A META-ANALYSIS OF WUI RESIDENTS' WILLINGNESS TO PAY FOR WILDFIRE RISK MITIGATION

by

ROBERT ANTHONY BROOKS

(Under the Direction of Craig Landry)

ABSTRACT

A meta-analysis of the existing contingent valuation literature on homeowners' willingness to pay for wildfire risk mitigation was conducted. Results indicate that the absolute level of risk reduction and whether a proposed program is public or private have significant systematic impact on WTP estimates. There is evidence that time elapsed since the last major destructive fire in the area, and the provision of specific baseline risk information in the survey instrument, both can increase estimates; further research into these affects seems warranted. Findings point to ways in which further CV surveys in this field may be standardized and improved.

INDEX WORDS: willingness to pay, WTP, contingent valuation, wildfire, wildfire risk, meta-analysis, risk information

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ROBERT ANTHONY BROOKS

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ROBERT ANTHONY BROOKS

Major Professor: Craig Landry

Committee: John Bergstrom
Jeff Mullen

Electronic Version Approved:

Suzanne Barbour
Dean of the Graduate School
University of Georgia
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CHAPTER

1. INTRODUCTION

Description of the Problem

Wildfires have become an increasingly costly threat in recent years, resulting from a combination of more frequent large wildfires and the continued expansion of the wildland-urban interface (WUI). Climate change has led to fire seasons 78 days longer, on average, than in 1970 (USDA 2019). Strader (2018) used wildfire occurrence and housing unit data to determine the increase in wildfire risk exposure in the coterminous US from 1940 to 1960, finding a 1350% overall increase in housing in wildfire-prone areas over this time period. Human influence on fire regimes extends beyond developing housing units: 88% of wildfire ignitions in the U.S. from 2014 to 2018 were human-caused (CRS 2019). Because of these and other factors, each of the five years with the highest total acreage burned on record in the United States are within the past 13 years (National Interagency Fire Council).

As the nation's exposure to wildfire damage increases, the need for efficient policy design also becomes more pressing. People move to the WUI for the positive amenities rural living provides, including privacy, recreation opportunities, and the beauty of the natural environment. However, choosing to live in an area at risk of wildfire carries with it the risk of having one's property damaged or destroyed. In addition to the costs incurred by individual households, wildfire risk exposure generates costs to society at large, since government agencies must use resources to defend structures from damage or destruction. Certain actions taken to increase a home's amenity value, such as allowing vegetation to grow close to one's home, may also create externalities for neighboring homes by increasing potential fuel.

In an idealized market, households have all relevant information regarding the objective risk of wildfire for a given area and make rational decisions regarding wildfire risk, according to this information and households' individual preferences and risk tolerance. In the real-world setting, households may not have all relevant information, leading to inefficient outcomes. They may also have preferences regarding wildfire management extending beyond risk reduction to include aesthetic and political considerations. Therefore, information regarding WUI residents' attitudes and preferences for wildfire management is valuable, potentially aiding policymakers in deciding which policies lead to effective, efficient risk reduction.

Researchers have used several techniques to estimate non-market economic values associated with environmental hazards, including wildfires. One of the most common techniques used in nonmarket valuation is the contingent valuation method, which elicits willingness-to-pay (WTP) and willingness-to-accept (WTA) estimates for non-market goods and services. Another common technique is hedonic valuation, which values the constituent components (including environmental amenities) of differentiated goods such as housing (Taylor 2017). Meta-analyses of studies using these, and other, techniques have become common in resource economics, valuing such goods and services as wetlands (Woodward and Wui 2001), preservation of endangered species, and price discounts associated with living in a floodplain (Beltran et al., 2018).

To date, there have been no meta-analyses of values for the reduction of wildfire risk. This thesis fills that gap. By examining the existing literature on wildfire risk, trends and systematic factors may be identified which could affect WTP estimates; awareness of these factors may improve future studies, including potential future meta-analyses on the subject. In addition, meta-analysis may be able to illuminate some of the issues associated with benefit transfer of existing studies, which poses considerable challenges due to the heterogeneity of wildfire risk and WUI amenity considerations.

Whether or not a household undertakes wildfire risk mitigation activity is influenced by the household's risk perception, which in turn is influenced by the information available. One of the primary benefits of conducting a meta-analysis of WTP for wildfire mitigation is to gain insight into the effects of information on WTP. Different surveys contain different levels of risk information, and different study areas possess different fire history. Meta-analysis provides a setting for testing for the effects of these large-scale differences. While many probable sources of differences in information remain unobservable, some that are observable, such as the provision of baseline risk and time elapsed since the most recent destructive fire in the area, are shown in the meta-analysis to have significant impact.

Another potential benefit of meta-analysis is insight into preferences for specific methods of wildfire risk reduction. Two broad categories of wildfire reduction are prescribed burning, where a landholder intentionally starts a controlled fire to reduce fuel buildup and preserve the health of fire-adapted ecosystems, and fuel clearing, which may range from clearing downed trees on public lands to homeowners maintaining defensible space. Mitigation programs may also be administrated by government agencies, or privately via individual actions or contractors. Identifying large-scale trends in preferences for program type via meta-analysis may help policymakers identify programs most appropriate in their area. It may also help provide context to benefit transfer exercises. This meta-analysis finds evidence that, while preferences among various public and private mitigation options are complicated, some general trends do exist across various study sites.

The hedonic and contingent valuation literature are considered in separate meta-analyses. Examination of the CV surveys found evidence for systematic effects in line with existing economic theory on decision-making under uncertainty, as well as evidence for between-study differences that had statistically significant effects on WTP estimates, and the likely presence of a publication effect, a common occurrence in meta-analysis. Conversely, there appear to be too few readily available hedonic studies on the effects of wildfire to obtain meaningful results from meta-analysis at this time.

The Structure of This Thesis

The rest of the thesis is organized as follows. Chapter 2 provides a brief overview of the theoretical treatment of the household's risk reduction problem, as well as describing the method employed by the studies used in this meta-analysis: contingent valuation (hereafter referred to as CV). Chapter 3 contains a review of the relevant literature. Chapter 4 will explain the methods used in the construction of the meta-samples. Chapter 5 describes the models used in the analysis, and gives the results of the analysis. Weighted least squares (WLS) estimation was used for the meta-analysis of the CV studies. Chapter 6 discusses the results of the models, and possible interpretations in light of the relevant economic theory. Chapter 7 concludes.

2. THEORETICAL BACKGROUND

Expected Utility Theory

The expected utility theory will be used as the basis for describing households' decision-making in uncertain situations. Under expected utility theory, a household endowed with initial wealth level W faces an uncertain situation, where the probability of a loss is equal to p(z). The household is then said to have an expected level of utility

$$E(u) = p(z)U(W-z) + (1-p(z))U(W)$$

In the case of wildfire, the probability of the event will generally be quite low (less than 1% per year), but the severity of the event will be quite high. One may reasonably assume that if a wildfire reaches their house, the house will be destroyed.

Households wishing to minimize their risk exposure may take actions to decrease it. Insurance purchases are one of the most common means of decreasing exposure to low probability, high-severity events, but they are not the only method, nor are they comprehensive to all losses a household may incur.

Talberth et al. (2006) provide a broad summary of how expected utility theory may be used to describe actions besides insurance; a brief version of this summary is reproduced here. They begin with Dixit's (1990) statement of the "moral hazard" problem in insurance: that, if full insurance is available, risk-averting activities are disincentivized. Talberth et al. point out that, while this would seem to preclude the possibility of positive WTP for risk-averting activities, Simmons and Kruse (2000) modify this approach to account for intangible losses, which cannot be insured. Constraining insurance purchases so that they may only be less than or equal to the expected value of tangible losses, Simmons and Kruse show that risk-averting activities are still possible even if full insurance is available. Averting activities may include taking or supporting physical actions to reduce the risk of wildfire; such actions are valued by CV studies. They may also involve a household choosing to live in a less risky area; the value of those types of decisions is measured by hedonic studies.

Contingent Valuation Method

The contingent valuation method is a commonly-used technique for eliciting values of non-market goods and services. CV studies use surveys to describe the good or service being valued, and then asks respondents for their willingness to pay for (or accept compensation for the loss of) that good or service (Boyle 2017). WTP is a Hicksian welfare measure, derived from the indirect utility function. CV methodology and reliability has improved dramatically since the early days of its use, and the method is now used to elicit values for a wide array of goods and services.

There are several methods of eliciting WTP values through the CV survey; three of them are used in the studies in this meta-sample. The open-ended method was used in the earlier days of CV studies and is less common now. Open-ended questions simply ask the respondent the most they would be willing to pay for the nonmarket good. Dichotomous choice questions are more common in recent CV surveys and are the most common means of eliciting payment in this meta-sample. These questions provide a bid amount and simply ask respondents whether or not they would pay that amount. Polychotomous choice questions allow respondents to choose one of several alternatives that they most prefer. CV surveys may also differ in the means in which

WTP estimates are computed from survey respondents. Probit and logit models are the most common, though other functional forms are used as the researcher sees fit.

3. LITERATURE REVIEW

Contingent Valuation Surveys

Many environmental goods and services are not available for purchase in a traditional market setting. For example, clean air, clean water, and ecosystem protection are all valuable, but open markets for these goods do not exist. Nonmarket valuation techniques have been developed as a means of providing estimates for the economic value of these goods and services when price signals created by markets are unavailable. They are common in environmental economics, health economics, and other areas branches of economics dealing with these "missing markets." Rigorous applications of nonmarket valuation techniques provide more accurate assessments of the economic impacts of proposed projects; if estimates for these valuable environmental goods and services are not included, their value is often improperly assumed to be zero, leading to inefficient decision-making.

