Ecological Economics 69 (2009) 355–365

Contents lists available at ScienceDirect

Ecological Economics

journal homepage: www.elsevier.com/locate/ecolecon

Methods

Flooding risk and housing values: An economic assessment of environmental hazard

Vanessa E. Daniel [a,*], Raymond J.G.M. Florax [a,b], Piet Rietveld [a,c]

[a] Department of Spatial Economics, VU University, Amsterdam, The Netherlands
[b] Department of Agricultural Economics, Purdue University, West Lafayette, USA
[c] Tinbergen Institute, Amsterdam, The Netherlands

ARTICLE INFO

Article history:
Received 23 November 2007
Received in revised form 29 May 2009
Accepted 11 August 2009
Available online 1 October 2009

Keywords:
Valuation
Environmental risk
Meta-analysis
Hedonic pricing

JEL Classification:
D81
Q51
Q54

ABSTRACT

Climate change, the 'boom and bust' cycles of rivers, and altered water resource management practice have caused significant changes in the spatial distribution of the risk of flooding. Hedonic pricing studies, predominantly for the US, have assessed the spatial incidence of risk and the associated implicit price of flood risk. Using these implicit price estimates and their associated standard errors, we perform a meta-analysis and find that an increase in the probability of flood risk of 0.01 in a year is associated to a difference in transaction price of an otherwise similar house of −0.6%. The actual occurrence of a flooding event or increased stringency in disclosure rules causes *ex-ante* prices to differ from *ex-post* prices, but these effects are small. The marginal willingness to pay for reduced risk exposure has increased over time, and it is slightly lower for areas with a higher per capita income. We show that obfuscating amenity effects and risk exposure associated with proximity to water causes systematic bias in the implicit price of flood risk.

1. Introduction

The occurrence and consequences of natural disasters such as floods, windstorms, and heat waves receive increasing media coverage worldwide. This is partly caused by improved technology in communication and broadcasting infrastructure, but it is also a result of a higher incidence of natural disasters, among which floods have been particularly prominent. Sizeable human and material losses are associated with flooding disasters, and this is the reason why this paper focuses on this specific type of natural disaster. The increased incidence of flooding has both natural and anthropogenic causes, which are potentially interrelated. Changing natural circumstances as well as human behavior simultaneously cause climate change, and bring about increases in the frequency and the magnitude of floods. Indeed, there is an increased chance of intense precipitation and flooding due to "greater water-holding capacity of a warmer atmosphere", and it is expected that "such events will continue to become more frequent" (IPCC, 2007, page 783).

Anthropogenic impacts on river flooding are clearly visible in changed river management practices. Construction in floodplains, channel straightening, building of dikes, and construction activity generating impermeable surfaces such as transport infrastructure and residential areas are examples of urbanization that increases the risk of river floods in small catchment areas and small river networks. Land use conversion is also a factor changing the spatial distribution of environmental risk. Particularly in developing countries deforestation for agricultural purposes causes intensified sediment transport rates of rivers and of deposition downstream (Kron, 2003).

The occurrence of these disasters is associated with substantial costs, in the form of human and material losses or disruption of economic activity. Still, the total value of the chance that such hazards effectively happen, effectively including non-material and subjective losses, typically exceeds these actual costs. We are interested in the economic valuation of these environmental risks for at least two reasons.

First, a spatial economic assessment of environmental risk is important in view of decision-making on public and private investments in protective infrastructure to reduce the impact of environmental disasters. Typically, a simple cost-benefit rule guides rational investment behavior of economic actors. van Dantzig (1956, p. 279) already notes that the optimal height of a sea dike is determined by "taking account of the cost of dike-building, of the material losses when a dike-break occurs, and of the frequency distribution of different sea levels." The cost of protective infrastructure comprises outlay for the construction of a dike and the subsequent nuisance it generates, with benefits accruing in terms of avoided human losses, material losses and reconstruction costs, crops losses, and breaks in economic activity.

An appropriate economic assessment also assists in the design and provision of price-efficient insurance policies against environmental

* Corresponding author. Institute for Environmental Studies, VU University Amsterdam, De Boelelaan 1087, 1081HV Amsterdam, The Netherlands. Tel.: +31 20 59 89799.
E-mail address: Vanessa.Daniel@ivm.vu.nl (V.E. Daniel).

0921-8009/$ – see front matter
doi:10.1016/j.ecolecon.2009.08.018

risk. Reliable information regarding actors' willingness to pay for a reduced exposure to the risk of flooding is needed for efficient insurance pricing as well. Unpredictability and damage magnitude make price-setting behavior difficult, in particular given problems of asymmetric information and adverse selection (Akerlof, 1970). Two types of private insurance, the optional system and the package or bundle system, are generally distinguished (Paklina, 2003). The optional variant clearly suffers from adverse selection, because it extends the standard policy to flood damage coverage in return for a mark-up premium. In the package system, flood damage coverage is only available along with other risks, such as fire, earthquakes, and hurricanes.

There are two ways to consider the assessment of the risk of flooding in developed countries. The value of this risk is either the cost of not being affected by the disaster, or the cost of bearing no loss when the disaster effectively occurs. In the empirical literature this value is typically elicited by means of revealed preference techniques, using housing market data. In practice, these studies search for an estimate of the implicit price for self-protection (the price of safety), or the capitalization of insurance premiums (when a market for flood risk insurance does exist) and uncovered damages in the price of the house. The latter includes the nuisance related to (partial) destruction of the house and belongings, and delays of reconstruction. An inventory of available flood risk valuation studies shows willingness to pay (WTP) estimates ranging from −52 to +58% of the average property price associated with a risk exposure of 0.01 per year (see Section 3). The variation in estimates may merely represent sampling or estimation variance, but it could also be caused by systematic variation in the unobserved population value of the willingness to pay. We are particularly interested in explaining the causes of variation in implicit price estimates. Meta-analysis, comprising an array of statistical techniques to analyze previously published empirical estimates, can be used to determine the extent of random versus systematic variation. It is a well-known tool in economics (see Roberts and Stanley (2005), for recent applications), with numerous papers on non-market valuation pertaining to air pollution, recreational fishing, health risks, endangered species, wetlands, and pesticide risk exposure (see, for instance, Smith and Huang (1995), Florax et al. (2005) and Brander et al. (2006)).

The remainder of this paper is organized as follows. The next section deals with the use of valuation techniques for risk of flooding assessment. Section 3 briefly discusses the sampling of studies, and provides the main characteristics of the estimated WTP for reduced flood risk exposure. We also investigate whether the sample drawn from the literature is homogeneous in terms of the underlying population value, and whether publication bias has a distorting effect on the sample. In Section 4, we provide an overview of factors that are potentially relevant in explaining structural variation of flood risk valuations, and we present the estimation results for the meta-regression analysis. Section 5 concludes.

2. Valuation, amenities and perception bias

Stated as well as revealed preference methods have been used to assess flood risks, with either method having its own advantages and disadvantages (see Freeman (2003), for an overview). Stated preference methods are based on interviews or surveys explicitly asking individuals about their willingness to pay for reduced flood risk exposure, using contingent valuation or choice experiments (e.g. conjoint analysis or contingent ranking). Arguably, the major disadvantage of stated preference methods is that it remains unclear whether the actual behavior of respondents corresponds to their self-reported potential behavior. List and Gallet (2001) show that, especially in risk assessment valuation, the impact of the so-called hypothetical bias is most likely strong.

The revealed preference method is concerned with actual consumer behavior in markets. The restriction to actual behavior obviously limits the method's ability to assess WTP values in different (real-world) constellations, and one cannot readily control the information shaping the risk perception of individuals. de Blaeij et al. (2000) and Florax et al. (2005) are examples of studies dealing with the valuation of risk. They both show that revealed preference techniques lead to significantly lower WTP values than stated preference techniques.

Most of the studies assessing the value of flood risk exposure use the revealed preference approach. The assumption underlying revealed preference studies in the presence of an environmental risk is that an exogenously determined (set of) risk(s) is considered when choosing the location of a house. House prices then reveal individual preferences regarding the acceptance of risk, assuming that appropriate controls for differences in the property and the location are included. A straightforward technique to assess such differences is to look at the average difference between prices of houses located inside and outside a specific flood risk zone, and to use a statistical test to assess the significance of the observed difference. Zimmerman (1979) and Shrubsole et al. (1997) use the difference in means approach.

