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Analysis

# Is Flood Risk Capitalised Into Property Values?

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ABSTRACT

Economic theory suggests that, other things being equal, properties located within a floodplain should suffer a price discount. A survey of the existing evidence nonetheless reveals that this price discount lies anywhere between −75.5% to a +61.0% price premium. In this paper we summarise and explore the wide variation in the results to obtain 'best' estimates with which to guide policy. Results from our meta-analysis comprising 37 published works and 364 point estimates indicate marked differences between studies according to when and where they were conducted. For coastal regions the results show that properties located in the floodplain command higher prices; this finding is however likely to be caused by a high correlation between omitted coastal amenities and flood risk. There is moreover, evidence that publication bias affects the coastal flooding literature. Results from meta-regression analyses intended to uncover sources of heterogeneity confirm that controlling for time elapsed since the most recent flood is especially important. For inland flooding the price discount associated with location in the 100-year floodplain is −4.6%. Although other estimates are defensible, we suggest this figure be used as a rule of thumb to determine the benefits of flood relief projects to households.

## 1. Introduction

Since the year 2000, there have been over 2800 major flood events reported globally (EM-DAT, 2017).[1] The economic losses reported to have been caused by these floods exceeds 538 billion USD globally (EM-DAT, 2017). Asia has been the region most affected by flooding in terms of the number of flood events (40%), number of victims (70%) and total flood damages (59%). Other regions such as Africa and the Americas have also been badly hit with each of these regions accounting for about 20% of the total number of flood events. Although the number of flood events that occurred in Europe represents only 13% of the total the economic losses in this region over the last 15 years account for 22% of global damages (EM-DAT, 2017). By the year 2050 the annual expected losses from floods are predicted to exceed 1 trillion USD (Hallegatte et al., 2013; World Bank, 2013; Munich Re, 2013).

Because of the scale of the problem, combatting floods is a significant policy issue. Considerable sums continue to be earmarked for flood relief projects although it is widely accepted that it is neither desirable nor feasible to protect all localities from flooding. Cost-benefit analysis of such projects entails comparing the monetised costs and benefits of each alternative and determining which yield an acceptable cost-benefit ratio. This enables scarce resources to be efficiently allocated between flood prevention schemes and other public works. In such analyses however the main challenge is to provide accurate measures of the benefits, not least because these combine both market and nonmarket impacts.

Some studies quantify the economic benefits of structural flood protection measures using a damage cost approach (Brouwer and Van Ek, 2004; Blonn et al., 2010; Jongman et al., 2012). Others use stated preference valuation techniques to estimate willingness to pay (WTP) for flood protection (Brouwer et al., 2009; Phillips, 2011; Veronesi et al., 2014). There are also studies estimating the economic value of flood protection afforded by natural ecosystems (Bateman and Langford, 1997; Bateman et al., 2001; Gibbons et al., 2014) and other non-structural defence measures (Holway and Burby, 1990; Meyer et al., 2012; Troy and Romm, 2004). Kazmierczak and Bichard (2010), Bramley and Bowker (2002) and Osberghaus (2015) consider the determinants of private flood mitigation measures. Another popular approach is to use hedonic analysis to estimate the benefits of a reduction in flood risk.

In an efficient housing market the price of property located inside the floodplain ought to be lower than the price of equivalent property outside. This price discount is interpreted as a measure of the benefits of a reduction in flood risk. Numerous authors have investigated the effect

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[1] As reported by "EM-DAT: The Emergency Events Database". To consult the inclusion criteria for quantification of natural disasters in the EM-DAT database see: http://www.emdat.be/.

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of location in a 500-year or 100-year floodplain on property prices for both inland and coastal locations. The results are however inconsistent and sometimes point to the presence of a price premium rather than the expected discount. Without a meta-analysis it is difficult to suggest a 'best' estimate of the percentage discount for floodplain location.

The use of meta-analysis is ubiquitous in environmental risk analysis. Smith and Huang (1995) use meta-analysis to infer WTP for reductions in air pollution based on evidence from hedonic studies. Nelson (2004) also conducts a meta-analysis of hedonic estimates of WTP for a reduction of noise from airports. More recently, Hjerpe et al. (2015) estimate the value of ecosystem conservation by means of a meta-analysis combining different stated preference valuation studies whilst Bergeijk and Lazzaroni (2015) use a meta-analysis to analyse the macroeconomic impact of natural disasters. Siriwardena et al. (2016) estimate the value of tree cover using a meta-analysis of hedonic studies undertaken in the US.

We make four main contributions to the literature. First, we update the only other meta-analysis of the discount for floodplain location. In so doing so we double the number of research papers and treble the number of observations used. Second, our analysis changes the way that primary studies contributing more than one study are weighted. Third, we conduct tests for publication bias and, finding that studies of coastal locations are severely affected by publication bias, exclude them from further analysis. Finally and most importantly, we collect supplementary information enabling us to control for the recent flood-history of locations where studies were undertaken.

The only existing meta-analysis of flood risk is Daniel et al. (2009a) who employ a meta-sample comprising 19 empirical studies and 117 estimates. They use meta-regression to explore the variation encountered using a set of 18 explanatory variables describing the spatio-temporal features of the studies, the design characteristics and the controls included in the original studies. Their paper finds that an increase in the yearly risk of flooding of 0.01 results in a change in house prices of − 0.6%.

We by contrast argue that knowing the flood history of areas in which primary studies were undertaken is of fundamental importance. The meta-analysis of Daniel et al. (2009a) distinguishes between those studies undertaken in periods during which there were no floods as well as difference in difference (DID) studies that provide separate estimates of the effect of floodplain location both before and after a flood event. They ignore the fact that all study locations possess a prior flood history the consequences of which may still be present. Distinguishing between studies by adding dummy variables identifying a before-the-flood or an after-the-flood DID estimate does not adequately control for recovery in prices. Our paper uses meta-regression to control for time elapsed since the most recent flood event in all of the primary studies.

Judging by the information we have assembled, existing studies tend to be undertaken in areas hit by recent floods rather than in areas which, although located in the floodplain, have avoided recent flooding. But by focusing on such sites the floodplain discount might have been overestimated.

Once we eliminate studies dealing with coastal flooding and control for the time elapsed since previous flood events, our preferred price discount for location within a 100-year floodplain is almost an order of magnitude different to that of Daniel et al. (2009a). Our findings call into question the simple pooling of studies undertaken in locations with different flood histories. They also serve as an example of how including in meta-analyses a set of observations that are severely affected by various forms of bias can seriously alter findings.

The remainder of our paper is arranged as follows. Section 2 describes the theory used to infer the impact of floodplain location, carefully distinguishing between the different sorts of evidence. Section 3 describes the data. Section 4 provides a meta-analysis of the change in property prices encountered in 100-year and 500-year floodplain locations. Section 5 addresses the issue of publication bias. Section 6 attempts to explain sources of heterogeneity in published results using meta-regression. Section 7 discusses these findings, assesses their robustness and explains why they differ from earlier ones. Section 8 concludes.

## 2. The Theoretical Model

This section defines the hedonic price function (HPF) in such a way as to consider explicitly the flood risk associated with particular properties. Based on the model of Hallstrom and Smith (2005), the subjective probability $p$ that a property will be flooded is a function $p(i,r)$ of the information set $i$ that the individual possesses about flood risk in the vicinity, and $r$ that represents all of the site attributes related to the risk of flooding e.g. elevation or proximity to water bodies. It is vital to differentiate the *subjective* assessment of the probability the house will be flooded from π which is the *objective* probability of flooding (Knuth et al., 2014). Nevertheless, in areas where the disclosure of the existence of flood risk is mandatory or publicly available, the set of information, $i$, might include the objective probability of flooding, π. The HPF is represented by the equation:

$$P = P(Z, r, p(i, r)) \tag{1}$$

Here $P$ denotes the price of the house and, whilst this is exogenous to prospective buyers and sellers, depends on the subjective risk perception $p(i,r)$; $Z$ represents an additional set of structural, environmental and locational characteristics of the house not indicative of flood risk. Following Brookshire et al. (1985) the decision of the household is modeled using a state dependent expected utility ($EU$) function:

$$EU = p(i, r) \bullet U^F[Z, r, Q] + (1 - p(i, r)) \bullet U^{NF}[Z, r, Q] \tag{2}$$

$U^F(\cdot)$ is the homeowner's utility in a state in which a flood occurs and $U^{NF}(\cdot)$ is the homeowner's utility when no flood occurs. The variable $Q$ denotes a composite commodity. The household's budget constraint is given by Eq. (3) where $M$ represents income:

$$M = P(Z, r, p(i, r)) + Q \tag{3}$$

Maximising expected utility with respect to $p$ subject to the budget constraint and then dividing through by the expected marginal utility of income results in the following expression:

$$\frac{\partial P}{\partial p} = \frac{U^F - U^{NF}}{p(i, r)\frac{\partial U^F}{\partial Q} + (1 - p(i, r))\frac{\partial U^{NF}}{\partial Q}} \tag{4}$$

Eq. (4) gives the coefficient on the subjective risk variable in the HPF. For optimality the implicit price of flood risk is equal to the difference in utility across states divided by the expected marginal utility of household income. Hence the household's locational decision provides a measure of WTP for a marginal change in the probability of flooding.

Empirical applications of the hedonic technique can be divided into two sorts. The first sort is able only to identify the impact on prices of changes in the subjective probability of flooding. The second sort is able to identify both the impact on prices of changes in the subjective probability of flooding as well as the impact on prices of the changes in information affecting the subjective probability of flooding. And whilst there are other sources of information, the variable that most obviously impacts the subjective probability of a flood is the actual occurrence of a flood. Consider the following HPF:

$$ln\, P_{it} = \alpha_0 + \sum_{j=1} \alpha_j Z_{ij} + \beta r_i + \gamma FPD_i + \delta Flood_{it} + \theta(Flood_{it} \times FPD_i) + \varepsilon_{it} \tag{5}$$

Here, $lnP_i$ indicates the log of the sale price of house $i$ *Flood* is a dummy variable that assumes the value unity if the transaction of property $i$ happened after some flood event and $FPD$ is a dummy variable which assumes the value unity if the property is located within the floodplain and $\varepsilon$ is a property specific error which is assumed $\varepsilon_i \sim N(0, \sigma^2 I)$. Floodplain location here serves as a proxy for the subjective probability of flooding whereas the occurrence of the flood event potentially alters the subjective probability of flooding. The parameter $\gamma$ measures the pre-flood relative price differential for house located in the floodplain, $\delta$ measures the relative sale price differential for all those properties which were sold after the flood whilst $\theta$ measures the impact on prices located within the floodplain arising out of the information conveyed by the flood.

Specifications like the one in Eq. (5) are termed DID models because they contain information about houses within and outside the floodplain, and a treatment i.e. flooding that effects only those houses in one group. Note also that according to the HPF the post-flooding price differential for houses located inside the floodplain is given by the sum of $\gamma$ and $\theta$.

There is evidence that the information effect $\theta$ will differ according to how long after the event it is measured. In particular, recent studies such as Atreya et al. (2013) and Bin and Landry (2013) find evidence suggesting that the information effect of flood events dies away. This suggests rewriting the HPF in such a way that $\theta$ is time dependent:

$$ln\, P_{it} = \alpha_0 + \sum_{j=1} \alpha_j Z_{ij} + \beta r_i + \gamma FPD_i + \delta Flood_{it} + \theta_t (Flood_{it} \times FPD_i) + \varepsilon_{it} \tag{6}$$

In this HPF the post-flood price differential is now $\gamma + \theta_t$. However, having admitted this, it is now no longer obvious whether the pre-flood estimate of the coefficient $\gamma$ might itself not also be affected by earlier floods.

In reality all estimates of the floodplain discount are post-flood estimates; even if no flood occurred during the period under observation it might be that a flood occurred just prior to the start of the study. In addition, the same issue effecting pre-flood estimates from DID models will also effect estimates of the floodplain discount from hedonic models estimated over periods during which no significant flood event occurred.

