Natural Disasters and Financial Markets

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ABSTRACT

Natural disasters and financial markets

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The first essay (chapter 2) examines the impact of major U.S. natural disasters on the stock returns and volatilities of firms based in disaster areas. We find that a small proportion of catastrophes (between six and eight percent) have a significant impact on returns, after controlling for false discoveries. The meaningful shocks are distributed over a relatively long period of time with the uttermost effects being felt in the two or three months following disasters. Furthermore, we observe that the second moments of returns of affected firms more than double when hurricanes, floods, winter storms and episodes of extreme temperature occur.

The second essay (chapter 3) studies the effect of major floods on new municipal bond issues marketed by U.S. counties. The results show that bonds sold in the midst of floods exhibit yields about seven percent higher than bonds sold at other times, which is a net loss of almost $100,000 in terms of proceeds on a $10 million debt issue. Consistent with a behavioral explanation based on the availability bias, the abnormal yields rapidly decay over time and are limited to first-time disaster counties. The evidence for an increase in credit risk is mixed and the results do not support lower market liquidity stories. Selection bias, underpricing activities and issuance costs are examined and are unlikely to materially affect the conclusions.

The final essay (chapter 4) focuses on the consequences of disasters on investor risk preferences. We infer the impact of major catastrophes on the risk-taking behavior of investors from a database of U.S. municipal bond transactions. As the effect of disasters is mostly regional, we exploit the geographic segmentation of the municipal bond market to estimate a measure of regional risk aversion using a conventional consumption capital asset pricing model. The findings strongly support the assumption that natural disasters cause a statistically and economically significant increase in financial risk aversion at the local level.

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To my wife and children

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CHAPTER 1
Introduction

Floods, hurricanes and other extreme weather events have a far-reaching impact on the population and the economy of disaster areas. Indeed, Baas, Trujillo & Lombardi (2015) estimate that natural disasters caused more than US$1.5 trillion in damage and 1.1 million deaths worldwide between 2003 and 2013. Previous research almost unanimously recognizes the adverse effects of extreme weather events on human health (e.g. McMichael, Woodruff & Hales, 2006; Noji, 1996) and a rapidly growing, yet often conflicting, literature relates disasters to lower economic growth.[1] While many papers study the impact of natural disasters on financial markets, that part of the literature is highly concentrated on the insurance and the housing markets. Accordingly, the consequences of extreme weather events on several important security markets remain largely unexplored. This thesis provides additional insights on this issue through an empirical examination of the short- to medium-term effects of natural disasters on the return and volatility of the stock of U.S. firms located near disaster areas (chapter 2), on the costs of issuing municipal bonds for flooded U.S. counties (chapter 3), and on disaster-induced changes in the risk-taking behaviour of local U.S. investors affected by a catastrophe (chapter 4).

We believe that this thesis is of interest for a large audience that goes beyond the borders of the Finance academic community. Actually, the relevance of this thesis' research agenda is coalesced with the subject of future climate change. There is a rising acceptance about the possibility that global warming will increase the frequency and/or intensity of extreme weather events (Douglas, Garvin, Lawson, Richards, Tippett & White, 2010; Francis & Vavrus, 2012; Kazmierczak & Bichard, 2010; Rahmstorf & Coumou, 2011; Thorne, Evans & Penning-Rowsell, 2007). Hence, an adequate understanding of the multi-dimensional impacts of natural disasters on the economy is of primary importance, not only for investors and portfolio managers who trade on financial markets, but also for academicians and practitioners from the fields of public economics, regional science and risk management.

[1] Many papers report a decline in economic growth following natural disasters (Hochrainer, 2009; Noy & Nualsri, 2011; Raddatz, 2009; Strobl, 2011). Yet, other studies present evidence of disasters having a neutral or positive effect on productivity (Baker & Bloom, 2013; Leiter, Oberhofer, & Raschky, 2009; Skidmore & Toya, 2002).

Note from the start that the three essays in the thesis all employ an event-study approach to assess whether or not extreme weather events have an economically and statistically significant impact on various metrics related to financial markets. Interestingly, our focus on natural disasters alleviates *de facto* most of the endogeneity concerns that plague many empirical studies in Finance (Gippel, Smith & Zhu, 2015) given that weather events are obviously uncorrelated with national and regional economic conditions and give a clear picture of the causality relation.

The first essay (chapter 2) studies the return and volatility of the stock of U.S. firms following large disasters. While previous papers mostly investigates whether or not disasters have a systematic impact on national stock markets (Worthington & Valadkhani, 2005; Worthington, 2008; Adelino, Cunha & Ferreira, 2017; Wang & Kutan, 2013), we take into account the fact that the impact of disasters is primarily local (Strobl, 2011; C. T. West & Lenze, 1994) and match firms with disasters on a state basis. We employ an ARMA-EGARCH model to assess abnormal returns and make sure to control for false discoveries (Barras, Scaillet & Wermers, 2010). Our firm-level approach allows us to distinguish firms that benefit from firms that suffer from disasters. Among other things, we acknowledge the long-lasting nature of some disasters and the fact that some time is needed for investors to obtain information about the consequences of a disaster. Accordingly, we study the effect of disasters using event windows of varying lengths.

The findings of chapter 2 reveal that around six percent of the firms are significantly affected by disasters. The results also help explain why many previous studies fail to observe any impact of disasters on the aggregate stock market. First, the impact of disasters on stocks is not felt immediately but rather in a two- to three-month period following the peak of the disasters. Thus, an event window larger than the usual one- to five-day period is required. Second, we observe almost as many positive as negative abnormal returns following disasters. These abnormal returns offset one another in the aggregate stock market. Third, we find evidence supportive of abnormal returns being more common for firms located near disaster areas. This suggests that future research on disasters should consider the distance to the disaster areas.

Chapter 2 also examines the effect of disasters on the volatility of stock returns using the volatility event-study approach of Białkowski, Gottschalk & Wisniewski (2008). While most major storms cause no significant shift in the variance, we observe that longer-lasting events such as floods and episodes of extreme temperature are associated with an important increase in variance. Our findings also suggest that hurricanes are somewhat special as they are the only type of disaster to produce a market-wide increase in volatility.

The second essay (chapter 3) investigates the relationship between major flood episodes and the cost of financing for U.S. counties that issue new municipal bonds. Natural disasters plausibly affect issuer creditworthiness through damage to properties (lower tax base), damage to infrastructures (investments needed to repair/rebuild) and civil protection and clean-up operations (unforeseen expenses). Natural disasters may also affect the trading of municipal bonds through a higher prevalence of financial distress, lower household income or a flood-induced variation in the way investors perceive the risk associated with natural disasters. Previous literature suggests that the geographic segmentation of the municipal bond markets (Cook, 1982; Feroz & Wilson, 1992; Greer & Denison, 2014; Pirinsky & Wang, 2011) magnifies the effect of local risks (Babina, Jotikasthira, Lundblad & Ramadorai, 2015) and presents evidence supportive of natural disasters impacting bond prices (Fowles, Liu, & Mamaril, 2009). We explore how major flood events and ex-ante measures of flood risk affect the issue yield of new municipal bonds sold by counties using a linear mixed-model methodology.

We obtain strong evidence supporting an increase in financing costs for bonds sold in the year following major flood episodes. However, we observe no significant changes in issue yields when the issuer is associated with a high (ex-ante) risk of flooding. We supplement our main results with an analysis of competing stories that describe several channels through which disasters plausibly affect financial markets. The first explanatory channel is related to the marketability of municipal bonds. We study whether or not disasters cause a shift in the average gross underwriting spreads or alter the level of underpricing activities. The results do not support the marketability explanation. Next, we verify how floods affect the propensity to issue new municipal bonds. To this end, we implement a logistic regression motivated by Heckman (1979) but find no evidence of a flood-induced impact on the likelihood that counties issue municipal bonds in various post-flood periods. Third, we test the assumption that floods increase the risk level of municipal issuers using several proxies for credit risk. We obtain mixed results in that we observe higher debt ratios, lower growth in the number of housing units and smaller budgetary surpluses for disaster counties in the years following major floods. Yet, we observe no impact in terms of credit rating, no changes in the characteristics of bonds issued in post-flood periods and no significant variations in property tax revenue. Also, we find no evidence of higher liquidity risk using transactions costs from the secondary market as an indicator of liquidity risk. The last explanatory channel consists of a behavioral response to floods following the intuition of the availability bias story of Tversky & Kahneman (1973) or of the myopia and amnesia theory of Pryce, Chen & Galster (2011). The aforementioned papers predict that the impact of a flood event is temporary and that the effect of a disaster is larger for first-time troubled issuers than for issuers with a history of earlier major flood events. Our results are consistent with both of the empirical predictions of the behavioral channel.

In short, the analysis presented in chapter 3 unveils a significant increase in financing costs for flooded municipalities. While credit risk is obviously one plausible channel through which disasters trigger a surge in investors' required returns, the results are particularly supportive of a behavioral explanation. Flood episodes heighten the fear of subsequent flooding.

The third essay of this thesis (chapter 4) assesses whether or not natural disasters are a significant determinant of the variation in aggregate investors' risk aversion over time. This research comes in the wake of a number of recent studies relating disasters to risk preferences[2] that support the hypothesis that dreadful experiences, such as enduring a natural disaster, reduce risk taking. While we remain agnostic as to why disasters increase risk aversion, previous literature mostly advocates psychological or emotional explanations. Among others, McMichael, Woodruff & Hales (2006) affirm that natural disasters often stress populations beyond their adaptation limits and Guiso, Sapienza & Zingales (2017) show that fear causes an increase in risk aversion. Previous studies often rely on survey and experiments to infer changes in risk aversions. We innovate by analysing whether or not changes in real asset prices are consistent with a disaster-induced increase in risk aversion. Thus, our approach departs from studies that estimate risk aversion at the individual level and allows us to detect temporary shifts in risk aversion for the representative investor.

We exploit the conventional power-utility consumption capital asset pricing model (Breeden, 1979; Lucas, 1978) to infer relative risk aversion (RRA) coefficients. We fit the model at the regional level employing local electricity consumption as the measure of aggregate consumption and state-based portfolios of municipal bonds as test assets. We rationalize our use of local municipal bonds in this context following the municipal bond market segmentation hypothesis and, in particular, Ang, Bhansali & Xing (2010) and Elmer (2014) who argue that local retail investors dominate the trading of municipal bonds.

[2] Examples include: Bernile, Bhagwat & Rau, 2017; Bucciol & Zarri, 2013; Cameron & Shah, 2015; Cassar, Healy & von Kessler, 2017; Goebel, Krekel, Tiefenbach & Ziebarth, 2015; Petrolia, Landry & Coble, 2013; Stewart, Ellingwood & Mueller, 2011 and Van den Berg, Fort & Burger, 2009.

The results reported in chapter 4 support the disaster-induced increase in risk aversion assumption. We obtain RRA coefficients that vary between 0 and 10 over states with a mean at 4.22. Such a magnitude is economically plausible and consistent with previous studies. Furthermore, the state ranking of the coefficients match our expectations associated with the dispersion of the prevalence of insurance across states, the impact of cultural traits such as religious beliefs on risk-taking (Kumar, Page & Spalt, 2011) and the historical amount of damage caused by large natural disasters. The findings are robust to alternative modelling strategies, to other specifications of the utility function and are not explained by variations in national or regional economic conditions.

Taken together, the three essays of this thesis contribute to the literature on the economic consequences of natural disasters by providing additional empirical evidence of the statistically and economically significant impacts of disasters on financial markets. Given that several findings appear to be associated with a behavioral explanation, the results suggest that a better knowledge about the likelihood and expected severity of natural disasters may help reduce the impact of extreme weather events on financial markets.

CHAPTER 2

Natural Disasters and the Stock Returns of Local Firms

2.1 INTRODUCTION

An increasing number of studies contend that the frequency and intensity of extreme weather events are rising as a consequence of global warming and climate change (Francis & Vavrus, 2012; Rahmstorf & Coumou, 2011). The consequences of weather-related disasters are already far-reaching with average annual property and agricultural losses amounting to more than 18 billion dollars over the last 25 years, according to the National Centers for Environmental Information (NCEI)'s storm events database.

Yet, the recent literature does not agree on the impact of extreme weather events on the economy of developed countries. Although Nakamura, Steinsson, Barro & Ursúa (2013) predict a significant decline in personal consumption following catastrophes, Mechler (2009) provides evidence that this negative effect is restricted to developing countries. Many papers report a decline in economic growth (Hochrainer, 2009; Noy & Nualsri, 2011; Raddatz, 2009; Strobl, 2011) and almost an equal number of studies observe extreme weather events having a neutral or positive effect on productivity (Baker & Bloom, 2013; Bernile, Delikouras, Korniotis & Kumar, 2017; Leiter, Oberhofer & Raschky, 2009; Skidmore & Toya, 2002). Papers examining the impacts of extreme weather events on financial markets also present some disagreement. On the one hand, Worthington (2008) observes no significant impact from disasters on the Australian stock market, Luo (2012) finds 'surprisingly' small and insignificant effects on six distinct national stock market indices, Asongu (2013) finds no evidence of spill-over in international foreign exchange markets and Wang & Kutan (2013) report no changes in the returns of American and Japanese stock indexes following catastrophes. On the other hand, Worthington & Valadkhani (2005) observe significant abnormal returns on the Australian stock market. These divergent findings emphasise the need for additional research on the economic impacts of natural disasters.

The primary goal of this paper is to examine the effect of severe weather events in the U.S. on the domestic stock market. While most of the aforementioned papers study the effect of catastrophes at the national level and restrain the event window to a very short period of one to five days, we examine local firms (i.e., firms located in the disaster state), and use various event window lengths ranging from one day to half a year. The focus on local firms is motivated in part by West & Lenze (1994) who argue that natural disasters are primarily regional in terms of their consequences, by Leiter, Oberhofer & Raschky (2009) who find that companies in regions hit by a catastrophe show higher asset growth than firms in unaffected regions, and by Strobl (2011) who observes that the impact of disasters on economic growth is both statistically and economically significant at the regional level but completely diversified away at the national level.

Employing a longer period to assess the effect of catastrophes also appears necessary as several considerations could lead to a delayed impact on stock returns. First, it often takes much more than a few days to obtain precise information on losses and other disaster-related consequences. In this regard, Downton & Pielke (2005) investigate the accuracy of disaster loss data and find that estimation errors are especially large in early damage evaluations but positive and negative errors tend to average out. Thus the market may have to wait before obtaining a clear signal of the effects of natural disasters, particularly for individual firms. Second, many natural hazards are relatively long-lived. Some floods and droughts in our sample last several months and restricting the event period to a few days following the beginning of an episode of severe weather would underestimate the consequences of such disasters. Third, investors may acknowledge that plausible short-term production interruptions can be compensated by a rise in production efficiency in the longer term (e.g. Leiter, Oberhofer & Raschky, 2009). It may take some time to observe which of these two offsetting effects is most important. Last, business disruptions can come from indirect channels such as supply-chain breakdowns rather than from direct disaster-related damages. The effect of these indirect channels may require a longer period to materialize because of inventories and short-term risk management practices (e.g. Norrman & Jansson, 2004).

Our methodology rests on an ARMAX-GARCH model which combines most of the advantages of both the intervention analysis (ARMAX) approach used by Worthington & Valadkhani (2005) and the GARCH-based processes employed by Worthington (2008) and Wang & Kutan (2013). Furthermore, we follow the method of Barras, Scaillet & Wermers (2010) to control for false discoveries.

A second objective of the paper is to assess whether or not natural disasters increase the volatility of stock returns. Wang & Kutan (2013) is one of the few studies to address this issue. Employing a GARCH dummy variable methodology, they provide evidence that disasters increase volatility on the U.S. market but have no impact on the Japanese market. They offer no explanation for the opposing conclusions. We re-examine the effect on volatility by once again focusing on the stocks of local firms instead of on the whole market. We employ the GARCH volatility event study approach of Białkowski, Gottschalk & Wisniewski (2008) and measure the significance of the changes in conditional variance with the semi-parametric sequential bootstrap technique developed by Essaddam & Mnasri (2015).

Our findings indicate that the stock returns are unaffected by extreme weather events over very short periods of one to five days, after controlling for false discoveries. However, when a two-to-three month event period is used, we show that a small proportion (around 6% or 7%) of the disasters has meaningful impacts on stock returns. Expanding the event window for more than a three months period results in a steady decrease in the proportion of significant disasters. Furthermore, the stocks of local firms react more strongly to natural catastrophes than that of firms located in nearby states. Overall, conclusions remain unchanged whether or not disasters are grouped by categories and firms are sorted into state-level portfolios. Periods of high market volatility such as the global financial crisis of 2008-2009 appear to exacerbate the impact of disasters on stock returns. As in previous papers, we obtain mixed results for the direction of the impact of catastrophes. Our sample is almost equally split between firms experiencing a positive effect and firms facing a negative effect. Firm sizes, industries, proximity to the worst affected area, media attention and disaster-related losses do not succeed in explaining either the strength or the direction of the abnormal returns.

The empirical results from our volatility event study provide evidence that the second moments of returns of the average local firm immediately increases when hurricanes, floods, severe winter weather or episodes of extreme temperature occur. Yet, tornadoes, hails, thunderstorms and other storm-like events have a neutral effect on volatility.

The rest of the paper is structured as follows: Section 2.2 describes the data. Section 2.3 defines the event-study methodology. Section 2.4 presents and discusses the main results for the impacts of natural disasters on returns and various robustness checks. Section 2.5 outlines the volatility event study approach and reports and interprets the findings of this investigation. Section 2.6 concludes.

2.2 DATA AND SUMMARY STATISTICS

2.2.1 Data on natural disasters

We rely on the information contained in two distinct disaster databases to identify natural disasters. They are the Federal Emergency Management Agency (FEMA)'s major disasters database and the National Centers for Environmental Information (NCEI)'s storm events dataset. FEMA's database reports all federally declared disasters since 1953. It identifies the start and end dates of each disaster and the counties where damage was serious enough to necessitate federal aid but does not directly provide damage estimates. For the January 1990 to July 2007 period, we infer the severity of several disasters from various sources including government agency reports, Congressional Research Service (CRS) reports and FEMA's Public Assistance program. Starting in August 2007, FEMA releases preliminary damage assessment reports that indicate the dollar impact of a disaster at the state and county levels. The NCEI's storm events database contains information on 48 types of natural disasters with detailed technical definitions.[3] The NCEI database provides estimates of property and crops damage (in actual dollar amounts) for most events. These estimates may come from insurance companies, from other qualified individuals or from 'guesstimations'. We aggregate NCEI records from 1990 to 2014 by date and state to obtain a list of statewide disasters. When a weather system brings havoc in several states, that system is regarded as one disaster per state. Our full sample contains a total of 1,092 disasters. However, many of these disasters may not be severe enough to affect the stock market at the state-level. Thus, we consider a disaster as being "major" when damage exceeds 25 USD per state resident (in constant 2014 USD).[4] Annual state population estimates come from the Census Bureau. After applying these deletion criterions, 393 events meet our major disaster threshold. The number drops to 247 when we consider disasters in states with enough available daily stock returns. Given that many disasters extend over several states, these numbers parallel the statistics from the NCEIs Billion-Dollar Weather and Climate Disasters (see Smith & Katz, 2013, for the dataset methodology) that identifies 151 billion dollar disasters between 1990 and 2014.

Event studies that examine the short-term impact of natural disasters face particular implementation challenges as catastrophes are heterogeneous events with imprecise start dates and variable event durations. We alleviate the impact of these issues using data on news reports

[3] Before 1996, the NCEI database only records tornadoes, thunderstorm wind and hail events

[4] We obtain quantitatively similar results using a loss threshold of $50 per capita.

from the Vanderbilt Television News Archive (VTNA), which records and compiles the U.S. television news broadcasts of the national networks since 1968. We manually collect information on news that is related to the 247 disasters in our sample. The date when a disaster receives the lengthiest coverage is deemed as the peak date of a disaster. Eisensee & Strömberg (2007) study how relief decisions following natural disasters are driven by news coverage of disasters and argue that news coverage signals that a disaster is highly noticeable to the American public and that the news broadcast in itself can increase the importance that people attach to the disaster. Thus, we interpret the disaster peak date as the moment when a disaster becomes the most salient to U.S. investors and utilize that day as the first event date. When the peak date occurs during a weekend or a market holiday, we simply use the next trading day as the event date.[5]

We analyse the effect of disasters on stock returns using two distinct approaches. Our first method rests on state-level portfolios where we group firms according to their geographic location. Following Worthington & Valadkhani (2005), we combine disasters into five mutually exclusive categories (storms, floods, extreme temperature, winter weather and fires) and require that at least three disasters of the same type take place in a state to consider that category for a state. It leaves 195 disasters distributed across 39 categories in 24 states for the analysis. Our second approach rests on pairing individual firms with individual disasters. We focus on the states that count ten disasters or more to alleviate the risk of finding spurious relationships due to disasters occurring at the same time as economic events. It leaves 11 states totaling 170 disasters in our analysis. Table 2.1 presents the number of major natural disasters by type and by state between 1990 and 2014. We observe that storms are the most frequent type of disaster with 143 occurrences or more than half of the events. Accordingly, 20 out of the 39 retained disaster categories under our first approach involve storms. Nine states totaling 33 events experienced enough floods to be included in our study. We also consider 17 occurrences of extreme temperature that span five states, nine severe winter weather events across three states and three fires. We believe that our results are not materially affected by the reporting of only disasters due to Tornados, Thunderstorms and Hail by the NCEI prior to 1996. Less than 15% of our final sample of disasters and less than 2% of the individual firm-disaster pairs are from the 1990-1995 period.

[5]The peak date matches the start date of the NCEI database for 162 disasters in our sample. Most differences come from long-lasting disasters such as floods and episodes of extreme temperatures.

[Insert Table 2.1 around here]

2.2.2 Data on stock returns

We start by identifying common stocks that traded in the U.S. over the January 1990 to June 2015 period and obtain daily total returns from CRSP. We determine firm geographic position according to the location of their headquarters as is frequently done in the local bias literature (Korniotis & Kumar, 2013). Historical and actual information on headquarter locations are from Bloomberg. In our first approach based on state-level portfolios, we calculate equally weighted daily portfolio returns by grouping all firms that have headquarters in a state. We require state portfolios to contain a minimum of five stocks each day to ensure a minimal level of diversification and keep states that include at least 15 years of daily returns between January 1990 and December 2014.[6]

Our second approach is based on pairing individual firms with individual disasters and we employ total daily returns from firms located in selected states. In order to properly model the price dynamics of individual stocks within an ARMAX-GARCH framework, we take special care in the processing of thinly traded stocks with periods of stale prices. We reject months of data when daily prices stay the same for more than 2 days in a row and months with more than four days with no price variation. We only retain firms that exhibit at least 15 years of continuous daily returns while having no domicile change. These restrictions shorten the number of admissible firms in the 11 selected states from 7,475 to 463 and result in the testing of 2,146 firm-disaster pairs. The exclusion process tilts our sample toward bigger and plausibly more geographically diversified companies. Consequently, it is likely that this set of results underestimate the full effect of natural disasters on local firms.

Table 2.2 presents the descriptive statistics for the state-based portfolios. Kansas' portfolio returns stand out from the others as they exhibit, by far, the highest positive skewness and the highest kurtosis. A Jarque-Bera normality test formally rejects the normality assumption for all states at any conventional significance level. We also test for the presence of autocorrelation using the portmanteau test of Ljung & Box (1978) and reject the null of no autocorrelations. This indicates that the returns are not independently distributed. Last, we perform the Lagrange multiplier test proposed by Engle (1982) to detect conditional

[6] Daily returns between January and June 2015 are only required to analyse the impact of disasters on the longer event periods of two, three and six months.

heteroscedasticity and always reject the null of no conditional heteroscedasticity. Thus, the returns and variances of our state-based portfolios appear to be time-varying. We do not report descriptive statistics for individual stock returns due to space considerations. Still, the vast majority of firm returns are right-skewed, leptokurtic, non-normally distributed and exhibit autocorrelation and conditional heteroscedasticity.

[Insert Table 2.2 around here]

2.3 EVENT-STUDY METHODOLOGY

Our event study methodology is inspired from Worthington & Valadkhani (2005) who use an intervention analysis framework and from Worthington (2008) and Wang & Kutan (2013) who employ a generalized autoregressive conditional heteroskedastic (GARCH) model. Intervention analysis is based on an autoregressive moving average (ARMA) model. The effects of natural disasters on returns are examined through the addition of dummy variables, termed intervention variables, to the ARMA structure (ARMAX). The advantage of the ARMAX model is that it can effectively describe several known characteristics of asset returns, including shocks arising from new market information, trends and mean-reversion. It allows us to take care of the autocorrelation in our return series. However ARMAX models assume that the volatility is constant over time and our returns exhibit conditional heteroscedasticity. Thus, we augment the classic intervention analysis framework with a GARCH model for the conditional variance. The following equations more specifically describe our analytic framework:

$$RET_{\mathrm{t}} = \alpha_0 + \sum_{\mathrm{i=1}}^{\mathrm{p}} \varphi_{\mathrm{i}}\, RET_{\mathrm{t-i}} + \sum_{\mathrm{j=1}}^{\mathrm{q}} \theta_{\mathrm{j}}\, \varepsilon_{\mathrm{t-i}} + \sum_{\mathrm{n=1}}^{\mathrm{N}} \beta_{\mathrm{n}}\, Dis_{\mathrm{n,t}} + \varepsilon_{\mathrm{t}} \qquad (2.1)$$

$$\varepsilon_{\mathrm{t}} = z_{\mathrm{t}}\sigma_{\mathrm{t}},\ z_{\mathrm{t}} \sim \mathcal{D}(0,1) \qquad (2.2)$$

$$\ln(\sigma_{\mathrm{t}}^2) = a_0 + \eta \ln(\sigma_{\mathrm{t-1}}^2) + \psi \frac{\varepsilon_{\mathrm{t-1}}}{\sigma_{\mathrm{t-1}}} + \delta\left(\frac{|\varepsilon_{\mathrm{t-1}}|}{\sigma_{\mathrm{t-1}}} - \mathrm{E}\left[\frac{|\varepsilon_{\mathrm{t-1}}|}{\sigma_{\mathrm{t-1}}}\right]\right) \qquad (2.3)$$

Equation (2.1) defines the conditional mean. It is a typical intervention analysis model with p autoregressive terms and q moving-average terms. $\mathrm{B_n}$ is the main coefficient of interest that measures the impact of natural disasters of category n on the returns. $Dis_{n,t}$ equals one during the event period of a natural disaster of category n and zero otherwise. ε_t is the residual. Equation (2.2) specifies that the standardized residuals $z_t = \varepsilon_t/\sigma_t$ are independent and identically distributed (i.i.d.) with mean 0 and variance 1 and $\mathcal{D}$ is their probability density function (PDF). For simplicity, we restrain $\mathcal{D}$ to the normal or the Student-*t* distributions. Equation (2.3) describes the GARCH structure for the conditional variance σ_t^2 of the residuals. As in Wang & Kutan (2013), we use the exponential generalized autoregressive conditional heteroscedastic (EGARCH) model of Nelson (1991) to model the logarithm of the conditional variance. The EGARCH model imposes few restrictions on the parameters and its asymmetric structure accommodates the leverage effect observed in asset returns where a price drop increases volatility more than an equivalent price increase increases volatility. a_0 captures the unconditional variance, η is the coefficient associated with the GARCH term, ψ is the coefficient associated with the ARCH term, and δ measures the impact of the leverage effect. Based on a comparison of more than 300 models accommodating conditional heteroscedasticity, Hansen & Lunde (2005) show that the parsimonious GARCH(1,1) model performs no worse than more sophisticated models. Only the addition of an asymmetric term, such as the leverage term in the EGARCH, is shown to perform better than the simple GARCH(1,1) structure. Hence, it appears reasonable to retain the parsimonious EGARCH(1,1) model for our analysis. As in most financial applications, we expect the leverage coefficient to be negative as a negative shock tends to have a greater impact on volatility than a positive shock of similar magnitude.

We start by investigating which model specification best fits the return data and check whether the model's assumptions are respected. We proceed by using the ARMA-EGARCH model (without the intervention variables) and test for 25 combinations of p and q with the standardized residuals following either a normal or a Student-*t* distribution. We employ the Bayesian information criterion (BIC) to discriminate between the 50 specifications. Equations (2.1) to (2.3) are estimated jointly using the Maximum Likelihood method in each state and each of the 463 retained individual securities. Then we formally perform model diagnostic tests to validate whether or not the empirical characteristics of our model are in line with the model's assumptions. We follow Ruppert (2012) and validate that the standardized residuals $\hat{z}_t$ and the squared residuals $\hat{z}_t^2$ exhibit no serial correlation. Also, we test that $\mathcal{D}$ effectively follow the assumed Student-*t* or normal distribution.

2.4 EMPIRICAL RESULTS

2.4.1 Model specification

We begin our empirical exercise by describing the model specification that achieves the best fit in each state and selected individual firm. The coefficients of the retained ARMA-EGARCH specifications for the state portfolios are presented in Table 2.3. In the conditional mean equation, we observe that all but a few AR and MA coefficients are significant at the 0.05 significance level. The signs of the coefficients are consistent with persistence and volatility clustering in the conditional volatility of state returns. In addition the negative coefficients for the leverage term support the observed fact that bad news has a greater effect on risk than good news. In unreported results, we find that an ARMA(1,1)-EGARCH specification is superior to the other models for more than half of the individual firms. The ARCH, GARCH and leverage terms are almost always meaningful and have the expected sign for more than 99 percent of the selected firms. The Student-*t* distribution better approximates the standardized residuals than the normal distribution in all cases except one.

[Insert Table 2.3 around here]

The diagnostic checks reveal that our model adequately fits the data in most states and for a large proportion of the selected individual firms. We observe that two state-level portfolios (Georgia and Kansas) fail the serial correlation test on $\hat{z}_t^2$ and that the residuals in Alabama and California significantly deviate from the Student-*t* distribution at a 0.05 significance level but not at a 0.01 level. About 85 percent of the firms have no serial correlation on $\hat{z}_t^2$ and the residuals of the individual securities are adequately model by a Student-t distribution in 60 percent of the cases.

2.4.2 Main results

Our analysis of the impact of natural disasters on local firms is based on two distinct approaches. The first approach groups firms into state-level portfolios and groups disasters by categories. The results from that method are easier to disclose in totality and easier to compare with previous studies. Moreover, the use of portfolios instead of individual securities allows the model to better fit the return series. The second approach associates individual firms with individual disasters. It allows examining how firm and disaster specific characteristics relate with abnormal returns and provide a more precise measure of proximity to a disaster. In both cases we estimate the coefficients of the intervention variables of the ARMAX-EGARCH model by holding constant the values of the coefficients of the constants, AR, MA, GARCH, ARCH and leverage terms in order to obtain valid comparisons for the effect of disasters across several event window lengths.

Table 2.4 presents our main result using state-level portfolios. Panel A provides the estimated coefficients of the intervention variables when only one day, the peak date, is included in the event period. We see that one out of 39 categories of disasters, storms in Kansas, have a significant positive impact on the state-based returns at a 0.05 significance level. In Panel B, we report the estimated dummy coefficients when the event window includes a 40 trading day period beginning the day preceding the peak date. The addition of several weeks to the event period produces greatly different results. We now observe that storms are significant in three states out of 21, including Kansas. Floods are meaningful in three states out of nine and episodes of extreme temperature are significant at the 0.05 level in one state out of six. We do not report results for other event period lengths due to space considerations but the results for 5-day, 10-day and 20-day periods are similar to those in Panel A. Many of the meaningful disasters in panel B of Table 2.4 are still significant using a 60-day event period. However only two intervention variables, storms in Indiana and in Kansas, give clear indications of abnormal returns when the event period is extended to include 125 trading days (six months). Even when the impact is not statistically significant, we observe that the economic impact of catastrophes on stock prices can be substantial. The compounded abnormal daily returns of floods over 40 days vary between -1.23% in Kansas to 5.12% in Oklahoma. Episodes of extreme temperature in Louisiana and severe winter weather in Oklahoma have the most pronounced economic impact with compounded abnormal returns of more than 6% over a 40-day period while only remotely statistically significant. On the whole, our main results indicate that some natural disasters have an impact on the local stock market. However, the impact is spread on a two-to-three month period instead of condensed in the days following the peak of a disaster. Thus, our findings support a longer event period duration story. Furthermore, the direction of the effect is quite difficult to analyse as about half (19 out of 39) of the intervention variables yield a negative coefficient estimate while the other half show a positive sign.

[Insert Table 2.4 around here]

The analysis of the effect of natural hazards on the stock returns of individual firms yields similar results. We display the proportion of significant abnormal returns across the 2,146 firm-disaster pairs in Table 2.5. In panel A, we observe that the more pronounced effects of disasters on stock prices are observable using a 40-days event period and the abnormal returns slowly die away as we increase the event period. Again we observe about as many positive and negative coefficients for the intervention variables. In panel B we group firms-disasters pairs according to disaster loss estimates and notice that the most damaging disasters are associated with the highest proportion of significant abnormal returns. Almost 90 percent of the disasters in these pairs are large hurricanes. Still sorting pairs by disaster category reveals that floods, extreme temperature and winter weather, while in average are less damaging, also lead to abnormal returns that peak in a 40-day event period. Finally we control for the duration of the disasters in panel C. Longer-lasting disasters tend to generate more significant abnormal returns but the effect of disaster duration appears to be limited to the 40- to 80-days event periods.

[Insert Table 2.5 around here]

The event period duration story is one of the two distinctive features of our paper. The second consists in testing the impact of catastrophes on the stocks of local firms instead of on the aggregate stock market. Therefore, we verify whether or not the consequences of severe weather events are primarily regional. Using the state-level portfolios approach, we proceed by estimating the abnormal returns of the most significant disasters of Table 2.4 on the state-level portfolios of neighbouring states. We measure state proximity using the closest distance between states' borders.

Table 2.6 illustrates the impact of disasters on the nearest states' stock markets. We observe that disaster-related consequences differ between events. In about half of the cases reported in Table 2.6 (storms in Kansas, floods in Colorado and episodes of extreme temperature in Louisiana and Texas), the effect of disasters is limited to the local state. However, we cannot reject the possibility of spillover effects on neighbour states for the remaining events. For example, we notice that stocks in Kentucky, Georgia and Florida also experience significant positive abnormal returns in the wake of storms in Tennessee. The fact that many of the state-level major events are part of weather systems that also affect nearby states is one plausible explanation for such spillover effects. Hence, the findings shown in Table 2.6 mostly corroborate the regional consequences story and favor the use of local firms in assessing the impact of disasters on the stock market.

[Insert Table 2.6 around here]

We also use our sample of individual firm-disaster pairs to assess if the impacts of disasters on firms are restricted at a regional level. To this end we select eight hurricanes (Georges in 1998; Irene in 1999; Lili in 2002; Ivan in 2004; Dennis and Katrina in 2005; Ike in 2008; Isaac in 2012) and compare the effect that these events have on stock returns. 14.3 percent of the firms in the states most affected by the disasters show significant abnormal returns in the 40 trading days period following the hurricanes. The percentage drops to 7.5 for firms located in other states. This strengthens the argument favoring the regional-level effect of natural hazards.

Although the main objective of the paper is to examine the impact of extreme weather events on stock returns and return volatilities, we make a brief *aparté* to examine if the results on individual firms can help explain why some firms are prone to face a negative impact while others actually benefit from natural disasters. We analyse the relation between the estimated abnormal return of a disaster and various potential determinants such as a firm's industry, firm size, and the intensity of media attention between each pairing of a firm with its associated disaster.

We start by classifying firms according to their primary industrial activity using the 4-digit global industry classification standard (GICS). Whatever the industry and the length of the event window, we observe that disasters have a positive effect on firms almost as frequently as a negative effect. The few exceptions include the transportation industry that seems to face more negative abnormal returns and the real estate and telecommunication businesses where more firms experience a positive impact. But the small sample size in these three industries prevents us from drawing general conclusions, especially as we monitor substantial intra-sector volatility in the results. Thus, an industry story does not succeed in explaining why some firms gain while others lose. We then focus on other potential determinants and calculate rank correlation coefficients between market capitalization, media attention, disaster-related losses or distances to the worst affected areas and estimated abnormal returns (or *p*-values). In unreported results, we notice that for the most damaging disasters, lengthier national news coverage is linked with larger negative abnormal returns but has no effect on firms that experience a positive impact. For less damaging disasters, greater media attention diminishes positive AR but is almost unrelated to the AR of firms negatively affected by a disaster. The effect of media attention is most obvious in the shorter event periods of 1 and 5 days and disappears quickly after a 60-day window. Firm size is not related to the level or sign of abnormal returns at any event period length. Hence, we are at best only partially successful in explaining why some firms gain while others lose from natural disasters.[7] This ends our brief *aparté*.

2.4.3 Robustness tests

We validate our main findings by running several additional tests. First, we modify our definition of state-level returns and employ residuals from various factor models instead of unadjusted portfolio returns. Second, we control for the expected proportion of rejected null hypotheses that are incorrect rejections using the false discovery rate approach described by Barras, Scaillet & Wermers (2010). Last, we distinguish between disasters in the 2008-2009 period and disasters in other periods to examine the effect of the global financial crisis.

Our first series of robustness tests consists in the use of alternate characterizations for the dependant variable of the ARMAX-EGARCH model. We follow Korniotis & Kumar (2013) and generate series of state-specific returns that correspond to the components of the state-level portfolio returns and individual return series that are orthogonal to recognised risk factors. More exactly, we test the impact of natural disasters on the residuals of the following factor model:

$$RET_t - r_{f,t} = b_{0,s} + b_{RMRF}(RMRF_t) + \varepsilon_t \tag{2.4}$$

where RET_t is either the equally-weighted daily return of the state-level portfolio or the raw return of an individual firm at time t as described previously, $r_{f,t}$ is the risk-free rate proxied by the one month treasury bill rate, *RMRF* is the equally-weighted return of the US stock market in excess of the risk-free rate from CRSP, b_0 and b_{RMRF} are the parameters estimated on a state-by-state basis using an OLS regression and ε_t is the residual at time t that is employed as the dependant variable in Equations (2.1) to (2.3).