A more elaborate technique derives from Rosen (1974) seminal paper, in which a housing unit is considered as a differentiated market good representing a bundle of quantitative and qualitative characteristics. Implicit shadow prices can be determined as the partial first derivatives of an econometric model that relates the observed selling price of a house to a set of characteristic features of the house, and the neighborhood or location of the house. It is important to note that p is the equilibrium price on the housing market, and variables describing the process of equilibrium price formation should not be part of the hedonic price function.[1] A subset of the neighborhood or location characteristics can be concerned with environmental aspects, such as the risk of natural hazards, or air quality (see, e.g. Kim et al., 2003). Location choices hence include the choice of consuming a particular level of (dis)amenity. This technique has the advantage of being able to control for every element that potentially affects house prices. Yet, in the context of flood risk valuation, two difficulties remain. One is the potential bias in subjective individual perceptions of the level of risk, especially because in hedonic pricing models, as compared with stated preference studies, no additional information or explanation is provided to consumers. Another problem relates to the coincidence of water-related amenities and water-related risks.

Perception bias means the divergence between the objective probability of a given risk and an individual's perception of the risk. A proper appraisal of objective hazards, determined on the basis of recurrent patterns, can interfere with individual personal characteristics and subsequently give rise to biases in the perception of hazards. Specifically, an individual may be completely blind to a risk, in which case revealed preference techniques would elicit insignificant WTP values. Alternatively, individuals may perceive reality through a distorting mirror; in which case revealed WTP values are over- or underestimated (Viscusi, 1991). Two key propositions in expected utility theory and in prospect theory state that individuals overestimate low probability events, especially if fears are present. On the other hand, individuals also underestimate risks over which they have active control (Kahneman and Tversky, 1979; Viscusi and Zeckhauser, 1996). A way to at least partly identify differences between objective and subjective probabilities of risk is to compare house prices before and after the event. New information that can potentially affect subjective probabilities includes the occurrence of the event at risk and the individuals' experience with such an event, a concurrent change in insurance premiums, a change in disclosure rules concerning a specific risk, and increased visibility of the risk due, for instance, to increased media coverage. An illustration of the

[1] Some studies include the number of days on the market as a conditioning variable in the hedonic price function, although this does not seem appropriate. Such a variable either reflects the accuracy of the asking price versus the actual market price, or it reveals an unexplained selling difficulty specific to a house.

overestimation of the effects of low probability events is provided in Beron et al. (1997), who show that the decline in house prices due to location in an earthquake zone drops from 4.0% before the Loma–Prieta earthquake in 1989, to 3.4% after the quake.

Scarcity of information is also relevant with respect to the second complication, the confounding of positive and negative amenities related to proximity to the water. Exceptional rainfall can cause flood risk, but this risk is likely to be independent of rainfall in regions endowed with rivers, canals, or lake watersheds located nearby the coast or at low elevations. In those cases, the presence of water is associated with both positive (e.g. visual amenities, water sports facilities, and open space) and negative spatial amenities (hazard of flooding). As a result, a simple dummy variable signaling location within or outside a floodplain may effectively underestimate the value of the risk of river flooding, because positive and negative water-related amenities are not separately identified, and may hence partly cancel out in the capitalization of amenities in house prices. Advanced computational techniques and the use of geographic information systems have improved the extent to which researchers can account for the spatial organization of the data in terms of distance to the water front and elevation. It is also expected that amenities and risk do not exactly coincide. For instance, houses on hillsides with a direct view towards a river may have no canceling valuations for flood risk and amenities, whereas for houses with a view but at a lower elevation the valuations may cancel. This problem is addressed in the meta-analysis by controlling for the inclusion of distance and elevation related variables in the primary studies.

3. Empirical valuation of flood risk

The availability of empirical studies dealing with the valuation of flood risk is still rather limited and geographically confined to the United States, where flood insurance is not compulsory, although it can condition access to federal loans. This section deals with the selection criteria for primary studies, and presents an exploratory overview of the available empirical value assessments of the risk of flooding. The relevance of publication bias and heterogeneity is investigated statistically as well.

The selection of studies to be included in the meta-sample is governed by two desiderata, which may be at odds with each other. On the one hand, an all-inclusive approach contributes to avoiding the distorting effect of selection and publication bias, and increases the efficiency of estimation. On the other, a desire to save degrees of freedom to increase efficiency requires a sufficiently homogeneous data set in order to limit the number of control variables necessary to identify the relevance of observable differences between studies. In the sample selection process we try to strike the middle ground between the abovementioned desires by having the following requirements at the study (or observation) level:

(i) the assessed price of flood risk is determined by means of a revealed preference technique (either the difference in means estimator or a hedonic price model), and can be presented, eventually after recalculation, as a percentage of the average price of the house;
(ii) the risk of flooding is captured by a dummy, where the dummy refers to the expected occurrence of flooding;[2] and
(iii) the implicit price of a given risk of flooding is not a replication of a previously obtained result in another study included in our database.

The above restrictions lead to the exclusion of specific studies from the pool of available studies. For instance, requirement (i) impedes the inclusion of studies presenting a dollar-valued change in price due to location in a floodplain without available information on the average house price (Thompson and Stoenever, 1983; Holway and Burby, 1990). Requirement (ii) is even more restrictive and leads to the exclusion of studies using elevation and flood depth as control variables (Barnard, 1978; Tobin and Montz, 1994; Kriesel and Friedman, 2002; Zhai et al., 2003).

The initial sample contained two studies reporting a single estimate obtained with the difference in means estimators (Zimmerman, 1979; Shrubsole et al., 1997). These results are excluded, because their inclusion implies the need to introduce two additional control variables revealing the use of the difference in means estimator and location in Canada (as opposed to the US). The Shabman and Damianos (1976) study was excluded, because it only assesses land prices. The final database is made up of 19 studies and 117 point estimates. Fig. 1 illustrates the geographical location of the study areas in the different studies, and Table 1 provides an overview of the most salient features of the studies included in the sample.

Because all studies pertain to a single country, it is important at this point to briefly discuss the institutional context with regard to flood risk in the United States. Since 1968, it has been possible to subscribe to flood insurance via the National Flood Insurance Program (NFIP). This federally-backed insurance is available to home owners residing in communities which participate in flood reduction program (Wright, 2000). New residents would have to pay actuarially based premiums, while current residents would pay federally-subsidized premiums. In 1973, incentives were strengthened in order to increase the participation of local communities, and flood insurance became compulsory to require federally-backed loans. Still, participation remains highly related to both income levels and risk perception (Browne and Hoyt, 2000).

All studies use the actual selling price of the house as the dependent variable, except for 14 observations (US Army Corps of Engineers, 1998) which are based on appraised values.[3] The operationalization of the risk variable differs across studies. Most studies define the risk of flooding as the presence in an x-year floodplain, which means that the probability of flooding amounts to $1/x$ per year. Donnelly (1989), however, defines the risk variable as the product between the usual flood dummy and the property's tax liability. The coefficient associated with this risk variable represents the difference in selling price due to location inside or outside the floodplain, per dollar of property tax liability. The reported change in price is then computed for the average property tax. Almost all studies use the 0.01 flood zone contour to define the risk level.

A typical approach to account for subjectivity in probability assessment is to use pre- and post-event valuations. Table 1 shows that many studies use a specific flood or storm, but some studies consider changes in the design of insurance regulations, such as the National Flood Insurance Reform Act of 1994 or the California Natural Hazard Disclosure Law. Most studies contribute up to 10 observations to the meta-sample, except for the study by the US Army Corps of Engineers (1998) contributing as many as 26, and the paper by Pope (2006) comprising 22 estimates. Both the time span and the number of observations in the primary studies vary widely. Two studies employ spatial econometric techniques. Bin et al. (2008a) uses a spatial lag model and Bin et al. (2008b) a spatial error model allowing for spatial spillovers in the dependent variable and the error terms, respectively.[4]

[2] Most studies employ the implicit price of the location of a house within a 100-year floodplain contour, implying on average a minimum chance of being flooded of 0.01 per year.

[3] We have performed a robustness check in which we omitted the appraised values, but generally found results similar to those reported in Table 3.

[4] There is a subtlety in determining the effect size variable for the Bin et al. (2008a) study, because the marginal effect in a spatial lag model should account for spatial spillovers. We have incorporated this spatial multiplier effect and determined the associated standard error for the effect including the spillovers using the Delta method. For the spatial error model used in Bin et al. (2008b) this correction procedure is not necessary. Details are available upon request.