Differences in estimates of the floodplain price discount depend on floodplain designation. Clearly a greater price discount is expected for location in a 100-year floodplain than in a 500-year floodplain. We are for the purposes of cost-benefit analysis interested in how the discount varies according to floodplain designation. However, heterogeneity in the floodplain price discount is caused by combining analyses with different flood histories. This heterogeneity depends on the extent to which people overreact to flood events and how quickly the memory of such events fades. In order to control for this source of heterogeneity one has to know something about the flood history of each location.

Although it could certainly be the case that a flood event causes households to correct a poor subjective prior there are reasons to believe that the use of post-flood estimates from DID hedonic models is likely to result in biased estimates of the implicit price of flood risk in the housing market. The first argument is derived from Tversky and Kahneman's (1973) idea of an availability heuristic in the perception of risk; basically individuals assess the probability of an event by the ease with which the actual occurrence of such an event can be brought to mind (Wachinger et al., 2013; Knuth et al., 2014; Kellens et al., 2013). Cameron and Shah (2015) provide an illustration of this in a study of Indonesian households where they conclude that after a flood individuals inaccurately updated their perception of flood risk. In particular they find that individuals affected by flooding reported unrealistically high probabilities that another flood event would occur in the next year and that it would be severe, with an effect lasting for several years. The second reason for fearing bias is that studies looking at the post-flood discount of prices attribute the update in prices entirely to the effect of new information conveyed by the flood. They do not generally acknowledge the extent to which these changes might be driven by actual flood damages and a recovery of prices as individuals restore their property to its former condition. Atreya and Ferreira (2015) suggest that the majority of this effect might indeed be driven by flood damages, and that after identifying those properties directly affected by flooding there is no significant price update on other properties located inside the floodplain.[2]

## 3. The Data

We follow the reporting guidelines for meta-regression analysis in economics provided by Stanley et al. (2013), who list the four basic steps necessary to undertake a meta-analysis. First, it is necessary to define the theoretical relationship of interest. Second, one has to collect the population of studies that provide data on the relationship of interest. The third step involves coding the studies and computing the effect sizes. We detail these two steps in this section. The final steps which we present in Sections 5 and 6 are examination of the distribution of effect sizes and the influence of the moderating variables.

Our meta-analysis is founded on a systematic literature review employing widely-used proprietary databases and a secondary search through the bibliography of any studies thereby traced. Using Boolean operators the search was undertaken in English only. The combination of words used for this exercise were:

(Flood* OR Inundat* OR Hurricane*) AND (Propert* OR Hous* OR Resident* OR "Real Estate")

The use of * as the wildcard character permits us to enlarge the search by substituting a sequence of letters. For example, searching for *flood** will retrieve, *flood*, *floods*, *floodplain*, *flooding* and so on.

A number of databases e.g. the Social Science Research Network (SSRN) do not permit the use of wildcards or Boolean operators and here different strategies were employed. Other databases e.g. ProQuest retrieved too many irrelevant works. A chronologically-ordered summary of the studies identified in the literature review is contained in Table 1. This exercise successfully retrieved all of the papers in Daniel et al. (2009a) and many more besides.

In excess of 6000 records from various electronic databases were scrutinised and 164 studies earmarked for further inspection. A further update was undertaken in May 2014 to guarantee that any recently published studies were also included (resulting in an additional four studies). The rules for inclusion of studies into the meta-analysis are identical to those employed by Daniel et al. (2009a), chiefly:

(i) Estimates have necessarily to be obtained using an econometrically estimated HPF, either the DID or the standard hedonic model.
(ii) Estimates must be capable of being expressed, after recalculation if necessary, as a percentage of average house prices.
(iii) The risk of flooding should be captured by a dummy variable, where the dummy variable indicates location within the 500-year or 100-year floodplain.
(iv) For those studies which are available in more than one version of a paper only the most recent version is considered.

Adhering to these rules we compile a meta-sample which includes only those studies which are sufficiently homogenous for meaningful conclusions to be obtained through meta-analysis. These rules however, imply that certain sorts of studies were excluded. The first rule excludes those studies with estimates that were obtained through a comparison of average sale prices both outside and within a floodplain, such as Babcock and Mitchell (1980) and Zimmerman (1979), studies comparing rates of property price appreciation, such as Lamond et al.

[2] A referee has pointed out to us a further issue: whereas a standard hedonic study can identify the implicit price for flood risk reductions, a DID identifies capitalisation in the housing market which is not directly interpretable as marginal WTP (Kuminoff and Pope, 2014). For those who are concerned about this issue we also present results for hedonic and DID studies separately in appendix A2.

**Table 1**
Chronologically ordered summary of literature review.
Source: Own elaboration.

| Database | Date | Total of entries | Saved for further research |
|---|---|---|---|
| EconLit | 18/04/2013 | 365 | 59 |
| Social Science Citation Index and Conference Proceedings Citation Index | 24/04/2013 | 249 | 34 |
| IngentaConnect | 25/04/2013 | 982 | 12 |
| Environmental Valuation Reference Inventory | 30/04/2013 | 228 | 11 |
| AGRICOLA. US National Agricultural Library Catalog | 02/05/2013 | 143 | 0 |
| SSRN | 02/05/2013 | 285 | 16 |
| ProQuest | 03/05/2013 | 3776 | 32 |
| Total | | 6028 | 164 |

(2010) or Eves (2002) and those studies utilising repeat-sales models, such as Carbone et al. (2006) and Lamond and Proverbs (2006).[3] The second rule necessitates dropping studies that provide monetary estimates of the change in the price associated with floodplain location but fail to provide information on the mean house price e.g. Holway and Burby (1990). The third rule excludes studies such as Barnard (1978), Tobin and Montz (1994) and Shilling et al. (1985) who use the cost of flood insurance, elevation or flood depth rather than floodplain location as an indicator for flood risk. Also excluded is the study of Atreya and Ferreira (2012b) who use a solitary dummy variable in order to identify those properties that are located in a floodplain irrespective of designation, in addition to some results from studies by Bin and Landry (2013) and Bin et al. (2008b) where the extent of the risk is unspecified.

The resulting meta-sample contains 37 studies and 349 point estimates, twice the studies considered by Daniel et al. (2009a) and three times the number of point estimates. The publication dates of studies contained in the meta-sample ranges from 1987 to 2013. The meta-sample contains 33 studies from the United States as well as studies from Australia, New Zealand, the Netherlands and the United Kingdom (as shown in Table 2).

Whilst all the studies use dummy variables controlling for floodplain location the functional form selected for the HPF differs from study to study, and various adjustments were required. The effect size of interest is the relative house price differential for floodplain location and, adopting the notation of Daniel et al. (2009a), we refer to this as $T$, with $s_T$ the associated standard errors. The majority of studies employ a semi-log functional form as in Eq. (7), the variables are defined as in Eq. (5).[4] Here, the effect size $T$ and the standard error $s_T$ correspond to the parameter $\gamma$ and the standard error $s_\gamma$ as extracted direct from the primary studies themselves.[5] Studies by Donnelly (1989), Speyrer and Ragas (1991), Bialaszewski and Newsome (1990), US Army Corps of Engineers (1998), Shultz and Fridgen (2001) and Harrison et al. (2001) by contrast provide estimates that are taken from linear specifications such as those in Eq. (8) so that $T = \gamma/\overline{P}$ and $s_T = s_\gamma/\overline{P}$, where $\overline{P}$ is the mean selling price of the sample.

$$lnP_i = \alpha_0 + \gamma FPD_i + \sum_{j=1} \alpha_j Z_{ij} + \varepsilon_i \tag{7}$$

$$P_i = \alpha_0 + \gamma FPD_i + \sum_{j=1} \alpha_j Z_{ij} + \varepsilon_i \tag{8}$$

$$\frac{P_i^\lambda - 1}{\lambda} = \alpha_0 + \gamma FPD_i + \sum_{j=1} \alpha_j Z_{ij} + \varepsilon_i \tag{9}$$

Studies by MacDonald et al. (1987), Dei-Tutu and Bin (2002), MacDonald et al. (1990) and Bin (2004) resort to a Box-Cox specification such as that shown in Eq. (9). Here $T = \gamma \overline{\overline{P}}^{1-\hat{\lambda}}$, where $\overline{\overline{P}}$ represents the average price and $\hat{\lambda}$ is a non-linear parameter. In this instance $T$ depends upon two random parameters and accordingly $s_T$ cannot readily be calculated from the reported parameters. Using the same strategy as Daniel et al. (2009a) the standard errors have been approximated by means of the Delta method, as shown in Eq. (10).

$$s_T = \sqrt{\left(\frac{\partial T}{\partial \lambda}\right)^2 \sigma_\lambda^2 + \left(\frac{\partial T}{\partial \gamma}\right)^2 \sigma_\gamma^2 + 2\left(\frac{\partial T}{\partial \lambda}\right)\left(\frac{\partial T}{\partial \gamma}\right) r_{\gamma\lambda}\sigma_\gamma\sigma_\lambda} \tag{10}$$

Here $\sigma_i$ denotes the standard error of $\lambda$ and $\gamma$, respectively, and $r_{\gamma\lambda}$ is the associated correlation coefficient. For studies like MacDonald et al. (1990) and MacDonald et al. (1987) which fail to provide any estimate of $\sigma_\lambda$, this is approximated with a standard error of $\lambda/2$ as in Daniel et al. (2009a); a step which makes $\lambda$ significant at the 5% level of confidence. Since an estimate of $r_{\gamma\lambda}$ is typically unavailable, the value of $\pm$ 0.9 has been inserted depending on whether the expression $(\partial T/\partial\gamma)(\partial T/\partial\lambda)$ is positively or negatively signed to generate conservative standard errors.

All the estimates used in our analysis are based on actual sales data; 13 estimates from a study conducted by the US Army Corps of Engineers (1998) based on appraised values are excluded whilst other estimates from the same study are retained.[6] For studies using a spatial lag we record the total effect of flood risk location meaning that any spatial spillovers arising out of the prices of neighbouring properties are included.[7] Studies incorporating a spatial lag include Daniel et al. (2007), Posey and Rogers (2010), Bin et al. (2008a, 2008b), Atreya and Ferreira (2012a), Atreya et al. (2012), Atreya and Ferreira (2012c), Atreya et al. (2013) and Meldrum (2013).[8]

Finally, we collect information on the flood history of each study's location. Using this information we calculate time elapsed since the last flood. Some hedonic studies such as Bin and Landry (2013), Turnbull et al. (2013), Rambaldi et al. (2013), Bin et al. (2008a), Lamond and Proverbs (2006), Bin (2004), Bartosova et al. (1999) and US Army Corps of Engineers (1998) already discuss the flood history of the

[3] Repeat sales models value only the change in the price of property located in the floodplain following an 'informational update' i.e. a flood event. In other words, using results from repeat-sales models we are only be able to recover an estimate of $\theta$ as expressed in Eq. (5) which corresponds to the price update after a flood. Without information on $\gamma$ (the pre-flood price differential) we are unable to compute the post-flood price differential.

[4] For convenience we exclude the variable $r_i$.

[5] Note that, strictly, with a dummy variable contained in a semi-log functional form the marginal effect ought to be adjusted to $e^\gamma - 1$ (Halvorsen and Palmquist, 1980). Despite this, following Daniel et al. (2009a) these adjustments are ignored given the very small size of the coefficients.

[6] These were, included in Daniel et al. (2009a).

[7] For studies using spatial econometric methods the total effect was calculated following Golgher and Voss (2016). In an earlier version of the manuscript for spatial studies we included only the direct effect. The results are largely unaffected by using the total effect rather that the direct effect.

[8] Note that the average effect sizes contained in Table 2 correspond to the relative house price differential for floodplain location and are not adjusted for differing levels of risk. In contrast Daniel et al. (2009a) standardised both the effect sizes as well as the corresponding standard errors in order to account for any differences in the degree of risk reporting them as $T^* = T \times (1/\omega \times 100)^{-1}$, where $T$ denotes the unstandardised effect size and the recurrence interval is $\omega$. In our research no standardisation has been attempted and any differences arising out of differences in the levels of risk in our study are investigated as part of our meta-analysis.