In unreported results, we find that using residuals from the market model as the dependant variable corroborates our main conclusion. Many more disasters are significant at the 40-day or 60-day event periods than at a very short-term event window. We also reach qualitatively similar findings using additional characterizations for the dependant variable: the unadjusted state-level portfolio returns in excess of the risk-free rate and the residuals from the

[7] We also test a probit model that attempts to explain the likelihood of observing a positive disaster-related impact on stock returns. The probit model yields conclusions qualitatively similar to those of the straightforward rank correlation approach but suffers from non-convergence issues.

Fama and French three-factor model (Fama & French, 1993). The fact that we obtain similar results whether or not we control for the market suggests that the impacts of natural disasters are diversified away at the nationwide level.

One of the concerns with our approach is that we test the impact of disasters on a state-by-state basis and are unable to make claims on the prevalence of meaningful disaster-related abnormal returns throughout the U.S. In our second series of robustness tests, we control for the expected proportion of falsely rejected hypotheses by implementing the false discovery rate (FDR) method described by Barras, Scaillet & Wermers (2010).[8] The approach of Barras et al. rests on the fact that the *p*-values of 'true' zero-abnormal return disasters are uniformly distributed over the interval [0,1]. By definition, the vast majority of estimated *p*-values larger than a sufficiently high threshold (λ^*) comes from meaningless events. We can estimate the proportion of 'true' insignificant extreme weather events ($\hat{\pi}_0$) in the entire population of disasters by extrapolating the proportion of events with intervention variables' *p*-values above λ^* to the [0,1] interval such that:

$$\hat{\pi}_0(\lambda^*) = \frac{\widehat{W}(\lambda^*)}{N} \cdot \frac{1}{(1-\lambda^*)} \tag{2.5}$$

where $\widehat{W}(\lambda^*)$ is the number of funds with estimated *p*-values exceeding λ^* and N is the total number of events. We also follow Barras, Scaillet & Wermers (2010) and use the bootstrap procedure introduced by Storey (2002) to select λ^*. The proportion of 'true' significant disasters in the U.S. equals $1 - \hat{\pi}_0(\lambda^*)$.[9]

We need a rather large sample size to generate a consistent distribution of *p*-values. We consider that the 39 intervention variables used in the context of our state-portfolio approach would not be enough to obtain reliable estimates of $\hat{\pi}_0$. Thus, we employ the *p*-values from the tests on disaster-firm pairs (N = 2,142) as well as on another set of results based on the effect of individual disasters on the state-level portfolios (*N* = 195) to implement the FDR approach.

[8] Benjamini & Hochberg (1995) appear to be the first to suggest that the false discovery rate may be the appropriate error rate to control in many multiple testing applications such as the ones examined in this paper.

[9] We do not distinguish between the 'true' proportion of positive and negative abnormal returns as we believe conclusions would be unreliable given our relatively small sample sizes.

Table 2.7 reports the conclusions of the FDR tests. Panel A uses the 2,146 individual firm-disaster pairs and panel B the 195 disasters with the state-level portfolios. Controlling for false discoveries strengthen our claim that disasters have no immediate impact on the stock returns of local firms as we cannot exclude that false discoveries alone explain the few significant events in the 1-day and 5-day periods. In both panels, we observe the largest proportion of 'true' significant events at the 40-day and 60-day windows. Hence, even after taking false discoveries into account, we still find that around 6% or 7% of catastrophes have a significant impact on returns and that the uttermost effects of disasters is felt gradually in the two or three first months following the peak of an extreme weather event.

[Insert Table 2.7 around here]

Last, we distinguish between disasters that occur in the 2008-2009 period associated with the global financial crisis and disasters that happen in other periods. We report the results of that analysis in panel C of Table 2.7. We observe a much higher proportion of significant abnormal returns for disasters during the global financial crisis, even after controlling for false discoveries. Thus, times of high market turbulences appear to exacerbate the impacts of natural hazards on stock returns but this does not alter our finding about the progressive effect of disasters on stock returns that are felt on a two-to-three month period. Also, we cannot dismiss that some disasters in calmer market periods have a significant effect on stock returns.

2.5 VOLATILITY EVENT-STUDY

2.5.1 Method

In this section, we examine the impact of disasters on the second moments of stock returns. Wang & Kutan (2013) test for disaster-induced volatility on American and Japanese stock markets by adding dummy variables to the conditional variance equation. They find evidence consistent with a temporary increase of conditional volatility in the few days following a natural disaster in the U.S. but notice no such increase in Japan. Wang & Kutan (2013) provide no explanation for the conflicting results. We revisit the issue of disaster-induced volatility but employ a different methodology.

Lu & Chen (2011) examine the GARCH dummy variable methodology using Monte Carlo simulations and demonstrate that the distribution properties of the maximum likelihood estimator for the variance dummy variable coefficient in GARCH models may lead to misleading inferences in event studies focussing on short event windows. They suggest including at least 100 observations in event windows when using the GARCH dummy variable methodology to ensure reliable statistical inference. As our focus is on a relatively short event period, we opt for the alternative approach of Białkowski, Gottschalk & Wisniewski (2008) who basically compare the conditional volatility forecasted from a GARCH model to the variation in the residuals observed during the event period. Their test statistic represents the multiplicative effect of an event on volatility. More specifically, the abnormal volatility portrayed by the multiplicative effect variable M_t can be estimated as:

$$\widehat{M}_t = \frac{1}{N-1}\sum_{n=1}^{N}\frac{\left(N\cdot\hat{\varepsilon}_{n,t*+s}-\sum_{j=1}^{N}\hat{\varepsilon}_{j,t*+s}\right)^2}{N\cdot(N-2)\cdot \mathrm{E}\left[\sigma^2_{n,t*+s}|\Omega_{t*}\right]+\sum_{j=1}^{N}\mathrm{E}\left[\sigma^2_{j,t*+s}|\Omega_{t*}\right]} \tag{2.6}$$

where the numerator can be interpreted as the variance of the residuals demeaned using the cross-section average and the denominator as the event-independent demeaned residual obtained with the forecasts from the conditional variance model. *N* represents the number of events included in the sample, $\hat{\varepsilon}_{t*+s}$ is the estimated residual for event day s, $s = S_1, \dots, S_2$, and $[\sigma^2_{t*+s}|\Omega_{t*}]$ is the forecast of the conditional variance at the event date s based on the information known at time t^*. t^* is the last trading day before the event period and S_1 is the first day of the event window. Natural disasters do not impact stock volatility if $M = 1$. At the event date S_1, the abnormal percentage change in volatility is given by $\left(\widehat{M}_t - 1\right)$.

For a longer event window (S_1, S_2), the cumulative abnormal volatility can be calculated as:

$$CAV(S_1, S_2) = \left(\sum_{s=S_1}^{S_2}\widehat{M}_t\right) - (S_2 - S_1 + 1) \tag{2.7}$$

Essaddam & Mnasri (2015) advocate the use of a bootstrap methodology to assess the significance of CAV. We follow their method and initiate the bootstrapping technique by generating rescaled residuals that are used to compute the bootstrap *p*-value. The steps in their method are:

i. Estimate the conditional variance equation for each of the *N* disasters using the corresponding observations in the estimation window.

ii. Store all standardized residuals obtained from the conditional variance equations in a matrix called $\hat{\mathbf{E}}$.[10]

iii. From $\hat{\mathbf{E}}$, create N matrices $\widehat{\mathbf{EE}}_{\mathrm{n}}$ where $n = 1, \dots, N$. A matrix $\widehat{\mathbf{EE}}_{\mathrm{n}}$ consists of $(S_2 - S_1 + 1)$ vectors. Each vector is obtained by multiplying the vector $\hat{\mathbf{E}}_{\mathrm{n}}$ by the corresponding predicted conditional standard deviation σ_{t*+s}. Each resulting vector has zero mean and variance σ^2_{t*+s}, where $s = S_1, \dots, S_2$.

Essaddam & Mnasri (2015) then use a moving block approach where the rescaled residuals are chosen in a chronologically consecutive manner to deal with any potential serial correlation in the residuals. The iterative procedure they suggest can be described as:

(I) For each firm, quasi-randomly assign $(\mathrm{S}_2 - \mathrm{S}_1 + 1)$ elements from $\widehat{\mathbf{EE}}_{\mathrm{n}}$ so that the first element belongs to the first vector. To capture any potential systematic autocorrelation in the original distribution of the residuals, choose the elements in a chronologically consecutive manner.

(II) Compute the CAV according to Equations (2.6) and (2.7).

(III) Repeat steps I and II 10,000 times and sort the collection of resulting abnormal volatilities in an ascending order to obtain the empirical distribution.

The *p*-value corresponds to the percentage of simulated abnormal returns that exceed the abnormal volatility calculated from the original sample.

We adapt our methodology that rests on state-level portfolios to implement this approach. While we do test for abnormal volatility using the returns of the state-based portfolios in conjunction with individual major disasters, we now use a pre-event period of 500 trading days to estimate parameters in order to escape look-ahead biases. We also discard events when their estimation periods overlap with other natural disasters, events with insufficient return history to fill the estimation period, and disasters with insufficient return data in the event period. Finally, we leave aside the ARMA structure for the conditional mean equation and opt for a parsimonious GARCH(1,1) model with normally distributed standardized residuals for the

[10] Note that the distribution of the adjusted standardized residuals keeps the same characteristics and specificities of the original distribution of residuals in the event window under the null hypothesis of no abnormal volatility.

conditional variance equation in order to reduce the number of parameters to estimate and obtain more accurate CAV forecasts. The following equations describe the final model used to study abnormal volatility:

$$RET_t = b_0 + \varepsilon_t \tag{2.8a}$$

$$RET_t - r_{f,t} = b_0 + b_1 RMRF_t + \varepsilon_t \tag{2.8b}$$

$$\sigma_t^2 = \gamma_0 + \gamma_1 \sigma_{t-1}^2 + \gamma_2 \varepsilon_{t-1}^2 \tag{2.9}$$

We use two distinct specifications for the conditional mean equation for robustness purposes. In Equation (2.8a), only a constant term distinguishes the residuals from the portfolio returns. In Equation (2.8b), the residuals are from the market model. Equation (2.9) describes the conditional variance σ_t^2 as modelled by a classic GARCH(1,1) approach. Equations (2.8) and (2.9) are estimated jointly using the maximum likelihood method over a 500-days estimation period that immediately precedes disaster *n*.

Assuming that the standardized residuals are i.i.d. N(0,1), the s-day ahead forecast of the conditional variance becomes:

$$\left[\sigma_{t^*+s}^2 | \Omega_{t*}\right] = \hat{\gamma}_0 \sum_{s=S_1}^{S_2} (\hat{\gamma}_1 + \hat{\gamma}_2)^s + (\hat{\gamma}_1 + \hat{\gamma}_2)^{S_2} \hat{\gamma}_1 \sigma_{t^*}^2 + (\hat{\gamma}_1 + \hat{\gamma}_2)^{S_2} \hat{\gamma}_2 \hat{\varepsilon}_{t^*}^2 \tag{2.10}$$

2.5.2 Results

Table 2.8 reports the volatility event study results. We observe that, on average, disasters have no significant impact on volatility with slightly negative CAV for most event windows. However, an examination of the abnormal variance by disaster categories reveals an immediate and highly significant increase in variance for floods, episodes of extreme temperature and, to some extent, winter weather. The impact of hurricanes is less obvious. Hurricanes clearly increase individual stock volatility when we do not control for the market and use the conditional mean equation (2.8a). However, when residuals from the market model using equation (2.8b) are employed, changes in volatility stemming from hurricanes exhibit a similar level but are no longer significant. Higher market-wide volatility in the days before the peak date of hurricanes is one possible explanation for this unexpected finding.

[Insert Table 2.8 around here]

We verify whether our results on abnormal volatility are robust to changes in the length of the estimation period. We are unable to accurately model GARCH processes for many disasters and obtain unreliable estimations using a 250 day estimation period. With a 1,000 day estimation period, most of our results are similar to those reported in Table 2.8, but employing four years of data instead of two entails an important drop in the number of admissible disasters from 108 to 53. We also try a non-sequential bootstrap approach and observe slightly higher *p*-values. This does not change our conclusions in any material way. Last, we exclude disasters that occur during the global financial crisis and again obtain quantitatively similar results.

2.6 CONCLUSION

Recent papers in the literature examine the short-term impact of natural disasters on stock markets and reach opposing conclusions. Some studies find that disasters significantly disturb stock returns (A. Worthington & Valadkhani, 2005) while other studies maintain that returns are not affected by these events (Wang & Kutan, 2013; Worthington, 2008). This paper explores if the opposing results originate from a geographic or from an event period duration story by examining the impact of U.S. disasters on the stock returns of firms based in the damaged areas on event windows larger than the usual one to five days. The findings are consistent with both the geographic and the longer event period duration stories. Firms located in the disaster state incur greater abnormal returns on average than firms headquarter in nearby states. After controlling for the expected proportion of falsely rejected hypotheses, disasters produce no abnormal returns in very short-term event periods (one to five days), but a small proportion of between 6% and 7% of the severe weather events significantly affect stock returns when abnormal returns are investigated over a medium term (two-to-three months) event period. The results are similar if we control for the contemporaneous returns of the aggregate stock market and are robust to the grouping of disasters by categories and of firms into state-level portfolios.

The second goal of this paper is to investigate the impact of disasters on the volatility of stock returns. Wang & Kutan (2013) provide evidence that volatility increases in the American market but remains stable in the Japanese market. Using a different methodological approach, we find that conditional volatility clearly increases following hurricanes, floods, episodes of extreme temperature and severe winter weather. However, we detect no such change in the second moments of returns for other major storm-like events.

It remains unclear why some firms experience a positive impact from natural disasters while others face negative consequences. Disaster-related losses and periods of high market volatility appear to bring more frequent abnormal returns. However, our attempts to associate the strength and the direction of abnormal returns to potential determinants (such as the intensity of media attention firm size, firm industry or distance between a firm’s headquarter location and the worst affected area) result at best in weak relationships. Other firm-specific and region-specific factors are possibilities. This is left for future research.

CHAPTER 3
Municipal Financing Costs Following Disasters

3.1 INTRODUCTION

Catastrophic events such as major floods often generate heavy damage to the local economy. Among other things, these events can exacerbate financial distress, negatively affect personal consumption, disrupt business operations and damage infrastructures and utilities. Yet, as the consequences of natural hazards are primarily regional (West & Lenze, 1994), traditional investment theory assumes that investors are able to completely diversify away these shocks and suggests that floods should not command an extra compensation.

The bulk of the research on the effects of disasters on financial markets involves two types of assets[11]: stocks and residential real estate. Previous literature on the stock markets mostly conforms to the diversification rationale (Bourdeau-Brien & Kryzanowski, 2017; Ferreira & Karali, 2015; Wang & Kutan, 2013; Worthington, 2008; Yang, Wang & Chen, 2008). For its part, most of the residential real estate literature indicates that disasters such as floods have an economically significant effect on the price of residential real estate (Bin, Kruse & Landry, 2008; Harrison, Smersh & Schwartz, 2001; Shilling, Sirmans & Benjamin, 1989). The flood-related price discount in property prices supports the assumption that diversifiable risk is priced in the housing market, which is consistent with Englund, Hwang & Quigley (2002) who argue that there is a strong idiosyncratic component to the return from investing in an individual property that most households cannot diversify away.

In this paper we investigate how the primary market for municipal bonds reacts to floods. Municipal bonds (munis) are another class of assets that has been largely overlooked in the literature on natural disasters but that is plausibly impacted by catastrophes such as floods. Munis are similar to stocks in that they are traded by investors who can alleviate most idiosyncratic risk through proper portfolio diversification. Still the asymmetric taxation of these securities leads to a geographic segmentation of the market at the state-level (Cook, 1982;

[11] There also exists a rich literature on the relationship between disasters and the insurance industry (Kunreuther, 1996; Mills, 2005) as well as on the impact of disasters on the catastrophe bonds and the insurance derivative markets (Canter, Cole & Sandor, 1997; Perrakis & Boloorforoosh, 2018).

Hendershott & Kidwell, 1978; Lamb & Rappaport, 1987; C. A. Pirinsky & Wang, 2011) that magnifies the effect of local risks (Babina, Jotikasthira, Lundblad & Ramadorai, 2015). Municipal markets also share some characteristics with housing markets in that munis are mostly traded by local retail investors (Ang, Bhansali & Xing, 2010; Elmer, 2014), are deemed less liquid than most stocks and have underlying assets (or revenue/tax bases) that are geographically delineated. This leads us to consider the following research question:

Q1 Do flood episodes induce a price discount for subsequent new issues of municipal bonds?

We address this research question using a large sample of new issues of municipal bonds and focus on local floods for which the disaster area can be circumscribed to one or a few counties that sold municipal general obligation (G.O.) bonds. Unlike many event studies, our focus on floods alleviates *de facto* most of the endogeneity concerns given that flood episodes are obviously uncorrelated with national and regional economic conditions and give a clear picture of the causality relation. Note that our investigation relies on how investors perceive the uncertainty associated with the timely payment of the promised cash flows of new bond issues following a flood episode. Conversely, we do not assume that municipalities issue those bonds to finance disaster recovery.[12] As a minor contribution, we disentangle the intermingled effects of flood events from flood risk.

Addressing research question Q1 only provides a partial examination of the relationship between floods and new municipal bond issues. Since the impact of disasters on prices is of equal interest, we also investigate several non-mutually exclusive channels through which floods may plausibly affect financial markets. In particular, a no-impact result to Q1 may arise from some channels having opposing effects on bond prices. This investigation addresses the following research questions:

Q2 Do floods affect the marketing of new municipal bond issues?

Q3 Do floods affect the propensity to issue a new municipal bond?

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[12] See Settle (1985) for a discussion about how municipalities finance disaster losses and the recovery phase and Gao, Lee & Murphy (2016) for a discussion of state assistance and state-based risk sharing policies. Also note that we find less than ten new issues whose purpose can confidently be related to flood events and that our sample contains no disaster bonds.

Q4 Do floods affect expected bond returns due to credit risk or liquidity risk?

Q5 Do floods affect investor behaviors?

To the best of our knowledge no empirical paper has so far studied the effect of floods on the dynamics of the municipal bond primary market. We believe our study has several important implications for investors, portfolio managers and municipal authorities. Indeed, our results shed some light on how and why local sources of risk affect the pricing of bonds and offer evidence consistent with a significant interaction between extreme weather, investor behavior and public financing.

The rest of the paper is structured as follows. Section 3.2 develops our main hypotheses and provides our expectations. Section 3.3 states and details the methodology and presents the datasets. Section 3.4 reports and discusses the empirical results associated with our main research question (Q1) and re-examines the outcomes using various robustness checks. Section 3.5 documents the merit of various channels through which floods may impact bond prices (Q2 to Q5). Section 3.6 describes the main limitations of our study. Section 3.7 concludes.

3.2 HYPOTHESES DEVELOPMENT

3.2.1 Do flood episodes induce a price discount?

Previous literature on the effects of floods on the municipal bond market is sparse at best. The most closely related papers include Denison (2006) who observes that muni markets were quick to react to Hurricane Katrina, Fowles, Liu & Mamaril (2009) who find that earthquake risk matters in determining the interest cost for California municipalities, Hildreth (2009) who offers anecdotic evidence of disasters-induced disruption in the timely payment of municipal debt and debt financed rebuilding efforts, and, to a lesser extent, Ebdon, O'Neil & Chen (2012) who argue that "*Disaster costs may affect real and/or perceived risk to municipal bondholders*" (p.41).[13]

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[13] Note that another strand of the literature employs natural disasters as a source of exogenous variation to examine various dimensions of the municipal bond market (Perignon & Vallee, 2017).

More evidence favoring a link between floods and financial markets comes from the housing literature where the price discount for U.S. houses located in 100-year floodplains averages six percent. That average hides the fact that some papers find no flood-related discount (Bialaszewski & Newsome, 1990; Zimmerman, 1979) while others observe a discount in excess of ten percent (Donnelly, 1989; MacDonald, White, Taube & Huth, 1990). The most popular explanations for these somewhat conflicting results hinge on the relationship between flood risk and flood events. On the one hand, the information story of Troy & Romm (2004) assumes that flood episodes give investors new information about the probability distribution of flood risk. On the other hand, the rise in risk awareness story of Lamond & Proverbs (2006) states that the price discount observed in house prices originates from a temporary increase in risk awareness induced by flood events rather than by actual flood risk per se. Both stories predict that investors will require additional compensation for flood risk following a flood episode. However, the information channel implies that flood events have a more long-lived impact and can be interpreted as a "rational" explanation for a price discount while the risk awareness channel entails a more short-term impact due to behavioral considerations.

Given that the municipal bond market shares similarities with the housing market in terms of geographic segmentation and predominance of local retail investors, we expect to observe a statistically significant rise in yields following major floods.[14] Hence, our first research question (Q1) is associated with the following hypothesis:

> **H1:** Municipal bonds issued in the months following major floods exhibit significantly higher yields.

The existence of a price discount implies a higher cost of financing for the municipal issuer. In the context of this study, such a price discount is observable as a higher yield-to-maturity at issue.

3.2.2 How flood episodes impact new municipal bond issues

Distinguishing the source of the flood discount has significant implications for portfolio choice and investor risk management and we are particularly interested in contrasting a "rational" risk channel from a behavioral explanation. Yet, other plausible channels may help explain any relationship between disasters and municipal new issue yields.

[14] Luechinger & Raschky (2009) find that the negative impact of floods on life satisfaction is similar in magnitude to the price discount found in housing markets.

The first channel that we investigate involves the possible impact of floods on the marketing of new issues. Everything else being equal, higher issue yields implies higher costs of financing for municipalities provided H1 is true. However, changes in issue yields may undervalue (overvalue) the net impact that disasters have on financing costs if floods increase (decrease) the negotiating power of underwriters or modify the way underwriters sell municipal bonds. We focus on two metrics to assess the effect of floods on the marketing of new bonds: changes in gross underwriting spreads and changes in underpricing activities (Green, Hollifield & Schürhoff, 2007a). As the literature provides no guidance concerning the effect of disasters on underwriter behavior,[15] our base expectation linked to Q2 is that floods have an insignificant impact on these metrics.

H2.A: Floods have no impact on gross underwriting spreads.

H2.B: Floods have no impact on the level of underpricing for new municipal bond issues.

The second channel that we investigate arises from the possibility that a major flood event modifies the municipal decision to conduct a bond issuance or modifies the timing of the issuance. Municipalities may refrain from issuing new bonds if they acknowledge that investors will require abnormally high yields following floods. In such cases, only financially constrained counties that have no available alternative to bond financing may issue bonds. In contrast, higher investor recognition (Merton, 1987) or a greater pulling together for the social good following natural disasters (Seppala, 2012) may motivate municipalities to issue extra munis as they might expect to lower their interest cost due to an increased demand (broader investor base). Both situations may result in a selection bias (Heckman, 1979) that invalidates our main results regarding H1. Our base expectation associated with Q3 is that flood episodes are irrelevant in the decision to issue municipal bonds.

H3: Floods do not significantly affect the propensity to issue municipal bonds.

Our fourth research question examines the merit of familiar risk-based explanations that include flood-induced variations in credit risk and liquidity risk. We refer to explanations based on changes in the credit worthiness of issuers or on the expected liquidity of post-flood issues as the *rational* risk channel.

[15] While the previous literature does not specifically address the impact of disasters on underwriters, Green (2007) provides a thoughtful discussion of the competitive forces in the underwriting industry.

We define credit risk as the possibility that an issuer will fail to meet its obligations in accordance with agreed-upon terms. Although G.O. bonds in the municipal market are backed by the taxation power of the issuer, that taxation power does not alleviate all credit risk. Furthermore, several states place a legal limit on the authority of local governments to impose property taxes (constitutional tax limit) and some state legislations also limit the amount municipalities can increase taxes (tax caps). From a historical perspective, Feldstein & Fabozzi (2008) report around 10,000 municipal defaults since 1839 and provide at least one example of a municipal bankruptcy following a disaster: Galveston, TX, following a hurricane in 1900. For its part, Cohen (1989) observes that the incidence of municipal default greatly increases in economic downturns and argues that the same economic forces are likely to result in municipal defaults in the future. More recently, the high profile bankruptcy stories of the City of Detroit, Michigan (2013), of the City of San Bernardino, California (2012), or of Jefferson County, Alabama (2011) remind investors that municipal defaults are possible. Yet, it is not clear whether or not investors consider floods to have a large enough impact to distress issuers. While Moody's put 51 issuers affected by Hurricane Katrina on its credit watch list in 2005, the hurricane has not caused a single default.

The literature offers conflicting evidence regarding the effect of credit risk. While Marlowe (2006), Denison (2006) and others observe that the municipal bond market is quite resilient to major disasters, Hagendorff, Hagendorff & Keasey (2015) and Hofman & Brukoff (2006) argue that natural disasters can considerably affect public finances through a weakened revenue base and increased pressure on spending. Also, Mejia (2014) notices an increase in the sovereign debt ratio following a disaster that results from the combined effects of an increasing debt level and a lower GDP. Based on these papers, we anticipate that flood events are likely to reduce the financial flexibility and to increase the credit risk of municipal issuers.

We proceed with a collection of indirect observations to examine the potential of the credit risk channel. First, we monitor for changes in the credit rating of disaster-affected issuers and expect an increase in credit risk to be associated with a credit downgrade. Second, we observe whether post-flood issues are more often of the unlimited type, more frequently insured or more regularly secured by collateral assets. Third, we scrutinize for variations in total debt, median household income, housing units, general fund surplus and unaffected reserve funds. We argue that higher debt figures, declining household income, smaller number of housing units, lower surplus or lower reserve funds could indicate a drop in municipal creditworthiness. Last, we examine whether obtaining federal aid through the Federal Emergency Management

Agency (FEMA)'s presidential disaster declaration process mitigates credit risk and the price discount.

H4.A The credit rating of some muni issuers is downgraded following floods.

H4.B Post-flood issues are more frequently unlimited G.O., more often insured or more regularly secured by collateral assets.

H4.C Local economic or local financial indicators worsen following major floods.

H4.D The price discount is lower for floods declared major disasters by the President of the United States.

We also briefly explore the relationship between flood episodes and liquidity risk. On the one hand, natural disasters may scare away some investors and reduce market depth. The expected lack of marketability on the secondary market for the securities issued by flooded municipalities could explain a price discount. On the other hand, if flood episodes expand the media coverage of disaster areas, then it may also heighten investor recognition about the issuer (Merton, 1987). Such a recognition effect could increase market depth and contribute to explain a hypothetical price premium induced by floods.

The literature provides no guidance about the plausible net effect of floods on liquidity risk in the municipal bond market. Nonetheless, we anticipate observing few variations in liquidity measures, either because floods are expected to have an insignificant impact on the amount of trading or because the recognition effect is, on average, of a similar magnitude to the scaring effect.

H4.E Floods have no significant impact on the level of round-trip transaction costs in the secondary muni market.

We use round-trip transaction costs as our main measure of liquidity in the secondary municipal bond market. We follow Green, Hollifield, & Schürhoff (2007b) and match buy and sell trades with similar par amounts that occur over a short time period to estimate round trip transaction costs.

Last, we examine whether or not the data are consistent with the temporary increase in risk awareness story. Several theoretical explanations are consistent with this story. First, Pryce, Chen & Galster (2011) provide a behavioral story based on investors being myopic and amnesiac to explain why floods may affect house prices even if disasters are a merely idiosyncratic source of local risk. They argue that the risk perceived by investors can diverge considerably from its actual level, especially "*if a long period has elapsed since a local flood has occurred*" (p.262). The increase in risk awareness also goes along the lines of the availability heuristic well documented by Tversky & Kahneman (1973) and others which we describe as the tendency to assess the frequency of events based on how easily instances of that type of events come to mind. Accordingly, the availability heuristic may lead investors that recently experienced a flood episode to overestimate the frequency of future flood events. Moreover, Kousky, Pratt & Zeckhauser (2010) study the learning process following the occurrence of extreme events and state that a further occurrence of an extreme event, unlike the first occurrence, does not have much of an effect on risk. Hence, if the behavioral bias is to explain the price discount, one would expect price discounts to be higher for first-time disaster issuers than for regions where investors have previous experience of major flooding.[16] Given the large consensus in the recent housing literature on a temporary yet significant price discount associated with a greater fear of flooding following flood episodes, we expect our results to support the temporary increase in risk awareness story. That story produces the following testable hypotheses:

H5.A The price discount fades over time as past flood episodes are forgotten.

H5.B The price discount is larger for counties that experience a major flood for the first time than for regions with previous experiences of flooding.

The next section describes the method and data used to test our main hypotheses.

3.3 METHOD AND DATA

The two central challenges in the investigation of the impact of floods on municipal bond yields rest on (1) the determination of what should be the "normal" yield of a new issue through the identification of a suitable set of variables whose choice and functional form are grounded in the existing literature and (2) the choice of the appropriate econometric framework to capture the

[16] A natural validation test would consist in comparing the impact of floods between participating and non-participating counties to the National Flood Insurance Program. However, we are unable to run such a test of risk awareness given that all of the major flood events occur in participating areas.

correlation structure across issues and issuers that plausibly follow a hierarchical and geographical clustered pattern.

3.3.1 Response variable

We obtain the data on all tax-exempt general obligations (GO) bonds issued by U.S. county governments between January 2005 and June 2015 by merging information extracted from Bloomberg and the Municipal Securities Rulemaking Board (MSRB) transaction database. The MSRB began reporting real-time bond prices in January 2005 and Schultz (2012) claims that this innovation caused a sharp diminution in the dispersion of muni prices. Our analysis uses the initial offering yields determined by the underwriter at the issue date as the response variable. Most of the new issues are serial bonds and we refer to the individual maturities as the 'tranches'. Since our analysis is performed at the tranche level, our sample consists of 56,096 tranches originating from 4,134 issues spread across 1,050 counties in 43 states.

3.3.2 Model of normal yields

The literature on the determinants of municipal bond yields identifies several variables as being good candidates for explaining the cross-section volatility of new issue yields. We include in our base model the most commonly used variables while balancing the trade-off between parsimony and comprehensiveness. We incorporate additional explanatory factors to our model in robustness tests. We discuss the choice, the underlying economic rationale and the construction of the determinants of bond yields in Appendix A due to space considerations. Table 3.1 details the explanatory variables included in our model and the variables associated with flood risk that are discussed in section 3.3.3.

[Insert Table 3.1 around here]

In addition to the control variables, we also include year fixed-effects in our model. We expect the year fixed-effects to capture most unobservable changes in the dynamics of the muni markets.

3.3.3 Flood-related variables

The literature on housing and floods often distinguishes between the effects of flood risk and the temporary effects associated with recent flood events. Among others, Lamond & Proverbs

(2006) empirically show that flood-related premiums are essentially associated with flood events and not flood risk per se and that the premiums slowly fade away as flood episodes are forgotten. Accordingly, we construct disaster-related variables that are connected to flood risk or to flood events.

We use the data from the storm events database of the National Center for Environmental Information (NCEI) to identify the flood events. That database contains information on 48 mutually exclusive types of natural disasters with detailed technical definitions. Of prime importance to us, the observations in the database are segmented by counties. The NCEI database provides damage estimates for most events that may come from insurance companies, from other qualified individuals or from 'guesstimations'. We acknowledge that NCEI's damage estimates may be subject to large errors. Downton, Miller & Pielke (2005) observe that while the estimates are particularly inaccurate for smaller floods and smaller areas, they tend to average out. Given the concern with the damage figures, we mostly use the damage estimates to classify floods by categories rather than using the estimates as a continuous variable in our calculations. We first extract all county-level flood events in the 1990–2015 period and calculate the total per capita damage in constant 2015 US$. Given the scarce literature on the subject, we use two thresholds to define a damage threshold that identify floods deemed to have a sufficient magnitude to affect bond yields and to delimit a time period during which flood events may affect the muni market, respectively. We choose to consider floods that cause at least an average of 100$ of damage on a per-capita basis and observe the effect of these floods on munis issued in a 12-month period following a disaster. These choices reflect our desire to balance representativeness with sample size concerns. We create a dummy variable (IsFlood) that equals one in the year following a major flood and zero otherwise.

There exists no indicator that accurately discloses the probability distribution of flood risk at the county level. Therefore, we use a number of indirect indicators to assess the ex-ante risk of flooding for counties. The flood risk variables that we select can arguably be traced back to the information that is known to retail investors that trade municipal bonds. First, we employ the Federal Emergency Management Agency (FEMA)'s National Flood Hazard Layer (NFHL) geodatabase and calculate the proportion of a county's area that is located in high flood risk zones (%Area).[17] Second, we relate the location of all NFHL's high risk flood zones to U.S. Census block maps and estimate the proportion of a county's population that resides in small

[17] High risk areas are defined as zones that will be inundated at least once every 100 years.

areas that intersect high flood risk zones (%Pop) (Crowell, Coulton, Johnson, Westcott, Bellomo, Edelman & Hirsch,2010). Third, we design a flood risk variable based on the number of major floods experienced by a county in the 15 years preceding a bond issue (HistoFlood). Our first three flood risk variables somewhat proxy for the likelihood to experience a major flood. Last, we use the total damage, expressed in constant 2015 US$, of all major floods that occurred in the 15 years preceding a bond issue (DmgFlood). This final variable is linked with the historical intensity of flood episodes and is thus a measure of risk severity. We observe that the proportions both of %Area and of %Pop at high risk of flooding are uncorrelated with IsFlood while HistoFlood and DmgFlood are positively and significantly associated with the occurrence of a flood event.

3.3.4 Econometric approach

Papers that examine municipal bond yields tend to use increasingly sophisticated models to match the particular, often hierarchical, structure of the data. Fowles, Liu & Mamaril (2009) investigate the effect of earthquake risk on munis through an OLS regression model that includes year dummies. Gao & Qi (2013) study the effect of political uncertainty on munis and employ a WLS regression with month dummies in combination with year and issuer fixed-effects. They weight the observations according to the issuance activities by state. Reck & Wilson (2014) test the effect of new accounting rules on munis through an OLS model with year dummies. Apostolou, Apostolou & Dorminey (2014) investigate the effect of budget imbalances on munis. They explicitly acknowledge the presence of unobserved effects and run a panel regression with issuer-level fixed-effects to alleviate a possible omitted-variable bias.

The methodology we employ in this study relies on linear mixed effects models (LMM) in order to fit the unbalanced, hierarchical and/or clustered structure of the data (Molenberghs & Verbeke, 2000; West, Welch & Galecki, 2014). The LMM approach is increasingly employed in the epidemiology and the real estate literature to model neighbourhood effects (Ioannides & Zabel, 2003; Subramanian & O'Malley, 2010). LMM are deemed appropriate in our context as issue yields of individual tranches from the same issuer or the same state cannot be assumed to be independent from each other. While many observable variables help to explain the variations in yields, there always remain a non-negligible part of area-specific effects that is unobservable and can result in an omitted-variable bias. In LMM, unobservable effects can be treated as random effects (RE) for which the variance is estimated to improve statistical inferences on the coefficients of the observable regressors. The following general equation describes the structure of our model:

$$Y = C\delta + X\beta + Zu + \epsilon \tag{3.1}$$

where $Y_{n\times 1}$ is the vector of responses, $C_{n\times p}$ is the matrix of controls augmented with year dummies, $\delta_{p\times 1}$ consists of the coefficients of the control variables, $X_{n\times q}$ is the matrix of disaster-related variables and $\beta_{q\times 1}$ contains the main parameters of interest that convey the effect of floods on the response variables. Note that C and X are both observable regressors. $u_{k\times 1}$ is an unknown vector of random effects, $Z_{n\times k}$ is the random effects design matrix and $\epsilon_{n\times 1}$ is the vector of residuals. We assume that $u \sim N_k(0, \Sigma)$ and test various correlation structures for the random effects covariance matrix $\Sigma_{k\times k}$.

Although the model is estimated at the tranche level, we weight each tranche according to its relative size in the issue in order to put equal consideration on each issue. Following (Molenberghs & Verbeke, 2000), we fit our model by restricted maximum likelihood (REML) and use the Huber-White Sandwich estimator to obtain heteroskedastic-consistent standard errors.

Preliminary testing makes us favor two levels of random effects focussed on issuers and states, respectively, where a simple diagonal matrix is used to model the covariance structure. Alternative specifications used as robustness tests included traditional WLS regression, WLS with clustered standard errors at the issue level and LMM with random effects having a more complex covariance structure. Note that our main results are robust to these alternatives and are also unaffected by the substitution of random effects for issuer-level fixed-effects. While none of our tests perfectly model the spatial autocorrelation found in the data, the two-level random effects model helps to reduce residual spatial autocorrelation and lower Moran's index (row standardized) from 0.0214 to 0.0011.

3.4 RESULTS

3.4.1 Descriptive statistics

Summary statistics for the control variables are reported in panels A and B of Table 3.2. We observe that most tranches are issued at a premium and are used for general or refunding purposes. Almost all tranches have maturities ranging from 0.5 to 24.5 years with an average of around 7 years. While the average principal size is about $730k, we observe a lot of variation with very small outstanding amounts of about $25k to very large tranches worth over $20MM. Our sample covers relatively poor counties with average household income less than $30k as well as relatively rich counties with average income over $100k but the low standard deviation indicates that most of the data are centered on the mean annual income of $55k. The population variables also signal large variation in size as well as in attractiveness from county to county. Last, the high kurtosis of DEBT clearly shows the large tails of the distribution of total per capita debt that denotes that a few counties support very high debt levels although many counties have no or almost no debt.