Contingent valuation is one method of estimating non-market values among many. It is a stated preference technique, relying on assertions by survey respondents regarding preferences in the context of a hypothetical scenario. This may be contrasted with revealed preference techniques, such as hedonic valuation, which use data on observed consumer behavior. Early surveys were criticized on several fronts, including hypothetical bias (results from a hypothetical scenario may differ from real-life behavior) and the problem of protest responses (where respondents would give $0 responses for a variety of reasons) (Boyle 2017). In response to these criticisms, which became particularly vocal following the use of CV surveys in court to estimate damages caused by the Exxon-Valdez oil spill, the National Oceanic and Atmospheric Administration (NOAA) commissioned a “blue-ribbon panel” of economists in 1993 to advise on the validity of the CV method. The panel concluded that nonmarket valuation using well-designed, highly rigorous CV surveys is preferable to having no estimate of these values (Boyle 2017).

The first contingent valuation study to value potential programs to reduce wildfire risk was Fried et al. (1999), who surveyed households in Crawford County, Michigan, a WUI community which had been impacted by a major wildfire several years prior. Findings generally indicated positive WTP for risk reduction activities involving fuel clearing. While mean estimates for WTP values were higher for individual programs (i.e., hiring the contractor or doing the work personally), many respondents also expressed WTP for the public program (the same actions, but undertaken by government agencies), but not for the private program. The study also used an open-ended format; all subsequent surveys found used other methods.

Further studies introduced other risk-reduction techniques, such as prescribed burning, and addressed questions of benefit transfer by using CV methods in new geographic locations and testing for differences among various demographics. Following the destructive 1998 fire season in Florida (particularly the northeastern part of the state), Loomis et al. (2000) surveyed Florida residents (statewide) regarding WTP for prescribed fire, mechanical fuel clearing, and herbicide treatment programs to reduce wildfire risk. Households generally preferred prescribed fire and clearing to herbicide; preferences for herbicide generally corresponded with a lack of support for prescribed fire. The survey also tested for differences in support levels between English and Spanish-speaking residents, finding no significant differences. Tests for differences among demographic groups were also conducted by Loomis et al. (2002) (English and Spanish speakers in northeastern Florida), Gonzalez-Caban (2004) (Native Americans and the general population in Montana), Loomis (2004) (various groups in California), and Gonzalez-Caban (2017) (various groups in Florida).

Other studies have focused on testing for various forms of validity. Kaval and Loomis (2004) found that including GIS data on wildfire hazard (such as slope, distance from fires that burned in 2000, and fire simulation data) and defensible space increased the explanatory power of their WTP model. Talberth et al. (2006) examined the relationship between insurance and averting activities, finding that residents support risk-averting activities even when insurance is available. They also found strong support for Firewise certification (a program organized by the National Fire Protection Association in cooperation with the USFS to encourage adherence to standards in wildfire risk reduction). In one of several studies undertaken in the Mediterranean region, Valera et al. (2013) examined willingness to pay for several constituent components of a program to reduce wildfire risk in the area via fuel breaks (clearing strips of forest to prevent the further spread of fires when they occur), including density of fuel breaks and the treatment method applied to create them. Respondents indicated higher levels of support for controlled grazing, and low support for prescribed burning.

While most studies addressed the subject of wildfire risk information and how additional information changes risk perception, several discussed the topic in-depth. Mozumder et al. (2009) measured WTP for the provision of wildfire risk information (in the form of a wildfire risk map) near Albuquerque, New Mexico. They found that the benefits to residents outweighed the costs of producing and disseminating the information. Talberth et al. (2006), in exploring the relationships between insurance and risk-averting activities, found that risk information played a key role in households' decision-making. In both survey and experimental settings, participants who were informed they were at high risk of loss tended not to allocate resources towards public risk-averting activities. However, the preferred method of loss exposure reduction for these households depended on the setting. In the survey, households at higher risk levels tended to prefer averting activities, while in the experimental setting, this was not the case. The authors suggest that the lack of support for public programs among higher-risk groups might represent skepticism regarding the public programs' efficacy, potentially because of free-riding concerns. Katuwal et al. (2015) specifically tested for the effects of differing levels of information, as well as information framing, on WTP. The study used CV surveys with three different levels of information. The most informative level, while not providing a direct estimate of baseline risk, does provide information about the number of homes destroyed by wildfire annually in the U.S. and near the study area. Since survey respondents would begin with differing levels of prior information, the authors hypothesized that more information in the survey would lead to converging estimates of WTP. Results indicated higher WTP measures from the two surveys with less information, with WTP values converging for the higher-information survey.

Qualitative Surveys

Qualitative surveys are not includable in the meta-sample, since they do not give WTP estimates. These surveys can still provide important context on wildfire risk perception, though, which can help provide context to the results of the meta-analysis. Some surveys have examined public attitudes toward wildfire mitigation programs. Loomis et al. (2001) surveyed Florida residents regarding their attitudes toward prescribed fire, finding that many Florida residents were unfamiliar with prescribed fire, and that support for prescribed fire programs increased after the introduction of new information. Jacobson et al. (2001) also surveyed Florida residents following the 1998 wildfires, finding that WUI residents and those outside the WUI had similar levels of knowledge. Those who had previous experience with prescribed burning, however, tended to have more positive attitudes towards it, with those who had no experience overestimating the risks of prescribed burning. Nelson et al. (2004) interviewed residents in the WUI to determine attitudes toward defensible space. Many residents had already reduced vegetation around their house, which was typical of the results from CV surveys when the question was asked. Most respondents were also generally supportive of prescribed fire. However, there were a few respondents who strongly opposed prescribed fire and/or maintaining defensible space.

Heterogeneity and Spatial Externalities

Wildfire risk is typically quite heterogeneous. Even within a relatively small study area, the risk of wildfire can vary greatly among households. This presents problems in meta-analysis, where the researcher must often use average measures over areas that may have large variability. In addition, studies such as Shrafran (2006) show that averting decisions on WUI residents' property is affected by externalities. Choosing to clear fuel loads on one's property decreases fire risk for the homeowner and his or her neighbor, but also may affect the amenity values of the property. WUI residents may not take into account their neighbors' welfare when deciding not to maintain defensible space or clear fuel load, thus creating an externality. These externalities may have a significant impact on willingness to pay for risk mitigation, and contribute to unobserved variation in the meta-data.

Meta-Analysis

While meta-analysis has become common in environmental and resource economics, there have been few meta-analyses of CV for risk mitigation of any sort. A few meta-analyses of other types of studies, relating to flood risk, may hold some relevance to the topic of wildfire risk. Beltran et al. (2018) examined the relationship between flood risk and housing prices in hedonic studies. Using information such as location in the 100-year or 500-year floodplain, whether the study area is inland or on the coast, and the flooding history of the study area, they obtain an estimate of a 4.6% discount of housing prices for inland 100-year floodplains. Two of the variables used in this study (floodplain location and flooding history) would appear to have analogues in wildfire risk.

Although there are few meta-analyses involving CV measures of wildfire risk mitigation, there is a wealth of literature on conducting economic meta-analyses to provide guidance on best practices. Besides general resources on meta-analysis such as Borenstein et al.(2009), several papers exist on meta-analysis in economics specifically. Nelson and Kennedy (2008) document early uses of meta-analysis in natural resource economics and provide a series of ten “best practices” for producing quality meta-analysis. These include (but are not limited to) documentation of coding procedures, model specification tests, sensitivity analysis, and addressing the problem of publication bias. van Houtven (2008) provides guidelines for meta-analysis of WTP values specifically, noting some of the key challenges in working with WTP data and how to best address them.

4. DATA COLLECTION METHODS

Constructing the Meta-Sample

Data used in the meta-analysis was obtained from a literature review of relevant studies. Google Scholar was the primary search engine used. Searches were also made using the Environmental Valuation Reference Inventory and EconLit, though these did not contribute many additional studies not found in Google Scholar. The initial search was conducted in October 2018 and returned 84 studies that were saved for closer examination. Searches were run again in January 2019 and May 2019 to obtain studies that might have been missed, or that were published in the interim. Three new studies were saved for future consideration from the January search, and none from the May search. Several additional studies were located through references in found studies (none of these, though, were ultimately included in the meta-sample). Studies found included both published papers and papers from the so-called “gray literature” (e.g. dissertations, government reports, etc.).

After the initial search was completed and studies were reviewed, a set of criteria was established for determining inclusion into the final datasets, to ensure meaningful comparisons. For the CV studies, these criteria were:

1) Estimates must have been obtained via the contingent valuation method, whether in the dichotomous choice, polychotomous choice, or open-ended format.

2) The program being valued must reduce wildfire risk to homeowners in the area, and this reduction in risk must be among the primary goals of the program.

3) The study must have used some means of numerically conveying the reduction of risk from the program being valued, whether in absolute (i.e. a 0.5% reduction in wildfire risk per year) or relative terms (i.e. wildfire risk is 25% lower per year).

These requirements excluded surveys that provided useful information but would have made comparison difficult. Criterion 2 eliminates studies such as Loomis and Gonzalez-Caban (1994), which measured WTP to protect old-growth forests that provided habitat to the northern spotted owl, but was not adjacent to significant structures. It also excludes any surveys focusing on the effects of wildfire on the outdoor recreation industry. Criterion 3 eliminates studies such as Talberth (2006), which provided WTP estimates for wildfire risk reduction using CV, but did not provide a measure of the reduction in risk from the program.

Originally, studies outside the United States and Canada were to be included in the meta-sample. Only two CV studies were found outside the U.S., and neither presented mean WTP estimates for a specific program. Therefore, the analysis was limited to studies in the US; results should not be assumed to generalize to anywhere else. Ultimately, 33 studies were included in the final meta-samples; 24 CV studies, and 9 hedonic studies. From the CV studies, 75 estimates of WTP for various programs were used. From the hedonic studies, 12 estimates were obtained. Table 1, on the next page, describes the papers used in the final meta-samples.