**Table 2**
Summary of studies included in the meta-sample.

| No. | Authors | Year | Country[a] | Location | Flood risk (floodplain) | No. obs. | Effect size (*T*) | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | Mean | S.D. | Min. | Max. |
| 1 | MacDonald et al. | 1987 | US | Louisiana | 100 | 2 | − 0.077 | 0.014 | − 0.086 | − 0.067 |
| 2 | Skantz and Strickland | 1987 | US | Texas | 100 | 8 | − 0.025 | 0.019 | − 0.056 | − 0.012 |
| 3 | Donnelly | 1989 | US | Wisconsin | 100 | 1 | − 0.121 | – | – | – |
| 4 | Shilling et al. | 1989 | US | Louisiana | 100 | 1 | − 0.076 | – | – | – |
| 5 | Bialszewski and Newsome | 1990 | US | Alabama | 100 | 1 | 0.000 | – | – | – |
| 6 | MacDonald et al. | 1990 | US | Louisiana | 100 | 2 | − 0.100 | 0.024 | − 0.117 | − 0.083 |
| 7 | Speyrer and Ragas | 1991 | US | Louisiana | 100 | 4 | − 0.098 | 0.073 | − 0.204 | − 0.042 |
| 8 | US Army Corps of Engineers | 1998 | US | Texas | 100 | 14 | − 0.029 | 0.083 | − 0.268 | 0.080 |
| 9 | Bartosova et al. | 1999 | US | Wisconsin | 100 and 500 | 7 | − 0.016 | 0.074 | − 0.078 | 0.144 |
| 10 | Harrison et al. | 2001 | US | Florida | 100 | 4 | − 0.025 | 0.013 | − 0.041 | − 0.014 |
| 11 | Shultz and Fridgen | 2001 | US | ND and MI[b] | 100 and 500 | 4 | − 0.032 | 0.073 | − 0.102 | 0.031 |
| 12 | Troy | 2001 | US | California | 100 | 20 | 0.024 | 0.022 | − 0.017 | 0.061 |
| 13 | Dei-Tutu and Bin | 2002 | US | North Carolina | 100 | 1 | − 0.062 | – | – | – |
| 14 | Bin | 2004 | US | North Carolina | 100 | 4 | − 0.062 | 0.015 | − 0.076 | − 0.044 |
| 15 | Bin and Polasky | 2004 | US | North Carolina | 100 | 3 | − 0.060 | 0.023 | − 0.084 | − 0.038 |
| 16 | Troy and Romm | 2004 | US | California | 100 | 2 | − 0.011 | 0.030 | − 0.032 | 0.009 |
| 17 | Hallstrom and Smith | 2005 | US | Florida | 100 | 8 | 0.066 | 0.118 | − 0.113 | 0.173 |
| 18 | Bin and Kruse | 2006 | US | North Carolina | 100 and 500 | 9 | 0.107 | 0.235 | − 0.103 | 0.610 |
| 19 | Lamond and Proverbs | 2006 | UK | North Yorkshire | 100 | 2 | − 0.175 | 0.005 | − 0.178 | − 0.171 |
| 20 | Daniel et al. | 2007 | NL | Meuse River | 100 | 15 | − 0.033 | 0.054 | − 0.082 | 0.084 |
| 21 | Morgan | 2007 | US | Florida | 100 | 3 | 0.254 | 0.080 | 0.165 | 0.321 |
| 22 | Bin et al. | 2008a | US | North Carolina | 100 | 2 | − 0.146 | 0.026 | − 0.165 | − 0.128 |
| 23 | Bin et al. | 2008b | US | North Carolina | 100 and 500 | 6 | − 0.055 | 0.031 | − 0.078 | − 0.010 |
| 24 | Pope | 2008 | US | North Carolina | 100 and 500 | 22 | − 0.002 | 0.025 | − 0.045 | 0.038 |
| 25 | Daniel et al. | 2009b | NL | Meuse River | 100 | 4 | − 0.049 | 0.041 | − 0.086 | 0.005 |
| 26 | Kousky | 2010 | US | Missouri | 100 and 500 | 46 | − 0.024 | 0.017 | − 0.073 | 0.008 |
| 27 | Samarasinghe and Sharp | 2010 | NZ | Auckland | 100 | 4 | − 0.040 | 0.025 | − 0.064 | − 0.014 |
| 28 | Posey and Rogers | 2010 | US | Missouri | 100 | 2 | − 0.099 | 0.002 | − 0.100 | − 0.098 |
| 29 | Atreya and Ferreira | 2011 | US | Georgia | 100 and 500 | 6 | − 0.134 | 0.143 | − 0.375 | 0.042 |
| 30 | Atreya and Ferreira | 2012c | US | Georgia | 100 | 20 | − 0.187 | 0.245 | − 0.722 | 0.127 |
| 31 | Atreya and Ferreira | 2012a | US | Georgia | 100 and 500 | 18 | − 0.174 | 0.195 | − 0.677 | 0.102 |
| 32 | Atreya et al. | 2012 | US | Georgia | 100 and 500 | 22 | − 0.084 | 0.164 | − 0.382 | 0.101 |
| 33 | Rambaldi et al. | 2013 | AU | Queensland | 100 | 1 | − 0.013 | – | – | – |
| 34 | Atreya et al. | 2013 | US | Georgia | 100 and 500 | 40 | − 0.166 | 0.229 | − 0.755 | 0.089 |
| 35 | Bin and Landry | 2013 | US | North Carolina | 100 and 500 | 18 | − 0.096 | 0.113 | − 0.423 | 0.041 |
| 36 | Meldrum | 2013 | US | Colorado | 100 | 21 | − 0.057 | 0.060 | − 0.158 | 0.010 |
| 37 | Turnbull et al. | 2013 | US | Louisiana | 100 and 500 | 10 | − 0.006 | 0.016 | − 0.023 | 0.014 |
| | Overall | | | | | 349 | − 0.061 | 0.149 | − 0.755 | 0.610 |

[a] AU = Australia, NL = The Netherlands, NZ = New Zealand, UK = United Kingdom, US = United States.
[b] ND = North Dakota, MI = Minnesota.

location. In these cases, months elapsed since the most recent flood is then easily calculated by subtracting the date of the most recent flood from the median date of the transaction data used in the study. For DID studies the time elapsed since the most recent flood is, for 'pre-flood' estimates, clearly different to that for 'post-flood' estimates. Since DID studies invariably mention the date of the flood event around which the study is constructed it is easy to calculate time elapsed for the post-flood estimates. For recent studies by Atreya et al. (2012, 2013), Atreya and Ferreira (2011, 2012a, 2012c) and Bin and Landry (2013) which investigate how the information content of flood events diminishes with the passage of time as in Eq. (6), the resulting post-flood price differential is the price discount for floodplain location immediately after the flood.

To determine time elapsed since the previous flood for studies that do not discuss the flood history of the location as well as for the 'pre-flood' period of DID studies it was necessary to examine historical records. Using the google search engine we retrieved information from previous floods at the city or county level according to the area of interest in the primary studies. More specifically, we focus on identifying major flood events that could cause a significant impact on the price of properties. The search was conducted on a study by study basis based on four criteria: (1) the location of interest of the primary study, (2) the use of words such as "flood" or "inundation" to define the nature of the event, (3) the use of words such as "major", "large" or "extreme", to denote the magnitude of the flood, and (4) the date of occurrence of the flood before the start date of the sample. In general, the date of the previous flood is taken from official online reports of the local authority or reports from local news. The occurrence of the event was then confirmed using different sources to ensure a consistent description of the event as a "major" flood event. Table A1 in the appendix shows the date of the previous flood for each study considered in the meta-analysis. The number of months elapsed since the previous flood was calculated by subtracting the date of the previous flood from the median date of the transaction data used in the study.[9]

It is interesting to note that, although all primary studies investigate properties located in either the 100-year or the 500-year floodplain, according to the evidence we have compiled the time elapsed since the last flood event is surprisingly short. In those hedonic studies which do not observe a flood event during the period of the study, the average amount of time elapsed since the most recent flood was 15.8 years. For the post-flood component of the DID study the average time elapsed

[9] Note that there are 5 studies where the "event" corresponded to a change in law or regulation (Samarasinghe and Sharp, 2010; Troy and Romm, 2004; Pope, 2008; Troy, 2001; Harrison et al., 2001). When collecting the data from these studies we handled them in the same way as we handled the estimates of a flood. That is for the post-disclosure effect we add a pre-disclosure discount plus the update after disclosure. In this way, the disclosure is considered similar to the occurrence of a flood in that both events provide new information with which individuals are able to update their perception of risk. In this case, however, the date of the previous flood is the same for the pre- and post-disclosure effects.

was 2.5 years. But whereas choosing a location with a recent flood event is necessary for the conduct of a DID study even for these studies there seems to have been a prior flood event occurring shortly before the 'main' event. On average a prior flood occurred 21.2 years before the midpoint of the pre-flood period of DID studies.

Either researchers have chosen locations subject to recent flooding or these locations contain low-lying areas which are in fact subject to flooding far more frequently than their 100-year or 500-year floodplain designation suggests. We suspect part of the explanation is that a 100-year floodplain is defined by the risk at the edge of the floodplain. Highfield et al. (2013) and Czajkowski et al. (2013) note floodplain designations are neither an accurate nor sufficient indication of the level of risk because houses are treated equally regardless of their distance to the source of risk. Within a 100-year floodplain there are properties which can be affected by floods with a shorter return period e.g. of 1 in 50 years, and individuals with a different perception of risk (O'Neil et al., 2016; Ho et al., 2008). Despite this we suspect researchers are drawn to conduct analyses in locations with a recent flood history. In situation in which the price discount for locating within a 100-year floodplain is partially determined by the time elapsed since the most recent flood event a different set of results might be obtained from randomly selecting floodplains rather than concentrating on those recently flooded.

## 4. Meta-analysis

According to Table 2 there is general agreement that the price of property located in the floodplain is lower than that of an equivalent property outside; 33 out of 37 studies report, on average, a depressing effect of floodplain location. Properties situated inside a floodplain are reduced in value by on average − 6.1% although there is significant within and between study variation as revealed by the columns recording the standard deviations (SD) of the effect sizes and the minimum and maximum effect size. Estimates range from the − 75.5% price discount found by Atreya et al. (2013) to the + 61.0% price premium reported by Bin and Kruse (2006). Fig. 1 displays the 349 estimates of the effect size included in our meta-sample.

We now perform a meta-analysis of the estimated effect sizes. There are in the literature two commonly encountered models used for combining together effect-sizes: the fixed-effects and random-effects models. In either weights are usually chosen using the (inverse) error-variance with the consequence that greater weight is given to more precise studies. The essential difference between these two models resides in the assumptions that define the error variance.

With the fixed-effects model the assumption is that all studies possess a common effect size; any differences in the observed effects occur only because of sampling error. Alternatively put, if the sample size was infinite the observed effect would be the same for all studies. However,

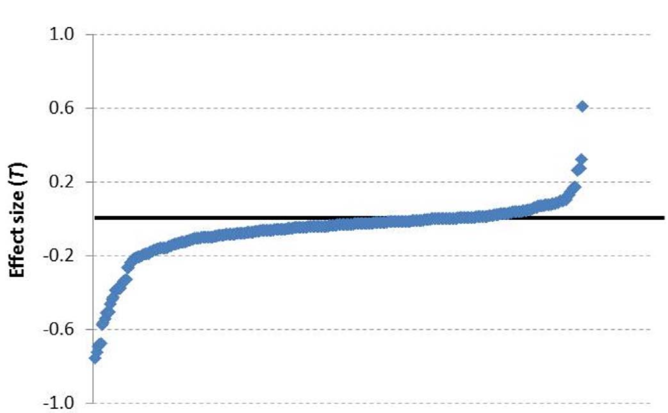


**Fig. 1.** Meta-sample: relative price differential for location in the floodplain.
Source: Authors' own elaboration founded on results from primary studies.

because studies commonly differ in terms of both the way that they are implemented as well as the underlying population the assumption of the fixed-effects is here implausible. By contrast the random-effects model allows the real effect size to differ across observations. In this case the goal is to estimate the mean of the distribution of true effect sizes.