[Insert Table 3.2 around here]

Panel C of Table 3.2 is of particular interest for this study as it compares some important characteristics of munis issued in a 12-month period following a major flood from other bonds. One of our major concerns lies on the size of our sample of post-flood issues. Given the time period and damage thresholds, we obtain 65 issues, totalling 810 tranches, marketed in the wake of a major flood. While a greater number of observations would have been desirable, 65 issues remain acceptable to draw statistical inferences.

In terms of characteristics, post-flood issues exhibit similar maturities to those issues in the control group. Both subsamples account for about 45 percent of medium-term (3-10yrs) tranches and 35 percent of lengthier maturities. Hence, counties do not appear to substitute long term bonds with short-term notes after flood events. However, post-flood issues are considerably smaller on average than other issues in the control group. We also notice a significant difference in the credit rating that originates from a higher proportion of unrated tranches in post-flood issues that is almost perfectly offset by a lower proportion of high quality tranches (AA+, AA and AA-) in the post-flood sample. That the post-flood issues are smaller and are more frequently unrated can probably be explained by the fact that the post-flood subsample is tilted towards smaller issuers based on population density. Indeed, the median density reaches 97 residents per square mile in the event sample versus a score of 259 residents per square mile in the control group. The tilt towards smaller issuers also affects the median level of per capita debt that is slightly smaller in the post-flood sample at $517 versus $781 and the average median value of a housing unit that reaches $172,239 versus $198,573 in the post-flood sample and control group, respectively. The lower average housing value suggests that issuers in the post-flood sample have a somewhat lower tax base. The other characteristics, including the prevalence of bond options and household income figures, are comparable across subsamples.

Rivers & Yates (1997) show that the determinants of muni yields differ between small and large cities. Accordingly, the differences in county size between the post-flood sample and the control group may lead to an estimation bias. We later address the plausible bias by (1) studying whether the issues not marketed in post-flood periods by the disaster counties also exhibit a price discount, (2) estimating the effect of floods on a subset of small issues and (3) estimating the effect of floods on a subset of small counties.

3.4.2 Flood-induced price discount (H1)

In this section, we present our main result regarding the effect of floods on new municipal bond issues. We start by displaying the general relationship between our variables of flood risk and flood events and issue yields before relating the price discount to various bond characteristics through the inclusion of interaction terms in our model. Then, we complete our analysis with some robustness tests.

Table 3.3 shows how our model performs in explaining new issue yields. We remind the reader that the response variable is the natural logarithm of the issue yield. The logarithmic transformation allows interpreting the exponential value of the estimated coefficients of the controls $\left(e^{\widehat{\beta}} - 1\right)$ as the estimated percentage change in Y per unit change in the regressor. Given that the average yields vary from almost zero to around five percent during our sample period, we deem it better to express our results in terms of relative importance rather than in basis points. We observe that most control variables exhibit their expected signs and, when it is not the case, the variables are simply insignificant. We note one exception: LEADING exhibits a positive sign while we expected a negative one. While LEADING is statistically significant, the economic importance of the coefficient remains weak as a one standard deviation increase in LEADING is associated with a rather small yield increase of 1.2 percent or a one basis point increase over the mean issue yield. The pseudo R-squares indicate that our model explains about 70 percent and 80 percent of the dispersion in yields between states and issuers, respectively, and about 83 percent of the variance within counties.

[Insert Table 3.3 around here]

The first four columns of table 3.3 reveal that none of the flood risk variables help to explain bond yields. The last column includes IsFlood, our main variable of interest. The positive and significant coefficient of 0.0686 associated with IsFlood signals that a flood event in the year preceding a new issue increases the issue yield by about seven percent above the yield expected for an issue with similar characteristics in a non-event period. Consistent with the previous housing literature, the impact of floods appears to arise from flood events and not ex-ante flood risk and implies that post-flood issues exhibit a significant price discount. A *p*-value of 0.0351 is associated with the flood events dummy using the robust sandwich variance estimator. For a new bond at par with a maturity of 8 years, a coupon rate of 2 percent and a principal amount of $10 million, the average flood-induced yield premium reaches about 14 basis points and the related price discount represents a net loss of about $100,000 in terms of bond proceeds for the municipality, everything else being equal. So, H1 is not rejected.

We now turn to the robustness of the flood discount results. In panel A of Table 3.4, we implement three additional tests. First, we examine the possibility that the observed price discount comes from unobservable county-specific characteristics that are common to the disaster-counties. We develop a dummy variable that equals one for munis issued by disaster-counties in periods that do not follow a flood episode (OTHER_ISSUES). We count 212 such bonds, totalling 2,656 tranches, marketed by disaster-counties in non-flood periods. We observe that OTHER_ISSUES is indiscernibly different from zero, whether or not we include IsFlood and the flood risk variables in the regression. Thus, we can discard the possibility that the yield premium is associated with some unobservable issuer-level effect and can more reliably connect the price discounts to the flood events. Second, investors may price differently small and big issues. If this is the case, then the coefficients of the control variables will vary according to the issue size. As the post-flood subsample is characterised by smaller issues than the control sample, it may be desirable to estimate the model based exclusively on smaller issues. Accordingly, we proceed by excluding all tranches from issues larger than $15 million. We obtain an even bigger positive coefficient for IsFlood. We reach similar findings for various small issues size thresholds ranging from $5 to $20 million. Third, we employ the same rationale as for the issue size to exclude large issuers from the sample. We estimate the model on a subset of issuers tallying a population of less than the median number of 164,200 residents per county, and also find evidence of a flood-induced price discount, although we note a lower statistical significance of the flood-induced yield premium.

[Insert Table 3.4 around here]

Our second round of robustness tests is based on alternative econometric frameworks. These results are reported in panel B of Table 3.4. We begin with a traditional weighted least-square (WLS) model with no random-effects and continue with a WLS model with errors clustered at the issue level. Results regarding the effect of flood episodes on issue yield remain unchanged. Last, we add clustered residuals at the issue level to state- and issuer-level random effects. We also remove the diagonal constraint on the covariance matrix of the issuer-level random effects to allow for correlated random effects. Doing so vastly improves the flexibility of the estimation of the unobservable components and also greatly increases computation time. Still, the end results remain the same and neither information criterions nor pseudo R-squares provide an incentive for the use of this more complex model.

We now test for the inclusion of additional explanatory variables in our main model. Table 3.5 briefly describes the new variables. More information regarding the economic intuition underlying the choice of indicators can be found in Appendix B.

[Insert Table 3.5 around here]

The use of the augmented model yields similar conclusions in that we observe a positive and highly significant coefficient of 0.0714 for IsFlood, which is slightly higher than that disclosed in Table 3.3. Tabulated results using the augmented model are presented in Appendix B.

In unreported results, we calculate standard errors employing the wild bootstrap (WB) approach developed by Liu (1988) to assess the statistical significance of our regressors instead of the sandwich estimators. The WB is a semi-parametric procedure that leaves the regressors intact and resamples the response variable based on the residual values assuming that the null hypothesis is true. As the I.I.D. assumption is not imposed to simulate WB errors, it is well suited for models that exhibit heteroscedasticity and it can also accommodate skewness in the residuals. Using wild bootstrapping, many more control variables become significant and the IsFlood dummy is now positive and meaningful at the one percent level. Still, we choose to report the results based on the sandwich indicator for conservatism reasons.

Taking everything into account, our results are consistent with flood episodes having a significant positive effect on the yield of new municipal bond issues. Hence, as anticipated by our first hypothesis, we observe economically and statistically meaningful price discounts. In other words, the robustness tests provide additional evidence in support of H1.

3.5 WHY FLOOD EPISODES IMPACT MUNICIPAL FINANCING COST

The next logical step consists in linking our evidence favoring a flood-induced price discount to municipal borrowing costs by testing for flood-induced variations in the marketability of new issues. To investigate H2.A, we employ a subset of new issues on which the gross spread is available. We obtain data on gross spreads from Bloomberg who defines the spread as underwriter expenses, takedown and other issuance fees expressed as a percentage of the total issued amount. The gross spread is available for 2,154 out of the 4,069 new issues in our sample totalling 28,855 tranches. The average spread is 0.75 percent but this amount varies from 0.06 to 3.27 percent from one issue to the other. The characteristics of the issuance cost subsample are similar to those of the complete sample in that the observations have similar coupons, time-to-maturities and ratings. The sale dates show an equivalent distribution through the years and flood risk measures are akin. The issues in the gross spread subsample are slightly larger in terms of outstanding principal and the county's issuers are also a tad more populous and indebted but the differences remain thin.

Table 3.6 reports the results of the LMM estimated at the issue level with the gross spread (GROSSPREAD) as the response variable. We note that issuance costs grow with the average time-to-maturity of the issue and also find evidence of economy of scope as issue size is negatively correlated with the gross spread. Otherwise, we observe that unrated bonds are more costly to issue and that issuance costs are lower when bonds are secured by collateral assets, when the issuer's population is larger and wealthier and when the economic prospects at the state level get better. Unlike the findings of Simonsen & Robbins (1996), we discern no significant differences in issuance costs between competitive and negotiated sales.

[Insert Table 3.6 around here]

Interestingly, our analysis provides evidence of lower issuance costs in post-flood periods. Indeed, in the wake of a flood, the average gross spread is reduced from 0.75 to about 0.66 percent, which represents an economy in terms of bond proceeds of about $8,700 for the municipality. However, the reduction in issuance cost is far from offsetting the higher financing costs of $100,000 that arise from the flood-driven price discount. Counties nearby disaster areas (IsNeighbour) also benefit from lower financing costs but the magnitude of the flood effect is almost twice smaller. Hence, floods appear to impact gross underwriting spreads but the effect does not explain the higher issue yields.

Besides the underwriting spread, we also examine whether or not flood events modify the relative importance of underpricing activities (Green, Hollifield & Schürhoff, 2007a) as we consider the possibility that underwriters could be tempted to lower bond prices in order to attract additional investors in post-flood periods. We investigate this prospect on an ex-post basis by calculating the level of underpricing as the difference between the average of the yields observed in secondary market transactions that occur in the two weeks following a new issue with the issue yield. We succeed in calculating underpricing measures for only 30 percent of the tranches of our sample, since the others were not traded at all in the weeks following their issuance. Notwithstanding this limitation, a standard t-test does not reject the assumption of an equal underpricing level between post-flood and other issues at the 5 percent level. Hence, our results are consistent with H2.B in that underpricing activities seem to have an immaterial effect on our main conclusions.

Next, we study the merit of H3, the variation in the propensity to issue municipal bonds channel. On the one hand, municipalities may refrain from issuing new bonds if they acknowledge that investors will require abnormally high yields following floods. In such cases, only financially constrained counties that have no available alternative to bond financing may issue bonds. On the other hand, higher investor recognition (Merton, 1987) or the greater pulling together for the social good following natural disasters (Seppala, 2012) may motivate municipalities to issue extra munis. Both situations may result in a selection bias. We examine selection bias by estimating how flood episodes affect the probability to issue municipal bonds using a logistic model. We proceed by generating a new dataset that displays one observation per county for every subperiod between January 2005 and June 2015. We construct a binary response variable to which we assign the value of one if the county has issued new bonds during the subperiod and of zero otherwise. We also include a dummy variable that equals one if a major flood occurred in the county in the year preceding the end of the subperiod (IsFlood). Then, we merge all exogenous variables associated with the municipal bond market and with local economic conditions that might be related to the probability to issue munis. Some of the explanatory variables that we include in the logistic regression are not listed in Table 3.1 or Table 3.5. In particular, AVGYLD represents the average yield of municipal bonds traded on the secondary market. That variable is constructed like the BMK variable except that the muni market universe is not segmented by maturities and ratings groups.

We estimate the logistic model using various time intervals for the subperiods ranging from monthly to yearly and we report the results in Table 3.7. Standard measures of goodnessof-fit reveal that the model has significant explanatory power. Furthermore, many of the muni market and local economic variables are meaningful in explaining the probability to issue new munis. Municipalities are more prone to issue bonds when the spread between long- and short-term bonds is smaller and when the default spread is larger. Counties are also more inclined to market new bonds at the same time as other in-counties and in-state municipal authorities. More indebted issuers as well as issuers with wealthier and larger populations are more likely to sell municipal bonds. The global financial crisis has no impact on the decision to market new munis and the panic selling of the Winter 2010 is surprisingly associated with a positive coefficient meaning that issuers were somewhat more active with new issues during the period. Interestingly, counties with a larger share of their population located in high risk floodplains are also more susceptible to issue G.O. munis. However, we observe that the probability to sell a new municipal bond issue stays largely unaffected by flood episodes. Indeed, disaster counties (IsFlood) and counties located less than 100 miles away from the boundaries of disaster counties (Neighbours) are not significantly related to the response variable for all frequencies.

[Insert Table 3.7 around here]

Furthermore, we perform a Wald test to contrast the effect of IsFlood from the effect of Neighbours on the probability to sell a new issue and find no differences at conventional significance levels. The insignificant differences in the probabilities suggest that the issues are not disaster driven. This conclusion holds even if we employ a conditional logistic regression with observations stratified by state or county and is also robust to the inclusion of additional county-level explanatory variables such as the number of housing units (HSGUnits), the general fund surplus (SURPLUS), the interest coverage ratio (INTCOVERAGE) or the total amount of money kept in cash and reserve funds (RESERVE). Thus, as expected given H3, flood events do not appear to impact the decision to issue municipal bonds.

We go one step further and implement a Heckman correction (Heckman, 1979) to validate that our main results do not suffer from selection bias. Following the insights of Grilli & Rampichini (2010), we suppose that the aforementioned logistic model estimated at the annual frequency, which contains no random effects, is an adequate representation for the first-stage equation. We construct the Mills ratio from the logistic model assuming that the errors follow a standard normal distribution and use the inverse Mills ratio as an additional explanatory variable (LAMBDA) in the linear mixed model. Not only does LAMBDA have an insignificant negative coefficient, but the inclusion of LAMBDA has no prominent impact on the coefficients or on the statistical significance of the other regressors.

Flood episodes may arguably affect muni prices through the weakening of issuers' financial situations. H4 investigates the '*rational*' increase in risk channel. Several variables are potential candidates to signal such deterioration in municipal creditworthiness. The most obvious and reliable indicator is probable the credit rating assessed by external agencies. However, about one third of our post-flood sample involves unrated bonds and many municipalities infrequently issue bonds so that changes in ratings cannot be observed. Furthermore, a bond rating can be artificially boosted through incremental full-faith-and-credit pledging, bond insurance or collateralization. Ratings can also be affected by modifications in the way credit agencies model distance to distress (Adelino, Cunha & Ferreira, 2017). Thus, in addition to the credit rating, we also examine for changes in the aforementioned bond characteristics that could be traced back to flood events.

We perform our analysis at the issue level and proceed by calculating the average credit rating[18], the percentage of the issue being insured and the percentage of the issue guaranteed by collateral assets. We identify a total of 277 munis issued by the 45 disaster counties between 2005 and 2015. Among those, 8 counties sold only unrated bonds and 12 additional municipalities have not marketed any bond before major flood episodes. This leaves 213 issues from 25 counties for our analysis.

We observe that the credit rating of 10 out of the remaining 25 issuers stays similar throughout the sample while the rating slightly improves in 8 cases. In one occasion, the post-flood issue is unrated and in another case, the lower rating may be associated with the fact that previous issues are insured while post-flood issues are not. This leaves five counties with, to some extent, inferior credit ratings in the post-flood period. Figure 3.1 illustrates the variations in credit ratings through time for eight representative counties. The vertical lines indicate the timing of the major flood events. Broome NY, Cumberland NJ and Watonwan MN are three of the five counties that exhibit a drop in credit rating during the post-flood period. Hancock OH, Hidalgo, TX and Lycoming PA are three counties that experience an improvement in ratings while Ottawa MI and Putnam TN see no change in their credit ratings following a major flood. While we cannot exclude that flood events may have had a sufficient impact to cause a rating downgrade, such instances appears mostly anecdotal. In most cases, changes in ratings are probably way more easily explained by other local factors. Thus, we reject H4.A.

[18] We first associate a numerical value to each rating that varies between 1 (AAA) to 20 (D) and calculate the simple average of the Moody's, Standard & Poor's and Fitch's ratings for all tranches. Then, we average the ratings at the issue level using outstanding principal amounts as weights for the tranches.

[Insert Figure 3.1 around here]

For its part, our investigation for the recourse to unlimited G.O. (full-faith pledge), bond insurance and collateral bears a no-result as we observe no change at all in bond characteristics that may indicate a higher level of financial distress. As a matter of fact, the few instances of changes in the recourse to insurance following floods can be traced back to the 2008 to 2010 period characterized by changes in the bond insurance market that stem from the global financial crisis (Ely, 2012; Moldogaziev, 2013). Our analysis of the data leads to a reject of H4.B.

Next, we momentarily forget about munis and instead focus on municipal annual financial statements and on local economic indicators that may reveal indirect impacts of flood events on municipal creditworthiness. We examine whether or not local economic indicators or financial numbers for counties are significantly affected by major flood events.

We proceed by constructing an annual dataset that include available economic and financial data for the 1,050 counties in our main sample throughout the 2005 to 2015 period.[19] We require the information on an economic or a financial indicator to be available at least for 5,000 observations out of 11,550 to include that variable in the dataset. We add dummy variables that equal one when a major flood occurred in a county in the year (FLOODYR=0) or in the two years preceding an observation (FLOODYR=+1 and FLOODYR=+2, respectively). The responses variables are built as the annual percentage change in the amount of total debt, in the median household income, in the number of housing units, in the general surplus or in the reserve funds. We proceed with an ordinary least square regression to assess whether or not the changes in the local indicators are impacted by major flood events.

Table 3.8 reports how local flood events help explain the changes in the financial numbers. While flood events have no immediate effect on counties' per capita total debts, disasters lead to a clear and significant increase in debt in the second year following a major flood episode. This finding is robust to the introduction of additional explanatory variables accounting for the level and for the one-year variation in interest rates. In the second column, we see that the variation in the number of housing units is significantly lower in disaster counties in the year following a major flood. This suggests that flood events may impede the progression of

[19] Given that the data come from various sources sampled at different moments in time, we choose to construct our dataset using calendar years. The most popular dates for the counties' fiscal year ends are June 30th (±40 percent), December 31st (±40 percent) and September 30th (±15 percent).

a county's tax base. Unfortunately, lack of data availability prevents us from studying directly the effect of flood events on median housing prices.

[Insert Table 3.8 around here]

We can observe from the third column of Table 3.8 that floods have no meaningful effect on variations in property taxes, but we cannot ascertain whether the similar property tax revenues between the flooded counties and the control group are due to a comparable progression in housing value or to higher property tax rates. The fourth column reveals that floods have an especially strong negative impact on general fund surpluses in the second year following a major disaster. Furthermore, we notice that IsFlood is negatively related, although not statistically significant, to the progression of unassigned reserve funds. In unreported results, we find that floods have no impact on total general revenues, sales tax revenues or income tax revenues. Flood episodes also have an insignificant impact on median household income. Taken together, the larger growth in per capita debt and the pronounced decline in general fund surpluses two year following a disaster suggest that floods do have an impact on the credit worthiness of municipal issuers and that higher levels of credit risk may contribute to the flood-induced price discount so that H4.C cannot be rejected.

The last section of our examination about an increase in the level of credit risk caused by a flood event rests on the effect of presidential major disaster declarations.[20] When the U.S. President declares that a major disaster exists in a county, federal funding becomes available to local governments for the repair or replacement of infrastructures damaged by disasters. The availability of federal aid should lessen the financial stress generated by floods for the municipal authorities. We extract the list of declared counties from FEMA's disaster declaration summary which is part of the U.S. Government's open data. One source of concern is that FEMA's disaster definitions are less detailed than – and significantly different from – the NCEI definitions. As a consequence, many events tagged as 'floods' in the NCEI dataset when floods follow hurricanes or storms are simply labelled as 'severe storms' or 'hurricane' or 'snow' in the FEMA database. Also declared major disasters are not necessarily the biggest ones in terms of damage. We address this issue by examining first the effect of all disaster declarations before segmenting the declarations according to whether or not the declaration involves a hurricane. Not only are hurricanes often associated with the largest amount of damage, but they also

[20] Information regarding the declaration process can be found on FEMA's website.

possibly generate the most intense media coverage at the national level and the greatest perception of havoc.

Table 3.9 presents the results of the LMM. We see that 51 out of the 65 post-flood issues were sold in months following a major disaster declaration. In fact, almost one issue out of four in our main sample was marketed in a 12-month period subsequent to the issuer being declared in a disaster area. The inclusion of variables accounting for all declared disasters somewhat blurs the picture. The flood-induced yield premium is distributed between IsFlood and the interaction term. Although not statistically significant, the fact that the interaction term obtains a positive coefficient suggests that federal funds originating from presidential disaster declarations do not reduce the additional financial burden for county authorities. A disaster declaration by itself seems to have no impact on issue yields of munis. Next, we control for the category of disasters that generate the presidential declaration by distinguishing hurricane from other extreme weather events. The focus on hurricanes produces greatly different results. New issues sold following a hurricane are associated with a highly significant price discount that is about half the magnitude of the discount caused by floods previously observed in our main results. The fact that another category of disasters, originating from a different source of data and with an event sample size thrice as large, also engender higher issue yields reinforces our overall findings. We note that the flood-related yield premium stay positive and significant when we account for hurricanes. The positive, yet insignificant, interaction term indicates that the flood premium is larger following a hurricane than following other weather events. The results from the last column on non-hurricane declarations are a bit surprising. New issues sold in the wake of a major disaster declaration unrelated to a hurricane exhibit a significantly lower yield on average than other issues. IsFlood remains similar whether or not we account for non-hurricane declarations. This suggests that disasters are not all made equal in terms of impact on municipal finance. Indeed, while hurricanes and floods appear to increase municipal financing costs, the declaration of other severe weather events instead shows a negative correlation with yields. The downward pressure on issue yields following non-flood and non-hurricane declared disasters may indicate that some counties benefit from federal aid funds or may reflect investors' anticipation of a silver lining period (Kliesen, 1994). Of particular interest for us, the interaction term between IsFlood and NON-HURRICANE is indistinguishably different from zero. Thus, our assumption that presidential declarations lessen flood-induced financial stress is not satisfied, which leads to a rejection of H4.D.

[Insert Table 3.9 around here]

All in all, the evidence regarding an increase in credit risk following floods is mixed. On the one hand, our results show a sharp increase in total debt and a net reduction in general fund surplus two years following disasters that are consistent with lower municipal creditworthiness. On the other hand, we note more occurrences of credit rating upgrades than downgrades following floods and observe that presidential declarations have no effect on the magnitude of the price discount. One plausible explanation is that floods have more pronounced, more difficult to assess or longer-lasting consequences than other types of natural disasters on municipalities. If this is the case, then it is likely that investors associate higher credit risk with post-flood issuers.

Besides credit risk, major flood events may exacerbate liquidity risk. We define liquidity risk as the difficulty to convert municipal bonds into cash without giving up capital due to a lack of marketability. We assume that the loss in capital resulting from a less liquid issue leads to an increase in the bid-ask spread as well as to higher commissions and to greater price impact costs. Accordingly, we use the roundtrip transaction costs indicator of (Green, Hollifield & Schürhoff, 2007b) that not only encompass intermediation costs, but also reflect the relative bargaining power of dealers with their customers. While some previous papers have studied the effect of idiosyncratic events on transaction costs in the municipal bond market,[21] we are aware of no literature related to the impact of natural disasters on liquidity in the municipal bond market.

We construct a dataset from the MSRB database of all seasoned transactions of G.O. munis sold by county authorities. Following Green, Hollifield & Schürhoff (2007b), we distinguish between new issues and seasoned issues. The trading activity is usually much higher in the weeks following the sale date than in the remaining life of the municipal bonds. In the new issues period, the sales of municipal bonds to customers greatly exceed the buys and this could obscure or mislead the analysis of roundtrip transaction costs. Accordingly, we focus on seasoned trades, i.e. trades where the difference between the trade date and the sale date is greater than 14 days.[22] While the MSRB dataset does not directly pair buy and sell transactions,

[21] Green, Li & Schürhoff (2010) find no evidence of a significance change in trading activity following macroeconomic announcements. Ciampi & Zitzewitz (2010) find higher trading costs in the midst of the 2008 global financial crisis. Chalmers, Liu & Wang (2016) show that trading costs of municipal bonds significantly decline following the introduction of the Real-Time Report System (RTRS) in 2005.

[22] As the two legs of a roundtrip transaction are not identified in the MSRB database, the new issues period is subject to many incorrect matches. This is due to many sell orders that do not originate from

we match purchases from customers with sales to customers in the same bond, for the same par amounts using a 'first-in-first-out' (FIFO) approach. We require the purchases and sales dates to be separated by no more than seven calendar days to consider the transactions as part of the same economic exchange. Roundtrip transaction costs equal the difference between the sale and purchase prices and can be expressed either in a percentage of the purchase price or in basis points. The resulting sample contains 334,208 estimates of trade costs on 52,502 tranches dispersed across 6,444 issues sold by 1,171 counties. We identify 2,553 transactions on 87 issues that occur in the year following a major flood.

We merge the transaction cost dataset with the control variables listed in Table 3.1 and estimate the LMM at the transaction-level. We exclude two explanatory variables, COMPETITIVE and UW_OTH, that relate to issuance characteristics and instead add the principal amount traded (TRADEDAMT) and the number of intermediate trades between dealers that separate the purchases from the sales (NBDEALERS) to the list of controls. We display the results for the effects of floods on roundtrip transaction costs in Table 3.10.

[Insert Table 3.10 around here]

Findings are similar whether trade costs are expressed in percentages of the purchase prices or in basis points. Consistent with the previous literature, we notice that higher traded amounts lower trade costs while NBDEALDERS is positively correlated with costs. The highly significant relationship between trade costs and one of our measures of flood risk, %POP, is a bit puzzling as a higher share of a county's population residing in a high risk flood zone seems to lead to lower trading costs. However, we observe that %POP strongly interacts with TRADEDAMT. The addition of an interaction term between %POP and TRADEDAMT completely removes the significance of %POP. The insignificant coefficient associated with our main variable of interest IsFlood informs that flood events bring no meaningful differences in transaction costs. Hence, we reject H4.D.

Last, we assess the merits of the behavioral channel and of its related empirical predictions described by H5. Table 3.11 presents the effects of flood events over time. In the first four columns, we observe that the impact of major flood episodes is limited to bonds marketed in the months following the disaster. This finding is consistent with the amnesia story advocating a

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purchases from customers and it leads to roundtrip-transactions that sometime combine tens of buys or sells.

temporary effect. The negative coefficient of -0.038 associated with IsFlood for munis sold between 13 to 24 months following an event is a bit surprising but the lack of significance suggests that there is a lot of variability between post-flood issues. Thus, the results are consistent with H5.A.

[Insert Table 3.11 around here]

In the last column of Table 3.11, we identify 274 new municipal bond issues sold by counties in the 60-month period following a major flood and we distinguish the impact of a flood and of the time elapse since a flood between first-time (Virgin) and repeat disaster (Experienced) counties. As predicted by the availability bias, the effect of flooding is much stronger for first-time flooded counties. Indeed, we find a positive and somewhat significant yield premium associated with IsFlood for first-time disaster area while the negative coefficient for the time-since-flooding variable indicates that the effect of flooding weakens over time. The abnormal yield premium is absent from counties that experience several occurrences of major flooding. This result supports our expectation for H5.B.

Thus, while the post-flood increases in municipal financing costs may arise in part because of an increase in credit risk, the behavioral channel is clearly the one for which we obtain the most supportive evidence.

3.6 FURTHER EMPIRICAL CONSIDERATIONS

As in any type of empirical study, ours faces some limitations. A first limitation arises from our implicit assumption that the flood zone delineations are constant throughout our sample. Therefore, our ex-ante measures of flood risk may not match the information available to investors at the time of bond issuance and the IsFlood variable may capture some of the flood risk premium. This may lead to an underestimation of the effect of the flood risk variables and could amplify the observed yield premium. We attempted to minimize this issue by building up four flood risk variables including two based on past flood episodes that do not suffer from this limitation. In unreported results, we also separate our sample in two subperiods and observe a positive yield premium of 4.5 and 6.1 percent in the 2005 to 2010 and the 2011 to 2015 subperiods, respectively. Yet, the small number of post-flood issues in both subsamples renders the IsFlood variable insignificant with Hubert-White *p*-values of about 0.20.

A second potential limitation arises from the inaccuracy of the flood damage estimates that prevents us from relying much on these numbers to deepen our understanding of the consequences of floods on municipal finances. In all of our tests, we used a damage threshold of \$100 in per capita constant 2015 dollars to identify major floods. This choice of threshold reflects a trade-off between the size of the event sample and the exclusion of smaller events that have arguably no impact on municipal financing costs. We investigate the effect of various damage thresholds on the price premium in Table 3.12. We notice that a lower damage threshold increases the size of the event sample but the additional events do not generate an issue yield premium. As a matter of fact, this analysis suggests that the price discount may be positively correlated with the magnitude of a disaster, but better damage estimates are needed to confirm this relationship.

[Insert Table 3.12 around here]

3.7 CONCLUSION

In this paper, we examine whether or not major flood episodes impact new municipal bond issue yields exploiting the fact that the state-level segmentation of the municipal security markets plausibly makes municipal bonds more sensitive than other assets to such disasters. In line with the literature, we examine several channels through which flood events could affect bond prices, including (i) a variation in the propensity to issue new bonds, (ii) a surge in credit risk, (iii) a drop in market liquidity, and (iv) a behavioral explanation based on the availability bias.

Our results provide evidence consistent with a meaningful flood effect as municipal bonds sold in the months following a catastrophe exhibit significantly higher issue yields that cannot be explained either by *ex*-ante measures of flood risk, or by differences in issues' characteristics. Moreover, our analysis suggests that issues post-flood are not disaster driven. For an average \$10 million issue, the flood-induced increase in yields translates into a loss of about \$100,000 in terms of bond proceeds. We also observe higher yields for municipal bonds sold by authorities in presidential declared disaster areas following hurricanes. Yet, the magnitude of the price discount appears larger for post-flood than for post-hurricane issues.

Our analyses also inform on why flood events affect bond yields. We find mixed evidence of an increase in credit risk and no material flood-related effect on roundtrip transactions costs which makes a drop in market liquidity unlikely. However, we observe that the higher borrowing costs rapidly fade away over time and are limited to regions that experienced a major flood for the first time. These findings are consistent with a behavioral explanation along the lines of the availability bias of Tversky & Kahneman (1973) and of the myopia and amnesia story of Pryce, Chen & Galster (2011).

Besides the particular interests of the findings for municipal managers and investors, we believe that the conclusions of this paper are also of interest from an academic perspective. Indeed, the results suggest that the response of municipal bonds to major floods shares more similarities with the way residential housing reacts to such idiosyncratic events than with how stocks are affected by such disasters.

CHAPTER 4

Disasters and Risk Aversion: Evidence from the Municipal Bond Market

4.1 INTRODUCTION

Risk aversion can be defined as the tendency of investors to avoid undertaking risks and to choose less risky alternatives.[23] There is a large body of psychological, behavioral and economic literature devoted to the identification of the determinants of risk aversion or to the estimation of the level of financial risk aversion. Recent years have seen a surge in interest in the relationship between natural disasters and decision-making under uncertainty.[24] The findings of those papers support the hypothesis that dreadful experiences, such as enduring a natural disaster, reduce risk taking.

A lesser propensity to take risks following catastrophes has major economic implications. Among others, (i) individuals may take suboptimal investment decisions or refrain from opening new businesses or from exploring new technologies. In turn, this can dampen the stimulus effect of disaster financial assistance programs, slow the recovery phase and entail lower economic growth. (ii) Investors may require higher expected returns on assets for which regional risks cannot be properly diversified which could increase the financing cost for states and local governments and also impact the local housing market. (iii) Taxpayers might become more inclined to invest in risk management programs and structures as extreme weather events get more frequent and more severe. Yet, the evidence provided by the current literature favouring a disaster-driven increase in risk aversion are almost exclusively drawn from surveys and experiments rather than from analyses of real-world financial transaction data. Therefore, whether or not financial assets are priced in a way that is consistent with an increase in risk aversion instilled by natural disasters remains an elusive question.

In this paper, we exploit the geographic segmentation of the U.S. municipal bond market to examine how extreme weather events affect the level of risk aversion implied by asset

23 Collins Dictionary of Economics, 4th ed.. S.v. "risk aversion." Retrieved July 5, 2017 from http://financial-dictionary.thefreedictionary.com/risk+aversion

24 Examples include: Bernile, Bhagwat & Rau, 2017; Bucciol & Zarri, 2013; Cameron & Shah, 2015; Cassar, Healy & von Kessler, 2017; Goebel, Krekel, Tiefenbach & Ziebarth, 2015; Petrolia, Landry & Coble, 2013; Stewart, Ellingwood & Mueller, 2011 and Van den Berg, Fort & Burger, 2009.

returns. Unlike most studies in the asset pricing literature, we perform our empirical analysis at the state-level using data on regional asset returns and on regional consumption. We draw our main findings from an event study approach and confirm the validity of our conclusions by estimating a parameter of disaster-induced risk aversion directly from the general consumption-based capital asset pricing model (CCAPM). We obtain state-level risk aversion estimates that vary between 0.02 and 9.45 (mean of 4.22) that are not only lower than those obtained in some previous tests of the complete-market CCAPM (e.g. Mehra & Prescott, 1985) but are compatible with the level of RRA predicted by Arrow (1971). We underline the practical implications of the geographic heterogeneity in risk taking behavior documented in this paper. Regional differences in risk aversion imply that fiscal incentives for private investment in risk management, housing, small businesses, etc., can be expected to differ in their regional effectiveness. As minor contributions, we also discuss the construction of municipal bond price indices following a repeated sales approach and address the issue of the determinants of risk aversion.

Given the importance of efficient financial markets on economic development (Levine, 1997), as well as the foreseen increase in frequency and intensity of extreme weather events, we consider that this study is of interest for a well-diversified audience embracing academicians and practitioners from the fields of public economics, regional science, risk management and asset pricing.

The rest of the paper is organized as follows. Section 4.2 explains the theoretical foundations underlying (i) the relationship between natural disasters and risk taking, (ii) the connection between risk aversion and asset prices and (iii) the segmentation of the municipal bond market. Section 4.3 describes the data and details the construction of the municipal bond price indexes. Section 4.4 outlines the asset pricing model from which the level of risk aversion is estimated and defines the event study framework. Section 4.5 displays and discusses the results from the empirical analysis. Section 4.6 concludes.

4.2 THEORETICAL BACKGROUND

4.2.1 Natural disasters and risk taking

In the last 50 years or so, the dominant paradigm in the field of human decision making under uncertainty has been the expected utility (EU) theory. EU postulates that an investor's decision process follows a rational assessment of the expected outcomes of available alternatives. The valuation of the possible outcomes varies between individuals as each may weight differently objective and subjective decision factors. In its standard form, EU implicitly assumes that individuals have stable risk preferences (or risk aversion) and that beliefs about the probability and the severity of alternative outcomes follow a Bayesian updating process.

Previous literature mostly supports the stable risk preference assumption. Bouchard & McGue (2003) find that individual psychological differences are "*moderately to substantially*" hereditary. This conclusion is shared by Zyphur, Narayanan, Arvey & Alexander (2009), Cesarini, Johannesson, Lichtenstein, Sandewall & Wallace (2010) and Sahm (2012), among others. At the same time, the literature provides some evidence opposing stable risk aversion and favouring time-variation in risk preferences (Barberis, Huang & Santos, 2001; Campbell & Cochrane, 1999; Chue, 2002; Guiso, Sapienza & Zingales, 2017; Kamstra, Kramer, Levi & Wang, 2014).

Besides the genetic determinants of risk aversion, several environmental factors are also found to impact risk preferences and may help explain time variations. In their study of depression babies, Malmendier & Nagel (2011) observe that individuals having experienced high stock market returns in the past report lower risk aversion and that the effect of past economic events fades only slowly with time. Bucciol & Zarri (2013) and Kim & Lee (2014) obtain similar findings using past traumatic events such as the loss of a child, having been in a natural disaster or having been extensively affected by war. These major life events are shown to increase risk aversion in a mostly permanent way. Bernile, Bhagwat & Rau (2017) corroborate this conclusion for disasters with extreme consequences but observe that low-intensity disasters desensitize decision-makers to the negative consequences of risk.

While extreme weather events wreak havoc locally, their upmost consequences are usually narrowed to a relatively small share of the population.[25] Thus, it appears unlikely that natural disasters would trigger a traumatic reaction to a large enough number of individuals such that the average investor in a financial market exhibit a surge in risk aversion, especially when we take into account that the relative impact of disasters is generally greater for low-income households (Masozera, Bailey & Kerchner, 2007) and that it has been shown that market participation is strongly and positively correlated with household wealth (Vissing-Jørgensen, 2002). Consequently, the explanations for a disaster-induced surge in aggregate risk aversion

[25] Among a sample of individuals severely affected by a disaster, Steinglass & Gerrity (1990) find that between 15 and 20 percent of the persons studied reported symptoms of posttraumatic stress disorders.

most likely lay outside the standard EU framework. We identify two interrelated theories[26] that are consistent with a reduction in financial risk taking behavior.

First, the availability heuristic described by Tversky & Kahneman (1974) informs that individuals are inclined to judge the probability of an event based on instances that come readily to mind. Indeed, people tend to overestimate the probability of rare, highly publicised events, particularly in periods following their occurrence (Dinman, 1985; Hertwig, Barron, Weber & Erev, 2004).[27] In addition, Tversky & Kahneman (1992)'s cumulative prospect theory suggests that individuals commonly overweight tail events in their decision-making relative to the EU theory which compounds the plausible effect of a disaster on risk taking behavior. Asgary & Levy (2009) advocate the use of prospect theory to model decision making following natural disasters.