Table 1. Description of surveys used in the meta-sample.

Authors	Year	Published?	Location	Avg. sample size	# of est. used	Programs being valued	Public or private program?	Population(s)
Fried et al.	1999	Yes	Crawford Co., Michigan	50	9	Fuel clearing (various kinds)	Both	General
Loomis et al.	2000	No	Florida (statewide)	1492	3	Prescribed burn, fuel clearing, herbicide	Public	General
Loomis et al.	2002	Yes	Northeastern Florida	301	8	Prescribed burn, fuel clearing	Public	English, Spanish speakers
Loomis et al.	2002	No	CA, FL (statewide)	639	15	Prescribed burn, fuel clearing, herbicide	Both	General
Gonzalez-Caban	2004	No	Pablo/ Browning; statewide (Montana)	178	4	Prescribed burn	Public	White, Native American
Kaval	2004	No	Front Range/ central Colorado	86	3	Fuel clearing (various kinds)	Public	General
Loomis et al.	2004	No	California (statewide)	127	2	Prescribed burn	Public	White, African American
Christman	2014	No	Nevada (statewide)	2589	12	Unspecified	Both	General
Katuwal et al.	2015	No	Flathead Co., Montana	586	3	"Expanded wildfire management program"	Private	General
Tabatabaei	2015	Yes	Larimer Co. Colorado	235	2	Prescribed burn, biofuel conversion	Public	General
Gonzalez-Caban	2017	Yes	Florida (statewide)	560	10	Prescribed burn, fuel clearing, herbicide	Both	Whites, AA, Hispanic

For hedonic studies, the rules were:

1) Estimates must have been obtained through hedonic valuation methods. Some flexibility was allowed for the choice of functional form, though the majority of studies found used log-linear forms.

2) The value being estimated is the change in home price attributed to a nearby wildfire.

2) Estimates of this value must be expressed as a percentage of the price of the house.

Criterion 2 eliminated some studies, such as Kim (2004) and Champ (2010), which provided value estimates for forest density reduction and fire-prone characteristics of housing structure, respectively. Eight studies were included; this did not yield enough observations to conduct meaningful meta-analysis.

Obtaining Supplementary Data

It is important to distinguish between the objective and subjective probability of wildfire. A household's decision to participate in a risk-averting activity is influenced by subjective risk perceptions. A household's subjective risk assessment may or may not agree with an objective assessment of wildfire likelihood, and objective risk information, if it is available, may help determine subjective risk. Since subjective risk is not observable, some proxy for subjective risk is needed. Two proxies are considered, both based, at least partially, on information from outside the surveys: simulated burn probability GIS data, and estimated time interval since the most recent fire causing significant property damage in the area.

Burn probability GIS raster data from the US Forest Service (Short et al. 2016) were used to obtain an estimate of the objective baseline risk of wildfire for each study area. BP data is simulated data using the USFS's FSim fire simulator. Raster values indicate the proportion of annual simulations (out of 10,000 to 100,000 simulations run) in which a given pixel was burned by wildfire. BP estimates for each study area were obtained by drawing an approximation of each study area, using this approximation to and calculating the average burn probability for each study area. Since urban areas, bodies of water, and highways are assigned burn probabilities of 0, and the study is concerned with determining the burn probability of burnable area, pixels with burn probability 0 were excluded from these calculations. Average probabilities (expressed as percentages) were rounded to the nearest tenth, providing a simple estimate of the objective baseline risk of wildfire in the area. Obtaining baseline risk information in this manner provides the advantage of standardized assessment of risk across study areas. Some studies did not report a measure of baseline risk; those that did obtained these measures from a variety of sources. Using one source for objective risk information allows for a more consistent basis for comparison across studies. There are, however, some drawbacks to this method. For one, the USFS BP data is from 2016; conditions at the study sites may have changed since the survey took place. However, the estimates of baseline risk obtained from the studies are so wildly different from each other that the BP data was judged most effective for the purpose of comparing the effects of baseline risk across studies.

As mentioned previously, though, it is the subjective probability that drives households' WTP decisions, and objective risk information (if present) is merely one factor that determines a household's subjective risk assessment. While subjective probability cannot be observed directly, one potential proxy is the fire history of the area. Following a large fire event, households are likely to revise their evaluation of the probability of a wildfire destroying their own home. As time elapsed since the fire increases, the memory of the event fades, and new residents (who may or may not have previous experience with fire) move into the area. These events may combine to decrease subjective risk perception for households in the area of the fire. Therefore, one might expect to see lower WTP values from areas where the last fire took place further in the past. Bin and Landry (2013) find evidence for this effect in the flood market; Beltran et al. (2018) adds to this by pointing out that "all study locations possess a prior flood history the consequences of which may still be present." Therefore, this study includes a variable to account for the time elapsed since the last major fire event.

Determination of this variable presents several challenges. For one, determining what is a "major fire" invites some degree of subjectivity. Thus, effort was made to use consistent criteria for defining a fire as "major". The primary criteria used were whether the fire caused significant damage to or destruction of multiple structures, and whether multiple news outlets reported on the damage caused by the fire. This is an adaptation of the criteria used by Beltran et al. (2018) to determine the flood history of study areas for hedonic studies where flood history was not mentioned. In many cases, the most recent fire was specifically mentioned in the paper. When this was the case, the fire mentioned in the paper was used as the most recent fire. When the paper did not report the most recent fire, fire occurrence data from the USFS and Google searches for news articles were used to make informed decisions about the fire most likely to have been the most recent. The USFS has publicly available GIS data on fire occurrence dating back to 1980, including fire names and acreage burned. This was used to identify fires; the names of the fires were then searched with Google. Some surveys covered an entire state, rather than a specific county or counties. For these surveys, major fire events were defined as those events with more destruction (and news coverage) than usual.

Given the lack of consistent information regarding the dates of surveys, and the subjectivity in defining a "major" fire event, the time since fire variable is demarcated in years.

Several functional forms for this variable were considered in addition to the linear form: using the natural log of the time variable, the square root of the variable, and a ratio form (the same transformations used by Beltran et al. (2018) and others in the literature for floodplain risk analysis).

Income statistics were missing from several studies. Missing income values were filled in using US Census tract-level (where appropriate) and county-level data. As with WTP estimates, all income estimates were converted to 2017 dollars using CPI.

Table 2. Summary statistics for variables used.

Variable	Description	N	Mean	Std Dev	Min	Max
YR	Year of study publication (earliest =1)	71	11.591	6.964	1	19
PUBLISH	=1 if study was published in an academic journal	71	0.408	0.495	0	1
SSIZE	Sample size	71	799.126	962.878	12	3504
SE	Estimate's standard error (if given)	45	125.740	226.464	0.84	880.068
WTP2017	WTP estimate (annualized and converted to 2017$)	71	299.558	242.733	3.2	953.69
LN_WTP	Logged WTP estimate	71	5.2153	1.1724	1.1631	6.8603
_2017INC	Median household income (converted to $2017)	71	62511	18528	33899	89399
LINC2017	Logged WTP estimate	71	10.997	0.307	10.431	11.400
dpYEAR	Annualized absolute % reduction in risk	71	0.702	0.960	0.05	4
LN_DP	Logged dpYEAR	71	-0.946	1.024	-2.995	1.386
BP	USFS burn probability estimate for study area	71	0.606	0.337	0.1	1.25
NO_BLINE	=1 if baseline risk was not included in survey	71	0.323	0.471	0	1
Y_S_FIRE	Years since the last major, destructive fire near area	71	1.538	1.264	0.25	4
LN_YSF	Logged Y_S_FIRE	71	0.048	0.946	-1.386	1.386
SQRTYR	Square root of Y_S_FIRE	71	1.136	0.499	0.5	2
EAST	=1 if study area is in the eastern US	71	0.521	0.503	0	1
CALI	=1 if study area is in California	71	0.140	0.350	0	1

FLA	=1 if study area is in Florida	71	0.394	0.492	0	1
COLO	=1 if study area is in Colorado	71	0.070	0.257	0	1
ROCKY	=1 if study area is in the Mountain West (CO/MT)	71	0.169	0.377	0	1
WCOAST	=1 if study area is on/near the West Coast (CA/NV)	71	0.310	0.466	0	1
PRES	=1 if program valued uses prescribed burning	71	0.338	0.476	0	1
CLEAR	=1 if program valued uses standard fuel clearing	71	0.465	0.502	0	1
HERB	=1 if program valued uses herbicide	71	0.183	0.390	0	1
OTHER	=1 if program valued uses some other method	71	0.282	0.453	0	1
PUBLIC	=1 if public program	71	0.676	0.471	0	1
DI_CH	=1 if dichotomous choice question	71	0.690	0.465	0	1
POL_CH	=1 if polychotomous choice question	71	0.183	0.390	0	1
OPEN	=1 if open-ended question	71	0.127	0.335	0	1
POC	=1 if respondents are entirely non-white	71	0.183	0.390	0	1
WUIONLY	=1 if respondents only live in the WUI	71	0.352	0.481	0	1