Table 3 reports the statistics from the random-effects model estimated on all 349 effect sizes. Also reported are results from different subsamples where observations have been put into categories according to the floodplain designation, inland versus coastal flooding and whether it is a before or after-the-flood DID estimate or an estimate from a standard hedonic model. The table additionally reports the between-study variance $\tau^2$, the 90% confidence intervals, the $I^2$ and the $Q$-statistic.

So far we have treated each observation as if it were a separate study, something which results in inappropriate weight being placed on those studies reporting in excess of one outcome. To rectify this shortcoming, Table 3 also shows the summary statistics invoking another common weighting scheme: conferring weights according to a study's sample size. The weight given to each particular observation corresponds to the square root of the mean sample size of each study, divided by however many estimates each study contributes to the final meta-sample. Hence studies are given more weight because of the information they contain and not simply because the researchers happened to report more estimates (Stanley and Doucouliagos, 2015).

The overall effect size for estimates in the 100-year floodplain points to a premium of + 3.7% rather than the expected price discount. This premium however, is attributable to properties threatened by coastal flooding. If we further divide the sample the overall effect size for properties affected by inland flooding points to a discount of − 5.6% but for properties at risk of coastal flooding a premium of + 14.8%. Bin and Kruse (2006), Hallstrom and Smith (2005) and Bin et al. (2008a) suggest such results are due to the failure to control for the presence of amenities associated with proximity to the coast. Using an equilibrium sorting model, Fan and Davlasheridze (2016) also find evidence that amenity values dominate flood risk in coastal regions. We believe that it is at present impossible to draw reliable inferences from studies carried out in coastal regions.

For properties subject to inland flooding in the 100-year floodplain, if we focus only on the evidence from DID models before the flood event, there is a sizeable discount of − 2.9%. After a flood the discount rises to − 6.9%. For houses in the 500-year floodplain, before a flood event DID models suggest that the price differential is + 0.3 but after a flood event there is a discount of − 5.2%.[10] This finding supports the idea that there is a significant updating of beliefs in localities where no prior capitalisation of flood risk has occurred (Kousky, 2010).

## 5. Publication Bias

So far, our meta-analysis has assumed the effect sizes from published studies constitutes a representative sample of the population of all possible studies, notwithstanding the possible tendency of researchers to conduct studies in locations with a recent history of flooding. The chance of sample selection has hitherto been ignored. Since De Long and Lang (1992), publication bias has however been acknowledged as an important issue in empirical work. Card and Krueger (1995) list three possible sources of selection bias:

1. Editors and reviewers might be inclined to accept papers with findings consistent with economic theory.
2. Researchers might use as a criterion for model selection the presence of an expected result.

[10] It has been pointed out to us that insurance is mandatory for houses in high risk areas in the US when the house is purchased using a mortgage from a federally regulated lender.

**Table 3**
Meta-analysis: summary statistics.

| Sample | Random-effects | | | | | | Sample size weights | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | N | Summary statistic[a] | 90% conf. interval | $\tau^2$ | Q-Stat[b] | $I^2$ | Summary statistic[a] | 90% conf. interval | Q-Stat[b] | $I^2$ |
| **All** | **349** | **− 0.027*****| **[− 0.035; − 0.020]** | **0.0032** | **5962.7***** | **94.2** | **− 0.032***** | **[− 0.037; − 0.027]** | **6940.3***** | **95.0** |
| 500 year | 93 | − 0.001 | [− 0.006; 0.005] | 0.0001 | 133.9*** | 31.3 | − 0.019*** | [− 0.030; − 0.009] | 374.7*** | 75.4 |
| 100 year | 256 | − 0.036*** | [− 0.046; − 0.027] | 0.0042 | 5736.4*** | 95.6 | 0.037*** | [0.043; 0.032] | 6569.1*** | 96.1 |
| Inland | 314 | − 0.028*** | [− 0.033; − 0.022] | 0.0011 | 2132.7*** | 85.3 | − 0.044*** | [− 0.049; − 0.038] | 3528.6*** | 91.1 |
| Inland 100-year | 226 | − 0.038*** | [− 0.044; − 0.031] | 0.0013 | 1794.7*** | 87.5 | − 0.056*** | [− 0.062; − 0.050] | 3248.6*** | 93.1 |
| DID Inland 100-year BF | 63 | − 0.017*** | [− 0.029; − 0.006] | 0.0011 | 239.4*** | 74.1 | − 0.029*** | [− 0.039; − 0.020] | 120231*** | 99.9 |
| DID Inland 100-year AF | 71 | − 0.079*** | [− 0.101; − 0.058] | 0.0039 | 222.4*** | 68.5 | − 0.069*** | [− 0.088; − 0.051] | 257.2*** | 72.8 |
| Inland 500-year | 88 | 0.003 | [− 0.002; 0.007] | 0.0000 | 104.5* | 16.7 | − 0.019*** | [− 0.030; − 0.008] | 347.1*** | 74.9 |
| DID Inland 500-year BF | 31 | 0.005 | [− 0.003; 0.013] | 0.0000 | 14.7 | 0.0 | 0.003 | [− 0.008; 0.014] | 1512.5*** | 98.0 |
| DID Inland 500-year AF | 32 | − 0.025*** | [− 0.041; − 0.009] | 0.0000 | 14.6 | 0.0 | − 0.052*** | [− 0.078; − 0.027] | 26.24 | 0.0 |
| Coast | 35 | 0.028 | [− 0.032; 0.089] | 0.0308 | 2074.2*** | 98.4 | 0.134*** | [0.122; 0.146] | 2076.9*** | 98.4 |
| Coast 100 year | 30 | 0.045 | [− 0.019; 0.109] | 0.0298 | 1813.7*** | 98.4 | 0.148*** | [0.134; 0.161] | 1819.4*** | 98.8 |
| DID Coast 100-year BF | 7 | 0.118 | [− 0.030; 0.265] | 0.0391 | 841.3*** | 99.3 | 0.241*** | [0.229; 0.253] | 969.7*** | 99.4 |
| DID Coast 100-year AF | 7 | 0.016 | [− 0.072; 0.105] | 0.0110 | 43.02*** | 86.1 | 0.100*** | [0.067; 0.132] | 45.47*** | 86.8 |
| Coast 500-year | 5 | − 0.066*** | [− 0.092; − 0.040] | 0.0000 | 1.35 | 0.0 | − 0.069*** | [− 0.100; − 0.037] | 1.40 | 0.0 |
| Hedonic | 138 | − 0.030*** | [− 0.040; − 0.021] | 0.0023 | 3383.3*** | 96.0 | − 0.039*** | [− 0.043; − 0.034] | 4299.4*** | 96.8 |
| DID Hedonic | 211 | − 0.033*** | [− 0.047; − 0.019] | 0.0067 | 2383.9*** | 91.2 | − 0.027*** | [− 0.035; − 0.019] | 2931.9*** | 92.8 |
| DID Hedonic BF | 101 | − 0.004 | [− 0.023; 0.015] | 0.0070 | 1928.7*** | 94.8 | − 0.001 | [− 0.008; 0.006] | 2071.7*** | 95.2 |
| DID Hedonic AF | 110 | − 0.060*** | [− 0.078; − 0.043] | 0.0039 | 339.0*** | 67.8 | − 0.053*** | [− 0.067; − 0.039] | 381.8*** | 71.5 |

*, ** and *** implies rejection of the null at the 10%, 5% and 1% significance level. BF = before flooding event, AF = after flooding event.
[a] H0: that the summary effect size is insignificantly different from zero.
[b] H0: that all studies contained in the sample share a single effect size.

3. A general tendency to view statistically significant results in a more favourable light.

We have avoided adding to this problem by undertaking a comprehensive review and by incorporating evidence irrespective of significance and the sign of the effect. However, as noted publication bias could originate from other sources, and hence our efforts might eliminate some, but not all the potential bias. The purpose of this section is to gauge whether publication bias is present in the literature.

Publication bias results from selective sampling. The literature identifies two main kinds of publication bias:

**Type I.** → Directional: Selection supports a particular effect, e.g. a negative or positive effect.

**Type II.** → Statistical significance: Selection favours statistically significant results, irrespective of their sign.

Meta-regression is now a popular means of identifying publication bias, in particular by testing for funnel plot asymmetry. Card and Krueger (1995) illustrate this technique by examining the effect of minimum wage legislation on employment. At its simplest, meta-regression for testing and correcting for publication bias takes the form of a regression between the effect sizes and their associated standard errors, as in Eq. (11).

$$T_i = \beta_0 + \beta_1 s_{Ti} + \varepsilon_i \tag{11}$$

In the absence of selection, effect sizes ought to vary randomly around $\beta_0$, and be independent of the standard error. Yet as Egger et al. (1997) notes when publication bias is present it is revealed by the sign and statistical significance of $\beta_1$, whereas $\beta_0$ can be regarded as the true effect size, after controlling for publication bias. The meta-regression is however frequently transformed because the error term $\varepsilon_i$ will be heterosedastic because primary studies use different sample sizes. Accordingly, a variant of Eq. (11) is typically used to obtain superior estimates. Dividing by the estimated standard errors results in Eq. (12). Stanley (2005) shows this procedure eliminates both sorts of publication bias noted above.

$$T_i/s_{Ti} = \beta_1 + \beta_0(1/s_{Ti}) + e_i \tag{12}$$

Table 4 reports the results for Eq. (12). Column 1 displays the results using the entire sample of effect sizes. We report Huber-White standard errors to account for remaining heteroskedasticity. A $t$-test on the intercept, $\beta_1$, points to significant publication bias with a tendency to reporting more negative impacts. Egger et al. (1997) suggest the power of this test is restricted and prefer to base evidence of asymmetry on the one tailed $t$-test with $p < 0.1$. Results of this second test are provided in square brackets in Table 4. This test also reveals evidence of publication bias. Additionally, the coefficient $\beta_0$ seems to suggest that the effect of flood risk on house prices is insignificant. However, the coefficients contained in column 1 weight all of the effect sizes as if wholly independent studies; something which results in an overrepresentation of particular studies contributing more than one observation. To address this issue we again assign weights in proportion to the square root of each study's sample size divided by however many estimates it contributes. Column 2 displays the results with weighting for the entire sample, and columns 3–7 report coefficients for different samples.

After accounting for the overrepresentation of studies contributing multiple estimates, the results in column 2 reveal that there is now no longer any evidence of publication bias in the literature as a whole. Critically however, the coefficient $\beta_0$ is at the same time insignificantly different from zero; something suggesting that the effect of flood risk on house prices is imperceptible when publication bias is removed.

Once we consider different subsamples results change. There is no evidence of publication bias for studies looking at the 100-year floodplain, inland flooding or inland flooding in the 100-year floodplain. By contrast, there is evidence of publication bias for studies examining the 500-year floodplain and coastal flooding. After correcting for publication bias properties located in coastal floodplains enjoy a price premium of 36.3%, although, as discussed, it is almost certain that this results from a failure to adequately control for any amenities associated with nearness to the coast. Fig. 2 displays, by the level and type of risk, the distribution of effect sizes. As expected, the distribution from properties located in the 100-year floodplain displays a larger discount, when compared to those properties located in the 500-year floodplain. Fig. 2.2 shows that most, if not all, of the high premiums for the 100-year floodplain estimates in Fig. 2.1 correspond to results from coastal regions.

## 6. Meta-regression Analysis

Our database comprises 50 variables describing the specific attributes of every study. However, it is impossible to incorporate all of

**Table 4**
Meta-regression: the Funnel Asymmetry Test.

| | | Sample size weights | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Variables | Full sample | Full sample | 100 year | 500 year | Inland | Inland 100 year | Coast |
| $1/Se_i$ ($\beta_0$) | − 0.002434 (0.00727) | − 0.0404 (0.0253) | − 0.0507* (0.0271) | 0.00815*** (0.00188) | − 0.0517** (0.0206) | − 0.0633*** (0.0202) | 0.363*** (0.0255) |
| Constant ($\beta_1$) | − 0.707*** (0.212) | 1.093 (1.008) | 1.479 (1.102) | − 0.956*** (0.128) | 0.831 (0.892) | 0.793 (0.938) | − 10.15*** (1.170) |
| t $\alpha$ = 0.10 | [0.000]*** | [0.139] | [0.112] | [0.000]*** | [0.176] | [0.199] | [0.000]*** |
| Observations | 349 | 349 | 256 | 93 | 314 | 226 | 35 |
| R-squared | 0.001 | 0.097 | 0.121 | 0.222 | 0.356 | 0.472 | 0.929 |
| Rmse | 4.109 | 8.284 | 9.705 | 0.835 | 4.747 | 4.870 | 5.002 |

Note: The dependent variable corresponds to the standardised effect size, i.e. the corresponding t-value. Standard errors in parentheses are Huber-White robust. Numbers in square brackets correspond to *p*-values for the one-tailed t-test. *, ** and *** means rejection of the null at the 10%, 5% and 1% level of significance.