Fig. 1. The geographical location of the case study areas in the different studies.

Table 1 also presents the price difference associated with location in a flood zone, which is defined as the estimated relative difference in the price of a house associated with location in a specific zone at risk, due to this specific risk. These results are, however, not directly comparable because the expression of the dependent variable in the meta-analysis, which we refer to as the effect size T, as well as its associated standard error s_T depends on the functional form of the hedonic price function in the primary study. Most studies use a semi-loglinear specification, for which the effect size T simply equals a, and s_T equals s_a, where a and s_a refer to the coefficient and standard error estimates for the dummy variable indicating location in the floodplain in the primary study. In the case of a linear specification $T = a/\bar{P}$ and $s_T = s_a/\bar{P}$, where $\bar{P}$ is the sample mean of the selling price. For a Box–Cox specification $T = a\hat{\bar{P}}^{1-l}$, where $\hat{\bar{P}}$ is the mean estimated selling price and l the estimated non-linearity parameter, and the standard errors depend on the specification of the Box–Cox model and can only be approximated by rather involved Delta method approximations.[5] Because the risk levels are different from 0.01 per year in some of the studies, standardizing the effect size T with the risk probability enhances comparability. The standardized effect size T^* equals $T \times (\text{risk probability} \times 100)^{-1}$, obviously with appropriately rescaled standard errors.

For the 117 point estimates of the meta-sample, the standardized relative change in house price due to location in the 100-year flood plain ranges between −52% and +58%, but on average it equals only slightly more than −2%. Table 1 shows the within-study variation, and Fig. 2 shows the point estimates and their associated 95% confidence interval. The figure illustrates that almost 70% of the available meta-observations is negative, and in absolute value the estimated price differentials are as a rule smaller than 20%. It is also apparent that greater standard errors are predominantly associated with higher price differential estimates. The latter is an indication of publication bias, although in general one should be cautious interpreting these results because differences in pre- and post-event assessments, as well as several other characteristics of the studies, are not accounted for.

The potentially obfuscating influence of publication bias has recently received considerable attention in the economic meta-analysis literature (see, e.g. Card and Krueger, 1995; Florax, 2002; Stanley, 2005). The underlying line of reasoning is that published study results may not be an adequate representation of the population of all possible study results because of selection effects. This selection effect is usually referred to as 'publication bias' and includes the effects of self-censoring of authors with respect to undesirable or implausible results ('file drawer problem'), and the possible tendency of journal reviewers and editors to be favorably disposed towards the publication of statistically significant and "positive" results. Various statistical tools and tests have been suggested to identify the occurrence of publication bias (see Roberts and Stanley, 2005; Rothstein et al., 2005). We use the so-called funnel plot and provide the results of two statistical tests for publication bias.

The funnel plot in Fig. 3 depicts the effect size against its associated standard error, and derives its name from the statistical expectation that the plot should have a funnel-like shape. The plot is hypothesized to show a distribution of estimated effect sizes centered on the true underlying population effect size, which is approximated as the inverse-variance weighted average of the effect sizes.[6] Typically, the latter is quite close to the estimate from the largest study in terms of number of observations (or correspondingly the smallest standard error). Smaller studies are less precise and will therefore show a greater dispersion, which results in a (inverse) funnel-like shape. Fig. 3 shows the funnel plot for the standardized effect size T^*. It is by and large symmetric, but it has substantially more observations outside the 95% contours than could be expected on the basis of chance. The funnel plot does not provide a strong indication for publication bias, although potentially publication bias is likely not entirely absent. This conclusion is reinforced by two statistical test results for publication bias. The adjusted rank correlation test (Begg and Mazumdar, 1994) uses the association between the standardized effect size and the sampling variance, measured by Kendall's τ, to detect publication bias.[7] The test shows a z-value of 1.72 ($p = 0.09$) for the meta-sample. Egger et al. (1997) present a regression asymmetry test to investigate the asymmetry of the funnel plot by determining whether the intercept deviates significantly from zero in a regression of the standardized effect estimate against its variance. The estimated constant for the meta-sample is 0.74, with a p-value of 0.10.[8]

The interpretation of the above tests for publication bias is hampered because the underlying population effect size may not be homogeneous. Publication bias may be mistaken for, or disguise, observable (e.g. differences in study design) and unobserved heterogeneity among the effect sizes. Homogeneity of the effect sizes implies that variation in the estimates is random and solely caused by sampling. On the other hand, in the case of heterogeneity, variation in the estimated effect sizes is caused by both sampling and real, observable or unobservable, differences between studies. This is investigated by testing the null hypothesis that the underlying population effect sizes θ_i ($i = 1, 2, \ldots, k$) are the same across studies,

[5] Full details and mathematical derivations for the different effect size definitions are given in the Appendix.

[6] In the meta-analysis literature this is known as the (pooled) fixed effects estimate.

[7] Macaskill et al. (2001) show, however, that the test is not very powerful.

[8] These results pertain to the standardized effect size T^*. In the meta-regression we use T rather than T^* as the dependent variable and include the level of risk as an explanatory variable. The adjusted rank correlation test and the Egger test for T are less indicative of publication bias. The results are 0.18 with a p-value of 0.86, and 0.64 with a p-value of 0.20, respectively.

Table 1
Studies included in the meta-sample with salient characteristics.

Study	Location (US state)	No. estimates	Flood risk (x-year floodplain)	Mean standardized effect size T^*	Standard deviation of T^*	Time span (years)	Context
Bartosova et al. (1999)	WI	7	500 to 100	0.0253	0.245	3.5	Flood in 1997
Bialaszewski and Newsome (1990)	AL	1	100	0.0002	NA	1	
Bin and Polasky (2004)	NC	3	100	−0.0597	0.023	10	Flood in 1999
Bin and Kruse (2006)	NC	6	500 to 100	−0.1738	0.226	4	
Bin et al. (2008a)	NC	1	100	−0.1281	NA	8	
Bin et al. (2008b)	NC	4	500 to 100	−0.1895	0.172	4	
Dei Tutu and Bin (2002)	NC	3	100	−0.0759	0.029	4.5	Flood in 1999
Donnelly (1989)	WI	1	100	−0.1206	NA	2	
Fridgen and Shultz (1999)	ND,MI	4	500 to 100	0.0289	0.144	3.6	
Hallstrom and Smith (2005)	FL	9	100	0.0719	0.095	21	Hurricane in 1992, change in insurance disclosure in 1994
Harrison et al. (2001)	FL	4	100	−0.0222	0.012	18	Reform in NFIP
MacDonald et al. (1987)	LA	6	100	−0.0921	0.017	0.25	Flood in 1982
MacDonald et al. (1990)	LA	6	100	−0.0923	0.020	0.5	Flood in 1978 and 1983
Pope (2006)	NC	22	500 to 100	0.0094	0.102	1.5	
Shilling et al. (1989)	LA	1	100	−0.0761	NA	1.2	
Skantz and Strickland (1987)	TX	7	100	−0.0267	0.020	4	Flood in 1979
Speyrer and Ragas (1991)	LA	4	100	−0.0983	0.073	16	Flood in 1978, 1980, 1983, change in insurance disclosure in 1998
Troy and Romm (2004)	CA	2	100	−0.0968	0.151	3	
US Army Corps of Engineers (1998)	TX,KY	26	500 to 5	0.0134	0.055	1 to 5	

H_0: $\theta = \theta_1 = \ldots = \theta_k$, using the Q-test, which reads as (Sutton et al., 2000):[9]

$$Q = \sum_{i=1}^{k} w_i (T_i^* - \overline{T}_\cdot^*)^2 \text{ with } \overline{T}_\cdot^* = \frac{\sum_i w_i T_i^*}{\sum_i w_i}, \tag{1}$$

where T_i^* is the i-th standardized effect size estimate and w_i the reciprocal of its variance. The Q-test has a χ^2-value with 116 degrees of freedom of 798.31, implying that it is highly significant (for the unstandardized effect size T_i the test value equals 766.21). In the next section, the identified heterogeneity is investigated through a meta-regression analysis.

4. Meta-regression results

The objective of the meta-regression is to determine the impact of observable differences between studies on the magnitude of the estimated relative selling price due to the risk of flooding, which account for non-observable differences between estimates from different studies. The latter group of non-observable differences also includes observable but unmodeled differences in order to arrive at a reasonably parsimonious specification. If one accounts for all observable differences,[10] the degree of multicollinearity becomes prohibitive, and a limited group of conditioning variables is therefore selected. Table 2 provides the descriptive statistics.

