts is demonstrated by all papers included, building on early work by Deschenes and Greenstone (2007), and in one case finding large effects by considering sub-daily temperature responses (Schlenker and Roberts, 2009).

5. **Identify responses to the full distribution of temperature and rainfall measures**. Many studies looked at single climatic events, or parts of the temperature or rainfall distribution (e.g., heatwaves in Anderson and Bell, 2011). As we are modeling annual impacts, we chose only those studies that included the full distribution of realized climate outcomes, and ensured the validity of results by comparison to numerous studies looking at single phenomena or sub-populations.

6. **Account for seasonal patterns and trends in the outcomes**. Cyclicality and seasonality of responses to climate forcings is a source of major concern, so we selected only those studies that robustly accounted for seasonal patterns and time trends in their analysis.

7. **Ecologically valid**. We required studies to be valid for real-life circumstances and levels of exposure, which led us to prefer studies that were quasi-experimental in design, using observational data. For example, in the case of labor, numerous laboratory studies exist on the intensive margin effects of temperature upon productivity (e.g., Seppanen et al., 2006). As these raised a question of ecological validity when applied to the labor sector, we chose to not include them.

Many of the impacts of climate change will unfold over years, but distinguishing between the role of climate change and the role of social, technological, and economic evolution is very difficult over any long time horizon. Our criteria for selecting studies requires that long-term trends are accounted for and are not reflected in the measured impact response functions. As a result, the impacts that we measure are from weather "shocks", short-term changes in temperature and precipitation which are not captured by long-term trends. This approach has both strengths and weaknesses. Its key strength is that it clearly identifies the impacts of weather as distinct from longer-term changes. However, it may miss many of the long-term impacts of climate change that do not take the form of increases in the size, frequency, and duration of weather shocks.

We identify a number of studies using panel data to isolate the variation within the relevant spatial unit, while controlling for unobservable differences between units. Estimates from each of the studies were combined, as detailed in section 5.3.1. We have been conservative in our choice of studies for the current analysis, using only studies which we think most credibly identify the impact of climate upon specific outcomes in each sector. However, our approach allows for future studies to be incorporated, introducing new findings, and modifying the current results. The following is a complete list of empirical response functions used in this study, shown in fig. 5.3. A detailed discussion of each of the studies can be found in Appendix 5.A.

Agriculture	Maize yields vs. temperature (East)
	Maize yields vs. temperature (West)
	Maize yields vs. precipitation (East)
	Maize yields vs. precipitation (West)
	Wheat yield vs. temperature
	Soybean yields vs. temperature (East)
	Soybean yields vs. temperature (West)
	Soybean yields vs. precipitation (East)
	Soybean yields vs. precipitation (West)
	Cotton yields vs. temperature
	Cotton yields vs. precipitation
	Maize yields vs. 100ppm CO_2 increase
	Wheat yields vs. 100ppm CO_2 increase
	Soybean yields vs. 100ppm CO_2 increase
	Cotton yields vs. 100ppm CO_2 increase
Crime	Violent crime vs. temperature
	Violent crime vs. precipitation
	Property crime vs. temperature
	Property crime vs. precipitation
Health	Mortality vs. temperature (all age)
	Mortality vs. temperature (younger than 1 year)
	Mortality vs. temperature (1 - 44 years)
	Mortality vs. temperature (45 - 64 years)
	Mortality vs. temperature (65 years and up)
Labor	Hours worked in high-risk industries vs. temperature
	Hours worked in low-risk industries vs. temperature

Results in agriculture were drawn from Long et al. (2006), Schlenker and Roberts (2009), Fisher et al. (2012), Burke and Emerick (2013), McGrath and Lobell (2013), and Hsiang et al. (2013a); results for crime were taken from Jacob et al. (2007) and Ranson (2014); results for health were from Deschênes and Greenstone (2011) and Barreca et al. (2013); and results from labor were from Graff Zivin and Neidell (2014).

Storage

In addition to the above impacts on yields, we observe that farmers store crops for sale in the future, and so the overall impact of climate on supply of crops may appear smoother than if there were no storage. For our projections, we also make use of Fisher et al. (2012, Appendix p.xi, table A4) to estimate crop consumption as a moving average process of crop production. We estimated the following equation for crop c,

$$\ln(consumption)_{c,t} = \sum_{l=0}^{L} [\beta_{c,l} \times \ln(production)_{c,t-l}] + \theta_c t + \gamma_c t^2 + \epsilon_{c,t}$$

where $L = 2$ and $L = 3$ for soybeans, and we account for linear and quadratic time-trends. Results of this process are shown in fig. 5.4. We project the smoothing of future crops with a time-series structure that incorporates these empirical results on storage. Weights for each crop are constructed from the lagged coefficients, β_l.

5.4 Application of Impact Functions

We apply a Monte Carlo approach for sampling the conditional distributions for each impact function. This captures the full range of uncertainty in impact functions estimates, under the assumption that each impact function is independent. For each of the 26 empirical distributions discussed in section 5.3.2 we randomly select quantiles and resample values to characterize the full distribution. The same quantile is used across the entire range of the conditioning variable. By evaluating each impact function at a quantile, we generate a single-dimensional, deterministic function which is used in the evaluation of the impact for each Monte Carlo or constant quantile run.

The impact results for crime, labor productivity, and mortality are all estimated by binning weather values. In these cases, we construct a continuous impact curve by linearly interpolating between the

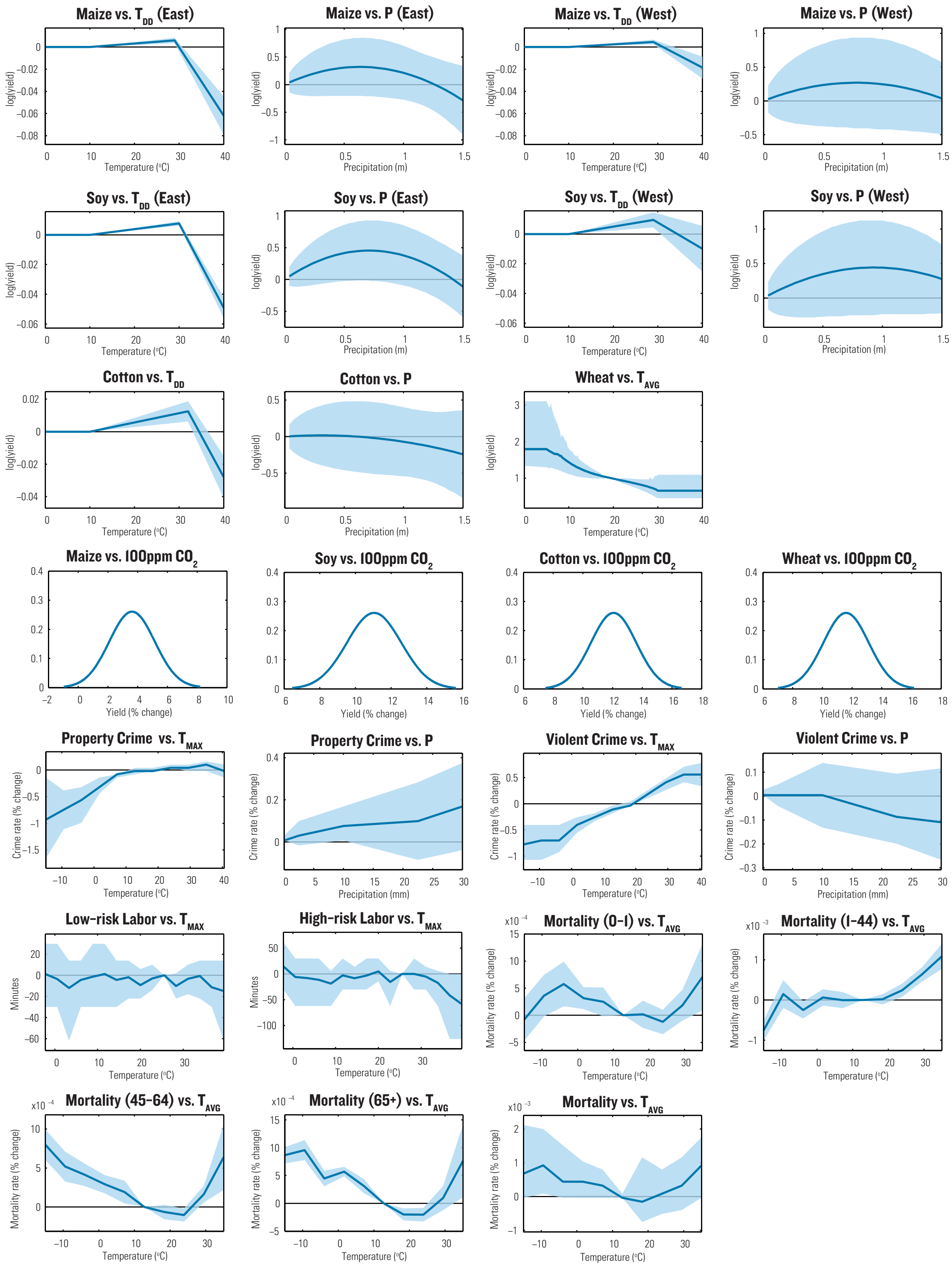


Figure 5.3: All 26 dose-response functions used in our analysis. 95% confidence intervals are shaded.