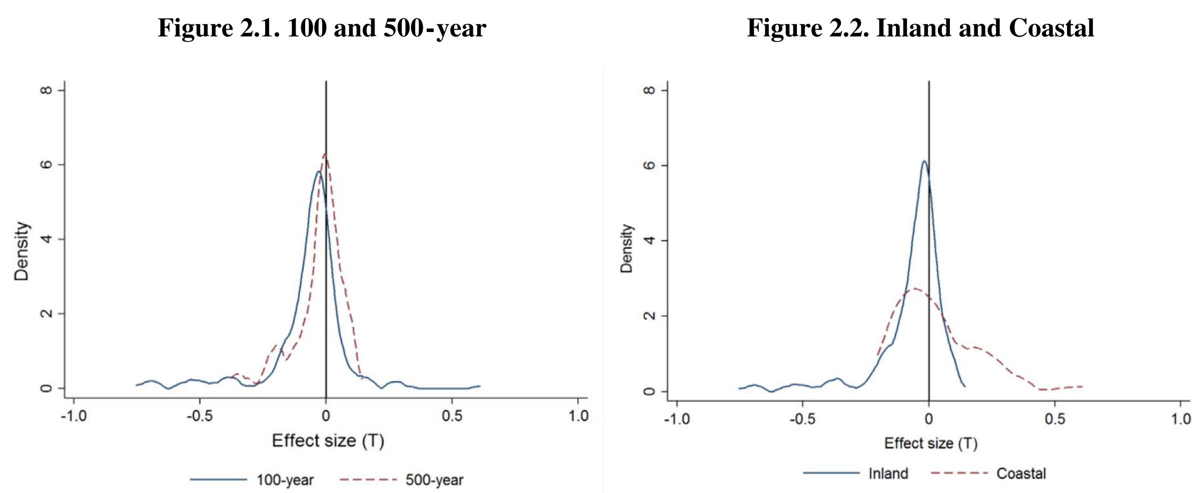


**Fig. 2.** Effect size: distribution density plots for different types of risk and different levels of risk.
Source: Authors' own elaboration based on the results of the primary studies.

them in the meta-regression since the necessary information is not always available. As a consequence, a set of 18 explanatory variables was included in the regression. These are described in Table 5. The first and for us most interesting variable controls for changes in flood risk perception caused by the amount of time elapsed since the last flood. The second group of variables controls for whether the study relates to a 100-year or 500-year floodplain. Following Daniel et al. (2009a) this variable is coded 0.002 for properties that are located in a 500-year floodplain and 0.01 for properties located in a 100-year floodplain. Making the assumption that any change in the objective risk of flooding ceteris paribus results in an equal change in the subjective risk of flooding we thus identify the relationship between the house price discount and the subjective risk of flooding from the inter-study variation in the objective risk of flooding. More specifically the coefficient on this variable can be interpreted as the percentage discount for houses located in the 100-year floodplain.

The third set of moderator variables accounts for context. It includes the log of the mean square footage of houses included in each primary study (*lav_feet*) in order to control for dissimilarities in the kind of houses and the log of the mean price of a house in 2010 USD (*lav-price_2010*) serving as a proxy for the incomes of households and for any income differences across studies. As Carbone et al. (2006) and Hallstrom and Smith (2005) point out, following a flood the price discount is expected to be more substantial because some homeowners are likely to have suffered flood damages; hence we include a dummy variable identifying studies which explicitly state that their estimated coefficient corresponds to properties that have been flooded (*flooded*). Similarly, following Pryce et al. (2011), we anticipate a somewhat higher discount for houses in the floodplain in the aftermath of a second successive flood (*scnd_flood*). Also included is a dummy variable (*dd_afterlaw*) that is used to identify the effect sizes drawn from those studies looking at the price differential for floodplain location following any changes in regulations and last of all a dummy variable (*coast*) is used to identify the effect sizes of studies undertaken in coastal locations.

A fourth set of moderator variables comprises two dummy variables representing the presence of a range of controls in primary studies. The first of these (*amenity*) assumes the value unity for studies that include variables controlling for proximity to water. As already mentioned, if amenity values that correlate with flood risk are omitted we expect resulting estimates of the value of risk to be biased. A second dummy (*real_p*) identifies those studies that explicitly use constant house prices in an attempt to control for time trends.[11] The fifth set of moderator variables refers to econometric differences exhibited by the primary studies. Two dummies (*linear* and *Box-Cox*) account for any differences in the functional form of the HPF; the omitted category corresponds to a semi-log functional form. A further dummy variable (*spatial*) highlights the use of spatial econometrics and the variable *dd_hpm* assumes the value unity for any estimates from DID models, either before or after a flood event.

The final set of moderator variables controls for miscellaneous features of primary studies. In order to account for the quality of each study, we include a dummy (*published*) taking the value unity to distinguish studies published in refereed journals from working papers,

[11] In a small number of instances it was not possible to establish whether the study did indeed use constant house prices.

**Table 5**
Description of variables included in the meta-regression model.
Source: Own elaboration based on estimates from primary studies.

| Variable | Description | Summary statistics | | | | |
|---|---|---|---|---|---|---|
| | | No. obs. | Mean | St. dev. | Min. | Max. |
| *Dependent variable* | | | | | | |
| Effect size (T) | Relative price differential for floodplain location. | 349 | − 0.061 | 0.149 | − 0.755 | 0.610 |
| *Flood risk perception* | | | | | | |
| months | Number of months elapsed since the previous flood. | 349 | 144.4 | 175.7 | 3.0 | 840.0 |
| *Flood risk* | | | | | | |
| Flood risk | Variable = 0.01 if the effect refers to the 100-year floodplain and 0.002 for a 500-year floodplain. | 349 | 0.0079 | 0.0035 | 0.002 | 0.01 |
| *Context of the study* | | | | | | |
| lav_feet | Natural log of the mean square feet of the properties per study. | 349 | 7.425 | 0.249 | 6.558 | 8.051 |
| lavprice_2010 | Natural log of the mean price of the houses per study in 2010 US dollars. | 349 | 11.861 | 0.557 | 9.191 | 12.956 |
| flooded | Dummy = 1, if the effect refers to flooded properties. | 349 | 0.025 | 0.155 | 0 | 1 |
| scnd_flood | Dummy = 1, if the effect refers to a second flood. | 349 | 0.033 | 0.179 | 0 | 1 |
| dd_after | Dummy = 1, if the effect corresponds to a post-flood DID estimate. | 349 | 0.329 | 0.471 | 0 | 1 |
| dd_afterlaw | Dummy = 1, if the effect is from a DID model following a change in a regulation for floodplain designated areas. | 349 | 0.074 | 0.262 | 0 | 1 |
| coast | Dummy = 1, if the study area has a coastline. | 349 | 0.102 | 0.303 | 0 | 1 |
| *Control variables of study* | | | | | | |
| amenity | Dummy = 1, if the study includes variables controlling for the amenity value of proximity to waterbodies. | 349 | 0.876 | 0.330 | 0 | 1 |
| real_p | Dummy = 1, if the study converts prices to a constant measure prior to estimation. | 349 | 0.667 | 0.472 | 0 | 1 |
| *Characteristics of econometric model* | | | | | | |
| linear | Dummy = 1, if the effect corresponds to a linear specification of a hedonic price function. | 349 | 0.071 | 0.258 | 0 | 1 |
| Box-Cox | Dummy = 1, if the study utilises a semi-logarithmic HPF. | 349 | 0.019 | 0.137 | 0 | 1 |
| spatial | Dummy = 1, if the effect corresponds to a spatial econometric model. | 349 | 0.343 | 0.475 | 0 | 1 |
| dd_hpm | Dummy = 1, if the effect corresponds to a DID specification. | 349 | 0.593 | 0.492 | 0 | 1 |
| *Characteristics of the study* | | | | | | |
| published | Dummy = 1, if the primary study is published in a refereed journal. | 349 | 0.580 | 0.494 | 0 | 1 |
| med_sampleyear | The median sample year of the study. | 349 | 1995.1 | 6.464 | 1978 | 2006 |
| time_span | The time span of the data covered in the study. | 349 | 7.173 | 6.416 | 1 | 40 |

dissertations or conference proceedings. The median sample year of each primary study (*med_sampleyear*) is included to identify time trends in the effect size; we also include the time span (*time_span*) of every study. Table 5 includes summary statistics for all these variables.

Recall that in our meta-regression the weight assigned to every observation is proportionate to the square root of the sample size divided by the number of estimates contributed by each study.[12] Lewis and Linzer (2005) show that, when used in conjunction with heteroskedastic consistent standard errors, this technique yields very satisfactory results. We use the Huber-White variance estimator (White, 1980) accounting for both heteroskedasticity and any correlation between effect sizes drawn from the same study (Williams, 2000).

Having addressed the issue of weighting in our meta-regression, we now test the hypothesis that the flood history of the locations matters and whether its inclusion means that there is no longer any statistically significant effect from differences in the objective risk. Following Atreya and Ferreira (2015), Atreya et al. (2013) and Bin and Landry (2013), we investigate four different transformations for the variable *months*: a linear specification ($f$(months) = months), a logarithmic transformation ($f$(months) = ln (months)), a ratio specification ($f$(months) = (months − 1)/months) and a square root specification ($f$(months) = *Sqrt*(months)). These three transformations impose different degrees of curvature in the recovery of house prices following a flood.

[12] The meta-regression presented by Daniel et al. (2009a) uses three different weighting schemes. First they use a random-effects model (mixed-effects) where the weights are as stated above. Secondly, they present an unweighted model using Huber-White standard errors robust to heteroskedasticity and cluster correlation among effect sizes drawn from the same primary study. Third, they present results using inverse-variance weights in a fixed-effect model with Huber-White standard errors. All of these models treat observations as if they were a separate study resulting in the above-mentioned problem of assigning improper weight to studies contributing multiple estimates of the effect size.

The results of our meta-regression are shown in Table 6 where models 1–4 are the results obtained from assigning weights as above with different transformations of the *months* variable. In each of the models the coefficient on the variable *months* and transformations thereof is highly significant. These results indicate that as expected the price discount for floodplain location is much greater immediately following a flood, after which it starts to decay. Based on the goodness of fit criterion our preferred specification is model 3 which uses the ratio transformation of the variable *months*. This is similar to the preferred transformation of the time variable used by Bin and Landry (2013). Our results seem to suggest that results from earlier studies might be influenced as much by changes in subjective risk caused by an actual flood as by objective measures of flood risk.

Turning to the other main variable of interest differences in the objectively assessed risk are significant and highly consistent across the different specifications. In the preferred model 3 it appears that there is almost a − 4.6% discount for being located in the 100-year floodplain compared to locations outside it. A number of other variables appear to have a consistent impact on the effect size including whether the primary study used a linear functional form as well as regional dummies. The coefficients on the regional dummies are always positive and highly significant. These coefficients are measured with respect to Georgia, US, which is the omitted region in our regressions and for which primary studies report the highest discounts in our meta-sample. Specifically, Atreya and Ferreira (2012a, 2012b) and Atreya et al. (2013) analyse the impact of tropical storm Alberto in 1994 using DID hedonic models. This storm is considered to be among the worst flood disasters in the history of the US (The Albany Herald, 2014).