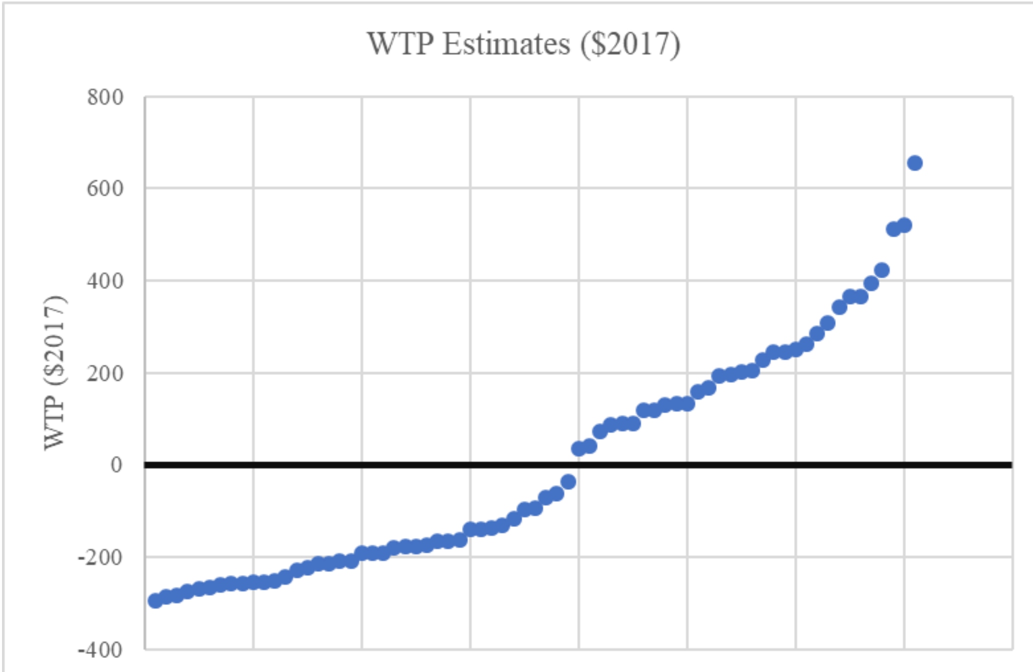

Figure 1. WTP estimates used in the meta-sample (converted into 2017 dollars)

5. WLS MODELS

Fixed Effects vs. Random Effects Specification

There are two broad categories of meta-analysis: fixed effects and random effects. As discussed in van Houtven (2008), these terms have different meanings in meta-analysis than their common usage in econometrics. Fixed effects models assume that there is one true effect size, and that all samples included in the meta-analysis draw from this one true effect size. Random effects sizes assume that there are meaningful differences among studies; there is no one “true” effect, but a composite of many effects with individual differences between them (Borenstein et al. 2007). In practice, the primary difference is in the handling of assignment of weights. Since fixed effects models assume one true effect, any study will be drawing from that effect; differences in variance are accounted for by differences in the sample size. Fixed effects models are said to only weigh to correct for the within-sample variance. Random-effects models, on the other hand, also account for between-study variance when assigning weights. This may result in more efficiency, but it also results in bias.

Given the heterogeneity likely present in the studies in this meta-sample, the random effects specification is preferred. Unfortunately, the random effects model requires variance information, which is not always present for WTP estimates (this will be discussed further shortly). Thus, a fixed-estimates model specification is used, noting the result of this specification is that variance estimates for the parameter estimates are lower than if the random-effects weights were readily computable. Borenstein et al. (2007) note that the most commonly used weight in fixed-effects models, the inverse of the variance, theoretically produces similar results to weighting by sample size. Van Houtven (2008) also describes sample size weighting as potentially providing "the most practical second best alternative" to the random-effects specification. Thus, this study weights WTP estimates according to sample size.

An issue related to the fixed vs random effects specification is that of controlling for the fact that multiple estimates are obtained from a single study. This gives the meta-sample a panel structure, with each study representing a cluster of data. Methods of addressing panel data are also referred to as "fixed effects" and "random effects," but as van Houtven (2008) points out, when variables have little variation within the sample, these techniques for working with panel data become problematic. This is the case in this meta-sample; many variables used in the meta-regression (such as income, elicitation method, and the proxies for subjective risk) do not vary much, if at all, within the study. Another method used in meta-analysis of WTP values is to report clustered standard errors, which "correct[s] the standard error estimates for potential correlation within clusters and unequal variance across clusters" (van Houtven 2008). All results reported in this thesis will report ordinary, heteroskedasticity-robust, and cluster standard errors.

Weights

Most studies included in the sample provided more than one estimate, whether because multiple risk reduction techniques were valued, or due to differences among various demographic groups, or other reasons. Therefore, the weights were refined by dividing the sample size of the estimate by the number of estimates the study contributed to the meta-sample. By doing this, studies are not overrepresented because they contributed a large number of estimates. This weight is very similar to one used by Bertran et al. (2018) in their meta-analysis of hedonic studies of the effects of flood risk, with the exception that the Bertran study used the square root of the average sample size for the study, rather than the sample size. Weighting by sample size of the observation was determined more appropriate here, though, since many estimates provided observations with greatly different sample sizes.

Developing the Models

Preliminary models used the baseline risk provided by the individual studies as a measure of objective risk, with USFS BP data filling in when baseline risk information was missing (from either the survey instrument or the study itself). This variable proved to be insignificant, with high standard errors, but the control variable indicating use of the BP variable was significant. Thus, use of baseline risk provided by studies was thrown out in favor of using BP data for all studies. (As mentioned earlier, this also provides the advantages of having a standardized assessment of objective risk. It also provides a measure of objective risk that does not have any influence on responses.) The control variable for the use of BP was then amended to become a control variable for the provision of some baseline risk measure in the survey itself, expressed as a probability. (Some studies provided baseline risk by the acreage burned by wildfires per year. While there is enough information present within the survey to obtain an estimate of baseline risk, it is not immediately apparent how to do so, and some respondents may not think to calculate baseline risk when reading the survey. These studies, then, were considered to not provide baseline probability.) Even when the BP data for all studies was used as the proxy for subjective risk, results were not significant and standard errors were high, with rare exceptions, which will be discussed later.

Two different functional forms of the data for time since the most recent major fire were used. Besides the untransformed, linear form, a square root transformation of the data was also tested. The linear form tended to provide more stable estimates, so it is presented as the default proxy for subjective risk perception. Selected results using BP data as well as the square root transformation are also presented in the appendix.

Other variables accounted for various known differences across estimates; these were tested for regardless of whether these factors were significant in the original study. For example, several studies tested for differences in WTP across various ethnic groups.

While it is potentially helpful to introduce geographic variables to control for these effects, doing so creates its own potential problems. Due to the low number of individual studies in the meta-sample, geographic variables create potential multicollinearity issues with the variables for income, time since the most recent major fire, and WUI-only studies. It is best to include geographic variables as controls for systematic differences due to the geographic location. Only a few systematic differences on a regional scale are known. Fires burn differently in the eastern and western US. This does not affect the probability of wildfire (burn probabilities are far too localized), but it may affect the unobserved amenity values of residents in these areas. Otherwise, the geographic variables are assumed to represent unknown trends, and are mainly used to determine the model's predictive power and test for robustness of the variables of interest to the model specification.

Breush-Pagan and White tests for heteroskedasticity (a common phenomenon in meta-analysis) were run. Only a few of the models showed evidence of heteroskedasticity under these tests; for those that did, the form of the heteroskedasticity is unknown. Therefore, heteroskedasticity-robust standard errors are reported for all models.

Publication Bias

Publication bias is a common problem in meta-analysis, not only in economics but in all fields where meta-analysis is commonly used. When publication bias is present, the meta-sample is not a true representation of all relevant research. Academic journals have a tendency to publish studies with significant results. When a meta-sample contains only published studies, the resulting estimates will be biased in favor of the direction of the significant results. Publication bias can be addressed partially by including studies from the "gray" literature (dissertations, government agency reports, working papers, etc.), as was done in this study. However, even when non-published studies are included, best practices in meta-analysis include testing for publication bias (Nelson and Kennedy 2008).

The simplest method of testing for publication bias is by inspecting a funnel plot for asymmetry, as demonstrated by Egger (1997). Funnel plots graph standard errors against effect sizes. Larger sample sizes ought to result in smaller standard errors, so if the meta-sample is representative of the true population of studies, the funnel plot should look like a pyramid symmetrical around the mean. If the funnel plot is asymmetrical, it is likely that there are more studies with significant results than in the true population of studies. Another straightforward method is to regress effect sizes against their standard errors, as was done in Beltran et al. (2018), to name one example. If the parameter for the standard error is significant, there is evidence for publication bias.

Contingent valuation studies, however, present a set of obstacles to accurate testing for publication bias. Chief among these is the lack of "true" standard errors associated with WTP estimates from CV studies. Note that both of the above tests rely on the use of standard errors. Standard errors for WTP estimates from CV are constructed from any of several methods and are thus approximations of the “true” standard error. While these approximations result in “reasonably accurate” estimates of standard errors, there are still some uncertainties (Hole 2006). For example, the commonly used Delta method assumes a symmetrical distribution around the estimate, which may or may not actually be the case for WTP measures (Ibid.) More to the point, these estimates are not always reported for WTP estimates. Further complicating matters is the fact that both tests operate on the principle that standard errors ought to decrease as sample size increases, and this dataset does not reliably exhibit that characteristic. There may be some other source of heterogeneity that contributes to the effects shown.

Nonetheless, meta-regression tests for publication bias were conducted for the subset of estimates from studies that provided this information. 51 estimates were included in these tests. Logged WTP (unadjusted for inflation; annualized when necessary) was used for the effect size. The resulting parameter for SE is positive and highly significant. This indicates that, if the observable effects are indeed due to publication bias, estimates from the meta-sample will be biased upwards relative to their “true” values. It should be noted, however, that this does not constitute proof of publication bias, for the reasons listed above.

At any rate, it is necessary to correct, as much as possible, for this apparent publication effect. Beltran et al. (2018) divide the effect sizes by their standard errors. This was attempted in this study for the subset of studies with estimates of standard errors, but doing so did not force the parameter in the subsequent meta-regression test to take on an insignificant value. Still, there are other ways of accounting for the apparent publication effect. In their discussion of the use of meta-analysis in environmental economics, Rosenberger and Johnston (2009) note the use of dummy variables in economic meta-analysis to control for the effects of published studies on the meta-sample. This is the approach used in this study.