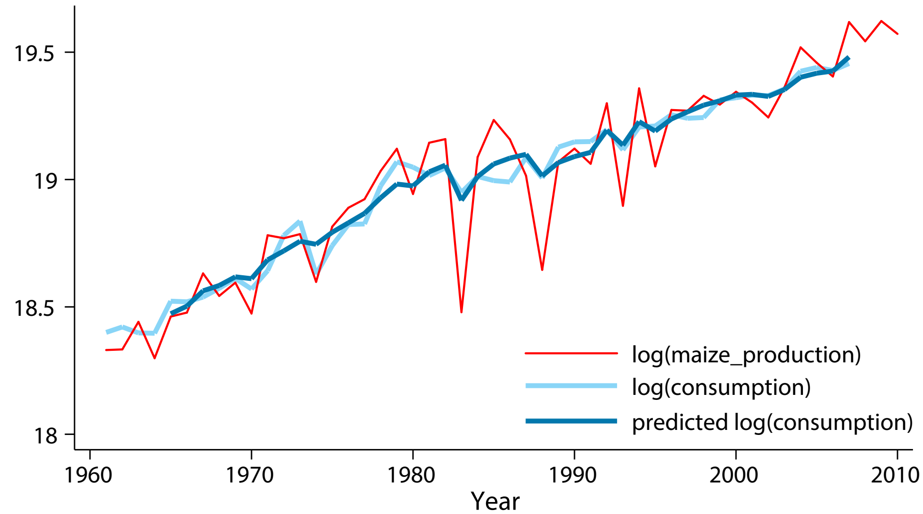


Figure 5.4: Predicted consumption of maize, modeled as a moving average of production. Predicted values compare well to observed consumption, and allow us to project the smoothed consumption values out to the end of the century.

midpoints of these bins. Values are assigned the impact of the top (bottom) response in the case when they are higher (lower) than the support of the response function. Impact results are calculated for each county in the United States four different RCP projections[6] (2.6, 4.5, 6.0, and 8.5), from all CMIP5 models (ranging from 28 to 44 in each RCP), and each with ten weather realizations. Impacts are reported *relative to 2012* throughout.

The twenty-six conditional distributions were used to generate 15 impact results. This number is reduced further by the aggregation of separate crops into an agricultural impact and age-specific mortality into a general all-age mortality response function. These calculations are described below. We use the notation T_{AVG} for mean daily temperature; T_{MIN} and T_{MAX} for minimum and maximum daily temperature, respectively; and P for for precipitation. All weather variables are provided at the county-level and on a daily basis. Below, where counties are index by j, weather variables are implicitly also indexed by j. We index days in the year by d and years by t.

Agricultural Yields and Production

Percent changes in agriculture production, relative to 2012, were generated using fixed, county-specific growing seasons. The growing season, denoted $S(j)$ for county j, is determined using the centroid

[6]Note that in this chapter we only display the results for RCPs 2.6, 4.5, and 8.5.

of the county applied to the planting and harvesting dates in Sacks et al. (2010). For maize and wheat, for which Sacks et al. (2010) provides two calendars (two croppings for maize, and summer and winter wheat), the calendar that represented the greatest portion of land area in each county was used. In the notation below, DD_{low} and DD_{high} are growing degree days below and above the crop-specific breakpoints specified in Schlenker and Roberts (2009), and calculated using a sinusoidal fit to the minimum and maximum daily temperatures ($T_{MAX,d}$, $T_{MIN,d}$). Appendix fig. 5.14 shows distributions of degree days for RCP 6.0, under the MIROC-ESM-CHEM model. Relative changes in yield are calculated based on seasonal temperatures and precipitation as follows:

Wheat Wheat uses a seasonal average temperature response function:

$$Y_{jt} = f\left(\frac{1}{N(S(j))} \sum_{d \in S(j)} T_{AVG,d}\right)$$

where $f(\cdot)$ is calculated by Hsiang et al. (2013a), as a function of average mean daily temperature over the growing season, and $N(S(j))$ is the number of days in the growing season for county j. This functional form was only used for wheat, since a degree-day representation was unavailable.

Cotton Cotton uses a single degree-day function:

$$Y_{jt} = e^{f\left(0.01 \sum_{d \in S(j)} DD_{low}, 0.01 \sum_{d \in S(j)} DD_{high}\right) + g\left(1 \times 10^{-3} \sum_{d \in S(j)} P_d\right)}$$

The functions $f(\cdot)$ and $g(\cdot)$ translate degree days and precipitation, respectively, into yield effects.

Maize and Soybeans Maize and soybeans have two degree-day responses:

$$Y_{jt} = \begin{cases} e^{f_{east}\left(0.01 \sum_{d \in S(j)} DD_{low}, 0.01 \sum_{d \in S(j)} DD_{high}\right) + g_{east}(1 \times 10^{-3} \sum_{d \in S(j)} P_d)} \\ e^{f_{west}\left(0.01 \sum_{d \in S(j)} DD_{low}, 0.01 \sum_{d \in S(j)} DD_{high}\right) + g_{west}(1 \times 10^{-3} \sum_{d \in S(j)} P_d)} \end{cases}$$

Here, $f_{east}(\cdot)$ and $g_{east}(\cdot)$ are used to the east of the 100th meridian, excluding Florida. This is the sample stratification used by Schlenker and Roberts (2009), as this region is less irrigated than the western states.

CO_2 fertilization is modeled as a multiplicative factor applied to yields, and estimated as a linear increase for each additional 100 ppm of CO_2:

$$Y'_{jt} = Y_{jt}\left(1 + \frac{C_t - C_{2012}}{100}X\right)$$

where C_t is the CO_2 concentration in year t under a given RCP, and X is the estimated CO_2 fertilization effect, which varies from 3% to 12% depending on the crop, from McGrath and Lobell (2013). McGrath and Lobell (2013) does not provide a value for cotton, so the Bayesian combination of all provided crop effects is used for it.

Economic output from the agricultural sector is not synonymous with yield, due to strategic storage. We model output as an autoregressive process of yields, as estimated from USDA data. For grains and cotton, the expression is:

$$I_{jt} = 0.51Y_{jt} + 0.28Y_{j,t-1} + 0.21Y_{j,t-2}$$

A four-year moving average is used for soybeans:

$$I_{jt} = 0.2Y_{jt} + 0.52Y_{j,t-1} + 0.19Y_{j,t-2} + 0.1Y_{j,t-3}$$

Crime

Both violent and property crime are calculated as,

$$I_{jt} = \left(1 + 0.01\frac{1}{12}\sum_{d\in y(t)} f(T_{MAX,d})\right)\left(1 + 0.01\frac{1}{12}\sum_{d\in y(t)} g(P_d)\right)$$

where $y(t)$ is the set of days in year t, $f(\cdot)$ is a function of daily maximum temperature, and $g(\cdot)$ is a function of daily precipitation. Both $f(\cdot)$ and $g(\cdot)$ are calculated as a Bayesian combination of the effect for each sub-category of crime estimated by Ranson (2014) and the average effect for violent or property crime from Jacob et al. (2007).

Mortality

Both average and age-specific mortalities are calculated as,

$$I_{jt} = \sum_{d \in y(t)} f(T_{AVG,d})$$

The parameters of $f(\cdot)$ are calculated as a Bayesian combination of the results from Deschênes and Greenstone (2011) and a modified form of Barreca et al. (2013). Age-specific mortalities, for newborns, ages 1-44, ages 45-64, and ages 65 and up, are provided by Barreca et al. (2013). Mortality is reported as changes in the mortality rate (*deaths per 100,000*), with age-specific mortality rates per county from Center for Disease Control and Prevention (2013).

Labor Productivity

The structure of the labor productivity calculation is identical for high-risk and low-risk sectors:

$$I_{jt} = \frac{H + \frac{1}{60} \sum_{d \in y(t)} f(T_{MAX,d})}{H}$$

where H is the average number of hours worked per year in the baseline. For high-risk labor, $H = 7.67 \times 365$, and for low-risk labor, $H = 6.92 \times 365$. The parameters of $f(\cdot)$ are provided by Graff Zivin and Neidell (2014).

5.5 Adaptation

The main analysis presents projections of climate impacts with an assumption of no adaptation. Yet, historically, when the climate imposed an economic cost upon society, adaptive responses were taken to minimize these costs. These adaptive behaviors, both autonomous and planned, can be expected to occur as climate impacts increase in the future.

To understand the extent to which adaptation might decrease some of the worst impacts of climate change, we empirically estimate adaptive responses. We do this in three sectors considered in the analysis - crop yield, crime, and mortality - and estimate adaptive capacity in two steps. First, looking at changes in climate impacts through time, we identify a historical rate of adaptation. Second, spatial differences in climate impacts are then used to stratify regions into more adapted or less adapted based