Table 2 shows that, apart from the usual constant, 18 conditioning or moderator variables are included in the meta-regression specification. The dependent variable in the meta-regression is the unstandardized effect size estimate T, and the right-hand side of the equation therefore contains an appropriate control for differences in the risk level. Two dummy variables signal the difference between *ex-ante* and *ex-post* valuations, and are intended to at least partially capture differences in the subjective and objective perceptions of risk. The turning point is marked by a notable change in the available information, either because of a flood occurrence or because of increased stringency in disclosure rules. Since valuations can be expected to be income dependent (see de Blaeij et al., 2000), per capita personal income at the county level in current US dollars is included as a rough proxy for personal income of respondents and to control for income differences across studies.

The next group of moderator variables identifies the space–time characteristics of the primary studies. Specifically, a distinction is made between study areas pertaining to a coastal zone or an inland

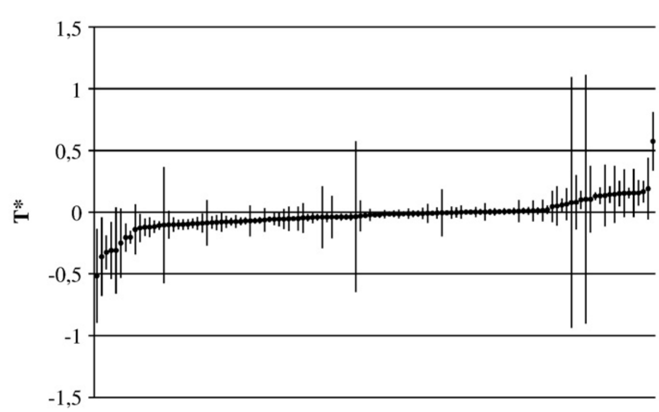

Fig. 2. Standardized effect size T^* including their 95% confidence interval ranked in increasing magnitude with deciles of the meta-sample size on the horizontal axis.

[9] The Q-test is designed for a single-sampling situation (where each study provides one effect size estimate), whereas in the current case frequently more than one estimate is provided by a study (see Table 1). As a result, effect sizes coming from the same study are likely not independent, which violates the distributional assumptions for the Q-test. This is, however, typically ignored and hierarchical dependence, within and between studies, has only recently been investigated in more detail in Gurevitch and Hedges (1999) for instance.

[10] In the initial database 40 observable differences across studies and estimates were coded. Some of these are not very informative, because they identify a specific observation (use of weighted least squares for instance) or they hardly vary over the entire data set (use of the surface as a covariate in the primary hedonic price model). In other cases there is a high degree of collinearity with other conditioning variables (e.g., use of level of pollution, tax level and insurance premium as conditioning variables in the primary study, assessed rather than actual selling price as the dependent variable, and delay on the market before being sold). This obviously complicates the interpretation of the results, but can only be avoided when a larger number of primary studies become available.

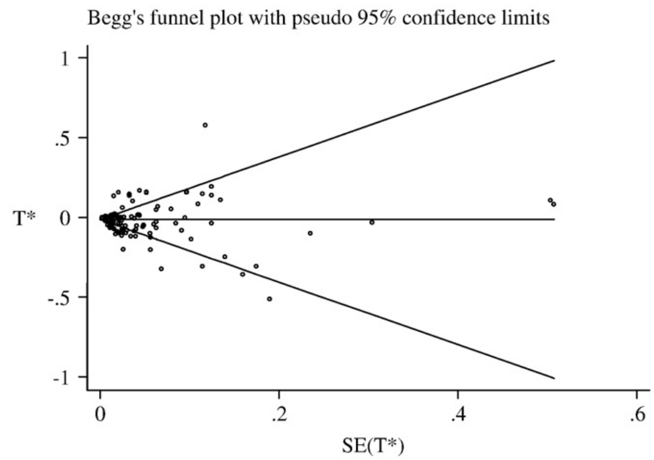

Fig. 3. Funnel plot of 117 standardized house price differentials due to location in a floodplain (T^*) against their estimated standard error, including the pooled fixed effects estimate (solid line) and its 95% confidence interval (dashed lines).

location, because coastal zones are more prone to floods caused by hurricanes and strong winds as compared with inland plains. A variable defined as the median sample year of the primary study is used to identify a systematic time trend in the estimated risk assessments, and the time span of the data covered in the primary studies is included as well.

The third group of moderator variables refers to differences in the study design of the primary studies. Specifically, we associate such differences with the type of data, the functional form of the valuation model, and the estimator used in the primary studies. In particular, one- and two-way panel data models are distinguished from cross-section data as the omitted category, and dummy variables referring to a Box–Cox or semi-log functional form for the valuation model distinguish non-linear models from the linear model, which is the omitted category. The fourth group of moderator variables identifies characteristics of the data and the estimator. A variable indicating that the effect size corresponds to a house price that is different from the average house price is used, and takes on the value of $+1$ or -1 depending on whether the selling price is higher or lower than the average selling price. The last variable in this group refers to the use of adjusted standard errors in the primary study, and identifies the use of heteroskedasticity-robust standard errors or the use of a model allowing for spatially autocorrelated error terms.

A fifth and final set of moderator variables concerns dummy variables that signal the inclusion of specific control variables in the primary study, in particular covariates related to comfort, neighborhood, quality, and finance. Finally, two dummy variables are used to reveal whether a primary study accounts for water-related amenities identifying either proximity to water and/or scenic views associated with water proximity. These two dummies identify the impact of controlling for the potential confounding of water-related amenities and flood risk in the valuation process.

Meta-regressions in economics have been estimated using a variety of estimators ranging from simple ordinary least squares (OLS) to generalized least squares with various alternative weighting procedures (e.g. fixed and mixed effects models, and the robust Huber–White approach) as well as hierarchical level models. These estimators have their own respective pros and cons (see also Abreu et al., 2005). OLS is obviously inefficient, because it discards the information on the estimated standard errors that can be taken from the primary studies, and disregards the autocorrelation that may result from sampling multiple estimates from the same primary study. Heteroskedasticity caused by unequal variances is taken into account in the fixed effects estimator, which is essentially weighted least squares using the inverse standard errors of the primary studies as weights. The fixed effect model is rather restrictive in the sense that it assumes the population effect size to be a fixed unknown constant that can be fully explained by observable differences between studies. This is a rather heroic assumption if the underlying studies are heterogeneous and differences across studies are only partly observable.[11] Instead of assuming a fixed population effect size, the mixed effect estimator rests on the assumption that the population effect size is drawn from a normal distribution centered on the "true" population effect size, with an unknown variance to be determined from the data. The heterogeneity in effect sizes is partly observable and can be specified as so-called moderator or conditioning variables in the meta-regression, and to the extent that it is not observable, it is accounted for in the additional random effect. This well-known estimator that is widely used in medical applications of meta-analysis (Sutton et al., 2000) is based on the following model:

$$
\begin{aligned}
T_i &= \theta_i + \varepsilon_i \text{ where } \varepsilon_i \sim N(0, \sigma_i^2) \\
\theta_i &= \alpha + x_i'\beta + \mu_i \text{ where } \mu_i \sim N(0, \tau_i^2)
\end{aligned}
\tag{2}
$$

where T_i is the estimate of the underlying population effect size θ_i of study i, α is a common factor, and x_i contains a set of design and data characteristics. Deviations of the estimated effect size T_i from the true effect size θ_i are random, and the true effect size and the precision of the estimated effect size σ_i^2 vary across studies. The term σ_i^2 is known as the

Table 2
Descriptive statistics for selected variables ($n = 117$).