Models and Results

Results for selected models are given below, along with regular, heteroskedasticity-robust, and cluster standard errors. Results for more models may be found in the appendix. All models specified use the following variables:

1) a proxy for subjective risk (whether USFS BP data or the time elapsed since the last major fire, or a transformation thereof)

2) the absolute reduction in risk offered by the program, specified as linear or logarithmic

3) estimated median income for the study area

4) controls for the type of program valued (e.g. prescribed burning, fuel clearing, or some other type)

5) some form of controls for the quality/heterogeneity of the study. This includes the dummy indicating whether a study was published in an academic journal; whether the study only surveyed WUI residents; whether protest votes were excluded from the sample used for WTP estimation; and whether the elicitation method was dichotomous choice or not.

The dummy variable NO_BLINE, a control for the lack of numerical, objective baseline risk information in the survey instrument, appeared to be significant after initial testing. It is also included in most models. However, in some cases (most often when BP was used as the proxy for subjective risk) introducing the YR variable to the model caused severe multicollinearity issues, which were addressed by excluding NO_BLINE from the model.

The following model describes the primary results from WLS estimation and and is presented both with and without a control for the year of the study. They include most controls for heterogeneity among studies, but not geographic variables, which are judged to be unreliable.

It should be noted (as will be discussed in Chapter 6) that the parameter estimates for LINC2017, NO_BLINE and DI_CH, though significant, are sensitive to model specification and should be interpreted accordingly.

Table 4. Regression results, primary model specification, with YR as a control.

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	2.5757	0.6040***	0.6342***	0.6249***
dpYEAR	0.2585	0.0765***	0.0782***	0.0185***
Y_S_FIRE	-0.6228	0.1613***	0.1026***	0.0566***
NO_BLINE	0.7312	0.3279**	0.2108***	0.1293***
WUIONLY	0.2405	0.2478	0.2588	0.1864
POC	-0.0840	0.3626	0.3211	0.2699
PROTEST	0.0805	0.3067	0.1595	0.1309
DI_CH	0.1782	0.3574	0.2950	0.1548
PRES	0.6452	0.1999***	0.2138***	0.3025*
CLR	0.4470	0.2431*	0.2062**	0.2403*
PUBLIC	-1.3007	0.1823***	0.2209***	0.3090***
PUBLISH	1.3964	0.2658***	0.2402***	0.1152***
YR	-0.1261	0.0277***	0.0263***	0.0238***
Intercept	-21.4436	6.6015***	6.8226***	6.6004***
N	71			
R-Sq.	0.7024			
Adj. R-Sq.	0.6345			

***: parameter estimate is significant at the 1% level. **: 5% level. *: 10% level.

Table 5. Regression results, primary model specification, without YR as a control.

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	1.1763	0.6017*	0.5130**	0.6876
dpYEAR	0.3218	0.0871***	0.0897***	0.0600***
Y_S_FIRE	-0.6320	0.1867***	0.1389***	0.2406**
NO_BLINE	1.2402	0.3567***	0.2700***	0.4267**
WUIONLY	-0.0364	0.2780	0.2549	0.2313
POC	-0.3936	0.4123	0.3824	0.3340
PROTEST	0.7443	0.3122***	0.2685***	0.4631
DI_CH	0.6164	0.3983	0.2834**	0.2954*
PRES	0.4697	0.2270**	0.1959**	0.2585***
CLR	0.2069	0.2747	0.2136	0.1814
PUBLIC	-1.0170	0.1982***	0.2321***	0.4988*
PUBLISH	1.5549	0.3050***	0.2575***	0.2962***
Intercept	-8.1871	6.8561	5.8104	7.6524
N	71			
R-Sq.	0.5943			
Adj. R-Sq.	0.5104			

The following model include all the variables to be tested and is presented both with and without a control for the year of the study. Including the geographic control variables introduces high levels of multicollinearity. This is especially the case when the control for the year of the study is introduced, as certain variables start to become linear combinations of others. Further examination of the geographic controls gives strong reason to suspect they are describing unintended effects. For example, studies in the Eastern U.S. typically examine lower-income and/or minority groups, while studies in the Rockies tend to examine higher-income households and record results from WUI residents-only studies. Therefore, the geographic control variables were excluded from all other models.

Table 6: Regression results, all variables included, with YR as a control.

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	4.1672	1.0100***	0.9700***	1.1614***
dpYEAR	0.2467	0.0754***	0.0801***	0.0154***
Y_S_FIRE	-0.8526	0.2078***	0.1548***	0.1024***
NO_BLINE	0.6889	0.3737*	0.2553***	0.1609***
WUIONLY	0.7432	0.3669**	0.4194*	0.4388
ROCKY	0.4673	0.4209	0.3132	0.1171***
EAST	1.1296	0.5592**	0.5421**	0.5357*
POC	0.2914	0.4081	0.2651	0.2637
PROTEST	-0.1960	0.3346	0.2089	0.2369
DI_CH	-0.1463	0.4503	0.3622	0.1786
PRES	0.6656	0.1986***	0.2225***	0.3246*
CLR	0.3752	0.2431	0.1811**	0.2114
PUBLIC	-1.2800	0.1801***	0.2211***	0.2885***
PUBLISH	0.8566	0.3760***	0.3793**	0.3001***
YR	-0.1556	0.0355***	0.0321***	0.0270***
Intercept	-38.8410	11.1806***	10.6751***	13.0228***

N	71
R-Sq.	0.723
Adj. R-Sq.	0.6474

Table 7: Regression results, all variables included, without YR as a control.

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	2.2109	1.0424**	0.9370**	1.6929
dpYEAR	0.2967	0.0858***	0.0893***	0.0479***
Y_S_FIRE	-0.4997	0.2205**	0.1823***	0.3441
NO_BLINE	1.5029	0.3730***	0.3038***	0.5041**
WUIONLY	0.3473	0.4092	0.4408	0.5102
ROCKY	-0.7094	0.3727*	0.3112**	0.3998
EAST	0.1263	0.5870	0.5464	0.8001
POC	-0.1119	0.4575	0.3252	0.3612
PROTEST	0.4224	0.3491	0.2616	0.3604
DI_CH	0.9612	0.4286**	0.3568***	0.4491*
PRES	0.5820	0.2275**	0.2212**	0.3051*
CLR	0.3268	0.2795	0.2141	0.1695*
PUBLIC	-1.0574	0.1988***	0.2377***	0.4809*
PUBLISH	1.5440	0.3932***	0.4042***	0.4085***
Intercept	-20.1963	11.8964**	10.7148*	19.1115
N	71			
R-Sq.	0.6264			
Adj. R-Sq.	0.533			

Since there appears to be a significant difference in WTP estimates between studies that do and do not include a baseline risk calculation in the survey instrument, it is also worth reporting results from the subset of studies that did include baseline risk. For one, it becomes possible to test the impact of the baseline risk value stated on the survey, without any imputed values from the BP data. There are, however, some drawbacks with modeling this subset: the small sample size requires some control variables to be left out of the model, to address multicollinearity issues. Results with the baseline risk information, and time since recent fire, as subjective risk perception proxies are reported below (including controls for the year of the study).

Table 8. Subset model, provided measure of baseline risk as subjective risk proxy, control for year of study

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	2.5330	1.1179**	1.1626**	1.2931
dpYEAR	0.2425	0.0912**	0.0746***	0.0162***
BLINE	0.0193	0.0360	0.0348	0.0075*
POC	-0.5629	0.7397	0.7837*	0.4452
DI_CH	1.2545	0.6364	0.6383**	0.2172***
PRES	1.1667	0.3723***	0.4403**	0.4966*
CLR	1.2194	0.6091*	0.5928**	0.5077***
PUBLIC	-1.4747	0.2263***	0.2430***	0.2119***
PUBLISH	1.9845	0.4572***	0.5940***	0.1520
YR	-0.0450	0.0651	0.0491	0.0484
Intercept	-24.3931	12.3145*	12.7896*	14.0878
N	48			
R-Sq.	0.6816			
Adj. R-Sq.	0.5956			

Table 9: Subset model, years since fire as subjective risk proxy, control for year of study

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	2.3712	1.1581**	1.2222*	1.4186
dpYEAR	0.2487	0.0877***	0.0769***	0.0037***
Y_S_FIRE	0.3858	0.6381	0.3748	0.5019
POC	-0.6237	0.7479	0.7929	0.4992
DI_CH	1.8566	1.2583	0.8175**	0.7805*
PRES	1.1844	0.3736***	0.4491**	0.5053*
CLR	1.6427	1.0091	0.5621***	0.5407**
PUBLIC	-1.4856	0.2270***	0.2497***	0.2130***
PUBLISH	2.2150	0.6559***	0.6216***	0.3790***
YR	0.0133	0.1229	0.0872	0.1150
Intercept	-24.8159	12.3067*	12.5789*	13.4398
N	48			
R-Sq.	0.6823			
Adj. R-Sq.	0.5965			

CHAPTER 6. INTERPRETATION AND DISCUSSION

The model presented in Table 4 represents the "best" model, in that it controls for several forms of heterogeneity across studies while keeping multicollinearity relatively low (VIFs for all variables in the model were under 10). However, relying on one model only ignores insights from other models. Some parameter estimates are quite sensitive to model specification, while others are not. The Interpretation section of this chapter will place results in the context of the set of estimated models, while the Discussion section will place the interpretations in the light of existing literature.

Interpretation

The coefficient for LINC2017, logged income converted to 2017 dollars, may be interpreted as the income elasticity. The literature gives mixed evidence on the effect of income on WTP for wildfire mitigation, and this is reflected in the degree to which parameter estimates are affected by model specification. This variation in the estimates produced by the models also reflects the error introduced by 1) obtaining income estimates for studies from outside sources, 2) uncertainty in using CPI conversions when the base year is unspecified, and 3) the imperfections of the CPI measure itself. In the primary model, LINC2017 is significant at the 1% level when YR is included, but has less statistical significance when YR is not included. Across the set of models, LINC2017 tends to take values between 2.5 and 3. This suggests WTP for wildfire mitigation may be somewhat elastic with respect to income. Higher quality estimates would make claims about the effects of income more reliable, however.