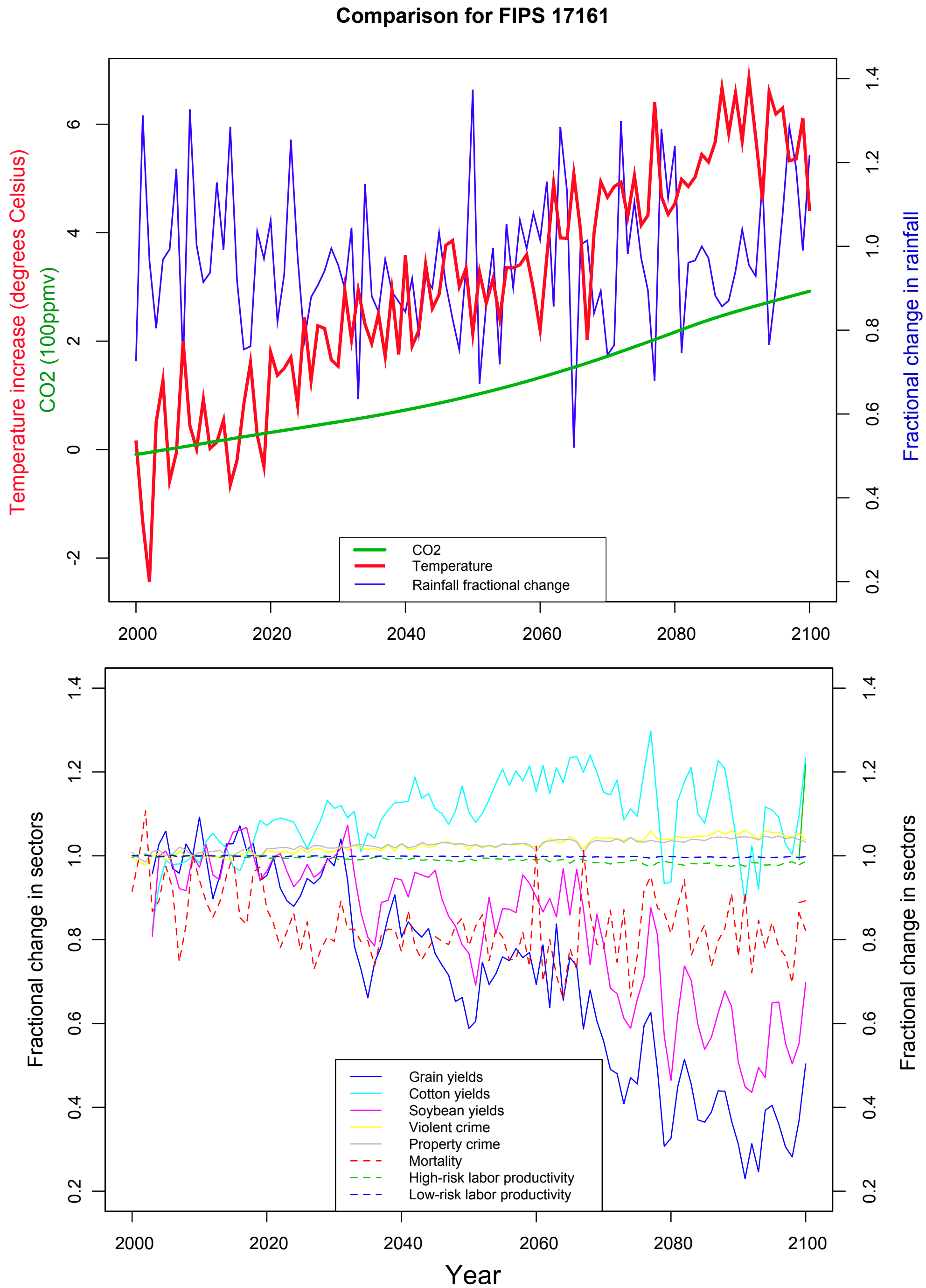


Figure 5.5: Evolution of impacts for FIPS 17161, Rock Island, IL, under RCP 6.0 as modeled by MIROC-ESM-CHEM. In the top panel, the left axis measures linear changes in temperature (°C) and CO_2 (100 ppmv). The right axis measures changes in rainfall relative to 2000-2009 annual mean. In the lower panel, both axes show fractional changes in each of the sectors relative to 2000-2009 annual mean.

on climate averages. As these averages change across counties in the US, we allow each to become more adapted at the rate identified in step one. We are then able to estimate the residual damages, assuming that only the historical adaptive behaviors have taken place.

Importantly, we are unable to estimate any costs associated with these adaptations, nor are we able to estimate more novel (for example, new technological discoveries) or more disruptive (for example, migration) adaptive behaviors.

However, an important insight is that historical adaptive behaviors may not be capable of reducing the worst impacts of climate change. The persistence of impacts in even the most exposed areas indicates that there are non-trivial costs associated with adaptation that will need to be met from other sources or through novel behavioral changes. Additional details are added in sections specific to each adapted response, below. Each impact response starts from the same baseline as the unadapted case, in the year 2000. It then transitions asymptotically toward a future shape. The evolution of each parameter of the distribution follows,

$$\beta(t) = \beta(\infty) + (\beta(t_{before}) - \beta(\infty))e^{-(t-t_{before})/\tau}$$

where $\beta(\infty)$ is the maximum possible adaptation, which in the cases considered here represents no response to temperature, $\beta(t_{before})$ is the parameter from a historical period, as estimated for a period centered on t_{before}, and τ determines the rate of evolution of the parameter.

Using parameters estimated for second period, we calculate the rate of adaptation, τ, as,

$$\tau = -\frac{t_{after} - t_{before}}{\log(\beta(t_{after}) - \beta(\infty))/(\beta(t_{before}) - \beta(\infty))}$$

where $\beta(t_{after})$ is the parameter value during a period centered on t_{after}. This process is represented on the left of figure 5.6.

We further manipulate τ into an incremental product, $\gamma = e^{-1/\tau}$, so that,

$$\beta(t+1) = \gamma\beta(t) + (1-\gamma)\beta(\infty) \tag{5.5.1}$$

With the exception of maize, a different $\beta(\infty)$ is used for determining the rate of evolution (τ and γ) as is used for estimating the final evolution of the parameter values. The $\beta(\infty)$ used to estimate the actual evolution of the parameters is thought of as the parameter value for a fully adapted region of a particular temperature. As a result, it is a function of average temperature: the response curve for New

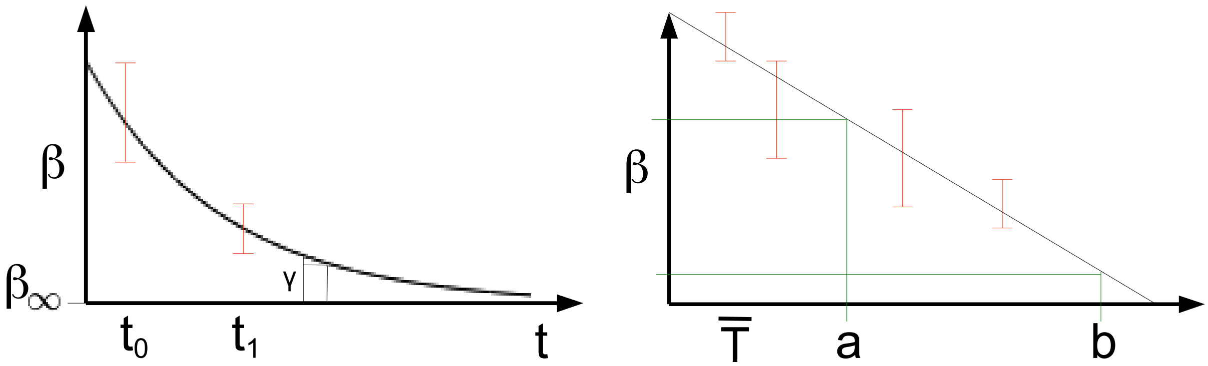


Figure 5.6: **Left:** Estimation of temporal evolution of variable values. A historical estimate (at t_0) and a current estimate (at t_1) are used to fit an exponential, characterised by γ. **Right:** Estimation of adaptation as a function of average temperature. Regional estimates (in red) are used to produce a linear approximation for the way that the coefficient varies with average temperature ($\bar{T}$), which is then evaluated (green) to determine coefficients at future times.

England, under the current climate, is considered to be fully adapted to a colder average temperature than the response curve for the Southwest. To estimate how $\beta(\infty)$ changes with temperature, we use values from several regions in the US, treated each as fully adapted to its current climate.

Specifically, we construct a linear approximation of the evolution of $\beta(\infty)$ across different regions, as characterized by their average temperature $\bar{T}$. Let this function be denoted $f(\bar{T})$. At any point in time, $\bar{T}$ is calculated as the average temperature over the previous 15 years, and $\beta(\infty) = f(\bar{T})$. This allows the fully-adapted coefficients to be predicted both between the temperatures of observed regions, and extrapolated beyond them. The process is shown on the right of figure 5.6.

Each section below describes the evidence used to estimate the temporal evolution of impact responses (the rate of adaptation, τ), and the spatial estimation (how $\beta(\infty)$ varies with $\bar{T}$).

Maize in the Eastern U.S.

Increasing the availability of irrigation is a key adaptation to climate change, since the additional water allows crops to continue to grow at higher temperatures. Fields to the east of the 100$^{\text{th}}$ meridian are much less consistently irrigated than those to the west. This helps explain the lower response to killing degree-days in the west ($\beta = -0.21$, compared to -0.62) (Schlenker and Roberts, 2009).

Temporal Evolution The killing degree-day coefficient has evolved over the course of the last half century, as shown on the left of figure 5.7. Using Monte Carlo draws from these distributions and fitting exponentials, γ is calculated to be $0.997\,22 \pm 0.005\,73$. We approximate the distribution of values for γ with a Gaussian, as shown on the right of figure 5.7. This corresponds to a very slow rate of adaptation, with a time constant of about 360 years (Burke and Emerick, 2013).