Variables	Mean	St. dev.	Min.	Max.
Effect size				
T	−0.023	0.072	−0.268	0.166
T^*	−0.026	0.122	−0.515	0.575
Risk and income				
Risk level (occurrence per year)	0.018	0.032	0.002	0.2
Ex-post flood or disclosure	0.231	0.423	0	1
Ex-ante flood or disclosure	0.239	0.429	0	1
Per capita personal income (USD), county level	20.369	6.812	5.590	28.374
Space–time features				
Coastal zone	0.410	0.494	0	1
Median sample year	1993	6.612	1979	2003
Time span (in years)	5.248	6.212	0	21
Primary study design				
One- or two-way panel data models	0.385	0.489	0	1
Box–Cox	0.128	0.336	0	1
Semi-log	0.547	0.500	0	1
Non average price	0.000	0.294	−1	1
Adjusted standard errors	0.342	0.476	0	1
Conditioning variables primary study				
Proximity to water	0.496	0.502	0	1
Scenic view	0.068	0.254	0	1
Comfort	0.624	0.486	0	1
Neighborhood	0.521	0.502	0	1
Quality	0.812	0.392	0	1
Finance	0.103	0.305	0	1

[11] In meta-analysis the fixed effect estimator typically pertains to the situation where the variation in estimated effect sizes is fully attributable to a limited number of observable differences between studies. In that case the estimator is equivalent to the mean of the inverse-variance weighted estimated effect sizes. This is equivalent to using weighted least squares (WLS) with appropriately defined weights. Since a typical (economic) model would not assume that differences are perfectly explainable by the observable factors, the variance reported for WLS and the fixed effect estimator are not identical. The WLS-estimated standard errors need to be rescaled by the square root of the residual variance (see Abreu et al. (2005) for more details).

Table 3
Estimation results for the mixed effects estimator and the weighted and unweighted Huber–White estimator with the effect size T as the dependent variable.[a]

Variables and diagnostics	Mixed effects		Huber–White, unweighted		Huber–White, inverse-variance weighted	
	(a)	(b)	(a)	(b)	(a)	(b)
Constant	20.679***	23.860***	21.888***	21.088***	22.677***	26.287***
	(6.433)	(3.674)	(6.173)	(2.378)	(3.650)	(2.019)
Risk and income						
Risk level (occurrence per year)	−0.629**	−0.637**	−0.304***	−0.290***	−0.831*	−0.873*
	(0.330)	(0.327)	(0.029)	(0.038)	(0.330)	(0.330)
Ex-post flood or disclosure	−0.022*	−0.024**	−0.034**	−0.028**	−0.020***	−0.021***
	(0.012)	(0.011)	(0.014)	(0.013)	(0.005)	(0.003)
Ex-ante flood or disclosure	0.021*	0.020*	0.024*	0.029**	0.012*	0.013***
	(0.012)	(0.011)	(0.014)	(0.012)	(0.006)	(0.005)
Per capita income (in thousands)	0.020***	0.022***	0.020***	0.019***	0.022***	0.024***
	(0.004)	(0.003)	(0.004)	(0.001)	(0.003)	(0.001)
Space–time features						
Coastal zone	0.112***	0.102***	0.072	0.094***	0.092***	0.089***
	(0.027)	(0.019)	(0.058)	(0.026)	(0.015)	(0.010)
Median sample year	−0.011***	−0.012***	−0.011***	−0.011***	−0.012***	−0.013***
	(0.003)	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)
Time span (in years)	0.010***	0.011***	0.008***	0.008***	0.011***	0.011***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Primary study design						
One- or two-way panel data	−0.052***	−0.054***	−0.042***	−0.037***	−0.062***	−0.064***
	(0.016)	(0.015)	(0.010)	(0.008)	(0.010)	(0.008)
Box–Cox	−0.021		0.021		−0.010	
	(0.036)		(0.041)		(0.024)	
Semi-log	−0.001		0.021		0.008	
	(0.020)		(0.037)		(0.013)	
Non average price	0.018		0.017***	0.017***	0.018***	0.017***
	(0.011)		(0.003)	(0.003)	(0.004)	(0.004)
Adjusted standard errors	−0.038**	−0.032***	−0.015	−0.022*	−0.048***	−0.042***
	(0.015)	(0.012)	(0.014)	(0.012)	(0.012)	(0.007)
Conditioning variables primary study						
Proximity to water	−0.150***	−0.143***	−0.141***	−0.144***	−0.137***	−0.128***
	(0.027)	(0.023)	(0.026)	(0.023)	(0.014)	(0.012)
Scenic view	0.041	0.047*	−0.013		0.065**	0.079***
	(0.031)	(0.027)	(0.045)		(0.026)	(0.024)
Comfort	−0.031	−0.044***	−0.039**	−0.033***	−0.037*	−0.053***
	(0.027)	(0.016)	(0.016)	(0.009)	(0.019)	(0.010)
Neighborhood	0.006		−0.014		0.007	
	(0.015)		(0.017)		(0.008)	
Quality	−0.087***	−0.084***	−0.043	−0.054***	−0.090***	−0.092***
	(0.025)	(0.024)	(0.030)	(0.016)	(0.014)	(0.013)
Finance	0.142***	0.141***	0.096**	0.109***	0.156***	0.155***
	(0.028)	(0.025)	(0.035)	(0.022)	(0.016)	(0.011)
Diagnostics						
Between study variance	0.00077	0.00078				
R^2			0.47	0.46	0.68	0.68
Root MSE			0.057	0.057	0.030	0.030

[a] Estimation results with standard errors in parentheses. Huber–White standard errors are robust to heteroskedasticity and within-study autocorrelation. Significance is indicated by *, ** and *** for the 10, 5 and 1% levels, respectively.

within-variance, and is taken from the primary studies. Any remaining heterogeneity between estimates is either explainable by observable differences modeled through moderator variables contained in x_i, or it is random and normally distributed with mean zero and variance τ_i^2, known as the between-variance. The unknown variance can be estimated by an iterative (restricted) maximum likelihood process or, alternatively, using the empirical Bayes method, or a non-iterative moment estimator (see Thompson and Stoenever (1983), for details). We use the iterative restricted maximum likelihood estimator with weights $\hat{\omega}_i = 1/(\hat{\sigma}_i^2 + \hat{\tau}_i^2)$ to obtain estimates for the regression coefficients and $\hat{\tau}_i^2$.

A popular estimator that simultaneously accounts for heteroskedasticity and cluster correlation among effect sizes sampled from the same primary study is the familiar Huber–White estimator (Williams, 2000). The Huber–White estimator is, however, rather restrictive because it assumes all differences across measurements and studies to be observable, and sufficient to explain the empirical heterogeneity. The Huber–White estimator does not fully exploit all available information because it estimates the difference in variances rather than also using the information on the estimated standard errors that is available from the primary studies. Simulation experiments have shown that it nevertheless performs reasonably well (Lewis and Linzer, 2005). In order to mitigate the latter, we also apply the Huber–White estimator to inverse-variance weighted data.

The results for the mixed effect estimator are provided in the first two columns of Table 3; subsequent columns provide the results for the weighted and unweighted version of the Huber–White estimator. Columns labeled (a) contain the full specification with the entire set of explanatory variables, while columns labeled (b) provide results for a restricted specification in which variables with a significance level smaller than 10% have successively been dropped (backward stepwise regression). The results across estimators are by and large very similar in terms of sign, magnitude and significance.

The dependent variable is expressed in terms of percentage change in selling price as a result of flood risk, and is negative two times in three. The marginal effect of flood risk associated with location in the 100-year floodplain is clearly negative and amounts to a decrease in the effect size of 0.006 to 0.008, except for the unweighted Huber–White estimate which shows a slightly smaller effect of −0.003.[12] As an illustration, we consider an observed 3% decrease in selling price due to the location of a given house in a 100-year floodplain as compared with a house in a safe zone ($t_{100yfp} = -0.03$). The same house located in a 50-year floodplain would then sell for 3.6% cheaper than in the safe zone ($t_{50yfp} = -0.036$). Indeed, the variation between the 100 and 50-year floodplain corresponds to an increase in risk of occurrence per year of $0.01 = 0.02 - 0.01$, and the variation in the observed effect size corresponds to $-0.036 = -0.03 + (0.01^* - 0.637)$ (based on the mixed-effect coefficient estimate). Interestingly, the results also clearly demonstrate the problems associated with subjective perceptions of generally very small risks. The marginal effect of risk exposure is enhanced (in absolute magnitude) when a recent flood has occurred, or increased stringency in disclosure rules effectively results in more objective information about the environmental hazard being available. *Ex-ante* evaluation, which is by definition based on a more subjective assessment of the environmental hazard, results in an implicit price of risk exposure that is smaller. This can be due to the fact that *ex-ante*, subjective perception of the risk level is too low with regard to the real risk level. New information helps individuals to re-assess this value and to effectively make subjective and objective risk levels more comparable. Although *ex-ante* and *ex-post* risk assessments mitigate or enhance the implicit price of flooding as expected, their effect is very small in (absolute) magnitude. We also find that the valuation of risk is positively associated with income, implying that the absolute value of the implicit price of risk is smaller for individuals with higher incomes, which can be traced back to the greater capacity to mitigate and adapt to the negative effects of flooding.