The coefficient for dpYEAR (absolute risk reduction per year) is significant at the 1% level regardless of the control variables used or the method used to compute standard errors (except for a couple instances when it is only significant at the 5% level). It is also highly robust to model specification; most models estimate dpYEAR at around .25. Thus, the model provides evidence that increasing the level of absolute risk reduction by 0.1% (dpYEAR is denominated in percentages) will increase mean WTP by around 2.5%.

Among the proxies for risk perception, Y_S_FIRE (years since the last major property-destroying fire in the area) outperformed both its transformation SQRTYR and the burn probability data BP. The parameter estimate for the BP data is never significant unless the YR variable is included in the model. Even then, the estimates are not reliably significant using cluster standard errors, and if Y_S_FIRE and BP are run in the same model, the coefficient for BP becomes much smaller and insignificant. Overall, it seems burn probability is an unreliable estimator of WTP in models where all observations are included.

The sign on the coefficient for Y_S_FIRE and its transformations is always negative, in line with the effects seen in the literature on flooding. This does provide some evidence that WTP measures for wildfire risk reduction programs are influenced by recent destructive fires in the area. Caution should be used in interpretation here. While every effort was made to determine the fire most likely to have made an impact, there is still a degree of subjectivity present in these efforts, and the time scale used (years) is longer than that used in the flooding literature (months). It is also important to note that Y_S_FIRE is insignificant when NO_BLINE is excluded from the model; much of the negative effect may be coming from the studies without baseline risk estimates. SQRTYR had much greater variation in both magnitude and significance; since it is far more sensitive to model specification than Y_S_FIRE, the square root transformation does not appear to be as useful.

The effects of the subjective risk proxies appear to differ depending on whether the full set of observations or the subset of observations from studies where baseline risk was provided (hereafter referred to as “the subset”) is considered. Because of increased multicollinearity effects with only 48 observations, severely affecting CLR (one of the variables of interest), several control variables were excluded from the subset model (specifically, WUIONLY, PROTEST, and POC) until the variance inflation factors for all variables reached levels under 10. The coefficient for Y_S_FIRE is positive and insignificant under this model specification, but unfortunately, multicollinearity issues associated with the small sample size complicate investigating the role of Y_S_FIRE in the subset further.

Other proxies for subjective risk perception (BP and BLINE or the baseline risk estimate provided by the survey) can also be tested on the subset. Interestingly, BLINE appears to have no effect on WTP. Whether or not baseline risk information is provided seems to have an effect on WTP, but the actual information does not. BP is only significant in the model where WUIONLY is removed and YR is included.

Interpretation of the geographic control variables is highly problematic. ROCKY (and WCOAST) are sometimes significant and sometimes not, change significance depending on the standard error calculation, and change signs depending on model specification Also, EAST is moderately correlated with several other variables. These variables appear to be capturing information other than systematic differences across geographic regions. Because of this and the multicollinearity issues caused by their inclusion, they do not appear to add any explanatory power with their inclusion. As for elicitation methods, the coefficient for DI_CH is positive, except for in the model where the geographic controls are included; there, the coefficient is slightly negative and insignificant. The significance of DI_CH changes a great deal depending on model specification and standard error calculation method; it seems to have value as a control, but little explanatory value.

The coefficient for PUBLIC is negative and significant regardless of model specification and is fairly robust to model specification in its value, with a coefficient of around -1.25 if YR is included and around -1 if YR is not included. This indicates a general preference for privately administered programs over public programs, other things being equal. Excluding WUIONLY does not have a major effect on PUBLIC, indicating that the general preference for private programs is likely held by WUI residents and non-WUI residents alike. The coefficient for PUBLIC is slightly more negative in the subset model, with a value of around -1.45 regardless of subjective risk proxy or whether YR is included.

The behavior of PRES (prescribed burning) and CLR (fuel clearing) is somewhat complicated, which reflects the dataset as well as underlying preferences. PRES and CLR are both consistently positive; the excluded category is herbicide and “other,” so the positive sign indicates general preferences for prescribed burning and clearing over these methods. Generally, coefficients for PRES are significant at the 1% level unless cluster SEs are used, which reduce the significance to the 5% or 10% level. The coefficient for PRES is higher across model specifications when YR is included, which likely reflects the growing support for prescribed fire over time found in the literature.

CLR, on the other hand, is much less consistent in its significance. This should not be interpreted as a preference for prescribed fire over fuel clearing. The public/private context for fuel clearing likely matters more than for prescribed burning. Among WUI residents, Nelson (2005) found support for "prescribed burning for fuel reduction when it was well done by qualified professionals." The literature indicates WUI residents prefer undertaking clearing activities on their own land personally (or via contractor) to government-run programs. With the limited number of studies, it is hard to quantify the complexity of attitudes toward clearing. More studies would allow for making distinctions between private and public clearing activities, homeowners maintaining defensible space vs. clearing of private and public lands, etc.

For the subset model, preferences for clearing are often greater than for prescribed burning, even when YR is included. It is unclear whether this is because of the demographics of the subset model (which does include more WUI-only surveys) or because risk information actually changes preferences for clearing activities. Talberth et al. (2006) found that households in higher-risk zones tended to invest less in public activities, viewing private activities as safer. It is unclear from the data in the meta-sample, though, whether the presentation of baseline risk information leads households to conclude they are in a high-risk or low-risk area.

PUBLISH is positive and virtually always significant, indicating that there is some effect associated with publication. Since the gray literature in the meta-sample includes authors who also have published papers in the sample, this should not necessarily be interpreted as meaning that higher-quality studies lead to higher WTP estimates, as some economic meta-analyses have interpreted this dummy variable. YR is intended to capture the effect of advances in CV methodology over time. Across most specifications it reports a small but significant negative effect. It also often introduces multicollinearity issues, particularly with the geographic control, LINC2017, and NO_BLINE variables. This is likely due to the small set of surveys the meta-sample draws from.

Discussion

Results indicate the importance of risk communication as a determinant of WTP values. These findings are similar to those of Loomis (1993), who found that WTP estimates for hazardous waste management in California differed with different methods of communicating risk (though both methods in the Loomis study, unlike here, communicated baseline risk). It is also consistent with the findings of Talberth et al. (2006) and Katuwal (2015), who found that risk information influenced WTP and/or affected mitigation behavior and Mozumder (2009), who found WUI residents' WTP for risk information exceeded the costs of producing and disseminating the information. Katuwal (2015) found that less information provided in the survey instrument leads to higher WTP. The meta-analysis in this thesis controls for one difference in information provided by the surveys in particular: whether there is a clear, prominent measure of baseline risk (as it happens, the Katuwal study is one such paper).

There is no clear explanation for why this effect occurs. One potential cause is that, in the absence of baseline risk information, respondents overestimate the probability of wildfire in the area. This is an interpretation provided by Katuwal (2015) for their findings. A complicating factor in this interpretation is the findings of Talberth (2006) that WUI residents in high-risk zones tended to prefer private programs to public ones. Another possibility, consistent with the results from this meta-analysis, is that, in the absence of a baseline risk, respondents may think the absolute risk reduction is larger than it actually is when provided with relative reduction information.

The concept of prior information is not used in the meta-analysis, but it may help explain some of the results related to risk perception. Survey respondents have differing levels of information prior to taking the survey; as more information is provided, one might expect WTP values provided by the survey to converge (Katuwal 2015). WUI residents will likely have different levels of prior information than non-WUI residents. Additionally, residents within each individual study area WUI will have differing levels of information, and results typical in one area may not describe another. For example, Champ et al. (2013), in a survey of Colorado Springs residents, found that few residents in high-risk areas were aware that they lived in a high-risk area. WUI residents in another area may be more aware of the risks, whether through dissemination of risk information by local agencies, the average resident having lived in the area for longer, or any number of factors.

Prior information is unobservable in the meta-analysis, but its effects may help explain why objective risk does not appear to have an impact on WTP. If survey respondents have differing prior levels of information prior to taking the survey, then some will incorporate objective risk, and others will not. Depending on what prior information the average respondent at different study areas possesses, the average subjective risk assessment may correlate better with the objective risk for some studies than others.

Another potential explanation for the insignificance of the BP data is that WUI residents do take objective risk information into account, but the BP data is a poor measure of the particular objective risk pertinent to survey respondents. The BP estimate is an average of a (sometimes large) geographic region, and wildfire risk tends to be highly localized. Localization may be further compounded if WUI households consider the actions of their neighbors when making mitigation decisions, as shown by Shafran (2008) and others. If this is the case, average BP data is far too simplistic a representation of objective risk to be of use.

The sensitivity of the BP data to the inclusion of the YR variable is interesting. Ostensibly, the purpose of the YR variable is to account for advances in CV methodology

(Nelson and Kennedy 2008). It may also account for certain other time-based effects, such as the apparent control for increased acceptance for prescribed burning over time. One possibility, then, is that respondents are more aware of the risks of living in the WUI over time: perhaps agencies have done a better job of disseminating information over time. It is also important to keep in mind that the apparent significance of BP in these situations may be suspect, as BP data is still usually insignificant when using cluster SEs.

Baseline risk, as provided by the survey, appeared to have no effect on WTP estimates for the subset of studies that provided baseline risk. One interpretation of this is that, once wildfire risk is put in context, differences in absolute risk do not seem to matter because most differences are quite low. Most locations have a probability of less than 1% per year. For comparison to a more developed field, flood risk, this is analogous to being somewhere between the boundaries of the 100- and 500-year flood plain. Therefore, most WUI residents, when choosing WTP for wildfire risk reduction, are evaluating mitigation activities for an event that, on average, will not occur during their residency. Evaluated in those terms, initial baseline differences may seem irrelevant. It may also simply be that neither of the baseline risk measures were consistent enough to capture the actual effect of the initial risk.