As a further test of robustness we include only those studies which are for inland flooding given our concerns about studies dealing with coastal flooding. In this approach, we follow DeCicca and Kenkel (2015) who suggest excluding from the meta-sample studies where the internal validity of the estimates could be compromised. In this case all

**Table 6**
Meta-regression results.

| Variables | Sample size weights | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | *mnths* | ln(*mnths*) | $\frac{(mnths-1)}{mnths}$ | *Sqrt*(*mnths*) |
| *Flood risk perception* | | | | |
| Mnths | 0.000285*** (5.54e − 05) | 0.0479*** (0.00850) | 1.117*** (0.210) | 0.00937*** (0.00177) |
| *Flood risk level* | | | | |
| Risk level | − 4.748*** (0.665) | − 4.683*** (0.628) | − 4.573*** (0.629) | − 4.748*** (0.639) |
| *Context of the study* | | | | |
| lav_feet | 0.0324 (0.0386) | 0.0571 (0.0366) | 0.0460 (0.0356) | 0.0456 (0.0378) |
| lavprice_2010 | 0.00503 (0.0555) | − 0.0180 (0.0536) | − 0.00506 (0.0533) | − 0.00988 (0.0547) |
| flooded | − 0.0303 (0.0623) | − 0.0103 (0.0644) | − 0.0255 (0.0633) | − 0.0153 (0.0643) |
| scnd_flood | 0.0218 (0.0348) | − 0.0260 (0.0361) | − 0.0623* (0.0355) | 0.00680 (0.0360) |
| dd_after | − 0.0627** (0.0293) | − 0.0338 (0.0280) | − 0.0137 (0.0291) | − 0.0523* (0.0288) |
| dd_afterlaw | 0.0584** (0.0293) | 0.0430* (0.0256) | 0.0192 (0.0239) | 0.0543* (0.0286) |
| coast | 0.0416 (0.0331) | 0.0336 (0.0310) | 0.0306 (0.0310) | 0.0388 (0.0320) |
| *Control variables of study* | | | | |
| amenity | − 0.0112 (0.0235) | − 0.0108 (0.0216) | − 0.00816 (0.0201) | − 0.00913 (0.0227) |
| real_p | 0.0551* (0.0306) | 0.0937*** (0.0311) | 0.0869*** (0.0307) | 0.0732** (0.0306) |
| *Characteristics of econometric model* | | | | |
| linear | − 0.165*** (0.0508) | − 0.172*** (0.0470) | − 0.179*** (0.0465) | − 0.167*** (0.0493) |
| Box-Cox | − 0.0351 (0.0289) | − 0.0387 (0.0274) | − 0.0311 (0.0268) | − 0.0397 (0.0275) |
| spatial | − 0.0232* (0.0125) | − 0.0186 (0.0119) | − 0.0157 (0.0116) | − 0.0215* (0.0123) |
| dd_hpm | 0.0173** (0.00812) | 0.0108 (0.00693) | 0.0105 (0.00666) | 0.0141* (0.00770) |
| *Characteristics of the study* | | | | |
| published | − 0.00860 (0.0189) | − 0.0182 (0.0170) | − 0.0160 (0.0156) | − 0.0130 (0.0184) |
| med_sampleyear | 0.00342* (0.00206) | 0.00221 (0.00204) | − 0.000760 (0.00210) | 0.00351* (0.00208) |
| time_span | 0.00512** (0.00212) | 0.000735 (0.00203) | 0.00130 (0.00184) | 0.00269 (0.00211) |
| *Regional fixed effects*[a] | | | | |
| louisiana | 0.162*** (0.0605) | 0.266*** (0.0669) | 0.168*** (0.0568) | 0.234*** (0.0661) |
| n_carolina | 0.160*** (0.0397) | 0.171*** (0.0388) | 0.106*** (0.0337) | 0.178*** (0.0407) |
| texas | 0.354*** (0.0531) | 0.343*** (0.0501) | 0.266*** (0.0455) | 0.367*** (0.0535) |
| wisconsin | 0.216*** (0.0607) | 0.204*** (0.0606) | 0.131** (0.0532) | 0.226*** (0.0624) |
| alabama | 0.453*** (0.0602) | 0.455*** (0.0563) | 0.371*** (0.0489) | 0.473*** (0.0610) |
| florida | 0.352*** (0.0574) | 0.375*** (0.0551) | 0.326*** (0.0512) | 0.374*** (0.0577) |
| california | 0.252*** (0.0547) | 0.304*** (0.0570) | 0.223*** (0.0503) | 0.292*** (0.0572) |
| missouri | 0.118*** (0.0434) | 0.104*** (0.0382) | 0.0380 (0.0348) | 0.128*** (0.0429) |
| colorado | − 0.00351 (0.0594) | 0.0279 (0.0570) | 0.0367 (0.0572) | 0.00974 (0.0580) |
| minnesota | 0.355*** (0.0637) | 0.373*** (0.0629) | 0.357*** (0.0641) | 0.366*** (0.0627) |
| nl | 0.196*** (0.0623) | 0.251*** (0.0620) | 0.211*** (0.0571) | 0.229*** (0.0628) |
| uk | 0.0937** (0.0460) | 0.171*** (0.0542) | 0.110** (0.0493) | 0.141*** (0.0509) |
| aus | 0.00794 (0.121) | 0.173 (0.122) | 0.105 (0.115) | 0.106 (0.123) |
| nz | 0.0834 (0.0746) | 0.0807 (0.0692) | 0.0402 (0.0697) | 0.0927 (0.0720) |
| Constant | − 7.362* (3.812) | − 5.023 (3.775) | 0.00363 (3.857) | − 7.515* (3.866) |
| Observations | 349 | 349 | 349 | 349 |
| $R^2$ | 0.674 | 0.692 | 0.697 | 0.682 |
| Adj. $R^2$ | 0.640 | 0.660 | 0.665 | 0.649 |
| Rmse | 0.0544 | 0.0529 | 0.0525 | 0.0538 |

The dependent variable is *T* the effect size. Standard errors are in parentheses; for results employing sample size weights they correspond to Huber-White robust standard errors. *, ** and *** means rejection of the null at the 10%, 5% and 1% significance level.
[a] The omitted region is Georgia, US.

35 observations belonging to studies undertaken in coastal regions are dropped. The results are displayed in Table 7. We continue to find strong evidence that the effect size of floodplain location is greatest immediately after a flood but there remains a statistically significant effect of differences in floodplain designation pointing to a discount of − 4.6% for location in a 100-year floodplain. The only difference is that now the effect size depends on average house size too. Even if they are biased and suggest a premium for floodplain location including studies dealing with coastal flooding does not make any difference.

## 7. Sensitivity Analysis

Our findings point to a statistically significant difference in effect size depending on whether properties are located in the 100-year or 500-year floodplain, as well as to the importance of the amount of time elapsed since the last flood event. These findings moreover, are unaffected by the inclusion of studies undertaken in coastal locations, although such studies are subject to publication bias and point to a price premium for floodplain location. In this section we try to understand why our results are almost an order of magnitude different to the ones obtained by Daniel et al. (2009a). We also offer some advice for cost-benefit analysis of flood-relief projects. We begin however with a further test of robustness.

Earlier, we argued that all estimates of the floodplain discount are 'post-flood' estimates and that the only substantive difference is how much time has since elapsed. As a further test of robustness we divide the sample effect sizes into four groups: those derived from standard hedonic studies undertaken during a period in which there was no flooding, those from DID hedonic models (both before and after), the 'pre-flood' ones from DID studies and the 'post-flood' ones from DID studies. We then investigate whether the observed relationships between the floodplain discount, location in the

**Table 7**
Meta-regression results: inland flood risk.

| Variables | Sample size weights | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | *mnths* | ln(*mnths*) | $\frac{(mnths-1)}{mnths}$ | *Sqrt*(*mnths*) |
| *Flood risk perception* | | | | |
| mnths | 0.000315*** (5.34e − 05) | 0.0451*** (0.00843) | 1.066*** (0.213) | 0.00974*** (0.00170) |
| *Flood risk level* | | | | |
| Risk level | − 4.884*** (0.584) | − 4.747*** (0.584) | − 4.604*** (0.590) | − 4.847*** (0.575) |
| *Context of the study* | | | | |
| lav_feet | 0.132*** (0.0312) | 0.150*** (0.0298) | 0.143*** (0.0284) | 0.146*** (0.0309) |
| lavprice_2010 | − 0.0196 (0.0626) | − 0.0367 (0.0616) | − 0.0296 (0.0602) | − 0.0347 (0.0626) |
| flooded | − 0.0133 (0.0584) | − 0.00361 (0.0605) | − 0.0172 (0.0586) | − 0.00112 (0.0606) |
| scnd_flood | 0.00866 (0.0287) | − 0.0379 (0.0295) | − 0.0733** (0.0300) | − 0.00774 (0.0296) |
| dd_after | − 0.0353 (0.0282) | − 0.0135 (0.0286) | 0.00814 (0.0293) | − 0.0265 (0.0283) |
| dd_afterlaw | 0.00741 (0.0271) | 0.000145 (0.0249) | − 0.0239 (0.0222) | 0.00537 (0.0269) |
| coast | – | – | – | – |
| *Control variables of study* | | | | |
| amenity | − 0.00743 (0.0180) | − 0.00698 (0.0175) | − 0.00771 (0.0171) | − 0.00522 (0.0174) |
| real_p | − 0.0880*** (0.0227) | − 0.0424 (0.0272) | − 0.0460 (0.0293) | − 0.0675*** (0.0244) |
| *Characteristics of econometric model* | | | | |
| linear | − 0.175*** (0.0617) | − 0.164*** (0.0576) | − 0.180*** (0.0656) | − 0.172*** (0.0582) |
| Box-Cox | 0.00892 (0.0254) | 0.00262 (0.0283) | 0.00962 (0.0277) | 0.00339 (0.0266) |
| spatial | − 0.00801 (0.00971) | − 0.00536 (0.00986) | − 0.00272 (0.00991) | − 0.00696 (0.00974) |
| dd_hpm | 0.00683 (0.00611) | 0.00245 (0.00595) | 0.00188 (0.00552) | 0.00424 (0.00609) |
| *Characteristics of the study* | | | | |
| published | − 0.0145 (0.0174) | − 0.0240 (0.0158) | − 0.0223 (0.0148) | − 0.0195 (0.0169) |
| med_sampleyear | 0.00135 (0.00198) | 0.000316 (0.00204) | − 0.00265 (0.00210) | 0.00145 (0.00203) |
| time_span | 0.00277 (0.00221) | − 0.000739 (0.00206) | − 0.000401 (0.00187) | 0.000391 (0.00214) |
| *Regional fixed effects*[a] | | | | |
| louisiana | 0.0662 (0.0556) | 0.153*** (0.0580) | 0.0687 (0.0509) | 0.136** (0.0584) |
| n_carolina | 0.116*** (0.0377) | 0.117*** (0.0379) | 0.0580* (0.0347) | 0.130*** (0.0392) |
| texas | 0.206*** (0.0414) | 0.186*** (0.0354) | 0.115*** (0.0414) | 0.213*** (0.0387) |
| wisconsin | 0.281*** (0.0492) | 0.253*** (0.0445) | 0.189*** (0.0419) | 0.287*** (0.0484) |
| alabama | 0.341*** (0.0608) | 0.324*** (0.0522) | 0.255*** (0.0572) | 0.354*** (0.0576) |
| florida | 0.246*** (0.0606) | 0.251*** (0.0510) | 0.215*** (0.0560) | 0.263*** (0.0564) |
| california | 0.167*** (0.0533) | 0.209*** (0.0558) | 0.141*** (0.0504) | 0.205*** (0.0562) |
| missouri | 0.150*** (0.0469) | 0.124*** (0.0413) | 0.0640* (0.0378) | 0.157*** (0.0460) |
| colorado | 0.0692 (0.0660) | 0.0893 (0.0627) | 0.106* (0.0604) | 0.0823 (0.0646) |
| minnesota | 0.246*** (0.0629) | 0.247*** (0.0585) | 0.246*** (0.0671) | 0.251*** (0.0587) |
| nl | 0.0808 (0.0588) | 0.138** (0.0580) | 0.109** (0.0526) | 0.117* (0.0601) |
| uk | 0.0281 (0.0379) | 0.0945** (0.0457) | 0.0426 (0.0430) | 0.0740* (0.0425) |
| aus | – | – | – | – |
| nz | – | – | – | – |
| Constant | − 3.552 (3.584) | − 1.559 (3.719) | 3.531 (3.772) | − 3.727 (3.689) |
| Observations | 314 | 314 | 314 | 314 |
| $R^2$ | 0.602 | 0.607 | 0.614 | 0.607 |
| Adj. $R^2$ | 0.560 | 0.565 | 0.573 | 0.565 |
| Rmse | 0.0465 | 0.0462 | 0.0458 | 0.0462 |

The dependent variable is *T* the effect size. Standard errors in parentheses; for results using sample size weights they correspond to Huber-White robust standard errors. *, ** and *** means rejection of the null hypothesis at the 10%, 5% and 1% significance level.