Second, the risk-as-feelings theory of Loewenstein, Weber, Hsee & Welch (2001) posits that "*responses to risky situations result in part from direct emotional influences".* According to this theory, emotions are complementary to the cognitive assessment of the uncertain outcomes in the decision process. That emotions influence judgement and choices is consistent with evidence presented by Lopes (1987), Lerner & Keltner (2000), Eckel, El-Gamal & Wilson (2009) and others. Kuhnen & Knutson (2011) confirm that negative emotions induce investors to take fewer risks in the context of financial decisions and Guiso, Sapienza & Zingales (2017) demonstrate that time-variation in the level of risk aversion in the wake of the global financial crisis better matches an emotional response story than explanations based on variations in beliefs, in wealth or in total habit.

The importance of emotions is put forward in many papers that study the impact of extreme weather events on risk aversion, although some remain agnostics about the cause of the relationship. Smith (2008) corroborates that extreme weather events alter subjective risk perceptions in the general population. He finds evidence of lower optimism and higher risk aversion. Van den Berg, Fort & Burger (2009) concur that the occurrence of a disaster causes

[26] Many studies relate the risk aversion and loss aversion factors of the prospect theory to emotions (e.g. (Campos-Vazquez & Cuilty, 2014; Farnham, 1992))

[27] We acknowledge that many studies do not control for media coverage and report different findings. For example, the Insurance Research Council (1996) argues that people underestimate the likelihood of natural disasters unless an event has occurred relatively recently. For the purpose of this study, an upward revision of the probability of a disaster has the same effect as a temporary overestimation of the likelihood of a tail event.

an increase in risk aversion and mention the risk-as-feelings channel. Petrolia, Landry & Coble (2013) assert that experiencing a disaster heightens sensitivity to risk and argue that models of decision making under uncertainty that ignore subjective risk factors likely suffer from omitted variable bias. Goebel, Krekel, Tiefenbach & Ziebarth (2015) show that a significant proportion of people informed about a disaster that are directly affected by it exhibit an increase in risk aversion. Cameron & Shah (2015) observe that extreme weather events increase individual risk aversion as well as the real-life prevalence of insurance measured at the community level. Cassar, Healy & von Kessler (2017) control for regional demographic characteristics, distance to high risk areas and post-disasters migration and also find clear evidence that individuals hit by disasters display higher risk aversion than individuals in the control group. Interestingly, they estimate the increase in risk aversion to be roughly 20 percent.

4.2.2 Risk preferences and asset prices

The dominant framework that describes how asset prices are set is an application of the EU theory. According to the theory, prices "*should equal expected discounted value of the asset's payoff, using the investor's marginal utility to discount the payoff*" (Cochrane, 2009 p.3). Given that the *true* investor's utility is unobservable, the asset pricing literature provides a great variety of mathematical representations for investors' utility functions and often expresses utility as a function of consumption. The various structural forms associated with utility functions are increasing in consumption given investor's non-satiation and usually concave given that investors are assumed to be risk averse, at least for large stakes (Rabin, 2000). The fundamental asset pricing equation can be written as:

$$\mathrm{P}_t = E\left[\beta \frac{U'(C_{t+1})}{U'(C_t)} \mathrm{X}_{\mathrm{t+1}} \middle| \Omega_{\mathrm{t}}\right] \tag{4.1}$$

where P_t is the price of an asset at time t, β is the subjective discount factor depicting investor impatience, $U'(\cdot)$ is an investor' marginal utility defined over the current value of consumption C_t and future values of consumption C_{t+1}. $\mathrm{X}_{\mathrm{t+1}}$ is the payoff of the asset and Ω_{t} is the information set that is known to the investor when making his investment decision. Given that future consumption and future asset payoffs are uncertain, beliefs about the prospective states of the world can implicitly be interpreted as weights for the expectation operator $E[\cdot\,]$.

Optimal investment decisions entail a trade-off between the (known) loss in utility associated with the purchase of a unit of an asset that reduces consumption at time 0 and the (uncertain) increase in utility related to the additional payoff of the asset that will augment consumption at time 1. In the most parsimonious models, risk preferences enter Equation (4.1) in the form of a constant risk aversion parameter embedded in the utility function. In concordance with basic economic intuition, an increase in the risk aversion parameter lowers the utility of the uncertain future asset payoff. This decreases the optimal number of shares that an investor should buy and leads to a drop in the asset price.

Some additional assumptions are generally made in order to employ an asset pricing model such as the one described in Equation (4.1). First, although streams of literature recognize and make use of the investor's heterogeneity (Brav, Constantinides & Geczy, 2002; Chan & Kogan, 2002; Constantinides & Duffie, 1996; Jacobs, Pallage & Robe, 2013; Sarkissian, 2003), it is common to assume the existence of a representative investor. Second, most research also assumes that financial markets are in equilibrium. Recall that the individual preferences of investors cannot be observed. In fact, the exercise is one of model fitting where historical data on the market value of the assets and on consumption are used to estimate the values of the impatience and risk aversion parameters such that departures from Equation (4.1) are minimal. In this respect, the representative investor and the market equilibrium assumptions ensure that the market value of assets follows Equation (4.1) and comfort the use of aggregate consumption data that are more easily available and less noisy than estimates of individual consumption. As a result, the measure of risk aversion inferred from asset prices can be seen as a useful benchmark of average risk preferences in a market over time. In the context of the fundamental asset pricing equation, disaster-driven changes in risk aversion can be detected through an event study of the model's residuals.

4.2.3 Municipal bond market and market segmentation

Previous discussions suggest that a large proportion of the investors trading an asset need to be affected by a disaster, whether cognitively or emotionally, before an increase in risk aversion becomes perceptible at the representative investor level. This condition may not be easily met in all financial markets. Indeed, little evidence supporting a disaster-driven price impact has been found in stock markets (Bourdeau-Brien & Kryzanowski, 2017; Ferreira & Karali, 2015; Wang & Kutan, 2013; Worthington, 2008; Worthington & Valadkhani, 2005) while the housing literature mostly agrees about the existence of a flood-related price discount for residential properties (Bin, Kruse & Landry, 2008; Harrison, Smersh & Schwartz, 2001; Lamond & Proverbs, 2006; Lamond, Proverbs & Hammond, 2010; Shilling, Sirmans & Benjamin, 1989).[28] Among other things, these

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[28] See, however, the insurance literature reviewed by Perrakis & Boloorforoosh (2018), in which the observed prices of disaster events are strongly inconsistent with the diversifiability of disaster risk.

two financial markets differ in their respective geographic size. Although the literature on investor heterogeneity (Jacobs, Pallage & Robe, 2013; Korniotis & Kumar, 2011) and on the local home bias (Coval & Moskowitz, 1999; Huberman, 2001) provides, to some extent, evidence of regional stock market segmentation within the U.S., the housing market is often considered as much more regionally segmented (Goodman & Thibodeau, 1998) than the U.S. stock market. However, the fact that the payoffs from an investment in the housing market are not easily measurable[29] and that data on home sales through time across a variety of regions cannot easily be obtained makes it impracticable to carry out a study of risk aversion in the housing market.

Geographic market segmentation is certainly a desirable feature in order to detect disaster-driven changes in risk aversion. Indeed, as the consequences of extreme weather events are primarily regional (C. T. West & Lenze, 1994), assets whose investor base is predominantly constituted of local individuals are more likely to reveal a representative investor affected by a disaster. Put differently, disaster-induced changes in risk aversion need to be examined at the local level by calibrating the risk preferences of the average investor at a regional level. In this paper, we define regions along U.S. state geographical limits as this allows for the use of many available regional variables including returns on assets that are traded locally and measures of consumption. We rationalize our assumption that risk aversion varies across states by noting that several key components of the investors' environment, such as the laws or regulations and most of the welfare policies that influence precautionary saving and income risk policy (Bird & Hagstrom, 1999), are not only shared by investors at the state-level, but differ significantly from state to state. Aforementioned studies suggest that investors' risk preferences are influenced by their experiences and environment.

Another financial market where payoffs are well defined and trading is segmented along clear-cut geographic limits is the municipal bond market. Previous literature suggests that the municipal bond market is geographically segmented at the state-level due to asymmetric tax exemptions (Pirinsky & Wang, 2011), to the local home bias (Greer & Denison, 2014), to differences in relative security supplies (Hendershott & Kidwell, 1978; Kidwell, Koch & Stock, 1987) and to disparities in information costs (Feroz & Wilson, 1992). The aforementioned studies all focus on new municipal bond issues and present evidence of regional variations in issue

[29] For example, Piazzesi, Schneider & Tuzel (2007) note that payoffs from housing include utility from ownership as well as other kinds of dividends.

yields[30] consistent with geographic segmentation. Furthermore, the fact that many of the most important buyers of fixed income assets at the national level, such as pension funds, life insurance companies and foreign banks, generally do not benefit from the tax exemption on municipal bonds (Feldstein & Fabozzi, 2008) warrants that municipal bonds are concentrated in the hands of individual investors (Elmer, 2014; US Security Exchange Commission, 2012) and explains why local retail investors dominate the trading of tax-exempt municipal bonds (Ang, Bhansali & Xing, 2010). In summary, the municipal bond market combines the characteristics needed to empirically investigate changes in risk aversion at the local level caused by natural disasters.

Although the majority of asset pricing studies have used equity portfolios as test assets, we maintain that inferring investor preferences from municipal bonds has some non-negligible advantages. First, Vissing-Jørgensen & Attanasio (2003) and others offer strong evidence that asset pricing models should be tested using after-tax returns. Unlike most other categories of assets, municipal bonds provide a clear view of after-tax returns. Second, since we cannot observe investors' true expectations, empirical tests of asset pricing models assume that ex-post realized returns are a good proxy for the ex-ante expectation of the next-period returns (e.g. Donaldson & Kamstra, 1996). We argue that a larger proportion of the returns from municipal bonds than from most other financial assets is expectable given the fairly assured coupon payments, the relatively low incidence of defaults and the fact that default risk accounts for more than three-quarter of the average bond spread (Schwert, 2017). Finally, Campbell (1980) reports evidence consistent with the fact that the municipal bond market is efficient, and Fischer (1983) finds that yields quickly reflect new information even for infrequently traded bonds.

4.3 DATA

In this section, we outline the three main datasets containing information on natural disasters, state-level consumption and municipal bond price indexes, respectively. We limit ourselves to the January 2005 to December 2016 period for which municipal bond transaction data are available and use data sampled at a monthly interval in order to maximize the power of the tests.

[30] The definition of issue yields varies from one study to the other and encompasses reoffering yield, net interest cost (NIC), true interest cost (TIC) and Salomon Brothers' analytical record of yields on newly issued bonds.

4.3.1 Extreme weather events

The data on natural disasters come from the storm events database of the National Center for Environmental Information (NCEI). That database contains information on 48 mutually exclusive types of natural disasters with detailed technical definitions. The NCEI database provides damage estimates for most events that may come from insurance companies, from other qualified individuals or from 'guesstimations'. We acknowledge that NCEI's damage estimates for individual events may be subject to large measurement errors. Gall, Borden & Cutter (2009) document six types of estimation biases that include the over- (under-) representation of flood (drought) events due to data collection procedures and the difficulty to compare data over time due to improvement in loss accounting. Downton, Miller & Pielke (2005) observe that while the damage estimates are particularly inaccurate for smaller floods and smaller areas, they tend to average out over larger geographies. Our study alleviates most of these biases by aggregating disasters losses at the state level and by focussing exclusively on recent observations from the 2005 to 2016 period. The inaccuracy of the damage estimates for small events is not a big concern given that our analysis relies on a continuous variable that sums the monthly state-level loss per capita (DISASTER) rather than on a binomial indicator expressing the occurrence of a natural disaster. The continuous variable seems appropriate as there is no exact damage (or live loss) thresholds from which a weather event is considered a natural disaster. For its part, the normalisation of the damage estimates on a per capita basis helps in comparing states that greatly differ in terms of population so that DISASTER represents the average loss for the representative investor.

Table 4.1 displays the summary statistics for DISASTER. We observe a large discrepancy between median and mean monthly per capital losses that denotes that a handful of weather events are of exceptional magnitude. Most states suffer from at least one disaster-month with total damages in excess of $25 per capita.[31] The state-level disaster series exhibit weak cross-sectional correlations. The average pairwise correlation is 0.08 and the median correlation is 0.05. The lowest pairwise correlation is -0.01 (South Dakota – Louisiana) and we find two pairs with correlation in excess of 0.7 (Louisiana – Mississippi at ρ=0.72 and Pennsylvania – New York at ρ=0.74).

[Insert Table 4.1 around here]

[31] Notable exceptions include Connecticut, New Hampshire and Washington where the costliest disaster-month shows total per capita losses of $7.85, $18.27 and $22.37, respectively.

4.3.2 State-level consumption

Consumption data are central to empirical tests of CCAPM. However, the search for a suitable proxy for local consumption is challenging as no official statistics are available at the state level on personal consumption expenditures (PCE) or on consumption of non-durable goods and services (NDS). Recent publications have employed data on electricity consumption as a way to overcome the issues related to retail sales data and to explain asset prices. Da & Yun (2010) find that electricity consumption works better than NDS in CCAPM. Da, Yang & Yun (2015) use residential electricity usage to proxy for the service flow from households in order to explain stock returns. Da, Huang & Yun (2017) show that the growth rate of industrial electricity usage predicts future stock returns. Often cited advantages of using electricity consumption over traditional measures of personal consumption include the facts that electricity consumption is (i) measured very precisely, (ii) measured with no delay and (iii) consumed over the life of the product (Do, Lin & Molnár, 2016). In the U.S., data on electricity consumption is available monthly by state and by user type through the Electric Power Industry Report of the U.S. Energy Information Administration. Appendix C gives details on the construction of the state-level gross electricity consumption series (ELECT).

Table 4.2 presents summary statistics on the monthly consumption growth series of selected states. By construction, the average monthly consumption growth is close to one. ELECT ranges between 0.90 and 1.14 and the first four moments of its distribution exhibit significant cross-state variations. The cross-state correlation varies between -0.25 (Florida – Rhode Island) and 0.88 (North Carolina – South Carolina) while the average pairwise correlation across states reaches 0.35. This somewhat weak correlation suggests that investor's consumption patterns are not synchronized as expected across states.

[Insert Table 4.2 around here]

4.3.3 Municipal bond returns

We obtain data on transactions of municipal bonds between January 5th, 2005 and December 30th, 2016 from the Municipal Securities Rulemaking Board (MSRB) transaction database. We begin by matching bond purchases with associated sales to extract the mid-price of each round-trip transaction. We then compute total return prices (TRP) by adjusting clean mid-prices for accrued interest and coupon payments. The adjustment is similar in spirit to the way stock prices are commonly adjusted for splits and dividends. Next, we cluster TRP by state and time-to-maturity groups in order to build four maturity-based portfolios per state. Last, we employ the S&P/Case-Shiller Home price index methodology of Shiller (1991) to construct state-level monthly asset returns indices from the TRPs. Appendix D details the construction of the state-level municipal bond repeated-sales indices (RSI).

We succeed in estimating RSIs in all states. However, the estimates for the states of Alaska, Hawaii and Wyoming, as well as for the District of Columbia, are much more volatile than those of the other states and sometime contain missing monthly estimates. As a consequence of the somewhat poor model fit, we exclude these regions from our analysis.

Table 4.3 shows summary statistics for the monthly returns of the aggregate state-level municipal bond RSI of selected states. We see that the mean return is about the same across states but the return series show more divergence in terms of higher moments. The cross-state correlation varies between 0.26 (Minnesota – Delaware) and 0.98 (New-York – New-Jersey) while the average pairwise correlation across states reaches 0.78. The relatively high average correlation across states is not much of a surprise given that the prices of municipal bonds arguably depend on common factors such as the level of interest rates and the shape of the treasury yield curve.[32] Interestingly, the average cross-state correlation is larger for portfolios of short-term bonds ($\bar{\rho}$ = 0.81 for bonds maturing in less than 2.5 years) but drastically diminish for the portfolios of bonds distant from maturity. Indeed, the average correlation drops to 0.10 for RSIs estimated with bonds having a remaining life greater than 7.5 years.

[Insert Table 4.3 around here]

4.4 MODELLING FRAMEWORK

4.4.1 Consumption-based capital asset pricing model

As mentioned earlier, the fundamental asset pricing equation described by Equation (4.1) can be used to infer the rate of risk aversion from data on consumption and on asset prices provided that we assume a functional form for the utility function. In line with many influential papers in the asset pricing literature (Breeden & Litzenberger, 1978; L. P. Hansen & Singleton, 1983; Mehra & Prescott, 1985), we assume that investors exhibit time-separable constant relative risk aversion (CRRA) and opt for the convenient power utility function $U(C_t) = \frac{C_t^{1-\gamma}}{1-\gamma}$ where γ is a parameter

[32] We take into account these common factors later in robustness tests.

that measures the rate of relative risk aversion (RRA).[33] Adding the power utility function to Equation (4.1) and dividing both sides of the equations by P_t, the price of the asset, we obtain the following equation for the consumption-based capital asset pricing model (CCAPM):

$$1 = \mathrm{E}\left[\beta \frac{\mathrm{C}_{t+1}}{\mathrm{C}_t}^{-\gamma} \mathrm{R}_{t+1} \,|\Omega_t\right] \tag{4.2a}$$

We see from Equation (4.2a) that the choice of a power utility function helps in keeping the fundamental asset pricing equation simple and tractable. Indeed, the ratio of marginal utilities $\frac{U'(C_{t+1})}{U'(C_t)}$ simply equals $\frac{\mathrm{C}_{t+1}}{\mathrm{C}_t}^{-\gamma}$, where $\frac{\mathrm{C}_{t+1}}{\mathrm{C}_t}$ corresponds to ELECT, the growth in consumption. The division by P_t allows expressing the equation in terms of gross returns $\mathrm{R}_{t+1} = \frac{\mathrm{X}_{t+1}}{\mathrm{P}_t}$ rather than of payoffs. Gross asset returns are generally preferred over asset prices because they are typically stationary over time.

One issue with Equation (4.2a) is that we do not observe an investor's information set and are unable to explicitly model Ω_t. For simplicity, we follow Brav, Constantinides & Geczy (2002) and Jacobs, Pallage & Robe (2013) and focus on unconditional models. That is, we consider that the information set contains only a constant equal to one. We also assume that β, the impatience parameter, equals one. In this respect, Jacobs, Pallage & Robe (2013) argue that setting $\beta = 1$ scales the pricing errors but does not bias the estimation of γ.

We use a generalized method of moments (GMM) procedure to estimate the risk aversion parameter γ implied by historical data. Equation (4.2a) says that expected discounted returns should always be the same. Accordingly, pricing errors can be defined as:

$$u_t(\gamma) = \mathrm{m}_t \, \mathrm{R}_t - 1 \tag{4.2b}$$

where $\mathrm{m}_t = \beta \frac{\mathrm{C}_t}{\mathrm{C}_{t-1}}^{-\gamma}$ is referred to as the stochastic discount factor.

As we assume that the municipal bond market is segmented at the state-level, we estimate the value of γ on a state-by-state basis using state-level aggregate consumption data and the return on four diversified portfolios of municipal bonds classified according to their time-to-maturities issued by in-state local authorities. Accordingly, we obtain four moment conditions,

[33] The parameter γ is also often referred to as the curvature of the utility function or as the Arrow-Pratt measure of relative risk aversion.

one per portfolio. The moment conditions are constructed as the simple average over time of the pricing errors. The GMM estimation seeks to minimize:

$$\mathrm{obj} = \min_{\gamma}[\mathrm{u_T}(\gamma)' \mathrm{W}\, \mathrm{u_T}(\gamma)] \tag{4.2c}$$

where $\mathrm{u_T}(\gamma)$ is a vector containing the four moment conditions and W stands for the weighting matrix.

We select the second-moment matrix of returns $\mathrm{W} = \mathrm{E}\left[\mathrm{R_j R_j'}\right]^{-1}$ as the weighing matrix. Employing the second moment matrix leads to economically interesting results. Indeed, the square root of the objective function of Equation (4.2b) corresponds to the Hansen-Jagannathan distance (L. P. Hansen & Jagannathan, 1997) that can be interpreted as the least-squared distance between the stochastic discount factor implied by the data and the class of stochastic discount factors that price assets correctly. Simply put, we estimate the coefficient of risk aversion such that Equation (4.2c) minimizes arbitrage possibilities instead of pricing errors per se.

4.4.2 Event-study framework

We perform the estimation of the CCAPM on each state for which we have enough data and, accordingly, we obtain one estimate of the average rate of risk aversion for 47 states. As we fit four maturity-based portfolios per state, this results in a panel of 188 time-series of pricing errors containing 143 monthly observations from which to run an event-study. The identification strategy for a disaster-driven increase in risk aversion rests on the assumption that the average rate of risk aversion, as captured by the time-invariant parameter γ, underestimates the '*true*' rate of risk aversion in the wake of major natural disasters. The underestimation of γ has a direct impact on the CCAPM's pricing errors as it overvalues (undervalues) the stochastic discount factor when the growth in consumption is greater (smaller) than one. Accordingly, the overvalued (undervalued) stochastic discount factor translates through Equation (4.2b) into more positive (negative) pricing errors.

We proceed by estimating the following regression:

$$\begin{gathered} u_{njt} = \delta_1 \mathbf{1}_{cg} DIS_{j\{t-k,\ t-1\}} + D\_MAT_n + D\_STATE_j + D\_TIME_t + \xi_{njt} \\ \mathbf{1}_{cg} = \begin{cases} +1 & \text{when ELECT} > 1 \\ -1 & \text{when ELECT} < 1 \end{cases} \end{gathered} \tag{4.3}$$

where u_{njt} is the pricing error for the bond return series of maturity class n in state j for month t. We acknowledge that u_{njt} is measured with error. Measurement errors in the dependent variable should not affect the consistency of δ_1 as long as the measurement errors are uncorrelated with DIS or ξ, but can be seen as an additional error term in Equation (4.3) which reduces the power of our statistical tests. There is no obvious way to address the measurement error issues. While we do not take measurement errors into account in our base tests, we employ two different approaches in the robustness section to show the reliability of our results. DIS is the sum of disaster-driven damage in state j that occurred between months t-k and t-1. The literature offers somewhat conflicting results regarding the time period during which disasters may impact risk taking behavior with most authors (Bucciol & Zarri, 2013; Kim & Lee, 2014; Malmendier & Nagel, 2011) favouring an almost persistent effect, but some (Cameron & Shah, 2015; Eckel, El-Gamal & Wilson., 2009) observing differences between short-term and long-term effects. Baker & Bloom (2013) use natural disasters, terrorist attacks and political disasters as shocks to the economy and observe that shocks have an average half-life of six months. Accordingly, we use four distinct intervals k in our analysis: three short periods of 1-, 6- and 12-months, respectively, and one long-term period of 10 years. $\mathbf{1}_{cg}$ is an indicator variable that takes the values of plus (minus) one when the gross consumption growth ELECT is greater (smaller) than one. The indicator variable ensures that the relation between u_{njt} and $\mathbf{1}_{cg}DIS_{j\{t-k,\ t-1\}}$ shall always be positive (negative) if disasters are to increase (decrease) risk aversion. D_MAT, D_STATE and D_TIME are maturity classes, states and time (months) fixed effects. Our expectation is that δ_1 is significant and positive.

Equation (4.3) is estimated using the M-estimation method developed by Huber (1973). This method produces estimates that remain reliable in the presence of various types of noises and is robust to outliers in the pricing error series.[34] Chan & Lakonishok (1992) document the efficiency gains from using the M-estimation as an alternative to ordinary least squares. Also, the fact that the M-estimation put less weight on outliers than a least-squares regression makes it more difficult to find a significant relationship between disasters and risk aversion as large disaster-induced pricing errors are discounted in the analysis. We perform the M-estimation using an iteratively reweighted least-squares (Holland & Welsch, 1977).

[34] We consider that the presence of outliers in the independent variables is not much of an issue in our setup given that disaster damage is the only observable variable on the x-side of the equations. We verify that the largest damage estimates correspond to the biggest catastrophes and stress that these extreme events must be included in such a study of natural disasters.

4.5 RESULTS AND DISCUSSIONS

In this section, we first present the outcomes of the estimation of the CCAPM that gives insights on the distribution of the risk aversion parameter across states and produces the pricing errors series. Then, we display our main results that describe how the pricing errors are affected by disaster-related damage and discuss how the conclusions can be linked to a disaster-driven increase in risk aversion. Last, we investigate the robustness of our results.

4.5.1 Estimation of state-level risk aversion parameters

We perform the GMM estimation of the CCAPM described by Equations (4.2a to 4.2c) using monthly data over 47 states. We employ the Nelder-Mead algorithm to minimize the departures to the non-arbitrage condition and estimate the state-level risk aversion parameters γ. We use the lambda method to estimate the standard error of γ. We report the estimated parameters in Figure 4.1.

[Insert Figure 4.1 around here]

In Figure 4.1, we observe that the estimated values for γ vary between 0.02 (Connecticut) and 9.45 (New York), with a mean value of 4.22, and are significantly greater than zero for 41 states out of 47 from a statistical standpoint. As expected given basic economic intuition, we obtain no negative coefficients. Furthermore, many well-known studies argue that economically reasonable values for the coefficient of RRA should range between one and two (Arrow, 1971; Friend & Blume, 1975), and Mehra & Prescott (1985) restrict the value of γ to be a maximum of ten. Our mean estimate of γ is mostly consistent with this expected order of magnitude and is lower but comparable to the RRA estimates of other asset pricing studies assuming market incompleteness based on geographic segmentation (Jacobs, Pallage & Robe, 2013; Korniotis, 2008; Sarkissian, 2003).[35]

We further examine the plausibleness of our estimates by examining the distribution of the risk aversion coefficients across states. We believe that this analysis is important given that the reliability of the pricing error series is a direct function of the $\hat{\gamma}$. To this end, we study the relationship between $\hat{\gamma}$ and known indicators or determinants of risk aversion. The demand for insurance is an often-used indicator of risk aversion. Among others, Szpiro (1986) and Halek & Eisenhauer (2001) use data on property and liability insurance to estimate the degree of RRA assuming that higher risk aversion leads consumers to buy more insurance. We use the volume

[35] For example, Sarkissian (2003) estimates RRA to be 5.7.

of direct written life and health insurance premiums per capita from the annual statement of the National Association of Insurance Commissioners (NAIC) for the year 2010 (Schedule T) as a proxy for the demand for insurance by state. We obtain a positive and significant cross-state Spearman's correlation coefficient (ρ^{sp}) of 0.34 between $\hat{\gamma}$ and the demand for insurance series. Such a relatively high correlation suggests that $\hat{\gamma}$ are higher in states where the average individuals spend more on insurance, as expected. Next, Noussair, Trautmann, Kuilen & Vellekoop (2013) and Kumar, Page & Spalt (2011) observe that Catholics are less risk averse than Protestants with regard to financial risks. Accordingly, we construct a Catholic-to-Protestant ratio at the state level using the U.S. Religious Congregations and Membership Study of 2010 available on the Association of Religion Data Archives (ARDA)'s website. As expected, we observe that states with a higher Catholic-to-Protestant ratio exhibit lower $\hat{\gamma}$ ($\rho^{sp} = -0.26$).

Last, we perform an informal analysis of the effect of disasters on risk aversion by calculating the correlation between $\hat{\gamma}$ and the total amount of disaster-related damage experienced between 1990 and 2005 and obtain $\rho^{sp} = 0.17$. The positive correlation is consistent with the disaster-driven risk aversion story. However, the correlation coefficient is not meaningful at conventional significance levels. The absence of significance suggests that the total amount of physical damage associated with extreme weather events are not a first-order determinant of regional differences in risk taking behavior. We investigate later, in section 4.5.2, the merits of the disaster-driven increase in risk aversion hypothesis.

As with all model fitting exercises using real data, the estimation of the CCAPM produces residuals that can be interpreted as pricing errors, that is, as deviations to the non-arbitrage conditions. Table 4.4 presents descriptive statistics on the series of pricing errors aggregated by states or by maturity classes. The means and medians of all series are not statistically different from zero. We observe that, on average, the sorting by maturity groups makes almost no difference on the moments of the series. As expected given that the CCAPM is estimated on a state-by-state basis, we see many more differences in pricing error series aggregated by states.

[Insert Table 4.4 around here]

4.5.2 Impact of disasters on risk aversion

We estimate Equation (4.3) using the pricing error series as the dependant variable. Disaster-related damage is the main explanatory variable. Average damage per capita figures are summed over various time periods ranging from one month to ten years and are expressed in dollars (DIS). Table 4.5 displays the results of our main event-study model. We observe that δ_1

is positive and highly statistically significant. This finding is consistent with an increase in risk aversion following disasters. However, our results do not allow distinguishing whether the increase in risk aversion is caused by recent disasters or by the memory of older large catastrophes. The fact that there exists a significant average correlation ($\rho^{sp} = 0.73$) between disaster-related damage in the 1990-2005 period and damage in the 2005-2015 period obfuscates the comparison between periods.

[Insert Table 4.5 around here]

We further examine the merit of the disaster-driven increase in risk aversion story by analysing the pricing errors using alternative modeling schemes. In particular, we run (i) a simple OLS model with $\mathbf{1}_{cg} * DIS_{j\{t-k,\ t-1\}}$ as the sole explanatory variable. We also estimate (ii) an OLS model specified as Equation (4.3) with residuals assumed to be spatially correlated at the state level. We use the latitude-longitude coordinates of the mean centers of population by state from the U.S. 2010 Census to build the distance weighing matrix.[36] Last, we test (iii) an OLS model with no state fixed effects but with residuals clustered by state and sandwich standard-errors.

We report the outcomes of the alternative models in Table 4.6. For ease of illustration, we show the results for two damage period lengths, the short-term 1-month period and the 10-year period. We observe patterns similar to those in Table 4.5 in that δ_1 is positive and highly statistically significant in all cases. The exclusion of fixed effects, the incorporation of spatial correlation or the clustering of the residuals have no material impact on our main conclusion. The use of alternative modelling strategies brings additional support to the disaster-driven increase in risk aversion story.

[Insert Table 4.6 around here]

Last, we examine in greater depth the relationship between pricing errors and disaster damage by sorting monthly per capita disaster-related damage into five categories and estimating the following OLS model:

$$u_{t+1} = b_0 + b_1 Small\_dmg_t + b_2 Medium\_dmg_t + b_3 Large\_dmg_t + b_4 Extreme\_dmg_t + \xi \qquad (4.4)$$

[36] Coordinates of the population centers are available on the U.S. Census Bureau's website at: https://www.census.gov/geo/reference/centersofpop.html (page consulted on July 31st, 2017).

The five mutually exclusive damage categories are dummy variables that contain state-month observations with total damage per capita between 0 and $1 (No dmg), 1 and $15 (Small dmg), 15 and $50 (Medium dmg), 50 and $250 (Large dmg), and more than $250 (Extreme dmg), respectively. The no damage category is used as the reference class so that the average effect of experiencing a disaster on pricing errors is expressed in relative terms to calm periods. Not only does the estimation of Equation (4.4) clearly illustrate the impact of disaster intensity on our main results, but it also allows studying how the sign of ELECT and how the maturity class interrelate with our conclusions.

Figure 4.2 displays the estimated coefficients and related confidence intervals of Equation (4.4). Panel A contrasts periods of negative consumption growth (ELECT < 1) with periods of positive consumption growth (ELECT > 1). We observe that the average pricing errors are not statistically different from zero in calm times or in the month following disasters of low intensity. In contrast, we clearly see that highly destructive events cause a significant deviation from zero. Negative consumption growth is associated with negative pricing errors while ELECT > 1 is linked to positive errors, as expected. The fact that the point estimate coupled to Extreme_dmg is larger when ELECT < 1 than when ELECT > 1 suggests that the effect of disasters on risk aversion is more acute in an economic downturn than in an upturn. More evidence is required to confirm this relationship. Panel B shows that the effect of disaster intensity is similar for municipal bond indices of different maturities.

[Insert Figure 4.2 around here]

4.5.3 One-step estimation using GMM

One of the drawbacks of our main approach to identify disaster-driven change in risk aversion is that we assess the impact of disaster-related damage on pricing errors instead of on the risk aversion parameter directly. Hence, we are unable to quantify accurately the increase in risk aversion. In this section, we develop a second approach to assess the impact of disasters on the risk aversion parameter that consists of the simultaneous GMM estimation of the risk aversion of the 47 individual states plus an additional disaster-driven risk aversion parameter that appears in the months following the largest disasters and whose impact is common across states and time. This approach is analog to a modification of the stochastic discount factor of Equation (4.2b) that can now be expressed as:

$$\mathrm{m_{j,t}} = \frac{\mathrm{C_{j,t}}}{\mathrm{C_{j,t-1}}}^{-\left(\gamma_j + \mathbf{1}_{j,k}^{FEMA}\gamma^D\right)} \tag{4.5}$$

where $\frac{C_{j,t}}{C_{j,t-1}}$ is the growth in consumption in state j at month t, γ_j is the relative risk aversion parameter associated with state j, $\mathbf{1}_{j,k}^{FEMA}$ is an indicator variable that takes the value of one in the k months following the occurrence of a major disaster in state j and zero otherwise and γ^D is the disaster-driven risk aversion parameter.

The state-specific estimates of γ_j reported in Figure 4.1 are used as starting values and γ^D is set to zero to begin the minimization procedure. The approach also requires that we explicitly identify major disasters. In order to avoid fixing an arbitrary damage threshold to transform the continuous disaster-damage series into the binary variable $\mathbf{1}_{j,k}^{FEMA}$, we follow another path and use the information available on major disaster declarations made by the U.S. President under the dispositions of the Stafford Act. The list of all federally declared disasters is accessible as an open government dataset.[37] The list provides information on declared counties, on disaster starting dates and on disaster categories. We manually augment the list with damage data coming from the preliminary damage assessment reports (PDA) available from the Federal Emergency Management Agency (FEMA). The declaration process provides a straightforward way to identify major disasters. Indeed, the Stafford Act instructs that PDA can be waived when extreme weather events are of "*such unusual severity and magnitude that formal field damage assessments are not required to establish the need for supplemental Federal assistance*".[38] Accordingly, we define major disasters as declared events for which the requirement to perform a PDA has been waived. Note that PDAs are available online starting from October 2007 so that no major disasters are counted for the years 2005 and 2006. We obtain a list of 39 major disasters. Several major disasters refer to the same weather event. For example, four entries (states) are associated with the Mississippi River flooding of 2011 and four other states are linked with Hurricane Matthew in 2016. Mississippi and Louisiana are the most frequently identified disaster areas with a total of five major disasters between 2007 and 2016. Major disasters span 23 states. Although disasters are somewhat clustered in time with ten entries in 2008, and eight in 2016, at least one major disaster occurred each year from 2007 to 2016. We set the binary variable $\mathbf{1}_{j,k}^{FEMA}$ equal to one for a 6 months period (k=6) following the beginning of a major disaster. The 6-month period is preferred over other longer and shorter

[37] The dataset is referred to as the FEMA Disaster Declarations Summary and is hosted at https://www.fema.gov/media-library/assets/documents/28318 (Page consulted on July 30th, 2017).

[38] Title 44 of the Code of Federal Regulations §§ 206.33(d) and 206.36(d).

periods for this exercise as this duration matches the findings of Baker & Bloom (2013) regarding the period during which natural disasters and other shocks affect economic growth.

Table 4.7 discloses the results from the ‘one-step’ simultaneous GMM estimation of Equation (4.5). Interestingly, we observe that the disaster-driven risk aversion parameter γ^D is positive with an estimated value of 0.38. This estimate implies that the average rise in risk aversion caused by extreme weather events is about nine percent. This result reinforces the disaster-driven increase in risk aversion story.

[Insert Table 4.7 around here]

4.5.4 Conditional CCAPM

Performing an event study on extreme weather events excludes *de facto* most of the endogeneity concerns given that disasters are obviously uncorrelated with national and regional economic conditions. Accordingly, we interpret our main findings as a clear indication that disasters *cause* an increase in risk aversion. Yet, as disastrous events are somewhat clustered in time, a first set of robustness tests consist of examining whether the timing of disasters may coincide with patterns in economic conditions.

The rationale underlying the impact of economic conditions rests on the assumption that changes in municipal bond returns over time might reflect time-varying national and regional economic conditions. If that is the case, then a conditional version of the CCAPM should be preferred over its unconditional counterpart. In other words, we need to relax the assumption that the information set Ω_t contains only a constant equal to one. We follow the *scaled payoffs* approach sketched by Cochrane (2009, p.132-136) where variables that account for economic conditions and that are added in Ω_t are treated as instruments in order to estimate the CCAPM. Let z_t represents the vector of instruments, Equation (4.2a) becomes:

$$1 \otimes z_t = \mathrm{E}\left[\beta \frac{\mathrm{C_{t+1}}}{\mathrm{C_t}}^{-\gamma} (\mathrm{R_{t+1}} \otimes z_t)\right] \tag{4.6}$$

where $\otimes$ refers to the Kronecker product and $\mathrm{R_{t+1}} \otimes z_t$ is interpreted as the returns of a managed portfolio at time $\mathrm{t}+1$ for which investors observe the value of the instruments at time t in order to make investment decisions.