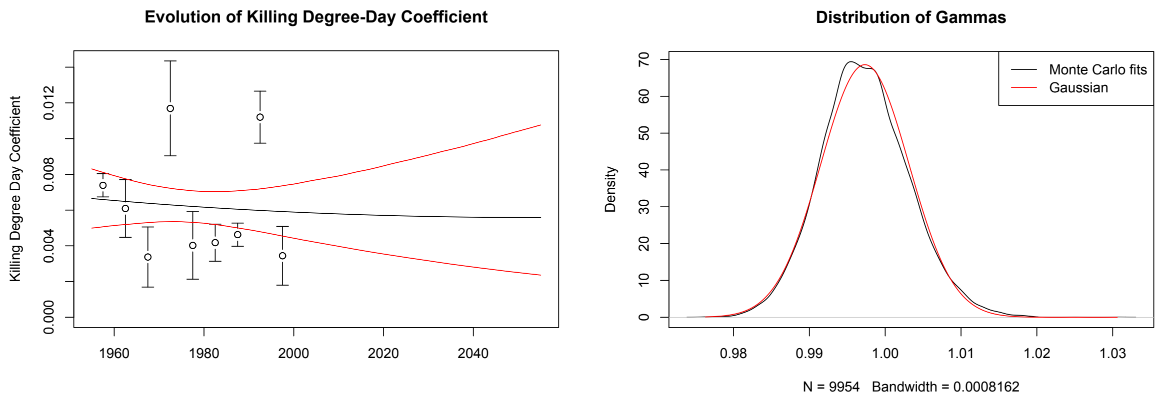


Figure 5.7: **Left:** Killing degree-day coefficients computed for 5-year periods between 1955 and 2000. Red lines show the 90% confidence interval on the exponential evolution of the killing degree-day coefficient. **Right:** Values of γ computed in the Monte Carlo, and the corresponding Gaussian approximation.

We hold constant the growing degree-days coefficient; only the killing degree-day coefficient adapts. Since the growing degree-day coefficient is higher in the east than in the west, this may result in an optimistic consideration of the trade-offs that result from adaptation.

Spatial Estimation No extrapolation is performed for maize. Instead, each year the killing degree-day coefficient in the East is updated according to equation 5.5.1.

Temperature-Related Mortality

Temporal Evolution For temperature-related mortality, we assume that there are two parallel adaptations occuring: one for cold temperatures and one for hot temperatures. The rate of temporal evolution for these processes is estimated as the mean of the values of γ for each bin for which $T < 65°F$, and for each bin for $T > 65°F$, respectively. Figure 5.8 shows the coefficient estimates for each bin and the calculation of γ for the median of these coefficient distributions. In the evaluation of actual impacts, Monte Carlo draws are taken from the various distributions, and the value of γ is estimated separately for each Monte Carlo run. The time constant for the cold-related mortality is 31.3 years, and for heat-related deaths is 84.0 years.

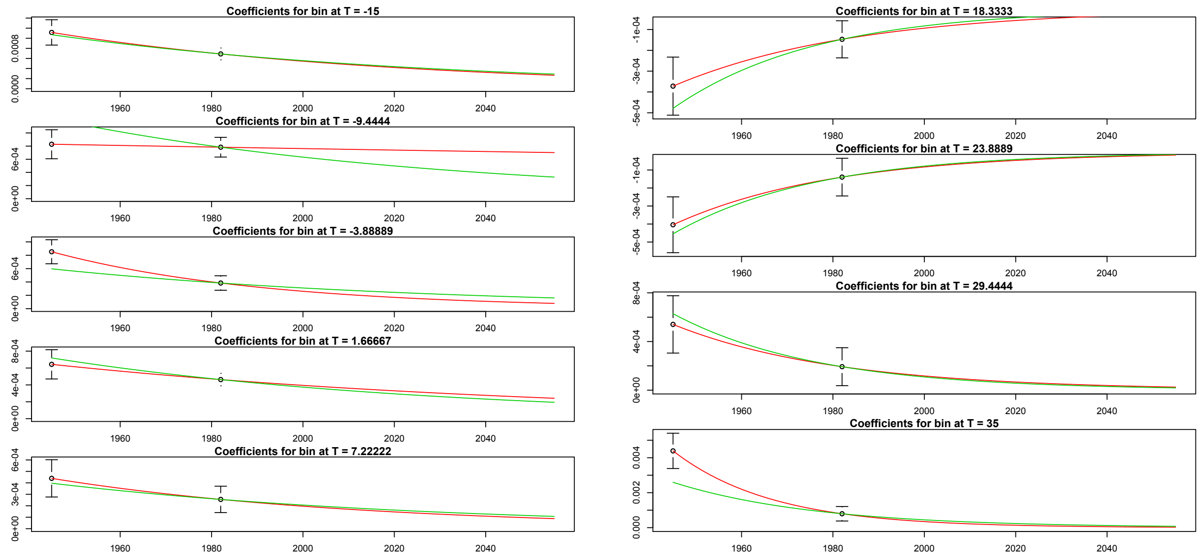


Figure 5.8: Historical coefficient estimates for cold-related (left) and heat-related (right) mortality. Red shows the time evolution based only on the single coefficient, while green shows the pooled time evolution for all coefficients in that set.

Spatial Estimation Estimates were given for four regions: the Northeast, Midwest, South, and West. These, along with the pooled estimate, were used to construct a linear approximation. See figure 5.9.

Violent and Property Crime

The process for estimating adaptation for crime is similar to the process for temperature-related mortality. Pooled results for all forms of property crime and all forms of violent crime were used, as segregated by regions with different maximum temperatures, from Ranson (2014). Since the maximum temperature for the national estimate was very similar to the segregated $65°F$ region, and because the national estimate for the highest temperature bin is outside of the coefficient range of the segregated regions, we used the $65°F$ response function as the baseline for estimating adaptive capacity.

Temporal Evolution Estimates for periods centered at 1960 and 2000 were used to estimate the temporal evolution of crime adaptation. Two rates were calculated: one for bins below $65°F$ and one for bins above that value (which also corresponds to the dropped bin in Ranson (2014)). These calculations are shown in figure 5.10.

For violent crime, the time-constant is 71.2 years for the lower range of temperatures and 139 years for the upper range. For property crime, the time-constants are 793 years and 16.4 years, respectively.

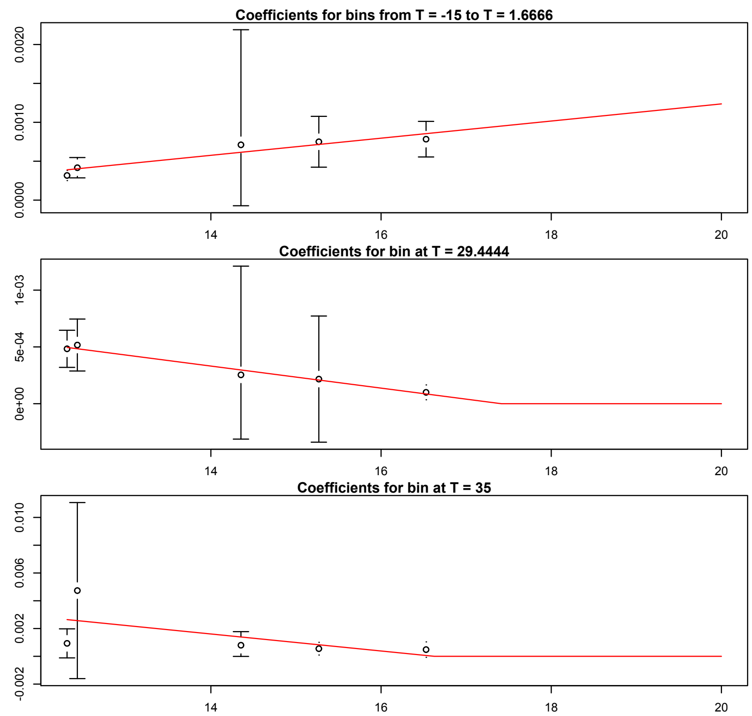
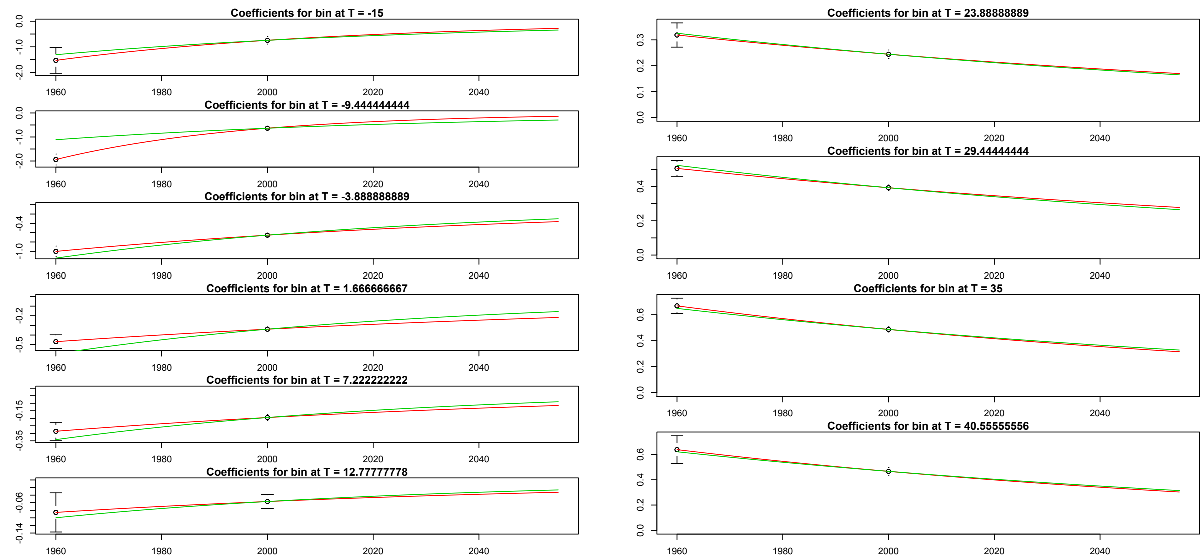


Figure 5.9: Linear approximations for β as a function of $\bar{T}$. Excess mortality for days of a given temperature is clipped at 0. A single coefficient was provided for cold-related mortality ($T < 65°F$).