[a] The omitted region is Georgia, US.

100-year rather than the 500-year floodplain and the amount of time elapsed change. These results appear in Table A2 of the appendix for our preferred specification using the ratio transformation of the *months* variable ($f$(months) = (months − 1)/months).

Analysing separately these observations yields a very similar set of coefficient estimates for the effect of location in the 100-year rather than the 500-year floodplain, as well as the effect of time elapsed for standard hedonic studies and DID estimates. But for the regression analysis using the ‘pre-flood’ subsample as well as for the ‘post-flood’ DID subsample none of the coefficients on time elapsed are statistically significant. This is not however surprising. For ‘pre-flood’ DID estimates the average time elapsed with respect to the preceding flood is 21 years (254 months) which we believe is too long to capture any ongoing recovery of prices. For ‘post-flood’ DID studies by contrast, the sample is typically not long enough to draw inferences about the subsequent recovery in property prices.

We now compare the results of our meta-regression with those from Daniel et al. (2009a). Before doing so however, recall that being within a 100-year floodplain does not mean the probability of flooding therein is 1% per year. The reason is that a floodplain is defined by the probability of flooding only at its boundary. It would be wrong to interpret our results as suggesting that the effect of a 1% increase in the probability of flooding is equal to a − 4.6% reduction in the price of a property. The correct interpretation of our results is that location inside a 100-year floodplain is associated with a discount of − 4.6%.

In Daniel et al. (2009a) the authors regress effect size against an objective measure of risk which, as here, takes the value of 0.01 for location in a 100-year floodplain and 0.002 for location in a 500-year floodplain. The coefficient on this variable is, in their preferred specification, − 0.6. They then use this finding to associate a − 0.6% reduction in house prices with a 0.01 increase in the probability of flooding. But as we have just noted, this finding is more correctly interpreted as indicating that location inside a 100-year floodplain is associated with a − 0.6% discount. Properly interpreted the − 0.6 figure nevertheless sounds too small and is certainly very different from our estimate of − 4.6.

To determine why their estimates are almost an order of magnitude different to ours we compiled the same set of observations from the same research papers used in Daniel et al. (2009a). We also therefore now include those observations from the US Army Corps of Engineers (1998) referring to 50, 20 and 10-year floodplain locations which are based on appraisals. The results appear in Table A3 of the appendix. Despite the fact that our control variables are slightly different to those used by Daniel et al. (2009a) we nonetheless obtain a similar, statistically insignificant estimate of − 0.4% for location in a 100-year floodplain, even when we use our preferred weighting method and include time elapsed since the last flood. But critically, when 37 observations relating to coastal flooding are removed the coefficient associated with the price discount for location in a 100-year floodplain increases to a statistically significant estimate of − 2.7%.

Combining coastal and inland studies in the same analysis is clearly problematical even if a dummy variable is used to identify studies in coastal locations. We observed earlier that studies in areas prone to coastal flooding are subject to significant publication bias and produce estimates pointing to a substantial price premium – a result others have attributed to failure to control for amenities associated with coastal location. Although Daniel et al. (2009a) note the problem of omitted variable bias and test for publication bias critically they do not distinguish between studies undertaken in coastal and inland floodplains. The question remains however, as to why including coastal studies in our meta-regression has almost no effect. The reason is that whereas in Daniel et al. (2009a) coastal studies contribute 41% of the observations in our study they contribute only 10% of the observations – most if not all of the recent research has dealt with inland locations.

It now remains only to recommend which estimate of the floodplain discount should be used in cost-benefit analyses of flood relief projects. In fact there are several candidates. Our meta-analysis of estimates of the floodplain discount produced an estimate of − 5.6%. This estimate, which is based on 226 observations, is for location in the 100-year floodplain and refers exclusively to inland flooding. Another candidate is the − 6.9% discount observed immediately after a flood. Our preferred estimate however is taken from our meta-regression, once more limited to inland flooding and implicitly referring to location inside the 100-year floodplain, points to a − 4.6% discount based on 314 observations.

The chief difference between these estimates is that the third excludes short term impacts from recent floods whereas the former two ignore this source of heterogeneity. The first estimate has the advantage that it explicitly accounts for study heterogeneity whereas the second estimate is immediately after a flood when the consequences of flooding are most salient.

We suggest that the more conservative figure of − 4.6% be used as a rule of thumb for benefits estimation whenever a flood relief project in effect changes the boundaries of the floodplain. For example, if a project in effect removes a property from the 100-year floodplain placing it instead in the 500-year floodplain then the benefit of that project should be $4.6 \times (0.01–0.002) = 0.037$ of the average price of property. Benefits will be greater for properties exposed to a higher level of risk. For instance, properties within the 25-year floodplain that are to be protected by a flood defence designed to prevent all but a 100-year event would enjoy benefits of $4.6 \times (0.04–0.01) = 0.138$ of the average price of property. Obviously this figure does not consider potential positive or negative impacts associated with the defences e.g. visual intrusion or loss of access.

## 8. Conclusions

Economic theory suggests that housing markets provide a suitable means of measuring the benefits of flood risk reduction. Empirical evidence however indicates that properties within a 500-year or 100-year floodplain attract anything between a − 75.5% discount to a + 61.0% premium. This paper contains the results of a meta-analysis looking at the reported price discount for location inside the floodplain. The goal of the analysis is to provide answers to three questions: what is the most defensible point estimate of the price discount for floodplain location, is there any evidence of publication bias and what factors explain the observed variation in effect size?

Our results suggest there are important differences between alternative estimates of the effect-size. Dealing first with inland flooding, discounts for houses within a 100-year floodplain are − 2.9% rising to − 6.9% immediately after a flood. And although there seems to be a premium of + 0.3% for location in 500-year floodplains after a flood properties are discounted by − 5.2%. Such findings appear to confirm the view that recent floods cause homeowners to alter their perceptions of flood risk.

Evidence suggesting that properties exposed to coastal flooding enjoy a premium of + 13.4% seems to be a consequence of the correlation between floodplain location and the unrecorded amenities associated with proximity to the coast. Not only do studies into coastal flooding produce estimates of the percentage price discount with the 'wrong' sign there is also evidence of publication bias. Remarkably the results of the publication bias test suggest the real effect of flood risk in coastal regions after filtering out publication bias is even greater (+ 36.3%) and that there is a bias towards publishing results with smaller estimates. This leads us to agree with other researchers that studies of coastal floodplains are generally unreliable.

That researchers adopting a DID methodology are able to present 'pre-flood' and 'post-flood' estimates of the floodplain discount one tends to forget that all estimates have a prior flood history. We find that including a variable measuring time elapsed has considerable explanatory power. There is a significant recovery in house prices following a flood. But even after controlling for short term impacts objectively determined differences in floodplain designation have an impact equivalent to − 4.6% for location in a 100-year floodplain. Although there are other candidates this is our preferred estimate for use in cost-benefit analysis of projects that effectively change properties' floodplain designation. Such findings might have implications for attempts to conduct studies into the impact of other natural hazards. Here too it seems important to control for prior history of study locations.

Our ability to generalise on the basis of these results is hindered by the limited number of studies from outside the US. Out of 37 studies contained in the meta-sample only 5 are from countries other than the US. Hence it is possible that our conclusions are applicable mainly to the US, and the observed floodplain discount is determined by US flood policies. Even inside the US evidence is limited to only 12 States. More research is therefore required for the purposes of understanding better the impact of flood risk on property in other countries.

Apart from the need to conduct studies outside the US other priorities include the following. First it is clear that location within a floodplain is a poor measure of the probability of any individual property flooding. Second, it is necessary to include superior controls for the amenities associated with nearness to the coast e.g. a view of the sea. Finally, it would be interesting to examine the effect on property prices of major engineering projects that in effect change properties' floodplain designation. A repeat sales study of property prices before the announcement of such works and after their completion would help isolate the benefits of reduced flood risk from other water-related amenity values.

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Table A1
Main characteristics of the studies included in the meta-sample.

| Study ID[a] | Author | Year | Estimation period | Year last flooded[b] | Flood risk (floodplain) | Average sample | Hedonic specification | Functional form of dependent variable | Econometric model | Notes |
|---|---|---|---|---|---|---|---|---|---|---|
| 1 | MacDonald, Murdoch and White | 1987 | Jan 1985–Mar 1985 | 1983 | 100 | 139 | Standard | Box-Cox | OLS | |
| 2 | Skantz and Strickland | 1987 | Jul 1977–Jul 1981 | 1975 | 100 | 176 | Standard/DND | Semi-log | OLS | |
| 3 | Donnelly | 1989 | Jan 1984–Dec 1985 | 1981 | 100 | 334 | Standard | Linear | OLS | |
| 4 | Shilling, Sirmans and Benjamin | 1989 | Dec 1982–Feb 1984 | 1973 | 100 | 114 | Standard | Semi-log | OLS | |
| 5 | Bialszewski and Newsome | 1990 | 1987–1989 | 1983 | 100 | 93 | Standard | Linear | OLS | |
| 6 | MacDonald et al. | 1990 | Jan 1988–Jul 1988 | 1983 | 100 | 183 | Standard | Box-Cox | OLS | |
| 7 | Speyrer and Ragas | 1991 | 1971–1986 | 1969 | 100 | 999 | Standard | Linear/Semi-log | OLS | |
| 8 | US Army Corps of Engineers | 1998 | Apr 1988–Mar 1993 | 1981 | 100 | 344 | Standard | Linear | OLS | 13 flood events during 1974 and 1986, the most destructive in 1981. |
| 9 | Bartosova et al. | 1999 | Jan 1995–Jul 1998 | 1986 | 100 and 500 | 1431 | Standard | Semi-log | OLS | |
| 10 | Harrison, Smersh and Schwartz | 2001 | 1980–1997 | 1964 | 100 | 22,411 | Standard/DND | Linear | OLS | |
| 11 | Shultz and Fridgen | 2001 | Jan 1995–Aug 1998 | 1969 | 100 and 500 | 3783 | Standard | Linear | OLS | |
| 12 | Troy | 2001 | Dec 1996–Jan 2000 | 1996 | 100 | 15,716 | Standard/DND | Semi-log | OLS/WLS | Before and after the implementation of the 1998 California Natural Hazard Disclosure Law |
| 13 | Dei-Tutu and Bin | 2002 | Jan 1998–Jun 2002 | 1996 | 100 | 5122 | Standard | Box-Cox | OLS | |
| 14 | Bin | 2004 | Jul 2000–Jun 2002 | 1999 | 100 | 1397 | Standard | Semi-log | OLS | |
| 15 | Bin and Polasky | 2004 | Jul 1992–Jun 2002 | 1991/1999 | 100 | 8375 | Standard/DND | Semi-log | OLS | Before and after Hurricane Floyd in 1999. |
| 16 | Troy and Romm | 2004 | Dec 1996–Jan 2000 | 1996 | 100 | 21,693 | DND | Semi-log | WLS | Before and after the implementation of the 1998 California Natural Hazard Disclosure Law. |
| 17 | Hallstrom and Smith | 2005 | 1982–2000 | 1960/1992 | 100 | 5212 | DND | Semi-log | OLS | |
| 18 | Bin and Kruse | 2006 | Sept 2000–Sept 2004 | 1995 | 100 and 500 | 2895 | Standard | Semi-log | OLS | |
| 19 | Lamond and Proverbs | 2006 | 2000–2005 | 2000 | 100 | 159 | Standard | Semi-log | OLS | |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 20 | Daniel et al. | 2007 | 1990–2004 | 1926/1993/1995 | 100 | 9505 | Standard/DND | Semi-log | OLS/spatial | Before and after floods along the Meuse River in 1993 and 1995. |
| 21 | Morgan | 2007 | Jan 2000–Feb 2006 | 1998/2004 | 100 | 20,882 | Standard/DND | Semi-log | OLS | Before and after Hurricane Ivan in 2004. Ivan was the 4th Hurricane to hit Florida in 2004. |
| 22 | Bin et al. | 2008a | 1995–2002 | 1991 | 100 | 990 | Standard | Semi-log | OLS/spatial | |
| 23 | Bin et al. | 2008b | Sept 2000–Sept 2004 | 1999 | 100 and 500 | 3106 | Standard | Semi-log | Spatial | |
| 24 | Pope | 2008 | Jan 1995–Sept 1996 | 1989 | 100 and 500 | 9349 | Standard/DND | Semi-log | OLS | Before and after Residential Property Disclosure Act in 1995. |
| 25 | Daniel et al. | 2009b | 1990–2004 | 1926/1993/1995 | 100 | 9505 | Standard/DND | Semi-log | OLS | Before and after floods along the Meuse River in 1993 and 1995. |
| 26 | Kousky | 2010 | 1979–2006 | 1973/1993 | 100 and 500 | 291,831 | Standard/DND | Semi-log | OLS | Before and after flood in 1993 along the Mississippi-Missouri River. |
| 27 | Samarasinghe and Sharp | 2010 | 2006 | 2001 | 100 | 2241 | DND | Semi-log | OLS/spatial | Before and after publication of maps outlining flood-hazard boundaries. |
| 28 | Posey and Rogers | 2010 | 2000 | 1997 | 100 | 69,022 | Standard | Semi-log | OLS/spatial | |
| 29 | Atreya and Ferreira | 2011 | 1985–2010 | 1959/1994 | 100 and 500 | 15,650 | Standard/DND | Semi-log | OLS | Before and after Tropical Storm Alberto in 1994. |
| 30 | Rambaldi et al. | 2013 | 1970–2010 | 1931 | 100 | 3944 | Standard | Semi-log | Spatial | |
| 31 | Atreya and Ferreira | 2012c | 1985–2010 | 1959/1994 | 100 | 3005 | DND | Semi-log | OLS/spatial | Before and after Tropical Storm Alberto in 1994. |
| 32 | Atreya and Ferreira | 2012a | 1985–2010 | 1959/1994 | 100 and 500 | 9958 | DND | Semi-log | OLS/spatial | Before and after Tropical Storm Alberto in 1994. |
| 33 | Atreya, Ferreira and Kriesel | 2012 | 1985–2010 | 1959/1994 | 100 and 500 | 10,348 | Standard/DND | Semi-log | OLS/spatial | Before and after Tropical Storm Alberto in 1994. |
| 34 | Atreya, Ferreira and Kriesel | 2013 | 1985–2004 | 1959/1994 | 100 and 500 | 8042 | DND | Semi-log | Spatial | Before and after Tropical Storm Alberto in 1994. |
| 35 | Bin and Landry | 2013 | 1992–2008 | 1992/1996/1999 | 100 and 500 | 4080 | Standard/DND | Semi-log | Spatial | Before and after Hurricane Fran 1996 and Floyd 1999. |
| 36 | Meldrum | 2013 | 1995–2010 | 1974 | 100 | 25,512 | Standard | Semi-log | OLS/spatial | |
| 37 | Turnbull, Zahirovic and Mothorpe | 2013 | 1984–2005 | 1983 | 100 and 500 | 22,351 | Standard | Semi-log | OLS/spatial | |