The next important question is to decide on the variables to include in Ω_t. In an ideal world, one should add all instruments that predict discounted returns. However, the number of moment conditions increases as we add instruments and we must make a trade-off between parsimony and completeness. Therefore, we choose to include five variables in addition to a constant to Ω_t. The first two variables are related to interest rates and are the monthly variations of the effective federal funds rate (FEDFUNDS) and of the term spread between the 20-year U.S. treasury bonds and the 4-week U.S. treasury bills constant maturity indices (TERM). The third variable consists of the monthly variations in the default spread between the 10-year high quality market corporate bond index and the 10-year U.S. treasury bonds index (DEF). FEDFUNDS, TERM and DEF describes country-wide economic conditions and are constructed using data from the Economic Research Division of the Federal Reserve Bank of St. Louis. Variants of the FEDFUNDS, TERM and DEF variables are used in several bond pricing studies (e.g. Acharya, Amihud & Bharath, 2013; Fama & French, 1993; Goyenko, Subrahmanyam & Ukhov, 2011). The two last instruments relate to the regional economic environment. We include in Ω_t the monthly growth of state coincident indexes (SCI) from the Federal Reserve Bank of Philadelphia. This indicator portrays the current state of the economy at the regional level in a single statistic. The index is based on a dynamic single-factor framework developed by Stock & Watson (1989) that combines four local economic variables related to employment, unemployment, hours worked in manufacturing and labor wages. The growth rate of SCI is also employed by Pirinsky & Wang (2006) to explain the local component of stock returns.

The second regional instrument is the change in the annual maximum combined federal and state marginal income tax rates (TAX) from the TAXSIM model (Feenberg & Coutts, 1993).[39] Varying tax rates over time alter the relative benefit of the tax-exemption characteristic of municipal bonds. Therefore, it is likely that TAX impacts investment decisions.

Table 4.8 presents summary statistics on the five instruments. We observe that FEDFUNDS, SCI and TAX exhibit high first-order autocorrelation which is not surprising given our use of monthly data. Change in tax rates across states are highly correlated with $\bar{\rho}^{sp} = 0.80$. The average cross-state correlation in SCI reaches 0.48 and varies between 0.07 (Minnesota – Louisiana) and 0.86 (Nevada – California).

[Insert Table 4.8 around here]

We estimate the conditional CCAPM using Equation (4.6) to account for the instruments. We obtain estimates of RRA that are in line with those reported in Figure 4.1, both in terms of

[39] The maximum state income tax rates dataset is hosted at http://users.nber.org/~taxsim/state-rates/ (Page consulted on Aug 2nd, 2017).

order of magnitude and of state-ranking. Indeed, coefficients vary between -0.10 and 9.37 with great similarity in state ranking ($\rho^{sp} = 0.91$). The new $\hat{\gamma}$ are positively correlated with the volume of direct written life and health insurance premiums per capita by state ($\rho^{sp} = 0.24$), negatively correlated with the Catholic-to-Protestant ratio ($\rho^{sp} = -0.30$) and positively correlated with historical disaster-related damage ($\rho^{sp} = 0.15$). Importantly, the bigger number of moment conditions in the GMM estimation produces a larger panel of residuals with 940 series of 143 monthly pricing errors. In turn, the additional observations allow us to estimate more precisely the parameters of the event study in Equation (4.3) and to reduce the plausible bias in inferences arising from measurement errors in the pricing errors. Note that, by construction, the new pricing errors series are unrelated to the first lag of the instruments included in the representative investor's information set.

Table 4.9 exposes the results of the event study associated with the pricing errors. In panel A, we see that the inclusion of economic variables as instruments does not overly affect the strength of the relationship between disaster damage and pricing errors. Indeed, the coefficients of $\mathbf{1}_{cg}$*(log DIS) are similar in magnitude to those reported in Table 4.5 and are highly statistically significant for all disaster-related damage periods, except for the 1-month event window. Panel B brings additional support for the disaster-driven increase in risk aversion assumption by testing Equation (4.3) on another disaster-related variable. This time, instead of using DIS, we employ $\mathbf{1}_{j,k}^{FEMA}$, the indicator variable that is set equal to one in the k months following a major declared disaster for which the requirement to perform a PDA has been waived. As expected, the results are also consistent with an increase in risk aversion.

[Insert Table 4.9 around here]

One could rightly argue that our list of instruments represents all but a small subset of the representative investor's information set. The impossibility to control for a large set of control variables constitutes a limitation of our approach but is common to studies that use such an approach. However, the occurrence of the global financial crisis gives a natural way to ensure that the increase in risk aversion is due to disasters and not to the dreadful business conditions of 2008. The last column of panel B of Table 4.9 excludes all data from the year 2008 in order to estimate Equation (4.3). Although we obtain a coefficient of $\mathbf{1}_{cg}$ * $\mathbf{1}_{j,k}^{FEMA}$ somewhat lower than that for the full sample, the result is still highly supportive of the disaster-driven increase in risk aversion story. In unreported results, we exclude data from 2008, 2009 and 2010 and obtain a coefficient of 0.763, which is significant at the 0.05 level.

4.5.5 Determinants of relative risk aversion

In this section, we make a brief aside to examine how various demographic and economic characteristics of the representative investors succeed in explaining the variation of risk aversion over time and states. We exploit the fact that the addition of instruments is analog to the addition of test assets to artificially increase the number of observations on which to estimate the RRA parameters (i.e. dynamic spanning). Yet, unlike in our previous tests, we employ the larger sample to estimate state-level RRA at an *annual* frequency.[40] In order to simplify the estimation process, we employ the identity matrix instead of the second-moment of returns as the weighting matrix in Equation (4.2c).

Doing so results in a panel of 564 estimates of RRA (47 states times 12 years) that can be tested against historical state-level demographic and economic statistics. However, as individual yearly regional RRA are estimated on a relatively small sample size, we observe a surge in parameter uncertainty. Annual RRA vary from -1.13 (Colorado, 2009) to +50.57 (Ohio, 2010) with a mean value of 1.97 and a median of 0.19. About 9 percent of the estimates are negative (52 out of 564), but only one of the negative RRA is statistically significant at the 0.05 level, assuming normality. About 26 percent of the estimates (146 out of 564) are significantly positive at the 0.05 level. The rest of the RRA in the panel are not statistically different from zero due to the high parameter uncertainty. The rank correlation between the average annual RRA over years and the RRA estimated using the whole sample and reported in Figure 4.1 equals 0.85. While we acknowledge that our annual estimates of risk aversion suffer from substantial measurement errors, we have no reason to believe that the RRA are systematically over- or underestimated. It is our contention that an analysis of the determinants of risk aversion using our regional annual RRA estimates constitutes a noteworthy contribution to the existing literature.

We ground our choice of variables to explain the variability of our annual RRA estimates on the past literature (Brown, 2007; Cho, 2014; Dorn & Huberman, 2005; Hryshko, Luengo-Prado & Sørensen, 2011; Kryzanowski & To, 1986; Outreville, 2015; Sapienza, Zingales & Maestripieri, 2009; Yao, Gutter & Hanna, 2005). Data on several plausible determinants can be found at the state and annual level on the American Community Survey through the FactFinder's web-interface. These include the state population per million inhabitants (POP), the size of the

[40] Note that in unreported tests, we estimate state-level RRA at a monthly frequency and time aggregate the monthly RRA for each state into averaged yearly RRA values (Carrieri, Chaieb & Errunza, 2013). That approach leads to more volatile RRA estimates.

labor force relative to the total working-age population (LABOR), the male-to-female ratio (GENDER), the median age of residents (AGE), the proportion of state population above age 25 that has completed a bachelor's degree or higher (BACHELOR), the proportion of state population that is non-white (NONWHITE) that is an indicator of ethnicity and the proportion of non-English speakers (LANGUAGE) that is used as a proxy for cultural diversity. We manually construct a dummy variable that equals one if a state governor is Republican and zero if it is a Democrat (GUBERNATORIAL). We also include variables that convey information on various dimensions of wealth. Such variables include the median house value expressed in $1,000 (HOUSE), the median gross rent (RENT), the monthly growth rate of disposable personal income (INC) and the state domestic product (SDP). Data on INC and SDP are from the U.S. Bureau of Economic Analysis. Next, we consider three time-invariant determinants, namely a proxy for the level of public corruption (CORRUPT) based on the aggregated number of public corruption convictions per 100,000 population between 1976 and 2010 from the table 7 of Simpson, Nowlan, Gradel, Zmuda, Sterrett & Cantor (2012, p.16), the Catholic-to-Protestant ratio (CPRATIO) of Kumar, Page & Spalt (2011) previously described and the proportion of the workforce that is employed by the finance and insurance sector (SOPHISTICATION) that accounts for investor literacy and sophistication (Christoffersen & Sarkissian, 2009; Dhar & Zhu, 2006; Dorn & Huberman, 2005). Finally, we take in our main disaster-related per capita damage variable DIS as well as a binomial variable that equals one when a portion (at least 90 days) of a 12-month period following a major declared disaster occurs in a state-year and zero otherwise (FEMA). As some statistics are not yet available for 2016, we limit our analysis to the 2005 to 2015 period.

Table 4.10 describes the results of the study of the determinants of risk aversion. Model 1 uses the full sample of annual RRA estimates as dependant variable while Model 2 restricts the analysis to economically plausible RRA estimates that range from 0 to 10. Our models explain about 25 to 30 percent of the annual variations in state-level RRA. Most explanatory variables have the expected signs but only LABOR, BACHELOR and NONWHITE are statistically significant at the five percent level. A higher labor force participation rate is associated with lower risk aversion while higher educational attainment is linked with higher risk aversion. If we interpret NONWHITE as a measure of cultural diversity, then the results suggest that a greater diversity is associated with higher risk aversion. Yet, only model 2 shows a significant relationship between NONWHITE and risk aversion. As for the disaster variables, we see that both DIS and FEMA are positive as expected. Still, the significance of these variables remains low. DIS is only significant at the 10 percent level. We obtain similar results whether or not we exclude from our sample economically implausible RRA estimates that are negative or greater than ten. This ends our brief *aparté*.

[Insert Table 4.10 around here]

4.5.6 Robustness tests

We consider several additional robustness tests in addition to the inclusion of instruments. Among other things, we increase the new issue bond exclusion period from 28-days to 90-days. We also form municipal bond RSI using other variables than the remaining time to maturity. Using information extracted from Bloomberg, we segment the bond universe according to the type of issuer (state, county, city or district) and according to the type of security (general obligation or revenue bonds). Data unavailability prevents us from segmenting the municipal bond market by credit rating. The longer new issue period reduces the number of roundtrip transactions kept to compute the RSI. This change brings no significant change to the means, medians and standard deviations of the bond returns series. The higher moments of the distribution of returns and extreme percentiles are more affected by the longer new issue period. For its part, the segmentation of the municipal bond universe by issuer type or by security type has no impact on the total number of CUSIP kept for the calculation of the RSI but engenders large differences between the numbers of roundtrip transactions across series. For example, our sample contains zero roundtrip transactions issued by counties in Vermont and we are forced to discard that state from our test as no RSI can be estimated. Similar limitations force us to exclude the states of Idaho, North Dakota, Oklahoma, South Dakota and West Virginia from our test when the bond market is segmented by issuer type, and to discard the states of Nevada and South Dakota from our test when we divide the municipal universe according to security type.

Table 4.11 provides the empirical outcomes of the additional robustness tests. We see that the estimated coefficients are similar to those reported in our main results and strongly support the disaster-driven increase in risk aversion assumption. Employing the amount of disaster-related damage (DIS) or a dummy variable that equals ones in the wake of the disaster of great consequences ($\mathbf{1}_{j,k}^{FEMA}$) both lead to the same conclusions. Results in Table 4.11 are not overly sensitive to the use of post-disaster periods other than six months. Shorter and longer periods have no material effects on the coefficient of DIS but reduce the coefficient of $\mathbf{1}_{j,k}^{FEMA}$ which remains significantly positive, save for the long 10-year period which is not significantly different from zero.

[Insert Table 4.11 around here]

As another validation exercise, we repeat the analysis of the impact of economic variables on the main results but relax the implicit assumption that the RRA parameter is constant over time. In other words, we replicate Table 4.9 using a different structure for the stochastic discount factor. The RRA parameter is now estimated at the *annual* frequency rather than estimated only once so that it acts for a (time-invariant) average value for the full 2005 to 2016 period. Table 4.12 reports the results from this robustness test. In panel A, we observe that the use of annual RRA estimates yields the exact same conclusions than those arising from Table 4.9. Disaster-related losses precede a rise in risk aversion that peaks around 6 months following the beginning of an event before gradually diminishing over a few quarters. However, DIS is not significantly related to risk aversion for very short and very long event windows. In panel B, the coefficients $\mathbf{1}_{cg}$ * $\mathbf{1}_{j,k}^{FEMA}$ mostly support the disaster-induced increase in risk aversion assumption in that we observe positive coefficients for all event windows. Surprisingly, the coefficient associated with the 6-month disaster damage period is not significant at conventional significance levels.

[Insert Table 4.12 around here]

Our next test of robustness consists in studying the impact of the business cycle on our results. To this end, we utilize the US business cycle reference dates from the National Bureau of Economic Research (NBER).[41] The NBER determines that the U.S. economy was in expansion between January 2005 and November 2007 and between June 2009 and December 2016 and in contraction between December 2007 and May 2009. Therefore, we independently estimate one RRA coefficient per state for each of the three business cycle periods and use the resulting pricing errors to examine the disaster-induced increase in risk aversion. The conclusions based on untabulated results available upon request are quantitatively similar to those reported in Table 4.9. The coefficient of $\mathbf{1}_{cg}$* DIS is positive and statistically significant for most disaster damage periods k and peaks around $k = 6$. When taken at face value, the results show that the impact of disasters is slightly higher in expansions than in contractions but a Wald test reveals that the difference is not statistically significant. The coefficients of $\mathbf{1}_{cg}$ * $\mathbf{1}_{j,k}^{FEMA}$ are also positive for all k. When a variable representing the fraction of the year when the U.S. economy is in contraction is added to the analysis of the determinants of RRA, the variable is insignificant and does not alter our previous inferences.

[41] Business cycle reference dates are available at http://www.nber.org/cycles.html (Page consulted on August 14th, 2017).

Last, we examine the sensitivity of our main results to alternative choices of the utility function. We consider both the recursive preference setup of Epstein & Zin (1989) that disentangles RRA from the elasticity of intertemporal substitution (EIS) and the model of Barberis, Huang & Santos (2001) that takes into account loss aversion in concordance with prospect theory and allows for time-varying risk aversion. Appendix E describes the recursive preference setup and presents the related results. Appendix F outlines the second alternative model and displays the attendant results. Both alternative modeling strategies generate conclusions that are consistent with the disaster-induced increase in risk aversion assumption.

As in any empirical study, our study faces some limitations. Resorting to the CCAPM means that we directly relate changes in the stochastic discount factor to changes in the RRA parameter with no consideration for other explanatory channels. One complementary alternative consists in disaster-induced changes in the subjective probability distribution of future disasters. In other words, the occurrence of a disaster could modify investors' beliefs about the likelihood of similar events. In turn, the weights associated with the various states of the world in the expectation operator $E[\cdot]$ of Equation (4.1) should reflect the time-variation in beliefs. Empirical tests of the CCPAM such as ours commonly put equal weights on each realized past period. Still, the occurrence of a major disaster has arguably no direct impact on the real probability distribution of disasters. This lets us suppose that disaster-induced changes in beliefs should be relatively short-lived and could only partially explain the long-term effect of disasters on asset prices that we documented. We refer interested readers to Chen, Joslin & Tran (2012) for a discussion of the relationship between disasters, beliefs and asset prices. Ambiguity aversion represents a second related alternative explanatory channel that in essence leads to the possibility that the probability distribution of disasters is unknown to investors. However, if one is ready to accept the premise that the occurrence of a disaster gives investors some insight about the likelihood of such rare events, then disasters should lessen the ambiguity and have a depressing impact on expected returns for ambiguity averse investors. The evidence favouring an increase in risk aversion in the wake of disasters is mostly inconsistent with the ambiguity aversion channel.

4.6 CONCLUSION

In this paper, we study asset pricing in the wake of disasters at the regional level and examine the assumption that extreme weather events *cause* an increase in risk aversion. Instead of relying on an incomplete market framework to account for regional heterogeneity in investor's risk preferences, we retain the well-known CCAPM in a complete market framework using state-level consumption data and state-level series of asset returns using municipal bond transaction data. We exploit the fact that trading of municipal bonds is dominated by in-state investors to empirically obtain estimates of regional relative risk aversion.

The risk aversion parameters arising from our analysis are much lower than those obtained in previous tests of the complete-market CCAPM. The cross-sectional dispersion of our RRA estimates is also consistent with indicators of risk aversion often cited in the literature such as the demand for insurance.

Our findings clearly support the disaster-driven increase in risk aversion assumption. States that historically suffer from more disaster damage exhibit higher RRA and the results indicate that contemporary extreme weather events also temporary raise regional risk aversion. The increase appears to peak around 6 months following the beginning of an event before gradually diminishing over a few quarters. These conclusions are robust to the use of alternative econometric models for the event study and remain qualitatively similar when a binary variable is used to identify the occurrence of major disasters instead of a continuous disaster damage variable.

In addition, we implement a modification to the traditional CCAPM in order to directly quantify the change in risk aversion induced by major disasters. Not only does this approach confirm our main results, but it also indicates that extreme weather events cause an increase in RRA of about nine percent on average.

The assessment of risk preferences implied by asset prices at the regional level is, by itself, an original contribution to the economic literature. Yet, the importance of this study rests to a large extent on the evidence that extreme weather events have a material impact on financial risk taking behavior. This is particularly true given the consensus in the scientific community that climate change should increase the frequency and intensity of disasters. A lower propensity to take financial risks has many major implications regarding (i) economic growth as it may impede business start-ups and impede companies from making risky investments; (ii) financial markets and portfolio management as it may adversely affect asset prices by raising required returns; and (iii) public management decision making as it potentially impacts the perceived value of risk management programs and infrastructures.

An interesting next step would consist of the examination of the cumulative effect of successive extreme weather events on risk aversion over time.

CHAPTER 5
Conclusion

The destructive capability of natural disasters is undisputed and the consequences of extreme weather events on local communities in terms of health issues, social hardship, environmental damage and economic well-being are the subject of a large and growing body of literature. The impacts of natural disasters on financial markets are less documented and existing studies mostly focus on the insurance or housing markets. Therefore, the main motivation of this thesis consists in the investigation of the impact of natural disasters on two additional financial security markets, namely the stock market (first essay) and the municipal bond markets (second essay). The last essay of this thesis examines the effect of extreme weather events on the risk preferences of the representative investors. Changes in risk preferences over time are intimately associated with the returns required by investors to bear risks on financial markets and are thus an important determinant of asset prices.

The first essay (presented in chapter 2) documents that the stocks of around 6 percent of the firms located in or near disaster areas exhibit abnormal returns in the wake of extreme weather events after taking into account false discoveries. However, most of the impact of disasters is felt in a two- to three-month period following the peak of the disasters rather than immediately. In line with most previous literature, we find no evidence of a market-wide impact on stock returns. Furthermore, we examine the effect of extreme weather events on the volatility of stock returns. The results provide evidence consistent with long-lasting disasters, such as floods and episodes of extreme temperature, causing a surge in idiosyncratic return volatility. Yet, most storms-like events have no significant effect on the second moment of returns. Hurricanes appear to be a special type of disaster in that they are the sole group of events to be associated with an increase in market-wide volatility.

The second essay (presented in chapter 3) examines the effect of major local floods on the issue yields of municipal bonds sold by U.S. counties. The essay provides strong evidence supporting an economically and statistically significant increase in financing costs for counties that issue new bonds in the months following disasters. The rise in financing costs averages seven percent which represents a loss of about $100,000 in terms of bond proceeds for a typical $10 million bond issue, everything else being constant. This finding is robust to alternative modelling strategies and is not explained by the individual characteristics of issuers in flooded areas. We test whether disaster-induced changes in the behavior of underwriters (variation in underwriter spreads and in underpricing activities) may explain the results but find no support for that explanatory channel. We also study the relationship between floods and the likelihood to issue new municipal bonds but reject explanations based on a selection bias. Thus, the decision to issue new bonds in the wake of a major flood does not appear to be disaster-driven. We investigate an increase in risk in the credit-risk channel using a collection of risk indicators at the issue and issuer levels. We obtain mixed results in that less than half of our indicators are consistent with lower issuer creditworthiness. While we cannot reject the credit-risk channel, our analysis suggests that the flood-related increase in municipal financing costs is associated with a behavioral explanation that implies that flood episodes heighten the fear of subsequent flooding.

The third and final essay (presented in chapter 4) studies if natural disasters alter the risk-taking behavior of investors. The essay departs from the existing literature by focussing on the representative investor instead of on individual investors and by inferring risk preferences from observed transaction data for municipal bonds instead of from surveys and experiments. We exploit the state-based segmentation of the municipal bond market documented in previous studies to infer the risk preference of the representative investor at the state-level using the conventional consumption capital asset pricing model. We obtain regional relative risk aversion (RRA) coefficients that average 4.22, which is not only coherent with economic intuition, but also somewhat lower than the RRA estimates reported in other papers that play on investor heterogeneity. The dispersion of our RRA estimates over states is also consistent with known determinants of investors' risk preferences. The examination of the pricing errors arising from the model fitting exercise reveals significantly larger pricing errors in regions that recently suffer from major disaster-related damage. This finding is robust to the use of alternative identification strategies as well as to the addition of controls for current national and regional economic conditions. The results suggest that large disasters cause a significant increase of about nine percent in relative risk aversion.

The foreseen increase in the frequency and/or severity of extreme weather events arising from global warming plausibly explain part of the growing interest in research directed to the consequences of natural disasters. On a broader perspective, this thesis extends existing literature by exploring the relationships between large disasters and selected financial security markets and by presenting novel results that bear important implications for investors, portfolio managers, insurers and municipal authorities. Indeed, our results shed some light on how and why local risks, such as those arising from natural disasters, affect the pricing of stocks and bonds and offer evidence consistent with a significant interaction between extreme weather, investor behavior and public financing. We believe that the conclusions of this thesis are also of interest from an academic perspective. Indeed, the results suggest (i) that the response of stocks to disasters is gradual rather than immediate, (ii) that the response of municipal bonds to large flooding shares more similarities with the way residential housing reacts to idiosyncratic events than with how stocks are affected by such disasters and (iii) that large disasters are a significant determinant of the time-variation in financial risk aversion.

Finally, we remind the readers that the conclusions of this thesis rest on U.S. data. Consequently, one should be careful when generalizing the findings to other economies because cultural traits or other business and regulatory environments may affect the level of risk awareness or the way investors perceive and react to disasters. In future work, we plan to examine how natural disasters impact financial markets in other countries, and in particular, in Canada.

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APPENDICES

APPENDIX A – Description of the control variables for Chapter 3

This appendix discusses the choice of determinants for the issue yield of municipal bonds in light of the existing literature. It also explains our base expectation regarding the sign and importance of the variables and, when relevant, provides details about data source and construction method. We proceed in a hierarchical manner and start with a presentation of the tranche-level variables before discussing the issue-, issuer-, state- and national-level variables.

We begin by including a variable that signals general interest rate conditions in the U.S. The benchmark variable (BMK) indicates the market rate of interest that prevailed the week before the issue date. Previous studies used various definitions of the market rate of interest. Marks & Raman (1985) used the average yield of AAA-munis reported by Salomon Brothers, Rivers & Yates (1997) and Robbins & Simonsen (2012) employed the Bond Buyer Municipal 20-Bond index as a benchmark and Apostolou, Apostolou & Dorminey (2014) include the level of the federal funds overnight rate as a proxy for interest rate conditions. We employ the MSRB database that contains over 55 million individual transactions on munis to construct 36 weekly series of average yields where transactions are clustered according to six maturity groups and six credit rating categories. Accordingly, the benchmark yield associated with a tranche will account for both its maturity and its credit quality while varying from week to week depending on macro-economic conditions. Issue yields should be positively and significantly correlated with their benchmarks.

Our second set of observables contains individual tranche characteristics. We follow Cook (1982) and include dummies to identify tranches sold at a discount (CPN=Discount) and at a premium (CPN=Premium), respectively. Consistent with the previous literature, we expect discount bonds to exhibit higher yields for tax considerations and premium bonds to also display higher yields because of greater reinvestment risk (Hopewell & Kaufman, 1974). We also include the par value of the tranche, expressed in natural logarithms (SIZE). Cook (1982) observes that there is no generally accepted theory of how issue size should affect yields, notably because larger issues entail a supply effect where supply exceeds demand but this effect may be offset by larger issues having higher marketability. Given that our study is performed at the tranche level, our intuition is that the marketability argument will overweigh the supply effect and that we should observe a negative relationship between SIZE and issue yields.[42] We include dummy variables for the presence of three types of options often embedded in muni. The first option is the traditional call provision (IsCALL) that permits the issuer to redeem the tranche prior to maturity under precise considerations. The second option is the presence of an extraordinary redemption (sometime called extraordinary call) provision that bestows to the issuer the right to redeem a tranche following an unusual one-time occurrence (IsEXTRACALL). A common situation that leads to extraordinary redemptions is a decline in interest rates that allows the issuer to refinance its obligations. The presence of both call options should be associated with higher issue yields given that municipalities will exercise these options only if it benefits them. However, the magnitude of the price discount will depend on individual call considerations and the market's expectation about future interest rates. The third option relates to sinkable tranches where issuers must save money in a sinking fund to ensure the timely repayment of principal and coupons (IsSINK). The setting up of a sinking fund greatly reduces default risk and should imply lower issuer yields (Capeci, 1991). Last, we add a dummy variable that equals one for tranches unrated by major credit agencies (UNRATED). Given that unrated bonds in our sample are mostly associated with small issuers, Rivers & Yates (1997) inform that the effect of the BMK variable may overestimate the yield of unrated tranches. Thus, we expect UNRATED to exhibit a negative sign.

We also utilize a collection of variables that reflect issue-level characteristics. G.O. bonds are usually backed by the credit and taxing power of the issuer. While the exact extent of the commitment to raise revenues to pay back the bondholders may vary from one issuer to the other, G.O. bonds are often classified either as unlimited-tax or as limited-tax. According to the Farlex Financial Dictionary, limited-tax bonds place a maximum possible tax increase on what a municipality can levy to repay the bonds. We include a limited-tax dummy in our study (IsLIMITED) and expect limited-tax bonds to exhibit a greater issue yield than unlimited-tax bonds. Beyond their full taxing power, some issuers pay a premium to insurance companies to guarantee the payment of capital and interest (IsINSURED) or use portfolios of securities as collateral to secure their offering. Most of the time, the collateral assets are made of bonds issued by the US government, states, or other governmental authorities such as the Federal Home Loan Mortgage corporation. We add a dummy variable to account for secured bonds (COLLATERAL). We expect both IsINSURED and COLLATERAL to lower issue yields. Another

[42] In unreported tests, we include a size variable constructed at the issue level. The issue size variable has no significant effect on issue yields.

issue-level proxy is a categorical variable that signals the purpose of the bond issue (PURPOSE). We distinguish between four categories of bond purpose: bonds issued for general purpose, bonds issued for refunding purpose, bonds issued to fund pension deficits and a mixed-up class that groups bonds that do not seem to fit into any other category. Next, we control for the pricing mechanism by including a binary variable that takes the value of one if the issue was competitively bid and of zero otherwise (COMPETITIVE). Following Apostolou, Apostolou & Dorminey (2014), Rivers & Yates (1997) and Simonsen, Robbins & Helgerson (2001), we anticipate competitive sales to lower yields. Last, Daniels & Vijayakumar (2007) report that munis marketed by more prestigious underwriters have lower yields and Butler (2008) finds that underwriters with a local presence also sell bonds at lower yields. We take underwriter considerations into account by defining local underwriters as companies domiciled in the same state as the issuer and by classifying prestigious underwriters as companies that operate in at least 30 states and have marketed more than 100 munis. We create a dummy variable that equals one when the lead underwriter is neither a local underwriter nor a prestigious underwriter (UW_OTH). We expect the dummy to be positively correlated with yields.

Unlike the previous sets of controls that focus on bond characteristics, issuer level variables mostly relate to regional (county) market conditions. Following previous studies, we include the average per capita household income (INCOME) that is expected to be negatively correlated with issue yields (Horton, 1969; Rivers & Yates, 1997; Robbins & Simonsen, 2012; Ziebell & Rivers, 1992). We include the county's population (POP) expressed as the natural logarithm of millions of residents as well as the 5-years population growth (POPgwth5y). The population variables are employed in several studies (Capeci, 1991; Horton, 1969; Rivers & Yates, 1997; Ziebell & Rivers, 1992) which either associate higher population with higher tax revenue potential or with higher demand for municipal services. On the basis of the findings reported in past papers, we expect the population variables to have a positive sign. For county indebtness, we use the amount of total outstanding debt annually reported by county governments, expressed on a per capita basis (DEBT). We follow Marks and Raman (1985) and others and anticipate higher issue yields for more indebted counties.

We add to our list four state-level control variables by including the magnitude of the tax-exemption advantage (TAXADV), a proxy for the level of public corruption (CORRUPT), a dummy variable for gubernatorial election periods (ELECTION) and the state leading indexes (LEADING). Rivers & Yates (1997) use an in-state yield advantage variable to account for differing state personal income tax treatment of muni interest and find that greater tax advantages lead to lower bond yields. Butler, Fauver & Mortal (2009) examine political integrity and observe that more corruption is associated with higher bond yields. We use state ranking based on the number of public corruption convictions per 100,000 population between 1976 and 2010 from the table 7 of Simpson, Nowlan, Gradel, Zmuda, Sterrett & Cantor (2012, p.16) as our measure of state corruption and assume the level of corruption remains constant through time. We introduce gubernatorial election period dummies that are set to one for issues sold in a 100-day period preceding an in-state election. Following Gao & Qi (2013), we anticipate election periods to increase bond yields. The last state-level variable is the state leading indexes from the Federal Reserve Bank of Philadelphia. The leading indexes give a six month-ahead forecast of the variations in local economic conditions. We expect a negative correlation between the leading index and bond yields. Indeed, Poterba & Rueben (2001) show that better local economic conditions reduce municipal financing costs at the state level.

Last, we include as controls three US-wide statistics that relate to the volatility of the interest rate environment (IRVOL), to investor sentiment (SENTIMENT) and to municipal market uncertainty (UNCERTAINTY). Marks & Raman (1985) and Robbins & Simonsen (2012) include proxies for interest rate volatility in their model and observe a positive relationship between volatility and bond yields. We use the average value of the CBOE 10-year US Treasury note volatility index in the 20 trading days preceding a new issue as a proxy for the volatility of interest rates. Lee, Shleifer & Thaler (1991) and Bodurtha, Kim & Lee (1995) interpret variations in closed-end fund (CEF) premiums and discounts as an indicator of investor sentiment. We collect data on monthly premiums and discounts for U.S. closed-end municipal bond funds from Morningstar. Morningstar defines a premium or price discount as the amount by which a fund's market price is greater or less than the net asset value, expressed as a percentage of the net asset value. To avoid biases arising from changes in the underlying composition of the fund sample, we restrict our analysis to CEF with a national geographic focus and a long-term perspective. An optimistic sentiment may boost demand in municipal bonds and lower issue yields Remolona, Kleiman & Gruenstein (1997). Thus higher values for SENTIMENT should be associated with higher issue prices and lower issue yields. Besides the magnitude of the CEF discounts, the cross-sectional dispersion of the discounts is also informative. Indeed, in a different context, Petajisto (2017) shows that the cross-sectional dispersion in ETF premiums peaked during the global financial crisis and is significantly correlated with the VIX index. Consequently, one can imagine that the cross-sectional standard deviation of municipal CEF discounts is interpretable as an indicator of noise in the municipal bond market. More noisy periods should be linked with higher financing costs.

APPENDIX B – Additional control variables for Chapter 3 and related robustness tests

This appendix describes the additional control variables that are added to the model to investigate the robustness of the main results and presents the results from the analysis.

We begin by building three additional variables following a long-minus-short methodology (Fama & French, 1993) that illustrates yield differences between small and large tranches (SMB), long-term and short-term tranches (TERM) and lowly-rated and prime tranches (DEF), respectively. These variables aim at capturing the changes over time in the extra returns that investors demand to bear various forms of risk.

Next, we account for other county-level characteristics such as the proportion of the labor force that works in the Finance and Insurance industry (FINDENSITY) that may proxy for a larger pool of mutual fund managers or of high-wealth investors,[43] or the dispersion of household income (INEQUALITY) that may be related with an expansion in services offered by local government (Boustan, Ferreira, Winkler & Zolt, 2012). We also include in the model variables that account for the total outstanding value, in billion $US, of new municipal bonds sold in a 30-day period preceding a new issue by state (STATE_ISSAMT) and counties (COUNTY_ISSAMT) to account for the relative supply of local debt (Rivers & Yates, 1997).

Then, we add dummy variables that account for the global financial crisis period (GFC), defined as the interval between September 2008 and June 2009, for the panic selling of January and February 2010 that resulted from the *"billions of dollars worth of defaults"* prediction of Meredith Whitney (PANIC), and for the period following the liquidity coverage ratio (LCR) requirement adoption in September 2014 that compels large banking organizations to hold a minimum amount of high-quality assets which does not include municipal securities.[44] We also include month-of-the-year dummies (MONTHofYEAR) to account for plausible seasonal patterns in issue costs.

Last, we develop a new variable (MONTHLYRISK) that accounts for months with high risk of flooding. The new variable complements our main flood risk indicators that control for high risk areas. MONTHLYRISK represents the expected weather-related losses in a given month by

[43] See Christoffersen & Sarkissian (2009; 2011) for an interesting discussion on the influence of financial centers.

[44] The Federal Reserve somewhat relaxed the rule in July 2016 by allowing certain investment grade munis to be considered as high-quality liquid assets.

state. It is built from the average historical per capita damage incurred by month and state between January 1990 and December 2005 as reported in the NCEIs' storm event database.

Table B.1 shows the estimation results for the augmented model. We observe that times with larger maturity spreads are associated with larger issue yields. Interestingly, the amount of concurrent new issues is not significant at the state-level but becomes highly significant at the county level. This suggests that issuers compete against neighbouring entities to attract investors and may hint that the investor base is segmented at an intra-state level. GFC and PANIC are not significant, perhaps because the high uncertainty associated with these periods is already captured by the year fixed effects. However, issues sold following the introduction of the LCR requirement exhibit higher issue yields. Month-of-the-year dummies reveal the existence of a seasonal pattern in issue yields in that financing costs are smaller in October-to-April relatively to the May-to-September period. Of particular interest for this study, high risk periods as proxied by MONTHLYRISK are not associated with higher yields. All in all, the coefficients of our main variable of interest are largely similar to those reported in Table 3.3. Post-flood issues entail higher financing costs for municipalities and the net effect represents an increase of 7.4 percent.

[Insert table B.1 around here]

APPENDIX C – Construction of the electricity consumption series (ELECT) for Chapter 4

This appendix describes the construction of the monthly state-level gross electricity consumption series that are used to infer risk aversion in the context of the CCAPM.

Our metric of state consumption follows Da & Yun (2010) and is based on the sum of residential and commercial electricity monthly usage[45] and controls for the effect of temperature as well as for within-year seasonal variations in electricity demand. We obtain data on temperature from the NCEI's global historical climatology network that provides monthly summaries for land-based weather stations. The summaries include measures of heating (HTDD) and cooling (CLDD) degree days that are frequently employed in academic studies to quantify the demand for energy needed to heat and cool a building, respectively. For each state, we take the arithmetic average of HTDD and of CLDD over all in-state land-based stations. The number of stations varies from state to state as well as over time. It ranges between 4 stations (Delaware) to over 400 (Texas and California). The electricity consumption series are estimated as:

$$\frac{(R+C)_t^j}{(R+C)_{t-1}^j} - 1 = \Delta\overline{HTDD}_t^j + \Delta\overline{CLDD}_t^j + \sum_{i=1}^{11} MONTH_i + ELECT_t^j \tag{C.1}$$

where $(R+C)_t^j$ stands for the sum of the residential and commercial electricity usage in state j in month t. $\Delta\overline{HTDD}_t^j$ and $\overline{\Delta CLDD}_t^j$ are respectively the monthly variations in the average of heating degree days and cooling degree days over all land-based stations located in state j between month t and month $t-1$. $MONTH_i$ are dummy variables for the months of January to November and are included in the model in order to capture the within-year seasonal variations in electricity demand. $ELECT_t^j$ are the model residuals that represent the growth of electricity usage that is orthogonal to weather-driven and average within-year variations in power demand. We use $ELECT_t^j + 1$ (ELECT) from Equation (C.1) as a proxy for the monthly state-level gross consumption growth.

[45] Da & Yun (2010) obtain similar results in the context of CCAPM whether or not they exclude industrial electricity usage from total electricity consumption.

APPENDIX D – Construction of the repeated sales indices (RSI) for Chapter 4

This appendix offers a step-by-step discussion of the construction of the monthly gross municipal bond index returns used as test assets in the context of the CCAPM. The raw data comes from the Municipal Securities Rulemaking Board (MSRB) transaction database and covers the January 2005 to December 2016 period.

The MSRB dataset gives basic descriptive information on bonds such as the CUSIP identification number, the issuance date, the maturity date and the coupon rate, in addition to the date, time, quantity and clean price of each transaction. Among other things, the dataset also discloses a trade-type indicator that tells whether the trade is initiated by a buyer, a seller or a bond dealer. The dataset counts about 113.2 million observations on 2.4 million unique CUSIP. Moreover, we are able to associate individual municipal bonds to 50,036 municipal authorities, knowing that the first six characters of the CUSIP uniquely identify security issuers. Next, we use a web scraping tool to extract the state within which each issuer is located from the MSRB website. We exclude from our sample a few issuers that are spread across several states and bonds issued by American territories. We also discard bonds with missing or time-varying coupon rates to ensure that the calculation of accrued interest is possible.