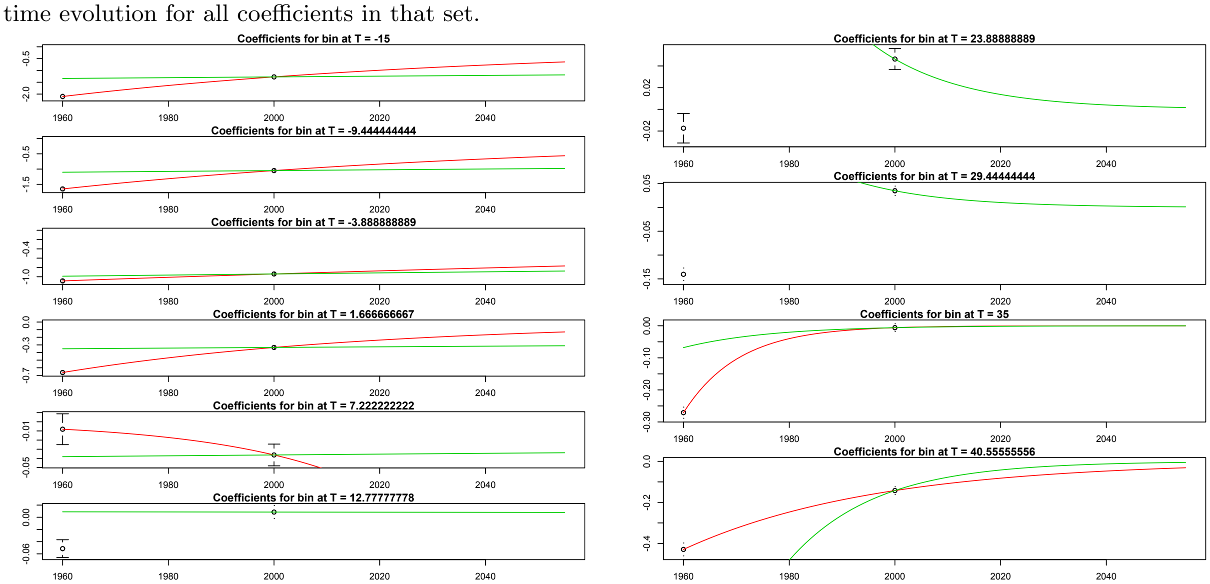
Spatial Estimation Estimates were analyzed from regions with an average maximum temperature of $T = 45°F$, $T = 55°F$, $T = 65°F$, and $T = 75°F$. Figure 5.11 shows these estimates by bin.

5.6 Impact aggregation

County-level results are aggregated to the state- and national-levels as weighted sums. Of the 36000 results generated across all RCPs, models, weather realizations, and Monte Carlo draws of the impact functions, .3% had to be dropped due to outliers in the estimates of additional mortality in the age 1 - 44 cohort. These outliers were unrealistically high estimates of national averaged mortality, in excess of 100 additional deaths per 100,000. The results from runs that had outlier mortality rates were dropped for all impacts.



(a) Historical coefficient estimates for violent crime driven by maximum temperatures below (left) and above (right) $65°F$. Red shows the time evolution based only on the single coefficient, while green shows the pooled time evolution for all coefficients in that set.



(b) Historical coefficient estimates for property crime driven by maximum temperatures below (left) and above (right) $65°F$. Red shows the time evolution based only on the single coefficient, while green shows the pooled time evolution for all coefficients in that set. The value of γ for the bins at $T = 12.7778$, $T = 23.8889$, and $T = 29.4444$ cannot be estimated since the two estimates are on opposite sides of the zero-line.

Figure 5.10

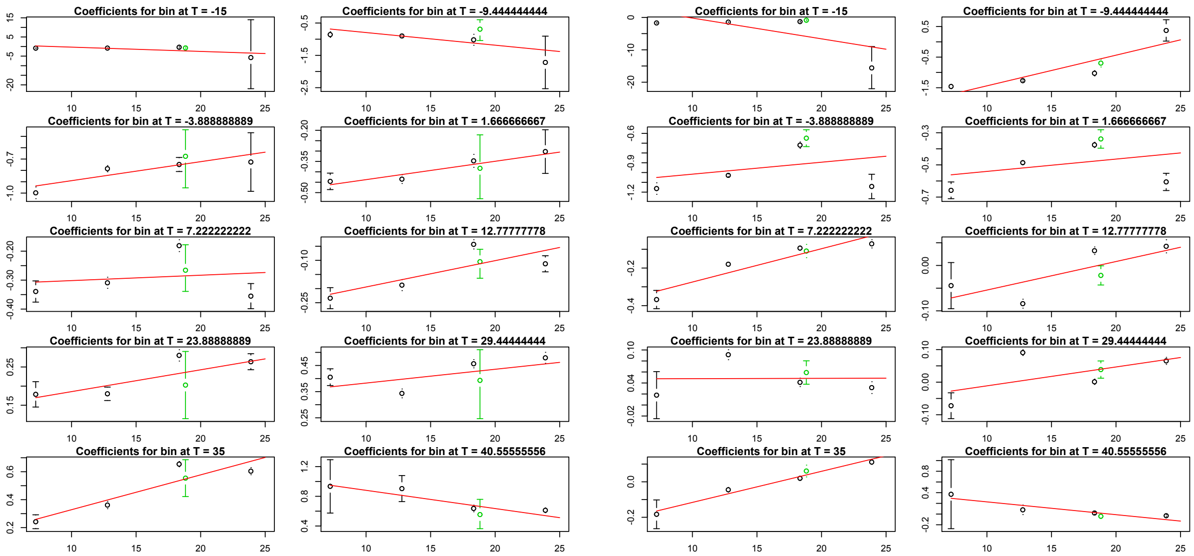


Figure 5.11: Estimates for the effect of temperatures within each temperature bin, for four regions (characterized by $T = 45$, $T = 55$, $T = 65$, and $T = 75$). The green estimate is the national average estimate, at a national average maximum temperature of $65.88603°F$, but was not used for the estimation. The red lines were used for interpolating and extrapolating coefficient values, based on temperature.

Agricultural aggregation Grains yields (maize and wheat) are both combined within the same county and aggregated to higher scales by calories totals. Within each county, the average impact is,

$$I_i = \frac{I_i^{wheat} A_i^{wheat} C^{wheat} + I_i^{maize} A_i^{maize} C^{maize}}{A_i^{wheat} C^{wheat} + A_i^{maize} C^{maize}}$$

where A_i^{wheat} is the average acres of wheat planted between 2000 - 2005, and A_i^{maize} is the average acres for maize. C^{wheat} and C^{maize}, the calorie density of wheat and maize, are taken to be 1690 calories/kg and 1615 calories/kg, respectively. The weighting of each county result for aggregation to higher scales is

$$W_i = A_i^{wheat} C^{wheat} + A_i^{maize} C^{maize}$$

Cotton and soybean results are aggregated by weighting by the total area planted, as averaged over 2000 - 2005. Since impacts are proportional to changes in yields, and yields are calculated relative to area planted, this is equivalent to weighting counties by production.

All crops are weighted by production (in 1000 MT) for constructing total agriculture impact distributions and maps. Production is calculated from the USDA reported bushels measurement using 56 lbs/bushel and 60 lbs/bushel for soy and wheat, and averaged over 2000 - 2005. The data used here come from reproduction data for Schlenker and Roberts (2009).

Crime aggregation Counties are weighted by the number of reported property and violent crimes from the Uniform Crime Statistics, averaged over 2000 - 2005, and provided for reproduction of Ranson (2014). Counties that are not explicitly identified at the county level (of which there are 172) are aggregated using the mean country rates of property and violent crime. Furthermore, since we use the 2010 census for county populations, these rates are scaled by 0.9339, the ratio of the average national population in 2000 - 2005 to the population in 2010, before being scaled by the individual county populations in 2010, to maintain comparability.

Labor aggregation Labor employment by county is averaged over 2000 - 2005, as reported by the Bureau of Labor Statistics (Bureau of Labor Statistics, 2014). Following Graff Zivin and Neidell (2014) high-risk sectors consist of agriculture, forestry, fishing and hunting; mining, quarrying, and oil and gas extraction; utilities; construction; manufacturing; and transportation and warehousing. All others are considered low-risk.The BLS statistics exclude the counties represented by FIPS codes 02105, 02195, 02198, 02230, and 02275, since these were created after 2005.

Mortality aggregation Counties are weighted by 2010 census populations for aggregating mortality.

5.6.1 Distributions across result sets

Within each RCP, models are weighted to capture a desired distribution of temperatures, according to the weighting scheme discussed in section 2 of this chapter. The weighting process for results for a given weather realization of a given RCP involves constructing an ECDF as follows. Let the calculated value of an impact in a given county and year for model $m \in \{1, \dots, M\}$ be I_m. The CDF of this impact across all models is,

$$F(I) = \frac{1}{\sum_m w_m} \sum_{m \text{ for } I_m \leq I} w_m$$

where w_m is the weight given to model m provided in Tables A3 - A6. Extending this process to multiple weather realizations and multiple batches is done by simply including all available results in the weighted ECDF[7].

[7] Unweighted results are reported in table 5.10.