Previously we have mentioned that it is essential to carefully distinguish between positive and negative water-related amenities. Omitting a control for positive water-related amenities in primary studies is likely to confound the effect of amenities and risk, and therefore underestimate the implicit price of the risk of flooding. This is confirmed by our estimates in the sense that controlling for the proximity to water in the primary study increases (in absolute value) the implicit price of flooding. Controlling for a view does not, however, show the same effect unequivocally, and the meta-analysis results are much more mixed. One reason for this may be that the availability of scenic views concurrently acts as a proxy for lying on high lands, and thus being less flood-prone.

Another issue that warrants further investigation is the positive estimated effect of a coastal location. A negative sign can be expected for at least two reasons related to perceived risk and actual risk, respectively. First, perception may be such that the risk associated with hurricanes and thunderstorms in coastal areas is greater. In terms of actual risk the hazard may be greater because coastal zones can be subject to river flood risk as well, effectively causing a situation in which different types of flood risk cumulate.[13] A reason for the counter-intuitive positive sign may be that in coastal areas, the water amenity value is less well captured than in river zones. If the coastal location reduces to coastal amenities, and those are higher valued than water risk, a positive sign is to be expected. As a simple check we estimate a variant of the meta-regression model presented in Table 3 replacing the coastal zone and proximity to water controls by cross-products of coastal zone and proximity to water, and non-coastal zone and proximity to water. Interestingly, the proximity to water in an inland zone has a larger coefficient than the proximity to water in a coastal zone in absolute value (−0.143 and −0.041, respectively, both with a p-value < 0.01). The interpretation that water amenities are captured inadequately or insufficiently in coastal zones is therefore warranted. In coastal zones, proximity to water most likely does not cover the diversity of wide-ranging amenities, such as easy access to water transportation and harbor activities, tourism and recreation, open space and lower fragmentation of the landscape, and stronger winds causing lower pollution levels. It may also be that because of higher property values on the coast, estimated percentage changes in transaction price due to flood risk are lower. In this case the specification in the primary study of the hedonic price equation should account for a non-linear relationship between risk and house prices by, for instance, making use of a Box–Cox transformation.

In modern societies, improvements in safety and health levels are believed to go together with amplified concerns about risk (Kunreuther and Slovic, 1996), implying that the increase in quality of life makes people more risk averse. This aspect is confirmed in our results by the negative sign associated with the time trend variable referring to the median sample year. The time span variable is also in line with this interpretation, as the longer the time span, the older the data set under study. Indeed, with the modern improvements in data collection and ability to deal with large data sets, recent studies cover longer time periods, and as a result the median sample year is further back in time. The fact that the time span variable is associated with a positive coefficient estimate does not have a meaning in itself, but this positive effect is decreasing over time as the median sample year is more recent. One should note that the abovementioned societal improvements have to be distinguished from a mere accumulation of wealth, because we control separately for variation in income levels. Both income as well as the closely related evaluation of implicit prices for higher than average house prices show that the implicit price of flood risk is lower (in absolute value). In terms of design characteristics of the primary studies, the functional form of the valuation model does not seem to cause significantly different valuations, but estimators allowing for heteroskedasticity and/or spatial autocorrelation exhibit slightly higher implicit prices (in absolute value).

Finally, it is important to note that the omission of certain covariates in primary studies can effectively make the estimator biased. The results show that the inclusion of variables describing quality characteristics of the house (age, maintenance level) increases the adverse effects of risk. Variables describing the comfort level of the house (central heating, fireplace) have a similar effect, whereas the inclusion of neighborhood effects does not affect the implicit price level in any significant manner. Control variables in the primary study that capture debt financing of the house have exactly the opposite effect, and their (absolute) magnitude is greater. The results with respect to conditioning variables included in the primary study clearly reveal that the omission of certain covariates in the primary study may lead to an under- or overestimation of the implicit price of flood risk.

5. Conclusion

The purpose of this paper is to explore the magnitude and determinants of the implicit price of the risk of flooding. We use a meta-analysis of 19 studies, exclusively from the US providing a total of 117 point estimates, to investigate the impact of exposure to

[12] Instead of operationalizing risk as a continuous variable defined as the annual expected occurrence of a flood, we have also experimented with a less restrictive specification in which a series of dummy variables representing specific flood zones is used. However, this specification does not work well, because virtually all observations are in the 100- and 500-year floodplain, and only a few observations are available in the 5 to 50, and 200 to 400-year floodplains. We have also investigated S-shape transformations of the continuous risk variable. The use of these non-linear specifications of risk leads to an impact on the effect size that is very similar to the linear form for the 5-year floodplain, while lower risks are over-estimated. This is consistent with prospect theory and the over-weighting of low probabilities.

[13] Note that risk is characterized both by the probability of occurrence and the expected damage of an adverse event.

flood risk in terms of the implicit price differential associated with the location of a house in a flood zone. Specifically, we use the meta-analysis to shed light on the difference between pre- and post-event valuation, and the potentially confounding effect of the coincidence of positive water-related amenities with flood risk. The distinction between *ex-ante* and *ex-post* valuation is innate to the problematic nature of the perception of risk, and effectively makes a distinction between "subjective" risk assessment (*ex-ante*) and "objective" appraisal of the hazard (*ex-post*) following either the occurrence of an event or the implementation of more stringent disclosure rules.

The results of the meta-analysis are useful, both because they contribute to a more precise assessment of the implicit price of an environmental hazard such as flooding, but also because they provide guidance as to where the important voids are in our current understanding of the willingness-to-pay to reduce exposure to environmental hazards. Prior to discussing the noteworthy inferences in that respect, we would like to point out that the available empirical literature on flood risk is still relatively scant. In conjunction with model uncertainty giving rise to considerable heterogeneity across studies, which needs to be accounted for in the meta-analysis, there is a clear-cut need for more empirical work on this topic. In particular, studies pertaining to locations outside the United States can improve our understanding of the impact of different geospatial and geoclimatic settings (e.g. countries with substantial areas below sea level, or countries in the tropics where weather influences are much more severe and significant).

An overview of the 19 available studies shows that estimates of the implicit price of flood risk vary considerably. A multivariate meta-analysis, controlling for observable and unobservable differences across studies through fixed and random effects, shows that the marginal effect of an increase in the probability of flood risk of 0.01 in a year amounts to a difference in transaction price of an otherwise similar house of −0.6%. The actual occurrence of a flooding event or increased stringency in disclosure rules causes a difference between *ex-ante* and *ex-post* price effects, but the effects associated with *ex-ante* or *ex-post* measurement are very small in magnitude. Arguably, this is only a fairly crude way of modeling difficulties associated with risk perception in the case of small risks, and it points to the need for a closer investigation of this aspect of economic risk assessment.

The meta-analysis also shows that there is a real danger of confounding positively valued water-related amenities with negatively valued exposure to flood risk. The way in which these countervailing effects have been incorporated and identified in valuation studies has to date been rather underdeveloped. Given the size of the associated impacts we discover in the meta-analysis, and given the still inconclusive nature of their interpretation, this is an issue that warrants more attention in future studies on flood risk.

Appendix A

The implicit price of the risk of flooding depends on the specification of the hedonic price model. For a linear specification, $P=\alpha F+X\beta+\varepsilon$, where P is the house price, F a flood zone dummy, X conditioning variables including a constant, ε the error term, and α and β parameters to be estimated, $\partial P/\partial F=\alpha$. Similarly, for a loglinear specification, $\ln P=\alpha F+X\beta+\varepsilon$, the implicit price is[14] $\partial P/\partial F=\alpha.\exp(\alpha F+X\beta+\varepsilon)=\alpha P$. For a Box–Cox specification with a transformed dependent variable:

$$P^{(\lambda)}\equiv\frac{P^{\lambda}-1}{\lambda}=\alpha F+X\beta+\varepsilon, \tag{A1}$$

where λ is the non-linearity parameter, the implicit price equals:

$$\begin{aligned}\frac{\partial P}{\partial F}&=\frac{1}{\lambda}(\lambda(\alpha F+X\beta+\varepsilon)+1)^{\frac{1}{\lambda}-1}\lambda\alpha\\&=\alpha(\lambda(\alpha F+X\beta+\varepsilon)+1)^{\frac{1-\lambda}{\lambda}}\\&=\alpha(\lambda P^{(\lambda)}+1)^{\frac{1-\lambda}{\lambda}}\\&=\alpha(P^{\lambda})^{\frac{1-\lambda}{\lambda}}\\&=\alpha P^{1-\lambda}.\end{aligned} \tag{A2}$$

It is straightforward to show that this result is also valid for the gradient of a Box–Cox model where one or more of the continuous right-hand side variables are also transformed, as in $P^{(\lambda)}=\alpha F+X^{(\lambda)}\beta+Z\gamma+\varepsilon$ where, except for the flood zone dummy, $X^{(\lambda)}$ contains the transformed variables and Z the untransformed variables (including the constant).