[a] Corresponds to the same ID as in Table 2.
[b] For studies using a DND approach before and after a flood the dates correspond to the year of the previous flood for the pre-flood and post-flood sample.

Table A2
Meta-regression results: standard hedonic and DID estimates.

| Variables | Sample size weights | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Standard Hedonic | DID estimates | DID before | DID after |
| *Flood risk perception* | | | | |
| Mnths | 0.830** (0.342) | 0.787*** (0.248) | 1.079 (0.683) | − 1.622 (1.355) |
| *Flood risk level* | | | | |
| Risk level | − 2.770*** (0.979) | − 3.934*** (0.711) | − 2.519** (1.217) | − 5.292*** (0.656) |
| *Context of the study* | | | | |
| lav_feet | 0.164*** (0.0343) | 0.0648 (0.193) | − 0.388 (0.477) | 0.185 (0.153) |
| lavprice_2010 | − 0.0367 (0.0675) | − 0.108 (0.257) | 0.305 (0.540) | − 0.0300 (0.137) |
| Flooded | 0.0217 (0.0306) | − 0.00592 (0.0776) | – | − 0.0114 (0.0647) |
| scnd_flood | – | − 0.0835** (0.0383) | − 0.0667 (0.0560) | 0.0130 (0.0155) |
| dd_after | – | − 0.0622* (0.0341) | – | – |
| dd_afterlaw | – | 0.0473* (0.0256) | – | 0.0514* (0.0281) |
| coast | 0.133*** (0.0408) | – | – | – |
| *Control variables of study* | | | | |
| amenity | 0.00367 (0.0139) | − 0.250 (0.243) | 0.177 (0.536) | 0.782 (0.705) |
| real_p | − 0.0718 (0.0448) | − 0.0427 (0.105) | 0.232 (0.232) | 0.148 (0.160) |
| *Characteristics of econometric model* | | | | |
| linear | − 0.224*** (0.0463) | − 0.408 (0.263) | − 0.000767 (0.529) | 0.734 (0.736) |
| Box-Cox | 0.0291 (0.0393) | – | – | – |
| spatial | − 0.0113 (0.00855) | 0.0108 (0.0263) | − 0.0176 (0.0350) | 0.0169 (0.0271) |
| dd_hpm | – | – | – | – |
| *Characteristics of the study* | | | | |
| published | − 0.0258 (0.0278) | − 0.00520 (0.0181) | − 0.0272* (0.0151) | 0.00778 (0.0165) |
| med_sampleyear | 0.00364 (0.00333) | 0.00332 (0.00246) | − 0.0140*** (0.00513) | 0.0294 (0.0235) |
| time_span | 0.00493** (0.00221) | 0.00391 (0.00272) | − 0.00614 (0.00777) | − 0.0496 (0.0318) |
| *Regional fixed effects*[a] | | | | |
| louisiana | − 0.108 (0.0721) | – | – | – |
| n_carolina | − 0.0673 (0.0543) | 0.139 (0.0888) | 0.128 (0.177) | − 0.359 (0.229) |
| texas | 0.135* (0.0692) | − 0.0889 (0.360) | 0.335 (0.759) | 0.920 (0.907) |
| wisconsin | 0.156*** (0.0458) | – | – | |
| alabama | 0.274*** (0.0470) | – | – | |
| florida | 0.179*** (0.0559) | 0.209 (0.133) | 0.479 (0.294) | 0.526* (0.290) |
| california | 0.0664 (0.0606) | 0.219 (0.196) | 0.0453 (0.403) | − 0.340 (0.260) |
| missouri | 0.00675 (0.0585) | 0.102 (0.0835) | 0.0741 (0.185) | 0.389* (0.216) |
| colorado | − 0.0512 (0.0645) | – | – | |
| minnesota | 0.194*** (0.0505) | – | – | |
| nl | − 0.118* (0.0690) | 0.252 (0.193) | 0.0446 (0.408) | − 0.0280 (0.108) |
| uk | − 0.0640 (0.0729) | – | – | |
| aus | − 0.294** (0.138) | – | – | |
| nz | – | 0.199 (0.358) | − 0.163 (0.744) | − 0.894 (0.612) |
| Constant | − 8.823 (6.466) | − 6.419 (5.452) | 25.80** (10.98) | − 58.48 (47.48) |
| Observations | 133 | 216 | 115 | 101 |
| $R^2$ | 0.769 | 0.748 | 0.690 | 0.916 |
| Adj. $R^2$ | 0.710 | 0.718 | 0.628 | 0.896 |
| Rmse | 0.0430 | 0.0521 | 0.0643 | 0.0257 |

The dependent variable is the effect size *T*. Standard errors in parentheses; for results using sample size weights they correspond to Huber-White robust standard errors. *, ** and *** means rejection of the null hypothesis at the 10%, 5% and 1% confidence level.
[a] The omitted region is Georgia, US.

Table A3

Meta-regression results: only including sample in Daniel et al. (2009).

| Variables | Sample size weights | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Inc. coast (no months) | No coast (no months) | No coast (inc. months) |
| *Flood risk perception* | | | |
| Mnths | | – | 0.601*** (0.152) |
| *Flood risk level* | | | |
| Risk level | − 0.403 (1.541) | − 2.717** (1.174) | − 2.283** (0.972) |
| *Context of the study* | | | |
| lav_feet | − 0.0606 (0.0786) | − 0.0867** (0.0383) | − 0.100** (0.0404) |
| lavprice_2010 | − 0.0408 (0.0698) | 0.0422 (0.0267) | 0.0448* (0.0267) |
| flooded | – | – | – |
| scnd_flood | – | – | – |
| dd_after | − 0.0734* (0.0429) | − 0.0241** (0.00915) | − 0.0488*** (0.0113) |
| dd_afterlaw | 0.0650* (0.0373) | 0.0103** (0.00420) | 0.0418*** (0.00742) |
| coast | 0.0947 (0.0730) | – | – |
| *Control variables of study* | | | |
| amenity | − 0.0444 (0.0280) | 0.166** (0.0815) | 0.0601 (0.0888) |
| real_p | − 0.0307 (0.0403) | 0.0226 (0.0223) | 0.0760*** (0.0175) |
| *Characteristics of econometric model* | | | |
| linear | − 0.129*** (0.0373) | 0.131** (0.0570) | 0.0910 (0.0553) |
| Box-Cox | − 0.0485* (0.0264) | − 0.0454** (0.0170) | − 0.0625*** (0.00834) |
| spatial | − 0.164* (0.0904) | – | – |
| dd_hpm | 0.00952 (0.0109) | − 0.00364 (0.00418) | − 0.00420 (0.00341) |
| *Characteristics of the study* | | | |
| published | − 0.0221 (0.0161) | − 0.0213 (0.0143) | − 0.0316*** (0.00765) |
| med_sampleyear | 0.00140 (0.00481) | − 0.00407 (0.00249) | − 0.00485** (0.00198) |
| time_span | 0.000418 (0.00406) | − 0.00435** (0.00203) | − 0.00545*** (0.00142) |
| *Regional fixed effects*[a] | | | |
| louisiana | − 0.0327 (0.0340) | 0.0660 (0.0489) | 0.00432 (0.0494) |
| n_carolina | 0.0796 (0.0993) | − 0.0130 (0.0555) | 0.0553 (0.0569) |
| texas | 0.0618 (0.0479) | 0.109** (0.0539) | 0.0896** (0.0442) |
| wisconsin | – | − 0.139*** (0.0135) | − 0.137*** (0.0116) |
| alabama | 0.135*** (0.0371) | – | – |
| florida | 0.123* (0.0700) | 0.0450** (0.0207) | 0.0488*** (0.0158) |
| california | 0.0868 (0.114) | − 0.0180 (0.0506) | 0.0645 (0.0505) |
| missouri | – | – | – |
| colorado | – | – | – |
| minnesota | 0.135 (0.0939) | − 0.181* (0.0910) | − 0.0531 (0.0971) |
| nl | – | – | – |
| uk | – | – | – |
| aus | – | – | – |
| nz | – | – | – |
| Constant | − 1.854 (9.993) | 8.164 (5.022) | 9.237** (4.044) |
| Observations | 104 | 77 | 77 |
| $R^2$ | 0.411 | 0.736 | 0.813 |
| Adj. $R^2$ | 0.241 | 0.635 | 0.736 |
| Rmse | 0.0589 | 0.0204 | 0.0173 |

The dependent variable is the effect size $T$. Standard errors in parentheses; for results using sample size weights they correspond to Huber-White robust standard errors. *, ** and *** means rejection of the null hypothesis at the 10%, 5% and 1% confidence level.

[a] The omitted region is Georgia, US.

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