5.7 Results

5.7.1 Sectoral climate change damage functions

Figure 5.12 displayed the sectoral damage functions for 6 impacts across 4 sectors. Color correspond to the different RCPs, white lines are a cubic spline fit of the median, with a shaded gray region bounded by a cubic spline fit of the 95%iles of each model's distribution. With the exception of agriculture, each of the damages is approximately linear in temperature. This is true despite highly non-linear response functions for mortality and labor. Though we have only percentage changes on these figures, we can still roughly interpret which of them will have the largest magnitudes. Owing to the VSL value of life attached to mortality, even modest increases in mortality will dominate other impacts, with labor productivity impacts being the second largest. With these estimates in place, we can apply cost calculations to estimate the overall damage function of ciliate change.

5.7.2 Adaptation results

Figure 5.13 shows the results of the adaptation exercise described earlier in this chapter. Surprisingly, we see that even with no constraints from costs, historical adaptation will not greatly reduce impacts in most sectors. Mortality is likely one in which the largest gains from adaptation are shown. Results for agriculture and crime are largely supported by estimates of historical adaptation in Burke and Emerick (2013) and Ranson (2014), respectively.

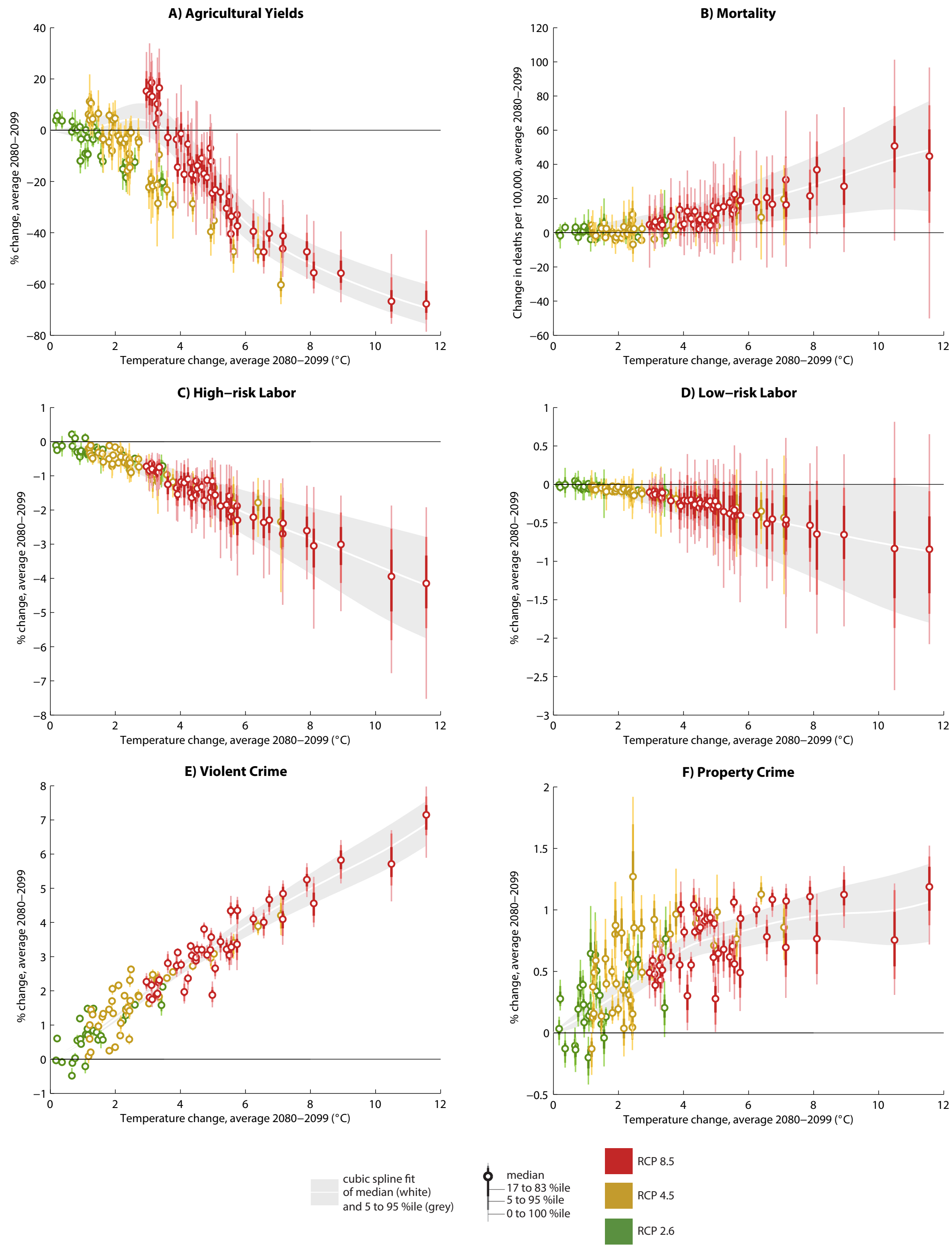


Figure 5.12: Percent damages versus temperature for six economically important impacts across four sectors. All data are plotted from end of century impacts and model temperatures, each averaged over 2080-2099.

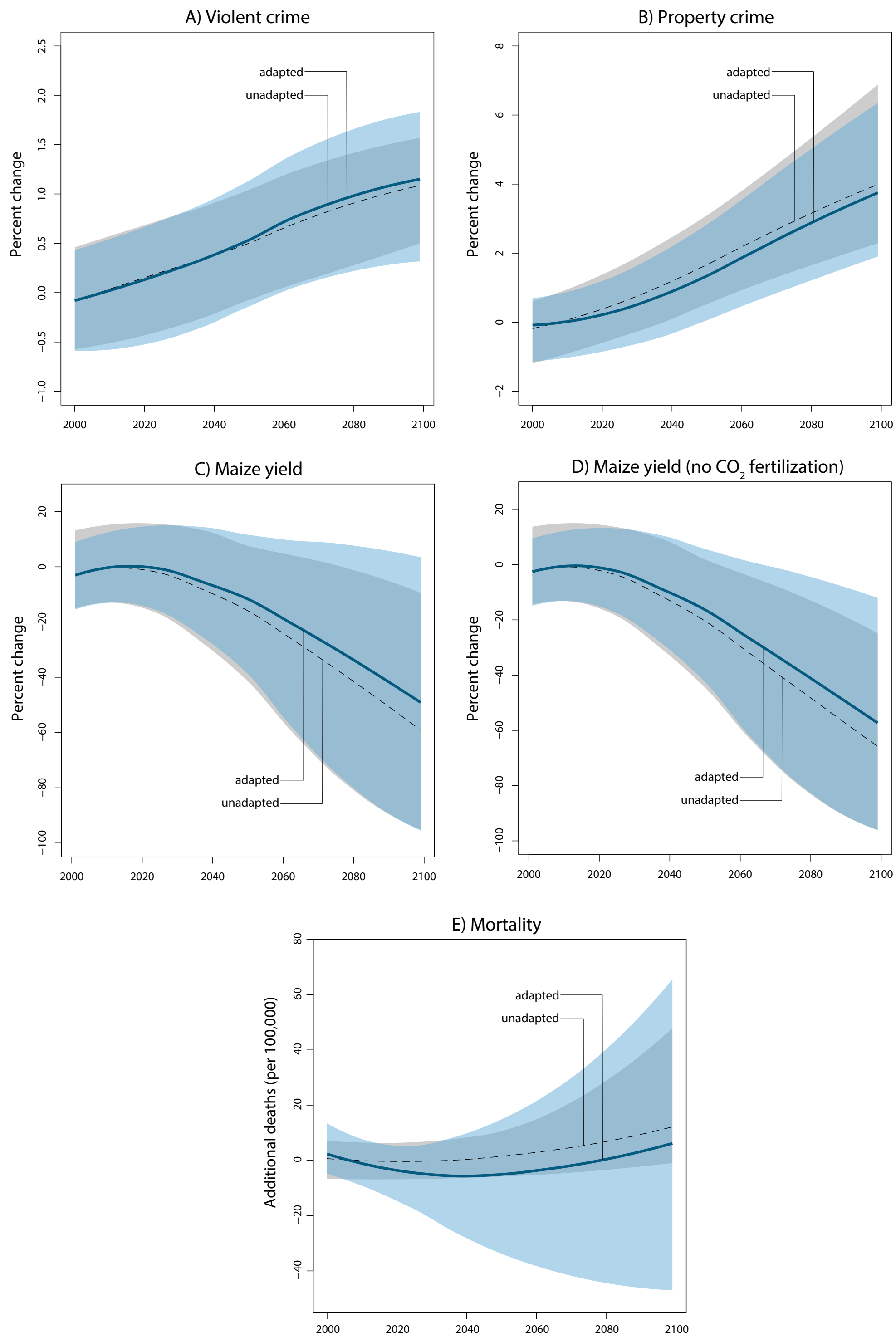


Figure 5.13: Residual damages after adaptation for 3 sectors.

Appendix

5.A Micro-studies used in aggregation

Agriculture

Schlenker and Roberts (2009)

Outcome data:	Yields for maize, soybeans, and cotton from US Department of Agriculture National Agricultural Statistical Service.
Climate data:	PRISM temperature and rainfall, temporally downscaled to daily resolution.
Sample period:	1950-2009
Sample unit:	County-years, for counties with recorded yields of maize, soybeans, or cotton
Methodology:	Piecewise linear response of log(yield) to cumulative temperature (degree days) and polynomial response to precipitation (seasonal total), controlling for county fixed effects and state-specific quadratic trends. Piecewise linear models are specific to each crop type, with thresholds that capture the beneficial effects of temperatures below a certain point, and the deleterious effects above.
Result:	Modified version of Schlenker and Roberts, 2009, SI Appendix, p. 9, fig. A3; and p. 20, fig. 10
Impact function:	We contacted the authors of the study to select a preferred response function from the multiple methods they had employed, selecting a piecewise-linear specification using degree days for temperature and seasonal total precipitation. We obtained impact functions for each of the three crops studied, for both temperature and precipitation. The authors note the distinct difference in response between counties to the east and west of the 100^{th} meridian for maize and soybeans, so we obtained separate response functions in for these regions. On December 19^{th}, 2013, we were sent a complete list of response functions that were updated span the time period up to and including 2011 (as presented in Berry et al., 2012).