The interpretation of baseline risk as being irrelevant because it is so small, however, is at odds with the highly significant values for absolute risk reduction under the framework of expected utility theory. If households did not consider a baseline probability of 0.8% vs 0.4% important, neither would they consider a reduction in probability from 0.8% to 0.4% worth paying for. There may be several explanations for this. For one, recall Dixit and Kreuse's (2000) assertion that averting activities still exhibit positive marginal benefits when intangible assets are present. The positive WTP for higher levels of risk reduction may be interpreted as households' desire for greater protection of intangible assets, without a great deal of sensitivity to the initial degree of loss. Another explanation is outside the bounds of traditional expected utility theory. This analysis uses expected utility theory as it is the standard theory in these surveys. Kahneman and Tversky's Prospect Theory states that people are more risk averse over losses. Thus, while people may not be sensitive to the initial level of risk, they may be willing to pay to avoid the risk of loss over the range of baseline risks.

The significance of time since major fire may be interpreted as an example of prior information influencing survey respondents. The most straightforward explanation for the effect invokes the Availability Heuristic, whereby the probability of an event is judged by the ease with which instances of that event are recalled (Tversky and Kahneman 1973). If the availability heuristic applies here, time since fire may be more important information for respondents with little experience with wildfire or who have not lived in the WUI for long. Champ et al. (2016) found that WUI residents in Colorado's Front Range did not update their perceptions of the probability of a wildfire (as recorded in 2007) following wildfires in 2010. Respondents did update their perception of the severity of the event, but not dramatically. Prior experience with wildfire and/or prescribed fire is shown to be a predictor of support for wildfire management tools in several studies (Jacobson 2001, Champ 2013). WUI residents who are more experienced, then, may have already formed their own opinions about wildfire risk.

One complicating factor is respondents' understanding of the effects of a wildfire event on subsequent wildfire risk. Since wildfires burn fuel, which must then be replenished over time, the occurrence of a wildfire decreases the probability of another wildfire in the burned area until the area recovers. It does not, however, affect the probability of areas that were not burned, meaning that subsequent wildfires can still potentially occur in the area if enough fuel remains.

WUI residents may feel that the probability of a wildfire may decrease following a wildfire, which is the opposite effect to the one shown in the meta-analysis. In the Champ et al. (2016) study, no evidence was found suggesting this effect, In another study, however, Champ et al. (2013) note a news article reporting on increased resistance to new local fire codes following a fire outside Colorado Springs; one resident expressed skepticism that another fire would occur in the area.

Results of the meta-analysis indicate several broad trends regarding mitigation methods: private programs enjoy higher WTP than public programs; prescribed fire and fuel clearing are preferred to herbicide; and the coefficient of prescribed fire is increased when the year of the study is controlled for, reflecting the increasing acceptance of prescribed fire over time descripted in the literature. These general trends do obscure some important details which are hard to explore in the meta-analysis due to the small sample size. For one, the fact that clearing produces less significant and often smaller parameter estimates than prescribed burning does not necessarily indicate a preference for prescribed burning over clearing. This is reflected in the results for the meta-analysis of the “baseline risk provided” subset: parameter estimates for CLR are higher than PRES in most of the model specifications tested, even when the year of the study is accounted for.

The literature indicates that the private/public context seems to matter more for clearing activities than for prescribed burning, and policy implications should be considered in this context. Fried (1999) found that more survey respondents assigned responsibility for protecting structures from wildfire to individuals (26%) than the government (21%). CV surveys that ask whether households already maintain defensible space do find some previous use of the practice, which is itself an indication of positive WTP for clearing. More CV studies would allow meta-analysis to better synthesize the information on risk mitigation by including model specifications that break down methods into public clearing, private clearing, public prescribed fire, and so on. It is possible that significant differences in support exist between private and public clearing programs. With only 71 observations (and, importantly, 11 studies) included in this meta-sample, classifying mitigation programs using technique and administration simultaneously might produce unreliable results.

7. CONCLUSION

Summary of Findings

WTP estimates from 11 surveys across the United States were combined in a meta-sample. Impacts of risk information and various measures of risk, intended as proxies to describe broad measures of subjective risk perception, were tested for their impact on WTP. Objective risk measures do not explain much of the variation in WTP estimates; it is uncertain to what degree this is due to a mismatch between risk perception and objective risk, and to what degree this is because wildfire risk is too localized to be adequately described over large areas. Full provision of baseline risk information in surveys produces significant differences in WTP estimates, with higher estimates for surveys that did not directly communicate baseline risk. Differences in the context of the survey, such as the demographics of respondents and the treatment of protest votes, did not appear to be primary determinants of WTP estimates. Results indicate a general preference for private programs over public ones. This preference becomes more complicated when considered alongside the method of risk reduction (i.e. prescribed burning, fuel clearing, etc.). The small sample size complicates detailed analysis of these preferences; future meta-analyses, with a larger pool of studies, may be able to better define trends in these preferences.

The findings of this meta-analysis may be useful to various parties. Policymakers may find the results useful in the context of benefit transfer. Results indicate an increase in WTP of approximately 2.5% for an additional tenth of a percent of reduction in the level of absolute wildfire risk. No clear evidence of large-scale differences in WTP between geographic regions in the U.S. was found; differences between studies may be more attributable to unobservable differences, and differences in methodology, than systematic trends among geographic region. Researchers may also be interested in addressing some of the issues discussed in this meta-analysis, in particular the effects of risk information, which is shown to play a major role in determining WTP estimates, and the effect of time passed since major fires on risk perception and WTP.

Further Areas of Research

Based on the findings of this meta-analysis as well as those of Katuwal (2015), and other studies in the literature, it is recommended that more research be undertaken to determine the effects of information conveyance on WTP measures for risk-reducing activities. Specifically, it appears necessary to further understand the effects of providing varying levels of information on WTP estimates from individuals of varying prior information levels. Besides testing for these effects, standardization of surveys for risk mitigation (which would aid in benefit transfer), would be facilitated by providing clear baseline risk information in the survey instrument. This information appears to be pertinent, aiding respondents in making informed decisions. In addition, there is some evidence for time elapsed since a major fire impacting residents' risk perceptions, though it is sensitive to functional form and based on subjective data collection methods, increasing the likelihood of having collected incorrect data. More research into this relationship also appears warranted.

Should future meta-analyses be conducted, they would benefit from increased sample size. While there were enough observations to conduct a meta-analysis of CV studies via OLS, the low number of surveys used inflates standard error estimates and creates potential issues with multicollinearity. This muddles the question of significance for some variables, which were significant depending on functional form. The sample size for hedonic studies was too small to perform OLS estimates; clearly, more studies in this field are necessary.

Because meta-analysis necessitates a thorough literature review, this stage of the process also illuminates gaps in the research. All CV studies found elicited WTP values to reduce risk to residents. Little is currently known about the effects on WTP for risk reduction for vacation homes specifically (which would likely have fewer intangible amenities), for example. This information would be especially relevant for areas such as Gatlinburg-Pigeon Forge, TN (which experienced a wildfire in 2016), as well as provide interesting opportunities for research into determinants of risk perception. There are also some locations within the US with high risk of wildfire that have not been studied with CV or hedonic methods, such as southern Idaho, which has the highest increase in assets exposed to wildfire loss according to Strader (2018).

Refining Future Surveys

Besides the small sample size, one of the major challenges this meta-analysis had to overcome was missing data that would be of tremendous help in establishing more reliable models and estimators. Benefit transfer has become a major tool for policy analysis (Moeltner 2009), and meta-analysis is one of the primary means of conducting benefit transfer, so including data that facilitates higher-quality meta-analysis would pay dividends for the accuracy of policy analysis. For example, changing the underlying assumptions of the model from fixed effects to random effects may potentially overcome some of the obstacles faced in this survey. Without standard error estimates from all surveys, though, calculation of the weights necessary for random-effects estimation becomes impossible. Therefore, it is recommended that CV surveys provide standard error and/or confidence interval information. Other missing data like income and (as mentioned above) baseline risk would also improve the results of meta-analysis by improving standardization of survey, allowing for easier comparisons between surveys. Still, the findings of this study largely fit well into economic theory and the existing literature on wildfire risk, which speaks well of the ever-increasing accuracy and validity of CV surveys.

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APPENDIX A. RESULTS FROM VARIOUS MODEL SPECIFICATIONS

Table 10: Primary model output with USFS burn probability data (BP) as proxy for subjective risk and control for year of study

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	3.4036	0.7802***	0.7735***	1.0204***
dpYEAR	0.2810	0.0822***	0.0766***	0.0427***
BP	1.0265	0.4559**	0.4128**	0.6170
NO_BLINE	0.0491	0.2693	0.2024	0.2590
WUIONLY	0.1719	0.2671	0.2781	0.2027
POC	0.5771	0.3728	0.3134*	0.2129**
PROTEST	-0.5008	0.3569	0.2941*	0.4749
DI_CH	1.0318	0.3056***	0.2778***	0.2645***
PRES	0.8071	0.2140***	0.2534***	0.3485**
CLR	0.4436	0.2625*	0.2257*	0.2361*
PUBLIC	-1.2725	0.1966***	0.2251***	0.3013***
PUBLISH	1.1757	0.3001***	0.3021***	0.2389***
YR	-0.1332	0.0299***	0.0289***	0.0349***
Intercept	-32.1576	8.7296***	8.5614***	11.3187**
N	71			
R-Sq.	0.6553			
Adj. R-Sq.	0.5766			

***: parameter estimate is significant at 1% level. **: 5% level. *: 10% level.

Table 11: Same as above, but no control for year.