Following Green, Hollifield & Schürhoff (2007), we distinguish between new issues and seasoned issues. The trading activity is usually much higher in the weeks following the sale date than in the remaining life of the municipal bonds. In the new issues period, the sales of municipal bonds to customers greatly exceed the buys. This suggests that the market price of newly issued bonds may be above the equilibrium price and this could obscure or mislead our analysis. We tackle this issue by excluding trades for which the difference between the trade date and the sale date is less than 28 days.[46] We also make sure that our calculations of bond returns are not biased by the bid ask spread. In the MSRB database, the two legs of a transaction are not identified. For example, if most transactions are initiated by the buyer and are followed by a seller-initiated observation, we may expect returns to be underestimated given that the database will exhibit a sequence of ask followed by bid prices. We proceed following Green, Hollifield & Schürhoff (2007)'s first-in-first-out (FIFO) approach to unite the two legs of a transaction. That approach consists in matching buyer-(seller)-initiated transactions with subsequent bond sells(buys) equal in par value. We allow for a maximum time interval of seven

[46] Green, Hollifield & Schürhoff (2007) use a 90-day long new issues period, but we prefer employing a shorter 28-day period. We later examine the sensitivity of the results to the 28-day period threshold.

days between matching transactions. We ignore the observations that cannot be associated with the legs of a round-trip transaction. Then, we calculate the mid-price of each round-trip transaction and assume that trades take place at the closing date of the transaction.[47] Next, we take for granted that coupons are distributed at a semi-annual frequency and that a 30/360 ISDA day count convention applies to calculate dirty bond prices. Starting with the dirty prices, we assume that coupons are fully reinvested upon payment to assess forward-looking total return prices (TRP).[48] Last, we extract the tax characteristics of the remaining bonds from Bloomberg and keep the CUSIP that are exempt from income tax at both the federal and state levels. The exclusion criteria and the construction of round-trip transactions greatly reduces the number of observations from 113.2 to 9.2 million on 1.1 million unique CUSIP sold by 35,252 distinct issuers.

The TRP dataset permits for the construction of municipal bond indices. Given our focus on state-level asset returns, we groups TRP by state. However, we deem desirable to calibrate CCAPM on more than one test asset. As it has been shown that differences in municipal borrowing costs across states vary by maturity (Kidwell, Koch & Stock, 1987), we choose to cluster our TRP dataset not only by state, but also by maturities using the remaining life of bonds at the time of a transaction as grouping variables. This grouping scheme is similar to the way firm sizes and the book-to-market ratios are often used to form stock portfolios in the literature. We create four maturity groups that respectively include bonds maturing in: (i) less than 2.5 years, (ii) between 2.5 and 5 years, (iii) between 5 and 7.5 years, and (iv) more than 7.5 years. The 2.5, 5 and 7.5 years thresholds correspond roughly to the first, second and third quartile of the distribution of the time-to-maturity. Thus, most of the empirical asset pricing tests in this paper employ four maturity-based portfolios of municipal bonds per state.

It is no surprise that most municipal bonds are seldom traded (Downing & Zhang, 2004; Harris & Piwowar, 2006; Reck & Wilson, 2006). The average of nine TRP by CUSIP means that

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[47] More than two-thirds of our round-trip transaction sample has opening and closing dates less than two days apart. More than 90 percent of the trades are completed within five calendar days.

[48] More precisely, TRP are estimated as: $TRP_t = P_t^{Dirty} \times \left(1 + \frac{CPN}{100}\right)^n$ where n stands for the number of coupon payments between the bond issuance and time t. A more exact formula would divide each coupon payment by P_{t-1}^{Dirty}. However many bonds are infrequently traded such that P_{t-1}^{Dirty} is unknown. Also, transactions are often clustered in time so that the effect of dividing all coupons by 100 remains negligible on returns calculated from two subsequent TRP.

constructing returns series at the individual bond level is not possible and that conventional methods used in the literature on stock markets to construct indexes are inadequate in the context of municipal bonds. Instead, we opt for a repeat-sales approach to build state-level returns series. Repeat-sales indexes (RSI) are widely used in Real Estate to estimate the price progression of the average house (Bailey, Muth & Nourse, 1963; Case & Shiller, 1987). Outside the housing literature, Hwang, Quigley & Woodward (2005) favour a RSI for private equity firms, Harris & Piwowar (2006) use a RSI to investigate transaction costs in the municipal bond market, and Beaupain & Heck (2016) study a RSI for the corporate bond market.

The repeat sales approach utilizes multiple transactions on a municipal bond and pools the information over all bonds in a market across the sample period using a stochastic process to estimate the average price change. While there exists multiple ways to construct a RSI[49], we follow the well-known method of Shiller (1991) and build an arithmetic RSI. Goetzmann (1992) and others argue that arithmetic RSIs facilitate cross-sectional comparisons, are more appropriate, and less biased than geometric indices. The approach of Shiller (1991) is employed to construct the S&P/Case-Shiller Home price Index. The fact that this method is computationally efficient and easy to implement is especially important given the large size of our dataset.

Haurin & Hendershott (1991) discuss the drawbacks of the RSI approach used to model house prices. They argue that a RSI does not account for depreciation and renovation, that house attributes may change over time, that data samples are often not representative of the stock of housing and that a large number of sales are required for the estimation to have decent power. The application of the RSI approach to our sample of municipal bonds is mostly unaffected by those criticisms. Indeed, depreciation and renovation is hardly an issue. While some bond characteristics may change through time, we believe that the extent of these changes is way less pronounced than in the housing market. The fact that trades tend to be somewhat clustered in time also lessen the issues related to time-varying characteristics. Finally, the large size of our data sample ensures that our estimation process produces robust RSI series. Still, many municipal bonds do not trade at all and we are forced to exclude about one quarter of the bonds in our sample as they are traded only once in the post new issues period. In addition to these considerations, we resort to the simple aggregation scheme used by Beaupain & Heck (2016) and retain the last TRP of the month for each bond in order to generate a RSI

[49] RSI can be constructed using either a geometric or an arithmetic average of bond prices. Besides the conventional RSI methodology, Case & Quigley (1991) and Quigley (1995) advocate the use of a hybrid hedonic-repeat sales model while Nagaraja, Brown & Zhao (2011) promote an autoregressive approach.

sampled at the monthly frequency. The information retained in the dataset is then reorganized such that each row corresponds to a combination of two subsequent transactions on the same CUSIP.

The approach of Shiller (1991) consists of a weighted-least square (WLS) regression for which the first step consists in estimating the following equation:

$$\begin{aligned} P_{i0} &= \beta_{t'} P_{it'} + \varepsilon_i && \text{first trade at time 0} \\ 0 &= \beta_{t'} P_{it'} - \beta_t P_{it} + \varepsilon_i && \text{first trade at time t>0} \end{aligned} \tag{D.1}$$

where P_{it} is the TRP of municipal bond i in month t. Let t be the time at the first sale of a transaction pair and t' the time at the second trade ($t' > t$). Note that month 0 corresponds to January 2005 and month $t = T$ refers to December 2016.

The classic RSI of Bailey, Muth & Nourse (1963) uses the reciprocal of β_t in Equation (D.1) as index values. However, the OLS regression assumes that the residuals have constant variance and Case & Shiller (1987) argue that the variance should increase with the time interval between subsequent sales. Accordingly, they propose a WLS regression where the weights are obtained by regressing the squared residuals from Equation (D.1) on the time interval, in months, between succeeding trades such that:

$$\hat{\epsilon}^2 = b_0 + b_1(t' - t) + \eta \tag{D.2}$$

where $\hat{\epsilon}^2$ are the squared residuals from Equation (D.1) and $(t' - t)$ is the number of months between a transaction and the previous one. The reciprocal of the square root of the fitted values from Equation (D.2) are the weights W to be used in the final WLS regression.

$$W = \left(\hat{b}_0 + \hat{b}_1(t' - t)\right)^{-0.5} \tag{D.3}$$

The weighing scheme reduces the impact of trade pairs with large time interval on the index estimates. The value of the RSI corresponds to the reciprocal of the betas of the WLS regression. However, our use of bond prices, a stochastic variable, among the independent variables violates the assumption that the explanatory variables are fixed (Shiller, 1994 p.148). To accommodate this issue, the WLS regressions are estimated using an instrumental-variables approach such that the betas are obtained as:

$$\beta = (Z'\Sigma^{-1}X)^{-1}(Z'\Sigma^{-1}Y) \quad \text{(D.4)}$$

where Σ is a diagonal weighting matrix that is built as the identity matrix in Equation (D.1) and where the weights W of Equation (D.3) are the diagonal elements in the final-stage WLS regression. The matrix Z of Equation (D.4) is constructed as X, the matrix of regressors in Equation (D.1), where $\mathrm{P}_{\mathrm{it}'} \neq 0$ are replaced with 1 and $P_{it} \neq 0$ are replaced with -1.

APPENDIX E – Alternative CCAPM with recursive preferences for Chapter 4

Our main analysis in Chapter 4 has used the conventional power utility CCAPM framework. As advocated by Epstein & Zin (1991) and others, that framework constraints the elasticity of intertemporal substitution (EIS), or how investors prefer to smooth consumption over time, to be the reciprocal of the RRA parameter. Mehra (2012) notes that this restriction follows no underlying economic rationale. One of the popular alternative models of decisions under uncertainty is the recursive utility framework first described by Kreps & Porteus (1978) that says that current utility depends on the expected values of future utility. One major advantage of recursive preferences is that it allows for a disentanglement of RRA from EIS.

Our empirical tests of the disaster-driven increase in risk aversion assumption under recursive preferences employ the well-known utility function presented by Epstein & Zin (1989; 1991). Epstein and Zin show that the stochastic discount factor implied by their recursive utility specification can be expressed as:

$$\mathrm{m_{j,t}} = \left\{\frac{\mathrm{C_{j,t}}}{\mathrm{C_{j,t-1}}}\right\}^{\frac{-\rho_j(1-\alpha_j)}{(1-\rho_j)}} \left\{\frac{1}{R_t^w}\right\}^{(\alpha_j-\rho_j)(1-\rho_j)} \tag{E.1}$$

where $\frac{\mathrm{C_{j,t}}}{\mathrm{C_{j,t-1}}}$ is the growth in consumption in state j at time t and R_t^w is the gross return of the (unobservable) wealth portfolio that is assumed to be common to all states. $1/\rho_\mathrm{j}$ is the EIS parameter and α_j is the RRA parameter. Following Epstein & Zin (1991) and others, we implement recursive preferences using the market portfolio, that we define as an equally-weighted portfolio consisting of US stocks traded at the NYSE, AMEX and NASDAQ available from the Center for Research in Security Prices (CRSP), as our proxy for the wealth portfolio.

We proceed by using a two-step approach. First, we obtain parameter estimates one state at the time from Equations (4.2b – 4.2c) when the stochastic discount factor is defined by Equation (E.1) instead of Equation (4.2a). Based on the results reported in Table E.1, we observe that the cross-state average RRA is 3.33 with one state (West Virginia) having an economically implausible negative coefficient. The RRA estimates obtained using recursive preferences are significantly correlated with those presented in Figure 4.1 obtained using the conventional approach ($\rho^{sp} = 0.62$) and also positively correlated with historical state-level disaster-related damage ($\rho^{sp} = 0.19$) and the demand for insurance series ($\rho^{sp} = 0.19$). Estimates of EIS are much lower than RRA with a cross-state average of 0.25. The somewhat low levels of EIS inferred from the data imply that consumption growth is relatively insensitive to changes in the real interest rate in the 2005 to 2016 sample period. Such an average EIS is consistent with the conclusions of the meta-analysis of Havránek (2015) who reports a mean EIS of zero for studies using macro data and a EIS of around 0.3–0.4 for studies using micro data. All states have a coefficient of EIS ranging between 0.10 and 0.41, except Massachusetts that obtains an implausible coefficient of -1.71 and West-Virginia that shows a coefficient of 2.48 which is at odds with the other states.

[Insert table E.1 around here]

We assess the effect of natural disasters on risk aversion in the second step where a disaster-driven risk aversion parameter is added to Equation (E.1) such that $\alpha_j = \gamma_j + \mathbf{1}_{j,k}^{FEMA}\gamma^D$, with $k = 6$. The parameters estimated in the first step and disclosed in Table E.1 are used as priors. Note that we leave the issue of disaster-driven changes in EIS to future studies. We estimate the value of the disaster-driven RRA parameter at 0.24, which implies that the average rise in risk aversion caused by extreme weather events is about 7.2 percent. This estimate is consistent with our previous results using the convention CCAPM.

APPENDIX F – Alternative CCAPM with loss aversion for Chapter 4

Our main analysis in Chapter 4 relies on the conventional power-utility complete-market CCAPM. This model assumes that investors are fully rational and make optimal decisions. The previous literature suggests that the most likely explanations for a disaster-induced increase in risk aversion are associated with behavioral rather than rational considerations. In other words, the effect of disasters may be more consistent with the cumulative prospect theory of Tversky & Kahneman (1992) or with the risk-as-feeling theory of Loewenstein, Weber, Hsee & Welch (2001) than with the expected utility theory. Accordingly, it seems interesting to examine how the disaster-related increase in risk aversion assumption fares under an alternative utility specification that allows for behavioral considerations. The asset pricing model developed by Barberis, Huang & Santos (2001) offers such an opportunity.

Consistent with available empirical evidence, Barberis, Huang & Santos (2001) assume that investors' decisions are driven in part by consumption, and in part by prior outcomes in terms of portfolio performance. The first component can be interpreted as a form of internal habit persistence. Barberis et al. then use insights from prospect theory to suppose that investors exhibit loss aversion, which means that investors are more sensitive to reductions than to increases in financial wealth. Barberis, Huang & Santos (2001) show that a model that combines the effect of loss aversion with the effect of prior financial outcomes leads to the following fundamental (unconditional) asset pricing equation:

$$1 = \mathrm{E}\left[\frac{\mathrm{C_{j,t+1}}}{\mathrm{C_{j,t}}}^{-a_j} R_{j,t+1}^{i}\right] + b_j \mathrm{E}[\hat{v}(R_{t+1}^{m})]$$

where (F.1)

$$\hat{v}(R_{t+1}^{m}) = \begin{cases} R_{t+1}^{m} - R_t^f & \\ \lambda\left(R_{t+1}^{m} - R_t^f\right) & \end{cases} \quad for \quad \begin{matrix} R_{t+1}^{m} \geq R_t^f \\ R_{t+1}^{m} < R_t^f \end{matrix}$$

Equation (F.1) can be thought of as an extension of the base model. Indeed, we see that Equation (F.1) reverts to the standard power-utility CCAPM when the parameter b_j is set to zero. b_j is a positive state-level constant that defines the importance of utility from gains and losses in financial wealth relative to utility from consumption. We use the value-weighted portfolio consisting of US stocks traded at the NYSE, AMEX and NASDAQ available from the Center for Research in Security Prices (CRSP), as our proxy for the portfolio of financial wealth R_t^m. The risk-free rate R_t^f is based on the one-month Treasury bill rate from Ibbotson Associates available from Kenneth French' data library.[50] λ is a penalty factor that determines how investment losses are felt relative to gains. We follow Barberis, Huang & Santos (2001) and Tversky & Kahneman (1992) and fix the value of λ to 2.25. Hence, in our setup, we assume that the relative importance of investment returns (b_j) on utility varies from state to state but that the loss aversion factor (λ) is constant throughout the U.S. This asymmetry in our assumptions reflects the fact that we acknowledge that most financial markets, including the stock market, the government bond market and the corporate bond market, are not segmented along geographical lines. Accordingly, whereas R_t^m and R_t^f are plausibly suitable indicators of the financial gains and losses of all state-level aggregate investors, they cannot be used to infer a regional loss aversion factor.

We proceed with the estimation using a two-step approach. First, we obtain parameter estimates one state at the time from Equations (4.2b – 4.2c) when the stochastic discount factor is defined by Equation (II.1) instead of Equation (4.2a). We use the identity matrix as the weighting matrix in Equation (4.2c) for simplicity. We report the results from this analysis in Table F.1. We observe that this approach gives more volatile RRA estimates that vary between -8.2 and 36.3 with a mean of 3.9. The parameter b_j also exhibits large variations from state to state, but is not statistically different from zero in all cases. The mean value of b_j is 7.8.

[Insert Table F.1 around here]

We assess the effect of natural disasters on risk aversion in the second step where a disaster-driven risk aversion parameter is added to Equation (F.1) such that $a_j = \gamma_j + \mathbf{1}_{j,k}^{FEMA}\gamma^D$, with $k = 6$. The parameters estimated in the first step and disclosed in Table F.1 are used as priors. Adding a country-wide disaster-induced risk aversion parameter brings no material change to the other estimated parameters. We obtain $\gamma^D = 0.23$ which supports the disaster-induced increase in risk aversion story.

[50] URL : http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html

TABLES

TABLE 2.1 Major natural disasters by type and state

Table 2.1 presents the number of natural disasters by state and by type of natural disasters between 1990 and 2014. We only consider disasters that caused major damages estimated at more than 25$ per resident (in constant 2014 USD) at the state-level. We require at least 15 years of state-specific historical stock returns and at least 3 disasters of the same type to include a category in our approach using state-level portfolios.

State	Total	Storms	Floods	Extreme Temp.	Winter Weather	Fire
Nb Events-Total	**247**	**143**	**47**	**29**	**21**	**7**
		Retained events for the state-level portfolio analysis				
Alabama*	10	8	-	-	-	-
Arizona	3	3	-	-	-	-
California	9	-	-	4	-	-
Colorado*	17	9	4	-	-	3
Florida	17	14	-	-	-	-
Georgia	8	3	-	3	-	-
Iowa*	18	-	-	3	-	-
Illinois*	11	5	4	-	-	-
Indiana	6	3	-	-	-	-
Kansas*	16	11	-	-	3	-
Kentucky	8	3	3	-	-	-
Louisiana*	20	11	4	4	-	-
Massachusetts	3	3	-	-	-	-
Maryland	4	4	-	-	-	-
Minnesota*	13	7	5	-	-	-
Missouri*	16	7	4	-	3	-
North Carolina	6	6	-	-	-	-
New Jersey	7	4	-	-	-	-
Oklahoma*	17	10	-	3	3	-
Oregon	3	-	3	-	-	-
Pennsylvania	5	3	-	-	-	-
Tennessee	8	4	3	-	-	-
Texas*	15	10	3	-	-	-
Virginia	7	5	-	-	-	-
By category						
Nb Disasters	195	133	33	17	9	3
Nb Categories	39	20	9	5	3	1

Notes: State names followed by a star (*) are the ones included in our approach based on pairing individual firms with individual disasters.

TABLE 2.2 Descriptive statistics of the state-level portfolio returns

Table 2.2 presents the descriptive statistics of the state-level return series. Stock returns of U.S. firms between 1990 and 2014 are grouped into state-based portfolios according to their headquarter locations and the daily equally-weighted total returns of these portfolios are calculated. States are kept in the analysis if at least 15 years of historical returns are available. The null hypothesis of the Jarque-Bera test assumes that the data come from a normal distribution. The null of the Box-Ljung test assumes the data are independently distributed so that there is no serial correlation. The null of Engle's ARCH test assumes that there is no conditional heteroscedasticity. We use 10 lags for the Box-Ljung tests and one lag for the ARCH test proposed by Engle.

							p-values		
State	**Years of returns**	**Mean**	**Median**	**Std dev**	**Skewness**	**Kurtosis**	**Jarque-Bera**	**Box-Ljung**	**ARCH**
Alabama	19.3	.0007	.0010	.0106	0.1675	15.6977	.0000	.0020	.0000
Arizona	25.0	.0010	.0014	.0126	-0.2623	6.9790	.0000	.0000	.0000
California	25.0	.0010	.0020	.0126	-0.3132	8.3246	.0000	.0000	.0000
Colorado	25.0	.0011	.0015	.0123	-0.4928	11.5688	.0000	.0000	.0000
Florida	25.0	.0011	.0014	.0103	-0.3921	9.0553	.0000	.0000	.0000
Georgia	25.0	.0009	.0013	.0111	-0.2699	9.3695	.0000	.0000	.0000
Iowa	16.9	.0009	.0010	.0081	0.1421	4.9481	.0000	.0047	.0000
Illinois	25.0	.0008	.0013	.0106	-0.3143	11.1191	.0000	.0000	.0000
Indiana	25.0	.0008	.0011	.0099	-0.1818	13.2074	.0000	.0000	.0000
Kansas	17.2	.0010	.0010	.0130	2.6469	39.1563	.0000	.0000	.0000
Kentucky	25.0	.0008	.0009	.0101	-0.2343	8.3595	.0000	.0486	.0000
Louisiana	25.0	.0007	.0008	.0121	-0.0078	9.8323	.0000	.0056	.0000
Massachusetts	25.0	.0011	.0017	.0119	-0.2672	8.3654	.0000	.0000	.0000
Maryland	25.0	.0010	.0013	.0112	-0.1702	8.5678	.0000	.0000	.0000
Minnesota	25.0	.0011	.0014	.0100	-0.3478	8.6741	.0000	.0000	.0000
Missouri	25.0	.0008	.0012	.0108	-0.2167	13.0133	.0000	.0000	.0000
North Carolina	25.0	.0009	.0011	.0099	-0.0936	9.8219	.0000	.0000	.0000
New Jersey	25.0	.0011	.0016	.0101	0.0692	15.3257	.0000	.0000	.0000
Oklahoma	25.0	.0012	.0013	.0137	-0.1703	10.3146	.0000	.0000	.0000
Oregon	25.0	.0011	.0012	.0132	-0.0162	8.4491	.0000	.0002	.0000
Pennsylvania	25.0	.0009	.0013	.0103	-0.2155	11.2277	.0000	.0000	.0000
Tennessee	25.0	.0009	.0013	.0114	-0.3626	11.4048	.0000	.0000	.0000
Texas	25.0	.0010	.0016	.0119	-0.3640	14.0904	.0000	.0000	.0000
Virginia	25.0	.0009	.0013	.0100	-0.2912	9.6672	.0000	.0000	.0000

TABLE 2.3 Estimated coefficient of the ARMA-EGARCH model

Table 2.3 presents the estimated coefficients of ARMA-EGARCH model without the intervention variables. The dependant variable is the daily stock returns of state-level portfolios between January 1990 and December 2014.

State	Constant	AR(1)	AR(2)	MA(1)	MA(2)	Constant	GARCH	ARCH	Leverage
	Conditional mean equation					*Conditional variance equation*			
Alabama	.0008***					-.0618***	.9935***	.0994***	-.0450***
Arizona	.0001***	.8955		.7111***		-.1830***	.9799***	.1502***	-.0829***
California	.0001***	.9510***		.2649	-.1009	-.1528***	.9838***	.1741***	-.1018***
Colorado	.0001***	.9180***		.6785***		-.1719***	.9814***	.1372***	-.0871***
Florida	.0001***	.9336***		-.8919***		-.1860***	.9807***	.1625***	-.0880***
Georgia	.0001***	.9205***		.7570***		-.1665***	.9825***	.1635***	-.0896***
Iowa	.0010***	-.0767***		-.1544***		-.1328***	.9864***	.1044***	-.0320***
Illinois	.0001***	.9292***		-.8781***		-.1076***	.9890***	.1440***	-.0849***
Indiana	.0000***	.9469***		-.9215***	.0862***	-.0963***	.9902***	.1502***	-.0595***
Kansas	.0002**	.7700***				-.0636**	.9889***	.2743***	-.1401***
Kentucky	.0000**	.9302***				-.1431***	.9849***	.1292***	-.0710***
Louisiana	.0007***					-.1145***	.9875***	.1205***	-.0515***
Massachusetts	.0028***	-.9158***		-.8601***		-.2039***	.9782***	.1967***	-.0632***
Maryland	.0001***	.9391***		-.9121***		-.1375***	.9853***	.1410***	-.0751***
Minnesota	.0001***	.9491***		-.5124**	-.3431	-.1700***	.9824***	.1415***	-.0903***
Missouri	.0001***	.9349***				-.0934***	.9904***	.1346***	-.0735***
North Carolina	.0000**	.9645***		-.9224***		-.1138***	.9882***	.0964***	-.0897***
New Jersey	.0001***	.9161***		-.8429		-.1974***	.9798***	.1781***	-.0843***
Oklahoma	.0014***	-.0451***				-.1188***	.9867***	.1234***	-.0478***
Oregon	.0022***	-.9017***	-.0010	-.9068***		-.1395***	.9845***	.1462***	-.0491***
Pennsylvania	.0001***	.9179***		-.1676	-.6715***	-.1419***	.9855***	.1602***	-.0863***
Tennessee	.0000*	.9017***		-.9367***		-.1223***	.9870***	.1323***	-.0616***
Texas	.0001***	.9282***		.4154	-.0232	-.1371***	.9857***	.1700***	-.0859***
Virginia	.0001***	.9449***		-.9018***		-.1540***	.9840***	.1473***	-.0746***

*Notes: ***, **, * denote significance at the 1%, 5% and 10% levels, respectively.*

TABLE 2.4 The impact of natural disasters on state-based portfolio returns

Table 2.4 shows the estimated coefficients of the intervention variables of the ARMAX-EGARCH model. The dependant variables are the equally-weighted returns of state-level portfolios. The intervention variables are dummy variables that equal one when a disaster of a given category happens in a state. Disasters are grouped into five categories: storms, floods, episodes of extreme temperature, winter weather and fire. In panel A, the event window is one day long and corresponds to the day where the disaster is the most discussed in national evening news programs. Coefficients can be interpreted as the daily abnormal returns at the peak date. In panel B, the event period is 40 days long and starts the day preceding the peak date. Coefficients can be interpreted as the compounded abnormal daily returns over the 40-day period.

	Panel A – One day event period					**Panel B – 40-day event period**				
State	**Storm**	**Flood**	**Extreme Temp.**	**Winter Weather**	**Fire**	**Storm**	**Flood**	**Extreme Temp.**	**Winter Weather**	**Fire**
Alabama	.0008					.0107				
Arizona	.0013					.0025				
California			.0001					.0092**		
Colorado	.0001	.0023			.0002	-.0026	.0095**			-.0002
Florida	.0000					.0026				
Georgia	.0008		-.0004			.0008		-.0049		
Iowa			.0015					-.0205		
Illinois	.0004	.0001				-.0031	.0005			
Indiana	-.0008					-.0048				
Kansas	-.0019**			-.0045		-.0123**			.0001	
Kentucky	-.0004	-.0008				-.0021	-.0070**			
Louisiana	-.0006	.0007	.0010			.0341	.0067	.0676*		
Massachusetts	-.0053					.0094				
Maryland	-.0006					-.0048				
Minnesota	-.0005	.0007				-.0013	.0011			
Missouri	.0014*	-.0004		-.0002		.0047*	.0008		.0014	
North Carolina	-.0001					-.0009				
New Jersey	-.0017					-.0010				
Oklahoma	.0028		.0043	.0062*		.0512**		.0015	.0642	
Oregon		-.0042					-.0150			
Pennsylvania	-.0006					-.0023				
Tennessee	.0021*	-.0007				.0097**	-.0083			
Texas	.0003	.0005	.0023			-.0010	-.0079**	.0236*		
Virginia	-.0008					-.0037				

*Notes : ***, **, * denote significance at the 1%, 5% and 10% levels, respectively.*

TABLE 2.5 The impact of natural disasters on the stock returns of individual firms

Table 2.5 displays the proportion of individual firm-disaster pairs that exhibit significant abnormal returns (AR) at the 0.05 level. Abnormal returns correspond to the coefficients of the exogenous intervention variables of an ARMAX-GARCH model. The single-day event period corresponds to the peak date of a disaster. The longer event periods start the day preceding the peak date. The length of the event period is expressed in number of trading days. A 60-day event period is approximatively equal to three months while the 125-day event period corresponds to half a year.

Disaster category	Obs.	Event Period Length (in trading days)						
		1	**5**	**20**	**40**	**60**	**80**	**125**
PANEL A – All Firm-Disaster Pairs								
% Significant AR	**2,146**	**6.2**	**7.4**	**8.9**	**9.6**	**8.2**	**7.7**	**7.4**
% Positive AR	2,146	52.4	58.1	53.7	51.1	49.8	47.6	50.2
% Negative AR	2,146	47.6	41.9	46.3	48.9	50.2	52.4	49.8
% Significant Positive AR	-	8.2	9.1	9.2	8.7	6.9	6.0	6.5
% Significant Negative AR	-	4.0	4.9	8.7	10.7	9.6	9.3	8.2
PANEL B – Proportion of Significant Abnormal Returns by per Capita Disaster Loss Estimates (in %)								
< $75	786	5.9	6.9	8.5	8.7	8.3	7.1	7.6
$75-150	600	6.3	8.7	8.2	6.8	5.5	5.5	6.7
$150-500	499	5.8	6.0	8.6	8.8	7.4	6.8	5.6
+$500	261	7.7	8.4	12.6	20.7	16.1	16.5	11.5
PANEL C – Proportion of Significant Abnormal Returns by Disaster Duration (in %)								
1 day	526	9.3	8.4	10.3	8.9	7.0	4.8	6.8
2-3 days	603	4.3	7.6	7.3	8.3	6.6	6.6	6.6
4-7 days	618	5.3	6.8	9.5	11.5	10.0	10.2	8.4
8-21 days	242	7.0	4.5	8.3	7.4	9.1	9.5	7.4
+21 days	157	5.1	9.6	9.6	13.4	10.2	9.6	7.6

TABLE 2.6 Disaster-related cumulative abnormal returns on firms in neighbour states

Table 2.6 describes the impact of local disasters on firms headquartered in local and neighbour states. The distance between state borders is used to identify the closest neighbouring states. Numbers represent the compounded cumulative abnormal returns (AR) over a period of 40 days. Abnormal returns correspond to the estimated coefficients of the disaster-related dummy variables of an ARMAX-EGARCH model. The dependant variable of the ARMAX-EGARCH model is the daily returns on equally-weighted state-level portfolios. Panel A illustrates the effect of storm-like events. Panel B shows the impact of floods. Panel C depicts the effect of episodes of extreme temperature. Three states are selected in each panel according to the significance of the events on local firms. The impact of disasters on local firms (in grey) is the same as in Table 2.4.

Panel A – Storms in selected states

STATE	***Kansas***		***Oklahoma***		***Tennessee***	
LOCAL	KS	-.0123 **	**OK**	.0512 **	**TN**	.0097 **
CLOSEST NEIGHBOURS	*CO	.0011	*CO	.0524 **	*AL	.0049
	*MO	-.0050	*MO	-.0199	*GA	.0100 **
	*OK	.0002	*KS	-.0244	*KY	.0106 ***
	TX	-.0024	*TX	.0255	*MO	.0078 *
	IA	-.0027	LA	-.0184	*NC	.0068 *
	IL	-.0041	IA	-.0027	*VA	.0069 **
	MN	-.0009	TN	.0090	IL	.0067

Panel B – Floods in selected states

STATE	***Colorado***		***Kentucky***		***Texas***	
LOCAL	**CO**	.0095 **	**KY**	-.0070 **	**TX**	-.0079 **
CLOSEST NEIGHBOURS	*AZ	.0047	*IL	-.0059 **	*LA	-.0124 ***
	*KS	.0065	*IN	-.0059 **	*OK	.0002
	*OK	.0061	*MO	-.0035	KS	-.0059
	TX	.0043	*TN	-.0053	CO	-.0047
	IA	.0012	*VA	-.0051 *	AZ	-.0012
	CA	.0075	NC	-.0056 **	MO	-.0052
	MN	.0021	AL	-.0064 **	TN	-.0091 **

Panel C – Episodes of extreme temperature in selected states

STATE	***California***		***Louisiana***		***Texas***	
LOCAL	**CA**	.0092 **	**LA**	.0676 *	**TX**	.0236 *
CLOSEST NEIGHBOURS	*AZ	.0063	*TX	.0330	*LA	-.0009
	*OR	.0095 **	AL	.0131	*OK	.0163
	CO	.0099 ***	OK	.0389	KS	.0000
	TX	.0053	FL	-.0208	CO	.0183
	OK	.0063	TN	-.0056	AZ	.0070
	KS	.0002	MO	.0133	MO	.0075
	IA	.0054	KY	-.0286	TN	.0041

*Notes: State abbreviations that are preceded by a star (*) indicates neighbours that share a border with the local state. ***, **, * denote significance at the 1%, 5% and 10% levels, respectively.*

TABLE 2.7 The proportion of 'true' significant individual disasters

Table 2.7 shows the proportion of individual disasters with significant abnormal returns (1- $\pi_0(\lambda^*)$) according to the length of the event period after controlling for false discoveries. Abnormal returns correspond to the coefficients of the exogenous intervention variables of an ARMAX-GARCH model. The single-day event period corresponds to the peak date of a disaster. The longer event periods start the day preceding the peak dates. The length of the event period is expressed in number of trading days. A 60-day event period is approximatively equal to three months while the 125-day event period corresponds to half a year. In Panel A, the dependant variables are the total returns of individual stocks of firms headquartered in eleven selected states. In Panel B, the dependant variables of the ARMAX-GARCH model are the state-level portfolio returns. Panel C present the proportion of significant abnormal returns according to the event year. The 2008-2009 period is associated with the global financial crisis.

Disaster category		**Event Period Length (in trading days)**					
		1	**5**	**20**	**40**	**60**	**125**
PANEL A – Individual disasters and individual firms (N = 2,246)							
	$\boldsymbol{\pi_0(\lambda^*)}$	100.0%	100.0%	99.7%	93.2%	96.6%	97.5%
	$\mathbf{1-\pi_0(\lambda^*)}$	0.0%	0.0%	0.3%	6.8%	3.4%	2.5%
PANEL B– Individual disasters and state-level portfolio returns (N = 195)							
	$\boldsymbol{\pi_0(\lambda^*)}$	100.0%	100.0%	100.0%	98.9%	93.0%	99.8%
	$\mathbf{1-\pi_0(\lambda^*)}$	0.0%	0.0%	0.0%	1.1%	7.0%	0.2%
PANEL C– Impact of the global financial crisis							
2008-09	**% significant AR**	10.5%	14.6%	19.2%	28.3%	22.8%	16.0%
	% true significant AR	0.0%	8.0%	16.5%	34.0%	22.0%	9.6%
1990-07 & 2010-14	**% significant AR**	5.9%	6.7%	8.3%	8.8%	7.6%	7.1%
	% true significant AR	0.0%	0.0%	0.5%	2.2%	0.0%	0.0%

TABLE 2.8 Cumulative abnormal volatility around major natural disasters

Table 2.8 reports the cumulative abnormal volatility (CAV) around major natural disasters in the event window (S_1, S_2). Daily abnormal volatility represent the ratio between realized and expected conditional volatility using a GARCH(1,1) model. The significance of CAV is assessed by means of bootstrapping. The empirical distribution of CAV under the null of no abnormal volatility is constructed using 10,000 iterations. Results are presented for the full sample of major natural disasters as well as by categories of disasters. In Panel A, only a constant term enters the conditional mean equation. In Panel B, the state-based portfolio excess returns are regressed on the market excess returns to generate residuals.

Event Period (S_1, S_2)	Storms	Floods	Extreme Temp.	Winter Weather	All Disasters	Hurricanes	Other storms
PANEL A – Using conditional mean Equation 2.8a							
(0,1)	0.25	1.03***	1.59**	2.82***	0.89	1.00*	-0.02
(0,2)	0.61	0.88***	1.94***	6.46***	1.71	0.80*	0.43
(0,3)	0.69	1.63***	2.73***	8.31***	2.21	1.94*	0.11
(0,4)	0.79	1.55***	3.31***	8.73***	2.48	2.28*	0.14
(0,5)	0.66	1.60***	3.99***	8.53***	2.42	2.11*	0.05
(0,6)	0.62	2.37***	5.24***	9.16**	2.69	2.78*	-0.32
(0,7)	1.02	2.62***	5.30***	9.02**	2.90	4.73*	-0.35
(0,8)	0.91	2.11***	5.23***	9.25**	2.73	5.12*	-0.65
(0,9)	0.66	1.82***	5.28***	9.30**	2.55	5.12*	-0.98
(0,10)	0.37	1.34***	6.37***	8.90**	2.31	5.43*	-1.44
(-2,-1)	-0.10	-0.27	0.66	0.27	-0.09	-0.08	-0.13
Obs	**59**	**21**	**12**	**14**	**108**	**16**	**43**
PANEL B – Using conditional mean Equation 2.8b							
(0,1)	0.36	2.00***	0.93**	1.25	0.79	0.90	0.20
(0,2)	0.54	2.32***	1.34***	2.53	1.13	0.80	0.48
(0,3)	1.18	2.57***	1.87***	5.98**	1.92	1.90	0.99
(0,4)	1.77	2.80***	2.11***	6.95**	2.45	3.57	1.27
(0,5)	1.75	2.56***	1.97***	6.96	2.34	3.06	1.48
(0,6)	1.54	2.65***	2.12***	7.50	2.28	2.62	1.27
(0,7)	2.54	2.03***	2.67***	9.34	3.01	5.17	1.78
(0,8)	2.80	1.69***	2.81***	9.25	3.05	5.99	1.86
(0,9)	2.92	1.86***	2.69***	9.61	3.16	6.05	2.02
(0,10)	3.00	2.00***	3.41***	9.49	3.24	6.35	2.06
(-2,-1)	-0.18	-0.36	0.92	0.38	-0.10	-0.35	-
Obs	**59**	**21**	**12**	**14**	**108**	**16**	**43**

*Notes: ***, **, * denote significance at the 1%, 5% and 10% levels, respectively.*

TABLE 3.1 Definition of the variables

Table 3.1 describes the explanatory variables included in the base model of Chapter 3 and provides information about the expected sign of the related coefficient. The table also discloses the source of the raw data.