Hsiang, Lobell, Roberts, and Schlenker (2013a)

Outcome data:	USDA-NASS

Climate data:	University of Delaware monthly temperature and precipitation
Sample period:	1950-2007
Sample unit:	County-year
Methodology:	Non-linear response of log(yield) to crop-specific seasonal average temperature and precipitation, controlling for county and year fixed effects.
Result:	Hsiang et al., 2013a, p. 19.
Impact function:	We use the response of wheat to seasonal average temperature presented in the paper. Results were obtained from the author. Calorie weighted averages were taken between maize and wheat in order to combine results, as detailed in section 5.4.

McGrath and Lobell (2013)

Outcome data:	Yield from 1960-2004 from FAOStat.
Climate data:	Keeling CO_2 concentrations and country average P/PET.
Methodology:	Process model that develops the response of different crops to carbon dioxide concentrations and growing season P/PET from empirical studies. This is then used to estimate the changes to historical yields under a 100ppm increase in CO_2.
Result:	McGrath and Lobell, 2013, p. 5, fig. 4 (obtained US result from authors).
Impact function:	We contacted the authors and received estimates of the CO_2 fertilization relationship with yields of different crops on January 17$^{\text{th}}$, 2014, specifically for the US. Data were for 8 different crop types. We used an average of all types for cotton estimates.

Crime

Jacob, Lefgren, and Moretti (2007)

Outcome data:	FBI National Incident Based Reporting System
Climate data:	Weekly temperature and precipitation from the NCDC GHCN-Daily database.
Sample period:	1995-2001
Sample unit:	Jurisdiction-weeks

Methodology:	Linear response of log(crime_rate) to average temperature and precipitation, controlling for jurisdiction-by-year and month fixed effects, as well as jurisdiction-specific 4^{th} order polynomials in day of year.
Result:	Modified version of Jacob et al., 2007, p. 508-509, table 2.
Impact function:	We obtained data and replication files from the authors and generated coefficients for a month-long exposure window, to account for displacement of crime, as noted in the text. The climate variables are at weekly resolution, and in order to make this comparable to Ranson (2014) we reran the analysis using maximum temperatures and then scaled the coefficients in Jacob et al. (2007). We did this by first dividing the coefficient for the monthly exposure by 7, to get a daily response, and further by 4 to account for the lagged climate variables. This resulted in the marginal effect on crime of a 1°F increase in daily temperature. Taking a reference point of zero response at a temperature of 65°F (to coincide with the central point of the reference bin of Ranson (2014)) we derived a linear response of violent crimes and property crimes to temperature and precipitation.

Ranson (2014)

Outcome data:	FBI Universal Crime Reporting Data.
Climate data:	Daily temperature and precipitation from the NCDC GHCN-Daily database.
Sample period:	1960-2009
Sample unit:	County-months
Methodology:	Non-linear response of log(crime_rate) to maximum temperature and precipitation, controlling for county-by-year and state-by-month fixed effects. Temperature is transformed into number of days within 10°F bins, with the 60-69°F bin as a reference point.
Result:	Ranson, 2014, p. 9, fig. 4
Impact function:	We contacted the author and received updated estimates of the percentage change for each of 8 different classes of crimes on March 12^{th}, 2014. To derive response functions, we grouped these into violent crimes (murder, rape, aggravated assault, and simple assault) and property crimes (robbery, burglary, larceny, and vehicle theft), and combined results within each class of crimes.

Health

Deschenes and Greenstone (2011)

Outcome data:	National Center for Health Statistics Compressed Mortality Files.
Climate data:	Daily temperature and precipitation from NCDC
Sample period:	1968-2002
Sample unit:	County-years
Methodology:	Non-linear response of mortality to temperature, controlling for county-by-age-group and state-by-year-by-age-group fixed effects. Temperature is transformed into number of days in an year-long window within 10°F bins, with the 50-59°F bin as a reference point.
Result:	Modifed version of Deschênes and Greenstone, 2011, p. 9, fig. 2
Impact function:	We contacted the authors and received estimates on November 5$^{\text{th}}$, 2013. To make the study comparable to Barreca et al. (2013), the main analysis was rerun with log(mortality) as an outcome.

Barreca, Clay, Deschenes, Greenstone, and Shapiro (2013)

Outcome data:	Mortality from the Mortality Statistics of the US (pre-1959) and the Multiple Cause of Death files (post-1959).
Climate data:	Daily temperature and precipitation from the NCDC GHCN-Daily database.
Sample period:	1929-2004
Sample unit:	State-months
Methodology:	Non-linear response of log(mortality) to temperature, controlling for state-by-month and year-month fixed effects, and state-by-month-specific quadratic time trends. Temperature is transformed into number of days in a two-month window within 10°F bins, with the 60-69°F bin as a reference point.
Result:	Modified version of Barreca et al., 2013, p. 37, table 3, panel B

Impact function:	We contacted the authors and received estimates on 5th November, 2013. The preferred specification, to account for forward displacement, was to use monthly mortality with a 2-month exposure window to temperature. We used the estimated response from 1960-2004. To make this response comparable to the response of Deschenes and Greenstone (2011), the analysis was rerun with the reference point changed to the 50-59°F bin. To scale the coefficients, we divided each coefficient value by a factor of six. We also obtained age-specific response functions for ages 0-1, 1-44, 45-64, and 65+.

Labor

Graff Zivin and Neidell (2014)

Outcome data:	Hours worked from the American Time Use Survey.
Climate data:	Daily temperature, precipitation, and humidity from NCDC.
Sample period:	2003-2006
Sample unit:	Person-days
Methodology:	Seemingly-unrelated regression allowing for correlated errors between time spent working, or indoor and outdoor leisure. Non-linear response to maximum temperatures controlling for county, year-by-month, and day of week fixed effects, as well as individual level controls. Temperature is transformed into number of days within 5°F bins, with the 76-80°F bin as a reference point.
Result:	High-risk: Graff Zivin and Neidell, 2014, p. 15, fig. 3; Low-risk: Graff Zivin and Neidell, 2014, p. 16, fig. 4
Impact function:	We contacted the authors prior to publication and received full estimates for high-risk and low-risk labor responses to temperature on December 18th, 2013.

5.B Supplementary Tables and Figures

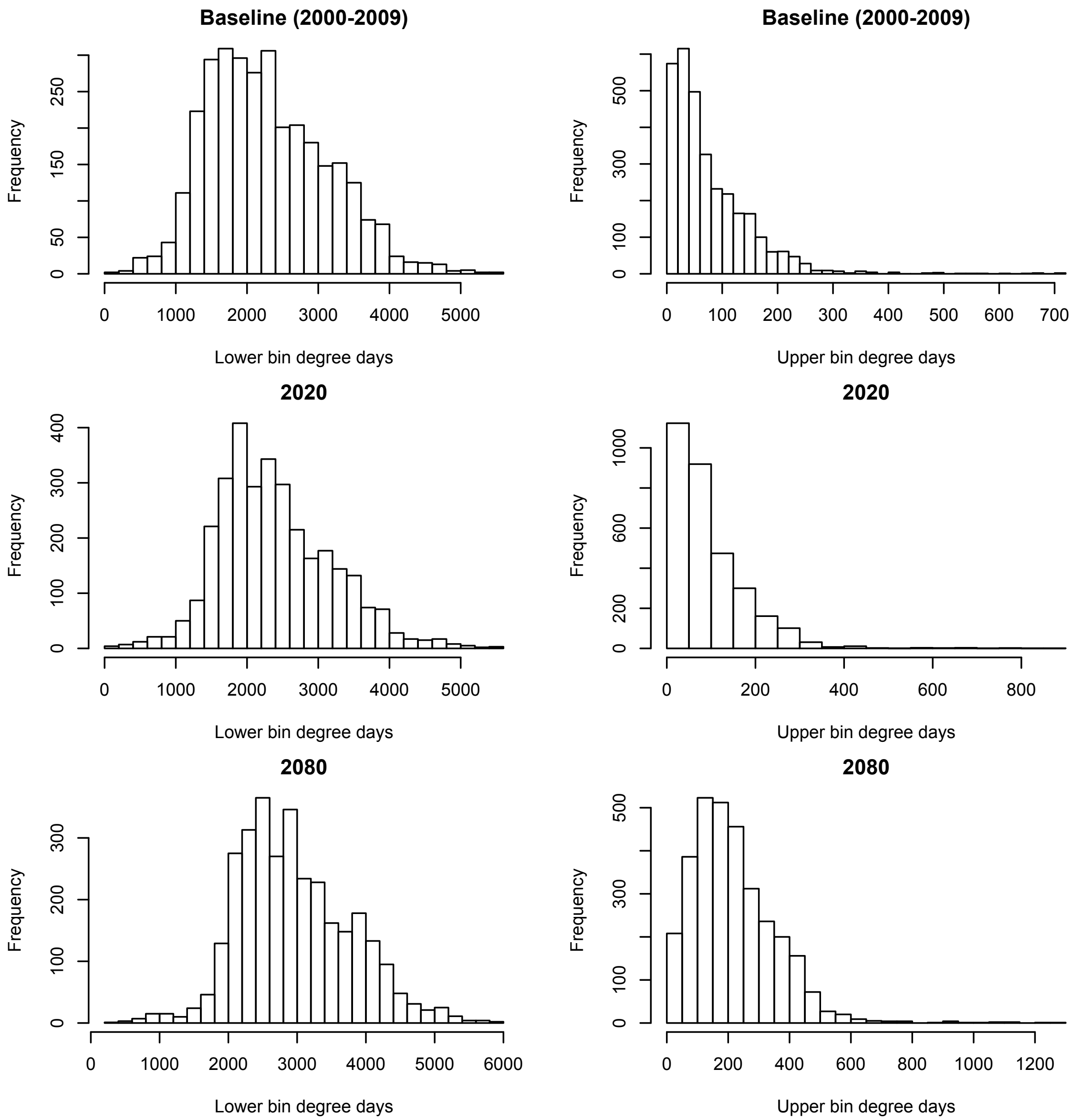


Figure 5.14: Distributions of the degree days across all US counties in the lower (left) and upper (right) bins of Maize, which has a 29°C bin threshold. The rows represent the average of degree days between 2000 and 2009, and for the years 2020 and 2080.