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	1.7717	0.7922**	0.6455***	1.0059
dpYEAR	0.3538	0.0927***	0.0929***	0.0964***
BP	0.8554	0.5226	0.3580**	0.5959
NO_BLINE	0.5376	0.2829**	0.2924*	0.5045
WUIONLY	-0.1427	0.2963	0.2774	0.2575
POC	0.2322	0.4195	0.3732	0.3663
PROTEST	0.2482	0.3620	0.3459	0.5596
DI_CH	1.5015	0.3300***	0.3245***	0.4210***
PRES	0.6165	0.2412**	0.2099***	0.2919*
CLR	0.1804	0.2942	0.2318	0.2279
PUBLIC	-0.9801	0.2132***	0.2424***	0.4996*
PUBLISH	1.3757	0.3414***	0.3268***	0.3913***
Intercept	-16.3445	9.1726*	7.4790**	11.6534

N	71
R-Sq.	0.5357
Adj. R-Sq.	0.4396

Table 12: Primary model output with both BP and Y_S_FIRE as subjective risk proxies and control for year of study

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	2.8797	0.7480***	0.6751***	0.7548***
dpYEAR	0.2529	0.0773***	0.0806***	0.0176***
BP	0.3347	0.4815	0.3258	0.3661
Y_S_FIRE	-0.5631	0.1834***	0.1122***	0.0705***
NO_BLINE	0.7096	0.3308**	0.2159***	0.1245***
WUIONLY	0.2634	0.2511	0.2708	0.2075
POC	0.0180	0.3927	0.2772	0.2004
PROTEST	-0.0537	0.3635	0.2003	0.1834
DI_CH	0.2635	0.3794	0.3087	0.1311*
PRES	0.6722	0.2045***	0.2329***	0.3310*
CLR	0.4611	0.2450**	0.2144***	0.2542*
PUBLIC	-1.2911	0.1836***	0.2234***	0.3126***
PUBLISH	1.3282	0.2845***	0.2730***	0.1346***
YR	-0.1281	0.0280***	0.0266***	0.0243***
Intercept	-25.1015	8.4657***	7.4123***	8.3665**
N	71			
R-Sq.	0.7049			
Adj. R-Sq.	0.6312			

Table 13. Primary model output with SQRTYR as subjective risk proxy and control for year of study

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	2.8539	0.6234***	0.6519***	0.7291***
dpYEAR	0.2741	0.0771***	0.0767***	0.0333***
SQRTYR	-1.6548	0.4627***	0.3154***	0.2920***
NO_BLINE	0.5553	0.3100*	0.2146**	0.2070**
WUIONLY	0.2068	0.2503	0.2622	0.2039
POC	-0.0003	0.3630	0.3258	0.2771
PROTEST	0.3181	0.3329	0.1961	0.2238
DI_CH	0.1604	0.3746	0.3008	0.2070
PRES	0.6267	0.2040***	0.2136***	0.2971*
CLR	0.4582	0.2471*	0.2159**	0.2489*
PUBLIC	-1.2750	0.1852***	0.2234***	0.3209***
PUBLISH	1.6362	0.2791***	0.2459***	0.1752***
YR	-0.1469	0.0287***	0.0281***	0.0287***
Intercept	-23.4423	6.7409***	6.9193***	7.5480**
N	71			
R-Sq.	0.6934			
Adj. R-Sq.	0.6235			

Table 14: Primary model output with SQRTYR as subjective risk proxy and no control for year of study

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	1.1650	0.6339*	0.5172**	0.6221*
dpYEAR	0.3570	0.0903***	0.0929***	0.0918***
SQRTYR	-1.2068	0.5444**	0.3739***	0.5660*
NO_BLINE	0.9240	0.3612**	0.3090***	0.5004*
WUIONLY	-0.1513	0.2880	0.2643	0.2524
POC	-0.2288	0.4316	0.3935	0.3303
PROTEST	0.9300	0.3723**	0.3218***	0.5809
DI_CH	0.8979	0.4143**	0.3279***	0.4165*
PRES	0.4668	0.2416*	0.1991**	0.2616
CLR	0.1666	0.2881	0.2296	0.1994
PUBLIC	-0.9640	0.2096***	0.2472***	0.5324
PUBLISH	1.7445	0.3335***	0.2773***	0.3838***
Intercept	-7.8126	7.2022	5.8935	6.8992
N	71			
R-Sq.	0.5522			
Adj. R-Sq.	0.4595			

Table 15: Subset model output, baseline risk measure given in survey as subjective risk proxy, no control for year of study

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	1.9984	0.8701**	0.7726**	0.6659**
dpYEAR	0.2430	0.0907**	0.0744***	0.0156***
BLINE	0.0191	0.0358	0.0345	0.0070*
POC	-0.8395	0.6426	0.7778	0.2768**
DI_CH	1.0879	0.5950*	0.5940*	0.2248***
PRES	1.1391	0.3686***	0.4309**	0.4781*
CLR	0.9596	0.5038*	0.4652*	0.3340**
PUBLIC	-1.4612	0.2244***	0.2395***	0.2357***
PUBLISH	1.9187	0.4466***	0.5719***	0.1775***
Intercept	-18.9429	10.0094	8.9022**	7.7191*
N	48			
R-Sq.	0.6766			
Adj. R-Sq.	0.6000			

Table 16: Subset model output, years since major fire as subjective risk proxy, no control for year of study

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	2.4393	0.9582**	0.9119**	0.9083*
dpYEAR	0.2501	0.0855***	0.0749***	0.0146***
Y_S_FIRE	0.3272	0.3330	0.2213	0.1803
POC	-0.5924	0.6806	0.7583	0.2625*
DI_CH	1.7689	0.9487*	0.7731**	0.3816***
PRES	1.1832	0.3685***	0.4419**	0.4984*
CLR	1.5872	0.8571*	0.6671**	0.5512*
PUBLIC	-1.4847	0.2239***	0.2459***	0.2076***
PUBLISH	2.1753	0.5366***	0.6127***	0.1805***
Intercept	-25.1801	11.6814**	11.0241**	10.8240*
N	48			
R-Sq.	0.6822			
Adj. R-Sq.	0.6070			

Table 17: Subset model output, burn probability data as subjective risk proxy, control for year of study

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	5.3284	1.7921***	1.6933***	2.5679
dpYEAR	0.2318	0.0837***	0.0799***	0.0250***
BP	3.6967	1.9200**	1.7066**	2.2487
POC	0.4091	0.8664	0.6778	0.9066
DI_CH	3.3752	1.2807***	1.1349***	1.4631*
PRES	1.1457	0.3564**	0.4272**	0.4813*
CLR	2.4570	0.8862***	0.7707***	1.1791
PUBLIC	-1.4048	0.2194***	0.2427***	0.2055***
PUBLISH	3.6595	0.9967***	0.9189***	1.0746**
YR	-0.2962	0.1423**	0.1248**	0.1846
Intercept	-56.8519	20.5025***	19.2304***	29.3277
N	48			
R-Sq.	0.7084			
Adj. R-Sq.	0.6296			

Table 18: Subset model output, burn probability data as subjective risk proxy, no control for year of study

Variable	**Parameter Estimate**	**Std. Err.**	**Robust Std. Err.**	**Cluster Std. Err.**
LINC2017	2.0296	0.8722**	0.7625**	0.6617**
dpYEAR	0.2580	0.0864***	0.0739***	0.0193***
BP	0.1038	0.8762	0.6580	0.6243
POC	-0.8401	0.6516	0.7881	0.2821**
DI_CH	1.0695	0.6701	0.6056*	0.3954*
PRES	1.1309	0.3716***	0.4285**	0.4796*
CLR	0.8921	0.4891*	0.4322**	0.3377*
PUBLIC	-1.4559	0.2274***	0.2481***	0.2457***
PUBLISH	1.8962	0.5475***	0.5870***	0.3160***
Intercept	-19.2649	10.1222*	8.5557**	7.8735*
N	48			
R-Sq.	0.6743			
Adj. R-Sq.	0.5971			

Table 19: Subset model output, square root of years since fire as subjective risk proxy, control for year of study

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	2.3712	1.1581**	1.2222*	1.4186
dpYEAR	0.2487	0.0877***	0.0769***	0.0037***
SQRTYR	0.7113	1.1767	0.6912	0.9256
POC	-0.6237	0.7479	0.7929	0.4992
DI_CH	1.8071	1.1879*	0.7874**	0.7187*
PRES	1.1845	0.3736***	0.4491**	0.5053*
CLR	1.6427	1.0091	0.5621***	0.5407**
PUBLIC	-1.4856	0.2270***	0.2497***	0.2130***
PUBLISH	2.1486	0.5794***	0.6030***	0.3039***
YR	-0.0114	0.0908	0.0683	0.0856
Intercept	-24.6044	12.2989	12.6477*	13.6148
N	48			
R-Sq.	0.6823			
Adj. R-Sq.	0.5965			

Table 20: Subset model output, square root of years since fire as subjective risk proxy, no control for year of study

Variable	Parameter Estimate	Std. Err.	Robust Std. Err.	Cluster Std. Err.
LINC2017	2.2807	0.8994***	0.8258***	0.7841*
dpYEAR	0.2474	0.0859***	0.0761***	0.0106***
SQRTYR	0.8153	0.8315	0.4721*	0.4189
POC	-0.6666	0.6579	0.7625	0.2315**
DI_CH	1.8766	1.0393*	0.7534**	0.4376**
PRES	1.1844	0.3688***	0.4430**	0.4985*
CLR	1.6842	0.9420*	0.6141***	0.5368**
PUBLIC	-1.4858	0.2240***	0.2463***	0.2120***
PUBLISH	2.1741	0.5363***	0.5967***	0.1864***
Intercept	-23.9704	11.0861**	9.9744**	9.4128*
N	48			
R-Sq.	0.6822			
Adj. R-Sq.	0.6069			