VARIABLE	DESCRIPTION	EXPECTED SIGN	SOURCE
BMK	Average yield of municipal bonds of similar credit quality and time-to-maturity.	+	MSRB
CPN [Tranche level]	Categorical variable that distinguishes discount bonds (coupon > yield), premium bonds (coupon < yield) and zero coupon bonds from bonds issued at par.	+ Discount + Premium + Zero	MSRB
IsCALL [Tranche level]	Dummy variable that equals one if the tranche is callable.	+	Bloomberg
IsEXTRACALL [Tranche level]	Dummy variable that equals one if an extraordinary call option is associated with the tranche.	+	Bloomberg
IsSINK [Tranche level]	Dummy variable that equals one if the tranche is sinkable.	-	Bloomberg
SIZE [Tranche level]	Natural logarithm of the tranche's principal amount.	-	Bloomberg
TTM [Tranche level]	Natural logarithm of the time-to-maturity (in years).	+	MSRB
UNRATED [Tranche level]	Dummy variable that equals one if the tranche is not rated by major credit agencies.	-	Bloomberg
COLLATERAL [Issue level]	Dummy variable that equals one if the tranche has high quality securities as collateral.	-	Bloomberg
COMPETITIVE [Issue level]	Dummy variable that equals one if the bond issue is competitive bid.	-	Bloomberg
IsLIMITED [Issue level]	Dummy variable that equals one if the issue is a G.O. limited-tax bond.	+	Bloomberg
IsINSURED [Issue level]	Dummy variable that equals one if an insurance company guarantees the timely payments of capital and interest.	-	Bloomberg
PURPOSE [Issue level]	Categorical variable that distinguishes tranches issued for general reasons, for refunding purposes, for pension funding or for other purposes..	- Refund + Pension + Other	Bloomberg
UW_OTH [Issue level]	Dummy variable that equals one if the lead underwriter is not domiciled in the same state as the issuer and is not a major, prestigious, company that marketed more than 100 issues spread over more than 30 states.	+	Bloomberg
DEBT [County level]	Per capita amount of total debt in $1,000. Missing data are estimated using linear interpolation.	+	Government Finance Database[51]

[51] Data available at http://willamette.edu/mba/research_impact/public_datasets/index.html (Page consulted on March 10, 2017). The database is constructed from U.S. Census' annual surveys of state and local government finance. See Pierson, Hand, & Thompson (2015) for a detailed methodology.

Table 3.1 (cont.) *Definition of the variables*

VARIABLE	DESCRIPTION	EXPECTED SIGN	SOURCE
INCOME [County level]	Annual estimate of the median household income by county in thousand $US.	-	U.S. Census
POP [County level]	Natural logarithm of the estimate number of residents expressed in millions.	+	U.S. Census
POPgwth5y [County level]	Population growth in the five-year period preceding a new muni issue.	+	U.S. Census
CORRUPT [State level]	Ranking of the states from the most to the least corrupted, based on the number of public corruption convictions per 100,000 population between 1976 and 2010.	+	Simpson et al. (2012)
ELECTION [State level]	Dummy variable that equals one if the issue is sold in the 100-day period preceding an in-state gubernatorial election.	+	Wikipedia
LEADING [State level]	Monthly state leading indexes produced by the Federal Reserve Bank of Philadelphia.	-	FRB of Philadelphia
TAXADV [State level]	Tax advantage (in basis points) calculated as the product of the maximum annual state income tax rate with a dummy that equals one if munis are exempt from state taxes.	-	NBER's TAXSIM
IRVOL [National level]	Average value of the CBOE 10-year US Treasury note volatility index in the 20 trading days preceding a new issue.	+	CBOE
SENTIMENT [National level]	Average monthly premium on U.S. closed-end municipal bond funds with a national geographic focus and a long-term perspective.	-	Morningstar
UNCERTAINTY [National level	Cross-sectional standard deviation of the monthly premium on U.S. Closed-end municipal bond funds.	+	Morningstar
%Area [Flood Risk]	Proportion of a county's area that is located in high flood risk zones based on the NFHL database.	+	FEMA
%Pop [Flood Risk]	Estimated proportion of a county's population that resides in small areas that intersect high flood risk zones based on the juncture between the NFHL database and the 2010 Decennial Census block maps.	+	U.S. Census and FEMA
DmgFlood [Flood Risk]	Total damage, expressed in constant 2015 million US$, of all major floods that occurred in the county in the 15 years preceding a bond issue.	+	NCEI
HistoFlood [Flood Risk]	Number of major floods experienced by a county in the 15 years preceding a bond issue.	+	NCEI
IsFlood [Flood Events]	Dummy variable that equals one if a major flood occurred in the county in the year preceding a bond issue.	+	NCEI

TABLE 3.2 Descriptive statistics

Table 3.2 presents selected summary statistics on the explanatory variables presented in Table 3.1. Panel A displays frequency information about the main categorical and dummy variables. Panel B informs about the continuous variables. Panel C compares some characteristics of the post-flood issues subsample with the control sample.

Panel A – Dummy Variables

Regressor	Freq	%Freq	Regressor	Freq	%Freq
CPN=Discount	4,290	7.6	IsINSURED	15,006	26.8
CPN=Premium	38,273	68.2	COLLATERAL	5,510	9.8
CPN=Zero	1,436	2.6	PURPOSE=Refund	23,615	42.1
IsCALL	16,907	30.1	PURPOSE=Pension	114	0.2
IsEXTRACALL	1,322	2.4	PURPOSE=Other	5,563	9.9
IsSINK	2,824	5.0	COMPETITIVE	34,152	60.9
UNRATED	4,669	8.3	UW_OTH	1,760	3.1
IsLIMITED	16,792	29.9	ELECTION	3,527	6.3

Panel B – Continuous Variables

Regressor	1_pct	Median	Mean	99_pct	St.dev.	Skew.	Excess Kurt.
BMK (%)	0.86	3.18	3.02	5.08	1.09	-0.26	-0.88
TTM (log)	-0.63	2.05	1.89	3.20	0.86	-1.07	1.45
SIZE (log of $)	10.13	13.51	13.49	16.58	1.40	-0.15	0.18
INCOME ($1k)	28.80	50.75	54.96	106.69	16.87	1.07	0.96
POP (log of 1k)	1.30	5.10	5.05	7.82	1.46	-0.43	0.14
POPgwth5y	0.88	1.08	1.12	1.77	0.17	2.21	7.00
DEBT ($ per capita)	0.00	0.78	1.25	5.88	1.48	5.00	55.67
TAXADV (%)	0.00	5.80	4.88	10.44	3.15	-0.41	-1.00
CORRUPT (rank)	3.00	22.00	25.73	50.00	14.02	0.21	-1.34
LEADING	-3.48	1.41	1.22	3.70	1.32	-1.62	5.75
IRVOL	3.94	5.91	6.46	13.02	2.06	1.32	1.63
SENTIMENT	-9.53	-3.03	-2.64	2.24	2.68	-0.01	-0.49
UNCERTAINTY	2.99	4.83	4.81	8.13	1.13	0.52	0.08
%Area	0.00	7.97	10.00	57.47	11.66	3.19	18.14
%Pop	0.00	3.72	5.30	32.21	9.30	11.79	225.97
HistoFlood	0.00	0.00	0.15	2.00	0.46	3.78	16.84
DmgFlood ($1M)	0.00	1.37	17.13	255.63	63.62	10.45	167.87
ISSUE YIELD (log)	-1.20	1.11	0.89	1.69	0.65	-1.54	2.79

Panel C – Characteristics of issues marketed in the year following a flood episode

Characteristics	Control group	IsFlood
Number of tranches	55,286	810
Number of issues	4,069	65
Number of counties	1,043	45
Median Maturity (in yrs)	7.97	7.94
Median Issue Size ($1k)	16,020	7,375
% Prime (AAA)	22.79	21.98
% High Quality (AA)	55.95	37.53
% No Rating	8.02	29.01

TABLE 3.3 Issue yield response to flood risk and flood events

Table 3.3 reports our main results regarding the impact of flood risk and of recent major flood events on the issue yield of newly sold municipal bonds. The explanatory variables are described in Table 3.1. The response variable corresponds to the natural logarithm of the issue yields of individual bond tranches. The model includes year fixed-effects and state- and issuer-level random-effects.

	Risk 1	Risk 2	Risk 3	Risk 4	Episodes
Intercept	-0.7264 ***	-0.7239 ***	-0.7279 ***	-0.7270 ***	-0.7184 ***
BMK	0.3500 ***	0.3500 ***	0.3500 ***	0.3499 ***	0.3499 ***
CPN=Discount	0.0439 ***	0.0439 ***	0.0439 ***	0.0439 ***	0.0440 ***
CPN=Premium	0.0225	0.0225	0.0225	0.0225	0.0227
CPN=Zero	0.0401	0.0401	0.0401	0.0402	0.0412 *
TTM	0.3203 ***	0.3203 ***	0.3203 ***	0.3204 ***	0.3205 ***
SIZE	-0.0194 ***	-0.0194 ***	-0.0194 ***	-0.0194 ***	-0.0194 ***
IsCALL	-0.0079	-0.0079	-0.0079	-0.0080	-0.0081
IsEXTRACALL	0.2440 ***	0.2439 ***	0.2440 ***	0.2439 ***	0.2439 ***
IsSINK	-0.1005 ***	-0.1006 ***	-0.1005 ***	-0.1005 ***	-0.0996 ***
UNRATED	-0.2607 ***	-0.2608 ***	-0.2607 ***	-0.2605 ***	-0.2625 ***
IsLIMITED	0.0652 ***	0.0652 ***	0.0654 ***	0.0649 ***	0.0656 ***
IsINSURED	-0.0628 ***	-0.0629 ***	-0.0628 ***	-0.0629 ***	-0.0640 ***
COLLATERAL	-0.2356 ***	-0.2356 ***	-0.2356 ***	-0.2354 ***	-0.2354 ***
PURPOSE=Refund	-0.0184 ***	-0.0183 ***	-0.0184 ***	-0.0184 ***	-0.0182 ***
PURPOSE=Pension	0.2790 ***	0.2790 ***	0.2789 ***	0.2790 ***	0.2801 ***
PURPOSE=Other	0.0254 *	0.0254 *	0.0255 *	0.0256 *	0.0258 *
COMPETITIVE	-0.0699 ***	-0.0699 ***	-0.0700 ***	-0.0710 ***	-0.0700 ***
UW_OTH	-0.0078	-0.0077	-0.0076	-0.0077	-0.0069
INCOME	-0.0014	-0.0014	-0.0014	-0.0014	-0.0014
POP	-0.0023	-0.0024	-0.0022	-0.0023	-0.0016
POPgwth5y	0.0562	0.0563	0.0566	0.0563	0.0526
DEBT	0.0130 *	0.0130 *	0.0130 *	0.0129 *	0.0120
TAXADV	-0.0002	-0.0003	-0.0002	-0.0003	-0.0005
CORRUPT	-0.0005	-0.0005	-0.0005	-0.0005	-0.0005
ELECTION	0.0002	0.0002	0.0002	0.0003	0.0007
LEADING	0.0123 ***	0.0123 ***	0.0123 ***	0.0124 ***	0.0120 ***
IRVOL	-0.0009	-0.0009	-0.0010	-0.0009	-0.0014
SENTIMENT	0.0032	0.0032	0.0032	0.0032	0.0031
UNCERTAINTY	0.0244 ***	0.0244 ***	0.0244 ***	0.0244 ***	0.0243 ***
%Area	-0.0000				0.0002
%Pop		-0.0004			-0.0006
HistoFlood			0.0023		0.0007
DmgFlood				-0.0001	-0.0001 *
IsFlood					0.0686 **
Year Fixed-Effect	YES	YES	YES	YES	YES
State Random Effects	YES	YES	YES	YES	YES
Issuer Random Effects	YES	YES	YES	YES	YES
Number of tranches	56,096	56,096	56,096	56,096	56,096
Number of issues	4,134	4,134	4,134	4,134	4,134
Pseudo R-Squares :					
Between states	0.7092	0.7104	0.7092	0.7071	0.7075
Between issuers	0.8094	0.8095	0.8096	0.8094	0.8098
Within issuers	0.8316	0.8317	0.8316	0.8317	0.8318

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.4 Robustness of the price discount findings

Table 3.4 reports the results from robustness tests of the findings reported in Table 3.3. The first column of Panel A examines the performance of the main model to predict the yield of new bonds issued by disaster-counties in periods that do not follow a flood episode. The other columns of Panel A test the main model on subsamples limited to small issues (principal amount ≤ $15 million) or small issuers (population < 164,200 residents), respectively. Panel B employs alternative modelling strategies that include a WLS model with no random effect, a WLS model with errors clustered at the issue level and a correlated random-effects model with errors clustered at the issue level.

Panel A – Alternative definitions of the sample

	Other issues of flooded counties	**Floods from small issues**	**Floods from small issuers**
IsFlood	0.0737 ***	0.0881 **	0.0585 *
OTHER ISSUES	0.0054		
Control variables	YES	YES	YES
Flood Risk variables	YES	YES	YES
Year Fixed-Effects	YES	YES	YES
State Random-Effects	YES	YES	YES
Issuer Random-Effects	YES	YES	YES
Nb tranches	56,096	27,328	28,031
Nb post-flood issues	65	51	44
Pseudo R-Squares :			
Between states	0.7081	0.7367	0.5856
Between issuers	0.8095	0.8441	0.8115
Within issuers	0.8318	0.8284	0.8329

Panel B – Alternative models

	Traditional WLS	**WLS with errors clustered at the issue level**	**Correlated random effects with errors clustered at the issue level**
IsFlood	0.0725 ***	0.0611 ***	0.0684 **
Control variables	YES	YES	YES
Flood Risk variables	YES	YES	YES
Year Fixed-Effects	YES	YES	YES
State Random-Effects	NO	NO	Diagonal
Issuer Random-Effects	NO	NO	Correlated
Clustered residuals	NO	Issue-level	Issue-level
Number of tranches	56,096	56,096	56,096
Number of issues	4,134	4,134	4,134
Pseudo R-Squares :			
Between states	n.a	n.a	0.7075
Between issuers	n.a	n.a	0.8098
Within issuers	n.a	n.a	0.8319
Adjusted R^2	0.8759	0.8197	n.a

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.5 Definition of the additional control variables

Table 3.5 describes explanatory variables that are added to the ones that are included in the base model and are described in Table 3.1.

VARIABLE	DESCRIPTION	SOURCE
SMB	Average yield difference between large (principal amount > \$5M) and small (principal amount < \$1M) bond tranches traded on the secondary market the week before a new issue.	MSRB
TERM	Average yield difference between long term (time-to-maturity > 10 years) and short-term (time-to-maturity < 3 years) tranches traded on the secondary market the week before a new issue.	MSRB
DEF	Average yield difference between lowly rated (average rating of A+ or worse) and prime (AAA) tranches traded on the secondary market the week before a new issue.	MSRB
FINDENSITY	Proportion of the labor force that works in the Finance and Insurance industry at the county-level.	County Business Pattern
INEQUALITY	Income inequality calculated from the 10^{th} and 90^{th} percentiles of the county-level household income statistics. The variable is expressed as a coefficient of variation that is scaled such that the average county in the U.S. obtains a score of zero.	U.S. Census
STATE_ISSAMT	Total outstanding value of all new municipal bonds (in \$billion) sold by in-state issuers in the month preceding a new issue.	MSRB
COUNTY_ISSAMT	Total outstanding value of all new municipal bonds (in \$billion) sold by issuers in a county in the month preceding a new issue.	MSRB
GFC	Dummy variable for the global financial crisis period that equals one if a new issue is sold on the market between September 2008 and June 2009 and zero otherwise.	n.a.
PANIC	Dummy variable that equals one for new issues sold in January or February 2010 following the *"billions of dollars' worth of defaults"* prediction of Meredith Withney.	n.a.
LCR	Dummy variable that equals one for new issues sold following the adoption of the liquidity coverage requirement in September 2014 that excludes municipal bond holdings from the list of high-quality assets.[52]	
MONTHLYRISK	Damage expected from natural disasters in the month of issuance calculated as the natural logarithm of all disaster-related damage experienced between 1990 and 2005 at the state-level in a given month of the year.	NCEI
MONTHofYEAR	Month-of-the year fixed effects	n.a.

[52] The Federal Reserve somewhat relaxed the rule in July 2016 by allowing certain investment grade munis to be considered as high-quality liquid assets.

TABLE 3.6 Determinants of issuance costs

Table 3.6 reports the results from the analysis of the determinants of issuance costs. The gross spread of new municipal bond issues, expressed in percentage of the total issued amount, is used as the response variable. Model 2 is similar to model 1 except that it includes the additional variable IsNeighbour that identifies counties located nearby (≤100 miles) disaster areas.

	Model 1	**Model 2**
Intercept	1.2535 ***	1.2526 ***
Avg TTM	0.1560 ***	0.1550 ***
Issue Size	-0.0011 ***	-0.0011 ***
NbTranches	0.0011	0.0013
AAA or AA vs. Unrated	-0.1136 *	-0.1112 *
A or BBB vs. Unrated	-0.0927	-0.0902
IsLIMITED	0.0264	0.0257
IsINSURED	0.0083	0.0072
COLLATERAL	-0.0659 **	-0.0654 ***
PURPOSE=Refund	-0.0311	-0.0297
PURPOSE=Pension	-0.0113	-0.0003
PURPOSE=Other	-0.0410 *	-0.0410 *
COMPETITIVE	0.0561	0.0569
UW_OTH	-0.0155	-0.0198
INCOME	-0.0037 ***	-0.0036 ***
POP	-0.0646 ***	-0.0647 ***
POPgwth5y	-0.0924	-0.0949
DEBT	-0.0138	-0.0138
TAXADV	-0.0038	-0.0042
CORRUPT	0.0011	0.0011
ELECTION	-0.0270	-0.0267
LEADING	-0.0218 *	-0.0213 *
IRVOL	0.0081	0.0090
SENTIMENT	0.0024	0.0025
UNCERTAINTY	-0.0128	-0.0116
IsFlood	-0.0877 **	-0.1044 **
IsNeighbour		-0.0596 **
Flood risk variables	YES	YES
Year Fixed-Effect	YES	YES
State Random Effects	YES	YES
Issuer Random Effects	YES	YES
Nb issues	2,154	2,154
Nb post-flood issues	31	31
Nb ‘*neighbour*’ issues	n.a.	378
Pseudo R-Squares :		
Between states	0.5575	0.5385
Between issuers	0.3319	0.3224
Within issuers	0.0837	0.0903

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.7 Probability to issue municipal bonds

Table 3.7 reports the results from a logistic model that aims at explaining the probability that a county issues new municipal bonds. The first four columns differ with regards to the time-frequency used in the analysis. The last column includes additional explanatory variables to explain the probability to issue municipal bonds by calendar year.

	Monthly	**Quarterly**	**Semi-Annual**	**Yearly**	**Yearly**
Intercept	-5.0955 ***	-3.7731 ***	-2.7687 ***	-0.7598	-5.2340 ***
AVGYLD	-0.0897	-0.1989 **	-0.3426 **	-1.4101 ***	0.9063 **
SMB	0.9251 **	1.9754 ***	2.6705 ***	11.5082 ***	-6.3280 *
TERM	-0.5408 ***	-0.8004 ***	-1.0896 ***	-2.8700 ***	0.7583
DEF	0.1623	0.1342	0.1185	-0.7736 *	1.5786 ***
STATE_ISSAMT	0.1386 ***	0.0518 ***	0.0229 ***	0.0105 ***	0.0126 ***
COUNTY_ISSAMT	0.2328 *	0.2467 ***	0.1828 ***	0.0105 ***	0.0947 *
INCOME	0.0102 ***	0.0120 ***	0.0150 ***	0.1789 ***	0.0203 ***
POP	0.2961 ***	0.2979 ***	0.3146 ***	0.3335 ***	0.3730 ***
POPgwth5y	0.0106	-0.1291	-0.3732	-0.9356 **	-0.7152
DEBT	0.1608 ***	0.1820 ***	0.2012 ***	0.2817 ***	0.3206 ***
TAXADV	0.0233 ***	0.0263 ***	0.0318 ***	0.0400 ***	0.0418 ***
ELECTION	-0.0422	0.0652	0.0749	-0.1090 *	-0.1736 **
LEADING	0.0334 **	0.0301 *	0.0277	-0.0071	0.0181
IRVOL	0.0291 **	-0.0148	0.0218	-0.1140 **	-0.0424
SENTIMENT	-0.0310 ***	-0.0488 ***	-0.0597 ***	-0.1923 ***	0.0364
UNCERTAINTY	-0.0804 ***	-0.0257	-0.0301	0.5273 **	-0.2576 *
GFC	0.0616	0.1425	0.0451	-0.1434	-0.8299
PANIC	0.1539	0.1098	0.2211 **	0.8805 ***	n.a.
HSGUNITS					0.8644 **
SURPLUS					-0.6600
INTCOVERAGE					0.0771
RESERVE					-0.7110 ***
%Area	-0.0009	-0.0005	-0.0004	-0.0012	-0.0017
%Pop	0.0038 **	0.0041 **	0.0048 **	0.0058 **	0.0051
HistoFlood	0.0377	0.0565	0.0844 *	0.1082 *	0.1334 *
DmgFlood	-0.0002	-0.0003	-0.0003	-0.0003	0.0000
Neighbours	-0.1568	-0.0680	0.0013	-0.0463	-0.1163
IsFlood	0.1265	-0.0151	-0.1057	-0.2104	-0.1498
Nb observations	132,300	44,100	22,050	11,550	7,198
Nb Issues (Y=1)	4,045	3,904	3,717	3,345	2,353
Nb Neighbours	1,951	673	286	164	120
Nb IsFlood	2,050	773	451	289	195
Nb post-flood issues	61	57	51	59	47
Generalized R-Square (Nagelkerke, 1991)	0.06	0.071	0.101	0.140	0.162
Somers' D	0.330	0.343	0.362	0.394	0.418

*Notes: ***, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.8 Changes in local economic conditions following floods

Table 3.8 reports the results from the analysis of the impact of major flood events on the variation of several local economic and financial indicators. FLOODYR is a set of dummy variables that takes a value of one if a major flooding occurs in the current fiscal year (FLOODYR = 0), in the previous fiscal year (FLOODYR = +1), or two previous fiscal years (FLOODYR = +2).

	Δ Per capita total debt	**Δ Housing units**	**Δ Property tax revenues**	**Δ General surplus**	**Δ Reserve funds**
Intercept	0.7666	-0.0261	0.1624	3.2932	0.2151
ELECTION	-0.0871	-0.0002	0.0157 **	1.2047	-0.1105 **
IRVOL	-0.0625	0.0025	-0.0056	0.1067	0.0027
LEADING	0.1062	0.0021 ***	-0.0060 ***	-1.3753	-0.0034
POP	-0.1281 *	0.0018 ***	-0.0145 ***	-0.3041	-0.0207
TAXADV	0.2176	-0.0002	0.0195	1.4541	0.0158
SENTIMENT	-0.0221	-0.0004	0.0048	0.0765	0.0088
UNCERTAINTY	0.0995	-0.0019	0.0172	0.4805	0.0274
FLOODYR = 0	0.2590	-0.0022	-0.0283	-9.1517	-0.0198
FLOODYR = +1	0.3993	-0.0062 ***	-0.0144	-0.4860	-0.0864
FLOODYR = +2	5.4510 ***	-0.0021	0.0121	-60.7234 ***	-0.0547
Year dummies	YES	YES	YES	YES	YES
Random effects	NO	NO	NO	NO	NO
Nb observations	10,048	9,450	6,551	5,811	5,757
Nb FLOODYR = 0	152	152	101	90	90
Nb FLOODYR = +1	143	145	92	93	93
Nb FLOODYR = +2	136	135	92	83	83
Rsquare	0.0056	0.0649	0.0024	0.0054	0.2151

*Notes: ***, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.9 The effect of major disaster declarations

Table 3.9 displays the effect of local weather events that have been declared 'major disasters' by the U.S. President under the dispositions of the Stafford Act on the issue yield of municipal bonds sold by counties. The model includes all of the control variables listed in Table 3.1.

	All declared disasters	**Declared hurricanes**	**Declared non-hurricane**
IsFlood	0.0484	0.0621 *	0.0852 *
DECLAREDall	-0.0064		
HURRICANE		0.0343 ***	
NON-HURRICANE			-0.0160 *
IsFlood * DECLAREDall	0.0288		
IsFlood * HURRICANE		0.0320	
IsFlood * NON-HURRICANE			-0.0015
Control variables	YES	YES	YES
Flood Risk variables	YES	YES	YES
Year Fixed-Effects	YES	YES	YES
State Random-Effects	YES	YES	YES
Issuer Random-Effects	YES	YES	YES
Nb post-declaration issues	977	187	790
Nb flood events not declared	14	57	25
Pseudo R-Squares :			
Between states	0.7081	0.7094	0.7072
Between issuers	0.8107	0.8098	0.8101
Within issuers	0.8321	0.8322	0.8321

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.10 Flood events and roundtrip transaction costs

Table 3.10 reports the impact of flood risk and of major flood events on roundtrip transaction cost observed on municipal bond secondary markets. Trade data are from the MSRB database and include all seasoned transactions of G.O. munis sold by county authorities.

	Trade costs in percentage	**Trade costs in basis points**	**Trade costs in basis points**
Intercept	0.4859 ***	0.4851 ***	0.4827 ***
TRADEDAMT	-0.1993 ***	-0.2082 ***	-0.1770 ***
NBDEALERS	0.1101 ***	0.1161 ***	0.1162 ***
BMK	0.1014 ***	0.0732 ***	0.0733 ***
CPN=Discount	0.4518 ***	0.2903 ***	0.2905 ***
CPN=Premium	-0.1694 ***	-0.1308 ***	-0.1303 ***
CPN=Zero	0.0493	0.0134	0.0131
TTM	0.5947 ***	0.6066 ***	0.6067 ***
SIZE	-0.0682 ***	-0.0650 ***	-0.0648 ***
IsCALL	0.3083 ***	0.3302 ***	0.3301 ***
IsEXTRACALL	0.1181	0.1802	0.1808
IsSINK	0.1397 ***	0.1205 ***	0.1207 ***
UNRATED	-0.1628 ***	-0.1222 ***	-0.1218 ***
IsLIMITED	-0.0431 **	-0.0454 **	-0.0462 **
IsINSURED	0.0637 ***	0.0567 ***	0.0568 ***
COLLATERAL	-0.0944 ***	-0.0995 ***	-0.0998 ***
PURPOSE=Refund	-0.0039	-0.0025	-0.0027
PURPOSE=Pension	0.1916	0.2527	0.2524
PURPOSE=Other	0.0090	0.0017	0.0011
INCOME	-0.0018 **	-0.0017 **	-0.0017 **
POP	-0.0073	-0.0090	-0.0091
POPgwth5y	0.3356 *	0.3419 *	0.3367
DEBT	-0.9367	-0.3099	-0.2850
TAXADV	-0.0146	-0.0136	-0.0136
CORRUPT	-0.0013	-0.0012	-0.0012
ELECTION	-0.0135	-0.0139	-0.0139
LEADING	0.0015	0.0001	0.0002
IRVOL	0.0119 ***	0.0101 ***	0.0101 ***
SENTIMENT	-0.0077 ***	-0.0039	-0.0040
UNCERTAINTY	-0.0089	-0.0124 *	-0.0123 *
%Area	0.0002	0.0001	0.0002
%Pop	-0.0018 ***	-0.0016 ***	-0.0006
%Pop x TRADEDAMT			-0.0090 **
HistoFlood	0.0110	0.0086	0.0084
DmgFlood	0.0001	0.0001	0.0001
IsFlood	-0.0146	-0.0145	-0.0149
Year Fixed-Effect	YES	YES	YES
State Random Effects	YES	YES	YES
Issuer Random Effects	YES	YES	YES
Number of transactions	334,208	334,208	334,208
Pseudo R-Squares :			
Between states	0.6281	0.5974	0.6177
Between issuers	0.7915	0.8123	0.8276
Within issuers	0.2905	0.2964	0.3259

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.11 The effect of flood events over time

Table 3.11 examines the effect of flood events on issue yields of municipal bonds under alternative starting and ending times for the event period. The last column of Table 3.11 distinguishes first-time disaster counties (VirginRisk) from counties that suffer from at least one extra major flood in the previous 15 years (ExperiencedRisk). The control variables included in the model are listed in table 3.1.

	Munis sold 0-12 months after a major flood	**Munis sold 13-24 months after a major flood**	**Munis sold 25-36 months after a major flood**	**Munis sold 37-48 months after a major flood**	**Munis sold 0-60 months after a major flood**
IsFlood	0.0686 **	-0.0380	0.0166	-0.0091	
VirginRisk					0.0478 *
Time-since-last-flood					-0.0178 *
ExperiencedRisk					-0.0120
Time-since-last-flood					0.0048
Control variables	YES	YES	YES	YES	YES
Flood Risk variables	YES	YES	YES	YES	YES
Year Fixed-Effects	YES	YES	YES	YES	YES
State Random-Effects	YES	YES	YES	YES	YES
Issuer Random-Effects	YES	YES	YES	YES	YES
Nb post-flood tranches	810	659	649	812	3,190
Nb post-flood issues	65	61	59	62	274
Nb post-flood *virgin* issues	n.a.	n.a.	n.a.	n.a.	185

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 3.12 Robustness to various flood damage thresholds

Table 3.12 replicates the results of Table 3.3 with alternative per capita flood-related damage thresholds ranging from \$25 to \$125. The model includes the control variables described in Table 3.1.

	Flood damage thresholds (per capita constant 2015 $US)				**Flood damage categories**
	25 $	**50$**	**75$**	**125$**	
IsFlood	-0.0055	0.0101	0.0067	0.0632 *	
DMG : $1-25					-0.0239
DMG : $25-100					0.0053
DMG : $100-500					0.0497 **
DMG : $500+					0.0722
Control variables	YES	YES	YES	YES	YES
Flood Risk variables	YES	YES	YES	YES	YES
Year Fixed-Effects	YES	YES	YES	YES	YES
State Random-Effects	YES	YES	YES	YES	YES
Issuer Random-Effects	YES	YES	YES	YES	YES
Nb post-flood tranches	1,754	1,279	977	699	7,972
Nb post-flood issues	144	105	80	56	567

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE 4.1 Summary statistics of monthly per capita disaster-driven losses

Table 4.1 presents descriptive statistics on the monthly disaster-driven damage of selected states. Values for losses come from the aggregation by month and by state of the damage estimates arising from all individual weather events reported in the NCEI's storm-event database and are expressed in dollars on a per capita basis. The sample covers the February 2005 to December 2016 period. The *All states* series is built by summing monthly damage over the 47 states retained in the study. Excluded regions are Alaska, Hawaii, Wyoming and the District of Columbia.

Selected states	1_pct	Median	Mean	99_pct	St.dev.	Skew.	Excess Kurt.
Alabama	0.00	0.12	7.08	223.31	62.33	12.12	159.42
California	0.00	0.02	0.86	16.85	4.21	9.16	98.64
Florida	0.00	0.06	2.81	35.09	20.18	10.64	118.21
Illinois	0.00	0.03	1.33	23.07	6.02	9.82	119.20
Kansas	0.00	0.33	4.73	63.67	16.55	8.49	92.39
Louisiana	0.00	0.12	19.80	300.70	171.05	11.29	132.93
Michigan	0.00	0.02	1.17	14.74	10.29	16.94	296.98
Nebraska	0.00	0.18	8.31	126.09	26.09	6.37	54.63
New-York	0.00	0.03	0.67	10.24	4.24	11.38	135.99
Ohio	0.00	0.10	1.46	28.28	5.78	7.49	68.98
Pennsylvania	0.00	0.04	0.71	18.90	3.39	8.67	87.47
South Dakota	0.00	0.01	2.37	33.26	6.77	4.22	20.60
Texas	0.00	0.47	5.96	71.81	25.42	10.59	129.68
Virginia	0.00	0.03	0.77	11.25	2.81	7.82	83.27
Wisconsin	0.00	0.02	1.89	30.41	9.15	10.01	114.76
All states	0.00	0.02	3.64	45.98	67.24	58.38	4,054.73

TABLE 4.2 Summary statistics of monthly consumption growth

Table 4.2 presents descriptive statistics on the monthly gross consumption growth series of selected states. State-level consumption is inferred from a regression model and corresponds to the portion of the state-level monthly electricity consumption that is orthogonal to the temperature-driven demand for energy and to intra-year seasonal patterns. Details regarding the construction of the state-consumption series can be found in Appendix C. Electricity consumption data are from the Electric Power Industry Report of the U.S. Energy Information Administration and monthly temperature averages are from the NCEI's global historical climatology network. The sample covers the February 2005 to December 2016 period. The *All states* series is built by pooling monthly consumption growth over the 47 states retained in the study. Excluded regions are Alaska, Hawaii, Wyoming and the District of Columbia.

Selected states	1_pct	Median	Mean	99_pct	St.dev.	Skew.	Excess Kurt.
Alabama	0.9139	1.0044	1.0086	1.1382	0.0475	0.5901	0.4613
California	0.8774	0.9985	1.0030	1.1124	0.0542	-0.0229	-0.1811
Florida	0.9237	1.0009	1.0010	1.0896	0.0326	0.2043	0.6754
Illinois	0.9365	1.0033	1.0047	1.0948	0.0359	0.3654	-0.1731
Kansas	0.9033	1.0022	1.0039	1.0956	0.0406	-0.0266	0.2444
Louisiana	0.8977	1.0025	1.0019	1.1123	0.0450	0.1630	-0.0009
Michigan	0.9228	0.9997	1.0051	1.1128	0.0397	0.6005	0.8996
Nebraska	0.8941	1.0070	1.0077	1.1668	0.0543	0.3628	0.0351
New-York	0.9466	1.0045	1.0049	1.0919	0.0311	0.2778	0.4792
Ohio	0.9017	1.0019	1.0075	1.1273	0.0419	0.4382	0.7140
Pennsylvania	0.9241	1.0052	1.0086	1.1448	0.0449	0.8098	0.9362
South Dakota	0.9316	1.0083	1.0067	1.1428	0.0455	0.4920	0.2779
Texas	0.9172	1.0007	1.0028	1.1083	0.0408	0.3691	-0.0003
Virginia	0.9231	1.0027	1.0072	1.1430	0.0433	0.6168	0.7444
Wisconsin	0.9294	1.0014	1.0042	1.0697	0.0312	-0.0365	0.1157
All states	0.8998	1.0031	1.0059	1.1442	0.0485	0.4483	1.6853

TABLE 4.3 Summary statistics of the municipal bond return series

Table 4.3 presents descriptive statistics on the monthly municipal bond index gross returns of selected states. State-level municipal bond indexes are constructed following a standard arithmetic repeated sales methodology and using MSRB's municipal bond transaction data. Details regarding the construction of the state-consumption series can be found in Appendix D. The sample covers the February 2005 to December 2016 period. The *All states* series is built by pooling monthly gross returns over the 47 states retained in the study. Excluded regions are Alaska, Hawaii, Wyoming and the District of Columbia.

Selected states	1_pct	Median	Mean	99_pct	St.dev.	Skew.	Excess Kurt.
Alabama	0.9812	1.0035	1.0033	1.0222	0.0075	-1.063	9.682
California	0.9768	1.0047	1.0036	1.0329	0.0087	-0.836	8.699
Florida	0.9835	1.0040	1.0033	1.0224	0.0080	-0.465	10.746
Illinois	0.9829	1.0040	1.0033	1.0215	0.0072	-0.206	5.463
Kansas	0.9805	1.0039	1.0032	1.0154	0.0067	-0.570	5.244
Louisiana	0.9831	1.0035	1.0034	1.0165	0.0073	-0.480	7.765
Michigan	0.9834	1.0035	1.0033	1.0214	0.0069	-0.450	6.448
Nebraska	0.9821	1.0035	1.0032	1.0202	0.0082	0.384	8.100
New-York	0.9839	1.0042	1.0032	1.0158	0.0074	0.315	10.973
Ohio	0.9803	1.0036	1.0033	1.0233	0.0081	0.254	4.731
Pennsylvania	0.9840	1.0037	1.0032	1.0165	0.0075	-0.010	11.342
South Dakota	0.9775	1.0038	1.0034	1.0213	0.0088	-0.382	0.944
Texas	0.9819	1.0040	1.0033	1.0194	0.0075	0.283	7.735
Virginia	0.9872	1.0038	1.0033	1.0207	0.0075	0.217	11.456
Wisconsin	0.9842	1.0040	1.0032	1.0184	0.0061	-0.039	6.257
All states	0.9779	1.0037	1.0033	1.0256	0.0082	-0.014	7.311

Notes: The return calculated from the municipal bond repeat sales index (RSI) in month t in state j corresponds to $ln(RSI_t^j) - ln(RSI_{t-1}^j) + 1$.

TABLE 4.4 Distribution of the pricing errors

Table 4.4 presents summary statistics on the pricing error series resulting from the GMM estimation of the CCAPM model at the state-level. We obtain 4 maturity-based series each containing 143 monthly pricing errors per state. These errors are averaged over maturity groups or states or maturity groups and states to produce the statistics embedded in the table. The sample covers the February 2005 to December 2016 period.