Table 5.10: Unweighted impact result percentiles

	RCP 8.5					RCP 4.5					RCP 2.6				
Percentiles	5	17	50	83	95	5	17	50	83	95	5	17	50	83	95
Agricultural Yields															
2080-2099	-42.44	-34.14	-16.49	0.42	13.26	-26.74	-19.37	-4.26	5.09	9.54	-16.64	-12.1	-2.99	1.34	4.57
2040-2059	-18.27	-11.31	-3.96	4.69	8.61	-14.11	-10.76	-1.59	4.83	8.28	-12.9	-9.58	-3.43	1.3	4.28
2020-2039	-9.55	-6.16	-1.92	2.58	7.98	-9.32	-6.86	-1.16	3.71	9.27	-13.61	-8.33	-2.11	1.32	2.77
Agricultural Yields (w/o CO_2fertilization)															
2080-2099	-59.76	-54.04	-40.86	-28.97	-19.24	-35.23	-28.84	-15.04	-6.71	-2.91	-19.12	-14.76	-5.99	-1.84	1.33
2040-2059	-27.49	-21.36	-14.89	-7.26	-3.91	-20.7	-17.56	-8.94	-3.01	0.15	-16.72	-13.49	-7.59	-3.09	-0.29
2020-2039	-13.59	-10.39	-6.36	-2.07	3.09	-12.45	-10.05	-4.54	0.12	5.44	-16.29	-11.1	-5.11	-1.72	-0.42
High Risk Labor															
2080-2099	-2.53	-2.06	-1.44	-0.98	-0.76	-1.18	-0.89	-0.59	-0.3	-0.13	-0.54	-0.42	-0.28	-0.11	0.18
2040-2059	-1.03	-0.77	-0.49	-0.26	-0.15	-0.74	-0.63	-0.38	-0.17	-0.02	-0.47	-0.4	-0.31	-0.13	0.15
2020-2039	-0.5	-0.38	-0.16	-0.02	0.11	-0.44	-0.37	-0.2	-0.03	0.12	-0.44	-0.33	-0.21	-0.03	0.15
Low Risk Labor															
2080-2099	-0.68	-0.46	-0.24	-0.1	0	-0.24	-0.16	-0.08	-0.03	0	-0.12	-0.08	-0.04	0.01	0.05
2040-2059	-0.22	-0.15	-0.08	-0.02	0.02	-0.15	-0.1	-0.06	-0.01	0.03	-0.11	-0.07	-0.04	0.01	0.05
2020-2039	-0.11	-0.07	-0.03	0.01	0.05	-0.1	-0.06	-0.03	0.01	0.05	-0.1	-0.06	-0.02	0.02	0.06
Mortality															
2080-2099	0.75	3.85	9.63	18.04	24.86	-2.53	-2.06	-1.44	-0.98	-0.76	-0.68	-0.46	-0.24	-0.1	0
2040-2059	-3.39	-1.27	2.48	5.89	8.75	-1.03	-0.77	-0.49	-0.26	-0.15	-0.22	-0.15	-0.08	-0.02	0.02
2020-2039	-3.82	-1.83	1.15	4.27	6.13	-0.5	-0.38	-0.16	-0.02	0.11	-0.11	-0.07	-0.03	0.01	0.05
Property Crime															
2080-2099	0.34	0.51	0.74	0.97	1.07	-0.03	0.14	0.51	0.83	1.01	-0.18	-0.01	0.23	0.46	0.61
2040-2059	-0.01	0.13	0.33	0.59	0.73	-0.13	0.03	0.36	0.67	0.85	-0.14	-0.03	0.22	0.44	0.59
2020-2039	-0.29	-0.07	0.1	0.31	0.46	-0.25	-0.09	0.19	0.48	0.63	-0.2	-0.06	0.11	0.32	0.42
Violent Crime															
2080-2099	1.84	2.2	3.05	3.76	4.41	0.21	0.74	1.54	2.31	2.54	-0.27	0.05	0.69	1.27	1.48
2040-2059	0.34	0.7	1.19	1.75	2.41	-0.02	0.42	1.09	1.65	1.84	-0.25	0.26	0.79	1.02	1.52
2020-2039	-0.29	0.04	0.45	0.9	1.34	-0.47	0.07	0.56	0.97	1.3	-0.25	0.01	0.48	0.81	1.06

Chapter 6

Conclusions and Future Research Directions

6.1 Lessons learned

One of the main outcomes of my education in Sustainable Development has been the accumulation of a number of approaches that I will follow in future research on environmental impacts. I collect a few of the most important here.

First, when considering the interaction of the environment and society, understanding and utilization of physical representations of te world is of extreme importance. Many physical phenomena interact with society in surprising ways, and using society's own measurement of that phenomenon may be subjecting ourselves to a biased view. Essentially, we can not use outcomes upon humans to predict outcomes. This is the case in a lot of research on natural disasters, where self-reported data is often used as a measure of physical exposure. Instead, we should model the physical exposure itself and look at the outcomes as a function of that.

Second, and related, is that simulated data can serve as a good proxy for observations, as long as it is used carefully. In many places around the world, we lack observational records of environmental variables. This can lead to a focus on estimating relationships in those places where the observations exist only. However, many places without good records are precisely the places that require the most research. When this is the case, data from models and simulations can and should suffice in estimating impacts.

Thirdly, measurement matters. Many of the effects shown here are ones that have not been quantified before. This is a reminder that we should not be complacent in our current understanding of environmental impacts. The act of demonstrating them is a step towards making policy to mitigate the effects.

6.2 Directions for future research

The work in this dissertation combines analytical economics methods with those from earth science - particularly climate physics, remote sensing, and ecology - to understand how the environment affects the structure of society in the long-term. A crucial component of crafting appropriate development policies is quantifying the role of geography, natural endowments, and the environment upon developing country societies. Yet information on the direct and indirect impacts from exposure to many biophysical phenomena remains limited. In my future work, I aim to measure the effect of extreme ecological and climate conditions upon populations, investigate how these populations adapt, and understand how they can manage these environments effectively.

The intersection of social and natural sciences is particularly important as it allows a deeper understanding of the core environmental processes that can be key determinants of well-being. The interaction between these two complex systems is understudied because a simultaneous understanding of both is essential but often lacking. My work will focus on conditions that are commonly and routinely faced by populations, in order to understand not just the effect of a single shock, but how the opportunities for growth may be persistently hindered in some environments. The chapters of this dissertation all aim to answer this question in part.

Engaging with policy is of critical importance for my work. For example, I am continuing my relationship with the Red Cross Society by conducting a quasi-randomized evaluation of a disaster early warning system currently being installed in Uganda (with Julie Arrighi, American Red Cross, in progress). In this way, I will engage with humanitarian policy makers and inform policy with rigorous research. Another major strand will be in the area of "quantitative human rights", which will be guided by a normative stance adopted from the human rights community, but will look at outcomes through an environmental and development economics lens. For example, I am looking at the environmental drivers of selection in sex work (and human trafficking) in India and Vietnam (with Jesse Anttila-Hughes, in progress). Such negative social outcomes can effectively act as an adaptation to climate variability and change, though few would argue that this strategy is desirable. This negative outcome, if not quantified, may look like a decrease in environmental impacts upon a population. I hope that these two cases can result in a more informed and nuanced understanding of the complexities of adaptation.

My future work will fall into the two categories of impacts and adaptation. In the former, I plan to examine the potential damages from sea level rise using an empirical specification based upon historical sea level fluctuations, as well as look at the persistent effects of groundwater pollution resulting from agriculture. In the latter, I aim to develop a coherent theory of adaptation to climate change, as well as develop ways to measure it empirically. The starting point for this is in my work on tropical cyclones and floods, where I show that the most highly exposed populations are still impacted by hazards. This suggests that adaptation is costly, in contrast to the widely held belief that adaptation may be relatively cheap and effective at limiting climate change impacts. Furthermore, adaptation may be either positive or negative exposed populations may make lower yielding investments to avoid climate impacts, effectively disinvesting in productive, but risky, assets and capital. Aside from examples from my own work, I will demonstrate this theory using further empirical examples from health (Barreca et al., 2013), crime (Ranson, 2014), and labor productivity (Graff Zivin and Neidell, 2014). In this way, I hope that my work not only increases understanding of the costs of environmental degradation, but also, by rigorously assessing the capabilities of and limits to adaptation, informs future policies that can manage and remove the hindrances to development brought about by extreme environments.

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