Eq. (A2), particularly the second line above, shows that marginal effects are not easily separable in terms of the parameters because the error term does not vanish in the gradient, except when $\lambda=1$. Wooldridge (1992) presents an alternative model in which the error term does not show up in the conditional expectation and the marginal effects. The standard procedure is, however, to ignore the presence of the error term and simply estimate marginal effects using $\hat{P}^{(\hat{\lambda})}$ determined with a maximum likelihood or an instrumental variables estimator. Ignoring the presence of the error term does render both estimators biased, and Abrevaya (2002) demonstrates that procedures accounting for the presence of the error term through numerical integration or "smearing," result in improved fitted values, conditional expectations and marginal effects.[15] Following standard practice, however, we simply ignore the error term in the below.

The effect size T is defined as the relative change in the price of the house, so the first derivatives need to be divided by P, which results in $T=\alpha/P$ for the linear model, $T=\alpha$ for the loglinear model, and $T=\alpha P^{-\lambda}$ for the Box–Cox specifications. These effect sizes can be evaluated for a given price of the house, say the average *observed* price $\bar{P}$ for the linear model, and (ignoring the error term) the average predicted price with F valued at its sample mean for the Box–Cox model.

The variance of the effect sizes for the linear and loglinear specifications can be derived in a straightforward fashion. For the linear specification $\bar{P}$ is a known constant, and hence the variance is equal to $(1/\bar{P})^2\mathrm{var}(\alpha)$. In the loglinear specification the variance can be taken directly from the reported results of the primary study, because it is simply the squared standard error of the coefficient of the flood dummy.

The variance of the effect size in the Box–Cox specification needs to be approximated because, as Eq. (A2) shows, the effect size is a non-linear function of two random variables even if one discards the error term. Using the Delta Method (Greene, 2003, p. 70), the asymptotic variance is given by:

$$\mathrm{var}(T)=\left(\frac{\partial T}{\partial\lambda}\right)^2\sigma_{\lambda}^2+\left(\frac{\partial T}{\partial\alpha}\right)^2\sigma_{\alpha}^2+2\left(\frac{\partial T}{\partial\lambda}\right)\left(\frac{\partial T}{\partial\alpha}\right)\mathrm{cov}(\lambda,\alpha), \tag{A3}$$

[14] Note that formally the derivative does not exist in the case of a dummy variable, and therefore the marginal effect should be adjusted to $e^{\alpha}-1$ (Halvorsen and Palmquist, 1980). In the current case, adjustments are not taken into account, because their effect is negligible given the estimated values of the coefficient.

[15] Since F is a dummy variable, the Box–Cox studies concerned (Dei Tutu and Bin, 2002, MacDonald et al., 1987, 1990) actually compute marginal effects as: $\hat{P}^{(\hat{\lambda})}_{F=1}-\hat{P}^{(\hat{\lambda})}_{F=0}$, and subsequently transform the predicted values to P in order to determine the implicit price differential of the risk of flooding. This is nearly identical to using Eq. (A4) with F fixed at its sample mean.

where the covariance can be further expanded to $r_{\alpha\lambda}\sigma_\alpha\sigma_\lambda$, where $r_{\alpha\lambda}$ is the correlation coefficient between α and λ. From Eq. (A2) we can determine:

$$\begin{aligned}\frac{\partial T}{\partial \alpha} &= (\lambda\alpha F + \lambda X\beta + 1)^{-1} - \lambda\alpha F(\lambda\alpha F + \lambda X\beta + 1)^{-2} \\ &= P^{-\lambda} - \alpha\lambda F P^{-2\lambda}\end{aligned} \tag{A4}$$

and an identical result can be derived for the extended Box–Cox specification with transformed right-hand side variables. The derivative to λ is different, however, depending on the starting point. For the Box–Cox model given in Eq. (A2) we obtain:

$$\begin{aligned}\frac{\partial T}{\partial \lambda} &= \frac{\partial \alpha P^{-\lambda}}{\partial \lambda} \\ &= -\alpha(\lambda\alpha F + \lambda X\beta + 1)^{-2}(\alpha F + X\beta) \\ &= -\alpha P^{-2\lambda}(\alpha F + X\beta)\end{aligned} \tag{A5}$$

and for the extended Box–Cox model we have:

$$\begin{aligned}\frac{\partial T}{\partial \lambda} &= \frac{\partial \alpha P^{-\lambda}}{\partial \lambda} \\ &= -\alpha(\lambda\alpha F + X^{\lambda}\beta - \beta + Z\gamma + 1)^{-2}(\alpha F + X^{\lambda}\beta \ln(X) + Z\gamma) \\ &= -\alpha P^{-2\lambda}(\alpha F + X^{\lambda}\beta \ln(X) + Z\gamma).\end{aligned} \tag{A6}$$

Substitution of Eqs. (A4) and (A5) or (A6), respectively, in Eq. (A3) gives the expression for the variance of T. This expression can be evaluated at the sample mean of the predicted price, $\bar{\hat{P}}$, with F, X and Z fixed at their sample mean(s), and using estimated values for α, β, γ, λ, σ_λ and σ_α. In the case where an estimate for σ_λ is unavailable, we use an approximated standard error of $\lambda/2$, which makes λ significantly different from zero approximately at the 5% level. Since $r_{\lambda\alpha}$ is generally unavailable, we use $r_{\lambda\alpha} = -0.9$ or $+0.9$ depending on whether $(\partial T/\partial\lambda)(\partial T/\partial\alpha)$ is negative or positive, respectively, in order to obtain sufficiently conservative standard errors.

References

Abreu, M., de Groot, H.L.F., Florax, R.J.G.M., 2005. A meta-analysis of β-convergence: the legendary two-percent. Journal of Economic Surveys 19 (3), 389–420.

Abrevaya, J., 2002. Computing marginal effects in the Box–Cox model. Econometric Reviews 21 (3), 383–393.

Akerlof, G.A., 1970. The market for "lemons": quality uncertainty and the market mechanism. The Quarterly Journal of Economics 84 (3), 488–500.

Barnard, J.R., 1978. Externalities from urban growth: the case of increased storm runoff and flooding. Land Economics 54 (3), 298–315.

Bartosova, A., Clark, D.E., Novotny, V., Taylor, K.S., 1999. Using GIS to Evaluate the Effects of Flood Risk on Residential Property Values. Marquette University, Milwaukee. Institute for Urban Environmental, Risk Management, Technical Reports.

Begg, C.B., Mazumdar, M., 1994. Operating Characteristics of a Rank Correlation Test for Publication Bias. Biometrics 50, 1088–1101.

Beron, K.J., Murdoch, J.C., Thayer, M.A., Vijverberg, W.P.M., 1997. An analysis of the housing market before and after the 1989 Loma Prieta earthquake. Land Economics 73 (1), 101–113.

Bialaszewski, D., Newsome, B.A., 1990. Adjusting comparable sales for floodplain location: the case of Homewood, Alabama. The Appraisal Journal 58 (1), 114–118.

Bin, O., Polasky, S., 2004. Effects of flood hazards on property values: Evidence before and after Hurricane Floyd. Land Economics 80 (4), 490–500.

Bin, O., Kruse, J.B., 2006. Real estate market response to coastal flood hazards. Natural Hazards Review 7 (4), 137–144.

Bin, O., Crawford, T.W., Kruse, J.B., Landry, C.E., 2008a. Viewscapes and flood hazard: Coastal housing market response to amenities and risk. Land Economics 84 (3), 434–448.

Bin, O., Kruse, J.B., Landry, C.E., 2008b. Flood hazards, insurance rates, and amenities: Evidence from the coastal housing market. Journal of Risk and Insurance 75 (1), 63–82.

Brander, L., Florax, R.J.G.M., Vermaat, J., 2006. The empirics of wetland valuation: a comprehensive summary and a meta-analysis of the literature. Environmental and Resource Economics 33 (2), 223–250.

Browne, M.J., Hoyt, R.E., 2000. The demand for flood insurance: empirical evidence. Journal of Risk and Uncertainty 20 (3), 291–306.