Selected series	1_pct	Median	Mean	99_pct	St.dev.	Skew.	Excess Kurt.
Average errors by state:							
Alabama	-0.591	-0.027	0.001	0.859	0.304	0.462	0.223
California	-0.190	0.002	0.002	0.295	0.102	0.401	0.259
Florida	-0.154	0.001	0.002	0.166	0.064	0.137	0.488
Illinois	-0.421	-0.010	0.002	0.495	0.212	0.255	-0.244
Kansas	-0.296	-0.012	0.000	0.487	0.158	0.775	1.567
Louisiana	-0.055	0.002	0.001	0.057	0.025	-0.087	-0.010
Michigan	-0.463	0.004	0.000	0.589	0.214	0.236	0.191
Nebraska	-0.499	-0.026	0.000	0.626	0.236	0.373	-0.212
New-York	-0.567	-0.036	0.002	0.682	0.293	0.828	1.384
Ohio	-0.611	-0.020	0.001	1.290	0.330	1.128	2.999
Pennsylvania	-0.693	-0.045	0.002	0.995	0.352	0.447	0.259
South Dakota	-0.541	-0.050	-0.000	0.529	0.250	0.237	-0.667
Texas	-0.263	0.000	0.002	0.290	0.118	0.084	-0.001
Virginia	-0.611	-0.015	0.002	0.745	0.282	0.415	0.100
Wisconsin	-0.415	-0.008	0.001	0.812	0.258	1.101	2.729
Average errors by maturity group:							
0 to 2.5 years	-0.535	0.001	0.003	0.693	0.215	0.957	5.650
2.5 to 5 years	-0.537	-0.003	0.000	0.691	0.214	0.964	5.647
5 to 7.5 years	-0.538	-0.003	0.000	0.682	0.214	0.955	5.637
More than 7.5 years	-0.535	-0.003	0.000	0.682	0.214	0.955	5.593
Average errors across all series	-0.536	-0.002	0.001	0.688	0.215	0.958	5.628

TABLE 4.5 Disaster-driven risk aversion

Table 4.5 displays the results of the event study based on the pricing errors arising from the GMM estimation of the CCAPM model at the state-level. The regression model is defined as:

$$u_{njt} = \delta_1 \mathbf{1}_{cg} DIS_{j\{t-k,\ t-1\}} + D\_MAT_n + D\_STATE_j + D\_TIME_t + \xi_{njt}$$

$$\mathbf{1}_{cg} = \begin{cases} +1 & \text{when ELECT} \geq 1 \\ -1 & \text{when ELECT} < 1 \end{cases}$$

where u_{njt} is the pricing error (expressed in thousands) for the bond return series of maturity class n in state j for month t. DIS is the sum of disaster-driven damage per capita that occurred between months $t - k$ and $t - 1$ in state j. 1_{cg} is an indicator variable that takes the value of plus (minus) one when the gross consumption growth (ELECT) is greater (smaller) than one. The indicator variable ensures that the relation between u_{njt} and $1_{cg}DIS_{j\{t-k,\ t-1\}}$ shall always be positive (negative) if disasters are to increase (decrease) risk aversion. D_MAT, D_STATE and D_TIME are maturity classes, states and months fixed effects. δ_1 is the main coefficient of interest that convey the effect of disasters-related losses on risk aversion. The regression model is estimated using a robust regression framework (Huber, 1973).

	Disaster Damage in the last month (k=1)	**Disaster Damage in the last 6 months (k=6)**	**Disaster Damage in the last year (k=12)**	**Disaster Damage in the last 10 years (k=120)**
$\mathbf{1}_{cg}$*DIS	0.0447*** (0.0125)	0.0430*** (0.0048)	0.0399*** (0.0034)	0.0211*** (0.0008)
Fixed effects:				
Maturity classes	Y	Y	Y	Y
States	Y	Y	Y	Y
Months	Y	Y	Y	Y
Robust R-square	0.1730	0.1740	0.1750	0.1872

Notes: Standard errors are reported in parentheses.

, **, * indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

TABLE 4.6 Disaster-driven increase in risk aversion under alternative models

Table 4.6 investigates the robustness of the results reported in Table 4.5 when the event study regression is estimated under alternative econometric approaches. The event study base model is defined as:

$$u_{njt} = \delta_1 \mathbf{1}_{cg} DIS_{j\{t-k,\ t-1\}} + D\_MAT_n + D\_STATE_j + D\_TIME_t + \xi_{njt}$$

$$\mathbf{1}_{cg} = \begin{cases} +1 & \text{when ELECT} \geq 1 \\ -1 & \text{when ELECT} < 1 \end{cases}$$

where $\mathrm{u_{njt}}$ is the pricing error (expressed in thousands) arising from the GMM estimation of the CCAPM and associated with the bond return series of maturity class n in state j for month t. DIS is the sum of disaster-driven per capita damage that occurred between months $\mathrm{t-k}$ and $\mathrm{t-1}$ in state j. 1_{cg} is an indicator variable that takes the value of plus (minus) one when the gross consumption growth (ELECT) is greater (smaller) than one. The indicator variable ensures that the relation between $\mathrm{u_{njt}}$ and $1_{\mathrm{cg}}\mathrm{DIS_{j\{t-k,\ t-1\}}}$ shall always be positive (negative) if disasters are to increase (decrease) risk aversion. $\mathrm{D\_MAT}$, $\mathrm{D\_STATE}$ and $\mathrm{D\_TIME}$ are maturity classes, states and months fixed effects. δ_1 is the main coefficient of interest that conveys the effect of disasters-related losses on risk aversion.

The alternative modelling strategies include an OLS regression with a constant term but no fixed-effects, an OLS regression with residuals assumed to be spatially correlated according to geographic coordinates of the state's population centroids and an OLS regression with residuals assumed to be clustered by state.

	OLS		**OLS with spatial correlation among residuals**		**OLS with residuals clustered by state**	
	k=1	**k=120**	**k=1**	**k=120**	**k=1**	**k=120**
Constant	0.6039 (1.3084)	1.8534 (1.2728)	*none*	*none*	*none*	*none*
$\mathbf{1}_{cg}$* DIS	0.0623*** (0.0173)	0.0432*** (0.0011)	0.0529*** (0.0152)	0.0242*** (0.0010)	0. 0521** (0.0237)	0.0241* (0.0137)
Fixed-effects:						
Maturity classes	NO	NO	YES	YES	YES	YES
States	NO	NO	YES	YES	NO	NO
Months	NO	NO	YES	YES	YES	YES
Residuals	Standard	Standard	Spatial correlation	Spatial correlation	Clustered by state	Clustered by state
Standard errors	Unadjusted	Unadjusted	Unadjusted	Unadjusted	Sandwich	Sandwich
(adj. or pseudo) R-squared	0.0004	0.0323	0.2634	0.2752	0.2647	0.2799

Notes: Standard errors are reported in parentheses.

, **, * indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

TABLE 4.7 Direct GMM estimation of disaster-driven risk aversion

Table 4.7 reports the results from a GMM estimation of a modified version of the unconditional CCAPM where the stochastic discount factor $\mathrm{m_{j,t}}$ is defined as:

$$\mathrm{m_{j,t}} = cg_{j,t}^{-\left(\gamma_j + \mathbf{1}_{j,k}^{FEMA}\gamma^D\right)}$$

where $cg_{j,t}$ is the growth in consumption in state j between months t and $\mathrm{t}-1$. γ_j is the RRA parameter associated with state j, $1_{\mathrm{j,k}}^{\mathrm{FEMA}}$ is an indicator variable that takes the value of one in the k=6 months following the occurrence of a major declared disaster in state j and zero otherwise and γ^{D} is a new disaster-driven risk aversion parameter.

States	γ_j	**States**	γ_j	**States**	γ_j
AL-Alabama	6.84*** [6.85]	MA-Massachusetts	4.11*** [4.11]	OH-Ohio	7.93*** [7.93]
AR-Arkansas	0.24*** [0.23]	MD-Maryland	8.07*** [8.07]	OK-Oklahoma	0.45*** [0.45]
AZ-Arizona	0.06 [0.06]	ME-Maine	0.44 [0.44]	OR-Oregon	6.06*** [6.06]
CA-California	1.91*** [1.91]	MI-Michigan	5.66*** [5.66]	PA-Pennsylvania	8.70*** [8.70]
CO-Colorado	4.17*** [4.16]	MN-Minnesota	6.43*** [6.43]	RI-Rhode Island	0.12 [0.12]
CT-Connecticut	0.02* [0.02]	MO-Missouri	6.51*** [6.52]	SC-South Carolina	7.48*** [7.48]
DE-Delaware	0.29 [0.29]	MS-Mississippi	0.61*** [0.67]	SD-South Dakota	5.77*** [5.77]
FL-Florida	1.96*** [1.95]	MT-Montana	6.66*** [6.66]	TN-Tennessee	0.06*** [0.07]
GA-Georgia	7.66*** [7.65]	NC-North Carolina	6.43*** [6.42]	TX-Texas	2.97*** [2.98]
IA-Iowa	0.20*** [0.19]	ND-North Dakota	5.69*** [5.70]	UT-Utah	0.54*** [0.54]
ID-Idaho	4.70*** [4.70]	NE-Nebraska	4.49*** [4.49]	VA-Virginia	6.93*** [6.93]
IL-Illinois	6.13*** [6.13]	NH-New Hampshire	0.32*** [0.32]	VT-Vermont	8.47*** [8.47]
IN-Indiana	6.32*** [6.33]	NJ-New Jersey	6.87*** [6.87]	WA-Washington	6.85*** [6.86]
KS-Kansas	3.79*** [3.79]	NM-New Mexico	5.55*** [5.55]	WI-Wisconsin	8.01*** [8.01]
KY-Kentucky	5.71*** [5.73]	NV-Nevada	0.09*** [0.09]	WV-West Virginia	0.13*** [0.13]
LA-Louisiana	0.42 [0.45]	NY-New York	9.43*** [9.45]		
γ^D	0.38** [0.00]				

Notes: Estimates of the relative risk aversion parameters from Figure 4.1 are used as priors and reported in brackets. Standard errors are estimated via the delta method.
, **, * indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively*

TABLE 4.8 Summary statistics of the instruments included in the information set

Table 4.8 describes summary statistics on five variables that are included in the information set of the state-level representative investors in the context of testing the conditional CCAPM. FEDFUNDS is the monthly variation of the effective federal funds rate. TERM is the term spread between the 20-year U.S. treasury bonds and the 4-week U.S. treasury bills constant maturity indices. DEF is the default spread between the 10-year high quality market corporate bond index and the 10-year U.S. treasury bonds index. SCI is the growth of state coincident indexes (Stock & Watson, 1989). TAX is the changes in the annual maximum combined federal and state marginal income tax rates. FEDFUNDS, TERM and DEF are built using data from the Economic Research Division of the Federal Reserve Bank of St. Louis. SCI is from the Federal Reserve Bank of Philadelphia. TAX is from the TAXSIM model (Feenberg & Coutts, 1993). FEDFUNDS, TERM and DEF are national indicator and thus common to investors across states. SCI and TAX are state-specific. AC(x) stands for the x^{th}-order autocorrelation. The sample covers the January 2005 to December 2016 period.

Panel A – Summary statistics (monthly data 2005/01 – 2016/12)

Instruments	**1_pct**	**Median**	**Mean**	**99_pct**	**St.dev.**	**AC(1)**	**AC(6)**	**AC(12)**
FEDFUNDS	-0.84	0.00	-0.01	0.24	0.15	0.69	0.38	0.16
TERM	-0.69	-0.01	0.00	0.76	0.27	0.13	0.15	-0.01
DEF	-0.60	0.00	0.00	0.77	0.21	0.32	0.00	-0.05
Cross-state average:								
SCI	-1.57	0.16	0.12	0.75	0.44	0.81	0.45	0.21
TAX	-0.35	0.00	0.03	0.13	0.46	0.91	0.47	-0.07

Panel B –Correlations (cross-state average)

	Contemporaneous correlations				***Lagged cross-correlations***		
	TERM	DEF	SCI	TAX		$ELECT_{-1}$	RSI_{-1}
FEDFUNDS	-0.29	-0.03	0.16	0.00	FEDFUNDS	0.02	-0.04
TERM	1	-0.25	-0.13	0.08	TERM	0.00	-0.36
DEF		1	0.01	-0.12	DEF	-0.03	-0.03
SCI			1	0.22	SCI	0.04	-0.03
TAX				1	TAX	-0.02	-0.02

TABLE 4.9 The effect of economic conditions on pricing errors

Table 4.9 displays the results of the event study based on the pricing errors arising from the GMM estimation of a conditional version of the CCAPM model at the state-level. The variables FEFDUNDS, TERM, DEF, SCI and TAX described in Table 4.8 are included as instruments. The instrument set also includes a constant. The regression model used in panel A is defined as:

$$u_{njt} = \delta_1 \mathbf{1}_{cg} DIS_{j\{t-k,\ t-1\}} + D\_MAT_n + D\_STATE_j + D\_TIME_t + \xi_{njt}$$

$$\mathbf{1}_{cg} = \begin{cases} +1 & \text{when ELECT} \geq 1 \\ -1 & \text{when ELECT} < 1 \end{cases}$$

where $\mathrm{u_{njt}}$ is the pricing error (expressed in thousands) for the bond return series of maturity class n in state j for month t. DIS is the sum of disaster-driven damage that occurred between months $\mathrm{t}-1-\mathrm{k}$ and $\mathrm{t}-1$ in state j. 1_{cg} is an indicator variable that takes the value of plus (minus) one when the gross consumption growth (ELECT) is greater (smaller) than one. The indicator variable ensures that the relation between $\mathrm{u_{njt}}$ and $1_{\mathrm{cg}}\mathrm{DIS}_{\mathrm{j\{t-k,\ t-1\}}}$ shall always be positive (negative) if disasters are to increase (decrease) risk aversion. $\mathrm{D\_MAT}$, $\mathrm{D\_STATE}$ and $\mathrm{D\_TIME}$ are maturity classes, states and months fixed effects. Panel B differs from panel A in one respect. The variable DIS is replaced by $1_{\mathrm{j,k}}^{\mathrm{FEMA}}$, which is an indicator variable that takes the value of one in the k months following the occurrence of a major declared disaster in state j and zero otherwise. The regression model is estimated using a robust regression framework (Huber, 1973).

	Disaster Damage in the last month (k=1)	**Disaster Damage in the last 6 months (k=6)**	**Disaster Damage in the last year (k=12)**	**Disaster Damage in the last 10 years (k=120)**	
Panel A – Event study with instruments using the logarithm of disaster damage					
$\mathbf{1}_{cg}$* DIS	0.0012 (0.0008)	0.0015*** (0.0003)	0.0012*** (0.0002)	0.0002*** (0.0001)	
Robust R-square	0.0035	0.0036	0.0036	0.0036	
Fixed effects:					
Maturity classes	Y	Y	Y	Y	
States	Y	Y	Y	Y	
Months	Y	Y	Y	Y	
Panel B – Event study with instruments using FEMA's list of major disasters					
	k=1	**k=6**	**k=12**	**k=120**	***k=6* (*Excl. 2008*)**
$\mathbf{1}_{cg}$ * $\mathbf{1}_{j,k}^{FEMA}$	2.495*** (0.832)	1.152*** (0.348)	1.156*** (0.252)	0.722*** (0.121)	0.675*** (0.113)
Robust R-square	0.0036	0.0036	0.0036	0.0036	0.0033
Fixed effects:					
Maturity classes	Y	Y	Y	Y	Y
States	Y	Y	Y	Y	Y
Months	Y	Y	Y	Y	Y

Notes: Standard errors are reported in parentheses.

, **, * indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

TABLE 4.10 Determinants of risk aversion

Table 4.10 reports the results from the analysis of the determinants of risk aversion. In this table, a modified version of the unconditional CCAPM is employed to generate *annual* estimates of RRA at the state-level. Annual regional RRA estimates are then used as the dependant variable and are matched against plausible determinants of risk aversion in the context of an OLS regression model.

Y	MODEL 1 Annual RRA estimates (full sample)	MODEL 2 Annual RRA estimates (between 0 and 10)
Intercept	5.988	-1.567
	(12.903)	(8.794)
POP	0.086*	0.000
	(0.046)	(0.027)
LABOR	-0.199**	-0.110**
	(0.081)	(0.052)
GENDER	2.753	8.145
	(11.682)	(8.022)
AGE	0.020	-0.013
	(0.103)	(0.059)
BACHELOR	0.172**	0.104**
	(0.073)	(0.048)
NONWHITE	-0.001	0.041**
	(0.024)	(0.020)
LANGUAGE	-0.062	0.009
	(0.050)	(0.030)
GUBERNATORIAL	0.219	0.193
	(0.371)	(0.237)
HOUSE	-0.005	-0.004
	(0.004)	(0.003)
INC	-0.025	-0.005
	(0.111)	(0.080)
SDP	0.122	-0.022
	(0.114)	(0.065)
CORRUPT	0.190	0.424
	(0.441)	(0.361)
CPRATIO	0.040	-0.150*
	(0.147)	(0.080)
SOPHISTICATION	0.130	-0.030
	(0.098)	(0.073)
DIS (x 1000)	0.473*	0.401*
	(0.244)	(0.212)
FEMA	0.726	0.280
	(0.627)	(0.501)
Fixed-Effect	Year	Year
R-squared	0.258	0.308
Observations	519	432

Notes: Huber-White standard errors are reported in parentheses.
, **, * indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

TABLE 4.11 Robustness of the results to various grouping schemes for the bond universe

Table 4.11 replicates the results of Table 4.5 using alternative criteria to segment the municipal bond market. The modelling approach is described in Table 4.5. The first test retains the maturity-based segmentation of the municipal market into four portfolios but increases the new-issue period from 28 to 90 days (Green et al., 2007b). The second test retains the 28-day new-issue period but segments the bond universe according to four issuer types (state, county, city and districts). The last test also retains the 28-days new-issue period but segments the municipal market according to two security types (general obligation bonds and revenue bonds). The regression model is estimated using a robust regression framework (Huber, 1973).

	90-days new issue period (k=6)	**Bond universe segmented by:**	
		Issuer type (k=6)	**Security type (k=6)**
$\mathbf{1}_{cg}$* DIS	0.0411*** (0.0080)	0.0397*** (0.0068)	0.0442*** (0.0029)
Fixed effects:			
Maturity classes	Y	Y	Y
States	Y	Y	Y
Months	Y	Y	Y
Excluded states	AK, HI and WY	AK, HI, ID, ND,OK, SD, VT, WV and WY	AK, HI, NV, SD and WY

Note: Standard errors are reported in parentheses.

*, **, *** *indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

TABLE 4.12 Revisiting the effect of economic conditions using annual RRA estimates

Table 4.12 replicates the results of Table 4.9 using pricing errors (expressed in millions) obtained from the conditional CCAPM where the risk aversion parameter is estimated at the annual frequency instead of constant throughout the 2005 to 2016 period. The modelling approach is described in Table 4.9.

.	**Disaster Damage in the last month (k=1)**	**Disaster Damage in the last 6 months (k=6)**	**Disaster Damage in the last year (k=12)**	**Disaster Damage in the last 10 years (k=120)**	
Panel A – Event study with instruments using the logarithm of disaster damage					
$\mathbf{1}_{cg}$* DIS	-0.1968 (0.2098)	0.2192*** (0.0813)	0.1507*** (0.0577)	0.0267* (0.0140)	
Robust R-square	0.0020	0.0020	0.0020	0.0020	
Fixed effects:					
Maturity classes	Y	Y	Y	Y	
States	Y	Y	Y	Y	
Months	Y	Y	Y	Y	
Panel B – Event study with instruments using FEMA's list of major disasters					
	k=1	**k=6**	**k=12**	**k=120**	***k=6*** **(*Excl. 2008*)**
$\mathbf{1}_{cg}$ * $\mathbf{1}_{j,k}^{FEMA}$	817.93*** (229.46)	123.69 (96.00)	139.98** (69.94)	36.20 (33.45)	-11.71 (106.03)
Robust R-square	0.0020	0.0020	0.0020	0.0020	0.0020
Fixed effects:					
Maturity classes	Y	Y	Y	Y	Y
States	Y	Y	Y	Y	Y
Months	Y	Y	Y	Y	Y

Note: Standard errors are reported in parentheses.

, **, * indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

TABLE B.1 The impact of flood episodes on issue yields using the augmented model

Table B.1 reports the impact of flood risk and of recent flood events on the natural logarithm of the issue yields of municipal bonds sold by disaster counties by testing the effect of additional explanatory variables. The variables included in the base model are described in Table 3.1 while the additional variables are presented in Table 3.5. The model also includes year fixed-effects and state- and issuer-level random-effects.

	Coefficients
SMB	0.0581
TERM	0.0797 ***
DEF	-0.0412
FINDENSITY	0.1138
INEQUALITY	0.0031
STATE_ISSAMT	-0.0028
COUNTY_ISSAMT	0.0500 ***
GFC	-0.0300
PANIC	0.0046
LCR	0.1179 ***
MONTHLYRISK	-0.0037
%Area	0.0002
%Pop	-0.0006
HistoFlood	-0.0019
DmgFlood	-0.0001 *
IsFlood	0.0714 **
Main control variables	YES
Month-of-the-year Fixed-Effects	YES
Year Fixed-Effect	YES
State Random Effects	YES
Issuer Random Effects	YES
Number of tranches	56,096
Number of issues	4,134
Pseudo R-Squares :	
Between states	0.6647
Between issuers	0.8100
Within issuers	0.8149

Notes: The Huber Sandwich estimator is used to estimate standard errors.
****, ** and * denote significance at the 1%, 5% and 10%, respectively.*

TABLE E.1 Disaster-driven risk aversion using Epstein-Zin preferences

Table E.1 reports the results from a GMM estimation of an unconditional CCAPM where investors have recursive preferences. The stochastic discount factor $\mathrm{m}_{\mathrm{j,t}}$ is defined as in Epstein and Zin (1991):

$$\mathrm{m}_{\mathrm{j,t}} = cg_{j,t}^{\frac{-\rho_j(1-\alpha_j)}{(1-\rho_j)}} \frac{1}{R_t^w}^{(\alpha_j-\rho_j)(1-\rho_j)}$$

where $cg_{j,t}$ is the growth in consumption in state j at time t and R_t^w is the gross return of the wealth portfolio. $1/\rho_j$ is the EIS parameter, α_j is the RRA parameter.

States	α_j	$1/\rho_j$	States	α_j	$1/\rho_j$	States	α_j	$1/\rho_j$
AL	2.74***	0.23***	MA	2.19***	-1.71***	OH	0.98***	0.33***
	(0.11)	(0.00)		(0.11)	(0.04)		(0.09)	(0.02)
AR	1.42***	0.30***	MD	1.86***	0.27***	OK	2.44***	0.26***
	(0.15)	(0.01)		(0.10)	(0.02)		(0.17)	(0.01)
AZ	1.55***	0.30***	ME	1.08***	0.33**	OR	5.03***	0.17***
	(0.18)	(0.01)		(0.29)	(0.17)		(0.10)	(0.00)
CA	1.39***	0.30***	MI	6.43***	0.15***	PA	6.04***	0.14***
	(0.20)	(0.01)		(0.14)	(0.00)		(0.07)	(0.00)
CO	2.90***	0.23***	MN	5.02***	0.17***	RI	1.04***	0.33***
	(0.14)	(0.01)		(0.12)	(0.00)		(0.24)	(0.12)
CT	1.43***	0.30***	MO	6.18***	0.14***	SC	3.86***	0.34***
	(0.14)	(0.01)		(0.10)	(0.00)		(0.11)	(0.00)
DE	2.07***	0.33***	MS	0.67	0.38***	SD	3.27***	0.22***
	(0.10)	(0.03)		(0.51)	(0.07)		(0.10)	(0.00)
FL	3.11***	0.23***	MT	7.20***	0.13***	TN	1.75***	0.28***
	(0.18)	(0.01)		(0.31)	(0.00)		(0.12)	(0.01)
GA	3.70***	0.35***	NC	2.40***	0.24***	TX	3.85***	0.21***
	(0.11)	(0.00)		(0.11)	(0.00)		(0.20)	(0.01)
IA	2.89***	0.23***	ND	3.00***	0.23***	UT	1.99***	0.21***
	(0.14)	(0.00)		(0.09)	(0.03)		(0.11)	(0.02)
ID	5.21***	0.19***	NE	2.97***	0.23***	VA	4.37***	0.18***
	(0.13)	(0.01)		(0.12)	(0.00)		(0.10)	(0.00)
IL	3.07***	0.41***	NH	0.32**	0.21***	VT	3.40***	0.38***
	(0.10)	(0.03)		(0.17)	(0.00)		(0.10)	(0.00)
IN	4.48***	0.18***	NJ	4.75***	0.17***	WA	4.48***	0.18***
	(0.10)	(0.00)		(0.11)	(0.00)		(0.09)	(0.00)
KS	3.44***	0.21***	NM	7.71***	0.12***	WI	5.49***	0.16***
	(0.13)	(0.00)		(1.15)	(0.00)		(0.08)	(0.00)
KY	3.39***	0.38***	NV	3.18***	0.22***	WV	-1.43***	2.48***
	(0.09)	(0.03)		(0.21)	(0.01)		(0.32)	(0.21)
LA	2.68***	0.26***	NY	8.89***	0.11***			
	(0.27)	(0.01)		(0.08)	(0.00)			

*Notes: Standard errors are estimated via the delta method and are reported in parentheses. *, **, *** indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

TABLE F.1 Disaster-driven risk aversion using the model of Barberis et al. (2001)

Table F.1 reports the results from a GMM estimation of an unconditional CCAPM where investors derive utility from their financial wealth and exhibit loss aversion. The fundamental asset pricing equation is defined as in Barberis, Huang & Santos (2001):

$$1 = \mathrm{E}\left[{cg_{j,t}}^{-\alpha_j} R^i_{j,t+1}\right] + b_j \mathrm{E}[\hat{v}(R^m_{t+1})]$$

where

$$\hat{v}(R^m_{t+1}) = \begin{cases} R^m_{t+1} - R^f_t & \\ \lambda\left(R^m_{t+1} - R^f_t\right) & \end{cases} \quad for \quad \begin{matrix} R^m_{t+1} \geq R^f_t \\ R^m_{t+1} < R^f_t \end{matrix}$$

$cg_{j,t}$ is the growth in consumption in state j at time t, R^m_t is the gross return of the market portfolio and R^f_t is the risk-free rate. α_{j} is the RRA parameter and the parameter b_j indicates the relative importance of consumption and financial wealth on utility. $\hat{v}$ is the loss aversion factor and is fixed at $\hat{v} = 2.25$.

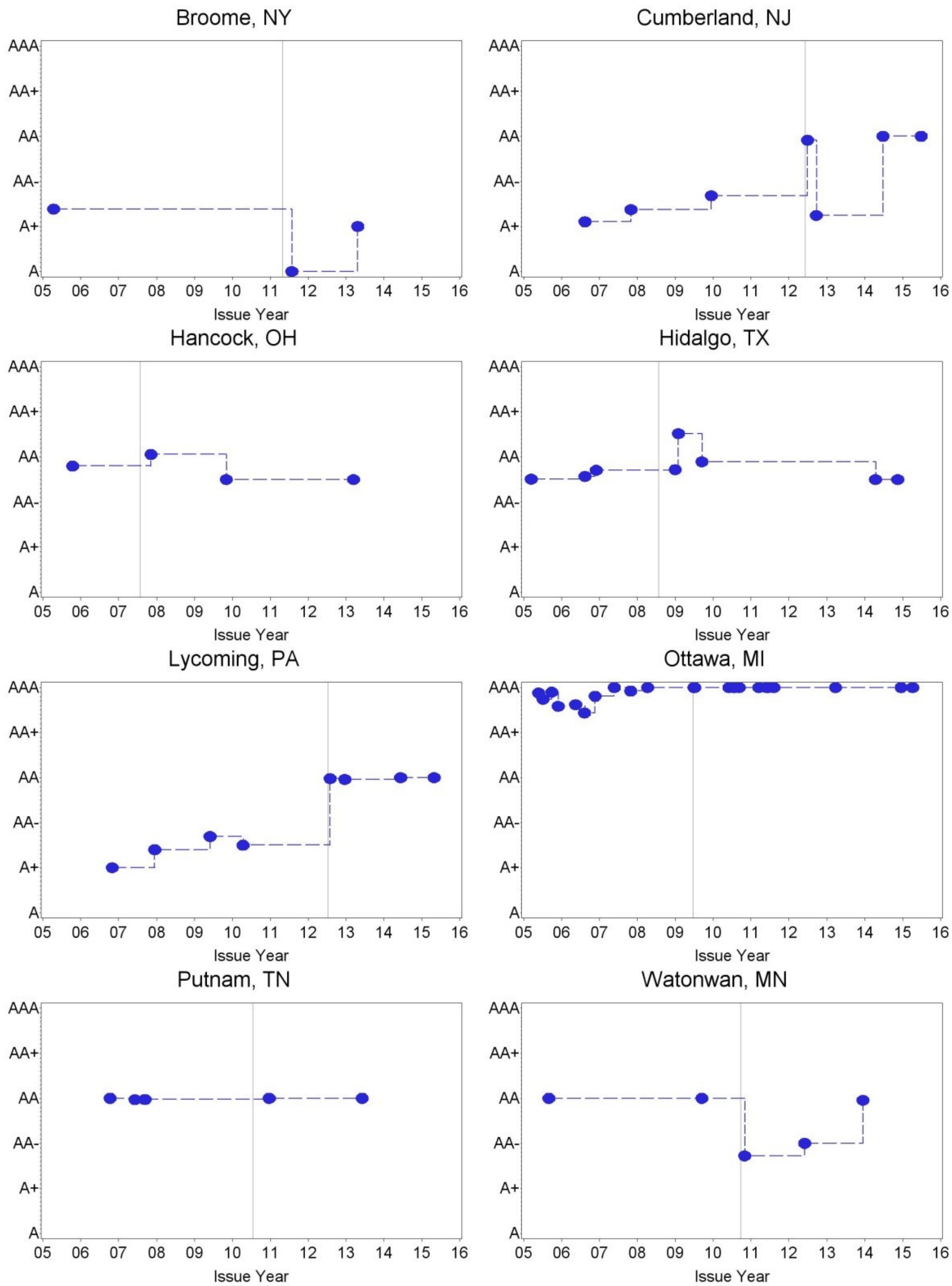
States	α_{j}	b_j	**States**	α_{j}	b_j	**States**	α_{j}	b_j
AL	-7.51*** (0.38)	11.78 (49.45)	MA	-1.37*** (0.13)	0.82 (4.32)	OH	-4.75*** (0.28)	5.10 (21.11)
AR	-4.65*** (0.24)	5.63 (24.20)	MD	-1.15*** (0.11)	0.88 (4.25)	OK	0.54*** (0.09)	0.00 (0.98)
AZ	4.23*** (0.30)	0.68 (6.11)	ME	-3.25*** (0.16)	4.08 (18.10)	OR	0.54*** (0.09)	0.00 (9.54)
CA	3.83*** (0.21)	1.19 (8.32)	MI	15.68*** (0.45)	10.61 (43.06)	PA	8.44*** (0.39)	0.00 (12.52)
CO	8.42*** (0.42)	1.87 (13.82)	MN	0.30 (0.21)	0.00 (0.56)	RI	-2.72 (2.15)	1.30 (6.86)
CT	-4.99*** (0.24)	6.39 (25.96)	MO	36.28*** (0.98)	112.04 (460.70)	SC	-0.78*** (0.07)	0.74 (3.29)
DE	4.47*** (0.19)	4.59 (21.47)	MS	-5.07*** (0.27)	5.18 (23.10)	SD	5.83*** (0.36)	0.01 (8.85)
FL	1.82*** (0.36)	0.16 (2.30)	MT	6.78*** (0.30)	0.07 (11.00)	TN	-0.30*** (0.04)	0.37 (1.67)
GA	-1.00** (0.09)	0.76 (3.50)	NC	0.13*** (0.02)	0.00 (0.40)	TX	28.11*** (0.90)	66.26 (276.69)
IA	7.15*** (0.26)	3.57 (20.40)	ND	-1.69*** (0.14)	1.10 (4.84)	UT	12.87*** (0.48)	16.31 (70.17)
ID	-7.43*** (0.37)	10.18 (41.69)	NE	6.08*** (0.25(	1.26 (12.16)	VA	0.16 (0.16)	0.00 (0.42)
IL	-8.24*** (0.43)	7.22 (31.11)	NH	7.87*** (0.32)	4.14 (23.96)	VT	-1.32*** (0.11)	0.80 (3.55)
IN	13.45*** (0.42)	9.19 (42.64)	NJ	0.25*** (0.03)	0.00 (0.48)	WA	10.56*** (0.34)	5.17 (32.94)
KS	14.91*** (0.52)	14.47 (65.48)	NM	19.17 (0.70)	13.17 (62.60)	WI	7.82*** (0.54)	0.00 (9.58)
KY	-7.27*** (0.52)	14.44 (60.85)	NV	0.27 (0.35)	0.00 (0.47)	WV	0.17* (0.11)	0.00 (0.67)
LA	7.76*** (0.35)	5.55 (25.88)	NY	10.47*** (0.03)	5.10 (21.11)			

*Notes: Standard errors are estimated via the delta method and are reported in parentheses. *, **, *** indicate statistical significance at the 0.10, 0.05 and 0.01 level, respectively.*

FIGURES

FIGURE 3.1 Variation in credit ratings surrounding flood episodes in selected counties

Figure 3.1 illustrates the change in the average credit rating across bond tranches sold by selected counties at different points in time.



Notes: Dots represent new municipal bond issues and shaded vertical rules show major flood events.

FIGURE 4.1 State-level relative risk aversion estimates

Figure 4.1 reports the estimated values of the RRA parameter ($\hat{\gamma}$) for the 47 states included in the study and reported in Table 4.7. RRA are estimated in a complete-market setup using the unconditional standard consumption-based capital asset pricing model using a GMM framework. State-level consumption series are inferred from a regression model and correspond to the portion of the state-level monthly electricity consumption that is orthogonal to the temperature-driven demand for energy and to intra-year seasonal patterns. Monthly municipal bond return series are constructed following a standard repeated sales methodology and using MSRB's municipal bond transaction data. Four maturity-based portfolios are used in the estimation process to generate state-level estimates of RRA. The sample covers the February 2005 to December 2016 period.

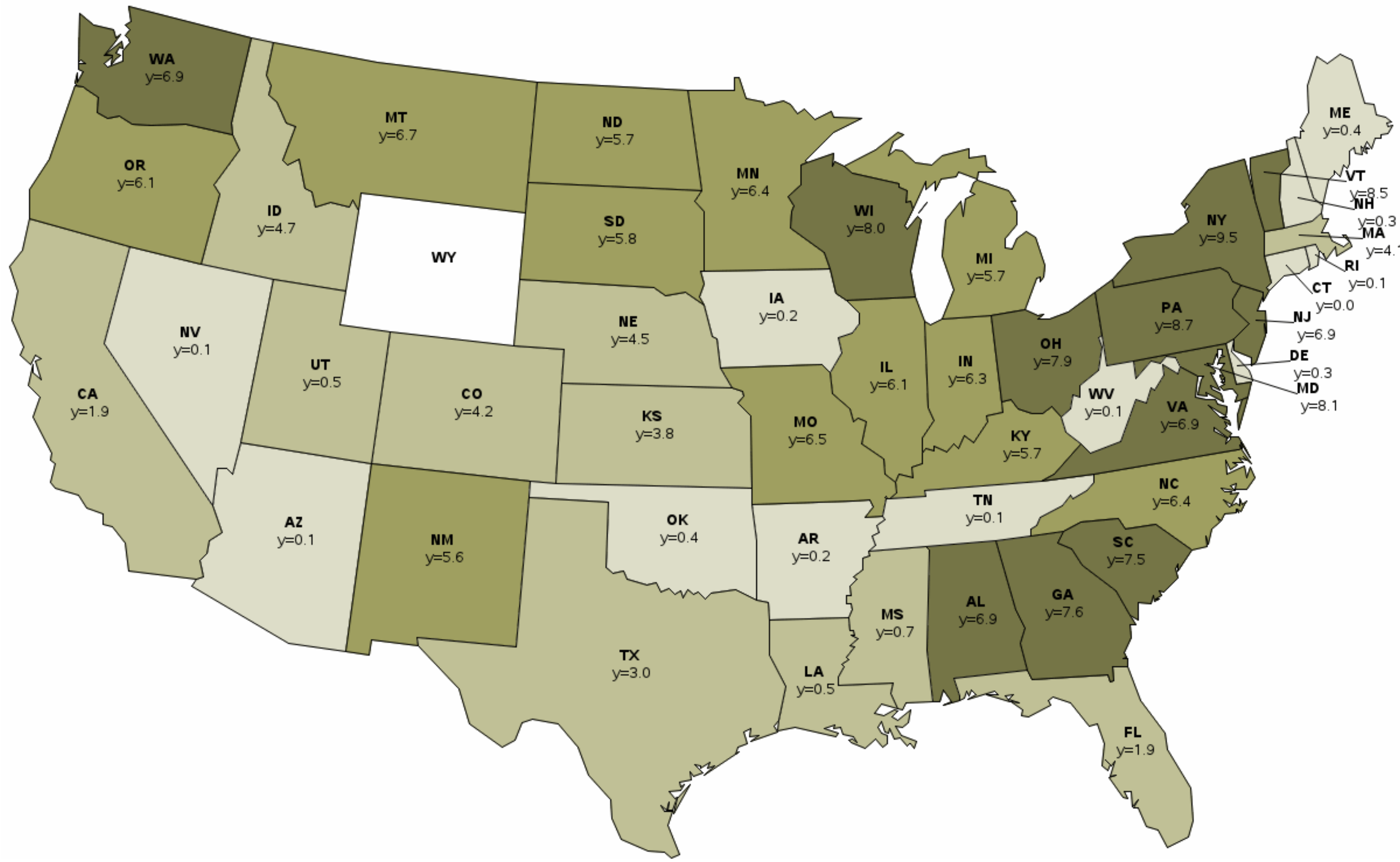


FIGURE 4.2 Patterns in pricing errors according to disaster intensity

Figure 4.2 illustrates the behavior of the CCAPM's pricing errors according to categories of disasters-related damage. The graphs report the betas and associated confidence intervals arising from the OLS regression:

$$u_{t+1} = b_0 + b_1 Small\_dmg_t + b_2 Medium\_dmg_t + b_3 Large\_dmg_t + b_4 Extreme\_dmg_t + \xi$$

where u are the pricing errors from the CCAPM. The Small_, Medium_, Large_ and Extreme_dmg variables are dummies that equals one if the total per capital state-level monthly disaster-related damage vary between]\$1,15],]\$15,50],]\$50,250] and]\$250,∞], respectively, and zero otherwise. Panel A distinguishes between state-month having positive and negative consumption growth. Panel B compares pricing errors associated with the short term municipal bond indices (bonds maturing in less than 2.5 years) and long term (bonds maturing in more than 7.5 years).