Card, D., Krueger, A., 1995. Time-series minimum-wage studies: a meta-analysis. American Economic Review 85, 238–243.

de Blaeij, A., Florax, R.J.G.M., Rietveld, P., Verhoef, E., 2000. The value of statistical life in road safety: a meta-analysis. Accident, Analysis and Prevention 35 (6), 973–986.

Dei Tutu, V.A., Bin, O., 2002. Flood hazards, insurance, and house prices — a hedonic property price analysis. Research Paper, East Carolina University, East Carolina Economic Review, M.S.

Donnelly, W.A., 1989. Hedonic price analysis of the effect of a floodplain on property values. Water Resources Bulletin 25 (3), 581–586.

Egger, M., Smith, G.D., Schneider, M., Minder, C., 1997. Bias in meta-analysis detected by a simple, graphical test. British Medical Journal 315, 629–634.

Florax, R.J.G.M., 2002. Methodological pitfalls in meta-analysis: publication bias. In: Florax, R.J.G.M., Nijkamp, P., Willis, K. (Eds.), Comparative Environmental Economic Assessment. Edward Elgar, pp. 177–207.

Florax, R.J.G.M., Travisi, C.M., Nijkamp, P., 2005. A meta-analysis of the willingness to pay for reductions in pesticide risk exposure. European Review of Agricultural Economics 32 (4), 441–467.

Freeman III, A.M., 2003. The measurements of environmental and resource values: theory and methods. Resources for the Future, Washington D.C.

Fridgen, P.M., Shultz, S.D., 1999. The influence of the threat of flooding on housing values in Fargo, North Dakota and Moorhead. Minnesota, Department of Agricultural Economics, North Dakota State University, Agricultural Economics Report. 417.

Greene, W., 2003. Econometric Analysis5th edition. Prentice Hall Publisher.

Gurevitch, J., Hedges, L.V., 1999. Statistical issues in ecological meta-analyses. Ecology 80 (4), 1142–1149.

Hallstrom, D.G., Smith, V.K., 2005. Market responses to hurricanes. Journal of Environmental Economics and Management 50, 541–561.

Halvorsen, R., Palmquist, R., 1980. The interpretation of dummy variables in semilogarithmic equations. American Economic Review 70, 474–475.

Harrison, D.M., Smersh, G.T., Schwartz Jr., A.L., 2001. Environmental determinants of housing prices: the impact of flood zone status. The Journal of Real Estate Research 21 (1/2), 3–20.

Holway, J.M., Burby, R.J., 1990. The effects of floodplain development controls on residential land values. Land Economics 66 (3), 259–271.

IPCC, 2007. Climate Change 2007: The Physical Science Basis — Summary for Policymakers — IPCC WGI Fourth Assessment Report.

Kahneman, D., Tversky, 1979. A., Prospect theory: an analysis of decision under risk. Econometrica 47 (2), 263–292.

Kim, C.W., Phipps, T.T., Anselin, L., 2003. Measuring the benefits of air quality improvement: a spatial hedonic approach. Journal of Environmental Economics and Management 45, 24–39.

Kriesel, W., Friedman, R., 2002. Coastal hazards and economic externality: implications for beach management policies in the American South East. H. John Heinz III Center for Science, Economics and the Environment, Washington DC.

Kron, W., 2003. Flood risk = hazard × exposure × vulnerability. Journal of Lake Sciences 15, 185–204.

Kunreuther, H., Slovic, P., 1996. Science, values, and risk. Annals of the American Academy of Political and Social Science 545 (1), 116–125.

Lewis, J.B., Linzer, D.A., 2005. Estimating regression models in which the dependent variable is based on estimates. Political Analysis 13, 345–364.

List, J.A., Gallet, C.A., 2001. What experimental protocol influence disparities between actual and hypothetical stated values? Environmental and Resource Economics 20 (3), 241–254.

Macaskill, P., Walter, S.D., Irwig, L., 2001. A comparison of methods to detect publication bias in meta-analysis. Statistics in Medicine 20, 641–654.

MacDonald, D.N., Murdoch, J.C., White, H.L., 1987. Uncertain hazards, insurance, and consumer choice: evidence from housing markets. Land Economics 63 (4), 361–371.

MacDonald, D.N., White, H.L., Taube, P.M., Huth, W.L., 1990. Flood hazard pricing and insurance premium differentials: evidence from the housing market. Journal of Risk and Insurance 57 (4), 654–663.

Paklina, N., 2003. Flood insurance. OECD, Paris.

Pope, J.C., 2006. Asymmetric information and the hedonic model: the impact of a flood zone disclosure on house prices, Working Paper.

Roberts, C.J., Stanley, T.D., 2005. Meta-regression analysis: issues of publication bias in economics. Blackwell Publishing, Oxford.

Rosen, S., 1974. Hedonic prices and implicit markets: product differentiation in pure competition. Journal of Political Economy 82 (1), 34–55.

Rothstein, A., Sutton, A.J., Borenstein, M., 2005. Publication Bias in Meta-Analysis: Prevention, Assessment and Adjustments. Wiley Publishing.

Shabman, L.A., Damianos, D.I., 1976. Flood hazard effects on residential property values. Journal of the Water Resources Planning and Management Division 102 (1), 151–162.

Shilling, J.D., Sirmans, C.F., Benjamin, J.D., 1989. Flood insurance, wealth redistribution, and urban property values. Journal of Urban Economics 26 (1), 43–53.

Shrubsole, D., Green, M., Scherer, J., 1997. The actual and perceived effects of floodplain land-use regulations on residential property values in London, Ontario. The Canadian Geographer 41 (2), 166–178.

Skantz, T.R., Strickland, T.H., 1987. House prices and a flood event: an empirical investigation of market efficiency. The Journal of Real Estate Research 2 (2), 75–83.

Smith, V.K., Huang, J.C., 1995. Can markets value air quality? A meta-analysis of hedonic property value models. Journal of Political Economy 103 (1), 209–227.

Speyrer, J.F., Ragas, W.R., 1991. Housing prices and flood risk: an examination using spline regression. Journal of Real Estate Finance and Economics 4 (4), 395–407.

Stanley, T.D., 2005. Beyond publication bias. Journal of Economic Surveys 19 (3), 309–345.

Sutton, A.J., Abrams, K.R., Jones, D.R., Sheldon, T.A., Song, F., 2000. Methods for Meta-Analysis in Medical Research. John Wiley.

Thompson, E., Stoenever, H.H., 1983. Estimating residential flood control benefits using implicit equations. Water Resources Bulletin 19 (6), 889–895.

Tobin, G.A., Montz, B.E., 1994. The flood hazard and dynamics of the urban residential land market. Water Resources Bulletin 30 (4), 673–685.

Troy, A., Romm, J., 2004. Assessing the price effects of flood hazard disclosure under the California Natural Hazard Disclosure Law (AB 1195). Journal of Environmental Planning and Management 47 (1), 137–162.

US Army Corps of Engineers, 1998. Empirical studies of the effect of flood risk on housing prices. Alexandria, Virginia, Water Resources Support Center Institute for Water Resources.

van Dantzig, D., 1956. Economic decision problems for flood prevention. Econometrica 24 (3), 276–287.

Viscusi, K.W., 1991. Economic theories of decision making under uncertainty: implications for policy analysis. In: Weimer, D.L. (Ed.), Policy Analysis and Economics. Developments, Tensions, Prospects. Kluwer Academic, pp. 85–109.

Viscusi, K.W., Zeckhauser, R.J., 1996. Hazard communication: warnings and risks. Annals of the American Academy of Political and Social Science 545 (1), 106–115.

Williams, R.L., 2000. A note on robust variance estimation for cluster-correlated data. Biometrics 56, 645–646.

Wooldridge, J.M., 1992. Some alternatives to the Box–Cox regression model. International Economic Review 33, 935–955.

Wright, J.M., 2000. In: Hessler, WendyL. (Ed.), The Nation's Responses To Flood Disasters: A Historical Account. Association of State Floodplain Managers.

Zhai, G., Fukuzono, T., Ikeda, S., 2003. Effect of flooding on megalopolitan land prices: a case study of the 2000 Tokai flood in Japan. Journal of Natural Disaster Science 25 (1), 23–36.

Zimmerman, R., 1979. The effect of flood plain location on property values: three towns in Northeastern New Jersey. Water Resources Bulletin 15 (6), 1653–1665.